Editor

Earth Surface Dynamics

September 20, 2020

Dear Editor,

We would like to thank you for your handling and response to our submission. Please find enclosed the revised manuscript for *Earth Surface Dynamics*, entitled "*Different coastal marsh sites reflect similar topographic conditions for bare patches and vegetation recovery*" [Paper #esurf-2020-56], and detailed list of our responses to the comments of the two reviewers. We highly appreciated the comments made by the reviewers, as they enabled us to greatly improve the manuscript.

Below we give a step-by-step response to the comments. The original comments of the editor and reviewers are copied below and shown in black. Our step-by-step replies are inserted and shown in blue. The line numbers that are mentioned refer to the line numbers in the revised manuscript with tracked changes.

Thank you very much for your continued consideration of this manuscript.

Looking forward to your reply.

Yours sincerely,

On behalf of all co-authors,

Chen Wang

"Different coastal marsh sites reflect similar topographic conditions for bare patches and vegetation recovery" [Paper #esurf-2020-56]

Chen Wang, Lennert Schepers, Matthew L. Kirwan, Enrica Belluco, Andrea D'Alpaos, Qiao Wang, Shoujing Yin, and Stijn Temmerman

List of response to the comments

1. Response to the comments from Reviewer #1

Interactive comment on "Different coastal marsh sites reflect similar topographic conditions for bare patches and vegetation recovery" by Chen Wang et al.

Anonymous Referee #1

This paper is generally well written, studied the topographical conditions determining the presence and revegetation of bare patches in three marsh sites with contrasting tidal range, sediment supply and plant species, respectively distributed in three countries. The introduction is detailed, the methodology is well described, the results are clearly described, and the discussion is well-founded and consistent with current knowledge of the subject. Even so, I suggest some minor changes that I detail below, which I hope would be useful to improve this paper.

Response: We thank the reviewer for this overall positive evaluation.

(1) The information about some materials and methods should be more detailed and clear in the abstract.

Response: This is addressed on lines 19-21 of the revised manuscript (the version with tracked changes), which are modified to emphasize information on the materials and methods:

"Based on GIS analyses of aerial photos and LIDAR imagery of high resolution (≤ 2 x 2 m pixels), we analyzed the topographic conditions under which bare patches occur, including their surface elevation, size, distance to and connectivity to channels."

(2) The Study area, Materials and data preprocessing, and Data analysis are too long, it's better to make them a bit more concise.

Response: We shortened parts in the Sections of Study area, Materials and data preprocessing, and Data analysis. Especially the Study area was considerable shortened. We refer to the revised manuscript (the version with tracked changes) to see the text revisions.

(3) It's better to detail some of the implications of the results and some useful advice to the policy maker in the discussion.

Response: We added a paragraph in the discussion at the end of section 6.3 (line 523-527):

"Finally, our results may be indicative to decision makers on salt marsh management, as the formation of bare patches may be indicative for marsh degradation towards an unvegetated state that may be difficult to recover. Our study indicates that early signatures for marsh degradation must be particularly monitored in marsh portions, farthest away from main channels and with lowest surface elevations. Monitoring of early signatures is especially advised in systems with very low tidal range and suspended sediment availability."

(4) I suggest to move some figures to the supporting information, as there are too many figures in the main text now.

Response: We moved figure 7 to the Supplement (Figure S4). Panels (b) of Fig. 1-3 are also moved to the Supplement. Fig. 9 (b) is removed, as suggested by the next reviewer. We also re-arranged Fig. 1-3 to save space.

2. Response to the comments from Reviewer #2

Interactive comment on "Different coastal marsh sites reflect similar topographic conditions for bare patches and vegetation recovery" by Chen Wang et al.

Maarten Kleinhans (Referee)

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1 Main comments

This manuscript presents a data analysis of bare patches in saltmarsh, in particular of the causal variables deemed to govern their formation and possible revegetation. Three different systems are analysed with different tidal ranges and sediment availability. Two main conclusions seem not sufficiently well supported.

The first is that sediment availability and tidal range determine the potential for revegetation, but three study areas are insufficient to isolate one of these two variables, let alone assess their effect in combination. Two of the areas have low sediment availability and the one area

with more sediment also has the highest tidal range.

Response: Indeed we need to emphasize that we only investigated 3 sites, which it is not enough to fully assess the impact of site-differences, such as in tidal range and sediment supply, on occurrence and revegetation of bare patches. We only notice that revegetation only occurred at the site with largest tidal range and sediment supply, while it was not observed at the two other sites with smaller tidal range and sediment supply. But a much higher number of different sites should be investigated, to further assess the effects of tidal range and sediment supply on the occurrence and revegetation of bare patches.

