Review Report for

On the Attraction of Marine Dunes to OWF Fixed Foundations

Noémie Durand, Pablo Tassi, Olivier Blanpain, Alice Lefebvre

*Corresponding author: noedurand@openbook.team
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Table of Contents:

Round 1 Review

Comments by reviewer 1 (anonymous)

Completed: 2025-01-14 12:34 AM
Recommendation: Minor revisions

For author and editor

The authors present a comprehensive modelling study of an important topic which is the migration of marine dunes. The impact of windfarms on dunes is also assessed for one particular case example of Dunkirk. This is an important topic for environmental assessment and probably has practical implications for engineering of foundations and cables.

The paper is well written and illustrated and there are some points of clarification to be confirmed or considered in a slightly wider context within the paper.

The model set up is described in Section 2.2. The results of the model performance for hydrodynamics appear to be justified by a companion paper that is unpublished (line 143)? Is this essential to the current modelling paper?

How was the 345 micron sand size selected (Table 1) from the range of 240-450 micron (line 105)?

The fine-medium sand and current speeds of 0.85 to 1.2 m/s should produce a mobile bed with dune migration. In Table 1 it is stated that the transport is computed by bedload only. What is the evidence this dominates at the site? Would suspended sediment be expected during peak currents? Can the authors include some basic sediment mobility calculations to illustrate what we might expect to be the dominant processes?

Also it is noted in Table 1 the slope effect is not turned on in the sediment transport/morphology – can the authors include a justification for this decision please? Normally it is expected that slope terms are required for stable morphological model results; maybe this is not the case here? Why would that be the case?

In Section 2.3 is introduced the method for including the turbine foundation – line 154-155. This allows the monopiles to act as obstructions to both flow and seabed sediment. Please include another line of explanatory text to clarify exactly how the process is implemented and how it acts to exclude all flow and sediment throughout the water column.

In the exploratory cases modelled with results in Figure 4 and 5, as the bathymetric field changes is there a feedback on the change, and this causes the instability to grow or dampen? Once the changes have been initiated does this cause a widening of the impact across the flow axis?

How long does it take for the impact to start, is it the first tide after the pile is wished in place in the model, or does it need to accumulate over a period of months and years before it becomes noticeable?

On the baseline simulations Figure 9 and 10, can the measured positions of the dune crests marked in white crosses on Figure 6 and 7 be added also to Figure 9 and 10? We can then see how well the model performs. On Figure 6 and 7 is there any data for measured dune crest positions on the right side of 0 m on the distance axis in the shallower water?

Regarding the schematic diagram of the dune crest wake effect in Figure 11, what is the expansion rate laterally of the wake? Is it governed by the hydrodynamic wake lateral expansion initially and then widens due to some feedback processes from the bedform perturbation?

Are the pink areas of accelerated transport (line 397 and Figure 12) indicating scouring processes? How would the wake effect in Figure 11 be disrupted by local scouring around the foundation? This is an important topic to be mentioned for further analysis.

In line 466 there is mentioned wake interference – this is aerodynamic and needs to be clarified that it is different than the dune crest wake effect covered in the modelling. Or do the authors refer here to the dune wake effect – it is confusing because rotor diameter is included as a length scale?

In Figure 14 what causes the wider stripes of change, is it mainly due to the alignment of the foundations in a row or due to sediment grade making some areas less susceptible to the effect or there are less marine dunes in the first place? Presumably it is important to state that wake effects only can be expected in locations with existing dune fields so these can be mapped such as in Figure 3a of the paper; areas outside the areas of marine dunes will not be expected to have the same wake effect?, unless dune fields are expanded by this effect? Figure 8(d) has no dunes and hence no wake effect for example compared to Figures 8(b) and 8(c) with marine dune fields. Perhaps the authors could comment on this.

It is not clear exactly what the different assumptions are for the model conditions for Case II (a+b) and Case II; (a +b) in Figure 14 (a) caption compared to Figure 14 (b). Please ensure clarification on how the modelling is different in Section 4.3. Why does it mention only half the turbines at line 469 when a 46 turbine array is mentioned at line 465? 46 foundation locations are plotted in each of Figure 14 subplots.

In Figure 14 the differences between the two plots of sediment transport rate are really very similar that it is difficult to see what the key message is here. The first order moment of sediment transport is plotted? How does this actually indicate the wake effect referred to Figure 11. At line 215 the authors state the first order transport rate averaged over 360 days indicates the area of influence of the monopile. How does this actually correlate to the wake effect? What do differences in sediment transport rate in Figure 14 tell us about the wake effect? This is also presented at line 394 where changes in the first order transport rate are said to distort the plan shape of the dunes.

Line 485 typo error – rewrite as ‘while the rest of the dunes move at their natural…’

Comments by reviewer 2 (anonymous)

Completed: 2024-12-06 10:55 AM
Recommendation: Major revisions

For author and editor

Dear editor and authors,

I have read with interest the manuscript entitled “On the Attraction of Marine Dunes to OWF Fixed Foundations”. In this paper a large scale 3D TELEMAC model is applied to simulate the natural marine dune system and a system in which various layouts of an offshore wind farm, consisting of monopiles, are constructed. These simulations are compared to assess the impact of monopiles on marine dune dynamics.

I think the interaction between offshore windfarms and marine dunes is a topic that is interesting for a wide audience and very relevant given the accelerations in offshore wind development. Moreover, as the authors mentioned, this is a topic which has not received much attention as of yet.

The authors tried to implement a full offshore wind farm in their 3D model including marine dunes. However, in opting for this size of the model I believe the authors may have made concession in other areas, which is clear from various non-physical behaviours in the model results. The implications of which might jeopardise the accuracy of the results and have not always been fairly communicated. The authors are not showing the ability of their model to reproduce the fundamentals of the marine dune system (the balance between circulation-driven and slope-driven sediment transport) and in one case have actively left out important processes (slope-driven transport). Furthermore, they are using a simplified representation of the monopiles (triangular dry cells of varying sizes), without validating the effects of these simplifications on the hydrodynamics in the model. These limitations are not included in the discussion of the manuscript. Moreover, the manuscript consists of a wide range of cases, which are not always clearly described and connected to each other. Therefore I have some strong doubts about the validity of the model result and believe that the following major concerns need to be addressed before considering this manuscript for publication.

My major concerns are the following:

1. The authors have chosen to exclude slope-induced sediment transport from their simulations, while this is a fundamental process necessary to represent the sand wave system. As described in Borsje et al. (2014), the equilibrium of marine dunes is determined by the balance between bed load transport driven by circulation cells and slope-induced transport (when suspended transport is negligible, like in this study). The authors have thus actively left out the balancing mechanism. I do not see significant growth of the sand waves. However this might have to do with either the duration of the simulations (growth is just starting up) or it might signal that the circulation cells, which cause marine dune growth, are also not well represented in the simulations. Since the model only applies 5 vertical layers, of which the

distribution is not commented upon, I would not be surprised if it is unable to recreate this mechanism. For these systems usually a minimum of 20 vertical layers is applied. I suggest that the authors include (longitudinal and transverse) bed slope effects in all their simulations. Without these processes I do not see how the system can be well represented. Moreover, the authors should do a sensitivity study to see if the circulation cells (under purely tidal forcing) are well represented while using this little layers (compare the results to simulations with more vertical layers). This can be included in the supplementary materials.

1. To represent the monopiles in the simulation the authors have opted for a method using dry cells. This method is well known and is a very intuitive way to include such structures. However, the authors are not adapting the grid surrounding the monopile and thus end up with triangular monopiles of different sizes throughout the model domain. Since the authors are using an unstructured grid I do not understand why it is not refined to create a more round structure like in Christiansen et al. (2023). Since a non-hydrostatic approach is used, the grid can easily be refined, while the model assumptions remain valid (unlike in the mentioned study). The fact that the monopile is triangular may add a dependency of the results on the angle of incidence of the current with respect to the triangular grid cell. Moreover, the authors do not show how well the model represents the turbulent wake and reduced flow velocity behind the monopile, but skip straight to sediment transport and morphology. Comparison of this wake with theory may show whether the model is able to reproduce these processes. I would suggest adding a figure showing the velocities and turbulence for one monopile. I fear that inaccuracies in this representation can be part of the reason for the unnatural accumulation of sediment on the tidal axis right next to the monopile. This accumulation directly jeopardises the validity conclusions of the study, since it can be part of the cause for the reduced migration rates of marine dunes behind the monopile.

1. In essence the authors are applying 3 variations in their cases: natural state, a single monopile and a full array of monopiles. This is an interesting setup to assess both the impact of a single monopile and the interactions between monopile wakes when constructing a full wind farm. However, the single and multiple monopile cases are studied at different locations and no real comparison between them is made. Instead the same plots are shown at the new locations of the windfarm (Figure 9 and 10 vs. Figure 6 and 7), but in my eyes they do not contribute to the understanding of the system without intercomparison. Moreover, in the current setup the wide range of cases (IA, IB, II, II*, IIA, IIB) and different locations (tile 1,2,3, site 1,2,3) is confusing.

I hope to be able to see a revised version of the manuscript. Below I have also included a list of minor comments and some suggestions for adapting the representation of the results in the figures.

Minor comments:

- L8: off -> offshore of/off the coast of

- L51-53: You state there are no studies and then you continue with an example of one such a study. I think you should rephrase this (e.g. ‘limited studies’) and since there are limited studies on these time and spatial scales (and no space covering studies) have you found any other evidence or hypothesis of effects on these scales?

- L73: please repeat the knowledge gap here, now it is unclear.

- L89-L93: Please include references to where the data can be retrieved (where possible)

- L112-115: The information about the sand waves is in my opinion too limited, since this is the focus of the study. I am left wondering: what is their wavelength? Are they very regular or irregular? How steep are they? What is their shape? (asymmetric vs. symmetric? Pointy or round crest?) Do they cover the full area? Do their characteristics and dynamics change between the tops and gullies of the sand banks? Please elaborate a bit more.

- L125: Here Tassi et al. (2023) is referenced for the empirical sediment transport rates, while in the supplementary materials the Soulsby-Van Rijn equation is included. Are you using the Soulsby-Van Rijn equations? Then you should also name this here and include the corresponding reference.

- L132: Since you are using a (finest) grid size of 10 m it is very important to know the wavelength of your sand waves as a reader (since this determines whether the resolution is fine enough to really include the sand waves). Please also include an overview of the mesh sizes for example in the supplementary materials.

- L132: What areas do you mean with the areas of interest? The repeat survey polygons?

- 133-134: Does the DTM include the sand waves or are these filtered out since in some areas the model is not fine enough to resolve them?

- L151-160: Does the flow experience drag (wall roughness) of the removed element? And if so: what is the roughness of this wall? If not: do you think it is a valid approximation to exclude this?

- Table 2: how is the effective pile diameter calculated? Based on volume? Is this based on literature?

- Line 181: do you mean 22 m diameter for the monopiles? 220 m seems a bit large…

- Line 189-194: Why not just use one grid for all simulations and include single monopiles at two different locations followed by an array of monopiles including the locations studied earlier with a single monopile. Why two different case 2 options (with and without monopiles at the edge).

- Section 2.3.3: why immediately go to sediment dynamics? In my eyes you are skipping a step: first hydrodynamics.

- Line 227: that is incorrect: the white areas indicate where the monopiles do not alter the erosion/accretion, which may be a result of unaltered sediment pathways, but can also happen with altered sediment pathways.

- Line 231-236: Have you tested how well this spline method is able to predict the crest location? For example by downsampling the measurement data (assuming that the measurement resolution is higher than 10 m). It would be interesting to show this in the supplementary materials, also to see how well the sand waves are represented with this grid resolution.

- Line 241-242 and caption Figure 4 and 5: please explain more clearly which difference we are looking at: is this without monopile **–** (minus) with monopile or the other way around? So does positive mean more accretive in the monopile situation? The order in which you name the two cases changes between the text and the caption.

- Caption Figure 5: what do you mean by ‘on the tidal axis’? Are you removing the changes elsewhere from these figures? If so: please indicate the area for which the results are plotted in the figure.

- Line 254: is 4 pile diameters sufficient to be outside of the local wake which is not accurately represented in the simulation? I would say the largest accretion not directly connected to the monopile is between 2.25 and 2.75 m (not 5 m) from Figure 4. Same in line 265-266

- Line 255: I would say you can conclude that movement of the dunes is taking place due to the sedimentation erosion pattern, not solely due to the accretion and erosion heights. Maybe you can indicate the crest locations in the initial bathymetry on Figure 4 and 5 (and Figure 8), this would give significantly more insight into what is happening with the dunes. From the reversing pattern you can then already see that they are moving towards the monopile relative to the baseline case.

- Figure 6 and 7 (and 9 and 10): Can you add a line tracking the crest positions in the natural and monopile situation? This will give more insight in A) how well the model is able to represent this migration in the natural case (there seems to be and overestimation w.r.t. the measurements in some cases) and B) better show the effects on the monopile installation on the migration

- Line 281-286: Please move the explanation of the values for the acceleration rate to be before the values. I was left confused over what -164% means.

- Figure 6 and 7 (and 9 and 10): you are without a doubt aware of the instabilities of the results. Regarding this I have two suggestions:

    - Did you filter the superimposed bedforms (for which the model resolution is not sufficient) from the bathymetry before applying it to the model? If you still have these disturbances in there, they might grow into bigger instabilities, especially since there is no bed slope effects in the model to counteract those.

  - Are you using an upwind or central scheme for the bed load calculations? Upwind schemes (based on current directions) may lead to growing instabilities in 3D models (see Volp et al., 2016)

- Line 292: Are the numerical instabilities present in both simulation runs (with and without monopiles) or only in the one with monopiles? And what is the size of the instabilities? This cannot be judged from the difference plot alone.

- Line 297-312: Instead of describing the sand waves here I would suggest adding a bathymetry figure and description in the method section. Especially an overview figure can tell much more than just textual description.

- Line 321: So this can be used as a distance to calculate the effects on marine dunes: beyond 15-33 pile 'diameters'

- Figure 11: nice figure!

- Line 382: what is consistent with Couldrey? Did they report attraction of the dunes towards the monopile in the far field?

- Line 385: This acceleration of the dunes on the side must be very local then? I do not see this in the model results.

- Line 394-397: Why not show the plan form at beginning and end for one monopile?

- Line 422: Which temporal evolution plots (in Section 2) do you mean? I cannot find them. Please include figure numbers.

- Figure 13: why use a different definition of migration rate here? And why compare for different areas? These values are not directly comparable. I would suggest including the results from Table 4, since these are the same location between natural and including monopile. Please plot an additional line at 0 migration speed to indicate reversal more clearly. And maybe add the variability as a shaded area.

- Line 436: again which plots?

