

General Comments:

This manuscript brings together two critical components that need to be integrated moving forward in science, models and local stakeholder input/citizen science. However, the authors spend too much time emphasizing the utility of the model while glossing over the groundtruthing that is necessary. The manuscript should be restructured to focus on methodology integration, data scarce regions, and softening quantitative nature of sea floor depths. These (Conclusion point 4 and softening of point 1) need to be the forefront of this manuscript. Very little time was spent reviewing any results from the stakeholder survey (though they were referenced offhand many times). If this was the only issue, I would recommend major revision but in light of many other issues listed below primarily apparent sloppiness/rush in preparation of the manuscript force me to reject it outright. Much more care needs to be taken in laying out and distinguishing the findings of the study and the speculation/recommendations (of which there are many). I think this study has great value and I appreciate the hard work that went into producing this comprehensive work.

Specific comments:

- Is 0.10 m really within the tolerance of the methodology? No mention is made about data resolution and error. How can it be a quantitative study without these? Regardless they need to be stated.
- The introduction is significantly lacking in introducing the problem on data scarce regions and the reason remote sensing datasets are needed. Further, it does not recognize the limitations of this methodology and the importance of ground truthing data. The beginning of the Materials and Methods (2.1) really belongs in the introduction.
- The results are littered with discussion comments and speculation which confuses the reader.
- **Need to be much more careful with saying the numbers are real. Models are estimates only. Spent too much time talking about how they were quantitative, without proper groundtruthing (which is lacking here) they aren't. See Section 2.3.2 and Line 101 comments below**
- Turbidity data need to be more clearly linked with rainfall data and phytoplankton blooms (not covered at all). Additionally, many more scenes are needed to accurately determine turbidity. Greatly concerned with community acceptance of "the Nechad algorithm". All results should read potential or estimated turbidity.
- The sea floor bathymetry model is not groundtruthed enough. As it stands it currently has only one LIDAR dataset and 2 navigational charts. Again change all results to "Estimated absolute change" in depth.
- Figure and table captions are all too vague
- Manual shoreline delineation renders much of the purpose of this study (to work quickly and effectively in data scarce areas) null as it is extremely time intensive. Didn't consider tidal variations in analysis, which is critical.
- Unclear how local stakeholder knowledge was integrated, need many more specifics in the methods, results, and discussion.
- Discussion is mixed in with the results

- Sloppy: Many grammatical, spelling, mapping, significant figures, and figure reference errors.
- Too many sections, often with little purpose, one or two sentences.
- Discussion does not reference enough material in the paper (no figures referenced) and lightly mentions other work. Most points are too much of a reach and need to be softened.
- This is not a sediment budget nor did this study accomplish any sediment budgeting. Thus, the authors cannot claim that the sediment budget is out of balance.

Line by Line Comments:

Line 30: it is important to note that local knowledge is also often flawed, exaggerated or misinformed.

Figure 2: These maps do not have north arrows and in the third panel the scale bar is not visible due to the dark background of the ocean. Moving the pop out to the bottom (red box) and right (blue box) will allow the reader to see the study area more clearly. The legend doesn't state anything about the region or even what country it is. There are no labels denoting the Gulf of California or the Pacific on inset 1 or 2. More work is needed here.

Line 43: This is referencing Figure 1 but it should reference Figure 2 (study area)

Line 45-46: has there been no other work to characterize the hydrodynamics and bay chemistry beyond this 1955 work? Have there not been changes since that time due to increased peak and normal flows due to development? Increases in water temperature? This is a long time ago to be assuming no change in a bay that the authors are arguing is changing.

Line 46-47: where does this data come from (normally not important but given that the manuscript is about bathymetry source and data collected is important here.

Line 47-48: This sentence must be cited unless this study determined they were key courses and become active during these times.

Line 51: figure reference is again incorrect.

Figure 3: given that this is a main figure in the manuscript, it needs to be cited. Where is this data coming from? As mentioned above if these streams are the primary driver of sediment into the bay indicate how this has been proven. Are there no city drainage systems that provide sediment input? Sheetwash? What are the sediment loads of these streams? Further the figure caption is too spartan, what is purpose of the dredge and ship locations? At this point the reader is not able to process this information.

Lines 59-65: Were phytoplankton blooms considered and the destruction of the thermocline in this bay? The authors mentioned above that it was "moderately stratified" which implies that the more nutrient rich bottom water could assist in a phytoplankton bloom? This should be addressed. It is not clear at this point that the chosen scenes were linked with rainfall data, it should be stated here.

Table 1: is the revisit frequency relevant given that the authors are only selecting 2 days per year?

Line 66: What pansharpening methodology was used?

Line 67-68: Is this a proven methodology? It cannot be stated that they provide the necessary detail to detect fine scale erosion without proof that this has in fact been accomplished in the past.

