

Dear Professor Davidson-Arnott,

Thanks again for a constructive review. Please find our response below in green.

Kind regards,

Bas Hoonhout

Comments from the editors and reviewers:

- Reviewer 1

This paper describes the testing and application of a simulation model to predict aeolian erosion, transport and deposition at the Sand Motor mega nourishment site on the coast of the Netherlands. It builds on earlier publications by the authors which have described the development of the model, and the data on morphological change at the site. The latter provide the data which are used initially to calibrate parts of the model and to evaluate the success of the model by comparing predicted erosion and deposition to measurements at the site. The model is designed especially for situations where sediment availability varies both spatially and temporally due to the presence of surface moisture and or the development of a shell lag surface.

The AeoliS model is designed to be applied over meso- time and spatial scales using hourly data and thus the modelling of winds and sediment transport is simplified and greater emphasis is placed on including fluctuations in surface moisture content following inundations by tides and waves, and the emergence of shells due to aeolian deflation. There is a clear need for modelling at this scale and the application here is appropriate. The model itself is a 'work in progress' and while the overall prediction compares well to the measured data, this is evidently in part due to the calibration process. As a result, an important contribution of the paper is the discussion around some of the limitations to the model as revealed by spatial and temporal discrepancies between measurements and predicted values. Overall I think this complements nicely their earlier papers dealing with aeolian processes on the Sand Motor and it will be of interest to researchers working in a range of environments where sediment availability for aeolian transport is curtailed by environmental conditions.

I just have a few specific comments related to the paper itself and to the approach and the way forward.

Comments:

- 1) I had some difficulty following the description of the operation of the model. This is probably a reflection of my own limitations but it might be possible to make it a bit easier to follow. My problems deal primarily with understanding which operations reflect a single input value – for example as I understand it:
 - There is a single value for incident wind speed and direction for each hour and this applies uniformly over the whole domain - correct?

Correct for the model version applied in this paper. A more recent model version implements a wind shear perturbation model following Kroy and Sauermann (2002) to overcome this limitation and allow the model not only to provide sediment supply rates, but also simulate dune formation and migration.

We adapted section 4.2 to emphasize the uniform wind fields in the model.

- It seems that erosion and deposition are confined to particular zones so that erosion can only occur in the mixed zone and in the aeolian zone, while deposition can only occur in the dunes and in the dune lake and lagoon – correct?

Not entirely correct. Both erosion and deposition can occur anywhere in the domain. There is no spatiotemporal varying model input that enforces this zonation, except topology that indirectly influences the erodibility of the beach as parts of the beach are continuously exposed to wind and water and others predominantly to wind. The zonation originates from spatiotemporal variations in sediment supply and transport and evolves from the simulation. It is therefore a model outcome and not input. For example,

the mixed zone is erosive during low tide, but accretive during high tide in combination with offshore wind. In the latter case, sediment is blown seaward and trapped in the water body very similar to what happens in the dune lake and lagoon.

We adapted section 4.3 to emphasize the fact that zonation is not imposed to the model, but just a method to detail the spatial varying performance of the model.

- In the marine zone it is assumed that eroded material is replaced by wave action so there is an unlimited supply – correct?

Not entirely correct, in the marine zone wave action causes mixing of the bed surface following the formulation of Masselink et al. (2007). Fine sediments from deeper bed layers therefore have an opportunity to surface. Eventually, also in the marine/mixed zone sediment supply ceases, unless a marine sediment supply is imposed. In our simulation no such marine sediment supply exist. Coupling a nearshore hydrodynamic model to simulate true marine sediment supply to the beach is also something we are working on. See the work of Nick Cohn, Oregon State University who defends his PhD thesis this week!

We added a paragraph to section 3 to explain the effect of waves in the model.

- The topography is used only to define the erosion and deposition zones and does not affect wind flow or erosion rate – correct?

Correct, but the erosion and deposition zones are not so much defined as they evolve during the simulation from spatiotemporal variations in sediment supply and transport due to variations in moisture, grain size and armouring. Topographies, for example, determine what part of the beach is flooded during high tide.

We trust that we covered this issue with our previous adaptation in section 4.2 regarding uniform wind fields.

