Responses to Reviewer 2

Review of “A realistic physical model of the Gibraltar Strait” by Tassigny et al.

People love laboratory experiments. Even in an age where the smallest scales of turbulence are starting to become numerically resolvable, lab experiment give a sort of immediate physical connection that is hard to reproduce through simulation. It is good to know that the large turntable in Grenoble is being put to good use.

The particular experiment described in this paper is a scale model of the Strait of Gibraltar and immediately surroundings. The exchange flow is set up using a dam break scenario with topography that is realistic except for a scale factor of 10 and a bit of smoothing. Semidiurnal tides are imposed using oscillating plungers. Using modern laboratory measurements, including PIV, PLIF, ADV, conductivity and temperature sensors, and interferometers to measure surface elevation, the authors are able to gather data on velocity and stratification along three parallel transects of the strait. These are used to features at different phases of the tidal cycle and to compare conditions under spring and neap tide forcing.

For me the most interesting sections of the paper are those that describe detachment of the Mediterranean layer as it spills down the western flank of Camarinal Sill (CS) under conditions of outflow of the barotropic tide. The authors argue that strong barotropic outflow conditions create the adverse pressure gradient required for detachment and discuss consequences for mixing. Also interesting are insights into the role of bottom boundary layer processes in mixing, tidal rectification of the baroclinic flow and properties of the internal bore that propagates eastward. I suppose that some of the results on mixing between the upper layer and salty lower layer must be taken with a grain of salt since the Reynolds numbers are much lower than in the ocean.

This important issue is dicussed in section 2.3 and in a forthcoming paper focusing on the high-frequency dynamics.

Although the paper is lengthy, the narrative is generally easy to follow and there are just a individual points that need to be cleared up. I also have some suggestions regarding quantification of results that I hope the authors will consider. My recommendation is for moderate revision.

We are really grateful to the referee for her/his thorough review and the positive comments. Her/his comments helped us refining certain discussions such as about the maximal/submaximal regime and on the composite Froude number.

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.

Main Points

One line 55 the text mentions “uncertainties” that remain and lists a few general categories such as internal hydraulics and instabilities, but it does not specify what the uncertainties are. The Introduction would be more helpful if it gave the reader more specific information about the issues that are unsettled and how a laboratory experiment (as opposed to a numerical simulation) will clarify or inform.

We thank the referee for this remark. We have revised the Introduction to more clearly identify the unresolved issues and to explain why laboratory experiments are essential, stressing on our contribution. In particular, we now specify the ’uncertainties’ we refer to, and emphasize the role of experiments in an era where numerical simulations have become powerful. Indeed, the representation of turbulent (non linear) processes, which control heat, mass and energy transfer, remain unresolved in numerical models and poorly constrained by observations. Inaccurate parameterizations can therefore lead to biased predictions and overlook the existence of multiple equilibria in climate systems. We clarify that large scale realistic laboratory experiments (such as on the Coriolis Platform) allow these processes to be isolated under controlled conditions, with systematic variation of key parameters and direct measurement of small-scale, non-hydrostatic dynamics, such as they appear in sea straits or in downwelling areas in the ocean. This approach provides physical insight and scaling laws that cannot be obtained from numerical simulations alone, where turbulence must still be parameterized, nor from field observations, where forcing and boundary conditions cannot be independently controlled. The resulting datasets provide a unique basis for improving our understanding of small-scale mixing, turbulence, and nonlinear processes that underpin spontaneous climate variability. These experimental data are hence particularly valuable for improving numerical model predictions through more physically grounded representations of unresolved turbulent processes, training artificial intelligence (AI) approaches, and developing diagnostic tools for nonlinear processes in geophysical flows.

We have made the following changes at page 3

~~but uncertainties remain regarding internal hydraulics, instabilities, energy dissipation, and the generation and propagation of internal waves.~~

Despite significant advances in climate and ocean modelling, the accurate representation of turbulent processes governing vertical heat and mass transport remains a major challenge. Inadequate parameterizations can lead to biased simulations, missing key nonlinear feedbacks and multiple equilibria observed in geophysical flows (Danabasoglu et al. (2010); Rubino et al. (2020); Gacic et al. (2021); Pierini et al. (2022); Shi et al. (2022); Pirro et al. (2024)).

