We thank the reviewer for their helpful suggestions. Below we describe how we will adjust the manuscript in the revised version in response to these comments.
We have formatted reviewer comments in italics.
Our responses are in normal font.

1 Specific Comments

Comment 1: I worry that the authors may underestimate the level of detail needed to accurately resolve the decimeter scale topography of the marsh platform in the requisite DEM. The authors rely heavily on widely available Lidar DEMS for the TIP method despite the fact that the overall relief of the marsh platform is often missed completely by Lidar sensors. Perhaps it doesnt really matter here since the authors are establishing marsh platform identification on the scarp perimeters...but, I wonder if there are any ways you might improve on your method to extend its usefulness to other marsh landscapes (those without scarps, and those characteristic of patchy, discontinuous areas of marsh platform that might be heavily dissected by intertidal creek networks.

The reviewer is entirely correct to point out that some scarp heights may be lower than the vertical accuracy of the lidar data. We will be more explicit about this limitation in the revision. This resolution and the relief plays a role in selection of the minum scarp height: please see our response to reviewer 2. We will include more sites in the appendix that push the limits of the method (e.g. in very low relief landscapes). We have used the method on the Wax Lake Delta in Louisiana (Figure 1), and the method can detect the marsh despite the fact that the maximum relief of the point cloud is 80 cm (including the returns from vegetation).

We do want to point out (to echo the comment above) that our test sites have used widely available lidar DEMs: in our test cases the method works well, and the scarps/platforms are correctly delineated by the algorithm. As suggested by the reviewer, the precise topography of the platform is not necessary for the TIP method to function, as our method is focused on detecting scarp platforms and filling at areas of higher elevation, rather than relying on the elevations of the platform itself. This has the effect of making the TIP method less sensitive to unequal removal of vegetation between different DEM sources. In response to comments below we will include a few more sites with smaller tidal ranges in the appendix, and in addition we will add more cautionary language about the use of the method, but we would also like to make clear that the method can work on even microtidal marshes as long as there are scarps (more on that point later). We show two examples in Figure 1 and Figure 2.

We do not aim to include patch detection or tidal creek detection in the TIP method for this manuscript. This requires the implementation of different algorithms as they have distinct morphological characteristics, and we feel this is a different topic. We agree with the reviewer that such an algorithm would be very beneficial, but we feel this is beyond the scope of the current manuscript. We have, however, tested the method on large patches (which have the same morphology as small marsh islands) and will include these results in the appendix.

Comment 2:The method presented here is only useful in marsh landscapes characteristic of steep scarps (as in erosional environments). I think that the title should reflect that in some way.

The term 'platform' in the title is meant to reflect the necessity of the presence of a scarp in our method. To address this, we propose to better define platform in the introduction rather than change the title. We would include the following definition: 'We here define salt marsh platforms as sub-horizontal surfaces in the coastal landscape, separated from surrounding intertidal flats by steep scarp features.'

We do feel this comment suggests the method is somehow a niche method only applicable to limited settings. The authors have personally conducted field campaigns across marshes in northern France (in a macrotidal environment), in northern Italy (in microtidal environments), across the UK (along a range of sea level rise rates and tidal ranges), along the Atlantic coast of the United States (South Carolina and North Carolina) and in the Gulf Coast of Florida. We have also recreationally visited marshes in Louisiana, California and Oregon. In all cases these marshes had platforms and scarps, despite the wide variety in vegetation species, tidal range, sea level rise rates, suspended sediment concentrations, temperatures, and wave climates. We acknowledge not all marshes have scarps and therefore not all marshes are amenable to the TIP approach, but we do wish to emphasise that this method should be broadly applicable over a wide range of geographic areas. In the appendix we will add some sites outside of the UK to demonstrate the method is not limited to the 6 specific site we chose for intensive method verification (see Figs 1 and 2). However we will also highlight, as suggested by the reviewer, places where the algorithm is unlikely to work to ensure that readers are aware of any pitfalls.

Comment 3: Perhaps you could include more descriptive information on the geomorphology of your study sites. For instance, you have high resolution DEMs for all, why not calculate drainage density, or some other metric to describe how heavily dissected the marsh platform is? Then, your results could vary as a function of drainage density and tidal range? Maybe. . .