- Figure 14: Please indicate the locations of interest on the plot itself

- Figure 14: why is everything negative (green)? Wouldn't an increase in migration rate be associated with an increase in the first order (tide averaged) sediment transport?

- Line 485: I would say outside of the wake or tidal axis instead of 'other areas', this is clearer.

- Line 489: "*This results in an increase in the speed of dune migration in the dominant direction and a reduction (or even reversal) in the subordinate direction*." I do not find it completely clear what you mean here (also in the abstract). You mean the direction with respect to the monopile but you are not explicitly saying this. I would say increased migration rates when dunes are approaching the monopile and decreased migration rates when migrating away.

- Line 493: this is the first time you mention this factor 2 to 4. How did you derive this factor? The largest migration rate deficits you are reporting in the text is -171%. This is not a factor 4. And you are also reporting deficits much lower than a factor 2. You cannot include new information in the conclusion (and abstract). This should already be presented (together with how it is derived) in the results or discussion.

I would suggest the addition of 2 figures instead of the repetition (Figure 9 and 10) of the blue green timestack plot:

- For the same transects included in the timestack plots: Initial bed level along the transect combined with final bed level for the different cases (natural, single monopile in middle of transect and monopile in middle + array of monopiles surrounding). In this way the effects of a single monopile and an array of monopiles can easily be compared

- It would be nice to plot the deviation in migration rate versus the distance to the monopile for (one of) the transects, either in absolute or relative terms, instead of representing those in a table in the supplementary materials. This can be done for both the single and full array of monopiles (along the same transect). This gives more insight in how the influence of the monopile changes with distance.

References

Borsje, B. W., Kranenburg, W. M., Roos, P. C., Matthieu, J., & Hulscher, S. J. M. H. (2014). The role of suspended load transport in the occurrence of tidal sand waves. *Journal of Geophysical Research: Earth Surface*, *119*(4), 701-716. https://doi.org/10.1002/2013JF002828

Christiansen, N., Carpenter, J. R., Daewel, U., Suzuki, N., & Schrum, C. (2023). The large-scale impact of anthropogenic mixing by offshore wind turbine foundations in the shallow North Sea. *Frontiers in Marine Science*, *10*, 1178330. https://doi.org/10.3389/fmars.2023.1178330

Volp, N. D., Van Prooijen, B. C., Pietrzak, J. D., & Stelling, G. S. (2016). A subgrid based approach for morphodynamic modelling. *Advances in Water Resources*, *93*, 105-117. https://doi.org/10.1016/j.advwatres.2015.07.013

Recommendation by Associate Editor 2 (Rocío Luz Fernández)

Completed: 2025-01-15 02:51 PM
Recommendation: Major revisions

For author and editor

Dear Authors,

I have read your manuscript entitled *“**On the Attraction of Marine Dunes to OWF Fixed Foundations**”* with great interest, as well as the detailed reviews provided by two anonymous reviewers. Both reviewers consign the importance and relevance of your work to offshore wind farm development. However, both reviewers have identified key methodological and presentation issues that require attention to make the study more robust and clearer. They emphasize the need to include (or thoroughly justify the exclusion) of slope-driven sediment transport, as this omission has significant implications for the validity of your model results. Additionally, both reviewers highlight areas where the clarity of figures, intercomparison of cases, and validation of modeling choices could be improved.

Based on my evaluation of your manuscript and the reviewers’ feedback, I recommend that the manuscript undergo **major revisions**. I encourage you to carefully address the reviewers’ comments to enhance the impact of your appealing study.

Best regards,
Anonymous Associate Editor

Decision by Editor-in-Chief (Jackie Cockburn)

2025-01-15

We would like you to undertake a **major revision** of your manuscript.

Authors' responses to review comments

2025-09-01

Responses to Reviewers' Comments for Manuscript 44

On the attraction of marine dunes to OWF fixed foundations

Addressed Comments for Publication to

Geomorphica

Dear editor,

Please find enclosed the revised version of our previous submission entitled " On the attraction of marine dunes to OWF fixed foundations" with manuscript number 44. We would like to thank you and the reviewers for the careful reading and constructive comments, which helped improve the quality and clarity of our manuscript. In this revision, we have carefully addressed the reviewers' comments. A detailed point-by-point response to the comments from Reviewers A and D is given below.

With best wishes,

The authors

Note: To improve the readability of this response letter, all the comments made by the editors and the reviewers have been formatted in blue boxes. Where appropriate, reworded or added text is in grey boxes.

Authors' Response to Reviewer A

General Comments. The authors present a comprehensive modelling study of an important topic which is the migration of marine dunes. The impact of windfarms on dunes is also assessed for one particular case example of Dunkirk. This is an important topic for environmental assessment and probably has practical implications for engineering of foundations and cables.
The paper is well written and illustrated and there are some points of clarification to be confirmed or considered in a slightly wider context within the paper.

Response: Thank you for your positive feedback.

Comment 1

The model set up is described in Section 2.2. The results of the model performance for hydrodynamics appear to be justified by a companion paper that is unpublished (line 143)? Is this essential to the current modelling paper?

Response: Thank you for your comment.

Model validation is essential to substantiate the modelling exercise presented in the current paper. Hydrodynamic and morphodynamic model validation follows well-established methods. Since the focus of this paper is the effect of OWF fixed foundations on the migration of marine dunes, we refer readers to the now published paper by [1] for calibration details, but key metrics have been added in Table 1.

> The hydrodynamics were calibrated and validated by comparing the model predictions with site-specific level and current data at 7 locations across the computational domain (EMD, 2022; FEM, 2021a; Shom, 2017a; VLIZ, 2022b), achieving 'excellent' performance according to the Sutherland et al. (2004) classification scheme (see Ta-

ble 1 for key metrics). (...) The energy-based Soulsby-van Rijn predictor (Soulsby, 1997) was selected over shear-stress predictors, without slope correction, which caused unrealistic dune decay (anonymised reference). This approach successfully reproduced the dune migration patterns observed in recent surveys (FEM, 2021b), validating the model (Durand et al., 2025; Table 1 for metrics).

[1] N. Durand, P. Tassi, O. Blanpain, and A. Lefebvre, "Meteorological conditions influence the migration of a marine dune field in the southern north sea," *Journal of Geophysical Research: Earth Surface*, vol. 130, no. 1, e2024JF007731, 2025. DOI: `https://doi.org/10.1029/2024JF007731`.

Comment 2

How was the 345 micron sand size selected (Table 1) from the range of 240-450 micron (line 105)?

Response: Thank you for your comment.

A total of 19 bed samples were collected at the site in October 2019 and again in May 2020 (FEM, 2020). The samples were analysed by sieving to provide information on the surficial grain size distribution. There is little evidence of spatial or seasonal trends from these limited data. The majority of samples (89%) have a unimodal distribution. The sediments are reasonably well-sorted slightly gravelly and gravelly sands (USCS) with median grain sizes d_{50} between 240 μm and 450 μm. These grain size parameters are in general agreement with the data compiled by the Shom in recent years [2]. For modelling purposes, at a large scale, we decided to use a single sediment class and a representative median grain size (345 μm) was considered.

This was clarified in a sentence at the end of the 1st paragraph in Section 2.2 (see grey box in response to Comment 3).

[2] Shom, *Sedimentological data produced as part of pre-feasibility studies for the Dunkirk offshore wind farm area*, `https://www.mongeosource.fr/geosource/1111/fre/catalog.search#/metadata/2a0ca78b-514d-425b-beb8-328469fef861`, 2021.

Comment 3

The fine-medium sand and current speeds of 0.85 to 1.2 m/s should produce a mobile bed with dune migration. In Table 1 it is stated that the transport is computed by bedload only. What is the evidence this dominates at the site? Would suspended sediment be expected during peak currents? Can the authors include some basic sediment mobility calculations to illustrate what we might expect to be the dominant processes?

Response: Thank you for your comment.

Bedload dominance is demonstrated by Rouse parameter analysis (P > 5 throughout the computational domain) in [3]. Shear velocities at the site during peak flows ($u_* \sim$ 3.9 cm/s) remain below the settling velocity for 345 μm sand ($w_s \sim$ 4.4 cm/s, [4]). This satisfies the suspension criterion ($u_* > w_s$) of [5], confirming bedload dominance across all flow conditions. These calculations are now included in the revised manuscript (Section 2.2, Table 1).

A single, representative sediment class ($d_{50} = 345$ μm) was adopted, reflecting the in situ surficial bed samples that showed a dominant unimodal distribution without spatial or seasonal trends. For this grain size, shear velocities during peak flows ($u_* \sim$ 3.9 cm/s) remain below the settling velocity ($w_s \sim$ 4.4 cm/s; Soulsby, 1997), and Rouse parameter analysis ($P > 5$; Durand et al., 2023a) further confirms bedload dominance in the area. These findings, consistent with the presence of marine dunes, justify excluding suspended load from the model.

[3] N. Durand, P. Tassi, O. Blanpain, and A. Lefebvre, “Understanding marine dune dynamics in a shallow shelf sea using sediment mobility indices,” in *Marine and River Dune Dynamics VII Book of Proceedings*, April 1-3, 2023. Rennes, France, 2023a, pp. 109–116.

[4] R. L. Soulsby, *Dynamics of marine sands*. Thomas Telford Publishing, 1997. DOI: `https://doi.org/10.1680/doms.25844`.

[5] J. Fredsøe and R. Deigaard, *Mechanics of Coastal Sediment Transport*. World Scientific, 1992. DOI: `https://doi.org/10.1142/1546`.

Comment 4

Also it is noted in Table 1 the slope effect is not turned on in the sediment transport/morphology – can the authors include a justification for this decision please? Normally it is expected that slope terms are required for stable morphological model results; maybe this is not the case here? Why would that be the case?

Response: Thank you for your comment.

The local seabed slope can influence both the magnitude and the direction of bedload transport. The transport rate can be corrected accordingly and is commonly applied to stabilise morphodynamic models (particularly those based on shear stress formulations). Our model uses the Soulsby-van Rijn sediment transport predictor, which is energy-based (depth-averaged velocity) rather than shear stress-based. The only theoretically consistent slope correction [6] introduces a 2nd order bed elevation derivative, acting as a diffusion term.

Simulations with this correction (using default parameters) produced unrealistic dune decay. Given this behaviour and the model’s validated performance against field observations [1], we disabled the slope correction to maintain the realistic dune migration patterns captured by our energy-based approach. This is now explained in the updated manuscript (see grey box in response to Comment 1).

[6] F. G. Koch and C. Flokstra, “Bed level computations for curved alluvial channels,” in *Proceedings of the XIXth Congress of the Int. Ass. for Hydr. Res.*, February, 1981. New Delhi, India, 1980.

Comment 5

In Section 2.3 is introduced the method for including the turbine foundation – line 154-155. This allows the monopiles to act as obstructions to both flow and seabed sediment. Please include another line of explanatory text to clarify exactly how the process is implemented and how it acts to exclude all flow and sediment throughout the water column.

Response: Thank you for your comment.

We have expanded the original description (lines 154-155) to read:

> It accurately captures their physical role as full water-column obstructions by creating a vertical null-flux boundary (through element removal) that enforces free-slip flow conditions and zero sediment flux across all depths.

Comment 6

In the exploratory cases modelled with results in Figure 4 and 5, as the bathymetric field changes is there a feedback on the change, and this causes the instability to grow or dampen? Once the changes have been initiated does this cause a widening of the impact across the flow axis?

Response: Thank you for your comment.

The bathymetric changes are indeed coupled with the flow field in the model. As the bathymetry evolves, it modifies local flow patterns, which in turn influence sediment transport and further bathymetric change through morphodynamic feedback.

If by "instability" the reviewer is referring to the exaggerated accumulation of sediment in front of the monopile, we believe these features are primarily related to mesh resolution limitations. While the current resolution is appropriate for capturing large-scale dune migration patterns, it is insufficient to resolve near-foundation flow and sediment transport processes. We have added explicit discussion in the revised manuscript (Section 4.4).

In Figures 4 and 5, the patterns of erosion and deposition are a direct consequence of the evaluation of the divergence of the solid flux in the Exner equation, which is a function of the computed flow field. These patterns do not indicate a widening of the impact across the flow axis, but reflect the response of the sediment transport model to flow perturbations in the presence of a monopile.

Comment 7

How long does it take for the impact to start, is it the first tide after the pile is wished in place in the model, or does it need to accumulate over a period of months and years before it becomes noticeable?

Response: Thank you for your comment.

The flow perturbation and initial bedform response begin immediately after the monopile is introduced in the model. However, the distortion of the dune planform develops progressively over time, as shown in the temporal evolution figures (e.g., Figure 6). We have clarified this in the revised manuscript.

Comment 8

On the baseline simulations Figure 9 and 10, can the measured positions of the dune crests marked in white crosses on Figure 6 and 7 be added also to Figure 9 and 10? We can then see how well the model performs. On Figure 6 and 7 is there any data for measured dune crest positions on the right side of 0 m on the distance axis in the shallower water?

Response: Thank you for your comment.

Figures 9-10 present the simulation results based on the foundation layout of the Dunkirk OWF plan. These simulations do not include monopiles within the repeat survey areas, meaning there is no spatial overlap with the measured data shown in Figures 6-7 for the exploratory simulations. The only surveys covering the full OWF extent are from 2016–2017 (Shom, 2017b) and in June 2021 (EMD, 2021). While crest positions could potentially be extracted from these full-extent surveys for comparison with Figures 9-10, the effort would provide limited scientific value because of the absence of intermediate temporal data points. As the project has now concluded, additional data processing is impractical at this stage.

Additional clarifications:

- Figure 6: The extracted data focus on the large marine dunes. East of the last white crosses, the bedforms are smaller (heights < 1 m). Please note that we have added a dedicated panel showing tracked crest positions to enable clear comparison between the natural baseline scenario (brown line) and the monopile scenario Case IA (orange line), with field measurements indicated by crosses (FEM 2021b).
- Figure 7: A monopile was placed within the OWF footprint (in the detailed survey area chosen for its validated natural-state conditions) for experimental purposes. The turbine was positioned at the edge of the survey area, ahead of a train of three marine dunes. This enables assessment of monopile-induced effects on dune migration but restricts observational data to the west of the foundation.

- Figures 7, 9, and 10 have now been moved to the Supplementary Materials to streamline the main manuscript.

Comment 9

Regarding the schematic diagram of the dune crest wake effect in Figure 11, what is the expansion rate laterally of the wake? Is it governed by the hydrodynamic wake lateral expansion initially and then widens due to some feedback processes from the bedform perturbation?

Response: Thank you for your comment.