Observational [...] small-scale processes.

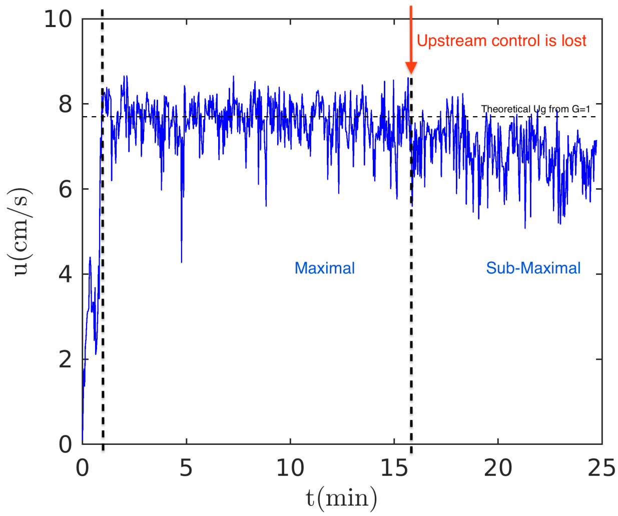
Large-scale laboratory experiments reproducing geophysical flows in dynamic similarity provide a complementary approach, allowing turbulent processes to be directly observed under controlled conditions, enabling independent variation of key parameters and measurement of small-scale, non-hydrostatic processes and their feedback on the mean flow by synoptic measurements, establishing robust scaling laws and physically grounded parameterizations.

Even if widely studied, several key processes remain under debate at the Strait of Gibraltar, including the persistence of hydraulic control at major topographic features (**?**Wesson and Gregg (1994); Bray et al. (1995); Pratt and Helfrich (2005); Pratt (2008); Sannino et al. (2009); Hilt et al. (2020); Roustan et al. (2023)), the observed detachment of the Mediterranean vein on the western flank of CS (Baines (2002); Roustan et al. (2024b)), the role of bottom boundary layers (Pratt (1986); Zhu and Lawrence (2000); Negretti et al. (2008, 2017)), the importance of 3D and non-hydrostatic effects (Zhu and Lawrence (2000); Pratt (2008); Sannino et al. (2009); Sánchez-Garrido et al. (2011); Sannino et al. (2014); Wirth (2025)), the quantification of turbulent dissipation rates and their unexpectedly high values measured during neap tides (Wesson and Gregg (1994); Roustan et al. (2024b)), and the generation mechanisms and properties of internal solitary waves observed in the Strait during neap tides (Roustan et al. (2024a))."

The paper does not really discuss the topic of maximal vs. submaximal exchange. Maximal exchange stems from the Stommel and Farmer (1952 and 1953 FMR) papers on overmixing in estuaries. It has turned out that estuaries are not good candidates for overmixing, but as clarified in the Armi and Farmer papers (different Farmer), the Strait of Gibraltar appears to lie close to a state of maximal exchange, with two hydraulic controls, one at CS and the other in the Tarrifa Narrows (TN). This picture is complicated by tides and I think the current thinking is that the mean exchange is close to maximal and is perhaps pushed intermittently into a maximal state. The present experiments are quite interesting: Figure 14 suggests that at no point in the tidal cycle is the flow is hydraulically critical at both CW and in the Tarrifa Narrows (TN). However, during much of the tidal cycle the flow appears to be critical at one or the other. Generally speaking, maximal conditions mean that signals from outside of the strait are unable to propagate into and through the straight. (They are blocked but supercritical flow at each end.) When the flow is critical at CS, only signals from the west are blocked, and when the flow is critical at TN, only signals from the east are blocked. So this would appear to be inconsistent with maximal exchange, but I'll let the authors weigh in on what they think. Some comment should be made on this historical debate.