We agree with the reviewer that it could be useful to look at the performance of the method in platforms with different degrees of dissection. Having published previously on drainage density (Clubb et al., 2016, JGR-ES, doi:10.1002/2015JF003747), we are slightly wary of using this specific metric. Drainage density is defined as the length of the channels in a basin divided by the basin area, but basin area in a marsh context is extremely difficult to quantify. Furthermore, many tidal channels in marsh environments are wide compared to the size of marsh features. Therefore, a metric of drainage density which just quantifies the length of channels may not be appropriate for constraining degree of dissection of a marsh platform. Creek extraction is also a major challenge (we have tried many methods). At this point we do not want to commit to plotting results as a function of drainage density because we are not convinced this is possible, but we will attempt to digitise the networks from imagery (our attempts to automatically extract the metrics have failed–marshes defy our channel extraction tools that are tuned to fluvial networks). However we think it is an excellent idea to try to quantify results as a function of geomorphic metrics so we can plot results as a function of tidal range and also we may, failing a satisfying extraction of drainage density, attempt a dissection proxy based on the statistics of elevations in the study sites.

Comment 4: The paper could use some organizational finesse to improve the flow of the narrative. There are many instances where results are stated in the Methods section, and there is no Discussion section, but discussion elements are mixed in with Results. I would also consider adding a separate section for Validation following or within the Methods section to describe how you evaluated the performance of the TIP method. It seems very out of place in its current position (Results and Discussion). See specific comments below

In order to make our manuscript clearer, we plan to change the structure to the following:

1. Introduction
2. Methodology
    (a) Test sites

(b) Preprocessing Topographic Data
   (c) Scarp routing
   (d) Platform identification

3. Results
   (a) Parameter optimisation
   (b) Performance analysis

4. Discussion
   (a) Limits of the TIP method
   (b) Future developments
   (c) Potential for monitoring

5. Conclusions

Comment 5: The Results section is a bit messy. Perhaps consider organizing into a more logical manner. For instance, I like the idea of presenting results as a function of tidal range (or drainage density see comment 3). . .start with S1, describe, then go on to S2. . .and so on. Then, in a separate section (see comment 4) you could demonstrate the effects of using the filter on TIP results. I think this approach would be fine, because you already told us that you dont want to use the filter. . .and thats OK.

We propose to reorganise the results section to follow the order of the figures. The discussion would then analyse these results in a comprehensive manner, as set out in our response to comment 4. However, we think that ordering the results on a site-by-site basis would lead to a much longer results section, as well as making it more difficult to link results from different sites to better illustrate our discussion and demonstrate the overall performance of the TIP method.

2 Technical comments

P1L3: The productivity and even survival of salt marsh. . .(remove even)
We will change this.

P1L3: . . .of salt marsh vegetation. . .(why vegetation here? Why not landscape? Seems out of place.)
We will change this to change to 'the sustained existance of the salt marsh ecosystem'

P1L5, P1L7, P1L15
We will make all the suggested changes.

P1L20: . . ., it also suggests. . . (what is it?)
We will change to 'we suggest'

P2L9: awkward, consider revising. . .perhaps something like, . . .makes monitoring the evolution of salt marshes imperative for management strategies and scientific endeavors. . ..
We will follow the reviewer's suggestion here.

P2L34: Right. . .but marsh platform slopes are on the order of 30cm total relief. . .and the overall structure is often misrepresented by lidar sensors with a nominal accuracy of +/- 15cm.

The vertical accuracy (z-accuracy) of airborne lidar and photogrammetry may indeed be close to the size of the smallest scarps, which may be 30 cm or less in height for micro-tidal areas, immature platforms or marshes situated high in the tidal frame. If we consider an unvegetated surface (or a 'cleaned' DSM), we argue that the nominal accuracy is a combined product of georeferencing and distance-measurement accuracy of the lidar itself, the georeferencing generally accounting for the main part of the vertical error. The TIP method is focused on relative elevations in local neighbourhoods, and is therefore not very sensitive to the z-accuracy. If we consider a vegetated surface however, DEM processing (such as ground-return filtering and rasterisation) may indeed lead to higher and more locally disparate z-accuracy values on the platform than on the tidal flat. We argue that the presence of vegetation induces positive errors, which plays in favour of the TIP method, as this artificially increases the platform height and therefore the scarp slope. We will make all these points in the revised manuscript, and include examples of very low relief marshes (in our response to Reviewer 2 we include an image from the low relief Wax Lake Delta in Louisiana).

P3L17: . . .horizontal resolutions. . .(do you mean horizontal extents? These are two very different things.)

By horizontal resolution we mean the grid cell size of a rasterised DEM. We will amend the text accordingly.

