Review Report for

Detection and characterization of aeolian sand strips on the beach using permanent laser scanning

Christa O. van IJzendoorn, Kay Boomaars, Sander E. Vos, Ad J.H.M. Reniers, Mieke Kuschnerus, Roderik Lindenbergh

*Corresponding author: vanijzec@oregonstate.edu
Paper DOI: 10.59236/geomorphica.v1i1.32

Table of Contents:

The work is licensed under CC BY 4.0 International

Round 1 Review

Comments by reviewer 1 (Gerben Ruessink)

Completed: 2024-04-04 01:17 PM
Recommendation: Major revisions

Reviewer Comments

For author and editor

Review of “Detection and characterization of aeolian sand strips on the beach using permanent laser scanning”, submitted for possible publication in Geomorphica.
The paper addresses the characteristics of aeolian bed forms, known as sand strips, based on permanent laser scanning. I think that the paper will appeal to a significant proportion of the readership of Geomorphica. This paper fits well with the rapidly growing amount of literature on aeolian processes and dynamics in the coastal zone. Furthermore, I believe that the technique (permanent laser scanning) and various aspects of the analysis steps will be of considerable interest to those interested in advanced measuring techniques and remote sensing. The data set is truly impressive and inspiring.
Based on my reading of the manuscript, I recommend ‘major revision’. In the following I will outline my main concerns, and then describe various minor issues. I believe that the revised paper should be reviewed again. I hope that my review will help to improve the quality of the paper.

Main concerns

1. I struggled with what the actual focus of the paper is. Is it about the laser scanning (PLS) and the analyses to characterize the sand strips, or is about the sand strips themselves? I noted that the Introduction does not provide a clear problem definition. For example, what do we not yet know about sand strips from a scientific point of view and why is gaining this knowledge relevant? And then, why is PLS the technique to be used? Is sand-strip height and long-term relevance to sand transport the main unknown, given that Hage already focused on length and migration? I can phrase this first main concern also as: what is the main novelty of the present work? Is this that PLS was applied (including the algorithm) or that we now know more about sand strips? The problem and scientific aim should be stated explicitly in the Introduction. Then, various parts of the paper may need to be restructured or be extended to better reflect the main novelty of the work (see also main concern 3 below).

2. There are various aspects of the methodology that the authors need to reconsider or at least explain/discuss in more detail:

    1. *Spectra*. If I understood correctly, only information up to dominant k is used for the IFFT step. Then, sand strips are sinusoidal by definition, I assume. Is this realistic? Sand strips don't have slip faces but one might expect them to be asymmetric, with steeper downwind than upwind slopes. Or, they might have other deviations from a purely sinusoidal signal, such as rather peaked crests. Figure 8c suggests that sand strips are

The work is licensed under
CC BY 4.0 International

indeed not sinusoidal, but rather peaked. To capture this 'asymmetry', it may have been advantageous to also include E(k) at larger k than the peak k. Why not include everything up to two times the peak k, where one would expect the largest contribution to the non-sinusoidal shape (this 2k is first harmonic of peak k)? Does the authors' choice not inherently underestimate the height of sand strips (peaked crests are missed, part of the spectrum is discarded)? I became particularly worried by the approach when I had a look at Figure 14d and g. Here, the IFFT approximation does not seem to be suitable to quantify the length and height at all. Or is Figure 14 an example where the lower L detection limit should have been lower than based on what Hage and co-workers found?

2. *Interpolation*. What method was used to interpolate the data, if interpolation was required? The reflectance and elevation data are spatially anisotropic. Variability along the crests of the sand strips is substantially less than in the migration direction. Was this anisotropy accounted for, for example, by direction depending semi-variograms? If not, what might the consequence of the anisotropy on the quality of the interpolation have been?

3. *Significant height*. Why did the authors choose the significant height? The IFFT sand strips are more or less sinusoidal, with strongly suppressed height-to-height variability (see Figure 14) compared to the actual signal. The rms value for a single sinusoid (or, $\sqrt{m_0}$ in spectral terms) equals $0.5\sqrt{2}$ times amplitude. With amplitude = 0.5 H, this provides H = 2 $\sqrt{2}$ rms, and not 4 rms as used by the authors. I feel that the 2 $\sqrt{2}$ rms approach is more appropriate.

4. *Migration speed*. I was somewhat surprised to see that the migration speed was, in essence, determined manually. Why did the authors not use cross-spectral techniques? This would have been more consistent with how the height and length were determined.

5. *False positive/negative*. It would be nice to know a priori what might cause false positive or negative detection of sand strips. In the present paper, this part of the method/algorithm section comes as a surprise. I see that some reasons are provided later on. Should Section 4.1 not be part of Section 3, to illustrate the performance of the algorithm? I think this relates back to the question what this paper is actually about: the algorithm, or the sand strips? If it is about the sand strips, then the Results should start with them, and not with the algorithm, I think.

3. Given the effort that has gone in detecting sand strips, I found the actual analysis of the sand strip characteristics to be rather meagre. It could be extended with, for example:

    1. Do sand strip height and length depend on environmental conditions, like wind speed or direction? Are length and height related (a longer sand strip has a higher height, for example)?

The work is licensed under CC BY 4.0 International

2. Is their a clear temporal signal in length and height (i.e., change with time) during each sand strip event? What does this temporal change (or absence thereof) imply? If there is no such temporal signal, then this is interesting information too.

3. Where do sand strips actually form? Mainly on the intertidal beach? Or on the upper beach? This also relates to whether tides can cause the disappearance of the sand strips. The tide doesn't reach the upper beach. Or did sand strips on the intertidal beach sometimes survive because the next high tide was not as high as the previous high tide?

4. Does rain intensity or amount affect whether sand strips 'survive' a rain event? You show results for a single event, but to what degree is this representative of the entire data set?