We thank the reviewer for this valuable comment. Under lock-exchange initial conditions, following gate removal, the flow first undergoes a brief unsteady adjustment during which the velocity at CS increases with time. The system then enters a maximal exchange regime, so termed because the maximum possible transport through the strait is attained. Once the pressure gradient weakens and the interface in the Mediterranean layer drops below a critical depth, the exchange becomes sub-maximal. Following the seminal work of Farmer and Armi, numerous studies have addressed two-layer hydraulic exchange flows, including those by Greg Lawrence and others (**?**Zhu and Lawrence, 2000; Negretti et al., 2007; Prastowo et al., 2006; Fouli and Zhu, 2011).

During the experimental setup, we carefully monitored the velocity at CS over extended periods in order to capture its temporal evolution and to identify the stationary maximal exchange phase of the lock-exchange flow. This behavior is illustrated in the figure below for an experiment with higher density difference $\delta\rho = 25kgm^{-3}$, where we clearly observe the loss of upstream control to the east at $t \sim 16$ min, followed by a gradual decrease in velocity thereafter. Note that the velocity during the steady maximal exchange phase is equal to $\sqrt{g' H_s/4}$ from the condition $G = 1$ at CS.



We have described this in the section 3.2.1 when describing the baroclinic forcing and have added now the following sentence at line 279 to further strengthen the message:
"During the maximal exchange phase, the strait is bounded by two hydraulic controls, which isolate the flow at CS from the time-varying boundary conditions in the two adjacent basins and thereby maintain a stationary exchange."
In general, as the referee correctly notes, critical conditions in two-layer exchange flows are reached either at constrictions, at sill crests, or at both. When only a sill or only a restriction is present, a so-called virtual control forms upstream with respect to the lower-layer flow, preventing upstream information from propagating into the region between the two controls, while the topographic control ensures blockage at the downstream end. Furthermore, there seem to be additional controls to the west, at Espartel sill and West Espartel site, the latter possibly permanent, which block any information propagating from the west toward CS. This is a separate question that can be addressed using our measurements conducted in this specific region. Consequently, no information can be transmitted between these two control points within the strait. When the exchange transitions to a sub-maximal regime, the upstream control is lost.
In our transects, we do not capture the TN location (by the way, precise localization of TN remains challenging, as its reported position varies by several kilometers in the existing literature), and it is true that the computation of G does not suggest a clear control in the Tarifa Narrow in the purely baroclinic case when considering total averages, although it does when the tide is applied. Nevertheless, additional measurements (not presented here, as they were intended to investigate internal wave propagation) indicate that the interface in the Mediterranean basin descends with time. This outer-basin evolution does not influence the dynamics at CS, where control is maintained over most of the tidal cycle and where no external disturbances affect the flow. We do not know the exact location of the control point to the east, which also does not appear to be permanently present.
As noted by the referee, the presence of tidal forcing further complicates the system by allowing control points to vary spatially and temporally and to appear non-simultaneously, while CS remains dynamically isolated. Since perturbations propagate at the speed of internal waves, when a control emerges east of CS during inflow and the control at CS is lost, this information may not propagate fast enough, remaining trapped elsewhere without affecting the dynamics at CS. These considerations remain speculative at this stage. A detailed analysis is planned for future work using additional data covering the full transect from west of CS to the eastern exit of the Strait in the Alboran Sea, in both northern and southern transects. This will help elucidate the complex temporal and spatial system of hydraulic controls in the Strait.
We have also added the following paragraph at the end of section 4.2.4 as suggested by the referee:
"The question of the location and permanence of hydraulic controls in the Strait of Gibraltar is central to resolving the long-standing debate on whether the exchange flow is maximal or sub-maximal. If the Strait is bounded by two hydraulic controls, it is hydraulically isolated from both the Mediterranean Sea and the Atlantic Ocean. In that case, the exchange does not depend

on the pycnocline depth on either side of the Strait, but only on the local bathymetry and the density difference between the two water masses (Armi (1986); Armi and Farmer (1986); Lawrence (1993)). The Camarinal, Espartel, and West Espartel sills, as well as the Tarifa Narrows, are often cited as likely locations for such controls (Farmer and Armi (1988)).
These issues, however, are beyond the scope of the present paper and are therefore not discussed further here. Additional measurements of velocity and density, collected from west of CS to the eastern outer exit of the Strait, as well as over the Espartel and West Espartel sites and including both northern and southern channels, will allow a more comprehensive assessment of these questions and will be addressed in future work. "