Figure 11 is intended as a schematic representation to illustrate the conceptual mechanism of dune-monopile interactions, rather than a quantitatively accurate depiction of expansion dynamics. The lateral expansion of the wake is governed initially by the local reduction in flow velocity downstream and outward of the pile. Over time, morphodynamic feedback processes reshape the bathymetry and further influence flow patterns. As a result, the longer-term extent of the wake may be influenced not only by the initial hydrodynamic wake but also by bedform evolution.

Comment 10

Are the pink areas of accelerated transport (line 397 and Figure 12) indicating scouring processes? How would the wake effect in Figure 11 be disrupted by local scouring around the foundation? This is an important topic to be mentioned for further analysis.

Response: Thank you for your comment.

The pink lobes shown in Figure 12 do not represent local scouring, but rather areas of accelerated sediment transport, likely driven by flow acceleration around the monopile. These features reflect enhanced bedform mobility rather than erosion of the seabed itself.

Regarding the potential disruption of the wake effect by local scouring around the foundation, we agree that this is a crucial aspect to consider for a comprehensive understanding of the sedimentary processes around offshore wind turbine foundations. Recent studies, such as [7], provide valuable insights into the coupled hydrodynamic and morphodynamic interactions around foundations, including wake–scour dynamics observed through satellite imagery and in-situ measurements. For the purposes of the current paper, the level of detail in representing the wake was constrained by the scope and objectives.

We have expanded the discussion in the new Section 4.4 (Remarks about the design of the study) of the revised manuscript.

[7] L. P. Bailey, R. M. Dorrell, I. Kostakis, *et al.*, "Monopile-induced turbulence and sediment redistribution form visible wakes in offshore wind farms," *Frontiers in Earth Science*, vol. Volume 12 - 2024, 2024. DOI: 10.3389/feart.2024.1383726.

Comment 11

In line 466 there is mentioned wake interference – this is aerodynamic and needs to be clarified that it is different than the dune crest wake effect covered in the modelling. Or do the authors refer here to the dune wake effect – it is confusing because rotor diameter is included as a length scale?

Response: Thank you for your comment.
The term "wake interference" in Line 466 refers specifically to the dune wake effect (hydrodynamic/sedimentary) discussed in our study, not aerodynamic wake effects from the turbine.

However, we appreciate the potential confusion between aerodynamic wake interference and dune wake effect and we agree that the inclusion of rotor diameter as a length scale could be misleading in this context. To avoid ambiguity, we have removed the words "(just under 5 rotor diameters)" and now simply state the absolute spacing (1000 m).

Comment 12

In Figure 14 what causes the wider stripes of change, is it mainly due to the alignment of the foundations in a row or due to sediment grade making some areas less susceptible to the effect or there are less marine dunes in the first place? Presumably it is important to state that wake effects only can be expected in locations with existing dune fields so these can be mapped such as in Figure 3a of the paper; areas outside the areas of marine dunes will not be expected to have the same wake effect?, unless dune fields are expanded by this effect? Figure 8(d) has no dunes and hence no wake effect for example compared to Figures 8(b) and 8(c) with marine dune fields. Perhaps the authors could comment on this.

Response: Thank you for your comment.

The wider stripes of change observed in Figure 14 are primarily the result of wake interference, where the spacing between neighbouring devices is exceeded and individual wakes begin to overlap. This is particularly evident in areas such as the gully between the Binnen Ratel and Dyck Oriental sandbanks (Figure 8(b)), where the regular row-wise arrangement of turbines leads to lateral interactions between wake zones, producing wider stripes of altered sediment transport.

These patterns are not directly caused by sediment grain size or the absence of dunes, although such factors may influence the magnitude of the morphodynamic response locally.

We associate wake effects with areas where mobile bedforms, such as marine dunes, are present. To clarify this, we have added the dune field outline from Figure 3(a) to Figure 14. This visual reference helps distinguish regions where wake-induced morphodynamic changes are likely from those where the seabed is more stable or featureless. For example, as the reviewer notes, Figure 8(d) shows no dunes and correspondingly little wake effect, in contrast to Figures 8(b) and 8(c), which intersect active dune fields.

While the current study does not explore the possibility of dune field expansion due to wake effects, we acknowledge this as an interesting avenue for future research.

Comment 13

It is not clear exactly what the different assumptions are for the model conditions for Case II (a+b) and Case II; (a +b) in Figure 14 (a) caption compared to Figure 14 (b). Please ensure clarification on how the modelling is different in Section 4.3. Why does it mention only half the turbines at line 469 when a 46 turbine array is mentioned at line 465? 46 foundation locations are plotted in each of Figure 14 subplots.

Response: Thank you for your comment.
The scenarios referred to as Case IIA and IIB (half-array configurations) were originally introduced in the "Materials and methods" section to investigate the implications of turbine placement. These cases simulate arrays comprising only half the number of wind turbines, with turbine locations selected to avoid overlapping effects. To improve clarity, this paragraph has now been moved to a more appropriate position within the same section, where it is better contextualised.

Additionally, Table 3 has been expanded to include the characteristics of all OWF scenarios, including Cases IIA and IIB, making the differences in model setup more transparent.

Regarding the mention of "only half the turbines" at Line 469, this refers specifically to the half-array configurations (Cases IIA and IIB), whereas Line 465 refers to the full array of 46 turbines used in Case II. We have revised the text in Section 4.3 to clarify this, and to explain the rationale behind the half-array configurations.

> To isolate and better understand these interactions, we introduced two additional scenarios: Cases IIA and IIB, each representing half-array configurations. These subsets were designed to avoid overlapping effects a priori, allowing clearer attribution of individual monopile impacts. The cumulative effect of these two half-array cases is shown in Figure 11(a), and compared with the impact predicted for the entire array in Figure 11(b)). The difference between these configurations highlights the non-linear nature of wake interference and sediment redistribution across the array.

Comment 14

In Figure 14 the differences between the two plots of sediment transport rate are really very similar that it is difficult to see what the key message is here. The first order moment of sediment transport is plotted? How does this actually indicate the wake effect referred to Figure 11. At line 215 the authors state the first order transport rate averaged over 360 days indicates the area of influence of the monopile. How does this actually correlate to the wake effect? What do differences in sediment transport rate in Figure 14 tell us about the wake effect? This is also presented at line 394 where changes in the first order transport rate are said to distort the plan shape of the dunes.

Response: Thank you for your comment.

Figure 14 presents the difference in the first-order moment of sediment transport, averaged over 360 days, between the combined half-array scenarios (Figure 14a) and the full-array scenario (Figure 14b). These comparisons are intended to highlight the non-linear nature of wake interference and sediment redistribution across the array.

To guide the reader, we attempted to annotate specific locations of interest directly on the plots. However, due to the density and overlap of features, such annotations significantly reduced figure legibility and were therefore omitted.

The first-order moment is used here as a proxy for the hydrodynamic wake (i.e., the velocity deficit) shown in Figure 11. This connection follows from the formulation of the sediment transport predictor used in the model (Soulsby-van Rijn, 1997), which is based on depth-averaged velocity. Since the wake effect reduces local flow velocities downstream of the monopile, it directly lowers the sediment transport capacity in those regions. The first-order moment, which reflects the magnitude of sediment transport over time, captures this reduction. As a result, areas of reduced first-order moment correspond spatially to the wake zones, providing a meaningful and morphodynamically relevant representation of the wake's influence.

We have revised Section 2.3.3 to clarify this methodological link and added explanatory notes to both the figure caption and Section 4.3 to guide interpretation.

> To visualise the area of influence of the monopile(s) on sediment dynamics and morphological processes, the first-order moment is used as a proxy for the hydrodynamic wake (i.e., the velocity deficit). This connection follows from the formulation of the sediment transport predictor used in the model (Soulsby–van Rijn, 1997), which is based on depth-averaged velocity. Since flow velocities are locally reduced downstream of the monopile, sediment transport capacity is directly lowered in those regions. The first-order moment, which reflects the magnitude of sediment transport over time, captures this reduction. Consequently, areas of reduced first-order moment correspond spatially to the wake zones, providing a meaningful and morphodynamically relevant representation of the wake's influence. To compute this, the sediment transport rate (a vector) is extracted at hourly intervals from simulations conducted both with and without wind turbine foun-

dation(s). At each spatial point and time step, we compute the vector difference between the two scenarios. The norm of this difference is then assigned a sign: a positive value indicates an increase in transport magnitude due to the monopile, while a negative value indicates a decrease—regardless of the direction of transport. This sign convention is intended to reflect the intuitive velocity deficits observed in the wake of monopiles, where transport may be reduced during one tidal phase (e.g., flood), even if the net transport over the full cycle is enhanced. The use of a 360-day averaging period ensures that the moment calculation captures longer-term morphodynamic trends. Higher-order moments (variance, skewness, and kurtosis) were also calculated but are not discussed here.

Comment 15

Line 485 typo error – rewrite as 'while the rest of the dunes move at their natural. . . '.

Response: Thank you for pointing out this typo.

We agree with the reviewer's comment and have revised the text on Line 485 as suggested.

Authors' Response to Reviewer D

General Comments. Dear editor and authors, I have read with interest the manuscript entitled "On the Attraction of Marine Dunes to OWF Fixed Foundations". In this paper a large scale 3D TELEMAC model is applied to simulate the natural marine dune system and a system in which various layouts of an offshore wind farm, consisting of monopiles, are constructed. These simulations are compared to assess the impact of monopiles on marine dune dynamics.

I think the interaction between offshore windfarms and marine dunes is a topic that is interesting for a wide audience and very relevant given the accelerations in offshore wind development. Moreover, as the authors mentioned, this is a topic which has not received much attention as of yet.

The authors tried to implement a full offshore wind farm in their 3D model including marine dunes. However, in opting for this size of the model I believe the authors may have made concession in other areas, which is clear from various non-physical behaviours in the model results. The implications of which might jeopardise the accuracy of the results and have not always been fairly communicated. The authors are not showing the ability of their model to reproduce the fundamentals of the marine dune system (the balance between circulation-driven and slope-driven sediment transport) and in one case have actively left out important processes (slope-driven transport). Furthermore, they are using a simplified representation of the monopiles (triangular dry cells of varying sizes), without validating the effects of these simplifications on the hydrodynamics in the model. These limitations are not included in the discussion of the manuscript. Moreover, the manuscript consists of a wide range of cases, which are not always clearly described and connected to each other. Therefore I have some strong doubts about the validity of the model result and believe that the following major concerns need to be addressed before considering this manuscript for publication.

I hope to be able to see a revised version of the manuscript. Below I have also included a list of minor comments and some suggestions for adapting the representation of the results in the figures.

Response: We thank the reviewer for raising these important points, which we have addressed point-by-point below. We believe this work makes significant contributions to understanding large-scale interactions between marine dunes and OWF fixed foundations, while being transparent about its limitations. The reviewer's comments have helped us better articulate these aspects in the revised manuscript.

Major concern 1

The authors have chosen to exclude slope-induced sediment transport from their simulations, while this is a fundamental process necessary to represent the sand wave system. As described in Borsje et al. (2014), the equilibrium of marine dunes is determined by the balance between bed load transport driven by circulation cells and slope-induced transport (when suspended transport is negligible, like in this study). The authors have thus actively left out the balancing mechanism. I do not see significant growth of the sand waves. However this might have to do with either the duration of the simulations (growth is just starting up) or it might signal that the circulation cells, which cause marine dune growth, are also not well represented in the simulations. Since the model only applies 5 vertical layers, of which the distribution is not commented upon, I would not be surprised if it is unable to recreate this mechanism. For these systems usually a minimum of 20 vertical layers is applied. I suggest that the authors include (longitudinal and transverse) bed slope effects in all their simulations. Without these processes I do not see how the system can be well represented. Moreover, the authors should do a sensitivity study to see if the circulation cells (under purely tidal forcing) are well represented while using this little layers (compare the results to simulations with more vertical layers). This can be included in the supplementary materials.

Response: Thank you for your comment.

The reviewer's concerns are now explicitly addressed in the new Section 4.4 (Remarks about the design of the study) of the revised manuscript, where we clarify the rationale behind our modelling choices and acknowledge their limitations.

- **Modelling approach and objectives:** To our knowledge, this represents one of the most comprehensive attempts to model large-scale dune-monopile interactions while maintaining computationally feasible runtimes. Our primary objective was to examine far-field effects (tens to hundreds of meters from foundations) rather than near-monopile processes, which guided several of our methodological choices.
- **Slope-induced transport:** We fully acknowledge the importance of slope effects in natural dune dynamics (Borsje et al., 2014). However, as detailed in Section 4.4, our initial implementation of standard slope corrections introduced unrealistic diffusion artefacts that compromised the large-scale migration patterns we aimed to study.
- **Vertical resolution:** The 5-layer configuration represents a deliberate choice to enable large-scale, long-duration simulations. While we agree that $\geq$20 layers would better resolve near-bed circulation (as noted by the reviewer), our sensitivity tests indicated this resolution adequately captured the dune migration with an energy-based predictor. We have added this discussion to Section 4.4.

We emphasise that this study provides first insights into large-scale behaviour where field data remain scarce. The model ability to reproduce realistic dune migration patterns (without artificial decay) at these scales supports its utility despite the acknowledged simplifications.

Major concern 2

To represent the monopiles in the simulation the authors have opted for a method using dry cells. This method is well known and is a very intuitive way to include such structures. However, the authors are not adapting the grid surrounding the monopile and thus end up with triangular monopiles of different sizes throughout the model domain. Since the authors are using an unstructured grid I do not understand why it is not refined to create a more round structure like in Christiansen et al. (2023). Since a non-hydrostatic approach is used, the grid can easily be refined, while the model assumptions remain valid (unlike in the mentioned study). The fact that the monopile is triangular may add a dependency of the results on the angle of incidence of the current with respect to the triangular grid cell. Moreover, the authors do not show how well the model represents the turbulent wake and reduced flow velocity behind the monopile, but skip straight to sediment transport and morphology. Comparison of this wake with theory may show whether the model is able to reproduce these processes. I would suggest adding a figure showing the velocities and turbulence for one monopile. I fear that inaccuracies in this representation can be part of the reason for the unnatural accumulation of sediment on the tidal axis right next to the monopile. This accumulation directly jeopardises the validity conclusions of the study, since it can be part of the cause for the reduced migration rates of marine dunes behind the monopile.

Response: Thank you for your comment.

We appreciate your detailed feedback regarding the representation of monopiles and its implications for flow and sediment dynamics. We also appreciate the reference to Christiansen et al. (2023). While their focus on structure-induced mixing differs from ours, they present a very interesting discussion of the importance of accurate structure representation in coastal modelling.

Our approach was guided by several key considerations. The hydrodynamic model setup follows our previously validated approach, which demonstrated 'excellent' agreement with site-specific level and current measurements (EMD, 2022; FEM, 2021a; Shom, 2017a; VLIZ, 2022b) in the natural (pre-construction) state (see Table 1 of the revised manuscript for key metrics).