- Is the spatial scale of each hourly calculation the 50x50 m grid square? Is a separate calculation of erosion from the bed made based on equation 1 and the effect of moisture (in the mixed zone) or armouring (in the aeolian zone) and does this mean, for example that the bed evolves at a different rate in each aeolian grid cell so that there is a spatial variation in the extent of armouring? Does the model allow for deposition within the Aeolian zone and, if so, what is the effect of the surface roughness on this?

These are multiple questions at once, but they touch the essence of the model (and hindcast). I'll try to answer them one-by-one:

Is the spatial scale of each hourly calculation the 50x50 m grid square?

Yes.

Is a separate calculation of erosion from the bed made based on equation 1 and the effect of moisture (in the mixed zone) or armouring (in the aeolian zone) and does this mean, for example that the bed evolves at a different rate in each aeolian grid cell so that there is a spatial variation in the extent of armouring?

Yes, the spatial variation in armouring evolves from the spatial variation in sediment supply and transport. Note that also without moisture this spatial effect is visible. The Sand Motor accommodates large fetches. Therefore, saturated sediment transport is almost always reached at some point along the fetch. Only in the upwind unsaturated zone net erosion occurs and an armour layer can develop. Only in case supply from these upwind zones diminish, sediment transport in the downwind zones will not be saturated anymore and these zones subsequently become erosive. Consequently, also in these downwind zones an armour layer can develop. Eventually, the entire Sand Motor will become armoured. Introduction of moisture complicates the system but does not change the essence of the simulated mechanisms.

This process of spatiotemporal varying sediment availability and supply is closely related to the critical fetch approach by Bauer and yourself. The key difference is that the critical fetch and the transport curve in the unsaturated zone is predicted by the model rather than used as model input. Therefore, the model allows for a temporally varying "critical fetch" dependent on temporally varying conditions.

On more regular beaches the same processes can play a role, but due to smaller fetches the effect of the individual processes is not so nicely separated as at the Sand Motor. The most striking result is often aeolian sediment transport peaking just after a storm.

We slightly adapted section 3 to emphasize these properties of the model but refer to our JGR 2016 paper for details.

Does the model allow for deposition within the aeolian zone and, if so, what is the effect of the surface roughness on this?

Yes, but deposition in the model version used for this manuscript is still limited. Following Raupach (1993) the surface roughness causes the local wind shear velocity threshold to increase. Therefore, surface roughness increases the likelihood of deposition. Also, wind shear velocity threshold maps can be imposed to the model that mimic the presence of vegetation (not in the manuscript). Germination and expansion of vegetation is not (yet) included. Also, the dip in wind shear in front of the dunes is not present (yet). Therefore, no new foredunes develop.

- You start with a bed consisting of 10 1cm layers. What happens if there is more than 10 cm of erosion?

The 10 1cm layers move with the erosion and deposition. In case of erosion, the bed layers are “lowered” as to keep the top bed layer flush with the instantaneous bed surface level. The other layers move down accordingly. The lowest bed layer is replenished with sediment from an infinitely deep base layer that always keeps the initial grain size distribution. See Fig. 2 from our JGR 2016 paper.

We slightly adapted section 3 to emphasize that the bed layers are moving along with erosion.

- 2) I also found the description of the development of the armoured surface over time in the model to be a bit lacking. There is a bit more in an earlier paper describing the initial model but I think there is a need here to explain how this is modelled and if there is any process that can reverse the process (other than wave action on the beach). You clearly end up with much higher values for the proportion of the surface that is armoured in the model than actual values. It may be, as you suggest, that you overestimate the proportion of shells in the initial sediment population. However, in the field there may be some dynamic mechanism(s) that can produce a reduction in surface lag – perhaps due to burial under high winds or weathering and/or abrasion and breakage of the larger shell fragments over time?

Fair point. Burial is included in the model. Also, during high winds the shells themselves can be mobilized, which is not included. Nevertheless, the construction height of the Sand Motor and consequently the absence of wave action still largely determines the severity of the armouring at the Sand Motor.

