# Author response to the review of the manuscript “Sea ice melt pond bathymetry reconstructed from aerial photographs using photogrammetry…” by Dmitry Divine

Black: Comments from the reviewer
Red: Responses from the authors

We were thrilled to receive such a positive assessment from the reviewer, who has proven experience in photogrammetric sea ice studies. The questions demonstrate a thorough understanding of the applied methods. We are pleased to be able to provide answers and have carefully considered all comments in the revised version, including minor adjustments that include the answers to technical questions to make them accessible to future readers of the study.

Line 141: Should it be just 2x2 km^2 ?

Many thanks for making us aware of this error. We changed it to 2 km x 2 km (here and later)

Line 146: “Photogrammetrically reconstructed DEMs from 30 June 2020 and 22 July 2020 were leveled to zero water level using a flat plane fitted trough all lateral snow/ice–open water boundaries positions in the DEM within the cropped…
”I wonder if these Z-control points were selected manually or identified automatically from “melt ponds” objects that fell into the edge?

We added: “These reference points were automatically extracted from the raster data DEM at the positions of touching surface class vector polygons.” to make this clearer and adjusted the paragraph to embed the modification.

I have a number of technical comments to Section 3, more to satisfy my curiosity need to say.
What lense correction model the authors have used? The “standard” one available in Agisoft or you ran a target based calibration specifically on the camera and the lens used in the setup?

We tried target-based calibration; however, we found during test flights over earth-fixed ground (Emden, Germany) with constant aperture that surface reconstruction still worked better with initialized but free parameter adjustment in the Agisoft camera correction model. Apparently, temperature, small changes in focus (see below), and other points impacted the calibration.

Also, how did you disable the autofocus (just curious, since any movement of lenses changes the optical parameters of the camera system, this must be made rigid in one or another way).

We adjusted the focus before the flight in the horizontal view to a point approximately at the distance of the flight height. We then completely switched off the autofocus, both electrically and mechanically, and secured the adjustment wheel on the lens with adhesive tape.

We added, “All aerial images of a survey flight were taken with constant exposure settings and a mechanically and electrically fixed autofocus set to the flight altitude during flight preparation.” And “We use the commercial photogrammetry suite Agisoft Metashape to calibrate the camera optics and solve the complex aerial triangulation equations to calculate orthomosaics and DEM as georeferenced raster data.” to the method section.

I wonder also if the authors worked with the raw image format or compressed jpgs? One of my challenges (some 10 years ago) was linked with a computational intensity of the entire process due to dealing with tiffs in the original resolution. How computer intensive, in general, the process was in your case. Did Agisoft manage to “digest” the entire “Fortress” in one go or you had to break the scene in pieces? How many images in total where involved into a bundle when building the “Fortress” DEM?

The full pre-processing of the used MOSAiC orthomosaics is described in Neckel et al. (2023) (https://doi.org/10.1038/s41597-023-02318-5). We used full resolution images converted to JPG before processing and could reconstruct even much larger areas than the Fortress in one go (from >2000 images). The efficiency of photogrammetry suites has massively increased over the last few years.

I also noticed that the authors did not apply any ice drift correction to camera positions prior to triangulation. From my experience the drift of sea ice causes the emergence of scene-scale gradients in the reconstructed DEM, but I assume, “forcing” the edges of the derived Fortress DEM through the plane could have helped to resolve the problem.

This is a very good point and a well-known problem of photogrammetric DEMs, whereby such large-scale gradients and bending can occur not only with drifting sea ice but also very distinctly over land. The so-called “Doming effect”. Its impact on our data is mentioned in section 3.5 of the manuscript. On PASCAL, we analyzed each pond separately to reduce the impact of large-scale gradients. This point was important for us to mention, as it also shows future users how to deal with it.
Since your question showed us once again the importance of the topic, we have touched on it again in the discussion 6.1.3:
“
In the photogrammetrically reconstructed topography, the DEM, large-scale gradients, also known as the doming effect, can typically occur. Since the same problem occurred with the PASCAL data, we have introduced tools that include a separate analysis of the ponds so that a derivation of the pond depth is still possible. The effect can be minimized by improving the calibration of the camera model through improved flight patterns, a slightly oblique camera perspective (Wackrow and Chandler, 2008), and, as we found when comparing the PASCAL and MOSAiC data, by not using an additional camera protection window in front of the lens.
“

Learning from PASCAL, gradients were already much smaller on MOSAiC. However, the remaining very large-scale gradients in the MOSAiC DEMs were corrected in comparison to the available airborne laser scanner (ALS) data (Neckel et al., 2023). On days with high ice drift speeds, camera positions were additionally corrected, but mainly to improve and speed up the alignment.

Line 263: Can you please discuss, how would it work with well elaborated melt ponds later in the season, when nearly vertical walls of the ponds with some 10-15 cm freeboard could emerge by melting?

