Responses to Reviewer 1

Review of "A realistic physical model of the Strait of Gibraltar" by A. Tassighy et al. A very interesting paper that presents the best laboratory physical model of the Strait of Gibraltar ever made so far. Congratulations to the authors. The experiment provides such an amount of experimental data that authors face a serious problem to select which ones are going to be presented in the manuscript, which one is the logical sequence to show the results, how are they connected to each other and so on. The final manuscript focus on tidal dynamics for different tidal forcing and compare the results with those already published in papers that deal with the same issue, either from observational or numerical approaches. There are a large number of experimental data related to high-frequency dynamics that are not addressed here (they will be covered in other ongoing articles) for obvious reasons of length. The analysis of the tidal regime alone has resulted in this extensive manuscript, which will be read with great interest by the oceanographic community working in this very special environment. The manuscript should be published in OS, although authors must revise it with care. Below is a list of (mostly) minor or very minor points, whose extensive length is a consequence of the interest with which I have read the manuscript.

We are really grateful to the referee for her/his thorough review and the positive comments. Her/his comments helped us to make the manuscript more clear and refining certain discussions such as about the composite Froude number and the transports and fortnightly modulation of the baroclinic velocity.
Given the general comment of the referee about the extensive dataset of the HERCULES experiment and the difficulty to organize and present the results in a first, general, overall dynamics paper, which has to include extensive description of the physical model as well, we added a perspective paragraph in the conclusion section to describe better the different processes and areas considered in the measurements, encouraging members of the community to contact us for using the available experimental data.
A point by point answer to each of the referee's raised issue is given below in blue text. All changes have been highlighted by blue text in the revised manuscript.

Comments to the manuscript (following the order of writing)

L35. "the Mediterranean Outflow Water strengthens the Atlantic Meridional Overturning Circulation" I wouldn't dare be so categorical. There is still considerable debate about the role of the Mediterranean outflow in the AMOC.
We replaced 'strengthens' with 'contributes to'.

L46. "observationally-based" rather than "experimental"?
We added 'based on observational data'.

L78-86. A short phrase stating the origin, orientation and positive direction of x and y-axes would be welcome. Origin is CS summit, as stated in line 216 later, but perhaps it is a good idea to mention it here. It seems rather obvious that orientation is along ("x") an across ("y") Strait, with positive directions eastward and northward, respectively. However, it is not until caption of Figure 4 (around L400) that this convention is explicitly said. The reason of this suggestion is that, here and there in the manuscript appear contradictions (writing errors?) regarding velocity and coordinate signs. Therefore, the convention must be clearly stated and this Section seems to be the right place.
We added at the beginning of section 2 'The coordinate origin is set at the Camarinal Sill (CS) summit. A right-handed coordinate system is used, with the x-axis oriented eastward and the y-axis oriented northward.'

Although not explicitly stated, the Atlantic and Mediterranean waters have the same temperature in all simulations, right? This makes density solely a function of salinity, which is important in certain parts of the work (i.e., transports). It would be appropriate to mention this at some point in Section 2.
We added at old line 270 'both at the same temperature of $\approx 18°$ so that the density is a function of salinity only in the experiment'.

L122. "In the vertical momentum equation (3c)..." equation (3c) is horizontal momentum isn't it? Done.

L 135-136. "and Hs $\simeq$ 100m is the half water depth at CS" Actually, water depth at CS is nearly 300m. Why this scaling factor of 100m? Does it have to see with the mean thickness of the Mediterranean layer at CS?
For the scaling, the order of magnitude is important, so we choose 100m for simplicity without any loss of generality.

L158. A "s" is missing. 35.77s Done.

L163. The Strouhal number is new to me. Other readers may encounter the same difficulty of understanding what does it mean. A definition and relevance of this number (similar to the one given for the Burger number in lines 151-153) would be welcome
We added at line 143 when defining the Strouhal number
'$Ro = U/(fL)$, $Re = UH/\nu$ are the Rossby and Reynolds numbers, respectively, and $St = L/(UT_{tide})$ is the Strouhal number which compares the time scale of the oscillation (tide) and the advection time scale, and hence it is a measure of the unsteadiness of the flow.'

L174. " .. on the slope angle.." Bottom slope? Interface slope? Yes, bottom slope, we added it.

L223. Delete "is" Done.

L229-230. References within brackets. Done.