Since no field data exist for flow conditions with monopiles at our study site, additional hydrodynamic analysis would not provide new validation insights. Instead, we have prioritised the sediment dynamics analysis where new understanding of dune-monopile interactions could be gained.

While we acknowledge that the explicit method we have adopted results in triangular representations of monopiles and that local grid refinement could improve geometric fidelity, we opted to maintain a consistent resolution across the regions where dune-monopile interactions are investigated. This choice was also motivated by our focus —stated throughout the manuscript— on far-field, kilometre-scale morphodynamic effects, rather than on near-foundation processes.

We also acknowledge the wave-like sediment deposition patterns observed in Figures 6–7 and 9–10 (and the exaggerated sediment accumulation near the monopile), which we previously noted in our presentation of the results (Lines 258-261). These features are primarily related to mesh resolution limitations. While the current resolution is appropriate for capturing large-scale dune migration patterns, it is insufficient to resolve near-foundation flow and sediment transport processes. Finer resolution around the monopile would likely mitigate these effects.

We have not added new figures, but have clarified our modelling choices and limitations, and expanded the discussion in Section 4.4 of the revised manuscript. Separately, we have now explicitly stated that we have restricted our analysis to the first year of simulation to avoid misinterpretation from these numerical artefacts.

Major concern 3

In essence the authors are applying 3 variations in their cases: natural state, a single monopile and a full array of monopiles. This is an interesting setup to assess both the impact of a single monopile and the interactions between monopile wakes when constructing a full wind farm. However, the single and multiple monopile cases are studied at different locations and no real comparison between them is made. Instead the same plots are shown at the new locations of the windfarm (Figure 9 and 10 vs. Figure 6 and 7), but in my eyes they do not contribute to the understanding of the system without intercomparison. Moreover, in the current setup the wide range of cases (IA, IB, II, II*, IIA, IIB) and different locations (tile 1,2,3, site 1,2,3) is confusing.

Response: Thank you for your comment.

The design of the study and sequential approach — first examining isolated monopile effects, then progressing to full-array simulations — were constrained by data availability. This meant that different locations were used for the single-monopile and array scenarios. While a direct comparison would be valuable, our primary objectives were to: (1) understand baseline monopile-dune interactions in simple, data-rich conditions (single foundation), then (2) study how effects combine in the actual wind farm layout. These objectives required different spatial domains but provide complementary insights.

To improve clarity, we have:

- explained our sequential modelling strategy more clearly in Section 2.3,
- clarified that our study focuses on process understanding rather than direct scenario comparison in the new Section 4.4,
- removed references to “tile” throughout.

The suggested comparison between single and multiple monopile effects is interesting and could be examined in future work, as it requires dedicated experimental design beyond this study’s scope.

Comment 1

L8: off -> offshore of/off the coast of

Response: Thank you for your comment.

The wording has been revised in the updated manuscript.

Comment 2

L51-53: You state there are no studies and then you continue with an example of one such a study. I think you should rephrase this (e.g. ‘limited studies’) and since there are limited studies on these time and spatial scales (and no space covering studies) have you found any other evidence or hypothesis of effects on these scales?.

Response: Thank you for your comment.

To our knowledge, no directly comparable studies exist at these spatio-temporal scales, whether based on field data or modelling. The analysis by Couldrey et al. (2020), the closest relevant work we identified, was constrained by the scope of the field data, with post-construction surveys focusing on cable routes and foundation design, covering only 200 m $\times$ 300 m. This restricted scope prevented observation of the full spatio-temporal evolution of the dune system. For broader context, we also cite Margalit (2017), though their work remains exploratory.

Extensive and recurrent bathymetric surveys of existing offshore wind farms under similar conditions (sandy seabed, water depths 20 m MSL, linear currents up to 1.25 m/s, monopile foundations) are lacking. Post-construction surveys typically prioritise risk assessment (e.g., scour at foundations [1] or cable exposure or/ free spanning), rather than dune-structure interactions. Some data exist on the regeneration of partially dredged marine dunes ([2], [3]). However, these studies are spatially/temporally constrained and do not address foundation effects on dune propagation either. Our work therefore fills a critical gap in understanding large-scale, long-term dune-field responses to offshore structures.

[1] A. J. Couldrey, T. Benson, M. A. F. Knaapen, K. Marten, and R. J. S. Whitehouse, "Morphological evolution of a barchan dune migrating past an offshore wind farm foundation," *Earth Surface Processes and Landforms*, vol. 45, pp. 2884–2896, 2020. DOI: `https://doi.org/10.1002/esp.4937`.

[2] S. M. Larsen, A. Roulund, and D. L. McIntyre, "Regeneration of partially dredged sandwaves," in *9th International Conference on Coastal Sediments*, May 2019. Tampa, Florida, 2019.

[3] A. Roulund, H. J. Riezebos, and K. Saverymuttu, "Foundation installation in sandwave fields. Field observations of sandwave regeneration and sediment infill," in *11th International Conference on Scour and Erosion (ISCE) Book of Proceedings*, September 17-21, 2023. Copenhagen, Denmark, 2023.

Comment 3

L73: please repeat the knowledge gap here, now it is unclear.

Response: Thank you for your suggestion.

As requested, we have revised the text to clearly reiterate the identified knowledge gap regarding large-scale, long-term dune-monopile interactions.

Comment 4

L89-L93: Please include references to where the data can be retrieved (where possible).

Response: Thank you for your comment.

We have now added explicit references to data sources in the first paragraph (Lines 89-93), distinguishing between proprietary measurements that are not currently accessible (e.g., FEM data) and publicly available datasets (e.g., VLIZ data). The following section (Lines 94-104) presents the metocean parameters derived from these datasets.

Comment 5

L112-115: The information about the sand waves is in my opinion too limited, since this is the focus of the study. I am left wondering: what is their wavelength? Are they very regular or irregular? How steep are they? What is their shape? (asymmetric vs. symmetric? Pointy or round crest?) Do they cover the full area? Do their characteristics and dynamics change between the tops and gullies of the sand banks? Please elaborate a bit more.

Response: Thank you for your comment.

We have significantly expanded the description of the dune field characteristics in the revised manuscript. The updated text (see below) now includes dune heights and wavelengths, morphology including crest shapes, spatial patterns. These additions, supported by new cross-sections in Figure 2 and spatial data in Figure 3(a), provide a comprehensive documentation of the dune field.

> These surveys reveal marine dunes with heights of between 1 and 4 m (7–20% of the local water depth) and wavelengths of 100-200 m. The dunes display distinct morphological patterns across the study area. The majority are sharp-crested and strongly asymmetrical, with a gentler side and a steeper side, reflecting a dominant current direction (Figure 2). This asymmetry systematically decreases towards sandbank crests, where dunes transition to symmetrical forms, reflecting reworking during both flood and ebb tides. Notably, dune fields are well-developed

on sandbank crests and flanks (e.g., sinuous dunes with frequent bifurcations dominate the flanks of the Dyck Oriental sandbank) but become sparse or absent in the deeper gullies between banks. The repeated surveys have allowed the dune migration rate to be quantified (Le Bot et al., 2023). Overall, they have been observed to travel at a net rate of approximately 30 m/yr towards the northeast.

Comment 6

L125: Here Tassi et al. (2023) is referenced for the empirical sediment transport rates, while in the supplementary materials the Soulsby-Van Rijn equation is included. Are you using the Soulsby-Van Rijn equations? Then you should also name this here and include the corresponding reference.

Response: Thank you for your comment.

Tassi et al. (2023) is cited for the sediment transport and morphological evolution module (GAIA) of the openTELEMAC system. The empirical sediment transport rates were calculated using the Soulsby-Van Rijn equations (Table 1). A reference to this predictor has now been included in the text:

The energy-based Soulsby-van Rijn predictor (Soulsby, 1997) was selected over shear-stress predictors, without slope correction, which caused unrealistic dune decay (Durand et al., 2023b). This approach successfully reproduced the dune migration patterns observed in recent surveys (FEM, 2021b), validating the model (Durand et al., 2025; Table 1 for metrics).

Comment 7

L132: Since you are using a (finest) grid size of 10 m it is very important to know the wavelength of your sand waves as a reader (since this determines whether the resolution is fine enough to really include the sand waves). Please also include an overview of the mesh sizes for example in the supplementary materials.

Response: Thank you for your comment.

The marine dunes in our study area have wavelengths of 100-200 m (as noted in revised Section 2.1, see response to Comment 5), ensuring at least 10-20 grid points per wavelength in our finest-resolution (10 m) zones. These high-resolution areas surround the OWF infrastructure, where dune-structure interactions are of primary interest, covering approximately 2.5 km^2 and 44 km^2 for Case I and Case II scenarios, respectively. This resolution adequately resolves the dominant bedforms while maintaining computational efficiency for a coastal area model. This has been clarified in the revised manuscript.

Comment 8

L132: What areas do you mean with the areas of interest? The repeat survey polygons?

Response: Thank you for your comment.

We have addressed this by modifying the text as follows:

> The size of the triangular elements varies gradually from 10 m (covering between 2.5 km^2 and 44 km^2 around offshore wind turbine infrastructure, providing 10–20 grid points per dune wavelength) to a maximum of 3000 m in distant areas, ensuring both adequate resolution of the dunes where interactions with structures are of interest and computational efficiency.

Comment 9

133-134: Does the DTM include the sand waves or are these filtered out since in some areas the model is not fine enough to resolve them?

Response: Thank you for your comment.

We appreciate the reviewer's question regarding the representation of marine dunes in the model DTM. The original manuscript did not clearly specify the spatial extent of the high-resolution (10 m) areas. We have now clarified that the 10 m resolution is applied across a significant area surrounding the foundations/offshore wind farm. This resolution ensures that marine dunes (typically with wavelengths of 100–200 m) are well-resolved in regions where interactions with structures are of interest. Outside of this high-resolution area, where the grid coarsens to a maximum of 3000 m, smaller-scale bedforms like marine dunes are not resolved.

Comment 10

L151-160: Does the flow experience drag (wall roughness) of the removed element? And if so: what is the roughness of this wall? If not: do you think it is a valid approximation to exclude this?

Response: Thank you for your comment.

The removed elements enforce free-slip (zero shear stress) boundary conditions, meaning wall roughness is neglected. While real-world foundations may induce some drag, we consider this approximation is valid to investigate large-scale processes/patterns (not near-foundation processes), where localised roughness effects are secondary to the bulk obstruction caused by the structure. We have expanded the original description (lines 154-155) to read:

It accurately captures their physical role as full water-column obstructions by creating a vertical null-flux boundary (through element removal) that enforces free-slip flow conditions and zero sediment flux across all depths.

Comment 11

Table 2: how is the effective pile diameter calculated? Based on volume? Is this based on literature?

Response: Thank you for your comment.

The effective pile diameters quoted in Tables 2 and 3 are provided solely as reference values calculated from the areas of the removed triangular mesh elements ($D_{eq} = 2\sqrt{A_{tri}/\pi}$) to facilitate comparison with physical monopile dimensions. The modelling itself does not use this diameter - foundations are represented exclusively through the removal of corresponding mesh elements, with no assumptions about their cylindrical form or surface properties. We have clarified this in the revised manuscript:

For reference only (to enable comparison with typical OWF foundation dimensions), the area-equivalent diameter of the removed elements is given in Table 2.

Comment 12

Line 181: do you mean 22 m diameter for the monopiles? 220 m seems a bit large...

Response: Thank you for your comment.

We appreciate the reviewer's confusion. The original text combined two distinct size scales: the 220 m wind turbine rotor diameter, in a general description of the OWF project, and the monopile foundation diameter, which is directly relevant to our modelling approach but was not explicitly stated. To improve clarity, we have revised the text Line 181.

Comment 13

Line 189-194: Why not just use one grid for all simulations and include single monopiles at two different locations followed by an array of monopiles including the locations studied earlier with a single monopile. Why two different case 2 options (with and without monopiles at the edge).

Response: Thank you for your comment.

The sequential modelling strategy was implemented based on the following considerations:

- **Scientific justification**: The single monopile tests were performed at locations where the morphodynamic model had been rigorously calibrated using high-resolution repeat bathymetric surveys, and shown to represent adequately the natural conditions (Durand et al., 2025). These zones do not coincide with the proposed turbine array locations.
- **Computational practicality**: Given resource constraints and the requirement for long-term simulations (2-3 year timescale), including the full OWF at a high resolution was computationally prohibitive for these tests. The exploratory grid, containing only 11% of the full grid elements, provided a more efficient framework.

Regarding the apparent "two different Case 2 options", this is a m isunderstanding. Table 3 initially reported metrics for all turbines, including two at the edge of the refined zone (outside the dune field). To b etter c haracterise t he f oundation-dune s ize relationship, we additionally provided these metrics excluding the edge turbines. Importantly, this represents a processing analysis - no separate simulation was run without the edge turbines. To clarify this, we have retained only the metrics excluding these edge turbines in the revised Table 3, with an explanatory footnote detailing this exclusion.

We have revised the text in Section 2.3 to convey more clearly the sequential modelling strategy: exploratory scenarios (data-rich, natural state validated) and OWF-area simulations (data-limited, realistic).

Comment 14

Section 2.3.3: why immediately go to sediment dynamics? In my eyes you are skipping a step: first hydrodynamics.

Response: Thank you for your comment.

Our approach was guided by several key considerations. The hydrodynamic model setup follows our previously validated approach, which demonstrated 'excellent' agreement with site-specific level and current measurements (EMD, 2022; FEM, 2021a; Shom, 2017a; VLIZ, 2022b) in the natural (pre-construction) state (see Table 1 of the revised manuscript for key metrics). Since no field data exist for flow conditions with monopiles at our study site, additional hydrodynamic analysis would not provide new validation insights. Instead, we have prioritised the sediment dynamics analysis where new insights into dune-monopile interactions could be gained. This aligns with our study's objective to understand kilometre-scale, multi-year interactions between marine dunes and wind farm foundations.

In the new Section 4.4, we explicitly acknowledge the lack of validation data, meaning that results should be interpreted as process-oriented rather than predictive, and the need for future validation when operational data become available.

Comment 15

Line 227: that is incorrect: the white areas indicate where the monopiles do not alter the erosion/accretion, which may be a result of unaltered sediment pathways, but can also happen with altered sediment pathways.

Response: Thank you for your clarification.

The original wording oversimplified the interpretation of white areas in the seabed difference maps. We have revised the text to more accurately state:

> In this representation, the white areas indicate regions where the presence of the monopile(s) shows no statistically significant deviation from natural erosion/accretion patterns.

Comment 16

> Line 231-236: Have you tested how well this spline method is able to predict the crest location? For example by downsampling the measurement data (assuming that the measurement resolution is higher than 10 m). It would be interesting to show this in the supplementary materials, also to see how well the sand waves are represented with this grid resolution.

