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# *Interactive comment on* "Processing and accuracy of topobathymetric LiDAR data in land-water transition zones" *by* M. S. Andersen et al.

**Anonymous Referee #1**

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General comments:

This manuscript describes a workflow to process a raw bathymetric LiDAR dataset to generate a digital terrain model of a tidal inlet system in the Danish Wadden Sea. The workflow allows extracting both the surface of the water and the seabed from the point cloud. As it stands, this paper reads more like a technical report. My understanding is that the method used to extract the water surface is novel, but I found difficult to grasp the novelty while reading the paper. Despite having valid scientific approach and applied methods, the work is not much grounded in the literature. Many key papers are not mentioned and the published works in water surface detection are not discussed, which prevents effectively identifying how this paper fills a gap in the knowledge. I also believe that the paper lacks context: it is unclear why the dataset was collected. This can have important implications for its processing. For instance, the workflow could not



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Discussion paper

be used to process a dataset collected to identify hazards to coastal navigation; hydrographers standards and protocols always identify the shallowest point of the seabed and use the worst case scenario in terms of low water level, while the workflow introduced in this paper uses the deepest point of the seabed and the maximum point of the water level. The discussion is quite short and could be more complete if wider scientific implications would be discussed. Also, I understand that the study of geomorphometry includes issues of data collection, but I find that this manuscript lacks some examples of morphological quantitative measurements that would make it more suitable for publication in a special issue on geomorphometry. Finally, I think that this paper has potentially many candidates for "lessons learned" that could help plan future surveys, but these take-home messages are not made explicit in the manuscript.

Here is my evaluation of the paper based on HESS review guidelines:

1. Does the paper address relevant scientific questions within the scope of HESS?

This paper does address relevant questions within the scope of HESS. However, the questions addressed are more technical than scientific. I think that the paper would benefit from being put in context within an application that would show how the proposed workflow can actually help answer scientific questions relevant to hydrology and earth systems.

2. Does the paper present novel concepts, ideas, tools, or data?

It is difficult to grasp the novelty in this paper. The essence of the workflow that is presented (i.e. raw data, correction, automatic and manual filtering, detection) is very similar to what is implemented in many bathymetric or LiDAR data processing software (e.g. Caris, Fledermaus, etc.). On page 4 (lines 6-8), the authors state that "The overall processing steps are known, but there is no standard or universal approach for dealing with the individual steps. In particular, there is no definitive method for detecting a water surface from topobathymetric LiDAR data." Methods however exist to detect a water surface from LiDAR data. For instance, water surface detection is

commonly performed by combining a laser in the green wavelengths with a laser in the near-infrared wavelengths, or if limited to only one laser, by identifying the different returns in the waveform. The novelty would be clearer and the paper would much benefit from a discussion on the state-of-the-art methods of water surface detection (i.e. a more complete literature review), and a clear argument on how the proposed method is different and/or better than the existing ones.

3. Are substantial conclusions reached?

The authors demonstrated the ability to use bathymetric LiDAR to detect fine-scale features in the coastal environment, which in itself has been demonstrated before in other studies. As mentioned in the previous points, the conclusions could be more substantial if the dataset was put in a more applied context (e.g. within a geomorphometric application to study morphodynamics in the tidal inlet system), and if it was made clearer how the proposed method improves water surface detection compared to existing methods. Why should we adopt this method over another one? The authors need to convince us that their method is better in some ways (e.g. cost-efficiency, accuracy, extraction of relevant information, potential applications?).

4. Are the scientific methods and assumptions valid and clearly outlined?

Except for a few minor issues (see specific comments below), the scientific methods are valid and clearly outlined. I particularly appreciate that the authors detailed the assumptions that were made and discussed their implications.

5. Are the results sufficient to support the interpretations and conclusions?

Since it is a very technical paper, it does not have much interpretation. However, the results support the conclusions.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

The authors did well in describing the processing and calculations which would allow

replicability.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

As previously mentioned, the literature review is very general, mainly focused on the use of LiDAR. It does not go very deep in the methods that were tested or are commonly used to process the data, which prevents the clear identification of the authors' own new/original contribution.

