Revision of manuscript “Tracing Suspended Sediment Fluxes using a glider: observations in a tidal shelf environment” *(EGUSPHERE-2024-4072)*

As a foreword, we would like to extend our sincere thanks to Dr. Jay Lee for his thorough revision of our manuscript, their positive feedback, and constructive comments, which have significantly enhanced the clarity and relevance of our study. We believe the improvements made will deepen the understanding of glider capabilities for monitoring hydro-sedimentary processes in shelf environments. Detailed responses to his comments are provided below (blue text), along with the corresponding changes made to the manuscript.

Please note that, in the revised manuscript, added text appears in magenta colour, and removed text appears in light grey (the same for the Figures and Tables).

Jay Lee, 21 Jun 2025

#General Comments

This manuscript presents a detailed and comprehensive study of hydro-sedimentary dynamics on the French Armorican shelf using a glider equipped with acoustic and optical sensors. The authors validate the measurements of currents and suspended particulate matter concentrations (SPMC) obtained from the instruments mounted on the glider against those from moored ADCPs and CTD-Rosette casts, offering valuable insights into the potential of autonomous platforms for sediment flux estimation.

The study is well-motivated, methodologically sound, and based on a robust dataset. The authors' efforts to calibrate acoustic and optical sensors for SPMC estimation and decompose current signals into barotropic and baroclinic parts are commendable. However, the manuscript suffers from occasional linguistic awkwardness, convoluted sentence structures, and inconsistent terminology, which make the text hard to follow. The scientific content is solid, but the presentation needs refinement. I recommend minor to moderate revision before acceptance.

#Specific Comments

Section 4.2 Barotropic velocity computation:

- The description of the barotropic current as a depth-average from −98 m to −52 m is somewhat unclear. Why were these bounds chosen? Are they related to sensor coverage, the tidal boundary layer thickness, or data quality limits?

Thank you for pointing out this. We clarified these boundaries as such in this Section 4.2: “The value 98 is the difference between 110 m (the typical ocean depth recorded) and 12 m (the typical thickness of the bottom boundary layer computed using the Soulsby (1983) formula). The value 52 m corresponds to the maximum range of the CIAM ADCP recorded at the end of the survey, as seen in Figure 7”.

Section 5.1.2 and Section 5.1.3:

- The manuscript states that the barotropic component accounts for nearly all of the total current (i.e., ratio ~1). In that case, is the subsequent discussion on the baroclinic component still necessary? It may help to clarify or simplify this section to avoid confusion and to emphasize the dominance of the barotropic signal

We thank the reviewer for this valuable comment on near-bed currents which helped clarify our description. Section 5.1 (Validation of glider currents) has been entirely rewritten: the Section on "baroclinic" currents has been removed and replaced (in the Section on the barotropic currents) with a detailed description of bottom boundary layer dynamics to account for the observed vertical patterns of the barotropic components.

Our winter observations show a vertically homogeneous water column which does not allow for the sustained generation or propagation of truly baroclinic modes. The near-bed discrepancies between platforms are explained by frictionally driven shear within the bottom boundary layer. The thickness of this layer, estimated at 9–14 m from the Soulsby, 1983 formulation, decreases in phase with the weakening barotropic current.

- The lower panel of Figure 8 shows that even in the mid-water column around 100 m depth, where glider data are expected to be most reliable (due to the highest overlap of measurements), the difference in baroclinic currents between glider and mooring still reaches up to ±6 cm/s. This implies that the relative error could be greater than (or equal to) the ground truth (mooring records).

Yes there are possibly large errors remaining sometimes. In the complete rewrite of this new Section 5.1.2 the values of these errors are mentioned explicitly ("enhanced differences are observed near the surface and bottom, reaching up to 0.06 m s-1)". They are located in particular near the bottom where there are less overlaps, but we agree that these overlaps don't fully explain these errors ("However, quality control (overlaps) alone cannot explain the observed differences, as they are more pronounced near the bottom than at the surface."). The origin of these errors is not fully explained.

- It seems the method used to derive the barotropic component is not clearly explained in the manuscript.

OK, this Section 4.2 has been updated accordingly, notably taking into account your previous comment about the values -52 and -98.

Section 5.2.2:

- The author claims that fine sediment was advected from coastal waters but would benefit from further supporting evidence. A reference to satellite imagery, model hindcasts, or literature describing coastal sediment plumes in the region would strengthen the claim.

Thank you for pointing out this possibly overstated claim, also consistent with the other reviewer's comment. We finally decided to remove the corresponding paragraph (of this Section 5.2.3).