L242. 6% is a clear overestimate. Tidal transport exceeds 3Sv (if you refer to the amplitude of the oscillation). 1.5% - 2% is more realistic. We thank the referee for this precision, we replaced 6% with 1.5% - 2%

Figure 1. Nice Figure showing the core of the experimental team (and the infrastructure). I like it.
We thank the referee for this appreciation, we put this picture to give an idea of the size of the model.

L249. Are units of the density difference ("kg/m3") correct? In fact, 0.19kg/m3 is a very tiny difference, ten times smaller than in the actual ocean, which contradicts the statement of L143-144 about enhancing the density difference (and, also, units in the color scale of some figures, i.e., Fig.9).
The referee is right, the value is 19kg/m3 and not 0.19, we corrected this in the text.

L260-261. No continuous red line in Figure 2a. Grey line perhaps, as stated in the caption? And y-axis indicate density difference, not salinity. On the other hand "..(25cm in the ocean).." is 25m, right?
The referee is right, we corrected all this in the text and in the caption.

L276. "... of the order of magnitude of 1m/s..." Where? At CS? Please indicate.
Yes, the value refers at CS, so we added 'at CS'.

L279. What is it meant by "purely barotropic velocity" here?. Does it refer to the amplitude of a purely barotropic tide, i.e., the tide resulting if both Atlantic and Mediterranean basins are filed with homogeneous water (no baroclinic forcing at all)?
The referee is right, we have made the precision in a footnote on page 12 .3as follows
'The purely barotropic velocity was measured in ad-hoc experiments in which the full rotating tank was filled with homogeneous fresh water with ρ_0 and only the barotropic forcing was applied.'

Figure 2 caption. (L-3 says "salinity measurement" while y-axis indicate density difference. L7 mention SSH variations, L8 mention SSH anomaly. What is the difference between both SSHs?Apparently, they refer to the same process. L9-10 about the phase shift: see comment L305 below.

We replaced salinity with density measurement. 'SSH variations' and 'SSH anomaly' refer to the same quantity so we replaced with 'SSH anomaly' everywhere in the text and in the caption.

L291 No "red curves" in Figures 2c,d, but cyan or grey. Done.

L305 A phase shift of 0.2Ttide between the Atlantic basin and Tarifa station is declared. Taking Ttide the period of M2 (12.4h approx.) the shift would be 2.5h or 70º phase difference for M2 frequency. In the actual ocean, this phase difference is hardly 15º, much less than the one reported in the manuscript (consult harmonic constants in the interactive web https://portus.puertos.es of Puertos del Estado, SPAIN). In the real ocean the phase difference is caused by the Earth rotation in order to maintain geostrophic balance at tidal scale, achieved via changing the sign of the cross-strait free surface slope at tidal time-scale. Moreover, as mentioned in lines 285-289, the relationship between the sea-surface oscillation and the tidal barotropic current in the actual ocean cannot be reproduced in the laboratory. In the experiment, eastwards flow corresponds to rising tide in the Atlantic, just the opposite that in the ocean. That means that the Coriolis force causing the cross-strait slope (and, hence, the phase shift in Tarifa) acts in opposite direction in the real ocean and in the experiment, which invalidates the comparison of results. I suggest removing these comments both here and in the caption of Figure 2.
The referee is right: since we are not in similarity with respect to the external Froude number and because of the opposite sign between the SSH and the sign of the barotropic velocity, the comparison is not possible. So we removed the sentence in the text and in the figure's caption as proposed by the referee.

L379. When speaking of mean density anomaly, do you refer to the ratio $\Delta\rho/\Delta\rho_0$ that appears in the top panels of Figure 4 (where $\Delta\rho = \overline{\rho} - \rho_0$ $\overline{\rho}$ being the averaged density over the seven tidal cycles ($\Delta\rho_0$ is already defined), right?). If so, it is not a density anomaly, but a ratio of density differences (dimensionless). Assigning a symbol to this anomaly, which is frequently discussed in the text, would make it easier to read.
As suggested by the referee, we have defined the dimensionless density anomaly ratio $\rho^* \equiv \Delta\rho/\Delta\rho_0$ and added in the text '... highlights the mean dimensionless ratio of density differences $\rho^* \equiv \Delta\rho/\Delta\rho_0 = \overline{\rho} - \rho_0/(\rho_M - \rho_0)$, with $\overline{\rho}$ being the averaged density over the seven considered tidal cycles.'We have then replaced it where mentioned with the new defined variable ρ^*.