4. The attempt to relate sand strip migration to sand transport rates is interesting, although I think the authors try to get too much out of it when they relate it to foredune volume growth. As the authors say, sand strips migrate predominantly alongshore. And for foredune volume growth, they should take the onshore component of the transport rate, see, for example Bauer and Davidson-Arnott (2002, Geomorphology; the cos α term in that paper). I expect the onshore component of the sand-strip flux to be minimal, and so their contribution to foredune growth. If the authors wish to have the foredune growth included in the discussion section, then the onshore component should be used, not the total component.

5. The Abstract and Conclusions can be made more quantitative. Do mention key numbers from your results, and only then compare to the existing literature. And, the Abstract should also state the main problem and aim. Possibly, also the Conclusions should be refocused when the problem and aim are stated more explicitly in the Introduction section, in order to better reflect the main novelty of the work.

Minor concerns:

1. I am missing a reference to Nield (2011, Geology) in the Introduction. This paper is one of the first that deals exclusively with sand strips and also proposes a generation mechanism.

2. In the method, should the stated footprint not depend on the range and the orientation of the target relative to the laser beam? If not, then I probably misunderstand what the authors mean by footprint.

3. I found the 10% error in gravimetric moisture content a somewhat confusing statement, given that gravimetric moisture content is given in % too. I suppose that if the moisture content is (for example) 5%, you consider the error to be 0.5%? And if so, is that then 5 ±5% (width of 1%) or 5 ± 0.25% (width of 0.5%). What is this 10% error based on? And, finally, where in the Results section is this error used? It does not appear to be used in Section 4.2.1.

4. Are you sure that the wind data from IJmuiden is at 4.4 m above bed level? This may be where the sensor is actually placed, but, in general, the KNMI provides data corrected to 10 m above the bed only (10 m is the international standard). If it is indeed 4.4 m, then it is not the same

The work is licensed under
CC BY 4.0 International

height as used in the various Hage papers. How does this influence the comparison to those papers?

5. I found it very interesting to see that (minimum) wind speeds for sand strip formation and occurrence are different. Is this comparable to the different thresholds (fluid versus impact) for aeolian transport? Or does it relate to the time needed for sand strips to actually form? It may be that they start to form at 8 m/s but are detectable only when the wind speed has already increased (with time) to more than 8 m/s.

6. Are sand strips during precipitation events (when detected) still active (e.g., do they still migrate?), or is it best to consider them 'fossilized'?

7. In the computation of the sand transport rates, the authors assume the sand strips to be triangular, whereas in their IFFT approximation the sand strips are (more-or-less) sinusoidal. Isn't there an equation for sinusoidal bedforms? If not, then what is the consequence of assuming a triangular shape? Are transport rates biased low or high because of the different shape in reality?

The work is licensed under
CC BY 4.0 International

Comments by reviewer 2 (“anonymous”)

Completed: 2024-03-25 12:06 AM
Recommendation: Minor revisions

Reviewer Comments

For author and editor

The paper describes a new method to detect aeolian sand strips using permanent laser scanning techniques. At the outset, I would like to appreciate the authors for an extremely well-written paper. The manuscript has an appropriate number of figures and detail and describes both the technique, and how results emanating from such methods can provide us information about general sediment transport rates of sand strips. I recommend publication of this paper after addressing the following minor changes.
Lines 61-63: It might be helpful to explain how the absolute values of reflectance be influenced by moisture content? Would it increase/decrease with increasing moisture?
Lines 143-145 A small graphic describing the coordinate axes might be helpful. Perhaps consider adding a high resolution satellite image of the beach with the location of the PLS setup and the coordinate directions annotated on it.
Lines 154-160 It is still slightly confusing for the reader to fully distinguish "reflectance" used in this context, as opposed to the general usage of the term "surface reflectance". I suggest considering using a different term throughout this paper and/or adding a few lines to clearly describe the difference between the two terms.

The work is licensed under
CC BY 4.0 International

Recommendation by Associate Editor 2 (Rocio Luz Fernandez)

Completed: 2024-04-15 04:55 PM
Recommendation: Minor revisions

Reviewer Comments

For author and editor

Dear authors,
I read with interest your manuscript entitled "**Detection and characterization of aeolian sand strips on the beach using permanent laser scanning**", as well as the reviews from two anonymous reviewers. Based on my reading of your manuscript and the reviews, I would like to recommend that the manuscript **be accepted after minor revisions.**
Once again, thank you for submitting your manuscript to Geomorphica.
Best wishes

Anonymous Associate Editor

The work is licensed under CC BY 4.0 International

Authors' responses to review comments

Submitted: June 7, 2024

We thank the reviewers and editors very much for their time. We appreciate the comments about the relevance and quality of our work and hope that we have sufficiently improved the manuscript based on comments and suggestions of the reviewers. The main change we have made is clarifying the problem statement in the introduction and aligning the rest of the manuscript with this. Additionally, we have adjusted and clarified the methods used to determine the height and wavelength of the sand strips. Our response to the reviewers is given in blue and the line numbers refer to the track changes document.

Reviewer 1:

The paper describes a new method to detect aeolian sand strips using permanent laser scanning techniques. At the outset, I would like to appreciate the authors for an extremely well-written paper. The manuscript has an appropriate number of figures and detail and describes both the technique, and how results emanating from such methods can provide us information about general sediment transport rates of sand strips. I recommend publication of this paper after addressing the following minor changes.

Thank you very much for the kind words.

Lines 61-63: It might be helpful to explain how the absolute values of reflectance be influenced by moisture content? Would it increase/decrease with increasing moisture?

We agree that this explanation was missing from the manuscript and have changed the sentence from:

"This reflectance intensity is related to the moisture content (…), …" to

Line 79: "This reflectance intensity decreases with increasing moisture content, (…), …"

Lines 143-145 A small graphic describing the coordinate axes might be helpful. Perhaps consider adding a high resolution satellite image of the beach with the location of the PLS setup and the coordinate directions annotated on it.

We have added an extra panel to Figure 2 that shows the location of the laser scanner above the beach and the way the coordinate system was defined (new Figure 2c).

Lines 154-160 It is still slightly confusing for the reader to fully distinguish "reflectance" used in this context, as opposed to the general usage of the term "surface reflectance". I suggest considering using a different term throughout this paper and/or adding a few lines to clearly describe the difference between the two terms.

