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

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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.

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.

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.

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.

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?)

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)?

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?

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.

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.

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

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.

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.

Figure 8 needs to mention in the caption that this concerns the Saeftinghe site only. Is distance to the closest channel calculated from a map of channels or from the DEM? 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? Why is there no data for the other areas? 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?

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. Is erosion the right term here? 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?

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.

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.

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*’.

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.

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.

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