A third issue is the reliance of 1D metrics to characterize a 3D flow, a practice that continues in spite of the fact that theory has moved beyond the 1D setting. A case in point is the composite Froude number, which as the authors acknowledge, tells one something about local hydraulic criticality and says something about the ability of locally generated disturbances to propagate upstream. The local Froude number does is not by itself an indication of hydraulic control of the exchange flow as a whole. For example, if a hydraulic control exists at the CS section, it is because the entire baroclinic exchange across that section is being choked by the topographic constriction. If the width of the strait there were made to contract, or the topography to become shallower, a disturbance would be generated that would be felt across the whole width of the strait, would propagate into the Mediterranean, and would result in a diminished exchange rate and a deeper pycnocline. (A terrific educational video could be created if this exercise were set up experimentally and filmed.) In any cases, statements such as "hydraulic control has been lost at CS in all three transects" (line 678) don't make much sense: hydraulic control is not a local phenomenon or a property of a section. It would be quite easy to use values from the three sections to at least estimate the bulk criticality of the flow at any cross sectional using the generalized composite Froude number that the authors refer to in their citation of Pratt (2008, JFM). These take into consideration the velocity distribution and stratification across the whole section. If this value dropped below unity, the authors would be justified in claiming that control at CS has been lost. These remarks also apply in the vertical. The separation between the pycnocline and the level of maximum shear in some locations is nicely documented (e.g. Fig. 12). The authors remark that this separation clouds the use of a composite Froude number (2-layer or 3-layer). In situations like this, the local hydraulic criticality of the flow can be assessed by calculating the continuous vertical modes of the stratified shear flow using the Taylor-Goldstein equation or one of its extensions. This has been done in places like the Bab al Mandeb and Hood Canal (see Pratt, et al. JPO, 2000 and Gregg and Pratt, JPO, 2010) where it is sometimes difficult to identify a distinct density interface. I'm not suggesting that the authors do this now since the present manuscript is rather long, and the exercise of sorting through the modes can be a bit of work, but perhaps something to think about for the future. At one point, Bill Smyth had a nice code available through his Oregon State webpage. It has a provision for including viscous and diffusive effect in case there are critical layers. The results are nice because they give you wave speeds and provide a stability analysis. In general, I think modern investigators need to get away from composite Froude number is situations where the stratification and velocity are not "layered" and look at these modes.

We thank the referee for this remark. We have already computed the composite Froude number along the three transects located at different cross-sections, as shown in figure 14 (and also in figures 4, 6, and those presented in the Appendix). These results show that, during inflow at CS, hydraulic control is lost contemporaneously at the three transects, although over different durations depending on both the transect location and the tidal strength. This led us to conclude that the control is lost across the entire cross-section at CS during these phases. In contrast, at other cross-sections located west of CS and associated with different topographic features in the Tangier basin, this simultaneous loss of control at all three transects is not observed during neap tides. For this reason, we do not further emphasize hydraulic control at those locations.
With regard to the referee's suggestion to compute the cross-strait variation of the composite Froude number following the methodology of Pratt 2008, we are concerned that this approach would require a number of strong assumptions regarding the cross-strait variations of both density and velocity. Given the limited spatial resolution of our dataset, namely, only three transects in the cross-strait direction and three transects in the horizontal planes, we do not believe that these assumptions would yield sufficiently reliable results.
Instead, we have performed additional computations of the composite Froude number using alternative definitions of the interface, in particular by considering the shear interface in addition to the isopycnal interface. We have also computed a three-layer composite Froude number, which is better suited to the structure of our data. The corresponding paragraph has been rewritten for improved clarity at lines 436, 445 and 724–746 (also in response to other referees' comments), and the additional Froude