For consistency with other papers (e.g., Vos et al. 2022) we have decided to keep referring to this parameter as reflectance. We have altered the explanation in this paragraph (Lines 178-186) to clarify the comparison between the unit of the values produced by the laser scanner and the SI-unit, and to explain why we have assigned the symbol *R* to the reflectance:

"The manufacturer of the laser scanner (Riegl) describes the reflecting intensity as a range-corrected amplitude, internally calculated by the scanner based on the calibration of the manufacturer (Pfennigbauer and Ullrich, 2010). This internally corrected reflectance value is dimensionless and therefore differs from the SI-unit for the target surface reflectance (Vos, Anders, Kuschnerus, et al., 2022). Here, we have assigned the symbol *R*

The work is licensed under
CC BY 4.0 International

to the reflectance values produced by the laser scanner. This clarifies the use of the reflectance in any derived units (e.g. Rm^2 in Figure 6)."

Reviewer 2:

The paper addresses the characteristics of aeolian bed forms, known as sand strips, based on permanent laser scanning. I think that the paper will appeal to a significant proportion of the readership of Geomorphica. This paper fits well with the rapidly growing amount of literature on aeolian processes and dynamics in the coastal zone. Furthermore, I believe that the technique (permanent laser scanning) and various aspects of the analysis steps will be of considerable interest to those interested in advanced measuring techniques and remote sensing. The data set is truly impressive and inspiring.

Thank you for outlining the relevance of our work.

Based on my reading of the manuscript, I recommend 'major revision'. In the following I will outline my main concerns, and then describe various minor issues. I believe that the revised paper should be reviewed again. I hope that my review will help to improve the quality of the paper.

Main concerns

1. I struggled with what the actual focus of the paper is. Is it about the laser scanning (PLS) and the analyses to characterize the sand strips, or is about the sand strips themselves? I noted that the Introduction does not provide a clear problem definition. For example, what do we not yet know about sand strips from a scientific point of view and why is gaining this knowledge relevant? And then, why is PLS the technique to be used? Is sand-strip height and long-term relevance to sand transport the main unknown, given that Hage already focused on length and migration? I can phrase this first main concern also as: what is the main novelty of the present work? Is this that PLS was applied (including the algorithm) or that we now know more about sand strips? The problem and scientific aim should be stated explicitly in the Introduction. Then, various parts of the paper may need to be restructured or be extended to better reflect the main novelty of the work (see also main concern 3 below).

The concern that the reviewer raises is valid and probably a result of the manuscript having two interconnected aims: 1) development of a generic method to detect aeolian sand strips and their characteristics, and 2) determining aeolian sediment transport rates related to sand strip migration. We acknowledge that the problem definition related to the second aim was lacking. Therefore, we have added a paragraph in the introduction that specifically addresses this gap in the literature. We think that adding this paragraph also helps to make the need for using PLS clearer. The added paragraph can be found on Lines 55-70:

"The migration of bedforms such as ripples can result in creep transport which contributes to total sediment transport rates. Sherman et al. (2019) quantified the average contribution of ripple migration to total transport as approximately 3.6%. Thus, ripple migration needs to be included when calculating total aeolian sediment transport rates, as has been recognized by, e.g., Uphues et al., (2022); van Rijn & Strypsteen (2020). Like ripples, aeolian sand strips might make a considerable contribution to total transport rates. However, sediment transport rates associated with sand strip migration are unknown and have not yet been addressed in the context of total sediment fluxes in a coastal setting.

The work is licensed under CC BY 4.0 International

Estimates of ripple migration transport have previously been made using (Jerolmack et al., 2006; Sherman et al., 2019; Zhang et al., 2021):

$$q_b = (1-p)\rho_s u_b H/2 \qquad (1)$$

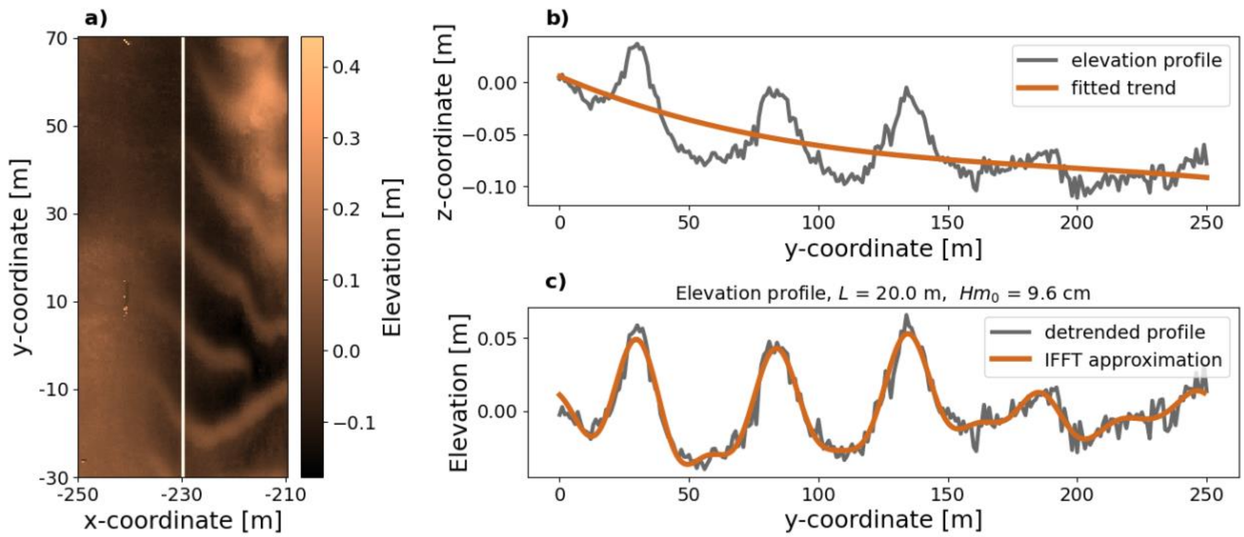
where q_b is the transport rate associated with the bedform, p (-) is the porosity (approx. 0.4), ρ_s is the sediment density (2650 kg/m3 for sand), u_b (m/s) is the migration velocity of the bedform, H (m) is the ripple height. Application of this equation to sand strips requires the assumption that sand strips have a triangular shape and requires co-occurring information about migration speeds and heights. "