L388. When saying " ..waters are more diluted (about 30%) etc..", do you mean that the density anomaly ratio is 0.3?
Yes, but this corresponds to the dilution of the Mediterranean waters with respect to the initial value. We have precised in the text 'of about $0.3\rho^*$' and evrywhere when indicated in %.

L390. "surface" instead of "layer" Done.

L391. The grey line, according to Figure 4 caption, is $\Delta\rho/\Delta\rho_0 = 0.5$ (dimensionless), which corresponds to $\Delta\rho = \Delta\rho_0/2$ (units of kg/m3). Please modify so that text and figure caption do not disagree. Moreover, this specific value is used (implicitly) to define the pycnocline, as stated later in lines 413-414. In my opinion, what is said in these two last lines should be moved there (to the end of line 391, more or less). Done.

L399. "deepen" or "descend" instead of "lower"? Done.

Figure 4 caption. L3 "... with the averaged velocities..." Actually, it shows the vector sum of averaged u and w with the later largely exaggerated. The same applies to Figure 8 and also to Figures B1 and B2 in appendix B.
The quiver plot uses different scaling in the vertical and horizontal directions; consequently, one centimeter in the vertical does not correspond to the same graphical distance as one centimeter in the horizontal. This introduces an ambiguity in how the arrow angle can be defined. Two main conventions exist for specifying arrow orientation. In this study, we adopt the data-coordinate convention, so that a velocity vector tangent to the seabed is displayed as an arrow tangent to the bottom topography. Because the figures are intentionally stretched in the vertical to enhance readability, preserving geometric angles would produce misleading impressions of the true flow direction. Arrow length is therefore determined solely by the in-plane speed, independent of any angle distortion. There is no unique or fully objective method for representing velocity vectors, and the

convention used here is relatively common. For clarity, we have now added this explanation to the manuscript and consistently refer to the quiver representations as "in-plane velocity vectors" throughout the text.

L5 I guess you speak of the time-averaged composite Froude number, do you? Please indicate. Yes, we specify now in the text that it is time averaged.

L405-406 The mean thickness computation in equation (9) requires the definition of an interface depth, which seems to be the pycnocline defined as $\Delta\rho/\Delta\rho_0 = 0.5$. This is said later in lines 413-414, but should be said before. Moving these lines (as I suggested above) will improve the text understanding.
We thank the referee for this comment (and those above related). We rewrote this paragraph for more clarity (also in consideration of the above referee's comments) and added in the appendix E the different computations of the Froude number. Since the most reliable results that are also in accord with the base assumption for the theoretical derivation of the composite Froude number, we replaced the old figures with the Froude number, that were computed with the constant initial density difference, using the local density difference instead. Please see appendix E for further details.

L423-424. "... given in figures B1 and B2 in the appendix". Remove ", respectively, that are given" Done.

L437. "horizontal velocity" rather than "horizontal flow". I have seen that the term "horizontal velocity" usually refers to the rotated (along-strait) component of the horizontal velocity throughout the text; particularly in the second half of the paper this seems to be the rule. But not always means this component. For instance, it does not in the aerial view maps of Figures 5 and 7, which actually show the "horizontal velocity". Perhaps it is convenient to state a criterion somewhere in section 2 or keep the notation "u-component" for the horizontal velocity in the second part (after presenting Figure 7). As you can see below in my review, there are a few comments addressing this point.
In the presentation of the results from vertical sections, the horizontal velocity always refers to along-strait horizontal velocity as we are only measuring in-plane velocities. To avoid confusion with the horizontal planes presenting horizontal velocities, we replaced the 'horizontal' velocity spelling of the measurements in the vertical sections with 'along-strait' velocity.

Figure 5 caption. L1 "Averaged horizontal velocity" instead of "Averaged meridional and zonal velocities". Insert "relative" before "vorticity" Done.

Figure 6 caption. Are the presented plots time-averaged values? Please indicate. L3 "dashed" should be "dotted" and "dotted" should be "dashed". The sentence "vertically integrated volume transport and below salt transport;" has nothing to see with the panels. Please, remove. Done.

L451-452. Remove the sentence "The bottom panels .... three transects" or, better yet, incorporate it at the caption of Figure 6. We removed the sentence.

L454. Remove "where ...... waters". Unnecessary; it has been already said. Done.

L466. "... to the shallowing pycnocline.." instead of "... to the decreasing pycnocline depth..." Done.

