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Interactive comment on "Accommodation space indicates dune development potential along an urbanized and frequently nourished coastline" *by* Corjan Nolet and Michel J. P. M. Riksen

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Thank you for allowing me to comment on your article and apologies for the delay in sending this review. I hope my feedback below is useful and I will be happy to further discuss changes to the paper should you wish to do so.

I have very much enjoyed your article, and I believe there is good material in it. I think the text needs work though and I would concentrate in two main areas: (1) the methods and (2) the definition of 'favorable' accommodation space. You have a really good dataset but the pixel resolution of both your LiDAR and Sentinel data has limitations. The error introduced by these limitations could be quantified, as this has implications

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Discussion paper

for both your results/conclusions and for the maps you produce. Additionally, some of the generalizations in this article may need field validation (in my opinion), especially those related to the concept of 'favorable accommodation space'. My concern with this concept (as it is currently defined in the text) is that it does not consider surface conditions having an effect on aeolian transport along this section of the coastline (which could in fact determine whether the accommodation space is indeed 'favorable' or not) or conditions for the colonization of new marram grass seeds on the beach. It might be a matter of further discussing this in the article? (I have included some ideas below which you may want to consider). Finally, and although I can see the benefit of looking at long sections of the coast, I feel that the article has the potential of providing much more detail on the morphological evolution of different areas by, for example, zooming into particular sections of the dunes.

Here are a few major comments:

1. Very nice introduction, and great to read the contextual and historical information about this part of the Dutch coast in section 2.1. I think, however, that there is a need to include some additional information on the peculiarities of aeolian transport at the Zandmotor. The paper gravitates around 2 conditions for the development of coastal dunes: 1) steady supply of wind-blown sand; and 2) wide accommodation space (or simply beach width). The second is important as it is related with fetch distances, etc. (I'm suggesting the paper by Delgado-Fernandez, 2010, on the fetch effect because it is a review article but please choose any other that may suit you well), additional to reducing storm impact as you describe in the article. However, accommodation space is only one of the many variables involved in sediment input to coastal dunes. Aeolian transport is a function of many surface conditions including the formation of lag deposits or the composition of sediment, which seems to be an important limiting aspect at the Zandmotor (as described by the group of de Vries and Honhout, etc.). This is, the Zandmotor does seem to have created accommodation space but the question is how 'favourable' this accommodation space really is for maximizing aeolian transport and

hence dune growth (not necessarily just limiting dune erosion). I would recommend that potential supply-limiting conditions affecting transport by wind at this location are reviewed in the introduction as these affect your condition n. 1 ('steady supply of wind-blown sand').

2. The inclusion of supply limiting conditions above might have implications for your discussion. Your section 4.1 suggests that you are using the presence of vegetation as a proxy for sediment input to the dunes (something I come back to in comment n. 9). I do not think 1 year is long enough for the analyses you make. However, even if it was, your figure 4 seems to suggest that vegetation in areas other than the Zandmotor has followed a similar trend than those fronted by the Zandmotor. Your morphological data tells a similar story. Volumetric changes of foredunes at the Zandmotor are quite comparable (and even lower) than volumetric changes of foredunes in another areas (e.g., to the W). This would suggest to me that the extra accommodation space provided by the Zandmotor is not having a clear effect (at least at this temporal scale) on sediment input to coastal dunes, and that other variables (e.g., supply limiting conditions as suggested by de Vries and Honhout) are playing an important role at this location.

3. Coastal morphology section. You mention in P4-L23 that 'Changes in coastal morphology (are) expressed by average change in height per year . . . ' Your figures seem to indicate that you've been calculating DTMs of difference? (i.e., simply extracting one DTM from the previous one to see spatial changes in elevation). This sounds good to me but if this is the case, then I would engage a bit more with the literature on DoDs, including the application of geomorphic change detection software to account for uncertainty in DEMs (e.g., Wheaton et al. 2010. Accounting for uncertainty in DEMs from repeat topographic surveys: improved sediment budgets. ESLP).

4. Following on the previous comment, your DTMs are fantastic in terms of their spatial extent, but 2m pixel resolutions are still quite coarse to detect some dune morphological changes in just 4 years (specially if it is dune accretion). I think it would be worth exploring the error associated to using 2 m pixel resolution DTM's in your volumetric analyses.

Also, could you mention in the methods how are the volumes reported in Figure 4C and in the results calculated? What do you mean by 'coast'? ('beach'+'dunes'?). Are you including embryo dunes in the beach budget or in the dunes budget (or in none)?

5. Figure 4 (and associated text) – how do you morphologically define embryo dunes in this figure? The beach is defined as areas from 0-6 m and foredune from 6-12 m (if I'm getting things correctly). However, how do you characterize embryo-dunes? They are classified as '<6m' which is basically the beach too, and a height below your foredune toe? Would it be possible for you to include the procedure by which you've morphologically identified embryo dunes using DTMs (and how these are different from bedforms)? Do you think the 2m pixel resolution DTM is limiting the analyses in here?

6. Figure 5 – how did you measure dune height? This seems like a straightforward question, but could you include some details of this procedure in the methods? Did you use transects along the coast and picked the highest point at the dune crest? Or maybe contour lines, etc.?

7. There are methodological details throughout the results that should be placed within the methods section: e.g., P6, information in this page about the extent of the spatial analyses (e.g., L24) or the area covered by the domain (L19). Also, could you elaborate on the spatial extent? What is the landward limit of the area you are looking at? Is it the road or the path that seems to run landwards from the main foredune in Figure 1?

