JOURNAL OF COASTAL AND HYDRAULIC STRUCTURES

Review and rebuttal of the paper

Laboratory experiments on the propagation of bores induced by offshore soliton forcing over reef flats

Yamaguchi, N. et al.

Editor handling the paper: Bas Hofland

The reviewers remain anonymous.

Review Round 1

Reviewer A:

Recommendation: Revisions Required

The manuscript presents a study on the influence of reef systems on the propagation of bores onshore. The manuscript is generally well-structured, there are a couple of minor grammatical points that are included in the attached documents. The study evidently suffers from some challenges related to the measurement of velocity and scale effects. However, I think the authors have done a good job at identifying those limitations and responsibly do not make any definitive claims on the effectiveness of reef systems. I think, as a scientific contribution, it is useful and can lead to important future research.

Please review the comments (grouped into Major and Minor).

The authors would like to thank the reviewer for the time spent reviewing the manuscript and the constructive criticism provided. All comments are answered point-by-point below (in red).

Major:

1) Figure 6: It would maybe be helpful to have the reef crest position in the figures as I find the figures a little hard to read. Normalizing the velocities by the theoretical celerity might also be interesting to show how closely they match the actual values.

   The following note was added to the caption: "Note that the reef crest is situated 550 cm from the wave generator (see Figure 1)."

2) Table 3 and 4: I am not a fan of the tables; they are difficult to read and draw conclusions from. I would tend towards applying a normalization to the data in the figures to show the same conclusions that the authors point to in the text.

   The authors feel that these Tables are already a form of normalization in a broader sense, as they express a ratio of velocity between two different gauges. They also allow the reader to easily inspect the values for each experimental case, and normalizing them could thus obscure a simple interpretation. The authors hope that the reviewer will accept this point of view.

3) Discussion: the discussion seems a little too focused on the limitations of the study. I would have liked to see it go a little bit more towards the objectives brought up in the introduction to tie that manuscript together a little more.

   The authors agree with the reviewer that this is less than ideal. However, the paper went through several rounds of review, with each reviewer asking for certain limitations to be clearly acknowledged. At >9000 words, this is already a very long manuscript. Nevertheless, the authors included the following sentence to tie things better with the introduction: *"Essentially, the experiments indicate that both the length of the reef-flat and the bottom roughness play an important role in modifying wave hydrodynamics after breaking over the reef edge, resulting in lower wave heights and reduced front velocities landward of the reef."*

Minor:

- Line 88: I am not sure the relevance of the Roeber et al. (2010) quote to the rest of this paragraph.

  This reference was deleted from the manuscript, as it is indeed not so relevant.

- Figure 5: Maybe a few less images would make it easier to see the process. As is, the figures are a little small.

  The authors agree, and instead have chosen to enlarge the images. Hopefully this makes it easier to understand them.

- Line 207 – 212: this sentence is a little confusing and there seems to be an extra bracket. The sentence after also seems to have an extra bracket.

  Apologies for this. An extra bracket was placed, which hopefully helps to clarify the sentence.

- Line 215: Was the bore tip used to estimate the cases where it was not a bore (or the crest)?

  The authors rephrased the sentence to clarify this, which now reads: *"although in the present work the concept of wave front velocity is used; i.e. the velocity of the particles at the very start of the wave, irrespective of whether it is a bore or solitary wave."*

Reviewer B:

Recommendation: Major Revision

This manuscript presents laboratory experiments investigating the influence of coral reef flats of different surface roughness and lengths on solitary wave propagation. The research question is relevant to coastal engineering and tsunami risk reduction, as it addresses the potential protective role of reef structures as nature-based solutions. The experimental program is extensive, with 48 test cases, and the description of the setup is detailed.

However, the study suffers from several important methodological and interpretational weaknesses. These include incomplete scaling considerations, inconsistencies in velocity measurement methodology, lack of statistical treatment of data, and conclusions that overstate what the results can reasonably support.

