Review Report for

Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments

Léo Pancrazzi *, Dominique Mouazé, Pierre Weill, Bernadette Tessier

*Corresponding author: l.pancrazzi@ucl.ac.uk

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Round 1 Review

Comments by reviewer 1 ("anonymous")

Completed: 2025-11-28 04:54 PM
Recommendation: Reject
Reviewer Comments
For author and editor
See the review comments file

General:

This paper attempts to study the subsurface sand-mixing on sand-and-gravel beach from laboratory experiments, using analogue materials as replacement for sand and gravel sediment. The methodology used is interesting and can provide a promising way to study gravel and sand beaches on a small scale, however a lack of description and validation of the scaled model is critically affecting the validity of the results. Moreover, current state of the knowledge is insufficiently contextualized, with key references missing, leading to objectives which are too broad and ambitious for the dataset used. Finally, the discussion is too speculative, and most of the results are not backed up by the data, with a clear lack of hydrodynamic processes analysis. We therefore recommend rejection, with strong encouragement to resubmit after addressing the following major comments.

Major Concerns

1) The introduction lacks proper contextualization of the conducted research, including missing key studies and state of the art on sand and gravel processes on composite (ex. Bayle et al., 2020; Bayle et al., 2021; Foss et al., 2023; Matsumoto et al., 2025) and MSG beaches (ex. Kirk, 1980), and how research fits and contributes to existing literature. Field study investigating swash processes and dynamics using high resolution Lidar measurements as the one from Almeida et al. (2015) for gravel beach, and Bayle at. Al (2023) for composite beach should be considered. Further differentiation of sand and gravel processes between composite and mixed sand and gravel beaches (MSG) should be extended and explicitly stating how the conducted research aims to address it. Explanation of real-world processes that are aimed at being replicated by placing sand at the toe of the beach (l.187) is recommended.

Almeida, L.P., Masselink, G., Russel, P.E., Davidson, M.A., 2015. Observations of gravel beach dynamics during high energy wave conditions using a laser scanner. Geomorphology 228, 15–27.
Bayle, P. M., Blenkinsopp, C. E., Conley, D., Masselink, G., Beuzen, T., & Almar, R. (2020). Performance of a dynamic cobble berm revetment for coastal protection, under increasing water level. Coastal Engineering, 159, 103712.
Bayle, P. M., Kaminsky, G. M., Blenkinsopp, C. E., Weiner, H. M., & Cottrell, D. (2021). Behaviour and performance of a dynamic cobble berm revetment during a spring tidal cycle in North Cove, Washington State, USA. Coastal Engineering, 167, 103898. https://doi.org/10.1016/j.coastaleng.2021.103898
Bayle, P. M., Blenkinsopp, C. E., Martins, K., Kaminsky, G., Weiner, H., & Cottrell, D. (2023). Swash-by-swash morphology change on a dynamic cobble berm revetment: High-resolution cross-shore measurements. Coastal Engineering, 184, 104341
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An improved introduction should also lead to more specific objectives that would better match the type of data collected. We highly recommend focusing on analysis of processes that are measured only, and how this can be validated/invalidated the existing literature and conceptual models. Without hydrodynamics data such as runup, a lot of the discussion becomes speculative and should be removed.

2) Methodology and Scaling: The methodology used for this work is very interesting and has the potential to provide a feasible alternative for small scale sand and gravel experiments, however there is lack of proper validation of the scaled model for this specific study (although we understand that the concept of using analogue material has been demonstrated in literature). There is a lack of clarity in what the model is trying to represent, such as energy conditions, type of gravel and sand beach and real-world processes. I was not convinced through the evidence provided that the model could properly represent real sand and gravel processes. The scaling is not referred a specified prototype, and morphodynamics behavior was not compared directly to anything in literature that would assure real-life processes.
   a) Sediment: How can the choice of size and material of analogue materials be justified through past research, or can calculated dimensionless parameters justify the scaling (Table 1)? You need to provide a complete scaling analysis for this specific study.
   b) What about porosity? At least provide the Df/DC. Can the Polymer sink in the foam and geotextile?
   c) Wave hydrodynamics: l.174-175 – Further clarification on how the scaling for Hs and Tp was done, through Froude scaling and how? l.451-452 – it should be clear what kind of energy state the model represents.
   d) Composite beaches have a distinct (but variable) separation between the lower dissipative sandy beach, and the gravel berm which was not represented here.
   e) Discussion section 4.3 – shows real concerns about scaling methodology which can invalidate methodology, stating that the low-density particles can “imply a poor-representation of beach dynamics” (l.408-409) and “transport if suspended low-density particles is more likely to differ from natural particles” (l.417-419). l.412-415 – the predominant mode of transport is not evident through experimental results and morphodynamics compared to literature was not made evident.

3) The manuscript lacks sufficient contribution to literature and empirical data to support stated arguments and conclusions.
   a) Besides infiltration of the fine particles into course particles through processes that were not validated as real-life sand and gravel morphodynamics, the results do not contribute further to existing literature and filed observation.

b) Data collected are specifically aimed at morphodynamics evolution of fine and coarse-grained sediment. Statements of runup, waterflow and groundwater are not supported through data or properly addressed in literature (l.332-334)
c) l. 385-387 – disagree that this is the case, the comparison between MSG and composite beaches is not properly compared and contextualized within literature. This is still a composite beach (why would it be mixed?). Fine grain erosion still occurs, without changing significantly the pure gravel layer in composite beach, as shown here?
d) l.332-338 – "step level there is no deposition" – how does it exactly correlate with groundwater flow? Is this gap in deposition seen somewhere else in literature in a specific type of gravel beach?
e) Conclusion: (l.471-478) – not enough comparison and validation to support that key morphodynamics features were reached, or that undesired effects are negligible.
f) l.479-484 – "clear flow asymmetry" is mentioned a few times (l.322-323) but it is not clearly defined what flow characteristics asymmetry is referring to and there is no data provided to specifically support it.