8. Does the title clearly reflect the contents of the paper?

The title could be more representative. I suggest adding a word referring to "workflow" or "protocol". Also, the authors need to make sure that the system used was really "topobathymetric" (see specific comment below). Finally, I wonder why the authors mention accuracy in the title but not precision, which they measure. I suggest using a more general term such as "quality".

9. Does the abstract provide a concise and complete summary? The abstract is concise but not complete. There is no mention of the goal of the paper, no mention of water surface detection, and no mention of the general steps of the methods. It goes directly from the general mention of what was done ("a method is developed to. . .") to the results.

10. Is the overall presentation well structured and clear?

Overall, this paper is clear, concise and well-structured. I really enjoyed the quality of figures and tables.

11. Is the language fluent and precise?

The use of English language would need a bit of work, but nothing of big concern.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes, everything was well defined. However, some of the equations presented in the introduction may not be necessary as they were not used directly by the authors in the methods. On the other hand, I understand that they may help readers with limited knowledge in LiDAR to understand the related concepts.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

I think that more background should be given on the scientific literature that looked at extracting the water surface from LiDAR. Also, the discussion could be improved by discussing the implications of the results for the wider scientific community. For instance, why not using multi-spectral LiDAR systems, or wavelengths that penetrate deeper? The system could not survey deeper than 3 m, a depth that is usually not covered by multibeam echosounders. Based on that and the fact that a gap in data would appear if merging with other datasets, can the authors really claim that their data provide a seamless coverage of the land-water transition zone?

14. Are the number and quality of references appropriate?

While reading the paper for the first time, I had the impression that many claims would benefit from a reference. I counted only 25 references in the list, which includes two technical reports, one thesis, one magazine, three user manuals, one textbook, and two abstracts from conference proceedings. Two of the remaining references are used to characterise the study area (i.e. tidal prism, average depth and width of the channels). I believe that the authors need more than 13 peer-reviewed full-length articles to set their work into the relevant literature and context.

Based on these 14 points, I do not recommend the manuscript for publication in HESS in its current condition. However, I laid down some specific comments below in the hope of helping the authors to improve their paper.

Specific comments:

-Could the authors define “topobathymetric” LiDAR? I know that some manufacturers advertise their systems as “topobathymetric”, but it was my understanding (and I may be wrong) that LiDAR systems operating within near-infrared wavelengths are “topographic”, that those operating within the green wavelengths are “bathymetric”, and that the multi-spectral systems that combine both near-infrared and green wavelengths are “topobathymetric”. According to these definitions, the LiDAR used in this study would be a bathymetric LiDAR, although used both above and under the water surface. It would help if the authors specify/clarify why their system is considered topobathymetric while it is only surveying in the green wavelengths. This could be expanded on p. 2, after lines 22-24.

-The technical word for a digital model that considers both elevation and depth would be Digital Terrain Model. I suggest that you use DTM rather than DEM. I also recently read that some authors prefer using Coastal Terrain Model (CTM) for seamless models. It is sometimes ambiguous when the authors write “elevation”, especially when underwater and talking about “depth”. I suggest revisiting the use of these terms and using a neutral term such as “altitude” when needed, for instance on page 9, line 4.

-Page 3, lines 18-20: This is not necessarily true as some systems adjust the scanning angle to allow for a constant laser beam footprint. This was done for instance in Costa et al. (2009, vol. 113, Remote Sensing of Environment). It could be useful to specify.

-Page 5, lines 11-13 should come after line 4 (within that bullet point) as it describes study site 1.

-Are study sites 2 and 3 really study sites, or validation sites? I feel like the paper focusses on study site 1 rather than the study sites 2 and 3, which are only used to measure accuracy and precision. I would recommend changing the term “study site” for “validation site”.