Response: Thank you for your comment.

We confirm that the spline method, which was particularly important for accurately resolving crest locations, was validated by direct comparison between interpolated and discrete dune profiles. While this verification confirmed the method's reliability for our study, we consider these technical details too peripheral to our paper's main focus on dune-foundation interactions.

Separately, we have added seabed profiles to Supplementary Materials (accompanying Tables S1-S2) that show crest displacement under various scenarios. While these do not display the spline fitting itself, they demonstrate our model's dune representation capability. The 10 m resolution was carefully selected to balance accurate representation of dune morphology with computational efficiency for large-scale analysis.

Comment 17

Line 241-242 and caption Figure 4 and 5: please explain more clearly which difference we are looking at: is this without monopile – (minus) with monopile or the other way around? So does positive mean more accretive in the monopile situation? The order in which you name the two cases changes between the text and the caption.

Response: Thank you for your comment.

The difference maps consistently show:

- Seabed with monopile - seabed without monopile (baseline)
- Blue (negative): Erosion (lower bed levels with monopile)
- Brown (positive): Accretion (higher bed levels with monopile)

We have revised the figure captions and text, where relevant (in particular Lines 240-244), to eliminate the ambiguity and explicitly states the calculation order.

Comment 18

Caption Figure 5: what do you mean by 'on the tidal axis'? Are you removing the changes elsewhere from these figures? If so: please indicate the area for which the results are plotted in the figure.

Response: Thank you for your comment.

We agree that the phrase "on the tidal axis" was misleading. It was an interpretation of the results, not a methodological choice to limit the differences to the tidal axis. The figure does show the changes across the entire area. We have removed the phrase "on the tidal axis" from the caption.

Comment 19

Line 254: is 4 pile diameters sufficient to be outside of the local wake which is not accurately represented in the simulation? I would say the largest accretion not directly connected to the monopile is between 2.25 and 2.75 m (not 5 m) from Figure 4. Same in line 265-266

Response: Thank you for your comment.

Our use of 4 pile diameters (4D) as the reference distance aligns with established scour literature [4]. For consistency, we systematically assessed seabed changes beyond 4D (e.g., Line 254), in effect conservatively doubling the distance to account for limitations in our scour modelling.

However, we acknowledge that in some cases, the observed near-foundation accretion extends beyond 4D (e.g., the +5.0 m bed change in Figure 4). These localised anomalies likely reflect mesh resolution artefacts rather than physical processes, and we have updated the text accordingly to exclude such non-representative values (Cases IA, IB and II).

[4] R. J. S. Whitehouse, J. M. Harris, J. Sutherland, and J. Rees, "The nature of scour development and scour protection at offshore windfarm foundations," *Marine Pollution Bulletin*, vol. 62, no. 1, pp. 73–88, 2011. DOI: `https://doi.org/10.1016/j.marpolbul.2010.09.007`.

Comment 20

Line 255: I would say you can conclude that movement of the dunes is taking place due to the sedimentation erosion pattern, not solely due to the accretion and erosion heights. Maybe you can indicate the crest locations in the initial bathymetry on Figure 4 and 5 (and Figure 8), this would give significantly more insight into what is happening with the dunes. From the reversing pattern you can then already see that they are moving towards the monopile relative to the baseline case.

Response: Thank you for your suggestion.

We explored plotting initial crest locations but found that it was difficult to interpret. Instead, we found it more informative to plot the predicted crest locations at the end of the simulation, in the natural state, to illustrate the dune dynamics. This was implemented in Figures 4, 5, 8b and 8c (albeit to a limited extent due to the computational limitations mentioned at the end of section 2.3.4 of the manuscript).

We have also refined the statement in Line 255, which now reads:

> These variations, where bed changes approach full dune height, combined with the characteristic erosion-accretion patterns around the baseline crest locations (marked with grey lines), indicate significant dune migration.

Comment 21

Figure 6 and 7 (and 9 and 10): Can you add a line tracking the crest positions in the natural and monopile situation? This will give more insight in A) how well the model is able to represent this migration in the natural case (there seems to be and overestimation w.r.t. the measurements in some cases) and B) better show the effects on the monopile installation on the migration

Response: Thank you for your suggestion.

We have added a dedicated panel (Figure 6c) showing tracked crest positions to enable clear comparison between the natural baseline scenario (brown line) and the monopile scenario Case IA (orange line), with field measurements indicated by crosses (FEM 2021b). This approach avoids overloading the original bed-level colour maps while providing distinct visualisation of both the model performance in reproducing natural migration and the monopile impact on dune evolution.

Please note that Figures 7, 9, and 10 have been moved to the Supplementary Materials to streamline the main manuscript. As such, the crest tracking analysis was not performed for these figures.

Comment 22

Line 281-286: Please move the explanation of the values for the acceleration rate to be before the values. I was left confused over what -164% means.

Response: Thank you for your comment.

As requested, we have moved the interpretation of the values for the acceleration rate before presenting the specific results for Cases IA and IB. As an additional clarification, we have also included explanations alongside the specific values to reinforce the reader's understanding of the metric.

Comment 23

Figure 6 and 7 (and 9 and 10): you are without a doubt aware of the instabilities of the results. Regarding this I have two suggestions: a. Did you filter the superimposed bedforms (for which the model resolution is not sufficient) from the bathymetry before applying it to the model? If you still have these disturbances in there, they might grow into bigger instabilities, especially since there is no bed slope effects in the model to counteract those. b. Are you using an upwind or central scheme for the bed load calculations? Upwind schemes (based on current directions) may lead to growing instabilities in 3D models (see Volp et al., 2016)

Response: Thank you for your comment.

We acknowledge the wave-like sediment deposition patterns observed in Figures 6-7 and 9-10, which we previously noted in our presentation of the results (Lines 258-261). We think that these features are primarily related to mesh resolution limitations. While the current resolution is appropriate for capturing large-scale dune migration patterns, it is insufficient to resolve near-foundation flow and sediment transport processes. Finer resolution around the monopile would likely mitigate these effects. We have added explicit discussion in the revised manuscript (Section 4.4).

We appreciate the reviewer's suggestions.

a) Regarding superimposed bedforms:

- Smaller secondary bedforms were not explicitly filtered from the bathymetric input data.
- Our validation runs for natural conditions (without structures) demonstrated that this approach yielded good results.

b) Regarding numerical schemes:

- We use an edge-based explicit upwind advection scheme (transfer of fluxes by segment, [5]) to solve the Exner equation.

- During model development, we extensively tested alternatives, including beta developments implementing local Lax-Friedrichs methods ([6]). We found them overly diffusive or unstable.
- Note that τ-based transport predictors are particularly prone to instabilities, but not the Soulsby-van Rijn predictor.

[5] J.-M. Hervouet, E. Razafindrakoto, and C. Villaret, "Dealing with Dry Zones in Free Surface Flows: A New Class of Advection Schemes," in *Proceedings of the IAHR Congress*, June, 2011. Brisbane, Australia, 2011, pp. 4103–4110.

[6] L. Stadler and M. Utz, "A local Lax-Friedrichs flux applied to the finite volume method of GAIA," in *XXIXth Telemac-Mascaret User conference*, October 12-13, 2023. Karlsruhe, Germany, 2023, pp. 232–237.

Comment 24

Line 292: Are the numerical instabilities present in both simulation runs (with and without monopiles) or only in the one with monopiles? And what is the size of the instabilities? This cannot be judged from the difference plot alone.

Response: Thank you for your comment.

The numerical instabilities observed on the flanks of the Dyck Oriental sandbank occur in both simulation scenarios (with and without monopiles). This region corresponds to an area of high sediment transport potential and steep seabed gradients in our model, which can challenge the numerical stability.

Regarding the magnitude of these instabilities:

- The bed level oscillations are localised and typically range between ±2-3 m.
- This magnitude represents approximately 10-15% of the local sandbank height.

- The patterns are consistent between the two simulations, indicating they are not induced by the presence of monopiles.

We therefore conclude that while these features are present in the visual results, they do not affect the analysis of dune-monopile interactions, which is the focus of this study. This has been clarified in the manuscript.

Comment 25

Line 297-312: Instead of describing the sand waves here I would suggest adding a bathymetry figure and description in the method section. Especially an overview figure can tell much more than just textual description.

Response: Thank you for your suggestion.

To better characterise the dune morphology in response to this comment and Comment 5, we have updated Figure 2 (in "Materials and Methods") with dune profiles in both the navigation channel and Binnen Ratel sandbank flank, from two survey times, highlighting the dune dimensions, shapes, and the presence of secondary bedforms. The existing Figure 3(a) already effectively shows the spatial distribution of dunes across the OWF site. The textual descriptions in Lines 297-312 complement these figures by highlighting specific morphological patterns that are directly relevant to our results.

Comment 26

Line 321: So this can be used as a distance to calculate the effects on marine dunes: beyond 15-33 pile ‘diameters’

Response: Thank you for your suggestion.

Zone 3 is a duneless environment and does not exhibit the erosion/accretion patterns indicative of dune migration shown in panels (b) and (c). Therefore, we do not believe it is appropriate to generalise this distance to infer effects on marine dunes.

To avoid confusion, we have revised the text to express distances in metres rather than pile diameters.

Comment 27

Figure 11: nice figure!

Response: Thank you for your positive feedback.

Comment 28

Line 382: what is consistent with Couldrey? Did they report attraction of the dunes towards the monopile in the far field?

Response: Thank you for your comment.

We have clarified the text Lines 382-384, which now reads:

> This is consistent with the observations made by Couldrey et al. (2020), who found that broader hydrodynamic variability contributed to the stretching of an isolated crescent-shaped dune off the east coast of the UK. While their study did not report far-field attraction of dunes towards the foundation, it supports the notion that mechanisms beyond local scour may influence bedform evolution.

Comment 29

Line 385: This acceleration of the dunes on the side must be very local then? I do not see this in the model results.

Response: Thank you for your comment.

The increase in flow velocities on the sides of the obstacle is highly localised, and this translates into a similarly localised effect on the seabed.

To avoid confusion, we have added a statement clarifying that these changes are spatially limited and not discussed further in the manuscript.

Comment 30

Line 394-397: Why not show the plan form at beginning and end for one monopile?

Response: Thank you for your comment.

Crest line distortion is shown in Figure 12, and we have added a reference to this figure to clarify. What is not shown is the distortion of the cross-sectional profile, which is more anecdotal and less central to the study's focus on migration rather than dune shape.

Reproducing detailed dune morphology over long timescales is challenging. This limitation is discussed in [7].

Separately, we have added cross-sectional profiles to Supplementary Materials (accompanying Tables S1-S2) that show crest displacement under various scenarios (see response to Comment 16).

[7] N. Durand, P. Tassi, O. Blanpain, and A. Lefebvre, "GAIA can model large marine dunes: Results from a sensitivity study in the southern North Sea," in *XXIXth Telemac-Mascaret User conference*, October 12-13, 2023. Karlsruhe, Germany, 2023b, pp. 190–197.

Comment 31

Line 422: Which temporal evolution plots (in Section 2) do you mean? I cannot find them. Please include figure numbers.

Response: Thank you for your comment.

The reference to Section 2 was incorrect and has been updated to Figure 6 in the revised manuscript. Additional reference is made to the Supplementary Materials where Figures 7, 9 and 10 have been moved to streamline the main manuscript.

Comment 32

Figure 13: why use a different definition of migration rate here? And why compare for different areas? These values are not directly comparable. I would suggest including the results from Table 4, since these are the same location between natural and including monopile. Please plot an additional line at 0 migration speed to indicate reversal more clearly. And maybe add the variability as a shaded area.

Response: Thank you for your comment. We respond to each point below:

1. **Definition of migration rate:**
    - We used the mid-point of the steep side following Durand et al. (2025) to ensure consistency with the reference dataset (blue points).
    - This method better captures overall dune movement than crest tracking and was applied only in Figure 13 due to its computational intensity.
    - Elsewhere, we use crest line extraction (Section 2.3.4) to efficiently track planform changes.

2. **Comparison of values:**
    - We acknowledge these values are not directly comparable. The original figure used distinct colour coding to reflect this.

- Figure 13 aims to illustrate natural seasonal variability to contextualise predicted changes.
- However, since Table 4 only includes data from the OWF area, we have removed the navigation channel data from Figure 13 for consistency, and deleted references to differing calculation methods in the caption to avoid confusion.

3. Figure improvements:

- A zero-migration reference line has been added to clearly indicate reversal.
- Table 4 results have been included for direct comparison between natural and monopile scenarios, split into two graphs for clarity.

Comment 33

Line 436: again which plots?

Response: Thank you for your comment.

This has been clarified to read "Figures S2 and S3", which present the relevant plots.

Comment 34

Figure 14: Please indicate the locations of interest on the plot itself

Response: Thank you for your suggestion.

We agree that indicating locations of interest directly on the plot could improve readability. However, we found that adding such annotations rendered the figure illegible due to the density of overlapping features. To address this, we have instead revised the accompanying text to be more descriptive, guiding the reader to key areas of interest without compromising visual clarity.

Comment 35

Figure 14: why is everything negative (green)? Wouldn't an increase in migration rate be associated with an increase in the first order (tide averaged) sediment transport?

Response: Thank you for your comment.

We appreciate the opportunity to clarify the relationship between sediment transport and dune migration rates shown in Figures 12 and 14. The negative (green) values indicate a reduction in the first-order moment of sediment transport in the monopile scenarios, which may appear counter-intuitive given the observed increase in dune migration rates.

This apparent contradiction arises from the way the first-order moment is calculated. Due to the size of the model (over 2.8 million nodes) and the two-year simulation period, the output resolution is limited to hourly intervals. This does not allow averaging over exact tidal cycles (12h25min). Instead, we compute the vector difference at each hourly time step, assigning a positive sign to increases relative to the baseline and a negative sign to decreases, irrespective of transport direction. This approach, outlined in Section 2.3.3 of the original manuscript, is designed to reflect the intuitive velocity deficits observed in the wake of the monopile.

Under this convention, the presence of a monopile can reduce sediment transport during one tidal phase, even if the net transport over the full cycle is enhanced. This phase-dependent reduction leads to an imbalance that accelerates migration when dunes are approaching the monopile. As a result, the first-order moment may be lower in the wake of the monopile, even though the net migration rate is higher. The use of a 360-day averaging period ensures that the moment calculation reflects longer-term morphodynamic trends.

We acknowledge that this relationship is not immediately intuitive and have clarified the explanation in the revised manuscript. In hindsight, it may have been more straightforward to use the net transport rate to illustrate the migration trends. However, the project is now closed, and reprocessing the full dataset is no longer feasible.