Specific comments:

- The ratios in table 1 are not always correct. Please double check all values.
- Method 2.2 is not clear enough. Add references and techniques explaining how the the topo-bathymetry is reconstructed.
- Is the flume drained for lidar scanning? It seems like it is the case, then the compaction plays an important role, as well as scouring. The detailed analysis of small steps in the beach profile seems over-killing given the measurement accuracy and experimental errors.
- The experimental procedure and tests conditions are not super clear, and could be improved: when are the tidal cycle played in phase 1? Duration of the experiment in phase 1, for each condition? A table showing the procedure for each test, tide cycle and water level would help.
- Figure 4B: Are you sure the volume is conserved? Compare final state with their respective initial profile. Explain standard deviation (shaded area) on the graph.
- Fig 4 and 5 C and D not really used. To remove.
- Section 3.3: what about the retreat of the crest, and height increase. This is more important than the 'step' at 55 cm from the shoreline.
- Figure 6: extend the panel to show the accretion of the gravel bar (same scale as Figure 5 for the x coordinates)
- 'Offshore transported particles' is discussed a lot, but not really shown. At least include a photo.
- L.391: Open framework? What do the authors mean. Consider discussing the findings of Bayle et al. (2021) and Matsumoto et al. (2025).

Comments by reviewer 2 ("anonymous")

Completed: 2025-11-16 01:46 PM
Recommendation: Minor revisions
Reviewer Comments
For author and editor

This is a very interesting paper trying to shed light on the internal sedimentary processes to provide a first-order approach. As such it has a somewhat abstracted experimental set-up of starting with a complete separation of sediment sizes which one does not find in nature, however, in coastal risk management the question of using a pure gravel beach is often posed, which, if placed on a beach where there is a sub/intertidal sand source has always been assumed to be ineffective as the sand would migrate into the gravel, which is confirmed in this paper, so thank you!
The authors are well aware of the difficulties of the experiment and provide a very useful discussion, though the only aspect not covered is the influence of the foam layer compared to a deeper beach wedge, which seem to reduce the beach cover near the beach toe a very thin covering. This should be added to the decisions on the set-up and the interpretation and discussions of the results.
The review includes a marked-up manuscript that contains a number of points that need clarification in relation to the text and some of the figures, but these should be easy to implement and thus require a minor revision fundamentally to make the paper easier to read and understand.
Suggestions are made for some text replacements and additions to the title/abstract/keywords to help potential readers to decide whether to read the paper or not.

Recommendation by Associate Editor 2 ("Matthieu Giaime")

Completed: 2025-12-08 10:32 AM
Recommendation: Major revisions

Reviewer Comments
For author and editor
Dear Authors,
Thank you for your submission, "Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments". After reviewing your manuscript and the feedback from two anonymous reviewers, I recommend that you make major revisions before resubmitting.
It is important to carefully address all Reviewer 2's concerns, particularly about how you describe and validate the scaled model used in your study.
In your revision, please make sure to provide more context about your research by discussing recent work on sand and gravel processes in composite and mixed sand-and-gravel beaches. Also, explain your results more clearly and show how they relate to what other researchers have found. This will help strengthen your arguments.
I look forward to receiving your revised manuscript.
Best regards,
Anonymous Associate Editor

Decision by Editor-in-Chief (Alice Lefebvre)

2025-12-11

Dear Léo Pancrazzi, Dominique Mouazé, Pierre Weill, Bernadette Tessier,

Thank you for submitting your manuscript entitled "Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments" to Geomorphica. I have now received two reviews and an Associate Editor recommendation (included at the bottom and/or attached to this email). I have looked at the paper, the reviews and the Associate Editor recommendation myself.

One reviewer is very critical of the paper in its current form, while the other provides constructive feedback. The associate editor feels that you can address the reviews with some major revisions, especially addressing the comments of reviewer 2. On this basis, we would like to ask you to undertake a **major revision** of your manuscript.

Authors' responses to review comments

Completed: 25 January 2026

Response to reviewers
Manuscript title: Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments
We thank the Associate Editor and the reviewers for their thoughtful and constructive comments. We have revised the manuscript accordingly and believe the changes have substantially improved the clarity and rigor of the work. Below we provide a detailed, point-by-point response.
Summary of most important changes:
• Revised the introduction to better frame the objectives.
• Clarified the Method section.
• Improved the quality of Table 1.
• Updated Figure 4, 5 and 6.
• Included a new figure in Supplementary material.
• Refined interpretations in the Discussion and Conclusion sections.
Formatting note: Reviewer comments appear in black, and the authors' responses appear in blue. All references to changes in the manuscript refer to the tracked-changes version of the revised document.
Reviewer 1
Major Concerns
1) The introduction lacks proper contextualization of the conducted research, including missing key studies and state of the art on sand and gravel processes on composite (ex. Bayle et al., 2020; Bayle et al., 2021; Foss et al., 2023; Matsumoto et al., 2025) and MSG beaches (ex. Kirk, 1980), and how research fits and contributes to existing literature. Field study investigating swash processes and dynamics using high resolution Lidar measurements as the one from Almeida et al. (2015) for gravel beach, and Bayle at. Al (2023) for composite beach should be considered. Further differentiation of sand and gravel processes between composite and mixed sand and gravel beaches (MSG) should be extended and explicitly stating how the conducted research aims to address it. Explanation of real-world

processes that are aimed at being replicated by placing sand at the toe of the beach (l.187) is recommended.

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An improved introduction should also lead to more specific objectives that would better match the type of data collected. We highly recommend focusing on analysis of processes that are measured only, and how this can be validated/invalidated the existing literature and conceptual models. Without hydrodynamics data such as runup, a lot of the discussion becomes speculative and should be removed.