number computations are now presented in Appendix D.
Furthermore, since the most reliable results, those that are consistent with the underlying assumptions used in the theoretical derivation of the composite Froude number, are obtained when using the local density difference rather than a constant initial density difference, we have replaced the original figures containing G with those computed using the local density difference. Further details are provided in Appendix D.
We also acknowledge, as pointed out by the referee, that alternative approaches based on stability analyses (e.g. via the Taylor–Goldstein equation) would better account for the situations where the stratification and velocity are not "layered". However, we chose not to extend the discussion further in the present paper, as it would go beyond its scope. These aspects are instead addressed in a forthcoming paper currently under submission, which focuses on the generation and propagation of internal solitary waves within the Strait (Tassigny et al., 2026).

Other Points

Figure 13 and discussion of processes east of CS: I'm curious about a certain aspect of the tidal flow in this part of the strait: namely the stripping of high potential vorticity water from the shallow shelf on the northern side, as seen in numerical simulations (see Dias, et al., JPO, 2025 Figs. 17 and 18). Water can be stripped away when the tide surges eastward and the advected plumes of high pv water lead to meanders in the Atlantic Jet. Do the authors see anything like this in the laboratory model?
Indeed, our experiments reveal the presence of coherent vorticity structures detaching from the shallow coastal regions on both the Spanish and Moroccan sides (as seen from the vorticity plotted in the horizontal planes in figures 5 and 7, which are subsequently advected eastward during inflow conditions. To quantify these features more robustly, we rely on complementary high-resolution numerical simulations (LES) performed by our collaborators at SHOM and LOPS using the non-hydrostatic CROCO model, which faithfully reproduces the laboratory experiments conducted on the Coriolis platform.
The numerical simulations initially faced significant challenges related to turbulence closure, bottom friction parameterization, and the representation of complex bathymetry. These issues have recently been addressed through careful calibration and systematic comparison with our experimental data. High-resolution simulations are currently underway, and we expect that these results will allow a more detailed and quantitative characterization of the processes highlighted by the referee.

Also, regarding the shallowing of the Atlantic layer along the north side of the Tarrifa Narrows (light yellow regions in Fig. 13). Armi and Farmer 1988, Fig. 11 show evidence that the pycnocline can intersect the surface, suggesting detachment of the Atlantic layer from the northern coast. Timmermans and Pratt (JPO, 2005) reproduce this using a rotating hydraulic model. Do the authors see this in the experiment?
Even though the upper Atlantic layer can become very shallow during spring tides and strong inflow conditions (on the order of ~ 12 m), we have never observed the interface intersecting the free surface.

I'm curious why are there no mention of Richardson numbers? Maybe mixing is discussed in the other paper. Yes, estimates of the Richardson numbers are provided and discussed in a forthcoming paper addressing the high-frequency dynamics around CS (Tassigny et al.). We further show there that, due to the strong influence of boundary layers, the Richardson number is not a reliable indicator of mixing and dissipation in this context...

The separation of the Mediterranean layer on the western flank of CS is nicely documented in Fig. 12. In many cases, the separation of a current from a rigid boundary is sensitive to details such as the boundary conditions imposed or the slope of the boundary. The fact that the slope in the experiment is magnified by a factor up to 10 may have some effect on the location of detachment (or lack thereof) of the descending overflow. Is this a concern?
In Section 4.2.3, we explain the detachment of the Mediterranean flow along the western flank of CS and provide dimensional arguments showing that the different aspect ratio affects this dynamics only at second order. Of course, the exact detachment location may vary in the real ocean compared to our physical model, primarily due to the topography smoothing we applied in the experiment. Nevertheless, comparison with the in situ data of Roustan et al. (2023) confirms that the detachment occurs at similar depths and distances from the sill crest.