Section 5.3.1:

- Line 456 attributes coarse surface signals to bubbles; this is unlikely below 20 m.

Yes, we agree that only gale force winds are able to drive bubbles down to 20 m. We removed this assumption in the rewritten Section 5.1.3 concerning PSD, assuming a possible presence of plankton instead.

- LISST peaks in the highest bin may be artifacts. If claiming biological origin, cite evidence.

Yes, effectively, the LISST data has been reprocessed: extreme size classes have been removed from the PSD following Mikkelsen et al., 2005, so that possible artefacts are being discarded. This processing has now been mentioned in Section 3.4 (CTD-Rosette instrumentation). Please also note that the corresponding Section (5.3.1) has been completely rewritten.

Section 5.3.2:

- The use of a 25-hour low-pass filter to extract the tidal signal may be insufficient, as the right panel of Figure 12 still shows a significant periodic pattern. To more accurately separate tidal and subtidal components, I recommend applying a harmonic analysis (e.g., Foreman, 1977; see https://www.sciencedirect.com/science/article/pii/0309170889900171).

Effectively a 25h running is not perfect for removing the tidal signal (Shirahata et al., 2016). So we performed a two-pass filter using both a 25h and 13h running average windows. The additional 13h window removes the remaining semidiurnal component (after applying the 25h filter). It appears to work well in our case: see the new Figure 15 displayed in place of the old Figure (Figure 14). Other filters are well known for removing the tidal signal but their running windows are too large for our case: 37h for Doodson's filter, 49h for Munk, 71h for Godin and Demerliac's ones. We would lose a large part of the beginning and end of our time series (which lasts about 96h in total). We also tested the harmonic analysis using the following tidal waves: M2, N2, S2, K2, K1, O1, P1, Q1. However the result appears noisy, which is expected in our case, because harmonic analysis expects "beautiful sinusoids" (to say with exact frequency) which can be relevant to time series of water elevation (but not too rich coastal signals), but is less relevant for time series of currents, and even less relevant we guess for time series of turbid fluxes.

#Interpretation and physical assumptions:

- Line 188: The thickness of the bottom boundary layer, stated as 9–14 m in the manuscript, might be quite large according to Soulsby (1983).

We double checked our computations and found the same results. The Figure R10 below gives you the full time series obtained. Here are some details of the computation for information. Formulae are based on Soulsby's 1997 book, page 49:

- The water column is homogeneous
- Latitude 47.26°N
- Main tidal wave M2 semidiurnal (used for sigma parameter)
- 8 full tidal cycles during the survey
- vertically integrated currents
- NB: Umin is positive because the tidal current vector rotates anti-clockwise (viewed from above) and the study area is located in the northern hemisphere.

  exemple cycle 2 CIAM: Umax=0.31m/s Umin=0.14m/s sigma=1.4052e-04 rad/s, f=1.4544*10^(-4)*sind(47.26) rad/s

  >> BBL thickness = 0.0038*((0.31*sigma-0.14*f)/(sigma^2 - f^2)) = 13 m

  → GV1: mean 11 m std 2m , CIAM: mean 12 m std 1 m

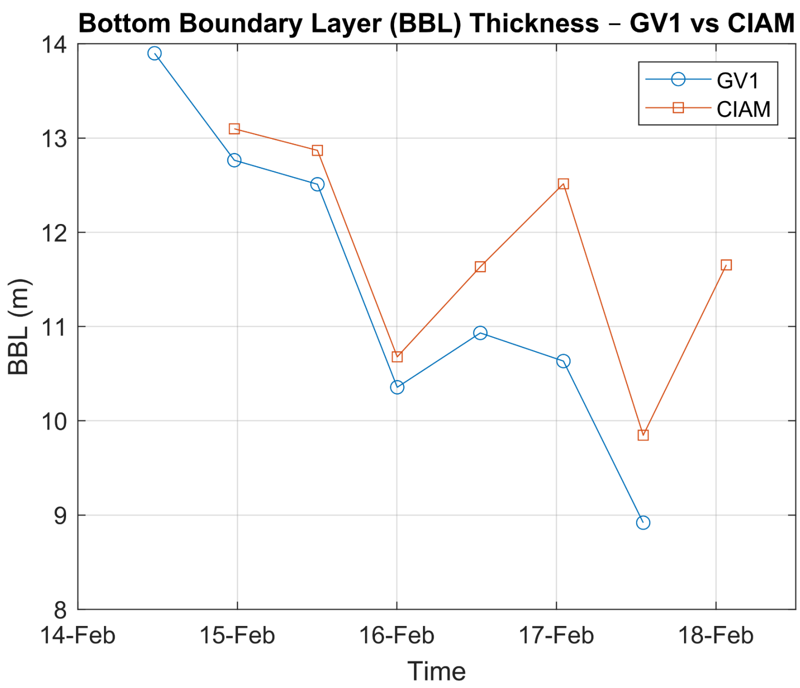

Fig. R10: BBL thickness in function of time, from the 2 ADCP moorings measurements.

