Responses to Review Anonymous Referee 1

Dear Reviewer anonymous referee 1,

Thank you for your observations regarding our preprint. Your suggestions helped to improve our manuscript. See below our responses marked in blue for each of your questions (marked in black).

The manuscript presents an interesting approach to deriving intertidal bathymetry from the waterline method through multispectral images, covering four (4) estuarine study areas on the east coast of Aotearoa New Zealand's North Island (Tauranga, Ohiwa, Maketu and Whitianga harbour). It represents a current thematic area, and it can be particularly useful to be applied in remote or inaccessible areas or where the bathymetric or cartographic data is very outdated. The main objectives of the study are to determine if multispectral images can be used to extract accurate intertidal bathymetric area and to assess the use of the SDB for hydrodynamic modelling of estuarine.

Good English level however the manuscript is not well-structured, quite confusing and the reader easily misses the main guidelines and the aim of the study. In section 1 (Introduction) is very difficult to establish a connection between the different ideas and paragraphs. A deeper revision of the state of the art is needed to bring the reader into the SDB theme and waterline method. The flow chart in chapter 2 is useful but does not really explain the methods used by the authors. Furthermore, the Methods Chapter establish that the main method was divided into 2 steps (1-SDB estimation and 2-Hydrodynamic modelling assessment) and that step 1 is also two methods for removing the bias, but a clear explanation of the methodology is not present in this section of the manuscript. A very short discussion and a shorter conclusions section are shown, where no clear main findings can be found. Modelling Storm surge is only referenced in the title of the manuscript.

The manuscript shows that a lot of work has been made, however, a big gap throughout the presented structure is noticed and the methodology used is not well described, creating a lot of misunderstanding between the methods applied and the different steps described by the authors. I, therefore, do not recommend the publication of this manuscript as it was presented. A major revision of the structure and methodology form is recommended.

Based on your revue and those provided by the other reviewers, we understand that we need to modify the structure of the paper because the current format is confusing. The new version has a deeper revision of the state of the art on SDB (adding new references and text to the introduction section). We have added a much clearer aim to the introduction, and worked on linking the methods to the aim in a much more clear and logical order. In the methods section, we added further explanations about the different methods implemented to remove the bias (i.e., statistical and dynamical methods). In the discussion and conclusion sections, we built further on the context provided in the new references added in the introduction part. Please note that we did not model an extreme storm surge, but we analyzed the maximum water level in all simulation scenarios and compared the outputs between scenarios using only surveyed bathymetry, only SDT, and mixed surveyed bathymetry combined with SDT. In terms of coastal flooding, the maximum water level is the main parameter studied in many places in the world, the water level is dominated by the tide. The discussion section has now 7 pages (previously 2 pages), and it is divided into several subsections as described in the text (4 subsections). Several new references were used (please see some of them in the end of the document).

My main critics are the following:

1. In the Introduction section the theme is not quite explained, and only part of the aim of the study is presented in the last sentence of the last paragraph. In this section is expected that the authors explain the reasons that have motivated this study, as well as what will be presented in the different sections of the entire manuscript.

We restructured the entire introduction section. We first said why elevation data are important in a coastal of flooding assessment (lines 24–46), stating that because of the limitation in the traditional methods, remote sensing techniques have been widely applied for topography and bathymetry data acquisition. Thus, we shown the main techniques regarding the satellite derived bathymetry (SDB) (lines 48–68) and topography (SDT) (lines 70–82). In the following lines (84–95), we said how cloud computation and satellite derived techniques have been allowed great progress in coastal science but there are only a few studies where numerical modelling and satellite derived elevation have been used in combination (which is the main motivation of our work). To finish the section, we clearly state our main and specific goals (lines 97–103) and the content of each section (lines 105–114).

2. The different figures do not follow a consistent presentation. The geographic coordinates in some cases are presented as latitude/longitude with no reference datum associated (Fig.2(a)); others as X/Y coordinates (km) WGS84/UTM60S (Figs. 2 (b), (c), 3 (a)) and even other examples as X/Y coordinates NZGD2000 (km).

All figures were changed to the same unit and datum (WGS84/UTM 60S). Only Figure2(a) could not be put in UTM because the package I'm using in python does not support it.

3. The same Figures, presented in different sections, have different SI unit references, like Fig.2 (b) and (c) are expressed in X/Z coordinate (km) and Figures S1 and S2 in X/Z coordinate (m) – show a lack of consistency.

All figures will be updated to use the same SI unit references (km).

7. The areas A, B, C and D depicted in Figure7 (central figure) are not quite perceptible, and the small figures (a1, a2, b1, b2, c1, c2, d1 and d2) do not have geographic coordinates associated, neither the scale factor.