2. There are various aspects of the methodology that the authors need to reconsider or at least explain/discuss in more detail:

   1. *Spectra*. If I understood correctly, only information up to dominant k is used for the IFFT step. Then, sand strips are sinusoidal by definition, I assume. Is this realistic? Sand strips don't have slip faces but one might expect them to be asymmetric, with steeper downwind than upwind slopes. Or, they might have other deviations from a purely sinusoidal signal, such as rather peaked crests. Figure 8c suggests that sand strips are indeed not sinusoidal, but rather peaked. To capture this 'asymmetry', it may have been advantageous to also include E(k) at larger k than the peak k. Why not include everything up to two times the peak k, where one would expect the largest contribution to the non-sinusoidal shape (this 2k is first harmonic of peak k)? Does the authors' choice not inherently underestimate the height of sand strips (peaked crests are missed, part of the spectrum is discarded)? I became particularly worried by the approach when I had a look at Figure 14d and g. Here, the IFFT approximation does not seem to be suitable to quantify the length and height at all. Or is Figure 14 an example where the lower L detection limit should have been lower than based on what Hage and co-workers found?

In the detection method and in the determination of the height, the IFFT is only used to check whether the found signal matches the elevation/reflectance profile, not to determine the sand strip characteristics. The height and wavelength of the sand strips are calculated using the characteristics of the found spectral peak. Specifically, the height is determined by using the height of the spectral peak, and the wavelength is determined by using the location (i.e., wave number) of the spectral peak. This means that currently the selected width in the spectrum does not affect the calculation of the height and wavelength. Thus, when applying an increased range from 0 to 2k the fit of the IFFT improves but the calculation of the wavelength and height do not change:

The work is licensed under CC BY 4.0 International



To further explain, the narrow definition of the section of the spectrum used for the IFFT was chosen to prevent detection/height determination in cases where spatial variations occur that are not related to the presence of aeolian sand strips. The assumption is that sand strips cause a strong peak in the spectral signal, however, we agree with the reviewer that this assumption is not applicable in every case. The results in Figure 14 show that the IFFT can deviate from the measured profile in scans with strong spatial variations. It should be noted that the specific scan in Figure 14a is relatively close to not being suitable for detection based on the RMSE between the measurements and the IFFT, being 0.39 for the reflection (limit was 0.5) and 0.014 for the height (limit was 0.02).

We acknowledge that the distinction between the use of the IFFT and the determination of the characteristics of the sand strips was confusing based on the information in the figures. We have added a specific note that the IFFT is not used to calculate the characteristics and have emphasized that the height calculation is based on the spectral peak height on Lines 476-479. We also have included that the current calculation method assumes purely sinusoidal bedforms which results in conservative height and transport estimates (Lines 474-476). Additionally, we have added that basing the estimate of the wavelength on a single location of the spectral peak assumes that wave patterns that are smaller or larger are not related to the presence of sand strips (Lines 428-429). To improve the accuracy of the determined characteristics, we will revisit these assumptions in future research and consider a summation of a wider part of the spectrum.

2. *Interpolation*. What method was used to interpolate the data, if interpolation was required? The reflectance and elevation data are spatially anisotropic. Variability along the crests of the sand strips is substantially less than in the migration direction. Was this anisotropy accounted for, for example, by direction depending semi-variograms? If not, what might the consequence of the anisotropy on the quality of the interpolation have been?

A simple linear interpolation was used for data interpolation (Line 288); thus, we did not account for the spatial anisotropy. We changed the title of the section (Line 286) to

The work is licensed under CC BY 4.0 International

"Subsampling the data to a grid and interpolating" to make sure this information is easier to find for future readers.

Not considering the anisotropy can result in the removal or creation of sand strip patterns where large gaps in the data are present. We have tried to limit the effect of interpolation on the detection method by limiting the application of the 2D method to low interpolation (< 20%) and the 1D method to medium interpolation (20% < interp < 25%). Additionally, we have tried to prevent the interpolation affecting the detection results by filtering the wavenumbers where large areas of interpolation are present (explained on Lines 410-420). Thus, we expect that the anisotropy has no significant effect on our results. However, we acknowledge that filtering the wavenumbers is treating the symptom and not the cause, so we will consider a more sound interpolation method for future versions of the method.

3. *Significant height*. Why did the authors choose the significant height? The IFFT sand strips are more or less sinusoidal, with strongly suppressed height-to-height variability (see Figure 14) compared to the actual signal. The rms value for a single sinusoid (or, $\sqrt{m_0}$ in spectral terms) equals $0.5\sqrt{2}$ times amplitude. With amplitude = 0.5 H, this provides H = $2\sqrt{2}$ rms, and not 4 rms as used by the authors. I feel that the $2\sqrt{2}$ rms approach is more appropriate.

The reviewer is right that there is a more suitable way to execute this calculation. We use the height of the found spectral peak, and thus a single sinusoidal signal, to represent the sand strips. Instead of using the spectral moment, we can directly convert the variance density value of the spectral peak to the amplitude and, in turn, the height H. To implement this, we have removed Equations 4 and 5, and instead included a new Equation 5. This Equation is the reversal of Equation 2, which we changed slightly to make this relation clearer (Line 324). We have implemented this way of calculating H throughout the manuscript, resulting in altered height and sediment transport values (e.g., Lines 485, 569, 572, 668, 771 and 851, and Figures 8, 11 and 14). Since the sand strips are approximated with a sinusoidal signal, peaked crests are not necessarily considered. Therefore, we have noted on Line 476 and 848 that the height and thus the sediment transport rates are a conservative estimate.

4. *Migration speed*. I was somewhat surprised to see that the migration speed was, in essence, determined manually. Why did the authors not use cross-spectral techniques? This would have been more consistent with how the height and length were determined.