To visualise the area of influence of the monopile(s) on sediment dynamics and morphological processes, the first-order moment is used as a proxy for the hydrodynamic wake (i.e., the velocity deficit). This connection follows from the formulation of the sediment transport predictor used in the model (Soulsby–van Rijn, 1997), which is based on depth-averaged velocity. Since flow velocities are locally reduced downstream of the monopile, sediment transport capacity is directly lowered in those regions. The first-order moment, which reflects the magnitude of sediment transport over time, captures this reduction. Consequently, areas of reduced first-order moment correspond spatially to the wake zones, providing a meaningful and morphodynamically relevant representation of the wake's influence. To compute this, the sediment transport rate (a vector) is extracted at hourly intervals from simulations conducted both with and without wind turbine foundation(s). At each spatial point and time step, we compute the vector difference between the two scenarios. The norm of this difference is then assigned a sign: a positive value indicates an increase in transport magnitude due to the monopile, while a negative value indicates a decrease—regardless of the direction of transport. This sign convention is intended to reflect the intuitive velocity deficits observed in the wake of monopiles, where transport may be reduced during one tidal phase (e.g., flood), even if the net transport over the full cycle is enhanced. The use of a 360-day averaging period ensures that the moment calculation captures longer-term morphodynamic trends. Higher-order moments (variance, skewness, and kurtosis) were also calculated but are not discussed here.

Comment 36

Line 485: I would say outside of the wake or tidal axis instead of 'other areas', this is clearer.

Response: Thank you for your comment.

The sentence has been revised as recommended to improve clarity.

> Our findings indicate that monopiles significantly affect their environment, causing sections of the dunes along the tidal axis to converge towards the monopile, while those outside of the wake continue moving at their natural speed.

Comment 37

Line 489: "This results in an increase in the speed of dune migration in the dominant direction and a reduction (or even reversal) in the subordinate direction." I do not find it completely clear what you mean here (also in the abstract). You mean the direction with respect to the monopile but you are not explicitly saying this. I would say increased migration rates when dunes are approaching the monopile and decreased migration rates when migrating away.

Response: Thank you for your suggestion.

We have revised the text as suggested, now stating: "increased migration rates when dunes approach the monopile and decreased rates (or even reversal) when migrating away from it." This phrasing has also been updated in the abstract for consistency.

Comment 38

Line 493: this is the first time you mention this factor 2 to 4 . How did you derive this factor? The largest migration rate deficits you are reporting in the text is -171%. This is not a factor 4. And you are also reporting deficits much lower than a factor 2. You cannot include new information in the conclusion (and abstract). This should already be presented (together with how it is derived) in the results or discussion.

Response: Thank you for your comment.

The "factor of 2 to 4" was derived from the annual mean migration speeds presented in Table 4 for the dunes closest to the monopile. These migration speeds were obtained using a different methodology (see response to Comment 32), based on the mid-point of the steep side rather than the crest, which is more susceptible to small-scale fluctuations. Using the convention applied elsewhere in the manuscript (acceleration rate = change / baseline speed):

- Zone 1 west of the monopile: $(20.9 + 34.4)/(-34.4) \approx -161\%$
- Zone 1 east of the monopile: $(-69.5 + 34.4)/(-34.4) \approx 102\%$
- Zone 2 west of the monopile: $(46.4 + 12.9)/(-12.9) \approx -460\%$
- Zone 2 east of the monopile: $(-57.2 + 12.9)/(-12.9) \approx 343\%$

We initially used the "factor of 2 to 4" phrasing for clarity and immediacy, but we appreciate the reviewer's observation that this may appear inconsistent with the reported acceleration rates. We have revised the manuscript to present these calculations explicitly in Section 4.2 and to clarify the conclusion and abstracts, now stating "up to a factor of 4 close to the foundation".

Comment 39

I would suggest the addition of 2 figures instead of the repetition (Figure 9 and 10) of the blue green timestack plot: • For the same transects included in the timestack plots: Initial bed level along the transect combined with final bed level for the different cases (natural, single monopile in middle of transect and monopile in middle + array of monopiles surrounding). In this way the effects of a single monopile and an array of monopiles can easily be compared • It would be nice to plot the deviation in migration rate versus the distance to the monopile for (one of) the transects, either in absolute or relative terms, instead of representing those in a table in the supplementary materials. This can be done for both the single and full array of monopiles (along the same transect). This gives more insight in how the influence of the monopile changes with distance.

Response: Thank you for your suggestions.
Figures 7, 9, and 10 have been moved to the Supplementary Materials to streamline the main manuscript.

In response to your comments, we have added new figures to the Supplementary Materials accompanying Tables S1-S2. These include cross-sectional profiles showing initial and final seabed levels along the same transects used in the temporal evolution plots, for both the baseline (natural) case and the with-monopile scenarios. This allows for a direct comparison of the morphodynamic impact of the monopile.

Additionally, we have included plots showing the deviation in migration rate (referred to as acceleration rate in the manuscript) versus distance to the monopile, for both the single-monopile and full-array scenarios. While these plots are somewhat redundant with the crest-tracking data added to Figure 6 at your request, we agree that they help visualise how the influence of the monopile varies with distance (Tables S 1-S2). For this reason, we have included them in the Supplementary Materials.

Round 2 Review

Comments by reviewer 2 (anonymous)

Completed: 2025-09-29 11:56 AM
Recommendation: Major revisions

For author and editor

Dear editor and authors,

Firstly I want to thank the authors for their elaborate response to my comments. I feel like the adaptations made have improved the clarity of the manuscript.

However, I feel that the authors have not sufficiently addressed some of my major concerns. I sense a reluctance to change any aspect of the model setup, even mentioning that additional data-analysis is impossible since 'the project is now closed' and ascribing the choices made to the goal of modelling far-field morphology. Still, it should be noted that, especially in a system determined by the fine balance of large gross effects, seemingly small changes in hydrodynamics and sediment transport processes can significantly affect large-scale and long-term morphological evolution. Especially the choice to leave out processes which are at vital for marine dune growth is not well supported in my eyes. Moreover, the lack of hydrodynamic validation is explained by the authors by the lack of hydrodynamic field data for the cases including monopiles. It is in my eyes short-sighted to say that, since there is no field data for this specific case, there is no validation possible. Even when one-on-one validation is not possible we can search for signs that the model reproduces the processes we know about from theory that should be distinguishable in the model results (such as the turbulent wake and increased mixing behind a monopile). In failing to show these basic processes, which cascade to sediment transport and eventually morphological development, all subsequent result, and therefore the manuscripts conclusions, can and should be questioned.

To accurately simulate marine dune development significant changes to the model set-up have to be made. The model should refine in the vertical, use near-bed hydrodynamics or bed-shear stress to calculate sediment transport and include bed slope processes. Moreover, the validity of the simplifications in the monopile inclusion should be better supported. In practice the authors have chosen to not change any part of the model setup, but rather include the reasoning behind the choices in the discussion. Although this puts more emphasis on the choices made, it still tells us little about the implications of these exclusions and simplifications for the applicability of the results and the model. If the authors are still reluctant to change the model setup, I strongly urge them to include these limitations more clearly in the manuscript (both in the method and discussion section). If the authors do not implement my suggestions on clearer communication I will have to reject this manuscript. I have included the specifics of what in my eyes should be included below.

Major concern 1

I thank the authors for the adaptations they made to the manuscript. It is now more clearly communicated which processes have been left out of the manuscript and the rationale behind these choices. However, I am still missing the implications of these lacking processes. As I understand now, the bed load transport is based on a depth-averaged flow velocity. This means that it does not matter how well the circulation cells driving marine dune growth are represented by the vertical resolution of the model, since these effects are averaged out when moving to sediment transport. The model will therefore never be able to simulate marine dune growth and what the authors describe as 'artificial decay' due to bed slope effects is actually a sign that the growing mechanism is excluded (and including the dampening mechanism will thus lead to too much dampening). In terms of marine dune modelling I would thus call this a 2D(H) or quasi 3D model rather than a full 3D model, since the important vertical processes are not resolved. I have seen that these types of models are able to reproduce the initial migratory response of marine dunes to forcing conditions. However, it should be noted that over time the marine dunes will dampen out, since the sediment transport maximum will always be at the crest of the marine dune due to flow continuity.

I see two options for the manuscript moving forward. Either the authors adapt their model set-up (increased vertical resolution to represent current circulation, sediment transport based on near-bed currents or bed shear stress and include bed slope effects on transport) or they adapt the text to clearly reflect the limitations. Since the authors do not seem prone to change the model setup (as was suggested already in my earlier review), I will go on to explain what needs to be done to reflect the limitations textually.

- Due to these limitations the model can only reproduce incipient migration of marine dunes and cannot simulate long-term morphological development. The authors should thus rephrase mentions of simulating for example marine dune dynamics (which also includes growth and decay) and replace these with 'incipient' or 'initial' dune migration (rates).
- It should be made clear that the 3D model is not resolving the 3D processes of importance for marine dunes. Even though it remains a 3D model the authors should find a way to make this clear whenever the term 3D (or any related term) is mentioned. This can be done by replacing the term 3D by for example semi/quasi 3D or adding an asterisk (*) or footnote which explains this.
- When discussing the sediment transport formulation in the methodology it should be clearly mentioned that it uses a depth averaged current (L163-165)
- In the discussion of the manuscript, for instance in section 4.4, it should be explicitly stated that the model is unable to simulate long-term morphological development of marine dunes due to choices made in the set-up.

Here are some of the mentions of dynamics which need to be adapted:

L10: dune migration -> incipient dune migration

L25: dune migration -> incipient dune migration

L83: replace 'dynamics, particularly the migration rate' with 'incipient migration rates'

L85: dynamics -> incipient migration rates

L200: dune migration -> incipient dune migration

L312: dynamics -> incipient migration rates

Section 4.1 heading: dynamics -> incipient migration rates

L624: marine dune fields -> incipient migration of marine dunes

L636: dynamics -> incipient migration rates

More cases can probably be found

Major concern 2

As mentioned I believe it short-sighted to state that, since there is no field data available for the specific case including monopiles, no hydrodynamic validation of the model is possible. Countless studies have been carried out to determine the wake effects behind elements of various shapes and sizes, both on the small and large scale. A plot of the impact of the monopile representations on the hydrodynamics will show whether the simplifications in the implementation (the triangular dry cells with varying size and orientation) are valid. The authors even cite a study: Rivier et al. (2016) where these kind of effects are shown in countless different figures. I cannot understand why the authors will not show these results, making these kind of figures should be standard procedure when setting up a synthetic model case. The only reason I can think of for not showing this (not even in the supplementary materials) is to hide unfavourable results. I fear the wakes behind the piles might be distorted due to the asymmetrical shape of the triangular monopile, or might significantly change from one monopile to another due to size and angle of incidence differences. These possible wake distortions and variations will impact the large scale morphodynamics. Since the results including monopiles are so instrumental to the conclusions of the manuscript I do not feel the authors can get away with skipping important validation steps.

The following steps need to be taken to improve the trust in the manuscript

- The authors should add figures showing the impact of the monopiles on the hydrodynamics (possibly in the supplementary materials) and compare these impacts to literature
- Since the authors mention that the model is unable to simulate the near-field effects and the representation of the monopile and grid resolution are not sufficient to resolve the processes near the monopile, these limitations should be clearly shown in the results. I suggest greying out an area with a radius of 100 m (based on the disturbances in the results) around each monopile in each of the figures. Moreover, the marine dunes which enter or start from this region should be excluded from the analysis. In this way it is clear to the reader that the results in these areas should not be trusted.

Major concern 3

Thank you for clarifying the case structure.

Minor comments:

Comment 11:

Thank you for clarifying how this area-equivalent diameter is calculated. However, I think this translation might be an oversimplification of the processes, especially when calling it the 'effective pile diameter'. A streamlined round element does not per definition have the same effect on the flow as a triangular element of the same area. Moreover, the drag imposed by the element is in case of a triangular element dependent on its orientation towards the flow. Depending on this orientation and the shape of the triangle, the drag may even be 4 times as high compared to a circular element which has the same frontal area, meaning that the effect of the removed triangular cells may be significantly larger than that of a monopile with the mentioned 'effective pile diameter'. I strongly suggest that the authors either:

1. Remove the 'effective pile diameter' and other mentions of pile diameter from the manuscript (also at other locations like Table 3 and within explanatory text). I understand that this measure is also used to explain the results, but this then should be translated into meters instead of (effective) pile diameters.
2. Or actually calculate the diameter of a representative circular pile based on the drag coefficient of the triangular shape of the cell and the angle of incidence of the flow. This diameter will have to be calculated for each monopile individually and will be different for ebb and flood flow resulting in two values per pile.

Comment 19:

Due to the oversimplified calculation of the pile diameter, I would suggest also using 100 m as a minimum distance from the pile for obtaining trustworthy results. This is based on the model results rather than theory, since it is questionable whether the model is able to accurately represent the effects from theory and which diameter should be used when using guidelines from theory.

Comment 32:

Thank you for making this figure more clear and comparable. There is however still one thing unclear to me: what is the difference between the orange lines with downward- and upward facing triangles? Are those two different dunes on different sides of the monopile? Please clarify in the manuscript, caption and figure legend.

Recommendation by Associate Editor 2 (Rocío Luz Fernández)

Completed: 2025-10-02 04:16 PM
Recommendation: Minor revisions

For author and editor

Dear Authors,

I have read the current version of the manuscript entitled ***"On the Attraction of Marine Dunes to OWF Fixed Foundations"*** with great interest, as well as the detailed reviews provided by the reviewers.

I believe the manuscript's wording around the model development could be clarified to better reflect the work done. The model itself was not newly developed by the authors, nor newly implemented in the Dunkirk area (this was already done in Durand et al. 2025). A friendly suggestion would be to phrase it as the TELEMAC/GAIA setup being **implemented or adapted** to study specific monopile scenarios, rather than "a model was developed". Rephrasing rows 8, 76, and 126 along these lines could help avoid any misunderstanding and make the description of the authors' contribution even clearer.

Regarding validation, I agree with Reviewer D (or #2) that the validation provided is only partial. As Reviewer D noted, even in the absence of direct field data, validation against theoretical expectations or well-known hydrodynamic patterns (e.g., turbulent wake and mixing behind a monopile) would strengthen the credibility of the setup. Importantly, Reviewer D does not request a full re-validation of the framework, but rather a demonstration that the model remains physically plausible when monopiles are introduced. If such a demonstration is not included, the authors should explicitly discuss this limitation within the manuscript, as suggested by Reviewer D.

In light of this, while further simulations are not essential, the manuscript would benefit from a more explicit discussion of validation and of the limitations when adapting the existing Dunkirk setup to monopile scenarios. I therefore recommend **minor revisions**, with particular attention to presenting the model's limitations clearly. I warmly encourage the authors to carefully address Reviewer D's comments in this process, as doing so will further strengthen both the clarity and the impact of the study.