We highlighted this in the discussion, by making the following modifications in the Discussion (section 6.3), lines 477-483:

"Previous modelling has suggested that pond formation increases and pond recovery decreases in marsh sites that are subject to a lower suspended sediment availability, smaller tidal range, and lower rate of relative sea level rise (RSLR) (Mariotti, 2016). First of all, we want to emphasize that we only investigated three sites, which it is not enough to fully assess the impact of site-differences, such as in tidal range, sediment supply, and rate of RSLR, on occurrence and revegetation of bare patches. Yet we notice that revegetation only occurred at the site (Saeftinghe) with largest tidal range and sediment supply, while it was not observed at the two other sites (San Felice and Blackwater) with smaller tidal range and sediment supply."

It was also highlighted in the conclusion, lines 600-603:

"However, we emphasize that our study only included three sites, and that further research comparing much more sites is needed, to further advance our understanding of why certain marsh sites are more vulnerable to others to formation and persistence of bare patches. Such knowledge will be important to inform decision makers on site-specific priorities for marsh conservation."

The second conclusion is that the appearance and possible disappearance of unvegetated patches in saltmarsh systems are acting as a bistable state system. While this concept is currently in fashion, the work done here is of interest in its own right and there appears no other support for the idea in this paper than the frequent use of it in other saltmarsh papers.

Response: The reviewer also comes back on this general comment in his more detailed comments below. We agree that our interpretation of results, in context of the alternative stable state theory, was inspired by previous literature on salt marshes, and interpretation of vegetated marshes and unvegetated tidal flats as alternative ecosystem states. We agree that, in this respect, the interpretation of our results is hypothetical. Therefore we followed the advice of the reviewer below, to remove section 6.4 from the discussion, and to remove panel (b) from Figure 9.

Accordingly, we also removed the last two sentences of the original abstract, where we summarized our interpretations in the context of the alternative stable state theory.

Furthermore there are some unanswered questions, such as whether inundation duration would not be a more appropriate biophysical boundary condition than the elevation in the tidal frame. A number of the variables that the study refers to, such as sediment availability, are not measured.

Response: These comments are further detailed below by the reviewer, and we will reply to them below.

Finally work needs to be done on the figures for a clearer presentation of the data and its context. These issues together, further detailed below, suggest that a moderate revision is needed.

Response: These comments are further detailed below by the reviewer, and we will reply to them below.

2 Detailed comments

2.1 Preamble and conclusions

The title does not reflect the contents and is ambivalent (do the similar topographic conditions refer to different coastal marsh sites or to bare patches and vegetation recovery?)

Response: We changed the title to make it more explicit, as follows:

“Different coastal marsh sites reflect similar topographic conditions under which bare patches and vegetation recovery occurs”

The abstract requires some clarification: the sentence "Our results demonstrate that ... distance from the main channels." Do the authors simply mean with ‘across’ that that all the sites show the same pattern? What kinds of channels are the patches connected to, since these are furthest away from the main channels (whatever they are)?

Response: We changed this sentence to make it clear (line 22-25):

“Our results demonstrate that, for the different marsh sites, bare patches can be connected or unconnected to the channel network, and that the width of the connecting channels increases with the size of the bare patches, in each of the three marsh sites. Further, pixels located in bare patches connected to channels occur most frequently at the lowest elevations and farthest distance from the channels.”

Hence we added here that the analysis is done for individual pixels (which are ≤ 2 x 2 m, as explained in the previous sentence in the manuscript), and that the frequency distribution of these pixels, located in bare taches connected to channels, peaks at the lowest elevations and farthest distance from channels. This is because connected bare patches are also the largest patches (this was added in the first sentence above), containing much pixels far away from channels. We don't make a distinction between 'main' channels and 'other' channels, or whatsoever, but we analyzed the frequency distribution of distances to all channels. Hence we removed the word 'main' channels.

The conclusion of the abstract that bare patches may form rapidly and become vegetated rapidly in the unstable zone at intermediate channel distances is based on only one of the sites, which begs the question whether the proposed existence of two stable states can be supported by the data, and how those bare patches at the other sites came about. Were they always unvegetated? Did they die off when the inundation duration increased, as the saltmarsh developed and reduced the outflow at these locations?

Response: Also in response to a previous remark above, we significantly reduced our interpretation of the results in terms of the theory on alternative ecosystem states. Because indeed, in this respect, our interpretation is rather hypothetical. Accordingly, in the abstract, we also removed this reference to alternative stable states.