We appreciate the suggestion to situate our work more firmly within the broader literature on sand-gravel beaches. In response, we have

incorporated most of the recommended references into the introduction to provide a more comprehensive context.
Regarding the request for further explanation of sand and gravel processes, we have refined the description of the real-world processes our study aims to model (L96-98).
With respect to the suggestion that the objectives of the study should be more explicitly framed, we respectfully maintain that the objectives are clearly stated and thoroughly addressed throughout the manuscript:
- Objective (1), which focuses on reproducing the morphological evolution of coarse-grained profiles under varying wave energy conditions and mean water level fluctuations, is detailed in Section 2.2.
- Objective (2), concerning the behavior of fine particles within a porous coarse matrix, is examined in Section 3.4.
- Objective (3), assessing the influence of fine sediment incorporation on beach morphodynamics, is analysed in Section 3.3 and 3.4.
These objectives are also revisited and synthesized in the discussion section, where their implications are explored in depth.
2) Methodology and Scaling: The methodology used for this work is very interesting and has the potential to provide a feasible alternative for small scale sand and gravel experiments, however there is lack of proper validation of the scaled model for this specific study (although we understand that the concept of using analogue material has been demonstrated in literature). There is a lack of clarity in what the model is trying to represent, such as energy conditions, type of gravel and sand beach and real-world processes. I was not convinced through the evidence provided that the model could properly represent real sand and gravel processes. The scaling is not referred a specified prototype, and morphodynamics behavior was not compared directly to anything in literature that would assure real-life processes.
a) Sediment: How can the choice of size and material of analogue materials be justified through past research, or can calculated dimensionless parameters justify the scaling (Table 1)? You need to provide a complete scaling analysis for this specific study.
In developing a reduced-scale model, we considered it essential not only to decrease particle size but also to adjust particle density to maintain

physically meaningful behaviour. To our knowledge, no established or universally accepted scaling procedure exists for this type of granular - fluid interaction experiment, and therefore no standard framework could be directly applied. Furthermore, as stated in the opening sentence of Section 2.1, our aim was not to reproduce the conditions of a specific natural site but rather to explore general process dynamics.
Within this context, the choice of analogue materials was necessarily influenced by practical constraints such as cost, availability, and need to achieve qualitatively realistic behaviour, as clarified in the manuscript (L113-119). We acknowledge that a more exhaustive scaling analysis would be valuable and represents an important direction for future work. Nevertheless, we believe that the exploratory experiments presented here already provide meaningful insights and constitute a useful contribution to the scientific community.
b) What about porosity? At least provide the Df/DC. Can the Polymer sink in the foam and geotextile?
All particle-size ratios are reported in Table 1. The geotextile prevents the smallest particles to infiltrate the foam, as it is permeable only to water. Additional, clarification on the specific role of the geotextile has been added (L180-182).
c) Wave hydrodynamics: l.174-175 – Further clarification on how the scaling for Hs and Tp was done, through Froude scaling and how? l.451 - 452 – it should be clear what kind of energy state the model represents.
Further clarification has been incorporated to explain more explicitly how the wave height and peak period were scaled (L203-205). As discussed in Section 2.3, however, we cannot fully quantify how the reduced particle density and the associated differences in settling and shear velocity affect the dynamic similarity of the system. These uncertainties limit our ability to assign a precise energetic state to the model. For this reason, the discussion section acknowledges the uncertainty regarding the effective energy regime represented by the experiments.
d) Composite beaches have a distinct (but variable) separation between the lower dissipative sandy beach, and the gravel berm which was not represented here.
As noted previously, the model was not designed to replicate a specific

natural site. We agree that the experiments do not fully represent a classical composite gravel beach, particularly because coarse particles were present within the lower foreshore. However, the system did develop a clear functional separation between two zones (Fig. 6): a low-slope lower foreshore composed predominantly in the active superficial layer of fine particles (a trend that became more pronounced as additional fine material was introduced during the experiment), and a steeper upper foreshore composed exclusively of coarse particles, which were progressively infiltrated by fine particle over time. While this configuration does not reproduce the full complexity of natural composite beaches, it does capture the emergence of a two-layered morphology that is characteristic of such systems.

e) Discussion section 4.3 – shows real concerns about scaling methodology which can invalidate methodology, stating that the low-density particles can "imply a poor-representation of beach dynamics" (l.408-409) and "transport if suspended low-density particles is more likely to differ from natural particles" (l.417-419). l.412-415 – the predominant mode of transport is not evident through experimental results and morphodynamics compared to literature was not made evident.

A wide range of plastic particles is now available for laboratory use, and although some authors (e.g., Petruzzelli et al., 2013) have expressed concerns about their suitability in reduced-scale beach models, we do not consider these materials to be inherently representative of all experimental possibilities. As a secondary aim of this study, we wished to continue exploring the potential of these low-density particles in laboratory settings and to document the practical implications of their use. Our intention is that this discussion provides useful context for future decisions regarding analogue sediment selection.

We acknowledge that certain aspects of particle behaviour—such as the predominant mode of transport—can only be described qualitatively in our experiments, and we do not claim otherwise. Moreover, in the absence of a natural prototype with measurements at a comparable scale, direct comparison with field observations or with other laboratory studies is challenging. As stated at the end of section 4.3, these experiments should be viewed as exploratory. Their purpose is to identify

key issues, highlight methodological uncertainties, and open the way for more targeted and quantitatively constrained investigations in future work.

3) The manuscript lacks sufficient contribution to literature and empirical data to support stated arguments and conclusions.

a) Besides infiltration of the fine particles into course particles through processes that were not validated as real-life sand and gravel morphodynamics, the results do not contribute further to existing literature and filed observation.

We fully agree that such validation would strengthen the interpretation of the infiltration processes. However, as noted in our previous responses, the experimental design was constrained by practical factors including material availability, facility limitations, and the cost and time associated with this kind of experiments. For these reasons, validation against natural sites was not part of the original scope of the study.

At the same time, we are not aware of any universally accepted or standardised validation protocol for laboratory infiltration experiments of this type. Existing field datasets are highly site-specific, and their transferability to controlled laboratory conditions remains limited. Our aim was therefore to isolate key mechanisms under controlled conditions, rather than to reproduce full natural morphodynamics.

Despite the lack of direct field validation, we believe the results produce useful insights into the relative influence of particle-size contrasts, packing configurations, and infiltration pathways. These findings complement existing literature by offering mechanistic understanding that is difficult to obtain from field observations alone, where processes are typically more complex and less directly measurable.

b) Data collected are specifically aimed at morphodynamics evolution of fine and coarse-grained sediment. Statements of runup, waterflow and groundwater are not supported through data or properly addressed in literature (l.332-334)

We acknowledge that these hydrodynamic parameters were not directly measured in our experiments. Nevertheless, because hydrodynamics inevitably influences sediment transport processes, we considered it important to discuss these aspects qualitatively.