line 32: outlet -> natural outlet. (don't forget the Suez canal) Yes, the referee is right. We added 'natural' as suggested.

lines 81-82. "The bottom topography is represented by the variable z=-hb(x,y)+h(x,y,t)... ". This makes it sound as though the bottom topography is time dependent. Some clarification or perhaps a definition sketch needed here.
The bottom topography is defined by the variable h_b, which does not depend on time but only on (x,y). But yes, saying that the bottom topography is represented by z is wrong. It is represented by h_b, whereas the vertical coordinate z is defined as specified. We have made the changes in the text.

Lines 126-127 suggest that the term containing the external Froude number in (3b) can be neglected, but this term is clearly much larger than the term containing the internal Froude number (Fr0 being small and Fr being O(1)). Perhaps the wording explanation is not clear?
Indeed, stating that the term is negligible may be misleading, as it could be interpreted as being small in amplitude. In fact, the term is neglected because its dynamics occur on time scales that are much shorter than those of the internal processes of interest, which are characterized by larger temporal scales. Variations in SSH are therefore treated as instantaneous, implying that the associated external dynamics are of higher order relative to the internal dynamics considered here. We have revised the sentence accordingly to avoid any misleading interpretation.
"However, since $Fr_0 \gg Fr$ and free-surface waves propagate much faster than the internal dynamics of interest, the term containing the external Froude number is of higher order and is therefore not further considered in the present analysis."

Fig. 1 caption. "optical measurements" is repeated. Also, the photo in the lower left frame is of poor quality. There is lots of glare coming of the black bottom of the tank, making it difficult to see the actual topography. I wonder if the photo can be retaken with different lighting, or perhaps the photo can be edited to reduce the glare?
We removed 'optical measurements' in the caption. The picture was included to provide a sense of the scale of the model. At this stage, it is not possible to take another photograph.

Line 253. Delete "of". Done.

*Line 271 "additional second" -> "second".*Done.

Line 278. "ADV" has not been explained yet. We added "(Acoustic Doppler Velocimetry, see section 3.3)".

*Line 285: "responding". -> "responding to"*Done.

Lines 299-306. I had trouble understanding the thrust of this paragraph. It sounds like some sort of adjustment was made in the plunger's amplitude to correct for some nonlinear process in the strait, but what is being corrected is unclear.
We had to adjust the amplitude of the plunger (which is related to the displaced volume of water) to achieve the right mean baroclinic velocities in the strait, which are slightly modified if only barotropic forcing is applied without the baroclinic exchange flow. We have modified the paragraph to make this clearer.

*Line 315 I was unable to locate the Bardoel et al. (2026) reference and therefore unable to view the map.*We are sorry for this unprecision, the paper is in preparation as well, now it is specified in the bibliography.

Line 390. "layer of zero horizontal mean velocity hu". Is hu the thickness of the layer, the depth of the layer, or just a label for the layer?
h_u is the depth of the layer at which the vertical shear of along strait velocity is maximum. We write clearly now in the text that we refer to the 'maximum vertical gradient of along-strait velocity'.

Line 394. "when the tide is applied west of CS" -> If I understand the meaning it would be better to write "west of CS when the tide is applied". Done.

Line 398. "lower" -> "decrease" Done.

Lines 420-421. I'm not surprised or worried that G2 does not quite get to unity in the regions where the flow visually appears to be supercritical. I have encountered the same issue in other straits where there is clearly a locally subcritcal-to-supercritical transition.
We have modified now the definition of the composite Froude number using a local g' as discussed above. Now, criticality is achieved in the mean in all three transects at CS for all tidal conditions.