We modified the colour scheme and sizes to make the Figure more readable. The ABCD areas are easily visible now. We also added the coordinates and a scale factor.

5. The profile lines drawn in Figure 9 (m1) are barely noticed. Maybe the authors could choose a different colour palette.

The colour and the thickness of the profile were modified to make it more legible.

6. In the text, the figures are not correctly cited, like Fig 2A (line 85); Fig 2B and 2C (line 88). In the Figures, the panels are mentioned with small letters (a, b and c), as well as in the figure capture.

We changed all the labelling to the format of the journal (always using small case letters).

7. The data access information at the reference links (lines 94-97) is missing.

We added the links.

8. I do not understand how the intertidal area is identified, the method is not well explained. Is used the tidal level at the time of the acquisition of each image? Or is used an average tidal range (tidal amplitude?) for all the images. Is also not clear the tidal level for each image, as depicted in Figure 2(d). All images are used to generate the intertidal area presented in Figure 3(a)?

The intertidal area is identified by calculating the standard deviation of NDWI over the whole collection of images, at each pixel (this is now better described in Section 2.2, using equation 1). Because the water level changes substantially through time (because the tide completely drains and inundates these areas), these areas are easily identified, in a collection of images, by the high standard deviation of NDWI. We use a threshold to find the areas with high standard deviation and define the 'intertidal' area as being the area of high NDWI standard deviation. We made this clearer now, by adding a chart flow of our waterline method (Figure 3) and reformulating some sentences in Sect. 2.2.

We also added information about the tide level, date, and time of the image acquisition for all images processed to generate the SDT in each estuary (Supplement C).

9. The threshold value used, and all the contour extraction method (lines 153-159) are quite confusing. And which values of threshold and water level were used for the other study areas, regarding that Figure 4 presents the water level and threshold values for each image. A table with this information, for all the different study areas, as supplementary information could be very useful.

We add extra infomation and a detailed chart flow of the waterline method (see response to comment #8).

10. What do you mean with the Stumpf-ratio method was applied for deeper areas (lines 164-165). The Stumpf ratio method (Stumpf et al., 2003) is not quite good for all different benthic areas and for very deeper areas. What was the maximum depth value which the authors have used this method?

We applied the Stumpf-ratio (now called ratio-log) for shallow waters (the areas within the estuary that are not intertidal zones or land) and intertidal zones. This is well described now by the chart flow in Figure 1. The motivations for using the ratio-log-SDB are explained in Sect. 2.3. More specifically in lines (219–221):

"... Originally, the ratio-log empirical approach has been used to derive shallow water bathymetry. However, because of the relative low turbidity of intertidal water in Tauranga Harbour, we hypothesized that the method could be suitable also for deriving topography on intertidal zones...."

We are aware of the method's limitations, and these are discussed in Sect. 4.3. Details on the implementation and result of the ratio-log SDT and SDB are shown in Supplement A.

11. It was not explained by the authors all the pre-processing steps applied to the multispectral images, such as sun glint correction (for example Hedley, J.D.; Harborne, A.R.; Mumby, P.J. Simple and robust removal of sun glint for mapping shallow-water benthos. Int. J. Environ. 2005, 113, 2107–2112). If this step was considered, it should be enunciated in the manuscript. The authors described that Level 2 image was used, with BOA values corrected for the effects of the top-atmosphere (lines 103-104), but it was not explained why they used these images rather Level 1 with atmospheric correction.

We used the level 2 images because they were already corrected for the effects of the top of the atmosphere, and we did not believe it necessary to undertake our own correction when the contrast between dry and wet cells was so clear. We did not apply a sun glint correction because the Otsu algorithm could detect the waterline well without this correction. We wrote an entire paragraph discussing the pre-processing of satellite images in Sect. 4.3, lines (519–526).

12. I can not understand if the evaluation of the model performance in section 2.4 is one of the results of this study. And if they are, why not present them in the results section? Lines 201-215 have a challenging interpretation.

We changed the structure of how we were describing the hydrodynamic model in the manuscript. We now explained why we are using the model (Sect. 2.4.) and said that we calibrated and validated it. But all results of the calibration and validation are shown in the supplement B.

13. Why an" Average" line in table 3. Does not make sense.

The "average" refers to the average of the error metrics over all the estuaries for the corresponding parameter (MAE, RMSE, R2). This has been made more clear.

14. Lines 234-239 should be included in the Discussion section, not here, where the results are presented.

We have deleted these lines in the revised manuscript. The discussion section was completely reformulated.

15. In the ESA Sentinel 2A images used as background in several figures are missing the data acquisition time and the water-level information (Figures 2, 7, S1, S2, S3, S5, S6, S7).