-Study site 3 is a steel frame located in a nearby river just below the water surface and is only used for precision assessment underwater. I wonder how representative of

the surveyed environment this study area really is. It seems to me that the river will present different environmental characteristics (e.g. turbidity, wave action, currents) than the main submerged area surveyed (i.e. flood channel). Also, would the precision measurement be different if the frame had been located deeper since light penetration is influenced by depth? These elements should be discussed at some point.

-Page 5, lines 23-26: What about the frame? It seems that if the frame is just below the water, a GPS could still be used to measure ground control points, as long as the pole on which the receiver is mounted is long enough to go on the frame without submerging the receiver.

-There was more than one month between the survey that assessed the accuracy and precision and the actual survey. Are there any potential implications? Was the surveying system used for different purposes in between these surveys? Were the environmental conditions similar enough?

-Page 6, lines 3-4: Could you provide a reference for this? Many surveyors would consider waves of half a meter high as not ideal conditions for surveying. Waves are known to influence the angle of penetration into the water, which then influences the way light refracts in the water column.

-Page 6, lines14-15: Table 1 summarizes the specifications of the LiDAR system, but are these corresponding to the survey characteristics? In other words, please specify if you actually surveyed at 400 m high that corresponded in a 400 m swath width, etc.

-Page 6, section 3.2: Please describe how these steps are different from the processing steps of any other surveys?

-Page 8, lines 14-15: The authors wrote that “based on visual inspection of the outcomes, it was impossible to reach a setting which would be optimal for all the different environments.” However, at lines 22-23, they say that “based on the visual inspection of the filtering sensitivity analysis, the chosen settings for the automatic filtering were:. . .”

If it was first considered impossible, how were the settings actually chosen? What are the implications of selecting settings that are not optimal for all environments? Does it only influence the level of manual editing needed after?

-Page 9, lines 16-17: It is indicated here that the maximum wave heights were 20-30 cm while it was mentioned earlier that they were 50 cm. Please clarify.

-Page 9, lines 17-18: I think it would be preferable to provide a reference that indicates that it was actually acceptable to assume a flat water surface, or elaborate on why it is acceptable.

-What are the implications of selecting the higher value of the point cloud for the water surface and the lowest for the seabed? As mentioned previously, this goes against what would be considered by hydrographers as the basis for bathymetric data processing that serves their purpose. I am sure that the proposed method is suitable for other applications, but these would need to be discussed.

-It seems to me that the refraction correction could/should be done before the automatic and manual filtering. In multibeam data processing for instance, the sound velocity correction is performed before the cleaning of the soundings. Since the refraction correction is influencing the real 3D positioning of the LiDAR points, it would seem appropriate to start with the correction and then filter the points; some points that were outliers before the correction may simply have been more refracted and come closer to the rest of the points once corrected. Please justify the course of action and/or provide the appropriate references that justify your choice.

-Page 12, lines 18-21: Do you have any references to support the use of these planes as measures of accuracy and precision?

-Page 14, section 4.2: I am quite surprised that the precision was lower on land than underwater. Could you elaborate on that in the discussion? Also, I found unclear how the precision was actually measured for the frame, as the text focusses on the cement

block.

-Section 5.1: Considering the importance of the dead zone and as demonstrated in Figure 13, wouldn't have it been more effective to either survey at high tides, or to repeat the survey both at low and high tides in order to cover in one of the two passes what could not be covered by the other pass? I understand that my concerns are based on results (i.e. dead zone), but I think that you could develop in the discussion the implications of the dead zone, and possibly make recommendations for future surveys attempting to perform similar seamless coverage (c.f. the "take-home messages" that I mentioned in my general comment). Also in this section, I think that it would be interesting to discuss the implications of missing these "ponds". Depending on the potential applications of this dataset, the lack of information on these ponds can be meaningful. For instance, these ponds are likely important areas in the broader coastal ecosystem as they likely provide shelter or food to many species during low tides.