The authors would like to thank the reviewer for the time spent reviewing the manuscript and the constructive criticism provided. All comments are answered point-by-point below (in red)

Topic relevance

The paper addresses how coral reef flats affect wave transformation, which is clearly relevant to coastal engineering and disaster risk reduction. The connection to nature-based solutions for tsunami mitigation is timely and of practical interest to the field.

The authors are glad that the reviewer sees the relevance of the work, in connection to nature-based solutions.

Novelty

The work provides additional laboratory data on solitary waves propagating over reef-like beds of varying roughness and length. While useful, the degree of novelty is somewhat limited, since similar flume experiments and numerical studies already exist (e.g., Buckley et al., Quironga & Cheung, Fernando et al.). The contribution would be stronger if the paper framed itself explicitly as a study of *roughness effects in laboratory solitary waves*, rather than suggesting it directly models tsunami conditions.

Agreed. The following sentence was added to the introduction: *"However, to date no comparative study has been conducted on how different types of coral colonies –represented by different surface roughnesses- and reef lengths might affect solitary wave hydrodynamics in a flume experiments."*

Literature review

The review is wide-ranging and cites many relevant sources. However, it reads mainly as a list of studies rather than a critical synthesis. It would benefit from clarifying why earlier findings differ, and where exactly this paper fills the gap. That would help substantiate the claim of novelty.

The authors agree that the literature needs a bit of tightening, and have tried to streamline it slightly. A sentence has been added to more clearly outline the gap:

"However, to date no comparative study has been conducted on how different types of coral colonies –represented by different surface roughnesses- and reef lengths might affect solitary wave hydrodynamics in a flume experiments."

Abstract and objectives

The abstract and introduction explain the broad purpose of the work, but the objectives are not always sharply distinguished from general background. These could be stated more directly—for example: *to quantify how reef flat length and surface roughness affect solitary wave height reduction in a flume experiment*.

As stated earlier, the following sentence was added to the introduction: *"However, to date no comparative study has been conducted on how different types of coral colonies –represented by different surface roughnesses- and reef lengths might affect solitary wave hydrodynamics in a flume experiments."*

Methodology and mathematics

The experimental description is detailed, but there is a mismatch between methods and results. The manuscript devotes considerable space to electromagnetic current meters (ECM) and PIV, yet both are later dismissed as unusable. In the end, the analysis relies entirely on wave front tracking. If this is the real method, it should be presented upfront as such, with proper uncertainty analysis.

The authors agree, and have added the following statement in the Experimental program: "*The velocity meters did not provide reliable results, and as thus the wave gauges were the main source of data for the experiments."*

Also, the use of Froude scaling without considering Reynolds similarity is a limitation, since friction and turbulence are central here. Finally, some derived parameters—such as combining velocity from one interval with wave height from another to form a Froude number—are not physically rigorous.

The authors agree that it is important to clearly acknowledge such shortcomings, and have added the following sentence to the discussion: *"It is also important to acknowledge that, while Froude similarity is appropriate for reproduction ghe dominant role of gravity in free-surface wave hydrodynamics, the effect of the bottom friction (particularly in complex flow experiments such as the ones here) is government by Reynolds similarity, which was thus not appropriately modelled in the present experiments."*

Support of conclusions by data

The conclusions currently go further than the data allow. For instance, Tables 3 and 4 do not consistently show sheets > stones > smooth, yet the conclusions present this hierarchy as universal. WG4 data, known to be affected by beach reflection, are still used in the analysis. No variability or error measures are reported, which makes it hard to judge significance. Conclusions should be toned down and aligned with the actual spread of results.

The authors would like to thank the reviewer and agree with the comments. Some of the wording in the previous version of the manuscript was too strong relative to the spread of the experimental results. The sentence now reads: "This reduction was *generally* greater for the case of sheets…". The conclusions have been also toned down.

Organization

Overall, the paper is structured in a logical way. Still, there is some repetition across Results, Discussion, and Conclusions. A tighter structure would make the paper more focused.