We added the information in all captions of the figures and also in Supplement C.

16. The authors cannot quantify as good or strongly correlated/related the R2 achievements (lines 242-245). Why R2=0.70 should be considered as strongly related? Once more the authors are discussing the presented results in the Results section, and it is a recurrent procedure throughout this section. Perhaps if the authors had previously described in section 1 the contents of each section, the reader could understand better the manuscript. The structure of each section is quite confusing.

Changes done.

We described the contents of each section in the introduction part (last paragraph) as required, and also added a chart-flow illustrating every step of the manuscript. Also, we moved the discussion related to the Figures 7 and 9 (now Figures 12 and 11) to the discussion section (Sect. 4.3 and Sect. 4.2, respectively)

17. Lines 274-278: R2 values assumption/classification (low/higher). And R2 is referred to as the coefficient of determination (line 276) and a coefficient of correlation (line 278) in the same paragraph. Is not coherent.

We changed this and all comparisons between the ratio-log and waterline method have been moved to the discussion part (Sect. 4.3), line 504.

18. The authors do not explain why the results and the application of the methodology were only presented for one study area (Tauranga Harbour). They are free to do it, even for editorial figures or pages restrictions, however, this fact should be mentioned and explained in the manuscript and the main results for each area should be resumed (table format perhaps) in the supplementary material section.

The results were presented for just one study site because of limitation number of pages/figures, and Tauranga Harbour is the only harbor for which we have a numerical model already validated. We have added a sentence to specifically say this “We only applied the modelling study to this estuary because it has already a previous model calibrated and validated for the bed roughness (following (Stewart, 2021)).” In the first paragraph of Section 2.4 (lines 230–231) . The results of the SDB estimates for each estuary are presented in the main manuscript, Sect. 3.1 (table 3, pg. 15) and in the supplement material (Figures S6, S7, and S8).

19. What is the spatial grid resolution value (line 298)? Is 20 m as assumed in line 336?

The grid resolution we applied here was 20 m. We have added this to the text (lines 234–235), Sect. 2.4.

20. Lines 313-316 are quite confusing. A better explanation is needed.

This entire section was moved to the discussion (Sect. 4.2) and rewritten to make it clearer.

21. The prediction of water level using the SDB is presented in section 3.4 for the 3 tide gauges (Omokoroa, Hairini and Oruamatua) (lines 323-331). The average error parameter presented is for each tide gauge and Figure 10 shows the average between all the tide gauges. Was this methodology that was used? I am confused.

We meant the average values between the three tide gauges records and not the average values for each tide gauge. We reformulated the sentence to make it clearer: “Figure 10 illustrates the average error parameters calculated when comparing the model output with the record of the three tide gauges. For a detailed assessment of each one of the gauges, please consult Supplement D” (lines 355–356).

22. Can I assume that, for lower tide values images, the presented methodology can not be used? Or only for the Stumpf ratio method application (SDB)? Lines 338-341. The Stumpf ratio method can not be directly applied to intertidal areas, exactly due to the image reflectance of the dry pixels (low water level images).

The model results for lower tide simulations showed the worst results. Thus, yes, it is not recommended to use SDB if the focus of the work is the processes occurring around the low tide, however for purposes of coastal flooding studies, usually, the highest values are of interest (as we show). The ratio-log-SDB is calculated using an image acquired at high tide. This is made more clear in the Methods (section 2.3).

23. What represents the rectangle-shaped figure in Figure 11? Survey bathymetry data or LIDAR data?

The bars represent the comparison between the in-situ observed water level at each of the 3 sites where there are water level sensors, and the hydrodynamic model output at those same sites, where the hydrodynamic model is run using 4 different bathymetries (S1, S2, S3, S4, are described in the text). We edited the figure and now all the labels and legends are more clear (now Figure 10).

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Falcão, A. P., Mazzolari, A., Gonçalves, A. B., Araújo, M. A. V. C., and Trigo-Teixeira, A.: Influence of elevation modelling on hydrodynamic simulations of a tidally- dominated estuary, J. Hydrol., 497, 152–164, https://doi.org/10.1016/j.jhydrol.2013.05.045, 2013.

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Geyman, E. C. and Maloof, A. C.: A Simple Method for Extracting Water Depth From Multispectral Satellite Imagery in Regions of Variable Bottom Type, Earth Sp. Sci., 6, 527–537, https://doi.org/10.1029/2018EA000539, 2019.

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Khojasteh, D., Hottinger, S., Felder, S., DeCesare, G., Heimhuber, V., Hanslow, D. J., andGlamore, W.: Estuarine tidal response to sea level rise: The significance of entrance restriction, Estuar. Coast. Shelf Sci., 244, 106941, https://doi.org/10.1016/j.ecss.2020.106941, 2020.