P3L18: remove dash after short.
We will make this change.

P3L27: can you provide any technical specs for the lidar survey? Seasonality? Tides? Etc. . .

We will include a link to the metadata for Environment Agency lidar. The exact flight times relative to tides is unknown, however lidar surveys by the EA are conducted around low tide.

P4L4: stronger than what?
'Stronger' refers to the previous site; we will make this clear in the revision.

P4L6: . . .provides. . .(change to provide)
We will make this change.

P4L8: what do you mean by numerous? How many more channels are at this site compared to the others? Consider using more physical descriptors throughout your study site description. What are the respective areas?

We will address this issue by modifying Figure 9 (see response to Specific Comment 3)

P4L10: What do you mean by levels? Elevation? Water level?
We will replace this with 'elevations'.

P4L19: Why three times the horizontal resolution of the DEM? Why not 5 or 6?
3 is the smallest window available in the referenced method for slope determination. We will make this clear in the revision.

P4L29-32: At what scale is this problematic? 100s of kilometers? 10s of kilometers? I thought we were focused on relatively small areas of marsh landscape. . .what are the relative sizes of each study site (see also comment P4L8 above).

The problems considered here are indeed small section of marshes. However, the local definition of kernels is unaffected by the DEM extent. The calculation time would however increase for larger marshes. At the time of writing, we have not tested the method on marshes larger than 12 km^2, for which the method did not encounter difficulties, despite the longer run times. We shall include these results in the appendix/supplementary material.

P5L6-20: Can you briefly describe what each of these means physically and the importance of information provided by each?

Although the non-dimensional values of elevation and slope indeed have physical meaning, we do not wish to detail extensively as this would require investigation into formative processes for each site and considerably lengthen the manuscript. The aim of the manuscript is not to explore the history of the six test sites but rather demonstrate the general applicability of the method, and these metrics are solely a means for us to apply the method over different landscapes.

P5L23 (and throughout): be careful to avoid stating results in Methods section. - accepted

P5L24: what is pdf? define.
"pdf" is here defined as a probability distribution function: we will make this clear in the text.

P6L20: large number of true scarps? Or do you mean large number of misidentified scarps that are actually creek banks?

This procedure produces a large number of scarps that could be creek banks and local DEM irregularities.

P7L7: why 11?

11 is a value we chose to obtain a significantly wider kernel. Other values have not been tested.

P7L16 (and throughout), P7L27 (and throughout), P8L2
We will make these changes.

P8L11 (throughout): Methods presented in results section. Consider providing a separate subsection in Methods for validation and then share results in the proper Results section.
We will do this: see response to Specific Comment 4.

P8L12-15: Im guessing TP, FN, etc. . . are obtained from subtracting? Maybe show that in Methods.

TP are obtained by counting the number of boolean True values for the detected marsh and the digitised marsh. Same for FP, etc. We will amend the text to be clearer about this issue.

P8L26: . . .the manual digitization. . .did you even discuss that in your Methods section? What software was used? Scale?

We will add the details of the digitisation, conducted in QGIS at a scale of 1/500.

P9L14: describe one figure at a time, and in chronological order.
See response to Specific Comment 4.

P9L19-29: discussion in Results section. Consider revising.
See response to Specific Comment 4.

P10L6-7: Isnt this simply a transition zone between marsh platform and tidal flat?

Although these zones correspond topographically to transition zones, they might not be vegetated (which was observed on aerial imagery for this site), and potentially unstable. We have therefore not called them 'pioneer zones' to avoid confusion with vegetated transition zones.

P10L8: . . .saltings. . .why are you defining this here? You referred to salting earlier with no definition. Define earlier.
We will replace saltings by 'fallen blocs' to avoid confusion, and define saltings where it appears earlier.

P10L17: . . .yes, but its limited to erosional landscapes with obvious scarps.
See response to comment 1. The reviewer quite correctly notes that this is limited to marsh landscapes with erosional scarps. This fits with our definition of 'salt marsh platform'. However this morphology is extremely common in salt marshes across a wide range of environments (many Atlantic and Gulf coast marshes in North America (Figs 1 and 2), many UK marshes (and all we have examined), marshes along the north coast of France, marshes in Italy, and these are just the examples that the authors have personally visited). So we believe the method is applicable to a significant fraction of global salt marshes.

P11L9: . . .algae. . .why is this here?? Did you test for this or are you speculating? Maybe you could instead say that your method works independent of such environmental factors. . .its implied, but not exactly tested for in this paper.