Our intention was not to make quantitative claims or introduce unsupported assumptions, but rather to situate the observed sedimentary behaviour within the broader conceptual framework of coastal morphodynamics. The discussion therefore relies on general, well-established hydrodynamics principles rather than on-site specific measurements or detailed modelling. We have now clarified this point in the revised text (L342-346, L424-427) to avoid any impression that the manuscript infers hydrodynamics processes beyond what data can support.

c) l. 385-387 – disagree that this is the case, the comparison between MSG and composite beaches is not properly compared and contextualized within literature. This is still a composite beach (why would it be mixed?). Fine grain erosion still occurs, without changing significantly the pure gravel layer in composite beach, as shown here?

Our intention is not to claim that the experimental beach fully transitioned from a composite to a MSG state. Rather, the analogy is used to highlight the direction of change in sediment organisation under controlled sediment-supply conditions.

In natural settings, we are not aware of documented cases where a pure gravel beach evolves into a MSG system solely through fine-sediment inputs. However, the controlled nature of our experiments allows us to isolate and observe the mechanisms by which increasing fine-sediment supply progressively alters beach morphology and internal structure. In particular, our results show that (i) increasing fine-particle content reduces the beach slope, and (ii) fine sediments are capable of climbing the profile and accumulating landward. These behaviours are consistent with processes described in MSG systems, even though the transition is not complete in our experiment (L454-457).

d) l.332-338 – “step level there is no deposition” – how does it exactly correlate with groundwater flow? Is this gap in deposition seen somewhere else in literature in a specific type of gravel beach?

Deposition of fine particles occur beneath the surface. Further clarification has been added (L393-395).

e) Conclusion: (l.471-478) – not enough comparison and validation to support that key morphodynamics features were reached, or that

undesired effects are negligible.
We agree that the conclusion should more clearly reflect the level of comparison and validation that is possible with the available data. We have therefore revised the conclusion to adopt a more cautious formulation.
f) l.479-484 – "clear flow asymmetry" is mentioned a few times (l.322-323) but it is not clearly defined what flow characteristics asymmetry is referring to and there is no data provided to specifically support it.
We have revised the conclusion to adopt a more cautious formulation regarding the interpretative hypotheses on the hydrodynamic controls derived from the observed morphosedimentary patterns. We also removed the previous allusion to "flow asymmetry" from section 4.1, which we agree was misleading.
Specific comments:
• The ratios in table 1 are not always correct. Please double check all values.
The values were double-checked and found to be correct. The confusion may arise from the fact that the values have been reported to two decimal digits.
• Method 2.2 is not clear enough. Add references and techniques explaining how the topo-bathymetry is reconstructed.
Additional information on how the topo-bathymetric surveys were performed has been added (L167-170).
• Is the flume drained for lidar scanning? It seems like it is the case, then the compaction plays an important role, as well as scouring. The detailed analysis of small steps in the beach profile seems over-killing given the measurement accuracy and experimental errors.
The flume is not drained for scanning. Further information has been added to clarify this point, as reported in the previous comment.
• The experimental procedure and tests conditions are not super clear, and could be improved: when are the tidal cycle played in phase 1? Duration of the experiment in phase 1, for each condition? A table showing the procedure for each test, tide cycle and water level would help.
The concerned points have been clarified in the text (L212-213).
• Figure 4B: Are you sure the volume is conserved? Compare final state with their respective initial profile. Explain standard deviation (shaded area) on

the graph.
In the original draft, each profile was compared to an average initial profile of several experiments shown in grey, with the shaded area representing morphological variance. This was confusing and has been replaced by an average profile that only includes the experiments described in this article. In the new version, we can better see the small changes that occur during the low-energy configuration. For the high-energy configuration, the volume is not conserved in the flume section presented. Some of the beach sediments are transported offshore. This point has been clarified in the text (L268-272), and in the caption of figure 4.
• Fig 4 and 5 C and D not really used. To remove.
10
While the slope and slope-difference panels are not explicitly referenced in the text, they provide useful contextual information for readers by helping them identify key sections and positions along each profile. They complement the topography and topographic-change panels and support interpretation of the spatial patterns shown. For this reason, we would prefer to retain these elements in both figures.
• Section 3.3: what about the retreat of the crest, and height increase. This is more important than the 'step' at 55 cm from the shoreline.
Additional information on the crest position has been added (L292-293).
• Figure 6: extend the panel to show the accretion of the gravel bar (same scale as Figure 5 for the x coordinates)
We agree that it would be useful to analyse what happened at both ends, but we were limited by the metal structure of the flume, which restricted our field of view to what is shown in Figure 6.
• 'Offshore transported particles' is discussed a lot but not really shown. At least include a photo.
A side-view photograph has been included in Supplementary material (Fig. S1).
• L.391: Open framework? What do the authors mean. Consider discussing the findings of Bayle et al. (2021) and Matsumoto et al. (2025).
The sentence has been rephrased (L458).
Reviewer 2
L24: 'shape' can be many things. does this refer to profile and/or planform shape?

Might be useful to clarify.
Replaced “shape” by “profile shape”
L26: better " in the beach"?
Replaced “underground” by “in the beach”
L30: Somehow, the non-technical summary contains a lot of information that is neither in the title, abstract or keywords. It would be useful to include this in one of these three standard parts, maybe even in the title like "insights from wave flume experiments under changing waves and water levels" or similar. This would help the potential reader early on to decide whether they want to read this paper or not.
The abstract has been modified to add missing information about experimental protocol (L11-12).
L46: and (3) where a msgb is fronted by a gently sloping sandy lower beach e.g. most beaches in southeast England. As the paper demonstrates that sand moves into gravel (there is also the mechanism of windblown sand entering from above and the temporary sorting of the top layer into just gravel or gavel and sand depending on suspended sediment and wave conditions. While not in the introductions, hopefully this is in the discussion.
The third type has been added (L50-53).
L81: most flume base studies in the 90 and 2000s (e.g. Keiming She or HRWallingford) used crushed anthracite which is angular and thus has rather different properties. Using rounded PET seems like a better material. Should this be included in the intro? Please consider.
Additional information about particle shapes has been added in the introduction (L88-91).
L92: given that the pet granules are very rounded/ spherical, which only applies to rather specific gravel beaches and will have an impact on the angle of repose, and the sand substitute is very angular contrary to the generally more spherical characteristics of sand, a discussion about shape in the material section and in the final discussion would seem to be required. Please add.
Additional information about particle shape has been provided in the methodology (L113-119). We agree that the angularity of the polymer abrasive has an impact on sediment transport, and therefore on beach morphodynamics, although unquantifiable. An entire paragraph dedicated to particle aggregation can be found in the discussion (L503-513).