Khojasteh, D., Glamore, W., Heimhuber, V., andFelder, S.: Sea level rise impacts on estuarine dynamics: A review, Sci. Total Environ., 780, 146470, https://doi.org/10.1016/j.scitotenv.2021.146470, 2021.

Lee, Z., Carder, K. L., Mobley, C. D., Steward, R. G., andPatch, J. S.: Hyperspectral remote sensing for shallow waters I A semianalytical model, Appl. Opt., 37, 6329, https://doi.org/10.1364/ao.37.006329, 1998.

Liu, Y., Li, M., Zhou, M., Yang, K., andMao, L.: Quantitative analysis of the waterline method for topographical mapping of tidal flats: A case study in the dongsha sandbank, china, Remote Sens., 5, 6138–6158, https://doi.org/10.3390/rs5116138, 2013.

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Salameh, E., Frappart, F., Almar, R., Baptista, P., Heygster, G., Lubac, B., Raucoules, D., Almeida, L. P., Bergsma, E. W. J., Capo, S., DeMichele, M. D., Idier, D., Li, Z., Marieu, V., Poupardin, A., Silva, P. A., Turki, I., andLaignel, B.: Monitoring Beach Topography and Nearshore Bathymetry Using Spaceborne Remote Sensing: A Review, Remote Sens., 11, https://doi.org/10.3390/rs11192212, 2019.

Salameh, E., Frappart, F., Turki, I., and Laignel, B.: Intertidal topography mapping using the waterline method from Sentinel-1 & -2 images: The examples of Arcachon and Veys Bays in France, ISPRS J. Photogramm. Remote Sens., 163, 98–120, https://doi.org/10.1016/j.isprsjprs.2020.03.003, 2020.

Turner, I. L., Harley, M. D., Almar, R., and Bergsma, E. W. J.: Satellite optical imagery in Coastal Engineering, Coast. Eng., 167, 103919, https://doi.org/10.1016/j.coastaleng.2021.103919, 2021.

Responses to Review Anonymous Referee 2

Dear Reviewer anonymous referee 2,

Thank you for your observations regarding our preprint. Your suggestions helped to improve our manuscript. See below our responses marked in blue for each of your main concerns (marked in black).

1- The manuscript presents a novel methodology for deriving intertidal bathymetry for four estuaries in New Zealand (Tauranga, Ohiwa, Maketu and Whitianga harbour) characterized by a complex morphology. I find this thematic interesting, as it allows to update and improve the boundary conditions of regional numerical models. However, I think the structure and writing of the manuscript require further work to reflect all the work done. The manuscript needs a better use of English, a restructuring of the chapters and above all to emphasize the purpose of the work as well as the authors' motivation and innovations. Therefore, I do not recommend the publication of this manuscript as submitted. This review is critical, nonetheless the authors have the potential to have a great manuscript and I would like to encourage them in their progress.

We appreciate that you found our manuscript interesting, despite the problems regarding its structure. The inadequate structure is a problem highlighted by all the 3 reviewers.

We restructured the entire introduction section (and manuscript as well). We first said why elevation data are important in a coastal flooding assessment (lines 23–46), stating that because of the limitation in the traditional methods, remote sensing techniques have been widely applied for topography and bathymetry data acquisition. Thus, we showed the main techniques regarding satellite-derived bathymetry (SDB) (lines 47–68) and topography (SDT) (lines 69–81). In the following lines (83–94), we said how cloud computation and satellite-derived techniques have allowed great progress in coastal science but there are just a few studies where numerical modelling and satellite-derived elevation have been used in combination (which is the main motivation of our work). To finish the section, we clearly state our main and specific goals (lines 96–102) and the content of each section (lines 104–113).

Please note also that we have added new references, some examples are at the end of the document.

2 - I mainly concern of the reasoning and the reading flow, which is quite confusing and the reader can easily miss the guidelines of the study. Section 1 does not clearly show the developments achieved by the scientific community, the relevance of the chosen methodology and, above all, the authors' motivations.

Please see the answer to comment 1. In addition, we re-structured the entire paper, and we added a chart-flow to illustrate all the processes and content of the sections (Figure1).
The motivation is now clearly stated in lines 89-94:
"... Despite the vast and growing application of SDB and SDT methods to coastal science and engineering (Turner et al., 2021), it is not yet clear whether the accuracy of the resulting estimates is suitable for modelling extreme water levels in coastal areas (e.g., estuaries and bays). Only limited studies exist relating to SDB and SDT and numerical modelling — generally aimed at using the model to assign the waterline height (Fitton et al., 2021; Khan et al., 2019; Salameh et al., 2020). For instance, Mason et al. (2010) used SDT to calibrate a morphodynamical model..."

