

Dear Editor,

We have now completed the revision of our manuscript considering all the comments from the 2 referees as listed below. We provide you with a detailed answer point after point (in blue hereafter). Also note that, while working on these comments, we identified some typos we fixed, as we also rewrote some sections of the manuscript for clarity.

Best regards,

On behalf of the authors,

Dr Jean Roger

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## Reviewer 1

The work entitled “First 2D record of a tsunami by SWOT satellite: observation data and preliminary numerical simulation of the 19 May 2023 tsunami near the Loyalty Islands” focuses on satellite radar observation of a tsunami event on 19 May 2023. It is the first dynamic observation of a two-dimensional tsunami wave field. The satellite’s orbit allowed the observation of the wavefront propagating in both directions from the epicenter, about 1 h after the earthquake. I wonder why the authors call it 2D observation, when in fact it is a 3D observation because it includes the sea surface elevations (Sea Level Anomaly) within a two-dimensional domain. These observations are compared with numerical simulations performed with COMCOT model, showing generally good agreement with observations, although some differences reveal weak points of the simulations and indicate that there is room for future model improvements. Simulations show a small delay in the main peak of the tsunami wavefront and underestimated amplitudes, especially for the secondary waves in their propagation to the north. The results presented are relevant and highlight the usefulness of SWOT data to improve tsunami predictions. Some paragraphs and figures show inconsistencies that must be corrected (see attached file). The manuscript presents new data, and I recommend publication once the minor changes have been addressed. I hope my comments are useful to the authors and help improve the quality of the paper.

→ We thank the reviewer for his positive feedback and constructive comments, and we have considered all the comments one by one in the revision process.

### Specific comments:

Title and abstract: Why 2D record? It is an image of a 2D domain, but it includes the sea level anomaly, which is the third dimension, therefore, perhaps it could be called a 3D record?

→ Indeed, this a 2D record from a 2D altimeter, compared to the usual 1D record done with 1D altimeters. In fact, traditional nadir altimeters survey data along a single track providing 1D profiles of the sea surface height. SWOT is equipped with a KaRIn which surveys data across a wide swath providing 2D (bidimensional) “snapshots” of the oceanic surface (e.g., Peral et al., 2024). For clarity we propose to rewrite the title this way: “First mapping of a tsunami wavefield by SWOT satellite: observation data and preliminary numerical simulation of the 19 May 2023 tsunami near the Loyalty Islands.”

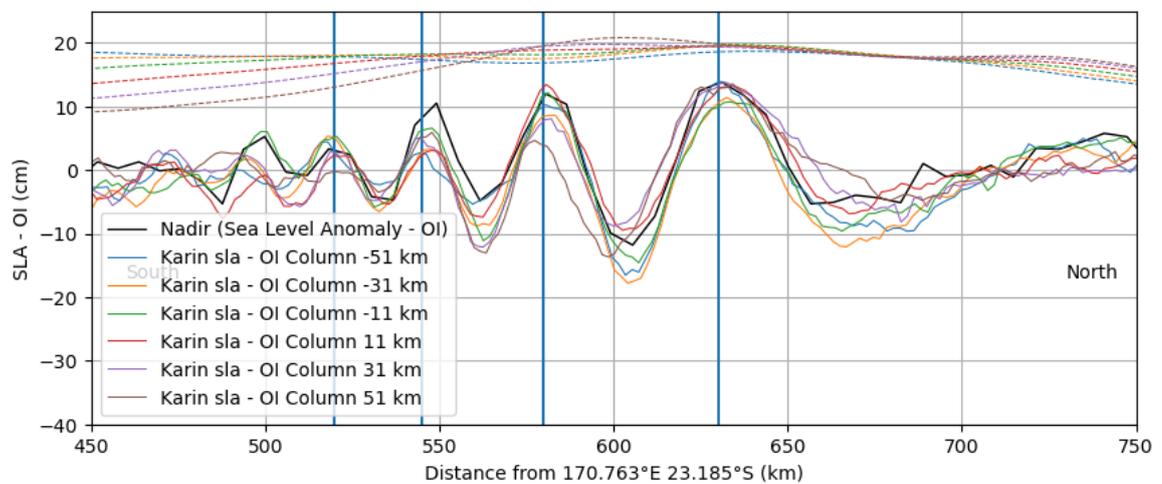
Lines 116 to 119: This paragraph starts “On 18 May 2023”.... “This corresponds to pass number 147 of cycle 525”. The tsunami occurred on 19 May, so I think this date is mistaken. And then, in line 118

it says “measurements from 19 May 2023 (pass 147 of cycle 524 ... and 525 ...)” this part must be clarified, as cycle 524 and 525 should correspond to different dates (are those 18 May and 19 May?).

- ➔ We corrected the numbers and cycles accordingly. First, you’re right, it starts with “On 19 May 2023...”. Then, the cycle numbers are right, but the date was wrong: it is now “measurements from the 18 May 2023 (pass 17 of cycle 524 before the tsunami...)”. Note that we identified a typo in the document with pass 147 being pass 17 in reality. In fact, there are only 28 passes per day in the considered period of time. This typo has been corrected everywhere in the manuscript.

Caption of Figure 3: “(the optimal interpolation OI is the MIOST)” Please include this “optimal interpolation” in figure 3b and explain it if necessary, as it is not clear in its current form.

- ➔ The optimal interpolation has been added to figure 3b and related details added to the caption, however, we are not sure it is relevant to show.



Updated caption : Figure 3: SWOT SLA and Power Spectral Density. (a) Zoom on SWOT SLA pass 17 on 19 May in 1D along track of the Nadir and KaRIn instruments for six across track distance, at 51, 31 and 11km either side of the Nadir position. The dashed lines represent the optimal interpolation removed for each profile, with respect to colors. (b) Power Spectral Density of SWOT SLA over the same area for 19 and 18 May (the optimal interpolation OI is the MIOST).