8. P4, L28 - There seems to be other type of vegetation species in Figure 1. How did you differentiate dune cover by marram grass from dune cover by other plants? (was this important?). Also, and although this is minor, references in page 5, L5 (NDVI) are good but a bit dated. Any examples of recent uses of NDVI on coastal dune vegetation? Also, could you explain how do you differentiate between vegetation cover over embryo dunes vs. foredunes? (P7, L10, or figure 4). Do you first identify embryo dunes morphologically (see comment 5) and then just simply map the vegetation data over them to separate embryo dune vegetation?

9. P7, L31 – You suggest that it is possible to 'verify' that there is a 'steady accumulation of wind-blown sand' by comparing changes in vegetation (from 2016-2017) with average yearly changes in dune heights (from 2013-2017). I am not sure I am looking at this from the correct angle, but I would argue that this assertion probably needs some field testing, or some validation using actual observations of sand transport at different locations along the coast. I wonder if there is already published material at this site giving you an order of magnitude of how much sand is moved by wind at different locations? (perhaps the work by de Vries and Honhout which you cite?). Also, vegetation can change (marram can grow or die) without morphological changes to the dunes (specially in such a short period of time of 1 year). This takes me back to L7 in this page (text corresponding to map 4D), where you propose that changes in marram grass cover can be used as a proxy for dune development (I would disagree with this because the marram can expand over 1 year without dune growth).

10. Really nice discussion on human impact and the role played by 'dynamic restoration'. I think your figure 4 and additional figure 8 are actually really good for this section, because they provide a very clear holistic view of the entire coastline, and help contextualize human impact. I think this part of the paper is important and I would leave it as is.

Some additional comments are:

Figure 3 – as you rightly identify in the text, the validation results for the Sentinel 2 cover are relatively weak. I agree with your explanations of figure 3, and I can see the value of using Sentinel 2 data for identifying relative trends in vegetation expansion. However, I think your results indicate that we should be rather cautions when using linear spectral unmixing over Sentinel data to derive actual values of vegetation cover. I've got two questions about this: 1) are the UAV data used in the validation vegetation density using the original 5 cm pixels or the aggregated 10 m pixels (the explanations in the current text are a bit confusing – page 5, L22 to page 6, L4); could results improve by using other type of analyses (if they exist) to derive sub-pixel proportions of

vegetation?

Figure 4B – why did you choose to present elevation change data using classes with different ranges? Your first class (-1.5 to -0.25) groups pixels with elevation changes of up to -1.25 m, your next two classes cover small elevation changes of +/-0.25 m, and your fourth class covers an elevation change of +1 m. Why this particular classification? It does not allow differentiating large elevation changes (+/-1 m) from relatively lower ones.

P6, L23 - . . . 'This indicates that the foredunes, since their construction in 2011, have been raised in height by 2 m due to aeolian deposition'. Could you elaborate on this? There are very large sections of the dunes in figure 4B with little to no changes (your red category). Could you indicate examples in your domain where the dunes have been raised in height?

P6, L29 – Following the first comment on Figure 4B, it is true that there seems to be a relatively continuous 'brown' line alongshore the entire domain in figure 4B. However, it is difficult to see whether this line corresponds to foredune or embryo-dunes growth. Also, it is difficult to understand the magnitude of elevation changes because 'brown' pixels are anything from 25 cm to 1.25 m. This takes me to a general comment about figure 4. I quite like it and I can see the benefits of looking at things over a large area. However, the reality is that the figure is quite difficult to interpret, and the large extent means that it is difficult to appreciate the details. I have enjoyed your arguments in terms of general trends but I think the paper would benefit from exploring different areas by sectors too? I'll leave this comment in here for you to decide.

Section 2.3.1 Linear spectral unmixing. I'm not familiar with this technique but I can see the benefits of it. However, I feel this section would benefit from including some additional references to recent work using this technique. Similar to the NDVI, these procedures are widely applied across different disciplines. Could you perhaps enrich the text by adding some recent examples of applications of these techniques to coastal

dunes or dune vegetation in general?

P9, L25 – I would recommend discussing this in the context of Davidson-Arnott et al's article (2018) on sediment budget controls on foredune height (ESPL 43).

Picky comments:

P2, paragraph starting in L7 – I quite like this paragraph and I think it is great to remind people how important coastal dune vegetation is for coastal safety. However, I wonder if you could substitute 'marram' here by 'vegetation' (specially when you talk about temperate areas in general). European marram grass is not native in many coastlines around the world where other important plants play the role of dune building.

Figure 1A –why not use a colour scheme for the 2017 elevation map? You could still overlap this over the grey area representing the extent of the 2013 Zandmotor.

P6, L19 – the area shaded in grey is not visible in Figure 4A. Perhaps there is a different way to represent this, such as a green line or other type of polyline?

In summary, a nice paper with a lot of potential. I think you've got a good amount of data and that the results are interesting, but I would work a bit more on a) trying to provide a more detailed account of the volumetric error associated with DTMs, and b) including the role played by supply-limiting conditions on the characterisation of a 'favourable' accommodation space.

Hope this helps. Do not hesitate to contact me should you need additional details, or should you want to discuss my comments. As usual, it is great to read new articles coming out of this incredible coastal experiment.

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