3 - Section 2 is very long and presents too many technical concepts, and even results, that is hard to follow how they were implemented. The study area should be expanded with a description of the main processes describing water level dynamic, since the work's title mentions storm surge modelling.
We have described the physical particularities of the study areas in Sect. 2.1 (lines 119–126):
"...The studied sites have micro-tidal regimes — the spring tidal range varies between 1.4 m to 1.9 m within estuaries – and the equinoctial spring tides combined with severe storm surges drive the extreme sea levels (Rueda et al., 2019; Stephens et al., 2020). In New Zealand, the surges caused by storms usually add < 0.5 m to the water level, but the maximum surge ever registered was 2.29 m (Stephens et al., 2020). In Tauranga Harbour, the maximum storm-driven surge ever recorded is equal to 0.88 m (Stephens et al., 2020), and the tide can be attenuated by 10% to 17% (M2 component) when the tidal wave propagates through the estuary (Tay et al., 2013). The water level inside the study site estuaries is not considered to be substantially affected by the action of waves (i.e., wave set-up) because all of them are enclosed coastal lagoons

with restricted entrances.”

The discussion section was increased to 7 pages, divided in 4 subsections :
4.1 Our proposed waterline method for deriving topography from space-borne images and its limitations.
4.2 Our proposed correction methods for waterline-SDTs.
4.3 Comparison between the waterline-method and ratio-log for intertidal zones.
4.4 Hydrodynamic modelling assessment

In the first three discussion subsections we addressed our first and second specific objectives, discussing our main findings and limitations when applying the waterline and ratio-log methods, including suggestion for improvements. In the fourth part, we addressed our third objective, discussing the challenges of using SDTs and SDBs in modelling extreme water levels within a complex-morphology estuary.

The Section 2 is now divided in 7 parts to better discretize each one of the methods used in the work. Also Figures 1 (pg. 5) and Figure 3 (pg. 8) bring a general guidance throughout the structure of the manuscript.

4- Results and figures in Section 3 present a lack of consistency of SI units, authors should homogenize them. I think criteria presented in Fig. 3 and 4 are unclear and need a deeper discussion. Errors should be accompanied by their percentage for better interpretation. Unfortunately, the color map chosen for Fig. 7 and 9 is not good for presenting such significant results. The conclusions are extremely short and not summarise the reasoning of the work.

We changed the figures to consistent SI units. We rewrote Section 2.2 with more details on the waterline method and added Figure 3 (pg. 8), a general chart flow of the method. We also start the section by stating all the stages of the method and use Figure 3 as a guide for the reading (lines 161–163):

“Our proposed framework to generate the SDT in intertidal zones using the waterline method (waterline-SDT) is composed of 4 stages, as illustrated in Figure 3: stage 1 is to query an image collection, stage 2 is to identify the intertidal zone, stage 3 is to determine the waterline position and height, and stage 4 is to post-process results. First, we acquired an image collection for each estuary through the Google Earth Engine application (Gorelick et al., 2017) using the Google Colaboratory environment….”

We changed the colour maps of Figures 7 and 9 (now Figures 12 and 11, respectively). We added several more references and pages to the discussion, as mentioned in the previous question 3. We also made the Conclusions more clear (although avoiding repeating the discussion).

Information about the range of depth in the LiDAR data for the intertidal zone is shown with general results for each estuary in Table 3 (pg.15). Thus, it is possible to directly infer how the waterline-SDT is performing related to the LiDAR. We also showed the digital elevation model (DEM) errors in the same table.

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Almeida, L. P., Efraim de Oliveira, I., Lyra, R., Scaranto Dazzi, R. L., Martins, V. G., and Henrique da Fontoura Klein, A.: Coastal Analyst System from Space Imagery Engine (CASSIE): Shoreline management module, Environ. Model. Softw., 140, 105033, https://doi.org/10.1016/j.envsoft.2021.105033, 2021.

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Responses to Review Anonymous Referee 3

Dear Reviewer anonymous referee 3,

Thank you for your observations regarding our preprint. Your suggestions helped to improve our manuscript. See below our responses marked in blue for each of your main concerns (marked in black).

1. I have read the paper entitled "Modelling tides and storm surge using intertidal bathymetry derived from the waterline method applied to multispectral satellite images" by Costa et al. The study aims to determine whether satellite imagery can be used to extract accurate intertidal bathymetric data; and assess the use of the SDB for hydrodynamic modelling of estuaries. The paper is interesting, and one can see that quite a lot of effort, based on the complex methodology, was put into it. However, the manuscript is quite confusing making it difficult to understand. Part of the problem I had was regarding the use of the term SDB to represent extracted shorelines when the term is coined to deriving bathymetry. The study seems to have its merits but needs a complete re-structure. I found results difficult to understand.