- Eq. (3) presents only the u component of sediment flux. Please include the v component or clarify.

Done

- The statement that 1 MHz acoustic sensors are “sensitive to particles of ~1 mm” may be incorrect. Please check this paper (https://www.nortekgroup.com/assets/documents/Monitoring-sediment-concentration-with-acoustic-backscattering-instruments.pdf).

Thank you for this reference. Also, from the other reviewer’s comment, it appears that this sentence gave a misleadingly oversimplified interpretation. Finally the corresponding sentence has been removed from the text (near the beginning of Section 4.4).

- The sentence “especially in winter” (Line 300) should be elaborated in details.

Thank you for the question. We clarified by quoting instead “for particles of mineral origin” (at the beginning of Section 4.9).

- Figure 9 shows two high-SPMC events. If the authors interpret these as resulting from advection of suspended particles from coastal sources, it would strengthen the argument to include flow direction or progressive vector plots to illustrate the possible transport pathways.

Thank you again for this question which also is consistent with the other reviewer’s comment. It appears that our claim (advection is more likely than resuspension) cannot be fully supported. We finally decided to remove the corresponding paragraph (of this Section 5.2.3).

- Please ensure that Durand et al. (2018) supports the claims described in the manuscript, particularly the statement regarding riverine export of particles (does particle show similar grain size class?)

Indeed this reference does not well explain the origin of these particles (of size round 5 µm). The origin remains unknown. We put at the end of this Section 5.3.1 that “It could eventually be flocculi, although flocculi should have typical sizes between 10 to 20 µm (Lee et al., 2012)”.

#Specifications and formatting inconsistency:

- Please clarify the operating range of the LISST-100X Type C, which is stated as 2.5-500 µm by the manufacturer, while the manuscript claims 2-380 µm.

OK, the text has been updated in Section 3.4.

- Include schematics or detailed configuration for the mooring setup if it is accessible.

OK, a new Figure (Figure 3) has been included in the article describing the scientific instrumentation used.

- Ensure the consistency of linear regression results throughout the manuscript. Some plots show R^2 values while others do not. Also, the p-value should be included as the linear regression is used.

OK, R2 has been added in the text (Section 5.1.2 rewritten) corresponding to all panels of Figure 8. Also, p-values have been added for corresponding Figures 4, 5 and 8, and put in the captions. (typically 12~m) (typically 12~m) (typically 12~m) (typically 12~m) (typically 12~m)

- The term "single yo" should be defined for readers unfamiliar with glider-specific terminology

Thank you for the comment. We use "yo" as a standard glider term referring to one full dive–climb cycle. We have clarified this definition at its first occurrence in the manuscript and kept it throughout for consistency. The revised sentence now reads: "*To obtain the ocean contribution as a full water column profile, the LADCP shear method (Visbeck,2002) was applied to each individual dive–climb cycle (hereafter yo) of the glider.*"

- Why are Tables 4–7 placed after the references instead of being integrated into the main text? It would improve readability and context if these tables were inserted closer to where they are discussed.

Solved (Table 5 was a bit too long and caused a Latex strange formatting!).

- Table and figure formatting is inconsistent: some figure captions refer to left/right incorrectly (e.g., Figure 11), and colorbar units/locations vary. Table 4's caption appears below the table.

Corrected.

- Please standardize equation references (e.g., use consistently "Eq. (9)" rather than a mix of formats in the manuscript).

Corrected.

- Verify the time boundaries (magenta line) in Figure 5 for BU and VM periods. I thought the BU was between the 14th and 18th of February, and VM is after the 18th of February, according to Figure 1b.

Figures have been checked. We confirm that the date that separates BU and VM is 17 February 1320 UTC.

-Several redundant statements are present in the manuscript (e.g., Line 475: 'thanks to

Equation 3'). The authors are encouraged to revise for conciseness and eliminate unnecessary descriptions.

OK, large parts of the text have been rewritten accordingly.