We agree with the reviewer that it would have been more consistent and more generically applicable to calculate migration rates using cross-spectral techniques. Since (successfully) implementing this technique would require significant time and effort we were not able to tackle this problem in this project. However, we do hope that in future applications of the method these types of techniques will be applied to extract migration velocities. We have added a comment on this improvement of the method on Lines 723-727.

The work is licensed under
CC BY 4.0 International

5. *False positive/negative*. It would be nice to know a priori what might cause false positive or negative detection of sand strips. In the present paper, this part of the method/algorithm section comes as a surprise. I see that some reasons are provided later on. Should Section 4.1 not be part of Section 3, to illustrate the performance of the algorithm? I think this relates back to the question what this paper is actually about: the algorithm, or the sand strips? If it is about the sand strips, then the Results should start with them, and not with the algorithm, I think.

The aim of the paper is to develop a sand strip detection method, apply it and use it to estimate sediment transport rates. Since the development of the algorithm is an important part of the manuscript, we have decided to leave Section 4.1 in its original location.

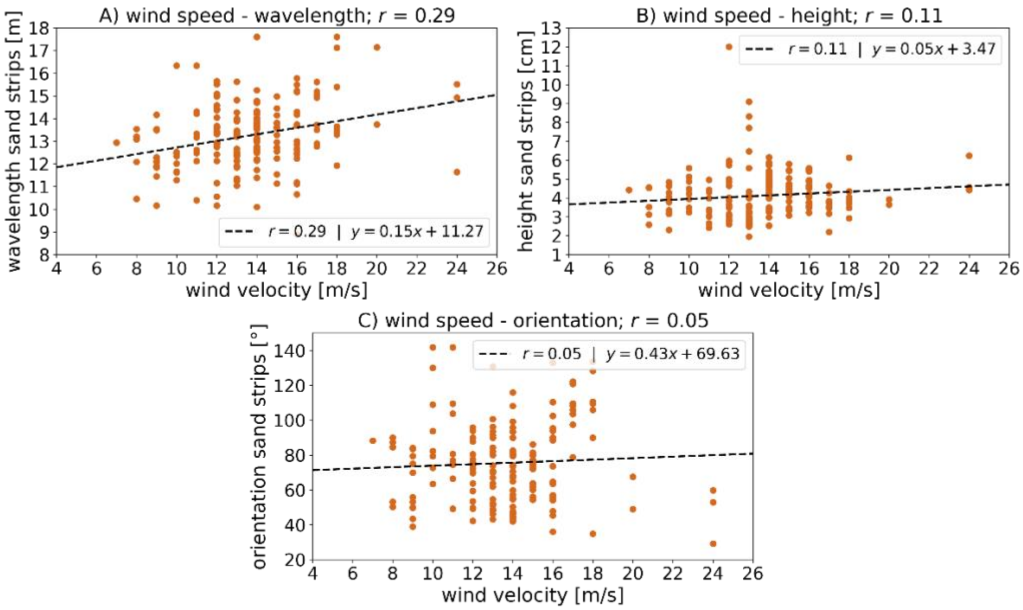
3. Given the effort that has gone in detecting sand strips, I found the actual analysis of the sand strip characteristics to be rather meagre. It could be extended with, for example:

The questions the reviewer raises below are interesting and relevant geomorphological questions relating to the spatial and temporal behavior of the sand strips. We have previously investigated some of these processes as part of an MSc thesis. At this point, we prioritize publishing the method and some of its capabilities over going into the detailed geomorphological processes. Therefore, we decided to make the main aim of this paper to develop the method, apply it and determine sediment transport rates. As a result, the more detailed process analyses are outside the scope of this paper. Below, we will show some of the results of our geomorphological analysis. Naturally, we hope that the use of this method in future research will result in answering many of the questions the reviewer raises.

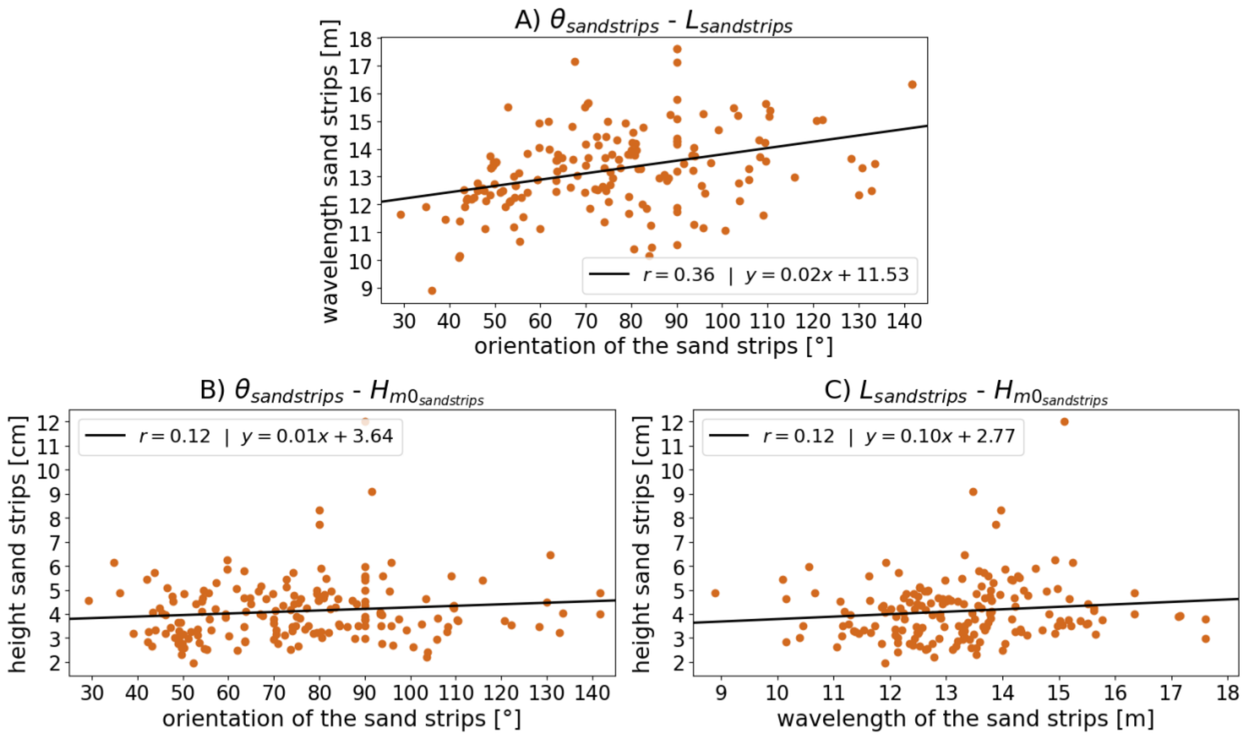
1. Do sand strip height and length depend on environmental conditions, like wind speed or direction? Are length and height related (a longer sand strip has a higher height, for example)?