We are pleased that the reviewer finds our manuscript interesting. We are aware (based on your review and others) that the structure of the paper is not ideal, and the structure makes the paper confusing to read. We re-structured the paper according to the reviewers' comments. With respect to the methods, we realized that the reviewer misunderstood us, and we have rewritten them to make ourselves more clear. The identification of the waterline position is done as part of the waterline method for estimating bathymetric data in the intertidal zone. We have now changed our definitions, and in the intertidal region we use "satellite-derived topography" and in shallow water we use "satellite-derived bathymetry". Following past work, we use the movement of shoreline over the intertidal as a way of detecting the topography. We made the methods more clear by also adding a chart-flow about the paper structure (Figure 1, pg.5) and our framework for the waterline method (Figure 3, pg.8).

2. Because it presents lots of technical concepts, I'd divide this paper in two manuscripts and focus on convincing the reader that waterline extraction can be useful to derive intertidal DEM in NZ and leave the SDB and modelling approach for another opportunity. A short discussion is also

presented in this submitted version for such complex topic. Therefore, a major revision or complete new submission is recommended

We understand that the novelty of our work is the part that discusses the assessment of hydrodynamic modelling. Thus, removing this part from our manuscript would affect the paper's novelty. The SDB techniques shown here are well discussed in the literature and our paper is not aiming to improve any of them in a significant way. We believe that now with the improvement of the discussion part (several new references and a new discussion section of 7 pages) and the clearer methodology will make the topic less complex, and the two parts of the study fit together more naturally.

3. It is not clear to me why one would embark on a shoreline extraction method to create an intertidal model, when one could use SDB (Stumpf and others) to obtain bathymetry, especially where white water/waves are absent.

The Stumpf method has the disadvantage of not working well in deep areas or when the turbidity of the water is great. The waterline technique has been proven to be efficient in intertidal zones, performing better than the Stumpf method in several studies, when compared. Also the Stumpf method requires in situ depth measurements, but the waterline method does not. We compared both methods in the discussion section (Sect. 4.3.).

4. I found the introduction a quite confusing as it mixed two different uses of satellite. One to derive bathymetry and the other to derive shoreline positions. This is carried out from the Abstract to Introduction to the other parts of the text, and therefore I suggest a complete rewrite of these sections.

Shoreline positions are used as part of the method for estimating bathymetric data through satellite images. We hope that this is more clear in the revised manuscript. We re-structured the paper, now we make it very clear that the satellite images are used to derive topographic data (SDT) (Sect. 2.2) and bathymetric data (SDB) (Sect. 2.3). We also added new references in the introduction (please see references at the end of the present document)

Some specific points below:

5. L9 - I'd suggest to modify text to make better use of the acronym – Satellite-derived bathymetry (SDB) which obviously differ from "use of satellite images to estimate bathymetry"

We changed "use of satellite images to estimate bathymetry" to "use of satellite images to derive bathymetry" (now line10).

6. L11 four instead of 4. Same in L16

Done.

7. L18 The use of the satellite derived bathymetry in hydrodynamic models does not result in significant differences in terms of water levels, when compared with the scenario modelled using surveyed bathymetry. This seems a big claim to me considering that the method was only used in microtidal settings and NDWI performance in macrotidal places can be more complicate due to the larger wet areas. Sea grass bed areas appears also to be an issue.

We agree. We modified the text to "For Tauranga Harbour, the use of SDT and SDB in hydrodynamic models does not result in significant differences in predicting high water levels when compared with the scenario modelled using surveyed bathymetry." (lines 20–21)

8. LN23- what about meteorological tides? They seem quite important for predicting floods

We agree that these are important. However, we decided to focus on the astronomical tides as many places around the world have extreme astronomical tides (spring and equinoctial tides) as the main driver for coastal flooding. In addition, storm surges in New Zealand are quite low (average of 0.5 m). We added a paragraph in Sect. 2.1, to better describe the physical processes involved in the flooding events and the particularities of New Zealand locations (lines 119–126):

" The studied sites have micro-tidal regimes — the spring tidal range varies between 1.4 m to 1.9 m within estuaries – and the equinoctial spring tides combined with severe storm surges drive the extreme sea levels (Rueda et al., 2019; Stephens et al., 2020). In New Zealand, the surges caused by storms usually add < 0.5 m to the water level, but the maximum surge ever registered was 2.29 m (Stephens et al., 2020). In Tauranga Harbour, the maximum storm-driven surge ever recorded is equal to 0.88 m (Stephens et al., 2020), and the tide can be attenuated by 10% to 17% (M2 component) when the tidal wave propagates through the estuary (Tay et al., 2013). The water level inside the study site estuaries is not considered to be substantially affected by the action of waves (i.e., wave set-up) because all of them are enclosed coastal lagoons with restricted entrances."