-Section 5.2: Most of the survey is actually above the water (topography rather than bathymetry). I believe that this may have an impact on the overall quality measurement of the dataset. Usually the accuracy is better when surveying land than the seafloor, and the accuracy is better in shallower waters than deeper waters. It would be interesting to discuss how the overall quality measurement may have been impacted by the amount of surveyed area that was above and below the water.

-Page 16, line 22: This is the first mention of the 4.1 cm. I suggest that the authors mention it before and following this sentence refer to the table from which it was taken from.

-Pages 16-17, lines 29-9: Many more studies compared the pros and cons of bathymetric LiDAR and multibeam echosounder data, including their accuracies. The conclusions are often that there are inconsistencies between the depth values collected from each system, and that multibeam echosounders are usually more accurate. However, I do not understand why the authors discuss these differences here considering

the study has nothing to do with multibeam sonars and that the argument that seems to be made is that multibeam is more accurate... That discussion does not support the results, on the contrary.

-Page 18, lines 9-15: I remember reading that slope does indeed reduce accuracy of LiDAR-retrieved bathymetry. The authors however need to support this affirmation with a reference. Also, is it possible to actually measure slope across the DTM and spatially compare slope and accuracy measurements? This would also relate the paper more to geomorphometry.

-Page 18, lines 21-23: How was that assessed? This affirmation (i.e. that LiDAR measurements are less precise in the channels) is not explained. How was it demonstrated? This also seems inconsistent with the fact that the frame (underwater) had a higher precision than the block (on land). How is that explained?

-Page 19, lines 3-5: "While bridging between spatial scales...". I do not understand this. It is the first mention of scale in the paper, and I do not see how this is a multiple scale analysis. Also, high accuracy does not necessarily result in high level of detail. A broad-scale dataset with low level of detail can have a very high accuracy, while a fine-scale dataset with a lot of details can have a very low accuracy.

-Pages 19-20, lines 30-2: I think that the differences and spatial variations of data quality throughout the study area are largely influenced by the differences in the environments (i.e. on land, on the tidal flat, in the channels and in the river) rather than simply the overlap between the swaths (although I agree that it is likely a factor).

-Page 20, line 10: Landscape needs to be defined as it can mean very different things for different persons, depending if they are remote sensing experts, landscape ecologists or urban planners.

-It could be relevant to calculate and add the kd factor of the area, measured with the attenuation coefficient and the maximum water depth. That could help explain the 3 m

maximum water depth that was achieved.

-What are the implications of the dead zone for particular applications? What are the implications of surveying only a very small proportion of the main channel, at a maximum depth of 3-4 m, when its average depth is of 15 m? This comes back to setting the paper into context. To which kind of applications would this particular dataset be useful?

Technical corrections:

-Page 5, line 19: “study site 2 and 3 was covered” should be “study sites 2 and 3 were covered”.

-Page 8, in “automatic filtering”: missing a reference for Fig. 4c.

-Page 19, line 2 and elsewhere: I suggest using “fine” and “broad” or “coarse” to characterise scale, as opposed to “small” and “large”. Small and large scales are ambiguous as they have different meaning for different fields and professions.

-Page 21, line 29-30: This reference should be Klemas (2013) and not Klemas (2012).

-Add a space after the “;” in references listing within the text.

Potentially useful literature:

-Fernandez-Diaz, J.C., Glennie, C.L., Carter, W.E., Shrestha, R.L., Sartori, M.P., Singhania, A., Legleiter, C.J., Overstreet, B.T., 2013. Early results of simultaneous terrain and shallow water bathymetry mapping using a single wavelength airborne lidar sensor. IEEE J. Sel. Top. Appl. Earth Observations Remote Sens. 7 (2), 623–635.

- Allouis, T., Bailly, J.S., Pastol, Y., Le Roux, C., 2010. Comparison of lidar waveform processing methods for very shallow water bathymetry using Raman, nearinfrared and green signals. Earth Surf. Proc. Land. 35 (6), 640–650.

-Klemas, V., 2011. Remote sensing for studying coastal ecosystems: an overview.

Journal of Coastal Research, 27, 2-17.

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