Table 1: the table is rather small in font size on A4. Please redraw.
The width of the columns has been decreased, allowing for larger text, and the table has been re-exported with a higher resolution.
L155-159: Sorry, I had to read this several times and compare it with the figures and am still not sure I fully understand the set-up. The foam is the brownish areas in Figure 2 a-c?. Why is this covered with a geotextile? Are the pores larger than the sediment?
FIgure 2A seems to be unnecessary as the experiment seems to start with 2B?
In 2c it appears as if between 2.5 and 2.3 m distance the foam is not covered by PET granules. Is that not a fundamental weakness in the set-up that should have a more or less consistent thickness of PET granules covering the foam? Why was foam chose rather than having the entire beach wedge in PET granules. Some additional clarification needs to be added please.
Additional explanations on the use of foam and geotextile are provided (L180-182), along with further information on this set up compared to more conventional ones. Figure 2a is not essential but does provide a clear view of how the initial sediment stock is placed.
L181: How does this compare with the general rule of thumb that it takes 1000 waves for the profile to fully develop, i.e. was water level changed too quickly compared to a natural beach? Given the tidal 'curve' in figure 3 this does not seem to allow much time for adjustment as would happen in a natural tidal cycle? Please add a couple of sentences.
We have added further clarification regarding the water level cycles in the revised manuscript (L212-221) to address the concerns raised. The chosen experimental protocol was primarily guided by practical constraints, including laboratory limitations and the need to conduct multiple trials within a feasible timeframe. As now explicitly stated in the text, these experiments are not intended to fully replicate the gradual adjustments of a natural tidal cycle. Instead, the focus lies on investigating the effects of repeated water level variations on the beach profile, providing a controlled framework to observe sediment transport and morphological responses under dynamic conditions.
Determining the “appropriate” cycle duration for profile adjustment is highly context-dependant and influenced by factors such as wave energy, sediment properties, and initial beach morphology. While longer cycles might allow for more extensive adaptation, the selected timescale serves as a practical starting point to

identify trends ins sediment redistribution and profile evolution. We acknowledge that comparing different tidal cycles durations could yield further insights, and this could be explored in future work. Given the constraints of the laboratory setup and the use of PET granules, this protocol serves as a reasonable initial approach to investigate the sensitivity of the beach profile to water level variations.

L194-195: is this the same as the "5 cm..." on the previous page (which is missing the +-)? Please clarify.

The same water level variation protocol has been applied in both phases, as indicated at the end of the sentence: “replicating the conditions of the first phase (Fig. 3)”

L195-196: The sentence would be clearer if changed to e.g. "3). Topographic profiles at three points in time of the test sequence at 145, 835 and 1075 minutes are used to showcase the morphological evolution of the beach in figures 4-6." or similar.

Suggestion incorporated.

L196: Should there be additional information somewhere that identify the numbers?

Phase 2 of the experiment is the only phase that needs precision on when the topographical survey was performed. Labels are described in the text (L236-239), indicated in the corresponding figure (Fig. 5) and described on Table 2.

Fig. 3: shame these were not also taken at t2. Any reason? Please add.

During low and high-water levels, an analysis of wave reflection was attempted, which required a certain stability in the water level, explaining these plateaus. The measurements were not considered usable and were not included in this article.

L210-211: Can you please explain where the sediment that has been lost from the IP to the LEP has gone to? Similar, as not assessable in graph, does the HEP have the same CSA as the IP, and if not, where would that sediment have come from? Please add.

The confusion regarding the sediment balance may be due to a lack of information about what the initial profile shown in Figure 4a represents. It is supposed to represent an average beach profile determined by comparing several initial states, which may differ slightly from one experiment to another. To avoid confusion, a new initial profile representing exclusively the initial state of the experiments presented in this article has been drawn in Figure 4a. The following plots in Figure

4 have been updated accordingly, as have the values mentioned in the text and those in Table 2. The differences between LEP and HEP, now considered redundant, have been removed.

In the new version, no sediment appears to be lost between IP and LEP. Regarding the sediment balance in the high-energy configuration, there is a clear positive balance in the beach profile section (D < 260 cm), generated by a negative balance offshore (D > 260 cm). Additional information explaining this onshore transfer of sediment is provided (L268-272).

L223-224: isn't it at ~180 cm? I do not quite understand this sentence. Please clarify.

A rewording is proposed to clarify how the shoreline at low water position can be identified morphologically (L266-268).

Fig 4: As commented above is the proximity of the foam layer introduce some artifical situation that impact the beach profile around the 225 cm mark? E.g. wave run-up infiltrates the porous PET beach and creates a current on the surface of the foam that exits at ~220 cm? Please explain.

The proximity of the foam at the bottom of the beach necessarily has an impact on the foreshore morphodynamics, but this is also the case with most physical beach models. However, in our experiments, instead of having the hard bottom of the channel in direct contact with the lower foreshore, we have a relatively permeable material that better replicates infiltration processes. We recognize that the lack of available sediment in this section is a limitation, but we do not consider this to be a major disadvantage or constraint compared to most physical beach models.

Fig. 5: It would useful to sketch here what the sand distribution was. Please add.

We agree that this would be useful, but due to the complex shape and nuances of the infiltrated fine particles, it would be confusing to combine the two pieces of information (i.e., the morphology of the beach and the internal stratigraphy).

L283: Can you please clarify whether the original clast-supported PET gravel (gravel particles are in contact with each other) has change to a matrix-support sediment where the sand has pushed the gravel apart so that gravel particles no longer touch each other? This is not clear from the statement. This is fundamental as at the point where the sediment switches from clast- to matrix-supported the beach starts to behave like a sand beach.

We agree that the transition from clast-supported to matrix-supported is a crucial

process, but it was only at the end of the experiment and after digging trenches on the beach that we were able to confirm that this transition had indeed taken place. Lateral photographs allow us to estimate the development of fine-grained layers (as described in section 3.4), but they cannot be used to track the geometric arrangement of particles in detail.