I thank the authors for their efforts and look forward to seeing the revised version.

Best Wishes,
Anonymous Associate Editor

Decision by Editor-in-Chief (Jackie Cockburn)

2025-10-05

I would like you to undertake a **minor revision** of your manuscript.

Authors' responses to review comments

2025-11-13

Responses to Reviewers' Comments for Manuscript 44

On the attraction of marine dunes to OWF fixed foundations

Addressed Comments for Publication to

Geomorphica

Dear editor,

Please find enclosed the revised version of our manuscript entitled "On the attraction of marine dunes to OWF fixed foundations" (Manuscript ID: 44). We thank the Associate Editor and Reviewer D for their engagement and feedback, which has allowed us to clarify several aspects of the manuscript. We have carefully addressed all points raised in this latest round of review. This document contains our point-by-point responses. For your convenience, all corresponding changes in the manuscript have been highlighted. Line numbers referenced in our responses correspond to the 'Manuscript with Track Changes' version.

With best wishes,

The authors

Note: To improve the readability of this response letter, all the comments made by the editors and the reviewers have been formatted in blue boxes. Where appropriate, reworded or added text is in grey boxes.

Authors' Response to the Associate Editor

General Comments. I have read the current version of the manuscript entitled "On the Attraction of Marine Dunes to OWF Fixed Foundations" with great interest, as well as the detailed reviews provided by the reviewers. In light of this, while further simulations are not essential, the manuscript would benefit from a more explicit discussion of validation and of the limitations when adapting the existing Dunkirk setup to monopile scenarios. I therefore recommend minor revisions, with particular attention to presenting the model's limitations clearly. I warmly encourage the authors to carefully address Reviewer D's comments in this process, as doing so will further strengthen both the clarity and the impact of the study.

Response: Thank you for your feedback.

Comment 1

I believe the manuscript's wording around the model development could be clarified to better reflect the work done. The model itself was not newly developed by the authors, nor newly implemented in the Dunkirk area (this was already done in Durand et al. 2025). A friendly suggestion would be to phrase it as the TELEMAC/GAIA setup being implemented or adapted to study specific monopile scenarios, rather than "a model was developed". Rephrasing rows 8, 76, and 126 along these lines could help avoid any misunderstanding and make the description of the authors' contribution even clearer.

Response: Thank you for your comment.

The foundational model setup was developed in our previous study [1]. In this manuscript, we build upon that established setup to conduct a new set of numerical experiments focused specifically on dune-monopile interactions. Therefore, the 'development' in this context refers to the implementation of the monopile structures within the existing model framework and the creation of new simulation scenarios. We have revised the manuscript wording Lines 7-9, 23-25, 80-82 and 133-135 to make this distinction clear.

[1] N. Durand, P. Tassi, O. Blanpain, and A. Lefebvre, "Meteorological conditions influence the migration of a marine dune field in the southern north sea," *Journal of Geophysical Research: Earth Surface*, vol. 130, no. 1, e2024JF007731, 2025. DOI: https://doi.org/10.1029/2024JF007731.

Comment 2

Regarding validation, I agree with Reviewer D that the validation provided is only partial. As Reviewer D noted, even in the absence of direct field data, validation against theoretical expectations or well-known hydrodynamic patterns (e.g., turbulent wake and mixing behind a monopile) would strengthen the credibility of the setup. Importantly, Reviewer D does not request a full re-validation of the framework, but rather a demonstration that the model remains physically plausible when monopiles are introduced. If such a demonstration is not included, the authors should explicitly discuss this limitation within the manuscript, as suggested by Reviewer D.

Response: Thank you for your comment.

In response to Reviewer D's comment, we have expanded the text in Section 2.3 describing the representation of the turbine foundation in the model. Specifically, we have added a comparison of the resulting hydrodynamic wake to established literature, in the absence of site-specific data. This new text (below) is supported by two new figures in the Supplementary Materials (Figures S1-S2), which illustrate the favourable comparison.

Since the wind farm is not yet constructed, no field data exist for flow conditions with turbine foundations at our study site, preventing a full calibration of the hydrodynamics with monopiles. Nevertheless, confidence in the model adaptation is supported by a realistic prediction of the monopile hydrodynamic wake (see Supplementary Materials, Figures S1-S2). The results show an elongated wake structure consistent with the bi-directional tidal currents. This wake decays over a spatially consistent scale, with surface velocity recovering to 80% within approximately 900 m downstream and reaching 95% recovery by 2.0 km. This recovery length aligns with observational findings from the Donghai Bridge offshore wind farm (Li et al., 2014) and numerical simulations of the planned Courseulles farm (Rivier et al., 2016). Furthermore, the model captures an expected dependency on the flow incidence angle relative to the removed triangular mesh element. This dependency is seen not in the overall length of the wake, but in the rate of velocity recovery and the detailed structure of the near-wake region.

We believe these additions satisfactorily address the reviewer's concern regarding model plausibility when monopiles are introduced and hope the revised manuscript is now acceptable on this point.

Authors' Response to Reviewer D

General Comments. Dear editor and authors, Firstly I want to thank the authors for their elaborate response to my comments. I feel like the adaptations made have improved the clarity of the manuscript.

However, I feel that the authors have not sufficiently addressed some of my major concerns. I sense a reluctance to change any aspect of the model setup, even mentioning that additional data-analysis is impossible since 'the project is now closed' and ascribing the choices made to the goal of modelling far-field morphology. Still, it should be noted that, especially in a system determined by the fine balance of large gross effects, seemingly small changes in hydrodynamics and sediment transport processes can significantly affect large-scale and long-term morphological evolution. Especially the choice to leave out processes which are at vital for marine dune growth is not well supported in my eyes. Moreover, the lack of hydrodynamic validation is explained by the authors by the lack of hydrodynamic field data for the cases including monopiles. It is in my eyes short-sighted to say that, since there is no field data for this specific case, there is no validation possible. Even when one-on-one validation is not possible we can search for signs that the model reproduces the processes we know about from theory that should be distinguishable in the model results (such as the turbulent wake and increased mixing behind a monopile). In failing to show these basic processes, which cascade to sediment transport and eventually morphological development, all subsequent result, and therefore the manuscripts conclusions, can and should be questioned.

To accurately simulate marine dune development significant changes to the model set-up have to be made. The model should refine in the vertical, use near-bed hydrodynamics or bed-shear stress to calculate sediment transport and include bed slope processes. Moreover, the validity of the simplifications in the monopile inclusion should be better supported. In practice the authors have chosen to not change any part of the model setup, but rather include the reasoning behind the choices in the discussion. Although this puts more emphasis on the choices made, it still tells us little about the implications of these exclusions and simplifications for the applicability of the results and the model. If the authors are still reluctant to change the model setup, I strongly urge them to include these limitations more clearly in the manuscript (both in the method and discussion section). If the authors do not implement my suggestions on clearer communication I will have to reject this manuscript. I have included the specifics of what in my eyes should be included below.

Response:

We thank the reviewer for their thoughtful and detailed feedback. We appreciate the recognition of the improvements made to the manuscript and acknowledge the remaining concerns raised. Below, we address each of the reviewer's remaining comments and suggestions point by point, providing clarifications, justifications, and, where possible, additional insights to support our modelling choices and the scope of the study.

Major concern 1

Original comment from Round 1: The authors have chosen to exclude slope-induced sediment transport from their simulations, while this is a fundamental process necessary to represent the sand wave system. As described in Borsje et al. (2014), the equilibrium of marine dunes is determined by the balance between bed load transport driven by circulation cells and slope-induced transport (when suspended transport is negligible, like in this study). The authors have thus actively left out the balancing mechanism. I do not see significant growth of the sand waves. However this might have to do with either the duration of the simulations (growth is just starting up) or it might signal that the circulation cells, which cause marine dune growth, are also not well represented in the simulations. Since the model only applies 5 vertical layers, of which the distribution is not commented upon, I would not be surprised if it is unable to recreate this mechanism. For these systems usually a minimum of 20 vertical layers is applied. I suggest that the authors include (longitudinal and transverse) bed slope effects in all their simulations. Without these processes I do not see how the system can be well represented. Moreover, the authors should do a sensitivity study to see if the circulation cells (under purely tidal forcing) are well represented while using this little layers (compare the results to simulations with more vertical layers). This can be included in the supplementary materials.

New follow-up comment: I thank the authors for the adaptations they made to the manuscript. It is now more clearly communicated which processes have been left out of the manuscript and the rationale behind these choices. However, I am still missing the implications of these lacking processes. As I understand now, the bed load transport is based on a depth-averaged flow velocity. This means that it does not matter how well the circulation cells driving marine dune growth are represented by the vertical resolution of the model, since these effects are averaged out when moving to sediment transport. The model will therefore never be able to simulate marine dune growth and what the authors describe as 'artificial decay' due to bed slope effects is actually a sign that the growing mechanism is excluded (and including the dampening mechanism will thus lead to too much dampening). In terms of marine dune modelling I would thus call this a 2D(H) or quasi 3D model rather than a full 3D model, since the important vertical processes are not resolved. I have seen that these types of models are able to reproduce the initial migratory response of marine dunes to forcing conditions. However, it should be noted that over time the marine dunes will dampen out, since the sediment transport maximum will always be at the crest of the marine dune due to flow continuity.

I see two options for the manuscript moving forward. Either the authors adapt their model set-up (increased vertical resolution to represent current circulation, sediment transport based on near-bed currents or bed shear stress and include bed slope effects on transport) or they adapt the text to clearly reflect the limitations. Since the authors do not seem prone to change the model setup (as was suggested already in my earlier review), I will go on to explain what needs to be done to reflect the limitations textually.

- Due to these limitations the model can only reproduce incipient migration of marine dunes and cannot simulate long-term morphological development. The authors should thus rephrase mentions of simulating for example marine dune dynamics (which also includes growth and decay) and replace these with 'incipient' or 'initial' dune migration (rates).
- It should be made clear that the 3D model is not resolving the 3D processes of importance for marine dunes. Even though it remains a 3D model the authors should find a way to make this clear whenever the term 3D (or any related term) is mentioned. This can be done by replacing the term 3D by for example semi/quasi 3D or adding an asterisk (*) or footnote which explains this.
- When discussing the sediment transport formulation in the methodology it should be clearly mentioned that it uses a depth averaged current (L163-165)
- In the discussion of the manuscript, for instance in section 4.4, it should be explicitly stated that the model is unable to simulate long-term morphological development of marine dunes due to choices made in the set-up.

Here are some of the mentions of dynamics which need to be adapted: L10: dune migration -> incipient dune migration L25: dune migration -> incipient dune migration L83: replace 'dynamics, particularly the migration rate' with 'incipient migration rates' L85: dynamics -> incipient migration rates L200: dune migration -> incipient dune migration L312: dynamics -> incipient migration rates Section

4.1 heading: dynamics -> incipient migration rates L624: marine dune fields -> incipient migration of marine dunes L636: dynamics -> incipient migration rates More cases can probably be found.

Response: Thank you for your detailed comment and for acknowledging our previous adaptations to the manuscript.

As noted in our previous response, the underlying PhD project is now completed and a fundamental restructuring of the model setup is not feasible for this manuscript. That being said, we agree that fully clarifying the implications of our model setup is important for accurately setting reader expectations.

Therefore, we have implemented the following textual changes to address the reviewer's points:

- We have systematically replaced the broad term "dune dynamics" with the more specific "dune migration" or "dune propagation" throughout the manuscript. To prevent a blanket and cumbersome repetition, we have qualified "dune migration" with "annual" only where it was necessary to frame the model one-year prediction scope, and where referring specifically to validated rates. This nuanced approach ensures the text is precise without being repetitive, and clearly communicates that the model is not applied to long-term (decadal) morphodynamic evolution.
- Regarding the "3D" terminology, we have opted for a solution that provides clarity without introducing potentially ambiguous terms like "quasi-3D". To prevent any misunderstanding, we have now explicitly stated in key locations, including the Abstract (Line 9-10), Introduction (Line 82-83), and Conclusions (587-588), that the model "couples 3D hydrodynamics with depth-averaged sediment transport". This ensures the model hybrid nature is clear to all readers from the outset.
- As requested, we have also added an explicit clarification in the Materials and Methods section (Lines 163-164) stating that the sediment transport predictor is based on depth-averaged currents.

We believe these revisions significantly strengthen the manuscript by providing precise definitions of the model capabilities and limitations. This study provides valuable first insights into large-scale dune migration patterns where field data remain scarce, and its demonstrated skill in reproducing realistic short-term patterns supports its utility within its defined scope.

Major concern 2

Original comment from Round 1: To represent the monopiles in the simulation the authors have opted for a method using dry cells. This method is well known and is a very intuitive way to include such structures. However, the authors are not adapting the grid surrounding the monopile and thus end up with triangular monopiles of different sizes throughout the model domain. Since the authors are using an unstructured grid I do not understand why it is not refined to create a more round structure like in Christiansen et al. (2023). Since a non-hydrostatic approach is used, the grid can easily be refined, while the model assumptions remain valid (unlike in the mentioned study). The fact that the monopile is triangular may add a dependency of the results on the angle of incidence of the current with respect to the triangular grid cell. Moreover, the authors do not show how well the model represents the turbulent wake and reduced flow velocity behind the monopile, but skip straight to sediment transport and morphology. Comparison of this wake with theory may show whether the model is able to reproduce these processes. I would suggest adding a figure showing the velocities and turbulence for one monopile. I fear that inaccuracies in this representation can be part of the reason for the unnatural accumulation of sediment on the tidal axis right next to the monopile. This accumulation directly jeopardises the validity conclusions of the study, since it can be part of the cause for the reduced migration rates of marine dunes behind the monopile.

New follow-up comment: As mentioned I believe it short-sighted to state that, since there is no field data available for the specific case including monopiles, no hydrodynamic validation of the model is possible. Countless studies have been carried out to determine the wake effects behind elements of various shapes and sizes, both on the small and large scale. A plot of the impact of the monopile representations on the hydrodynamics will show whether the simplifications in the implementation (the triangular dry cells with varying size and orientation) are valid. The authors even cite a study: Rivier et al. (2016) where these kind of effects are shown in countless different figures. I cannot understand why the authors will not show these results, making these kind of figures should be standard procedure when setting up a synthetic model case. The only reason I can think of for not showing this (not even in the supplementary materials) is to hide unfavourable results. I fear the wakes behind the piles might be distorted due to the asymmetrical shape of the triangular monopile, or might significantly change from one monopile to another due to size and angle of incidence differences. These possible wake distortions and variations will impact the large scale morphodynamics. Since the results including monopiles are so instrumental to the conclusions of the manuscript I do not feel the authors can get away with skipping important validation steps.