L472-473. "... 50% higher during neap tide compared to the spring tide condition with exception of the southern transect where values remain similar for all tidal conditions" This is not what can be deduced from Figure 6. Actually, it is in the southern transect where G2 differences between spring and neap tides are the greatest. Maybe you are referring to the northern transect in the caption?
The referee is right. We replaced 'approximately' with 'up to' 50% and replaced 'southern' with 'northern' transect.

L474. "x=-30cm and x=-45cm" instead of "x=30cm and x=45cm"? (this has to see with my comment to lines 78-86 above). Nice to indicate the corresponding distances in km. Done.

L480. "horizontal velocity" instead of "zonal and meridional velocities" Done.

L480. "tidal average"? Such an average would provide a unique value for the averaged period, that is, for a tidal cycle, whereas Figure 7 presents snapshots at different moments of a tidal cycle. Maybe authors are referring to the average of the seven resolved tidal cycles for "Outflow", "HWS", etc.?
Yes, we are referring to the average over the four (for these horizontal measurements) resolved tidal cycles at the given phase. We have now specified it in the text and caption.

L486. "…(latter two row panels)" Do you mean "third row", which is where inflow is presented
We refer to both panels, so we added in the text 'during outflow and high water slack' and 'during inflow and low water slack'.

L492. "averaged over the phases of maximum inflow" Again, do you refer to the maximum inflow averaged over the seven resolved cycles?
Yes, we are referring to the average over the seven resolved tidal cycles at the given phase. We have now specified it in the text and caption.

L497. "demonstrates that tidal forcing is capable of mixing the entire water column down to 500m". Not clear to me. I think you are referring to the periodic occupancy of the whole water column by Mediterranean waters (deep red colors in Figure 9). It could be mixing, but, in my opinion, is the uprising of the interface during the outflow which can eventually reach the surface during spring tides. If so, it is advection rather than mixing. A different issue is that, in HERCULES experiment in particular, the whole Mediterranean layer is well mixed from the bottom to the interface (which eventually can reach the free surface) with clear density differences between inflow (light red) and outflow (deep red) periods. These differences can be of the order of the mentioned percentage (15-20%, normalizing the density differences by $\Delta\rho_0$ I guess. Please, indicate)
We where not precise in the text. We wanted to show that tides are capable of advecting the full (mixed) water column to the east during inflow, causing periodic oscillations in the density of the order of $(0.15-0.2)\rho^*$. This is true also for the observational data, as seen from the colorbar, even if not homogeneously from cycle to cycle and down to the bottom. This is probably also due to the different composition of waters in the real ocean. We have rewritten the paragraph as follows:
'A closer inspection, presented in figure 9, demonstrates that tidal forcing is capable of advecting the full water column down to 500m of mixed waters east of CS during spring tide, with oscillations of the density ratio as a function of the tidal phase of the order of $(0.15-0.2)\rho^*$. Since in the experiment we do not have different density components in the Mediterranean and in the Atlantic waters, in the experiment the whole Mediterranean layer is well mixed from the bottom to the interface with clearer differences between inflow and outflow.'

*L504. "water can still overflow" instead of "flow can still surmount"*Done.

Figure 8 caption. Please, indicate what the bottom panel is.
We added: 'The bottom panel indicates the variation of the depth integrated barotropic velocity at CS over a tidal cycle with the dots indicating the maximum inflow and outflow corresponding to the above panels for neap and spring tides.'

Figure 9. Please, explain what are the numbers given at the left and right sides of the color scale. I guess, numbers on the left are the $\Delta\rho$ difference in the actual ocean, and numbers on the right are the same for waters in HERCULES experiment. If so, note that the values of the order of 10 kg/m3 that are representative of this scale are clearly contradicting (two order of magnitude greater) the values provided in line 249 for $\Delta\rho_0$ See my comment on L249 above as well.
We added explanation about the values of the colorbar in the plot, that are right here. The initial density difference given at the beginning of 0.19 was wrong ans it has been corrected following the referee's comment above.

L519. "The second-column panels show a clear offset between the pycnocline and the region of maximum velocity shear" The pycnocline ($\Delta\rho/\Delta\rho_0 = 0.5$) is clearly seen in Figure 9. The region (surface?) of maximum shear is not so clearly depicted. It can be inferred by the color contrast (reddish versus bluish shading, probably), but locating it with a certain degree of accuracy is not an easy visual issue. West of Camarinal this color contrast (shear "interface") is located in a well differentiated depth range than the pycnocline. However, both surfaces coincide acceptably well elsewhere. Maybe the comment is referring to the west of Camarinal specifically? Please clarify the quoted sentence above.