Fig. 6: Could you please add the water level into the sketches up 700 minutes and maybe even the swash excursion envelope.

Water levels have been added, but swash envelopes weren't measured.

L299: can you please clarify whether this infiltration is progressing 'horizontally along the geotextile' of whether the sand is in suspension/saltating and enters the gravel from above, i.e. are infiltrating vertically?

Additional information has been added to clarify the infiltration processes (L353-359).

L316: And then only within the active layer which may be quite thin? Please add.

Suggestion incorporated (L376).

Round 2 Review

Comments by reviewer 1 ("anonymous")

Completed: 2026-03-03 10:36 AM
Recommendation: Major revisions

Reviewer Comments
For author and editor
In the first round of reviews, most comments have been addressed, and some necessary changes were made, although some major concerns previously stated by reviewer 1 (and 2) were not addressed. Further work is encouraged by addressing framing research, validation, justification of methodology and more explicit contribution based on results. Major revision is required: without addressing the major comments below, I unfortunately won't see this manuscript suitable for publication.

Response to First Round of Revisions:
In the first round of reviews, most comments have been addressed, and some necessary changes were made, although some major concerns previously stated by reviewer 1 (and 2) were not addressed. Further work is encouraged by addressing framing research, validation, justification of methodology and more explicit contribution based on results. *Major revision is required: without addressing the major comments below, I unfortunately won't see this manuscript suitable for publication*.

On Reviewer 1's comments

Major comments:

1. There is still a gap between the represented processes and real-life conditions. Following this and in relation to **Reviewer 2's comment on Figure 2c** → Revisit comment. Is there a part of the profile not covered by PET granule? Why was this the case and what limitations might arise from this, and how could it affect processes being studied?

The framing of the experimental procedure still requires clarification:

- The experimental procedure undertaken is clearly about offshore sand (i.e., from the lower beach face and sand bar) infiltration processes within a pure gravel layer? How is this related to natural beaches? This is clearly not a MSGB, because it is not mixed initially. However, it is a natural process happening in composite beaches, when the pure gravel layer is relatively thick (winter condition), and offshore sand moves onshore and starts accumulating within the gravel berm. The point here is that your procedure is very specific, but your framing is too general. Your results might be useful for more general MSGB, but given the experimental procedure used, this should only be briefly discussed and not form the main narrative.
- Although your objectives are clearly stated, they are not clearly addressed:
    - You do not demonstrate reproduction of morphological evolution. This is very important actually. Since you are not performing a complete scale analysis, it is crucial to validate your observation with the existing literature (for both phase 1 and 2). I know this is what you are trying to do in Section 4.1, but in its current state, it is

buried in this discussion involving hydro, morpho, and porosity. You need to have a clear section (early on, something like 'Qualitative validation of scaling') addressing this: even if you stick to a qualitative comparison, combine your current comparison from 4.2 with a comparison with gravel beaches (for phase 1) and with the only available conceptual models from Bayle et al. (2021) and Matsumoto et al. (2025) (for phase 2). This is the only way to convince the reader early on that your method scales things qualitatively without going into a proper scaling analysis.

- Objectives 2 and 3 could be combined. But without objective 1 clearly addressed, you are simply showing how polymer abrasive and PET of a certain size interact together under wave action!

2. Methodology and Scaling

a) Sediment: Although the experiments are not a reflection of a specific site, please use the values of Table 1 to justify the scaling procedure and choice of material, relating it to existing studies or theory framework, where appropriate.

d) Agreed now. This should also be reflected when you frame your objectives (see comment 1).

e) The discussion is appreciated as it shows awareness of the limitations of your own research. L.480-483 – still not addressed, results don't explicitly shows mode of transport. Focusing on your results and data in comparison with literature in terms of what is already known about sand and gravel beaches, will reinforce that even though these concerns exist, the methodology is still valid.

3.a) As you state that "the aim was to isolate key mechanisms under controlled conditions rather than to reproduce full morphodynamics" → suggest using this as part of justification of research and redefine objectives. A dedicated section comparing with global trends and conceptual models is required.

b) The added statement is appreciated. If the discussion relies on well-known hydrodynamics principle, these statements should be cited so they do not become speculative (l. 395-408), such as the statements on groundwater flow l.395-397, l.402-403 and run-up l.450-451; use the hydrodynamics stated in the citations found in l.408 to back up your claim.

d) still not addressed – to say the transition is not complete, is to imply this is a process existing in natural settings, which is not documented or certain that will happen in your experimental conditions. This is still speculative. Remove or rephrase, state what the results or data shows.

e) Consider about adding a figure, that shows the foam, coarse and fine sediment at "equilibrium" and if it shows the main morphodynamics found in pure gravel, composite or mixed beaches. This could show support for this statement. It would fit well the suggested 'Qualitative validation of scaling' section…

f) l.557-558 – Interesting hypothesis, but since it is not addressed (proven or disproved) based on the data and as part of the objectives of this paper, should be rephrased or backed up by literature.

Minor comments:

- Hydrodynamic conditions during Phase 1 are still not clear. What are the water level at low and high energy states, and the orders of the tests? To improve clarity, consider adding a table in Section 2.3 summarizing, for each phase: water levels, wave conditions (regular or irregular, including Hs and Tp), and duration (please revisit the original comment).

- If chosen to keep figure 4 and 5 C and D, add reference and brief description in the text. An unused figure is more confusing than helpful to the reader.

On Reviewer's 2 comments

Fig. 4 – Revisit comment. The lack of sediment must be justified, along with the limitations this introduces to the experiments and the possible impact of processes in real world should be addressed in the paper. If this was done compared to physical beach models, it needs to be justified and cited so the experiment is not discredited.

Fig.5 – Revisit this comment. If the goal of these images is to recreate morphodynamics, it should be clear if this would be a fully mixed sand and gravel beach, or what specific morphodynamic behavior is referring to. Refer to L. 286-287 – it is not clear to the reader if sand is incorporated and what is the sediment distribution that is under effect.

L283: Is there anything that can be said about this process based on side observations, when compared to final trench dig?