Section 2.3.1: In regards to the author's model previously published in geomatics referenced in this manuscript (Lamas-Torres et. al 2025). Coastal lidar is unreliable especially in turbid waters. Lamas-Torres et. al, states that the model met the threshold for coastal mapping and navigational safety for their study site on Buck Island. Neither coastal mapping nor navigational safety require a resolution of sub centimeter which is what the authors claim in the manuscript under review in order to report a depth reduction of 10 cm and reporting values as such (though no data resolution or error is given for the methodology). Lamas-Torres et. al, 2025 then again used navigational chart data to validate their model in Mexico (as is done in this manuscript under review). I would argue much more groundtruthing is necessary before claiming this model is accurate, specifically to a multibeam bathymetry system. There are many regions with this data available so it would not be difficult to do so. Groundtruthing to a single LIDAR dataset and a navigational chart (which are not designed to be at the resolution that this manuscript is reporting or seeking) is not enough to then claim the model is valid for all coastal mapping applications and is high resolution.

Line 101-102: Authors cannot ground truth to a navigational chart, these are provided for navigational safety not scientific research. In order to prove either of these in shallow coastal zones further ground truthing is needed from either manual or remote sensing methods performed on the water. Further this chart is from 2018 which is both before and after the data being analyzed. How can the authors ground truth a model to a navigational chart from 2018 with imagery from 2016 and 2019 respectively? The authors need to ground truth at least to the year that the data was collected especially given the claims that almost 10 cm of depth reduction has occurred during this time period.

Section 2.3.2: Is the Nechad algorithm widely used for this application? A quick google scholar search show 37 citations for this paper. I see that Nechad has several papers detailing this algorithm but is it the accepted among the community? I see no papers that reference it as "The Nechad algorithm".

Section 2.3.3: more information is needed regarding how the shoreline was delineated manually, what cues were used to establish the shoreline?

Line 119-120: if it is considered the most accurate then cite the sources that utilize it or say it here. Did the authors account for tidal variation? What tide was the shoreline digitized? Depending on the tidal range, this is incredibly important.

Line 121: epoch is a much larger period of geologic time than what is being discussed here.

Line 136: This needs to be connected to rainfall data to be meaningful. This is done later so it should be alluded to here.

Line 145: one of my concerns here is the reliance on 'citizen science' like local news sources which have no obligation to report accurate information. This should be noted here. This data should be

displayed here or shown as to how it connects to the imagery data investigated. Possibly as a add to Table 2.

Section 2.5: what was the goal and what (in general) did the authors ask them?

Line 154: where is El Manglito? This should be on the location figure.

Line 155: what do these local organizations do?

Section 3.1: It is confusing to refer to it as a depth reduction. It is either deposition (accretion) or erosion as previously stated. I would be consistent here. I would also list it as estimated absolute change as these are not groundtruthed or real numbers.

Figure 4: It might be helpful to have the outline of the land here. 1.5 meters of deposition in this region is the high? Over 5 years this seems too high. Also it seems interesting that there is a circular spot of erosion in the center of the channel apparently approaching 2 meters, that is not how we typically see channel erosion. It would be useful to the note the minimum and maximum in the results section. Absolute is spelled with an 'e' at the end not absolut (which is a vodka brand). The quality of the legend is very low I'm not sure what is causing that but it should be remedied. Again the figure caption is severely lacking.

Table 4: This is where I would list the min and max values. The affected zones should be delineated in some way on a figure. What is the difference between and area and a sector? This can be on the location figure if necessary. Again use "assumed" erosion and deposition.

Lines 165-169 and Table 5: are the authors correlating this data with tidal fluctuations? Are the authors truly saying the data is accurate to a thousandth of a centimeter? That is what is shown here, fix the significant figures as the authors have not discussed resolution or error to show the data to be accurate to that level. List the error range on this table as well.

Line 178: figure not figures

Figure 7: I would make the land tan and the water blue so that it is more evident which is which. Erosion is spelled wrong.

Line 183: what is cibnor? Not mentioned in any other sentence, thus it isn't worth abbreviating. Where is the city's waterfront? These places must be highlighted on the study area map.

Line 184: Is there data on beach renourishment? If so it should be included here. This reads a lot like a discussion point vs. results.

Line 187: estimated turbidity, this is not actual measurement turbidity. Comparing landsat to sentinel data, this should be noted here.

Line 190-191: It also could indicate increased nutrient load, change in temperature, increased wave action or several others. This is speculation and should be in the discussion if anywhere.

Figure 8: the coast should be delineated here the gray implies that there is land in the channel (which there isn't). Need to explain what the gray is on this figure. Again the figure caption is extremely lacking.

Line 203: The authors are listing the reduction as 0.2832 m, this is misleading as it is reporting an accuracy to the thousandth of a centimeter. Please review significant figures. Also list it as estimated.