Line 387 provides an interpretation of why the bare patches sit on higher elevations. This is based on expectations (meaning inferences without evidence), rather than measurement, and not even basic calculations (or readings from the classic wind waveheight plots on the basis windspeed, fetch (here patch size) and depth) are provided. Possibly the ideas here are biased by the reviewed literature as well and other alternative hypothesis could explain the observations. In Brückner et al. (2019, https://doi.org/10.1029/2019JF005092, also situated in the Western Scheldt) the modelling shows that expanding saltmarsh may, counterintuitively, lead to increased inundation duration within the marsh, which then leads to die-off. Indeed, the elevation within the tidal frame (as used here) may not be the appropriate measure. I wonder what the inundation duration, or perhaps the hydrodynamic energy, is at the elevations of the connected and the disconnected bare patches, and whether too long inundation has to do with the die-off (assuming these patches were vegetated before), as suggested in Brückner et al. This also fits with the observation that sediment supply is needed to lift up the area and reduce inundation for revegetation.

Response: Indeed we discuss several possible hypotheses quite extensively in section 6.1., by referring to existing insights from the literature. We thank the reviewer for focusing our attention to the paper by Brückner et al., and added it in the hypothetical discussion in line 441-446:

"This micro-topography of levees close to channels and depressions further away from channels, is often associated with an increasing inundation duration after high tides, and decreasing soil drainage/aeration during low tides, with increasing distance from channels (e.g., Ursino et al., 2004). Also, a modelling study suggested that marsh vegetation expansion can lead to increased inundation time, and as such can feedback on increased stress and

chance for vegetation die-back (Brückner et al. 2019). This may be all mechanisms that may contribute to increased chance for occurrence of bare patches within marshes at farther distances from channels."

2.2 Comments on results

Figures 1 to 3 show insufficient context. One key variable for the authors is connectivity of the patches in terms of distance to channels, so the bigger context of the study areas must be show to see the bigger and smaller channels. This would be more interest- ing information than the photographs in the panels (which have different meanings for colour anyway). An image or lidar map showing the surrounding landscape including the channels would be more useful here, and the original images can go to the online supplement. For Saeftinghe I checked and the study location in Figure 7 is quite close to the embanked boundary of the system (so the white band on the bottom left is in fact an embankment). In fact the right zone is quite close to an old embankment within the area and one wonders whether that leads to enhanced ponding and a modified channel pattern like one can see further east along the dike.

Response: We moved the aerial images (panels (b) in Figs. 1 to 3) to the online supplementary materials. Also, we added images of the larger surrounding landscape for the three study sites. We added these images also in the online supplementary materials, instead of in the main manuscript, as it contains already quite a lot of figures, and the other reviewer suggested reducing the number of figures in the main manuscript.

Why are there bare patches not considered in Fig. 3?

Response: This is explained in the methods section 3.4, line 237-241:

"In the Blackwater study site, we selected a study area away from the influence of roads and uplands (Fig. 3). The small study area (marked with shading in Fig. 3) was chosen for the field survey. A larger study area (the entire colored region in Fig. 3) was later considered in order to increase the number of bare patches connected to channels wider than 1 m. Bare patches that are connected with narrow channels (< 1 m) and that are located outside of the small study area (blue polygons in Fig. 3) were not considered in the analysis."

Figure 4 has a lot of redundant header and axis text information and the real information is hidden on a few square centimeters. Likewise for Figure 5, where removal of the horizontal axis texts for panels a and b makes it possible to have higher plots on the same space, so that the data are more clearly shown and comparable. This is necessary, because what happens in the tails of these skewed distribution is interesting: the connected bare patches plot above the other distributions.

Response: In Figure 4, we removed the label "relative elevation" at the upper X-axis for panel (b) and (c). Further, we removed the label "Elevation (m MSL)" at the bottom X-axis for panels (a) and (b). As a result we could make the figures a bit larger in the vertical direction.

In Figure 5, we followed the same advice, by removing the label "distance to closest channel (m)" from the horizontal axis, and making the figures a bit larger.

Figure 6 contains novel information and shows interesting trends. However, the relative vertical axis per channel width class leads to a biasing emphasis on a very small number of cases for the largest channel widths. Perhaps another presentation would solve that problem: a matrix (pcolor in matlab) with log(patch size) on the horizontal axis, log(channel width) on the vertical, and log(number or fraction of total) as the colour scale. The channel width classes are not consistent with the possibly logarithic distribution of the number of patches against channel width and I suggest to simply use classes of a 2-base log or something here, which would also improve the horizontal axis in Figure 8c from non-equidistant class to a true width scale.