Line 430 states that the highest TKE values are found at the sheared interface for pure baroclinic conditions, but when I look at the bottom left panel of Fig. 4 I don't see any elevated TKE at the interface. It looks instead that TKE decreases monotonically from the surface to the bottom. Am I looking at the wrong thing? Perhaps there is a tiny elevation of interfacial TKE to the east of CS.
The high TKE values at the interface are observed at the onset of the descent. Further downstream, the interface becomes indistinguishable due to the development of the hydraulic jump, where TKE values increase substantially. During spring tides, the hydraulic jump is displaced further downstream, allowing the interface to remain identifiable over longer distances toward the west. We have now clarified this point in the text.

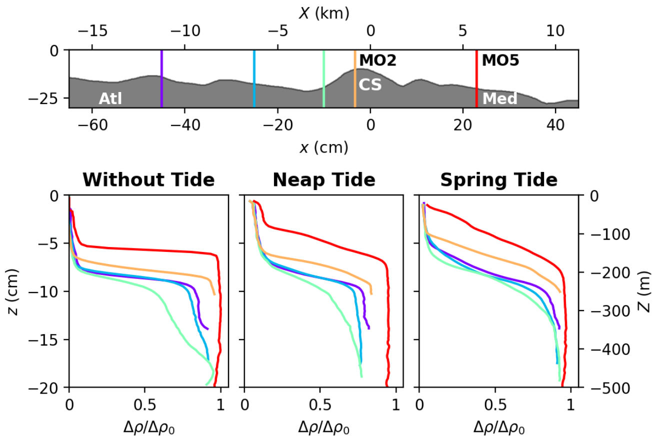
Fig. 6 caption. There is a reference to "vertically integrated volume transport and below salt flux" but I don't see either of those in the figure panels. It was an error referring to an old version of the figure. It has been removed.

Line 468. It is curious that G2 is maximum at CS and not immediately to the west of CS, where the Med layer spills down. Could this be due to the definition of the Med layer, which might include low-velocity contributions from the mixed wedge of water that lies between it and the Atlantic layer. One is probably justified in using a 1.5-layer Froude number for the overflowing layer in this case.
The maximum also extends to the west of the sill crest, reaching approximately 2.5 km beyond the crest. Its extent is sensitive to the choice of interface definition, as illustrated in Appendix E of the revised manuscript, where we compare different methods for computing the interface for the determination of the composite Froude number.

Line 495 claims that dilution is enhanced under neap tide conditions on both sides of the sill. Does this mean enhanced compared to spring tide conditions? (When I look at the 3rd from top, left panel in Fig. 8, I don't see much dilution in the water column east of CS, and the dilution to the west of CS does not seem to be any greater than in the lower left panel.) Yes, we mean enhanced compared to spring tide conditions. We agree with the referee that it is not easy to see this directly from the colorbar of the figure, but it is the case. Here we give vertical sections an both east and west sides of CS of the density anomaly for neap and spring tides, highlighting what we claim. We added this figure in the appendix C, and we added the following sentence at line 415:
"Direct comparison of the mean dimensionless ratio of density differences ρ^* at different position in the along-strait direction for the three tidal forcings is given in the appendix C".

Fig. 9. The caption is a little unclear here. The bottom panel shows results from the experiment, correct? Then what is HERCULES? Yes, that is. We have rephrased the caption. HERCULES is the acronym of the experimental campain.

Line 510. Why would "slightly elevated G2 to the east of the sill" suggest eastward propagation?
The previous formulation was incorrect. Our intention was to provide additional clarification regarding the nature of the supercritical regions. Specifically, an eastward supercritical region exists, in which internal waves can propagate only eastward, and a westward supercritical region, in which internal waves can propagate only westward. The hydraulic control located above the Camarinal sill therefore corresponds to a westward control, whereas the control forming to the east during inflow



corresponds to an eastward control. The distinction between these two types of control cannot be inferred from the composite Froude number alone, since $G^2 - 1 \propto c_+ c_-$ (Zhu and Lawrence (2000)), but instead requires knowledge of the sign of the characteristics $c_\pm$. Computing the characteristics explicitly is beyond the scope of the present paper; however, their sign can be inferred from the direction of the barotropic velocity. Consequently, the loss of control at the Camarinal sill during inflow relaxes the internal hydraulic jump and allows it to propagate eastward. We have therefore added the following sentence to clarify this point:

