Response to Reviewer 3 Comments

In this manuscript, the authors qualitatively assess the effects of tides on the temperature and salinity distributions in the Bay of Biscay through a comparison of two numerical experiments with and without tidal forcing. The manuscript is written plainly overall. However, it is not clear what kind of physical processes the authors are specifically interested in. Furthermore, the authors do not provide a deep insight into the dynamics responsible for the change brought about by the introduction of tides. As a result, the manuscript reads more like a technical report. I would therefore recommend that the authors resubmit the manuscript after major revision with more emphasis on the physics behind the computed results.

Response: We thank the reviewer for the constructive comments and suggestions aimed at improving the manuscript. In the revised manuscript, we provide details addressing the reviewer's comments.

Below are some examples of physical processes which could be pursued in more detail:

Point 1: The authors found that a strong SST front (Ushant front) is realistically reproduced only when tides are included in the numerical model. This may be an interesting result, but the authors do not at all examine why the front is created there and how it is maintained. The authors can, for example, examine the vertical cross-section of model-predicted vertical diffusivity, temperature, and circulation pattern to show the dynamical balance working at the front. They can also examine the so-called "Simpson and Hunter (1974) parameter" in terms of which the location of a tidal front is often discussed.

Response 1: We followed the reviewer's suggestions and we added a new Figure 6 in the revised manuscript to illustrate the location and dynamics of the Ushant tidal front, as well as the vertical water column properties in the area. We examined the Simpson-Hunter (SH) parameter near the entrance of the English Channel, comparing the model-predicted values with what is found in the literature (new lines: 227-238). A brief theoretical background of the SH parameter has been added in the Introduction and Methodology of the revised manuscript (new lines: 48-57; 145-157).

Point 2: The authors mention several times the importance of internal tides on the temperature and salinity distributions in the Bay of Biscay. They then need to examine more quantitatively where the internal tides are generated, how they propagate, and where they are finally dissipated to cause turbulent mixing. This can be achieved (at least partially) by carrying out a basic energetic analysis for both the barotropic and baroclinic fields.

Response 2: Following the reviewer's suggestion, we now investigate the areas where internal tides occur, by calculating the divergence flow as requested by the reviewer in another comment. For this, we have included a new Figure 8 illustrating the divergence field. We have also updated the text in the Methodology, and the Results sections including references (new lines: 129; 262-273).

The reviewer is correct that a more thorough investigation is needed to fully address the tidal energetics in the area. However, we think that, with the present tools at hand this is not possible at this stage of review and additional modelling tools/diagnostics are needed. The reviewer's suggestion could be the subject of a future work.

Point 3: The authors seem to suggest that internal tides interact with meso- and small-scale eddies in the open-ocean. This may be an interesting result, but if the authors want to emphasize this finding, they should clarify the physical mechanism of this interaction. For example, the eddy energy is most significantly enhanced at the wavelength of ~70–80 km through the interaction with internal tides (Figure 10b). Also, the meaoscale eddy activity is enhanced (suppressed) in the open-ocean (on the shelves) (Lines 202–204). Why are these?

Response 3: We carried out a wavenumber spectral analysis and we found an increase in the energy spectra at spatial scales ~70-80 km (Figure 10b). We qualitatively attributed this increase to the interaction of tides and eddies in the open-ocean, being more effective when their spatial scales coincide, in agreement also with references now added in the text (line: 390). As in point 2, the reviewer's suggestion could be the subject of a future work.

Below are other specific comments:

- Line 83: The vertical mixing scheme employed in the model should be briefly explained for both the bottom boundary layer and the interior ocean since vertical mixing is a key process in this study.

Response: We briefly explain the vertical mixing scheme employed in this study, based on the Generic Length Scale (GLS) turbulent closure scheme (new lines: 98-105).

- Section 2.2: Is the tidal motion induced only by open boundary conditions, or also by the tidal potential?

Response: The tidal motion in the model is induced by both tidal potential and open boundary conditions. This is now explained better in the revised manuscript (new lines: 110-111).

- Lines 165–167: More clear evidence is needed showing that this difference in the SST field is caused by the frontal displacement of river plumes.

Response: Evidence of freshwater river discharges modulating the SST on the shelves is discussed in the revised manuscript together with changes in the SSS fields (new lines: 209-210).

- Lines 168–170: How do internal tides cause SST cooling in this area? Are they breaking there?

Response: Following the reviewer's comment to investigate the divergence flow, we show that internal tides are breaking in the continental slope enhancing mixing in the shelves and leading to SST cooling, bringing cold bottom waters on the surface. References are also added in the revised manuscript (new lines: 212-215).

- Lines 173–175: In general, the SSS change can be caused either by "vertical mixing" or "horizontal advection". Although the authors seem to insist that the latter is more important, this is not demonstrated clearly.

Response: Following the reviewer's suggestion to include in our analysis the Simpson-Hunter parameter, we now investigate the location of the Ushant tidal front controlling the thermohaline properties at the entrance of the English Channel.

- Section 3.2: Why do the authors examine relative vorticity rather than, for example, horizontal divergence in which internal tides are expected to appear more clearly?

Response: We thank the reviewer for the helpful suggestion. We added a new Figure 8 and text to investigate divergence (new lines: 262-273). The signature of internal tides is now more clear in the revised manuscript.

- Lines 261–263: This sentence does not seem to be consistent with the sentence just below (Lines 264–266).

Response: We removed the "sentence just below" to avoid confusion.

- Lines 263–264, "most likely because of ...": I do not understand what the authors are trying to say here.

Response: We removed the phrase "most likely because of..." from the text.

- Lines 273–275: How can tidal "mixing" cause the SSH variability?

Response: We removed the phrase "tidal mixing" from the text.

- Line 320–322: This statement appears to be inconsistent with Figure 10b.

Response: We corrected the statement (line: 386).