The authors agree with this. Some redundant sentences in the Results section, which are later repeated in the Discussion, were deleted.

Conciseness and clarity

The manuscript could be more concise. The Introduction and Discussion are wordy in places, and expressions like "beyond doubt" are too strong for the level of evidence. More measured phrasing would improve objectivity.

Apologies for the use of this wording, it was deleted from the paper. The sentence now reads: "Despite such shortcomings, it is important to note that these do not detract from the overall conclusion of the paper, which clarified beyond doubt that the presence of reef flats helps to dissipate the energy of an incoming solitary wave, and that the protection offered increases with the length of this biological-legacy structure."

Language quality

The English is generally understandable, but grammar and style need polishing. Some terms are vague (e.g., "less healthy corals") and some phrasing is too absolute. A careful language edit would improve readability.

A healthy reef ecosystem is one in which there is extensive coral cover. As the ecosystem becomes less healthy, the coral ecosystems degrade. Indeed, as the reviewer suggests, the term "less" healthy is a bit vague. This sentence was rephrased as: *"Thus, the authors would like to suggest to investigate the effect of other degraded coral reef ecosystem (represented perhaps by a higher/lower density or by using smaller/larger stones) on solitary wave propagation over the reef flat."*

Illustrations and tables

Figures and tables are helpful, but could be improved scientifically. Error bars or standard deviations are essential. Data known to be influenced by reflection should be flagged. Some of the photographs of materials might be better moved to supplementary material, keeping the main figures focused on quantitative results.

The authors would like to thank the reviewer for this comment. Standard deviations are now given in Table 2. Some superfluous photographs were deleted from the description of the apparatus.

References

The reference list is extensive and relevant, though somewhat heavy and occasionally dated. A more selective set highlighting the most pertinent works would read better. Formatting is mostly consistent.

The authors are glad that the reviewer agrees the reference list is extensive.

Formatting and keywords

Formatting appears close to journal standards. A final check will be needed before acceptance. Keywords are reasonable, though adding more technical terms such as *solitary waves* and *reef flat roughness* would make them stronger.

The terms solitary waves and reef flat roughness were added to the keywords.

The review report is attached.

Major Issues (from pdf):

1) Scaling and representativeness

   The solitary waves generated in the flume correspond to prototype periods of only about 15–20 seconds, while real tsunami waves typically have periods of several minutes. This mismatch is fundamental. The work should be framed as a laboratory-scale hydrodynamic study, not as a direct analogue of tsunami conditions.

   The authors would like to thank the reviewer for this suggestion. The authors have changed the title of the paper to: "Laboratory experiments on the propagation of bores induced by offshore soliton forcing over reef flats".

   The scaling discussion only considers Froude similarity and overlooks Reynolds effects. Because turbulence and bottom friction are central to the interpretation, the absence of any discussion of viscous scaling weakens the representativeness of the results and should be acknowledged.

   Agreed. This is now mentioned in the discussion: *"It is also important to acknowledge that, while Froude similarity is appropriate for reproduction ghe dominant role of gravity in free-surface wave hydrodynamics, the effect of the bottom friction (particularly in complex flow experiments such as the ones here) is government by Reynolds similarity, which was thus not appropriately modelled in the present experiments."*

2) Velocity measurement methodology

   The manuscript describes the use of electromagnetic current meters (ECMs) and PIV in detail, but later dismisses both as unreliable. The analysis ultimately depends on bore-front tracking (distance/time), but this is only introduced at the end. This is confusing. If bore-front tracking is the effective method, it should be presented clearly as the primary technique in the Methods section.

The following sentence was added to the methodology to acknowledge this: “The velocity meters did not provide reliable results, and as thus the wave gauges were the main source of data for the experiments.”

There is no quantitative assessment of error in this approach. At the very least, the authors should discuss frame rate limitations, operator judgment in bore identification, and repeatability.