The referee is right saying that it is difficult to see the surface of maximum shear just looking at the color contrast of the along-strait velocity. A more direct comparison is given later in Figure 11. We slightly modified the text saying that the colormap suggests this offset, but we refer then to Figure 11 where it can be clearly seen. And yes, the offset is present west of CS and up to 20cm east, so we precise this as well in the revised text. We have also added and additional figure in the appendix D with correlation maps between S and N at different tidal phases along-strait and for neap and spring tides in figure D1.

L521-523. I have read the sentence "Vertical velocities ….. spring tide conditions" several times and I cannot catch its meaning and the reason to be here. Can you be a bit more explicit?

The hydraulic jump west of the Camarinal Sill is characterized by an abrupt reversal in vertical velocity, transitioning from strongly negative to strongly positive values over a short distance. Consequently, the vertical velocity field provides a clear indicator of the location and intensity of the internal hydraulic jump. Our observations show that during neap tides, the vertical velocity attains higher maximum and minimum values within a confined region west of the sill. In contrast, during spring tides, the vertical velocity extremes are lower in magnitude but spread over a broader area. This behavior indicates that the hydraulic jump is displaced farther west during spring tides, whereas it remains confined to a more restricted region during neap tides. We have rephrased this sentence as follows

'The hydraulic jump west of CS is characterized by an abrupt reversal in vertical velocity, transitioning from strongly negative to strongly positive values over a short distance. Consequently, the vertical velocity field provides a clear indicator of the location and intensity of the internal hydraulic jump. The internal hydraulic jump is evident at the sill for both neap tide and spring tide maximum outflow conditions, along with two additional control points farther downstream (to the west), more pronounced during spring tide. Our observations show that during neap tides, the vertical velocity attains higher maximum and minimum values within a confined region west of the sill. In contrast, during spring tides, the vertical velocity extremes are lower in magnitude but spread over a broader area. This behavior indicates that the hydraulic jump is displaced farther west during spring tides, whereas it remains confined to a more restricted region during neap tides.'.

L525. "even negative velocities"? Do you mean "even positive"? (cf, column 2, row4 in Fig 8, better seen in column 2, row4 of Figure 10) Yes, correction added.

L547-549. Same comments as in L419 above. I think the mismatch is limited to the west flank of CS.

The mismatch is found during both spring and neap tides, west of CS and up to 20 cm (5Km) east of CS (see also appendix D where we add an additional figure displaying both N and S).

L548. "…under spring tide". It rather seems "neap tide"

In fact the decorrelation is always present but with different amplitudes for neap and spring tides and HWS and LWS (see also reply to the following comment). We rewrote the sentence as: '... for both spring tide and neap tide conditions, even if less marked in this latter case (see also figure 11 and C1). The same holds true during low-water slack.'

Figure 11 caption. Are vertical profiles just above CS? Please indicate Yes, we have made the precision in the text. We have also added an additional figure in the appendix D to show the correlation map between S and N at different tidal phases, to show that it is more marked for spring tide conditions and also present west and up to $x = 20cm$ east of CS.

L572-573. "…or from velocity interface" Are you meaning "velocity interface (that is, u=0) or surface of maximum horizontal (u-component) velocity shear? The surface u=0 can eventually disappear during short periods of the tidal cycle and is useless for estimating layer thicknesses

We write clearly now in the text that we refer to the 'maximum vertical gradient of along-strait velocity'.

Figure 12 caption. "velocity" instead of "speed" Done.

L593. "(positive values of ub)"? isn't it "negative"? Yes, correction made.

L656. Add "(see the locations indicated in Figure 1)" after "conditions". Done.
Figure 13. As far as HERCULES experiment is unable to reproduce correctly the sea level phase relationship of the surface oscillation in the Atlantic and Mediterranean basins (lines 285-290 of the manuscript), it does not seem like a good idea to show the oscillation in both basins. I think the one in the Atlantic side is enough to delimitate the inflow and outflow periods in the Hovmöller diagrams below. I suggest to remove the Mediterranean sea level curve and add vertical dashed lines across the Hovmöller diagrams showing inflow and outflow periods.
We agree with the referee, and we propose to display the depth integrated barotropic velocity u_b at CS as done in the other plots. In figure 13, we remove the arrows showing the barotropic flow in the Hovmöller diagrams of the pycnocline as asked further below and make the figure more readable. This change between SSH and u_b has been applied also to figure 14. Note that the figure 14 has additionally changed since we compute now the composite Froude number using a local g' as explained in appendix E of the revised manuscript. Finally, In figure 15 we removed the SSH panels and we just keep the barotropic flow arrows in the Hovmöller diagrams of the buoyancy transport.