- ➔ It is important to remove mesoscale ocean variability (but also gyres and other potential large structures) to avoid confusing their signatures with tsunami signature. Mesoscale signature could be higher than tsunami amplitude (not the case for the signal at latitude of 18S°). Difference of signal with OI removed or not is visible on Figure 2 between 2b & 2c. This explanation is now added in the manuscript for better clarification.

Section 4: Please include an introduction paragraph between lines 176 and 177 indicating the contents of section 4.

- ➔ Section 4 is now introduced with the following sentence to describe its content: “Simulation results and measurement data are compared (§4.1) and the misfit between them is further discussed (§4.2) as well as the residual errors from SWOT signal (§4.3).”

Line 188: “tsunami propagation wavefield 62’ after the earthquake”. It indicates that figure 5 shows the simulation 62’ after the Te but in line 207 it says that the results are shown “at t=1h”. Clarify which time was used.

→ Indeed, the time on Figure 5 is 61 min (not 62 after verification) after the earthquake, and we will correct the time in the text.

Lines 206 and 207: “In five minutes, the tsunami would have travelled ~50 km”. Include a discussion about the lack of phase agreement in figure 4c. If the north wavefront was observed at 1h 2 min, the difference in phase of around 20 km (for the first wavefront) may be easily explained.

→ That’s true, we added a short sentence to reflect this estimation.

Section 4.1: In general, I think this section would be better structured if the results of figure 5 were shown first followed by the zoomed-in view of figure 4 on the north wavefront. Additionally, the results and analysis performed in figure 4 could be improved by showing the results of the COMCOT model at time 1h 2min which will likely achieve better agreement with the north wavefront observation.

→ We thank the reviewer for his suggestion. We have swapped figure 4 and 5 as it makes more sense effectively. The related text has also been reworked for clarity. After re-analysis of the simulation results, we confirm the simulation snapshot is already the one at 1h 2 min (Te + 65’) as suggested.

#### Technical corrections:

Figure 2 and lines 153 to 157: The caption of figure 2 refers to the different panels as left, center, and right, while the text in the next paragraph refers to 2a (line 153), 2b (line 154), b (line 156) and 2c (line 157). Please include the letters in figure 2, correct the caption, and line 156 accordingly.

→ The letters are now included in figure 2 as suggested, and the caption and line 156 has been modified accordingly.

Lines 170 and 173: References to figure 3b and 3c. It seems that they should refer to 3a and 3b, respectively. 3c does not exist.

→ The reviewer spotted an issue coming from a previous version of figure 3. We have updated the text, accordingly, keeping only mentions of figure 3a and 3b.

Figure 3: Please include the title of the X-axis in panel b.

→ The title has been added to the X-axis.

Line 180: It explains the results shown in figure 4c without introducing the contents of this panel c before. Please include a description of what the profiles of panel care.

→ The description of panel c has been added as requested. Note that this section of the manuscript has been reworked for clarity.  
Also, we noticed that the caption of Figure 4c does not explain what the vertical blue lines are: they symbolize the dashed circles visible on Figure 2. This has been added to the caption.

## Reviewer 2

This paper presents a very interesting study on the use of SWOT satellite data for detecting a tsunami event generated by the Mw 7.7 earthquake southeast of the Loyalty Islands (Southwest Pacific Ocean). The authors used the Level-3 Low Rate Sea Surface Height SWOT product, which is easily accessible to researchers, and compared it with numerical model simulations. Their analysis shows a strong consistency between the SWOT observations and the model results.

Tsunami is a type of event for which observations by satellites are rare and depends on “luck”, but when captured, they provide unique opportunities to study the event and its dynamics in ways that models alone might not allow (especially with a satellite like SWOT with high spatial resolution and advanced capabilities). This article therefore opens the door for more in-depth analyses and demonstrates the potential of SWOT studying such oceanic phenomena and events.

Although I am not a tsunami expert, I found that the literature review about tsunami detection from satellite altimeters is well presented in the introduction. The presentation of the SWOT altimeter and its capabilities is also concise and very clear.

→ The authors thank the referee for these encouraging remarks.

After checking the comments of the first reviewer, I find myself in agreement with their comments and I don't have further technical corrections to provide. I therefore propose a minor revision, and two additional comment/suggestions that could further improve the manuscript:

- To better quantify the time lag between the simulated main peak and the SWOT peak, I suggest providing a comparison spatially restricted between  $-19^{\circ}$  and  $-18^{\circ}$  latitude and using a simulation output closer in time to the SWOT overpass above this restricted area. This would make the comparison more precise.
- The objective of the paper is to highlight SWOT's ability to record tsunami. The numerical simulation was done only to confirm that SWOT recorded the tsunami wavefield, and we were expecting large discrepancies in amplitude and phase, which is finally not so bad. In fact, the tsunami generation model we are showing here is not the best and due to the complexity of the seismic source, being strongly non-double-couple, more work needs to be done on the source itself before going further in comparing numerical results and SWOT records (or example at DART locations). So, we could increase the sampling rate of the simulated snapshots to be able to choose a closer snapshot in time, but due to the source quality, it may not provide better comparison. However, we decided to show not only one, but two different snapshots on what now is figure 4: one corresponding to the journey of the satellite above the South Front, and one corresponding the its journey above the North Front.
- I also propose that the authors change the title of section 4 to something more generic like “Comparisons and discussion”, and change the title of section 4.1 to something like “Comparison between observations and model output” to better reflect the content and structure of the section.
- We agree with the referee suggestions to change the section titles and have updated them accordingly.