The authors would like to thank the reviewer for this important comment. A paragraph was added to the Discussion section: *“It is also important to acknowledge that the bore-front tracking approach is subject to several sources of bias. The identification of the bore patterns relies on judgment on the side of the authors that were conducting this analysis, although as explained in Section 3.1., the results obtained across the various experiments were rather consistent. Nevertheless, these relied on an analysis of various frames at given time intervals, and there was some degree of variation between these for the same experimental condition (although all showed the same overall patterns).”*

In addition, the method is applied at WG1–2 where the wave has not yet steepened into a bore, which is conceptually inconsistent and undermines confidence in the velocity data.

Agreed. The authors have changed the term “bore front velocity” to “wave front velocity” throughout the manuscript, to make it conceptually consistent.

3) Reef analogues and experimental setup

Polyethylene sheets are taken to represent branching corals and regularly spaced stones to represent boulder corals. These are oversimplified analogues. Hydrodynamically they act more as generic roughness elements, and the results should be framed in that way rather than as direct representations of real coral morphologies.

This is not fully the case, yet the phrase used was a bit misleading, so the methodology section has been slightly rephrased to “…to represent large coral boulders (sometimes referred to by divers as “bummies”, which sometimes can have diameters of several meters, with spacing of some metres between them).”.

Essentially, the manuscript is not just measuring different roughness in corals, but different types of coral fields found in nature. Some of these can look similar to the idealized setup shown in Figure 4. Note that this represents boulders several metres in diameter, with sand in between them (several metres between each “bummy”). Again, Figure 3 would actually be not too dissimilar to looking at a large field of branchiform corals (which can be found in nature in many places, with newer corals growing on top of older ones, with the result looking like the sheet shown in the experiments). Examples of both can be found in many places, as per the reference video included.

So, while they are idealizations, the way both actually look in nature, at the scale shown, is not too dissimilar. But indeed from a hydrodynamics point of view the inherent difference is, for the scale of the experiments, that it results in a different roughness.

The reef crest is modeled as a small wooden strip. From the results it is clear that this feature influenced wave breaking and transformation. This effect cannot be ignored; the authors should discuss how this artificial element shaped the results and how comparable it is (or is not) to a natural reef crest.

This was indeed not discussed. The following sentence was added to the discussion: “In the present case the reef crest, modelled with a wooden piece, which is typically shallower than the reef flat behind it (as conditions for coral growth are ideal in this area) had a significant influence on wave hydrodynamics, as what happens in reality”

4) Statistical robustness

Most experimental cases are repeated only three times, with one condition repeated seven times. This inconsistency is unexplained. Given the variability of the results, three repetitions are not enough to establish robust statistics.

The following sentence was added to explain this: “WG3 for this experiment was an area of high turbulence, but otherwise the experimental errors for the other wave gauges were considered small enough that the other experimental conditions were only repeated 3 times.”

The results are reported only as averages, with no standard deviations, error bars, or other measures of spread. Without this information, it is not possible to judge whether the reported differences between conditions are meaningful.[

The standard deviation is now given in Table 2.

5) Interpretation of results

The data show that the relative effects of sheets, stones, and smooth surfaces are not always consistent, but the discussion presents a fixed hierarchy (sheets > stones > smooth) as though it were universal. This overstates the findings and does not reflect the variability in the dataset.

As indicated earlier, the phrase was toned down, to indicate that this is a general trend, which is not always consisted (as per the answers in previous comments).

WG4 data are acknowledged to be affected by reflection from the beach, yet are still used in Froude number calculations. This compromises the reliability of those results.

Agreed. The following was added to the discussions: *"and thus the Fr numbers at this point should be treated with caution, and it should also be further noted that the definition of Fr number used here is not rigorous"*

The definition of the Froude number used here (mixing velocity from one interval with height from another) is not rigorous, and comparisons with field tsunami Froude numbers are not well justified.

Agreed. The answer to the previous comments also applies here: "Agreed. The following was added to the discussions: *"and thus the Fr numbers at this point should be treated with caution, and it should also be further noted that the definition of Fr number used here is not rigorous"*

The conclusions use language that is stronger than the data support (e.g., "beyond doubt"). Given the acknowledged limitations, the tone should be more cautious.