Figure 13, caption. If possible, mark with dots the locations of M02 and M05 in the bathymetry left panel of the center section.
Since we mention MO2 and MO5 in the discussion of figure 13 it makes sense to see the position of MO2 and MO5 in the vertical transect, so we add the position in the figure 13 as suggested. We think it is also appropriate to add it in figure 3a when defining the measurement regions.

Figure 13, caption. "vertically averaged horizontal velocity" or "vertically averaged u-component of horizontal velocity"?
In the presentation of the results from vertical sections, the horizontal velocity always refers to along-strait horizontal velocity as we are only measuring in-plane velocities. To avoid confusion with the horizontal planes presenting horizontal velocities, we replaced the 'horizontal' velocity spelling of the measurements in the vertical sections with 'along-strait' velocity.

L660-661. "Moreover, we also observe that the position of this front does not happen at the same time in the three transects" I cannot see it in Figure 13. Perhaps some indications/marks in the very Figure would clarify the issue.
L662. I cannot identify the 2s shift mentioned in the text. Perhaps the marks I suggest above may help...
As explained above, we removed the SSH panels and replaced them in the top panels with the barotropic flow. Consequently, the arrows on the Hovm"oller diagrams were also removed. Thanks to the referee's comment, we attempted a more quantitative estimate of the phase shift; however, we found it difficult to discern, and moreover, it varied from one tidal cycle to another. Therefore, as we are no longer confident in this result, we have removed it from the discussion throughout the manuscript and have updated the figure captions and corresponding text accordingly. We are investigating closer this issue in a forthcoming paper focusing on ISW. we have added instead in the revised manuscript line 712:
"We attempted a quantitative estimate of the phase shift using our experimental data; however, it proved difficult to identify and was found to vary from one tidal cycle to another, preventing any robust conclusion. This issue is more appropriately addressed in a forthcoming paper focusing on internal solitary waves (Tassigny et al., 2026)."

L670. "Garcia-Lafuente et al. (1990, 2018); Sanchez-Roman et al. (2018); Roustan et al. (2024a)" within brackets. Done.

L673. "...with respect to the reported phase shift between north and south" Are you speaking of the sea-level phase difference (i.e. barotropic tide)?
We referred to the internal tide, as said just above. We cannot say anything about the external tide and the SSH shift since we

are not in similarity with the external Froude number. As said above, we removed this discussion in the revised manuscript.

L680. "x=-30cm and x=-45cm"? Yes. We corrected it in the text.

L691. "x=-50cm"? Yes. We corrected it in the text.

L698. "... or the velocity interface, creating..." or " ...or the surface of maximum shear of the u-component of the horizontal velocity, creating.." Done.

L708. "the transport of salt." This is true as far as T=cte. I guess T=cte has been implicitly assumed in the study. I have already made a comment on the interest of stating this constancy in Section 2. Nevertheless, it wouldn't hurt to mention it again here.
The temperature within the tank is uniform. Density gradients are generated exclusively through variations in salt concentration, with ethanol added in some experiments to correct the refractive index. The Laser-Induced Fluorescence (LIF) technique allows measurement of Rhodamine concentration, which is directly related to fluid density following accurate calibration, as described in Appendix A. In the ocean, as the referee notes, density gradients result from both salinity and temperature variations; therefore, the quantity $\Delta\rho, u$ does not correspond exclusively to salinity transport. Assuming linearity of the equation of state, $\rho = \rho_0 + \alpha(T - T_0) + \beta(S - S_0)$, it is in principle possible to decompose $\Delta\rho, u$ into a temperature transport contribution $\alpha, \Delta T, u$ and a salinity transport contribution $\beta, \Delta S, u$. To avoid any misunderstanding and unnecessary additional discussion, we have relabeled the "salt transport" as "buoyancy transport."

L709, eq.(16). Limits of integrals in the equation are z=0 and z=bottom, aren't they? To mention the limits in the text (or in the equation) would be OK to avoid any confusion with transports in either layer. Done.