Figure 9: dates are listed in day/month/year and month/day/year. Be consistent.

Line 208: this estimated bit is how it should be referred to throughout the paper.

Line 210-211: was there rainfall in between this time? Did the river stage increase or decrease during this time?

Figure 10: see figure 8 comments.

Section 3.4.2 why didn't the authors estimate shoreline change for Hurricane Newton?

Line 219: SAA significant figures

Line 221: SAA significant figures, now up to hundred thousandth of a centimeter?

Figure 12: need to list where there is no data or no change northwestern coastline. This is like Figure 7 except there it had a tan background and here it is blue (blue is better). Consistency of figures with scale bars, north arrows, and coordinates is a real issue here.

Line 227: estimated turbidity

Figure 13: see figure 8 comments estimated turbidity

Line 240: reference relevant figure.

Line 260: who reported them lost? Some clarity here would be more useful (also on line 253)

Sections 3.6.1 and 3.6.2: Most of these I would recommend apply to this study, either the language must be softened significantly or more groundtruthing must be done. Either way, this list is gigantic and very expensive with a lot of technical expertise required, I would suggest making it more reasonable and realistic to accomplish or perhaps have the ideal wishlist (what is listed here) and then a "plan B" that is more realistic and doable.

Section 3.6.3 and 3.6.4: These are good recommendations but they need more information backing them up I think to be made here. Also is that the purpose of the manuscript? The introduction needs to be strengthened to demonstrate this if so. Then the results from the community would need further elaboration or citations at a minimum to make these actionable. They also have a slight AI generated feel to them.

Line 340: Now the authors are listing the significant figures at tenth of a centimeter resolution. What is the error and resolution for these measurements?

Section 4.1: calling it "shallowing" or "filling in" and then referring to a sediment budget is misleading. The authors have mentioned erosion and deposition several times. The most straightforward path has 2 possibilities. 1. Refer to everything as "estimated erosion or deposition" 2. Refer to everything as "reduction or increase in estimated depth". This way the guesswork is taken out of it. The authors are either estimating depth and assuming erosion and deposition or

estimating erosion and deposition. I personally think they should follow option 2 as they will get less feedback from the sedimentology community. Either way the authors need to be more clear that this is modeled data that isn't thoroughly validated.

Lines 342-346: If the authors are seeing a steady 30 cm of sediment every 5 years ("0.28 m shallowing") how is this representative solely of redistributed sediments? If the flows are more channelized this will increase peak flows which ultimately will deliver greater amounts of sediment from the surrounding watershed while possibly less from the channels if they have been paved in some way.

Line 346 – 348: Where are the vessels in the northwest? Where is the reported boat activity? How does this correlate with deposition or erosion and differ from the observed trend? All observed vessels (on Figure 2) are in the northeast region of the bay. Beyond the error here there needs to be more information beyond a summary of the local input if the authors are going to use it in the discussion. This is a reach to connect these two things. I would remove.

Line 350: "the **possible** export of fine grained sediments"

Line 358-359: broken reference.

Section 4.2: Just because an environment is erosional doesn't mean there is an imbalance. In fact as the authors pointed out there is accretion happening. Coastal environments are dynamic and thus cannot be relied upon to produce or move sediment in a consistent manner annually. Again renourishment is mentioned, when did it occur and where? Hardening where? When? This should be covered if the authors are going to hypothesize on sources.

Line 365-367: The discharge was likely lower during Lorena not less channelized. Intense rainfall is less likely to infiltrate and thus creates sheetwash. The scenarios the authors presented is not emblematic solely of rainfall, storm surge, wave action, and tidal currents strongly drive these processes (as alluded to earlier in this manuscript).

Line 369-370: this is only a possibility and must be presented as such.

Line 373: This is the first mention of altered currents. Where and when? This could provide insight into the trends the authors observed but they don't discuss them, why not?

Section 4.4: much more could be done here. This component needs to be addressed further and in more detail.

Section 4.5: This would be much stronger if the authors were to strengthen 4.5 and be more clear about their estimates as a model. Then it would be more clear to the reader that regular groundtruthed surveys are necessary. As it stands it does not seem this is the case as the authors have already done the work and could continue to do it without this additional effort or cost. If this is the author's final point then they need to point to this manuscript as preliminary and just exposing the issues at hand to then spark a larger project, instead of the "quantitative" proof as displayed here, if you will.

Line 391: I still argue that not only have the authors not investigated the sediment budget, they don't know if the system is unbalanced as they haven't established pre-anthropogenic conditions (nor is it clear if the community would even prefer those conditions).

Section 5: Point 2 needs to be softened it has not been proven. Point 3 needs to be altered as discussed above. I agree strongly with Point 4 and this needs to be emphasized more in this manuscript.

Line 406: why is this the first mention of Arroyo El Cajoncito? How does it play into this point?