We investigated the relationship between the wind speed and the wavelength, height, and orientation of the sand strips and did not find a strong relationship between the wind speed and these characteristics. The strongest relationship was found between the wind speed and wavelength:

The work is licensed under CC BY 4.0 International



Similarly, we investigated the relationship between the sand strip characteristics. Note that the orientation in the upper subplot is with respect to the shore-normal, so a value of 90 degrees implies alongshore oriented sand strips. We found the strongest relationship between the sand strip wavelength and orientation, suggesting that there are longer wavelengths when the orientation of the sand strips is more alongshore.



We did not investigate the relationship between the wind direction and the wavelength and height.

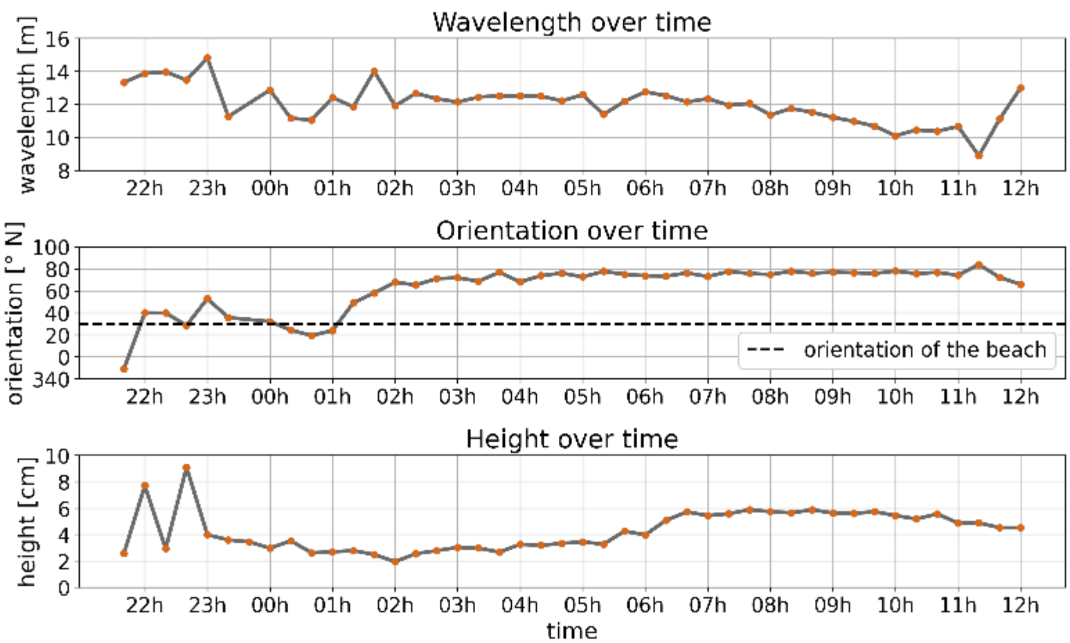
The work is licensed under CC BY 4.0 International

2. Is there a clear temporal signal in length and height (i.e., change with time) during each sand strip event? What does this temporal change (or absence thereof) imply? If there is no such temporal signal, then this is interesting information too.

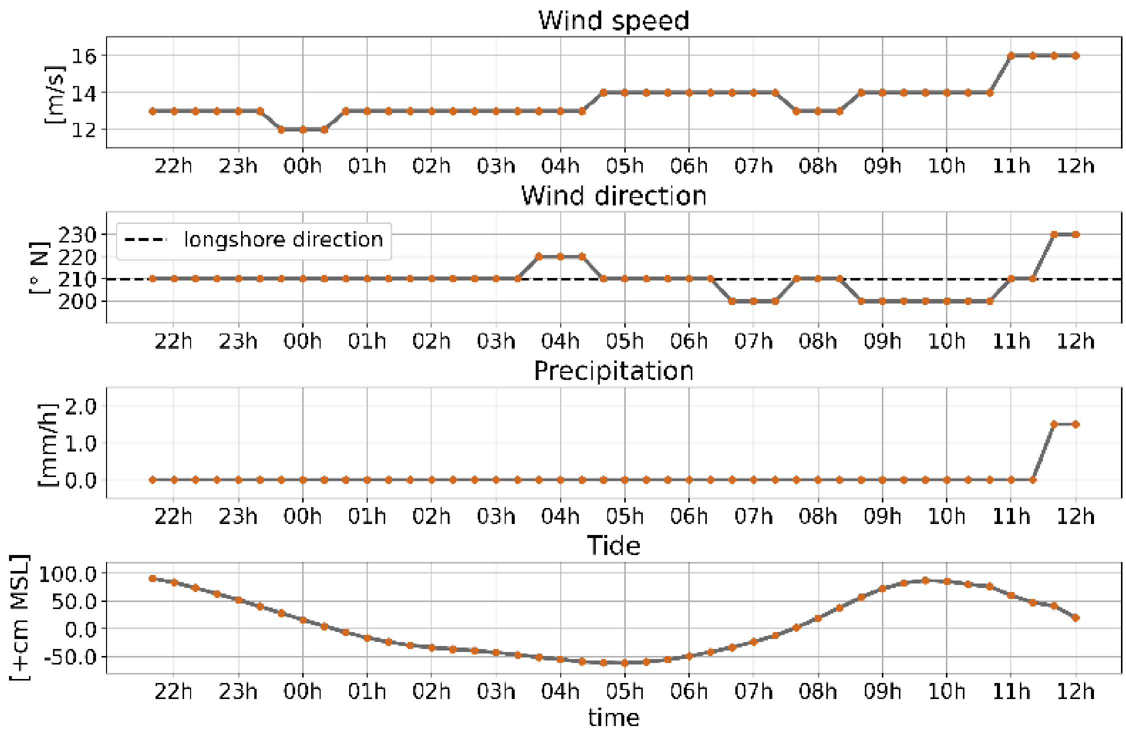
We analyzed the temporal variation in the sand strip characteristics based on the same event that we used to extract the migration velocities (23-24 February 2022). The figures below show the development in the characteristics and the environmental conditions during the sand strip event on 23-24 February. Note that the sand strip orientation in the second subplot is given in degrees with respect to north. Thus, a value of 30 degrees implies alongshore oriented sand strips.