Also, we discussed the effect of waves and storm surges in Sect. 4.4, last paragraph (lines 560–568):

"Although the differences in the resulting water level between the SDT, SDB, and surveyed bathymetry simulation scenarios show that satellite techniques compare well, our simulations were only conducted in one estuary, albeit a large and relatively complex estuary — where the astronomical spring tides are the main driver for estuarine flooding. Therefore, studies are required in sites with different physical conditions would be useful to validate the use of SDT and SDB more broadly. For instance, estuaries where the storm surge is the main driver for flooding; or/and exposed estuaries where the wave forces can increase the water level (i.e., wave set-up) (e.g., Bertin et al. (2019)). Furthermore, modelling studies focusing on understanding whether or not the use of SDT and SDB properly represent the tide-surge interactions within the estuary are encouraged, due to the importance of the topic in water level modelling (Spicer et al., 2019; Wankang et al., 2019; Zheng et al., 2020), especially in the context of sea level rise."

9. LN24 no hyphen in sea level

We have changed this throughout

10. LN25- to my knowledge atmospheric pressure is the one driving storm surges along the coast, fluvial discharge definitely adds to it, but it is the difference in pressure that elevates the water level

We agree and have changed the sentence. It is now written as follows: "In practice, predicting flooding depends on understanding the contribution from the astronomical tide, storm surge, wave run-up, changes in the sea level and, in some cases, the fluvial discharge and vertical land motion." (lines 25–27)

11. Ln34 the references following this sentence should focus on SDB and not shoreline "To overcome these issues, efforts have centred on using spaceborn remote sensing (RS) techniques (Bishop-Taylor et al., 2019; Bué et al., 2020; Caballero and Stumpf, 2019)," should be replaced

We re-wrote the entire manuscript dividing the techniques into satellite-derived topography (SDT) and satellite-derived Bathymetry (SDB). We used the waterline and ratio-log methods to derive topography data (as described in sections 2.2 and 2.3, respectively). We have also applied the ratio-log method for deriving bathymetry in shallow waters (Sect. 2.3). We explained in detail all the paper's structure in Figure 1 (pg.5) and the waterline framework in Figure 3 (pg.8). Details on the implementation of the ratio-log method are given in Supplement A.

12. LN41 Again I don't think the Bishop et al. ref is appropriate here, as their article addresses shoreline and not bathymetry

We agree and we have rephrased the reference to Bishop et al.: "…and extensive intertidal areas (Bishop-Taylor et al., 2019; Fitton et al., 2021)…" (lines 49–50)

13. LN 42 rewrite "use a radiometric approach, which uses the property that different wavelengths are attenuated to varying degrees in the water column".

We re-wrote all of the introduction. Now the equivalent sentence is located in lines 52–53: "Most of methods are developed around the process of light attenuation through the water column, and fall into two approaches".

14. LN55 detecting the land-water boundary has nothing to do with deriving bathymetry with satellites. The shoreline is the interface not the morphology of the seafloor. Bishop et al., didn't derive bathymetry. They derived intertidal DEM, linking terrestrial and bathy datasets

We have clarified the differences between techniques to derive topography and bathymetry data (SDT and SDB, respectively), and made it much clearer how the shoreline can be used to map topography. We are using both datasets to create DEM that can be use as the input in hydrodynamic modelling.

15. LN57-59 only here I start to get a feeling of why you are talking about SDB, but even after that I think that you are creating a DEM of a few mts- depending on the local amplitude- instead of lets say shallow water bathymetry

We agree, and we made modifications in all paper structure as answered in the previous questions in this document.

16. LN71 bathymetry. You are talking about creating a DEM based on shoreline positions. Some of these positions will be above tide datum. Does that make bathymetry or topography?

We separated in intertidal topography (Sect.2.2 and Sect. 2.3) and shallow water bathymetry (Sect. 2.3 and Supplement A).

17. LN77 2 main steps (Fig. 1).

Ok. Changes are done. (line104)

18. LN78 two instead of 2

Ok. Changes are done (line104).

19. LN88 the intertidal zone is easily distinguished by the colour of sand accentuating reflectance in the near infrared band – This sentence seems out of context or needs some further clarification as Fig 2 is not a false colour image.