These words are indeed too strong and were deleted.

Minor Issues

- Introduction: The literature review is thorough but too descriptive. It would help to synthesize why past studies differ and how the present work specifically addresses those gaps.

  The literature review was tightened slightly, in order to further focus on what is important.

- Figures and Tables: Should include error bars or shaded ranges. Data known to be affected by reflection (WG4) should be clearly marked.

  Agreed. The standard deviation is now given in Table 2.

- Language: Several terms are vague or too strong ("less healthy coral reefs," "beyond doubt"). Wording should be made more precise and neutral.

  The words "beyond doubt" were deleted. The sentence about the health of the coral reefs was rephrased to: "Thus, the authors would like to suggest to investigate the effect of other degraded coral reef ecosystem… on solitary wave propagation"

- Structure: Some repetition between Results, Discussion, and Conclusions could be removed to improve clarity.

  Agreed. Several redundant sentences from the Results section were deleted, as they are later repeated in the Discussion.

- References: Generally relevant, but a tighter and more up-to-date selection would improve readability.

  The literature review was tightened slightly, by deleting some references that were not important.

Recommendation:

The manuscript provides a sizeable dataset on wave transformation over reef-like roughness elements, which has potential value. However, there are major problems with scaling, velocity measurement, statistical treatment, and the way the results are interpreted. These issues are significant but, in my view, correctable.

To make the paper publishable, the authors should:

- Reframe the study clearly as laboratory hydrodynamics rather than tsunami-scale simulation.

  Agreed. The following sentences have been modified or added to various places, to clearly reframe the study. This has resulted in numerous changes to the abstract, introduction and conclusions. Some of the bigger changes are detailed below, and the rest can be seen in the document in track-changes mode.

  The abstract was almost entirely rewritten to "*This study investigates, in a two-dimensional wave flume, how idealized reef-flat roughness and reef-flat length affect the transformation of solitary-wave-induced bores. Field surveys following natural disasters show that there is some confusion about the degree of protection reef flats can actually offer, and various theories have been put forward. Some researchers claim that coral reefs offer protection from tsunamis, others that they enhance damage to settlement by accelerating the velocity of the wave, and a third group that they neither protect nor exacerbate the damage due to these events. In this study, the authors aim to examine the effect that coral reef analogues have on bores propagating over reef flats by means of laboratory experiments conducted on a two-dimensional wave flume, in order to provide some guidance the effect that idealized reef flats can play on disaster risk mitigation. Forty-eight cases of experiments were tested by using two different water depths, two lengths of reef flat, three types of surfaces (to represent different types of idealized corals), and four different input wave heights into the wave generator. The results indicate that the reduction in both wave heights and wave front velocities was greater in the cases where there were structures representing corals, and for longer reef flats. Also, the attenuation of wave energy was greater with reduced water depth, which in nature usually correlates with a healthy reef system.*"

  Introduction: *"However, to date no comparative study has been conducted on how different types of coral colonies –represented by different surface roughnesses- and reef lengths might affect solitary wave hydrodynamics in a flume experiments."*

- Present bore-front tracking upfront as the main velocity method, with error/uncertainty analysis.

  This is now acknowledged in the text, together with the standard deviation.

- Add statistical measures of variability (standard deviations, error bars).

  The standard deviation for the experiments is now given in Table 3.

- Revise the conclusions to reflect data variability and avoid overgeneralization.

  This was done, as responded in earlier comments.

- Explicitly acknowledge the limitations of the reef analogues and scaling.

  This is now explicitly mentioned in the discussion: *"It is also important to acknowledge that, while Froude similarity is appropriate for reproduction ghe dominant role of gravity in free-surface wave hydrodynamics, the effect of the bottom friction (particularly in complex flow experiments such as the ones here) is government by Reynolds similarity, which was thus not appropriately modelled in the present experiments."*

Review Round 2

Reviewer A:

accept submission

Reviewer B:

minor revisions, no new review required

Thank you for the revised manuscript and the point-by-point response. I think the manuscript has improved compared with the previous version. In particular, the study is now framed more clearly as a laboratory-scale hydrodynamic investigation, rather than as a direct analogue of tsunami conditions. The revised title, the change from "bore front velocity" to "wave front velocity", and the added discussion of scaling limitations are all helpful.