Response: We tried alternative representations of the results, in line with the suggestions made here, but it did not improve the representation. Actually, the pattern of increasing connecting channel width with increasing patch size, becomes less clear. Therefore, we chose to keep the same figure format.

Figure 8 needs to mention in the caption that this concerns the Saeftinghe site only.

Response: Indeed this was done.

Is distance to the closest channel calculated from a map of channels or from the DEM?

Response: Channels are mapped based on aerial images, which is explained in section 3.1

How is the information in panel c obtained; is that the same as in Figure 6a but then split up for the permanent and temporary bare patches?

Response: Indeed.

Why is there no data for the other areas?

Response: For the considered time periods (see sections 3.2 to 3.3) we did not observe revegation of bare patches in the other two areas (San Felice and Blackwater).

I suppose there are older images so this is open for analysis. As it stands now, there is very little data and support for the conclusions about stability and revegetation, especially since this plot is only for the system where the authors claim that revegetation is most likely. How do they know?

Response: Indeed our interpretations in the discussion, in the context of alternative stable state theory, were strongly hypothetical. Also in response to a previous similar remark by the reviewer, we left out this part of the discussion.

Figure 9a has four variables mentioned on the top arrow to the right, but is width of the connecting channel really increasing to the right, away from the widest main channel and into the bifurcating network? That is only possible if the reduction in depth goes much more rapid.

Response: Indeed, this may have been confusing. Therefore, the formulation was changed as

"connectivity of bare patches to channels". This is indeed small close to the main channel in the drawing, and increases with increasing distance from the main channel.

Is erosion the right term here?

Response: we rephrased it as "resuspension", as contrasting with the "sedimentation".

How is it possible that sediment disappears in such a strongly converging flow (meaning very low velocity in the patches landward of the first bit of well-defined channel)? Are waves important here, as high up on the marsh in a very shallow, vegetated and micro-fetch area? Waves are known to be important in this sort of system, but that is on saltmarsh edges where there is fetch and depth to generate waves. It is not simply saltmarsh collapse and disappearance of organic material that causes the bare patches?

Response: previous studies have proposed that waves play a significant role in resuspension (or erosion) of pond bottom material in marsh ponds, and in lateral expansion of ponds by wave-induced erosion of the pond-marsh boundaries (Mariotti, 2016; Ortiz et al. 2017). But these studies identified a critical interior marsh pond size of 200–1000 m for wave-induced erosion of vegetated pond edges (i.e. effect of fetch length). However, many of the bare patches considered in our study, are smaller, and hence wave-induced erosion is expected to be very small. Tidal currents are then the expected dominant control over hydrodynamics, eventually responsible for resuspension and export of sediments from bare patches.

In Figure 9b, the horizontal axis provides two complex variables: sediment supply and soil drainage, but how do you know that it concerns these two and not the many others mentioned in lines 77-80? These are entirely inferred here but not measured. Any concentration from literature such as in line 125 is meaningless because of the very large spatial variation and the sediment settling in the marsh so far from the channels. So the position of the blue and green curves in the graphic is really unknown and we cannot know whether there are really two disconnected lines or simply a single continuum. And that means that the connection to the bistable state diagram is entirely speculative. I know it is attractive to try and see the landscape through the filter of the concept from complexity theory (citations here go back to Scheffer but the idea is already reviewed in Thorn and Welford 1994 https://www.jstor.org/stable/2564149), but this connection needs to be supported by the data. At present, it is not, and removal of this panel and section 6.4 of the discussion would in my opinion increase the quality of the paper.

Response: We followed this advice, and removed panel (b) of Figure 9, and removed section 6.4 from the paper.

The lidar images in the supplement are barely useful as presented here. The gray scale and small image size, and the lack of colour scale bar makes it very hard to see anything at all here.

Response: We replace them by aerial images of the larger surrounding landscape.

2.3 Suggestions for the text

The present objective (line 108) is now to determine the topographic conditions determining the presence of bare patches, but the idea also seems to determine whether they can revegetate, so I suggest ‘presence and dynamics’.

Response: we followed this suggestion.

The authors define two kinds of bare patches, but surely this is a continuum and there is a certain image resolution. They need to indicate what size of connecting channel is the cutoff for an isolated or connected patch earlier than in line 397 in the discussion.

Response: Indeed, we also mentioned this threshold channel width in the methods section 3.1 in line 186-189.

The size of bare patches is important for the discussion (line 427) but size is not plotted in Fig. 6, only number of pixels and that could also indicate many small patches. A plot of patch size, and possibly analyses with patch size as a variable, are needed to make this argument.

Response: Patch size is indeed plotted on the X-axis of Figure 6.