Figure 15 caption. "Mean transport contributions... " Add "(per width unit)" so that the sentence reads "Mean transport (per width unit) contributions...." On the other hand, does "mean" here refer to tidally averaged? Also write "transects" instead of "planes"
Mean refer to tidally averaged, as indicated by the operator defined in equation 8. Corrections made.

Figure 16 caption. Indicate what the black arrows are. (Transports per width unit, aren't they?)
Black arrows are the vertically averaged along-strait tidal velocities u_b, giving the reference of the tidal phase. We have added this sentence in the caption of Figure 16.

L726. "..positive"? The mean (tidally-averaged?) transports in Figure 15 are mainly negative in all sections (north section could be a small exception). Figure 16 shows that the transport can be positive in spring tides in the eastern part, but only for short periods. But, when averaging over a tidal cycle, it appears to be negative, as seen in Figure 15. So what does "positive" refer to in this line?
We are sorry for this error, we should have written "negative" instead of "positive". Correction made.

Lines 743-796. This part of the text should be a new sub-section 4.2.6 named "Time-variable tidal forcing amplitude" or "Transient tidal forcing" or something similar. These lines clearly deal with a different topic than subsection "4.2.5 Transports"
We originally addressed this point in the transport section because the issue arose from examining the studies of Vargas et al. (2004, 2006), who demonstrated that the amplitude of the net baroclinic flow varies with tidal amplitude and subsequently quantified associated transports. The referee is correct that, as presented, this discussion is not directly related to transport estimates. We have therefore reorganized the manuscript by creating a new subsection, now Section 4.3, entitled "Fortnightly modulation of the baroclinic flow." The revised title with respect to the one proposed by the referee reflects our intent to include transient experiments only for reasons of direct comparison with observational data. In this paper, we deliberately avoid detailed analysis of transient dynamics (promising so far very interesting results), which will be the subject of a separate

manuscript.

L751. "horizontal velocity component" I think "along-strait" or "u-component" is meant (line 755). Please, correct. Same applies to Figure 17 caption. See response above about the same issue.

L761. "...three moments..." instead of " ...three points..." We wrote 'three instants'.

L763. "In the third row..." Done.

Figure 17 caption. Sentence "with a cutoff period....... Vargas et al., 2006)" is unnecessary as it is already in the text. I suggest a short "(see text)" instead. Add " in the transient tide experiment" after "(cyan line)" in line 7. Done.

L766. "... consistent with the reduced vertical velocities"? No vertical velocities are presented. Is it meant "...consistent with the reduced u-component velocity in the vertical profiles"?
We removed the sentence, since it refers to an old figure in which we also showed the vertical velocities.

L771. A similar comment about the sentence "...shape of the vertical velocity profiles"
We replace "vertical velocity profiles" by " vertical profiles of along-strait velocity"

L773-796. This part of the manuscript must be revised carefully. There is mention to Figures/panels that are not seen in the manuscript (i.e. L778 mention a "color scale" in Figure 18, as if this Figure were a sort of Hovmöller diagram);
Figure 18 is the correct figure. The three tidal forcing are labeled with three different colors, the lighter corresponding with no tide and the darker to spring tide. We replaced "color scale" by "colored lines" and "shades" with "colors" for more clarity.
line 773 indicates "Three possible mechanisms", when the text only addresses two;
We did not mention the third one explicitly in fact, so now we do it in the revised manuscript.
sentence in L786-789 "Consequently, the observed decrease in net baroclinic flux (flow?) at the sill cannot be attributed to tidal-to-mean energy transfer, as our results demonstrate the opposite effect" Apparently contradicts the previous sentence in L785 "...that the tidal flow transfers energy to the mean flow, thereby accelerating it".
In fact, the last part of the sentence 'Consequently, the observed decrease in net baroclinic flow at the sill cannot be attributed to tidal-to-man energy transfer, as our results demonstrate the opposite effect' was misleading. We now replace it with 'Consequently, the observed decrease in net along-strait baroclinic velocity at the sill cannot be attributed to the tidal-to-mean energy transfer.'.
The sentence "the flow tends to bypass the obstacle laterally rather than pass over it" in L790-791, when speaking, of a sill is confusing. How can a flow bypass laterally a sill? In my opinion, the flow either overflows the sill or remains stagnant in the upstream side
When the tidal strength increases, there is a larger flow fraction passing at the shallower sides of the sill (which is of course 3D), than that which overflows the highest part of the sill. In order words, the transports in the southern and northern transects increase for increasingly high tidal amplitude as compared to the middle transect which passes through the summit of the sill, as it is also shown in figure 15. We have now replaced the text as 'more water circumventing the sill than overflowing it above the summit'.
And an important question to be clarified, does the tidal flow in this final part have to see with the eddy-fluxes already discussed in several published articles in the literature. It seems that yes, but authors should clarify or comment the issue.
We thank the referee for this relevant remark. Previous studies state that an increase in tidal amplitude leads to a decrease in the net buoyancy transport, which is instead compensated by tidal transport via eddy fluxes. In the present study, we interpret the concept of eddy fluxes as representing the tidal and turbulent transports defined in equation (16) and shown in figure 16 (third and fourth rows, respectively).
As discussed in the text, the tidal and turbulent transports are approximately one order of magnitude smaller than the volume and buoyancy transports. In particular, the tidal transport is of order $\approx 5 \cdot 10^{-3}\,\mathrm{kgm^{-3}}$, while the turbulent transport is even smaller, whereas the buoyancy transport reaches values of order $\approx 5 \cdot 10^{-2}\,\mathrm{kgm^{-3}}$. Although we observe an increase in tidal