Thanks for mentioning this. We expect that the automatic smoothing of the reconstructed topography minimizes the effect. We added that point to the method description:
"Then, we extracted the relative height of the pond surface $h_{surf}$ at the pond margin from the DEM, which marks the transition from ice to pond in the smoothed topography. Due to the smoothing, we expect the method to be valid also for ponds with almost vertical walls later in the season (Fetterer and Untersteiner, 1998), which were not part of the evaluation set, however." However, as we are unable to test this, we decided to include it in the discussion as a possible uncertainty: "Due to a lack of data, we were also unable to test whether vertical pond walls that occur later in the season negatively impact the pond surface extraction from the DEM. However, we assume that in this case, smoothing reduces the error."

Line 294: Did you actually run the classifier first with all 9 subclasses and then combined, or you used 3 classes only directly?

We ran it with all 9 subclasses as tests in Fuchs, 2023, showed, that for the main classes, there is no gain or loss in accuracy if classified directly or from subclasses. We slightly adjusted the passage in the text to make this clearer: "Pixels in orthomosaics are classified into nine different sea ice surface sub-classes that belong to three main classes (Table 3): snow/ice, open water and ponds (including submerged ice). Adjacent sub-class pixels of similar main classes are subsequently combined into main class vector objects if these consist of, at minimum, 100 pixels (the threshold was chosen similar to Huang et al., 2016)."

Line 336: Are there any more details already published on this vast melt pond? Appears to be a rather unusual object, for the pond to be that deep.

It is indeed a vast pond. Therefore, it was especially interesting as a test site for the algorithm. This manuscript is the first peer-reviewed study to investigate its depth (besides the PhD thesis of Fuchs, 2023). It is mentioned in Calmer et al. 2023, but only in the context of its Albedo, and shown in Figure 4 of Webster et al. 2022, but in the context of pond coverage evolution. We already started to investigate its depth evolution (e.g, IGS Conference 2023 contribution, discussion section in this paper) and decided that the full analysis of the underlying physical processes is worth a separate publication.

Line 379: I do suggest referring to the Discussion section here where this phenomenon is discussed and the likely explanation proposed, as such a drastic change in elevation (over 1.5m!) immediately grabs attention.

Great idea, we added: “We discuss this significant loss in depth of the largest ponds (incl. Mystery lake) in the discussion section.”

Fig.14 caption: Please add grade shade scale bar for surface elevation (above 0). BTW, did you try to compare DEM from ALS and photogrammetric DEM from this study?

We clarified in the caption that grey areas show the orthomosaics; pond depth data are only overlaid. Based on the other reviewers' comments on figure captions, we revised all figure captions.

Photogrammetrically derived DEMs were not compared to ALS data here, since Neckel et al, 2023 used the ALS data to slightly adjust the photogrammetric DEMs of MOSAiC we used. A comparison can be found in the mentioned study. We have included this additional adjustment due to the data availability from MOSAiC. However, the data from PASCAL show that the pond depth determination works also purely with photogrammetry.

Line 388: What is the contribution of these two largest ponds into the total meltwater/pondwater budget? In general, one can consider making a pdf of pond sizes/pond water volume in order to see which ponds contribute most to the overall pond water budget.

Many thanks for this very nice idea. In response to your suggestion and another reviewer’s comment, we decided to incorporate pond volume and sizes on MOSAiC even more into the manuscript. We added a new figure to the result section, adjusted the result description and with that, we have added a new aspect to the discussion on satellite upscaling methods, as these are closely related to the size distributions. We are convinced that this additional information is worth the slight extension of the manuscript by one paragraph and does not change any methodological aspects.

Line 446: “young ice” or “FYI?”

Many thanks for noting that. We kept the definition unspecific, as the separation is based only on personal testimonies. However, it was currently not even specific in being unspecific. We corrected that.

Line 490: Good also to have the elevation (freeboard) measured at these GCPs too , close to the timing of overflight. My experience show that even without accurate XY GCPs, Z-control points already improve the accuracy greatly at they "force" the DEM into their proper position eliminating the elevation gradient.

That's a very good point. We added that to the paragraph: “Therefore, we recommend a system consisting of GPS base stations with regular freeboard measurements that are recognizable in images and record the geographic position in the earth system. Such stations act as optical and geospatial GCPs, also improving the photogrammetric analysis through accurate horizontal and vertical position reference."

Line 557: The effect of reducing pond coverage was also observed in Divine et al., 2015
(https://doi.org/10.5194/tc-9-255-2015) when melt pond fraction declined towards the edge of the MIZ due to decreasing floe sizes and hence stronger lateral drainage.

Many thanks for that great hint. We added a sentence to the discussion: "This may also reduce pond coverage on smaller floes as observed by Divine et al. (2015) in the marginal ice zone."