During the event on 23-24 February, we saw a decrease in wavelength co-occurring with an increase of the obliquity of the orientation of the sand strips. This matches the relationship between the wavelength and orientation shown above. Over time, the sand strips became more aligned with the alongshore oriented wind direction. The height of the sand strips increases in the second half of the event, which might be related to an increase in the wind speed. Information about the disappearance of these sand strips is unfortunately missing due to bad quality data.

We did not include this analysis in the manuscript since it is based on only one event, which limits its generic applicability. Improved processing methods for the output of the sand strip detection method would allow for a more elaborate analysis of these types of temporal variations for the entire study period and beyond.



The work is licensed under CC BY 4.0 International



3. Where do sand strips actually form? Mainly on the intertidal beach? Or on the upper beach? This also relates to whether tides can cause the disappearance of the sand strips. The tide doesn't reach the upper beach. Or did sand strips on the intertidal beach sometimes survive because the next high tide was not as high as the previous high tide?

We have not (yet) studied the spatial variation of formation and survival. Purely based on our visual inspection of the dataset, we have seen that the sand strips can form on the upper beach and then survive because the high tide does not reach them. For now, this analysis is outside of the scope of the manuscript.

4. Does rain intensity or amount affect whether sand strips ‘survive’ a rain event? You show results for a single event, but to what degree is this representative of the entire data set?

In the current time series, sand strips survived both during smaller (15 Feb) and larger (4 Feb) precipitation events. A longer time series would probably be needed to distinguish the effect of rain intensity on sand strip occurrence, especially since the overall moisture content (impacted by weather, tides, and wave conditions) is probably of influence as well.

Overall, there were 13 precipitation events in the study period during which sand strips were detected. During 3 of those, sand strips were detected during the peak in precipitation. For several events, the detection was impacted by bad quality data. The clearest events where sand strips did not occur during a precipitation peak, but they did occur before and after, took place on 16 and 20 February. The event on 16 February was shown in the manuscript, the event on 20 February showed similar behavior, i.e., the

The work is licensed under CC BY 4.0 International

signal in the reflection data reduced during precipitation but the signal in the elevation data remained present. We have added a note on Line 670 about the similarity between 16 and 20 February.

4. The attempt to relate sand strip migration to sand transport rates is interesting, although I think the authors try to get too much out of it when they relate it to foredune volume growth. As the authors say, sand strips migrate predominantly alongshore. And for foredune volume growth, they should take the onshore component of the transport rate, see, for example Bauer and Davidson-Arnott (2002, Geomorphology; the cos α term in that paper). I expect the onshore component of the sand-strip flux to be minimal, and so their contribution to foredune growth. If the authors wish to have the foredune growth included in the discussion section, then the onshore component should be used, not the total component.

We have included the calculation of the cross-shore component of sediment transport related to sand strip migration on Lines 856-858.

5. The Abstract and Conclusions can be made more quantitative. Do mention key numbers from your results, and only then compare to the existing literature. And, the Abstract should also state the main problem and aim. Possibly, also the Conclusions should be refocused when the problem and aim are stated more explicitly in the Introduction section, in order to better reflect the main novelty of the work.

We have included several average values (Lines 12 and 881) in the abstract and conclusions. Additionally, we increased the emphasis on the sediment transport rates in the abstract (Line 7) and conclusions (Line 886) to better align with the problem statement in the introduction.

Minor concerns:

1. I am missing a reference to Nield (2011, Geology) in the Introduction. This paper is one of the first that deals exclusively with sand strips and also proposes a generation mechanism.

Thank you for this suggestion. We have added it to the introduction on Line 43.

2. In the method, should the stated footprint not depend on the range and the orientation of the target relative to the laser beam? If not, then I probably misunderstand what the authors mean by footprint.

The footprint does indeed depend on the distance from the laser scanner and the incidence angle. The estimate we provided is for the area of interest on the beach. We have added this information on Lines 167-169.

3. I found the 10% error in gravimetric moisture content a somewhat confusing statement, given that gravimetric moisture content is given in % too. I suppose that if the moisture content is (for example) 5%, you consider the error to be 0.5%? And if so, is that then 5 ±5% (width of 1%) or 5 ± 0.25% (width of 0.5%). What is this 10% error based on? And, finally, where in the Results section is this error used? It does not appear to be used in Section 4.2.1.

The work is licensed under CC BY 4.0 International

The provided estimated error was based on the measurement method. The value is a crude estimation to account for sampling errors, loss of moisture in transport and weighing errors (added on Lines 205-207). The most important context it gives in the results section is that the measured difference between the sand strips and the surrounding beach is significant. We have added this context on Line 561.

4. Are you sure that the wind data from IJmuiden is at 4.4 m above bed level? This may be where the sensor is actually placed, but, in general, the KNMI provides data corrected to 10 m above the bed only (10 m is the international standard). If it is indeed 4.4 m, then it is not the same height as used in the various Hage papers. How does this influence the comparison to those papers?