transport with increasing tidal strength, this increase is not sufficient to explain the corresponding reduction in buoyancy transport.

Therefore, the diminished net baroclinic velocity reported in our experiments, as well as in observational data when tidal forcing is applied, cannot be attributed to the action of eddy fluxes alone. This discussion has now been added to the revised manuscript and is proposed as a motivation to investigate additional mechanisms that may contribute to the reduced net baroclinic velocity. We added this in the discussion presented in the new section 4.3:

'A third mechanisms which may explain the reduced net baroclinic velocity when increasing the tidal amplitude are tidally driven *eddy fluxes*, which can account for large portions (up to 40–60%) of the total exchange transport (Bryden et al., 1994; Vargas et al., 2006) in regions where hydraulic control is intermittently lost, such as at Camarinal. These studies state that an increase in tidal amplitude leads to a decrease in the net buoyancy transport, which is instead compensated by tidal transport via the *eddy fluxes*. Tsimplis and Bryden (2000) and Bryden et al. (1994) documented strong tidal variability and interface depth oscillations at CS, which are associated with significant fortnightly and monthly fluctuations in layer transports that reflect the influence of tidal eddying on exchange dynamics. These *eddy fluxes*, arising from the positive correlation between vertical interface and tidal current fluctuations, contribute to augment the net transport beyond what is predicted by traditional, steady two-layer hydraulic theory.

In the present study, we interpret the concept of *eddy fluxes* as representing the tidal and turbulent transports defined in equation (15) and shown in figure 15 (third and fourth rows, respectively).

As discussed above with figure 15, the tidal and turbulent transports are approximately one order of magnitude smaller than the volume and buoyancy transports. In particular, the tidal transport is of order $\approx 5 \cdot 10^{-3}\,\mathrm{kgm^{-3}}$ (the turbulent transport is even smaller), whereas the buoyancy transport reaches values of order $\approx 5 \cdot 10^{-2}\,\mathrm{kgm^{-3}}$. Although we observe an increase in tidal transport with increasing tidal strength (cf. third row of figure 15), this increase is not sufficient to explain the corresponding reduction in buoyancy transport.

Therefore, the diminished net baroclinic velocity reported in our experiments, as well as in observational data when tidal forcing is applied, cannot be attributed to the action of *eddy fluxes* alone.

I have no posted comments to "Conclusions" section, as it should be partially rewritten by the authors in the light of the list of comments above. Neither have I revised Appendix A, as it addresses issues far from my expertise.

We slightly modified the conclusion section according to the referee's comments and added an overview of the measured areas and processes for future work. We merged the three figures of appendix A in one unique figure.

L904, Appendix B. "... indicate the in-plane velocity vectors" Actually it shows the vector sum of u and w components (with the later largely exaggerated) in the northern transect. The same applies to Figure B1 caption for the southern transect.

See the response above relative to figure 4.

References

Tassigny, A., Bordois, L., Carton, X., and Negretti, M.: Internal solitary waves in a realistic laboratory model of the Strait of Gibraltar, Dynamics of Oceans and Atmospheres, p. submitted, 2026.