"The hydraulic control that forms above CS during the outflow phase is distinct from the control that develops during inflow to the east of the sill, in the direction permitted for internal wave propagation. The control at CS prevents the eastward propagation of internal waves, whereas the control located to the east blocks westward propagation. When the control at CS is subsequently relaxed, the internal hydraulic jump propagates eastward, potentially contributing to the generation of internal solitary waves. This behavior is more clearly illustrated in the slack-water dynamics presented below and in the Hovm"oller diagram of G^2 shown in Fig. 14. A detailed discussion of this process is provided in a separate study focusing on internal solitary wave generation (Tassigny et al., 2026)."

Line 525. By "negative" (that is, westward) do you actually mean "positive" (eastward)? Yes, we have made the signs corrections throughout the text.

Line 534. Has "dilution value" been formally defined? Following the suggestion of Referee 1, we have now defined the dimensionless density anomaly ratio $\rho^* \equiv \Delta\rho/\Delta\rho_0$.

Lines 537, 545 and 637. When you write "along the western flank" many readers will think you mean parallel to the flank (or along-isobath), just as "along coast" means parallel to the coast. I think you are describing the flow that is spilling down the western flank of the sill, and you might consider phrasing it that way. Done.

Lines 521 and 544 speak of a control and hydraulic jump to the west. It looks like these occur near x=-45. Is this the Spartel sill, and are these features due primarily to the presence of this sill? Is there some reason it (and Tariffa narrows) is not referred to by name?

As shown in Fig. 3b, the western end of the measured transects is located at the entrance of the Majuan Bank, but not yet at Spartel Sill. The eastern end of the transects lies close to, but does not reach, the Tarifa Narrows, which are located further east and extend so far we understood from the literature over several kilometers, e.g. there is not an exact location agreed from all the community as for Camarinal. Since no measurements are available there, this feature was not included in the original sketch. The Majuan Banc can be identified by the elevated plateau visible along the northern transect, although it is only partially captured at the western edge of the domain. The two topographic features referred to further downstream to the west of CS are not related to Spartel, they are present in the Tangier basin; however, we are not certain of their exact nomenclature.

In addition, combined velocity and density measurements were carried out in both the northern and southern channels around the Majuan Banc, the latter including Spartel, downstream toward West Espartel. This specific dataset will be presented and discussed in a forthcoming paper.

Line 598. "x" is the zonal coordinate, as in Fig. 12. When you say that x is the along-slope coordinate, this seems to be different. What does "along slope" mean? Without this understanding it is difficult to interpret eqs. (13) and (15). Are you interested in the zonal component of pressure gradient, or the component of pressure gradient tangent to the topography, or the x-component of pressure gradient tangent to topography? Precise mathematical definitions are nice.
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.

I can see the internal bore in Fig. 13, and I think I can see the time shift, with the change in the pycnocline depth first apparent at the southern transect and later at the northern transect.There is a later assertion that this time lag is independent of Kelvin wave dynamics, but if the eastward propagating bore has some characteristic of a Kelvin wave, the signal along the southern coast would tend to lead the signal along the northern coast. This was shown by Federov and Melville (JFM,1996), who developed a solution for a "Kelvin bore" whose leading edge curves away from the coast and would, in the case of Gibraltar, lead to the southeastward tilt cited on Line 667. Why is this not a possible explanation?
Following the comments of Referee 1, who was not convinced by the presence of a time shift, we attempted to quantify this effect more rigorously. In doing so, we found that the shift is not clearly identifiable for every tidal cycle and, moreover, that it is not consistently equal to 2 s. Since this result is therefore not robust and cannot be demonstrated convincingly in a quantitative manner, we decided to remove this discussion from the manuscript.

Well, I hope these remarks improve an already-nice paper. We thank the referee for these very pertinent remarks, which significantly helped us to improve the clarity and overall quality of the discussion in the manuscript.

References

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