We see that this was inelegantly introduced into the paper. It is here for a reason, however: initially when we started identifying marshes it was suggested to us that we simply use optical techniques. However in our imagery, and in particular at Shell Bay, algae and biofilms are widespread making much of the landscape green. We wanted to make the point that even if the landscape is green from algae, or if there are widespread biofilms, we could still detect a marsh platform. Of course, large accumulations of macro-algae (kelp, etc.) might trick the method. We shall include this observation.

Figure 3, 5, 7, 9, 11
We will make all these suggested changes.

Figure 8: maybe a table would be a nice complement to this figure?
Agreed. We will add this to the appendix.

Figure 10: scale?
Scale is included in text to avoid clutter on these already crowded maps.

3 Additional sites

In response to the comments of both Reviewer 1 and Reviewer 2, we applied the TIP method to two well-known salt marsh environments in the USA. The site of Wax Lake Delta (Figure 1) was chosen as an example of an accreting marsh with very low relief. We note, after the suggestion of reviewer 2, that the 20 cm buffer used when filling the platform does not yield satisfying results on a very low relief landscape. Reducing this value to 5cm yields better coverage of the marsh, shown in (Figure 1). This suggests that the TIP method would benefit from linking relief of the landscape to this buffer value. The site of Plum Island (Figure 2) was chosen as an example of a marsh impacted by human activity, as reflected by the linear ditches. It also displays a strongly pooled marsh surface on the south-eastern corner, which the TIP method partially classifies as marsh surface.