Ok. We re-wrote the sentence to: "The extent of the tidal flats is evident in Tauranga Harbour by comparing low (e.g., Figure 2B) and high (e.g., Figure 2C) tide satellite images." (lines 127–129)

20. LN89 Associated with tidal flats, mangrove forest can be observed in all the studied estuaries. Where can I observe mangrove and seagrass banks? Modify text

Ok. The sentence was modified to: "Mangrove forests can be observed in all the estuaries and seagrass banks are visible in Maketū, Ōhiwa and Tauranga Harbours (the latter was studied in Ha et al. (2020)). Detailed images of the intertidal zones in Tauranga Harbour and its seagrass banks and mangroves can be seen in Figure S3." (lines 130–131).

21. Ln92 I get confused here." For the implementation of SDB techniques, only tidal levels and imagery are needed. We used additional in situ bathymetric data to validate the SDB." Do you need bathy or not for implementing SDB?

The entire section was rewritten, but the corresponding sentence is now: "Imagery, tidal levels and topography data (e.g., LiDAR) were acquired to implement and validate the SDT techniques." (line 133)

22. LN114 t seems to me that some of the derived shoreline elevations cannot be considered bathymetry and this is part why I don't like the use of the term for this shoreline extractions. Some of the elevations will be above MSL. Can you still call it bathymetry? Shouldn't be topography then?

As mentioned above, we changed the term to satellite-derived topography (SDT).

23. LN140 you are using NDWI to define the intertidal area. Please explain know the 9 images for Tauranga or the 7 for Ohiwa are capturing the full extent of the intertidal area. Where they acquired during the lowest-highest tidal range?

You can see the full extension of the range captured by looking at Figure 7 for Tauranga harbour and in the supplementary material for the other locations. Indeed, the number of images limits the method, this aspect is discussed in Section 4.1 A greater number of images would make for smaller errors (lines 413–417):

" ...Furthermore, having enough images to characterise the morphology of the study site is also a limiting factor in the waterline method, as pointed out by several studies (Salameh et al., 2019; Liu et al., 2013). Our results are also clearly affected by the number of images in our collection. For instance, we observed gaps between different waterlines, where no topographic data could be derived, shown in Fig. 7 (Sect. 3.2). Although we have used Sentinel-2 images acquired every five days, they are often not useable due to cloud coverage. ..."

24. LN156 Again- colloquialism

We removed this (lines 203–204).

25. LN157 (Fig. 4)

Change done. All figures are referred as Figure.

26. LN158 once the waterline for a given image is identified, a height value is assigned to it accordingly to the corresponding tide level observed at the closest tide gauge (Omokoroa for the Tauranga Harbour case study, Fig. 2D). I see 4 gauges in the estuary. What's the rationale for choosing Omokoa? Oraumatua seems way closer to let's say Rangataua Bay! How do you account for the tidal lag? The level at the entrance is different than at the head.

We choose to use just Omokoroa tide observations to show the difference that could occur when the tidal lag is not accounted for, particularly when the tide gauge is situated a long way from the region of interest. In section 2.4 and 3.3 the use of what we called "dynamical correction" is applied to correct the effects of tide lag (where a local tide is used).

27. Ln202 hyphen mean-sea level

Change done. (line 240)

28. LN259 Tauranga Harbour's waterline-derived SDB (primary SDB)- Sometimes I get really lost- What's primary SDB and how it differs from the other SDBs. Please explain

We are no longer using this term, which makes the manuscript easier to follow.

29. Fig 8 font size too small

Change done.

30. Fig 9 Why are the coordinates in this map in NZGD? This figure needs to be improved. The colour scheme does not allow differentiation btw gauges and lines. It has 2 contradicting legends showing water lines as points and lines. Where are the LiDAR and the dynamic waterlines? Are they only shown in profile? I bit confusing to understand

Changes are done. We change the legend and the colour scheme, please see Figure 11. Lidar data are represented in black, only in profile.

LN323 The simulation scenarios showed that it is possible to obtain similar, or even enhanced water level predictions, by using the SDB rather than the surveyed bathymetry – I'm a bit lost here. My understanding is that we need bathymetry to do SDB! At least I had to use a few lines in the past.

We now are referring to SDT (satellite-derived topography) and SDB (satellite-derived bathymetry). Scenarios S1, S2, S3 and S4 explore different DEMs in hydrodynamic modelling. It is true that the SDB needs some calibration data, and we have noted this (lines 353–355).

31. LN 383 Bathymetric data are fundamental for solving the hydrodynamic equations in shallow water – This seems obvious, isn't?

We removed this sentence from the manuscript.

32. LN6that it? A 1.5 pg long discussion, for such a complex paper?

We expanded the discussion to 7 pages divided in 4 subsections. We hope the rewritten paper brings a richer discussion now.

NEW ADDED REFERENCES

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