Thank you for pointing this out. We have included the correct 10 m height (Line 147).

5. I found it very interesting to see that (minimum) wind speeds for sand strip formation and occurrence are different. Is this comparable to the different thresholds (fluid versus impact) for aeolian transport? Or does it relate to the time needed for sand strips to actually form? It may be that they start to form at 8 m/s but are detectable only when the wind speed has already increased (with time) to more than 8 m/s.

We also found this difference striking. To tease out whether this behavior is a supply or formation time issue, a detailed investigation of formation events could be executed. For example, formation events on 12 and 15 February would be interesting to study, especially by focusing on the available height information before and after initial detection. For now, this investigation is outside the scope of the manuscript.

6. Are sand strips during precipitation events (when detected) still active (e.g., do they still migrate?), or is it best to consider them 'fossilized'?

This is an interesting thought. We suspect that sand strips fossilize since aeolian sediment transport is reduced during precipitation. A detailed investigation of sand strip migration around precipitation peaks could shine a light on this, but it is currently outside the scope of this manuscript.

7. In the computation of the sand transport rates, the authors assume the sand strips to be triangular, whereas in their IFFT approximation the sand strips are (more-or-less) sinusoidal. Isn't there an equation for sinusoidal bedforms? If not, then what is the consequence of assuming a triangular shape? Are transport rates biased low or high because of the different shape in reality?

We are not aware of a sediment transport calculation for sinusoidal bedforms. The consequence of assuming a triangular shape is an underestimation of the volume in the sand strips and thus an underestimation of transport. An improved calculation of the sand strip height and a closer approximation of the sand strip shape could improve the accuracy of the calculated sediment transport rates. We have improved our explanation of the height calculation and its effect on the sediment transport rate calculation on Lines 469-479 and on Lines 847-848.

The work is licensed under *CC BY 4.0 International*

Round 2 Review

Comments by reviewer 1 (Gerben Ruessink)

Completed: 2024-07-15 12:59 PM
Recommendation: Minor revisions

Reviewer Comments

For author and editor

I thank the authors for their extensive reply to the comments that I raised during the first review round. I am particularly pleased to see that the authors have clarified the aims of the paper and I therefore understand that the authors consider several of my previous comments (e.g., my main concern #3 on additional sand-strip analyses) to be beyond the scope of the paper. I believe that the answers to several other concerns should not be provided in the rebuttal report only, but should also incorporated in the paper. This is not to discredit the chosen approach, but rather to tell the readers that they thought carefully about certain choices and their consequences, and to acknowledge that/how the algorithm can be improved further. On the whole, I would like the authors to consider the following points:
1. As the authors state, the height and wavelength of the sand strips are calculated using the characteristics of the spectral peak. This implies, as also stated by the authors, that variations at other spatial scales are not considered to be sand strips. I understand that this can be a pragmatic choice, but what is the physical basis for this choice? The authors should thus better motivate why they made the assumption stated in lines 428-429.
2. My suggestion to use a wide k-range to better approximate the non-sinusoidal shape or small-scale spatial variability in sand-strip characteristics can be added to the end of Section 5.1, especially because the authors state in their rebuttal "To improve the accuracy of the determined characteristics, we will revisit these assumptions in future research and consider a summation of a wider part of the spectrum." At the end of this subsection, the authors may also wish to refer back to Figure 14, and explain "the mismatch" between the measured elevation and the IFFT approximation. I am not convinced by the authors response to my concern w.r.t. Figure 14. If the IFFT is a poor representation of the elevation profile, then the height and length may not be representative either. After all, the IFFT and the height & length are based both on (parts of) the k-spectrum. If the authors use a wider k range in future work, the height can also be based on this range, rather than on the peak k. I would like to draw an analogy here to wind waves in the sea surface: their length is typically based on the peak wave period, while their height is based on an integration over a considerable part of the spectrum, and not just on the energy at the peak frequency.
3. The answer to my concern on the interpolation (anisotropy) should be incorporated in the paper, section 3.3.2. I think that readers will appreciate that the authors thought about this issue.
4. The answer to my minor concern #6 (on sand strips during precipitation) can be incorporated in the paper, near lines 816-817. The morphology may stay intact, but that doesn't mean the sand strips are

The work is licensed under CC BY 4.0 International

still active. It may be good to acknowledge this in the text, simply to avoid the impression that "being present implies being active".
5. Line 886: I think it would be wise to state here that the total flux is in the direction of sand strip migration (to avoid the impression that it is a cross-shore m3 per m alongshore distance).
6. Line 17 (Abstract): The phrase "considerably" is not substantiated here. The estimated onshore flux for a single 8-hour flux is 0.07 m3/m (line 858). In the Discussion section the authors say "If several of these events occur per year, transport rates associated with sand strips might contribute considerably to the amount of dune growth." What is "several"? If it were 10 (for example), then 0.7 m3/m is still less than (at most) 10% of the 10 - 20 m3/m annual foredune growth. Personally, I would not call that 'considerable'. To avoid a false impression from 'considerably' in the Abstract, I invite the authors to simply state the estimated total flux in the Abstract and to remove the remark here on whether this is 'considerable'. If the authors wish to keep 'considerably' in the Abstract, it should be put in context.

The work is licensed under CC BY 4.0 International

Recommendation by Associate Editor 2 (Rocio Luz Fernandez)

The recommendation regarding the submission to Geomorphica, "Detection and characterization of aeolian sand strips on the beach using permanent laser scanning" is: Accept Submission .

A minor comment from me is about the title. Estimating the sediment flux is one of the important parts of the manuscript, therefore, I advise that the authors may consider reflecting the “Estimating of sediment flux/transport” in the title. The title could be like “Detection and characterization of aeolian sand strips and estimation of sediment fluxes on the beach using permanent laser scanning” or “Estimation of sediment fluxes by aeolian sand strips on the beach using permanent laser scanning” or “Utilization of permanent laser scan to characterize aeolian sand strips and estimation of sediment fluxes”. Or similar one that includes the “estimation of sediment flux/transport”.

The work is licensed under
CC BY 4.0 International