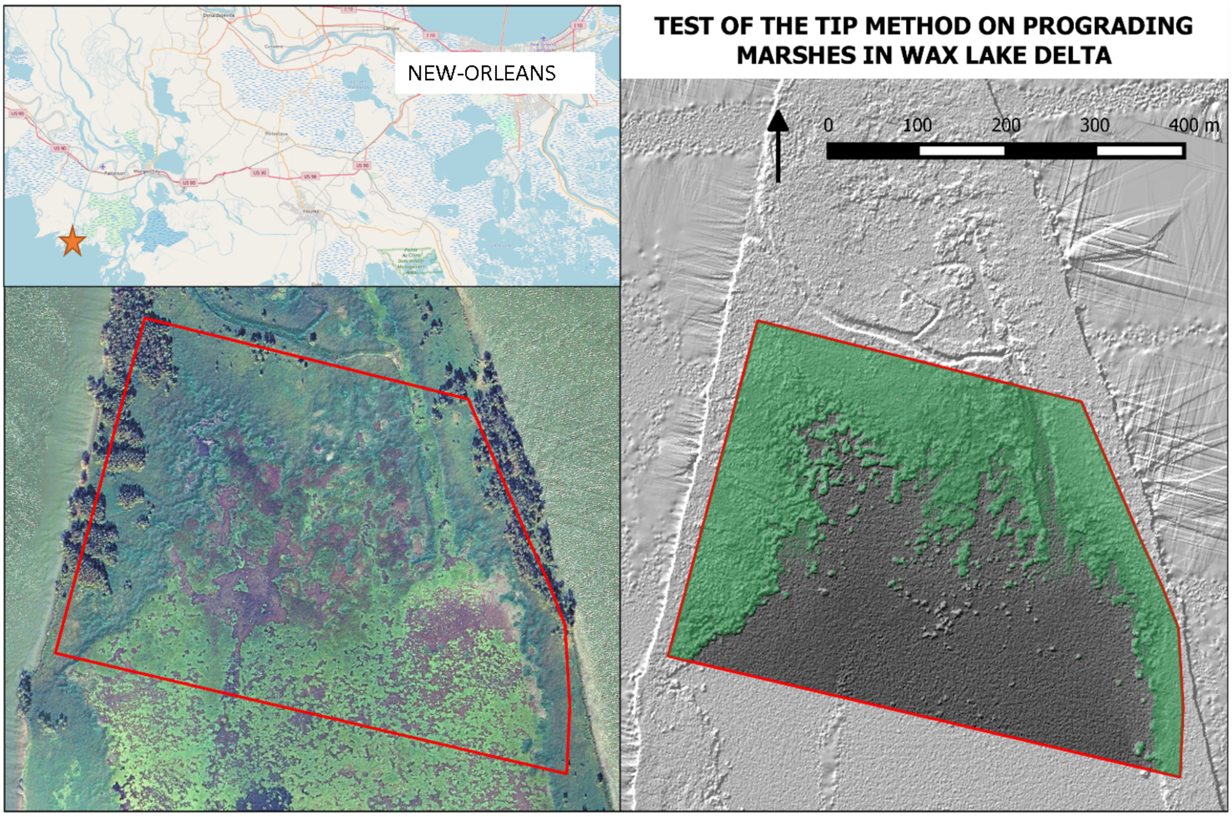

Figure 1: *Hillshade and RGB orthophotography of a portion of Wax Lake Delta marsh, LA (left). Bright green patches are likely to be pondweed or lentils, and are not considered part of the marsh emergent platform. The marsh detected by the TIP method is coloured in green (right) over a DEM layer (lighter colours indicate higher elevation). (Parameters: Sp_{thresh}=-2.0, ZK_{thresh}=0.85, rz_{thresh}=8)*

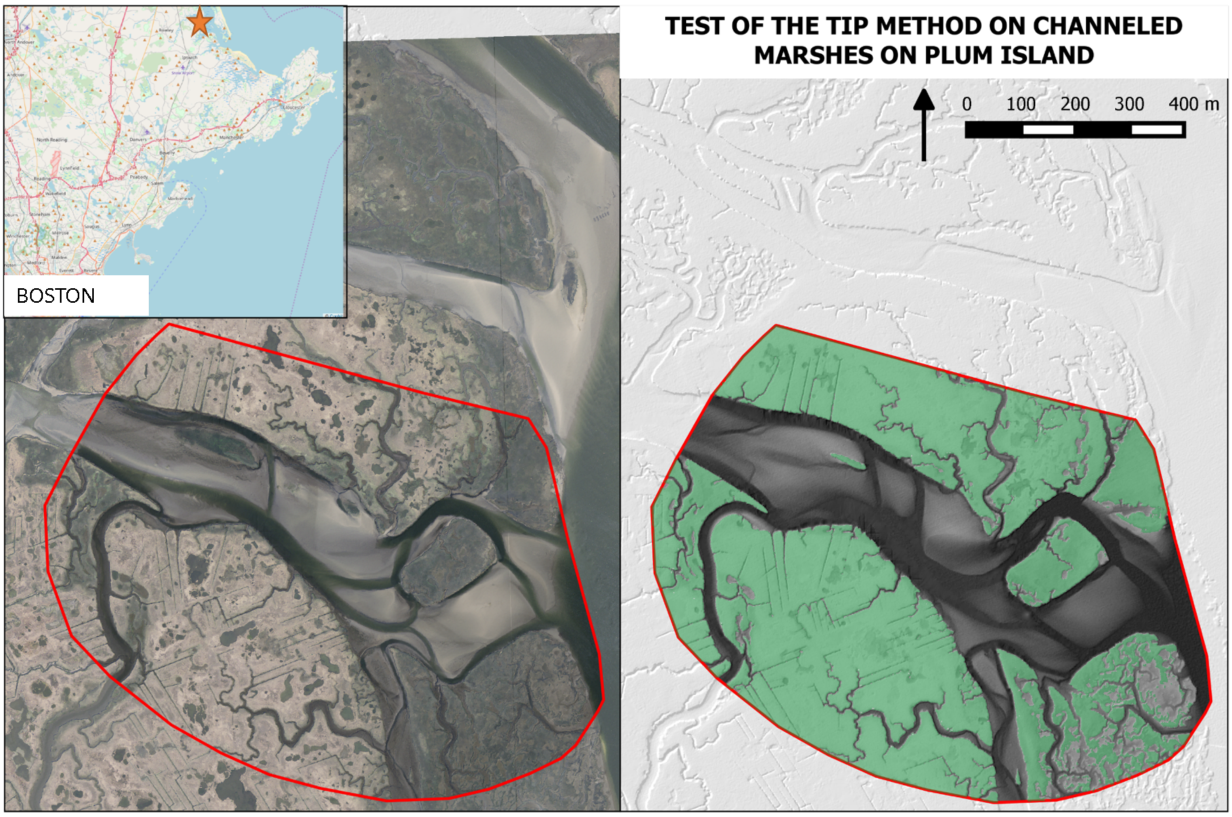

Figure 2: *Hillshade and RGB orthophotography of a portion of Plum Island marsh, MA (left). The marsh detected by the TIP method is coloured in green (right) over a DEM layer (lighter colours indicate higher elevation). (Parameters: Sp_{thresh}=-2.0, ZK_{thresh}=0.55, rz_{thresh}=4)*