Overall, I think the manuscript is close to being acceptable. I only have a few remaining comments, which I think can be addressed with minor revisions.

The authors would like to thank the reviewer for the time taken to review the manuscript, and the additional comments provided. All these have been addressed, as indicated in the point-by-point replies below.

1) The statistical treatment is still somewhat limited. The added standard deviation table is useful, but it only covers one experimental condition. Since some of the conclusions compare smooth, sheet, and stone cases, as well as different reef lengths and water depths, I suggest either adding a little more information on variability where possible, or making the wording around these comparisons slightly more cautious. I do not think this requires a complete reanalysis, but the limitations should be clear to the reader.

   The authors thank the reviewer for this important suggestion. The authors agree that the repeatability analysis was conducted for only one representative experimental condition, and that the variability associated with other conditions was not quantified to the same extent. As the experiments were repeated three times for all cases (and seven times for one representative case), the authors consider the overall trends to be robust. Nevertheless, to avoid overstating the precision of the comparisons between reef surfaces, reef lengths, and water depths, the authors have revised the manuscript and make the wording more cautious. We have also added a statement explicitly acknowledging that the variability analysis was limited to one representative condition and that the quantitative differences between cases should therefore be interpreted as indicative trends rather than exact relationships.

   Section 3.1. had the following sentence added: *"thus, while the overall trends observed in the experiments were pretty consistent, the quantitative differences between reef configurations should be interpreted as indicative rather than statistically definitive".*

   Section 3.4 had the following sentence added: *"although it is important also to note that the porosity of structures is not the same, and that the precise magnitude of these differences should be interpreted with caution given the limited number of repetitions in each experimental condition"*

2) The velocity method could be described more clearly. The change from "bore front velocity" to "wave front velocity" is a good improvement. Since the final analysis mainly relies on wave-front tracking rather than the ECM or PIV measurements, I suggest saying this more directly in the Methods section. A short explanation of how the front was identified, and what the main sources of uncertainty are, would also be useful.

   The authors thank the reviewer for this comment. The authors have revised the Methods section to explicitly state this: "*Hence, the final analysis relied on wave-front tracking rather than the ECM measurements (as will be detailed later).*"

3) The WG4 reflection issue should be made a little clearer. The manuscript already mentions this limitation, but WG4-based quantities are still used in some important comparisons. I suggest adding a clear note in the relevant table caption or discussion so that readers understand these values should be interpreted with caution.

The authors thank the reviewer for this observation. To make this limitation more explicit, the authors have added cautionary notes about this in the caption of Table 4: "*(note that values involving WG4 should be interpreted with caution as measurements at this location were affected by reflections from the beach section)"*

4) I suggest slightly toning down the final conclusion. The results support the conclusion that the tested reef-flat analogues reduced wave height and wave-front velocity under the present laboratory conditions. I would avoid broader wording that could be read as a general statement that coral reefs provide protection, unless the laboratory and idealised-roughness limitations are stated clearly in the same place.

   The authors agree with the reviewer, and have toned down the conclusion accordingly. The new wording reads: *"Hence, this study indicates that reef-flat analogues with greater roughness and longer lengths can enhance the attenuation of damage by solitary-wave-induced bores under the tested experimental conditions."*

5) The manuscript would benefit from a final editorial cleanup before publication. I noticed a few remaining language issues, for example "government by Reynolds similarity", which should be "governed by Reynolds similarity", and "reproduction ghe dominant role", which should be corrected. Please also check the consistency regarding Roeber et al. (2010), since the response states that this reference was deleted.

   A number of minor editorial changes were made to the document, as can be seen in the track-changes document submitted.