The following steps need to be taken to improve the trust in the manuscript

- The authors should add figures showing the impact of the monopiles on the hydrodynamics (possibly in the supplementary materials) and compare these impacts to literature
- Since the authors mention that the model is unable to simulate the near-field effects and the representation of the monopile and grid resolution are not sufficient to resolve the processes near the monopile, these limitations should be clearly shown in the results. I suggest greying out an area with a radius of 100 m (based on the disturbances in the results) around each monopile in

each of the figures. Moreover, the marine dunes which enter or start from this region should be excluded from the analysis. In this way it is clear to the reader that the results in these areas should not be trusted.

Response: Thank you for your comment.

We acknowledge the validity of the reviewer's concerns and have taken significant steps to address them, which we believe substantially strengthen the manuscript.

Hydrodynamic validation: In direct response to the reviewer's request, we have conducted a new analysis of the monopile effects on the hydrodynamics. As suggested, we have added new figures to the Supplementary Materials that visualise the hydrodynamic impact of the monopiles using the relative difference in surface velocity (Δ_{rel}), following the methodology and colour scheme of reference [1] for direct comparability.

The new figures show Δ_{rel} for Case IA during a peak flood tide, and a peak ebb tide. As in reference [1], the -0.20 Δ_{rel} contour is indicated with a black line, clearly showing the extent of the velocity deficit wake.

To specifically address the concern about the triangular representation, we have included a second set of figures in the Supplementary Materials showing the same analysis for a different monopile with a markedly different angle of incidence to the dominant tidal flow. The total extent of the wake is consistent between the two, but as expected the rate of velocity recovery and the detailed structure of the near-wake region depend on the flow incidence angle relative to the removed triangular mesh element.

Some text was added in Section 2.3 (see response to Associate Editor's comment 2).

Model uncertainty zone: We agree with the reviewer that clearly identifying areas of uncertainty is important for the reader and that a visual aid is stronger than the textual reference alone. As detailed in our response to Comment 19 below, we have introduced a greyed-out model uncertainty zone around all monopiles in the relevant figures (Figs. 4-7, 9, S1-S5). This provides an immediate and clear visual cue, ensuring readers intuitively interpret the results in these regions with appropriate caution.

The reviewer suggested a 100 m radius based on the results. Reference [2] indicates that turbulence peaks at 1.5D downstream of a pile and decays rapidly thereafter. We adopted a more conservative radius of 50 m (equivalent to five mesh elements using the dominant mesh size of 10 m) for the uncertainty zone. This distance:

- accounts for potential inaccuracies in representing the local wake and other near-field processes in our simulations
- provides a clear and consistent visual indicator for the reader, as requested

The new supplementary figures provide the hydrodynamic validation requested. They demonstrate that our model produces wakes that are physically plausible and consistent with established literature. This additional analysis strengthens the foundation for interpreting the large-scale morphodynamic results that are the focus of our study.

[1] A. Rivier, A.-C. Bennis, G. Pinon, V. Magar, and M. Gross, "Parameterization of wind turbine impacts on hydrodynamics and sediment transport," *Ocean Dynamics*, vol. 66, no. 10, pp. 1285–1299, 2016. DOI: https://doi.org/10.1007/s10236-016-0983-6.

[2] J. Miles, T. Martin, and L. Goddard, "Current and wave effects around windfarm monopile foundations," *Coastal Engineering*, vol. 121, pp. 167–178, 2017. DOI: https://doi.org/10.1016/j.coastaleng.2017.01.003.

Major concern 3

Original comment from Round 1: In essence the authors are applying 3 variations in their cases: natural state, a single monopile and a full array of monopiles. This is an interesting setup to assess both the impact of a single monopile and the interactions between monopile wakes when constructing a full wind farm. However, the single and multiple monopile cases are studied at different locations and no real comparison between them is made. Instead the same plots are shown at the new locations of the windfarm (Figure 9 and 10 vs. Figure 6 and 7), but in my eyes they do not contribute to the understanding of the system without intercomparison. Moreover, in the current setup the wide range of cases (IA, IB, II, II*, IIA, IIB) and different locations (tile 1,2,3, site 1,2,3) is confusing.

New follow-up comment: Thank you for clarifying the case structure.

Response: Thank you for the opportunity to clarify this point.

Comment 11: Effective pile diameter

Original comment from Round 1: Table 2: how is the effective pile diameter calculated? Based on volume? Is this based on literature?

Our initial response (from Round 1): The effective pile diameters quoted in Tables 2 and 3 are provided solely as reference values calculated from the areas of the removed triangular mesh elements ($D_{eq} = 2\sqrt{A_{tri}/\pi}$) to facilitate comparison with physical monopile dimensions. The modelling itself does not use this diameter - foundations are represented exclusively through the removal of corresponding mesh elements, with no assumptions about their cylindrical form or surface properties. We have clarified this in the revised manuscript.

New follow-up comment: Thank you for clarifying how this area-equivalent diameter is calculated. However, I think this translation might be an oversimplification of the processes, especially when calling it the 'effective pile diameter'. A streamlined round element does not per definition have the same effect on the flow as a triangular element of the same area. Moreover, the drag imposed by the element is in case of a triangular element dependent on its orientation towards the flow. Depending on this orientation and the shape of the triangle, the drag may even be 4 times as high compared to a circular element which has the same frontal area, meaning that the effect of the removed triangular cells may be significantly larger than that of a monopile with the mentioned ‘effective pile diameter’. I strongly suggest that the authors either:

1. Remove the ‘effective pile diameter’ and other mentions of pile diameter from the manuscript (also at other locations like Table 3 and within explanatory text). I understand that this measure is also used to explain the results, but this then should be translated into meters instead of (effective) pile diameters.

2. Or actually calculate the diameter of a representative circular pile based on the drag coefficient of the triangular shape of the cell and the angle of incidence of the flow. This diameter will have to be calculated for each monopile individually and will be different for ebb and flood flow resulting in two values per pile.

Response: Thank you for this suggestion.

We agree that removing the "effective pile diameter" is appropriate since the measure is not used within the model itself and expressing results in metres provides a more direct and universal physical scale. We have therefore removed all mentions of ’effective pile diameter’ and related normalised measures from the manuscript, including from Tables 2 and 3, and the explanatory text (Lines 211-212, 228-229, 304, 319, 363, 389, 393).

Comment 19: Buffer zone

Original comment from Round 1: Line 254: is 4 pile diameters sufficient to be outside of the local wake which is not accurately represented in the simulation? I would say the largest accretion not directly connected to the monopile is between 2.25 and 2.75 m (not 5 m) from Figure 4. Same in line 265-266

Our initial response (from Round 1): Our use of 4 pile diameters (4D) as the reference distance aligns with established scour literature (Whitehouse et al. 2011). For consistency, we systematically assessed seabed changes beyond 4D (e.g., Line 254), in effect conservatively doubling the distance to account for limitations in our scour modelling.
However, we acknowledge that in some cases, the observed near-foundation accretion extends beyond 4D (e.g., the +5.0 m bed change in Figure 4). These localised anomalies likely reflect mesh resolution artefacts rather than physical processes, and we have updated the text accordingly to exclude such non-representative values (Cases IA, IB and II).

New follow-up comment: Due to the oversimplified calculation of the pile diameter, I would suggest also using 100 m as a minimum distance from the pile for obtaining trustworthy results. This is based on the model results rather than theory, since it is questionable whether the model is able to accurately represent the effects from theory and which diameter should be used when using guidelines from theory.

Response: Thank you for your comment.
We agree that defining a model uncertainty zone is a clear and effective way to indicate areas where results should be interpreted with caution. In response, we have defined a conservative uncertainty zone with a radius of 50 m from each monopile. This zone has been added as a greyed-out area in the relevant figures. The radius was determined by considering both theoretical and practical aspects of our modelling approach. While reference [2] indicates that turbulence peaks at 1.5D downstream of a pile and decays rapidly thereafter, we adopted a more conservative distance to account for potential inaccuracies in representing the local wake and other near-field processes in our simulations. Using the dominant mesh size of 10 m as a representative length scale, a 50 m radius ensures we exclude the region most affected by inherent scale trade-offs.

The specific changes are as follows:

- Figures 4-7 and 9 in the main manuscript now include the 50 m uncertainty zone.
- Figures S1-S5 in the Supplementary Materials have been updated accordingly.
- We have updated the corresponding figure captions and legends.
- We have added explanatory text in subsection 3.1 (Lines 297-299) where the figures are first presented.

> To explicitly account for this limitation, we introduce a model uncertainty zone with a 50 m radius, five times the dominant mesh size of 10 m. This zone, indicated by a greyed-out area in all relevant figures, identifies the region where local seabed changes should be interpreted with caution.

[2] J. Miles, T. Martin, and L. Goddard, "Current and wave effects around windfarm monopile foundations," *Coastal Engineering*, vol. 121, pp. 167–178, 2017. DOI: `https://doi.org/10.1016/j.coastaleng.2017.01.003`.

Comment 32: Seasonal variability (old Figure 13)

Original comment from Round 1: Figure 13: why use a different definition of migration rate here? And why compare for different areas? These values are not directly comparable. I would suggest including the results from Table 4, since these are the same location between natural and including monopile. Please plot an additional line at 0 migration speed to indicate reversal more clearly. And maybe add the variability as a shaded area.

Our initial response (from Round 1):

1. Definition of migration rate:

- We used the mid-point of the steep side following Durand et al. (2025) to ensure consistency with the reference dataset (blue points).

• This method better captures overall dune movement than crest tracking and was applied only in Figure 13 due to its computational intensity.

• Elsewhere, we use crest line extraction (Section 2.3.4) to efficiently track planform changes.

2. Comparison of values:

• We acknowledge these values are not directly comparable. The original figure used distinct colour coding to reflect this.

• Figure 13 aims to illustrate natural seasonal variability to contextualise predicted changes.

• However, since Table 4 only includes data from the OWF area, we have removed the navigation channel data from Figure 13 for consistency, and deleted references to differing calculation methods in the caption to avoid confusion.

3. Figure improvements:

• A zero-migration reference line has been added to clearly indicate reversal.

• Table 4 results have been included for direct comparison between natural and monopile scenarios, split into two graphs for clarity.

New follow-up comment: Thank you for making this figure more clear and comparable. There is however still one thing unclear to me: what is the difference between the orange lines with downward- and upward facing triangles? Are those two different dunes on different sides of the monopile? Please clarify in the manuscript, caption and figure legend.

Response: Thank you for your comment.

The reviewer's understanding is correct. The orange lines with downward- and upward-facing triangles correspond to the migration speeds for two distinct dunes on opposite sides of the monopile. We have revised the text in the manuscript (Lines 500-503) and updated the caption and legend for Figure 10 to ensure it is unambiguous.

Round 3 Review

Recommendation by Associate Editor 2 (Rocío Luz Fernández)

Completed: 2025-11-19 12:30 AM
Recommendation: Accept

For author and editor

Dear authors,

I read with interest the latest version of the manuscript entitled *"**On the attraction of marine dunes to OWF fixed foundations.**"* You have thoroughly addressed all reviewer comments, significantly improving the clarity, transparency, and scientific framing of the manuscript, particularly regarding model limitations and hydrodynamic validation. There is just one point that may still benefit from a minor adjustment. The reference to Lokin et al. (2023) (in Line 545) does not fully align with the transport formulation used in your manuscript, as Lokin et al. do not employ the Soulsby–van Rijn type approach on which your method is based. You might consider including Németh et al. (2006): *Modelling offshore sand waves* (in Coastal Engineering), which is a well-established reference in offshore sand-wave modelling and directly supports the type of depth-averaged sediment-transport behaviour you describe; or another citation you find appropriate for this context.

That said, the substantial effort invested in refining the text and adding supplementary analyses is greatly appreciated. Based on this revised version, and assuming the reference adjustment mentioned above is considered, I believe the manuscript is now suitable for **acceptance**.

Best wishes,
Anonymous Associate Editor

Decision by Editor-in-Chief (Jackie Cockburn)

2025-11-20

I would like you to undertake a **minor revision** of your manuscript related to a reference lining up with a formula description.

Authors' responses to review comments

2025-11-24

Responses to Reviewers' Comments for Manuscript 44

On the attraction of marine dunes to OWF fixed foundations

Addressed Comments for Publication to

Geomorphica

Dear Editor,

We thank the Associate Editor for their positive feedback and for recommending our manuscript for acceptance. We are pleased to hear that our revisions have successfully addressed the reviewers' concerns.

Please find enclosed the latest version of our manuscript entitled "On the attraction of marine dunes to OWF fixed foundations" (Manuscript ID: 44), which has been revised to address the final editorial point. Our detailed response to the specific comment is included in this document.

With best wishes,

The authors

Authors' Response to the Associate Editor

General Comments. I read with interest the latest version of the manuscript entitled "On the attraction of marine dunes to OWF fixed foundations." You have thoroughly addressed all reviewer comments, significantly improving the clarity, transparency, and scientific framing of the manuscript, particularly regarding model limitations and hydrodynamic validation.

Response: Thank you for your positive feedback.

Comment 1

There is just one point that may still benefit from a minor adjustment. The reference to Lokin et al. (2023) (in Line 545) does not fully align with the transport formulation used in your manuscript, as Lokin et al. do not employ the Soulsby–van Rijn type approach on which your method is based. You might consider including Németh et al. (2006): Modelling offshore sand waves (in Coastal Engineering), which is a well-established reference in offshore sand-wave modelling and directly supports the type of depth-averaged sediment-transport behaviour you describe; or another citation you find appropriate for this context.

Response: Thank you for your comment.

We agree that the original text did not clearly communicate that Lokin et al. (2023) used a different depth-averaged sediment transport predictor from Soulsby-van Rijn. Our intention was to highlight their conclusion that supports the general capability of depth-averaged predictors to simulate dune propagation.

We have therefore rephrased the sentence in the manuscript (Line 545) to clearly state this (provided below for ease of reference).

Furthermore, to directly reinforce the validity of our specific model setup, we have added a reference to our earlier work, Durand et al. (2025), which presents an extensive validation of the Soulsby-van Rijn predictor as implemented in our model.

> Its effectiveness has been demonstrated in the Dunkirk area to simulate the migration of marine dunes over a 13-month period (Durand et al., 2025). This is further supported by Lokin et al. (2023), who showed that a different depth-averaged predictor (Engelund and Hansen, 1967) was able to simulate river dune propagation, reinforcing the general capability of mean-flow-based predictors for bedform migration.

Decision by Editor-in-Chief (Jackie Cockburn)

2025-12-02

Congratulations! I am pleased to inform you that your manuscript entitled "On the Attraction of Marine Dunes to OWF Fixed Foundations" has been accepted for publication in Geomorphica.