Recommendation by Associate Editor 2 ("Matthieu Giaime")

Completed: 2026-03-16 08:34 AM
Recommendation: Major revisions

Dear authros,
I have read with interest your revised manuscript entitled "Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments", as well as the feedback from the anonymous reviewer. After careful consideration, I recommend that the manuscript undergo major revisions.
While I acknowledge the improvements made since the initial submission and appreciate your efforts to address the reviewers' comments, some key aspects, particularly the presentation of the experimental procedure and the methodology, would benefit from further clarification. This will help ensure that the study is fully accessible and comprehensible to readers.

Best wishes
Anonymous Associate Editor

Decision by Editor-in-Chief (Alice Lefebvre)

2026-03-18

Dear Léo Pancrazzi, Dominique Mouazé, Pierre Weill, Bernadette Tessier,

Thank you for submitting your manuscript entitled "Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments" to Geomorphica. I have now received one review and an Associate Editor assessment (included at the bottom and/or attached to this email). I have looked at the paper, the reviews and the Associate Editor recommendation myself.

The reviewer is still quite critical of your manuscript and is asking for some substantial modifications to the paper, I feel their comments are detailed and constructive enough that you can address them. With this in mind, I would like you to undertake a **moderate revision** of your manuscript. As this is a moderate revision decision, we will first assess your answer to the reviewer's comments and the revised version internally and decide if we need to consult the reviewer again.

Authors' responses to review comments

Completed: 24 April 2026

Response to reviewers (R2)
Manuscript title: Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments.
We are grateful to the Associate Editor and the reviewers for their valuable and constructive feedback in this second round of review. Their insights have been instrumental in refining our manuscript. We have thoroughly addressed each comment and implemented revisions that we believe strengthen the clarity, precision, and impact of our work. Below, we outline our detailed responses to each point raised.
Summary of most important changes:
• Included a new table to clarify the experimental protocol
• Included a new discussion section about the qualitative validation of morphological scaling
• Overall refined discussion
Formatting note: Reviewer comments appear in black, and the authors' responses appear in purple. All references to changes in the manuscript refer to the tracked-changes version of the revised document.
1. Associate Editor comments
I have read with interest your revised manuscript entitled "Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments", as well as the feedback from the anonymous reviewer. After careful consideration, I recommend that the manuscript undergo major revisions.
While I acknowledge the improvements made since the initial submission and appreciate your efforts to address the reviewers' comments, some key aspects, particularly the presentation of the experimental procedure and the methodology, would benefit from further clarification. This will help ensure that the study is fully accessible and comprehensible to readers.
In response to your concerns regarding the presentation of the experimental procedure and methodology, and following the reviewer's suggestion, we have prepared a new table (Table 2) summarizing the experimental protocol in a clear and structured manner. This table provides a concise yet comprehensive overview of the key parameters, procedures, and conditions of our wave flume experiments, ensuring that readers can easily grasp the methodological framework of our work.
2. Reviewer comments
In the first round of reviews, most comments have been addressed, and some necessary changes were made, although some major concerns previously stated by reviewer 1 (and 2) were not addressed. Further work is encouraged by addressing framing research,

validation, justification of methodology and more explicit contribution based on results. Major revision is required: without addressing the major comments below, I unfortunately
won't see this manuscript suitable for publication.
2.1. On Reviewer 1's comments
Major comments:
1. There is still a gap between the represented processes and real-life conditions. Following this and in relation to Reviewer 2' s comment on Figure 2c → Revisit comment. Is there a part of the profile not covered by PET granule? Why was this the case
and what limitations might arise from this, and how could it affect processes being studied?
PET granules cover the whole profile. This point has been clarified in L249-251 and can also be verified in Figure 4a (Initial Profile, IP).
The framing of the experimental procedure still requires clarification:
- The experimental procedure undertaken is clearly about offshore sand (i.e., from the lower beach face and sand bar) infiltration processes within a pure gravel layer? How is this related to natural beaches? This is clearly not a MSGB, because it is not mixed initially. However, it is a natural process happening in composite beaches, when the pure gravel layer is relatively thick (winter condition), and offshore sand moves onshore and starts accumulating within the gravel berm. The point here is that your procedure is very specific, but your framing is too general. Your results might be useful for more general MSGB, but given the experimental procedure used, this should only be briefly discussed and not form the main narrative.
A full check of the manuscript has been carried out to ensure that there is no allusion that MSGBs are the main or only target of this series of experiments. Thus, modifications have
been made in Materials and methods (L104-105, L107), Discussion (L507-508), and Conclusion (L684).
- Although your objectives are clearly stated, they are not clearly addressed:
o You do not demonstrate reproduction of morphological evolution. This is very important actually. Since you are not performing a complete scale analysis, it is crucial to validate your observation with the existing literature (for both phase 1 and 2). I know this is what you are trying to do in Section 4.1, but in its current state, it is buried in this discussion involving hydro, morpho, and porosity. You need to have a clear section (early on, something like 'Qualitative validation of scaling') addressing this: even if you stick to a qualitative comparison, combine your current comparison from 4.2 with a comparison with gravel beaches (for phase 1) and with the

only available conceptual models from Bayle et al. (2021) and Matsumoto et al. (2025) (for phase 2). This is the only way to convince the reader early on that your method scales things qualitatively without going into a proper scaling analysis.
o Objectives 2 and 3 could be combined. But without objective 1 clearly addressed, you are simply showing how polymer abrasive and PET of a certain size interact together under wave action!
A new section entitled “Qualitative validation of morphological scaling” has been added at the beginning of the discussion (Section 4.1). This section now explicitly addresses the morphological scaling by integrating qualitative comparisons previously dispersed in the different discussion sections. This provides clear and early evidence of the qualitative scaling behaviour of our method, supporting its validity without requiring a full-scale analysis.
Additionally, Objective 1 has been rephrased in the introduction (L96-98) to better align with the experiments and analysis conducted, ensuring it is now clearly addressed in the results and discussion.
The rest of the discussion has been adapted to ensure a smooth integration of the new section, with several points moved to avoid overlap. We believe these changes full address your comments and strengthen the overall clarity and coherence of the manuscript.
2. Methodology and Scaling
a) Sediment: Although the experiments are not a reflection of a specific site, please use the values of Table 1 to justify the scaling procedure and choice of material, relating it to existing studies or theory framework, where appropriate.
We appreciate the reviewer's comment and would like to clarify that the primary purpose of Table 1 is not to justify our scaling procedure, but rather to highlight the differences between our analogue sediment mixture and their natural equivalents. This comparison is, to our knowledge, unprecedented in literature. Studies focusing specifically on sand-and-gravel mixtures are rare. Most existing research relies on classic formulations for natural siliciclastic sediments (e.g., Soulsby, 1997; Paphitis, 2001), which are the same formulations we used to create Table 1. As mentioned in the text (e.g., L131-134, L143-147), elements of Table 1 are already integrated into our discussion to emphasize the relevance of our findings to natural sediment dynamics.
d) Agreed now. This should also be reflected when you frame your objectives (see comment 1).
See answer to comment 1
e) The discussion is appreciated as it shows awareness of the limitations of your own research. L.480-483 – still not addressed, results don’t explicitly shows mode of transport. Focusing on your results and data in comparison with literature in terms of what is already known about sand and gravel beaches, will reinforce that even though