We added alternative explanations to the model/data discrepancy to section 6.1. We also added an extra paragraph at the end of section 3 that explains the simulation of beach armoring from the inclusion of the roughness elements in the model.

- 3) The calibration of σ seems to be used more as an overall calibration coefficient to bring predicted and modelled values in line rather than a true calibration of σ itself. It is convenient because, as McKenna Neuman et al. (2012) stress there are all kinds of issues associated with the application of the shear stress partitioning approach to small roughness elements such as shells and so we don't really know much about how to deal with them. In the field where flow is neither steady nor uniform shells do not offer any kind of permanent protection to any sand deposited in their lee and so any sand brought onto such a surface can be transported onward without much hindrance.

We partially agree. We agree that in the field sediment can be transported over a shell pavement relatively easy. Still, the shell pavement hinders erosion of the bed locally. Transport over a shell pavement therefore largely depends on sediment availability upwind of the shell pavement. In other words, your final statement is true if there is another sediment source upwind that is not (severely) armoured. This relates directly to the final paragraphs of our discussion section where we propose an adaptation of the model that captures exactly this difference between initiation and continuation of transport from/over a shell pavement. Data is lacking to actually implement this difference.

In addition, modeling meso time- and spatial scales always include some “convenient” parametrizations as to keep the model practically applicable. The challenge is here to make these parameterizations non-governing as to limit the dependence of the model outcome on these parametrizations.

Given the limited sensitivity of the model to σ (anything between 4 and 14 will do quite well, see Fig. 6), we are of opinion that parameterization using σ is an acceptable compromise between model efficiency and completeness.

- 4) The AeoliS model does not include the effects of precipitation and surface drying following rainfall. Clearly it could do this easily and I wonder if adding this would be a relatively easy way to improve the model and reduce the dependence on the artificial calibrating σ . I have done a bit of this myself using hourly precipitation data and a simple drying rate that varies between 1 and 2 hours.

Good point. I did quite a bit of testing with precipitation and surface drying myself. Also with dedicated (unsaturated) soil moisture models. This resulted in a meteo module in AeoliS that includes precipitation, evaporation and infiltration, but that I never actually used (<http://aeolis.readthedocs.io/en/latest/implementation.html#moisture-content>). This is for two reasons. First, I found that with precipitation rates common to the Netherlands cause a peak in surface moisture in the order of 1 hour. That is too close to the model time step to have a significant effect. Second, in the model I need to distribute the rainfall over a certain depth from which it infiltrates to deeper layers. This is a practical choice that originates from the fact that we discretized the bed itself in layers, unlike many other models. However, this practical choice appeared to be very important to the infiltration rate and the influence of rainfall to the modeled transports. Therefore this “storage depth” parameter is an example of a “convenient” parametrization that governs the model outcome too much. I did not feel comfortable to enable this module just yet, but I’m very interested in alternative approaches to implement rainfall more appropriately, including the effect of rain drops impacting saltating grains and thereby limiting transport.

We are of opinion that this elaborate consideration regarding the effect of precipitation and the reason why not to include it is outside the scope of this paper.

- 5) Did you do any short-term calibration based on field experiments, e.g. those described in Hoonhout and de Vries, 2017?

Working on it. But since I defended my PhD and have other obligations, things move a bit slower in here...

A major hurdle in here is that we use an implicit numerical scheme that is diffusive in time (i.e. solutions are smoothed in time). For meso-scale simulations this is a little sacrifice in return for unlimited numerical stability. For short-term simulations the diffusiveness of the numerical scheme is a problem. Therefore, we also implemented an explicit and semi-implicit scheme (actually we started off with these explicit schemes as they are much easier to implement). However, these schemes are unvalidated. First, we need to prove that the explicit scheme produces similar results to the implicit scheme. And the explicit scheme is stable only for very small time steps, so this takes time. Second, we can do a proper model/data comparison over different time scales.

We also still have a dataset of a few years of infrared imagery from the beach to investigate high **resolution** surface moisture variations that nobody looked at yet. We intend to apply these measurements in conjunction with a terrestrial laser scanner to validate the relations between surface moisture and wind-driven beach erosion to the decimeter and minute scale over periods of years. And compare that to the model results. Stay tuned ☺

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