these concerns exist, the methodology is still valid.
We acknowledge that our experiment did not include quantitative measurements of transport modes. However, our goal was to provide as detailed a description as possible of the potential limitations and behaviours associated with using low-density particles. Given the rarity of such experiments, we believe it is important to document and discuss all qualitative observations, even if they are preliminary. To address the reviewer's concern, we have revised the text (L583-590) to explicitly clarify that our assessments of transport modes are based solely on qualitative observation made during the experiments. We have also emphasized that these observations require further verification in future research.
3.a) As you state that "the aim was to isolate key mechanisms under controlled conditions rather than to reproduce full morphodynamics" → suggest using this as part of justification of research and redefine objectives. A dedicated section comparing with global trends and conceptual models is required.
The objectives have been reworded, and a new section devoted to discussion has been added (see answer to comment 1).
b) The added statement is appreciated. If the discussion relies on well-known hydrodynamics principle, these statements should be cited so they do not become speculative (l. 395-408), such as the statements on groundwater flow l.395-397, l.402-403 and run-up l.450-451; use the hydrodynamics stated in the citations found in l.408 to back up your claim.
We have clarified our results to distinguish them from the literature findings (e.g., L451-466, L540-545).
d) still not addressed – to say the transition is not complete, is to imply this is a process existing in natural settings, which is not documented or certain that will happen in your experimental conditions. This is still speculative. Remove or rephrase, state what the results or data shows.
Agreed. This statement was too speculative and was removed (L552-557).
e) Consider about adding a figure, that shows the foam, coarse and fine sediment at "equilibrium" and if it shows the main morphodynamics found in pure gravel, composite or mixed beaches. This could show support for this statement. It would fit well the suggested 'Qualitative validation of scaling' section…
We are not entirely certain whether the suggest pertains to morphological equilibrium, sub-surface equilibrium, or both. While figures 4, 5, and 6 illustrate the equilibrium of beach morphology and sub-surface sediment dynamics, we have not been able to provide a combined visualisation of both aspects. This limitation arises from the distinct methods used to capture the data (laser measurements for morphology and side photographs for sub-surface dynamics).
f) l.557-558 – Interesting hypothesis, but since it is not addressed (proven or disproved) based on the data and as part of the objectives of this paper, should be rephrased or

backed up by literature.
The conclusion point has been rephrased to emphasize speculative nature (L667-671).
Minor comments:
• Hydrodynamic conditions during Phase 1 are still not clear. What are the water level at low and high energy states, and the orders of the tests? To improve clarity, consider adding a table in Section 2.3 summarizing, for each phase: water levels, wave conditions (regular or irregular, including Hs and Tp), and duration (please revisit the original comment).
Following the reviewer's recommendation, we have added a table summarizing the experimental phases (Table 2).
• If chosen to keep figure 4 and 5 C and D, add reference and brief description in the text. An unused figure is more confusing than helpful to the reader.
While Figures 4 and 5 (panels C and D) are not explicitly cited in the text, they are referenced as a whole in multiple instances (e.g., L244, L255, L288, L303). Each panel contributes to the overall interpretation, and their details are thoroughly described in the figure's captions to ensure clarity.
2.2. On Reviewer's 2 comments
Fig. 4 – Revisit comment. The lack of sediment must be justified, along with the limitations this introduces to the experiments and the possible impact of processes in real world should be addressed in the paper. If this was done compared to physical beach models, it needs to be justified and cited so the experiment is not discredited.
We appreciate the reviewer's perspective but respectfully maintain that the notion of a "lack of sediment" in our beach profile is subjective. While the sediment layer may appear thin in a section of the initial profile, it thickens in subsequent experimental phases (Fig. 4a, 5a), which is why we did not elaborate on this point in the discussion. However, we acknowledge that the rationale for the initial sediment distribution was not explicitly stated, and we have now addressed this in the revised manuscript (L249-251).
Fig.5 – Revisit this comment. If the goal of these images is to recreate morphodynamics, it should be clear if this would be a fully mixed sand and gravel beach, or what specific morphodynamic behavior is referring to. Refer to L. 286-287 – it is not clear to the reader if sand is incorporated and what is the sediment distribution that is under effect.
The caption of figure 5 and the result section associated with these results (L287) have been revisited to clarify how fine particles are incorporated in the beach profile.
L283: Is there anything that can be said about this process based on side observations, when compared to final trench dig?
We believe that section 4.2 (previously 4.1) comprehensively covers relevant discussions based on our observations of fine particle infiltration. Expanding on additional aspects would be speculative and beyond the scope of our data.

Round 3 Review

Recommendation by Associate Editor 2 ("Matthieu Giaime")

Completed: 2026-05-16 08:03 AM
Recommendation: Accept

Dear authors,
I read with interest your manuscript entitled " Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments ".
I thank you for your clear and appropriate answer to reviewers and I would like to recommend for the manuscript to be accepted.
Best wishes
Anonymous Associate Editor

Decision by Editor-in-Chief (Alice Lefebvre)

2026-05-28

Dear Léo Pancrazzi, Dominique Mouazé, Pierre Weill, Bernadette Tessier,

Congratulations! I am pleased to inform you that your manuscript entitled "Sand-fraction transport and mixing in a sand-and-gravel beach face: Insights from wave flume experiments" has been accepted for publication in Geomorphica.