Second Review of “The Pléiades Glacier Observatory: high resolution digital elevation models and ortho-imagery to monitor glacier change” by Berthier et al.

https://egusphere.copernicus.org/preprints/2024/egusphere-2024-250/

David Shean

August 11, 2024

Many thanks to the authors for considering all of my detailed comments, and providing a thorough response document. I know how much time is involved to do this. The revised manuscript reads well, and I believe is ready for publication.

There are a few items below which I encourage the authors to consider in their final revision/proofs, as well as ongoing product distribution, and future efforts to update PGO products and use for analysis. I am recommending acceptance, and I don’t require a detailed response to this second round of comments, but I know the PGO team will understand these points, and I am happy to follow up on any of this, provide further explanation, and discuss corrections outside of the review process. Hoping we can get these things right, to establish best practices and deliver the best possible products for the community.

Product availability

I was able to log in and download sample products! I noted that transfer rates to Seattle, WA were ~300-500 KB/s. The file I chose was 2023-11-11_0503585_Mera_ASE.tgz is 579 MB, so this required 15-30 minutes to transfer - not ideal, but fine.

After decompressing the archive, I noticed that the tif files were not tiled or compressed. I ran my gdal_compress.sh utility (https://github.com/dshean/gdal_tools/blob/master/gdal_compress.sh) using lossless LZW compression for the 4 tif files, which significantly reduced file size:

- 2023-11-11_0503585_Mera_ASE_1B_DEM_BM_2m.tif from 673M to 203M
- 2023-11-11_0503585_Mera_ASE_1B_DEM_BM_20m.tif from 6.7M to 3.0M

I created a new tar.gz archive with these files, the total archive size was reduced from 579 MB to 387 MB. I recommend that the PGO team runs similar gdal_translate commands to tile and compress the tif files, creates new archives, and replaces the existing archives on the distribution site before official release. This will reduce archive storage and file transfer times for the community.

Response to Reviewer #1:

L246-L248: I agree with this point and it also raises the question why the authors did not coregister to a more precise dataset such as ICESat/ICESat-2 altimetry? Is there a particular reason? I could imagine track spacing and surface slope are an issue?

The reviewer is right that summer laser altimetry data would be ideal, at least to correct a final vertical shift. However, especially at low latitudes, the track spacing is such that few to no laser elevation measurements would intersect with the Pléiades footprint (max 20 km wide but often much less given the shape of our sites).

The author's response is likely incorrect. While I don't think integrating altimetry is necessary for this paper, I encourage the authors to explore the available coverage from the 5+ year ICESat-2 archive. At lower latitudes, systematic ICESat-2 off-pointing fills gaps between reference ground tracks, and even with missing data due to clouds, there should be sufficient coverage (1000s of ATL06 points) for a typical PGO DEM footprint. Try running SlideRule ATL06-SR query for one of your low-latitude footprints.

Follow up on track-changes and author responses

Line numbers from revised manuscript with track changes, and screenshots from response document

56 - Minor point - these are not "raw," they are Level-1B or "Primary" products, with many corrections by Airbus before delivery

70 - suggest "stereo images can be acquired in an along-track pair"

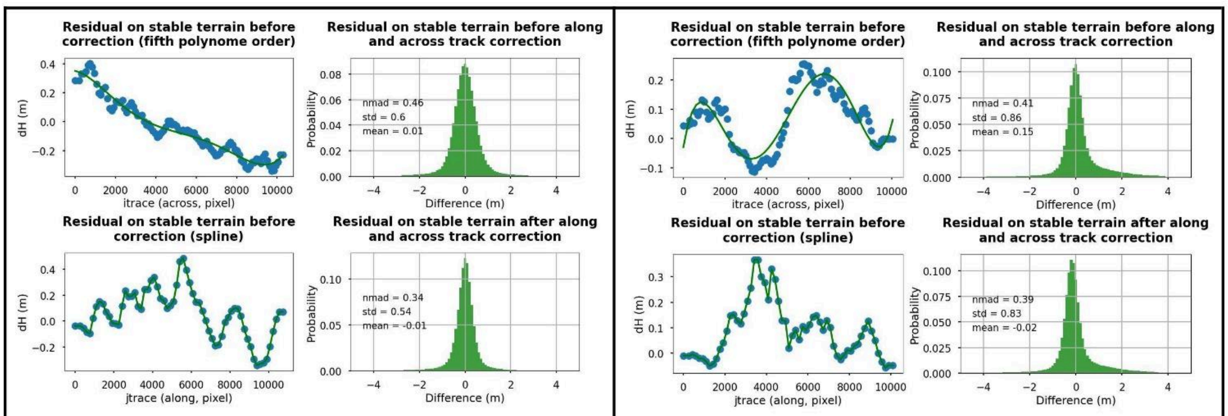
226 - delete "the demcoreg package, as described above"

228-230 and responses doc, regarding the 5th order polynomial fit to cross-track residuals

While I think the current approach is fine for the manuscript and initial release of the products, I maintain that this 5th order polynomial cross-track correction introduces unnecessary bias, and encourage the authors to carefully reconsider their methods for future corrections. I say this because I know that many in the community will use the PGO dh maps to compute geodetic mass balance without careful inspection.

210-221: I don’t understand why a 5th-order polynomial is needed for the cross-track direction, and there is no justification provided here. I just don’t see these artifacts in Figure 4, and I worry about over-fitting and introducing bias over adjacent glaciers from a limited sample. If there is a root cause related to the CCD geometry model in the Pleaides-HR camera model, or systematic “cross-track jitter” during collection of stereo pairs, then that provides justification for high-order polynomial correction, but I’m not sure that’s the case.

We confirm that across track biases are generally smaller than along track biases. Below, we show several examples where the spatially-structured biases should, we think, be corrected. A fifth-order polynomial performed suitably well to remove bias as used in Gardelle et al. (2013, http://dx.doi.org/10.5194/tc-7-1263-2013). We note that the corrections are all graphically (.jpg or .png) illustrated in the data products downloaded by the users. In case those generic corrections are not satisfying (and this is indeed sometimes the case), the user can easily come back to the uncorrected DEM and apply the relevant correction.



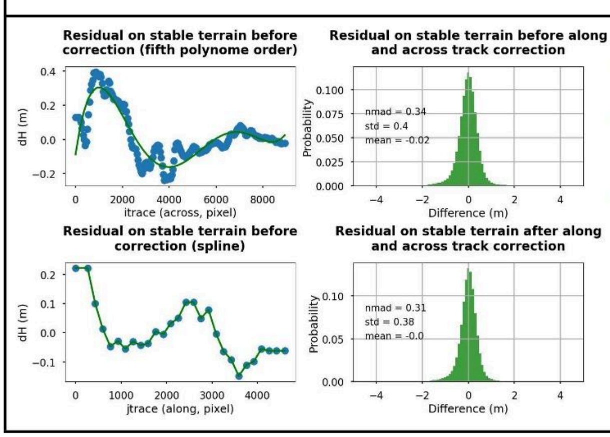
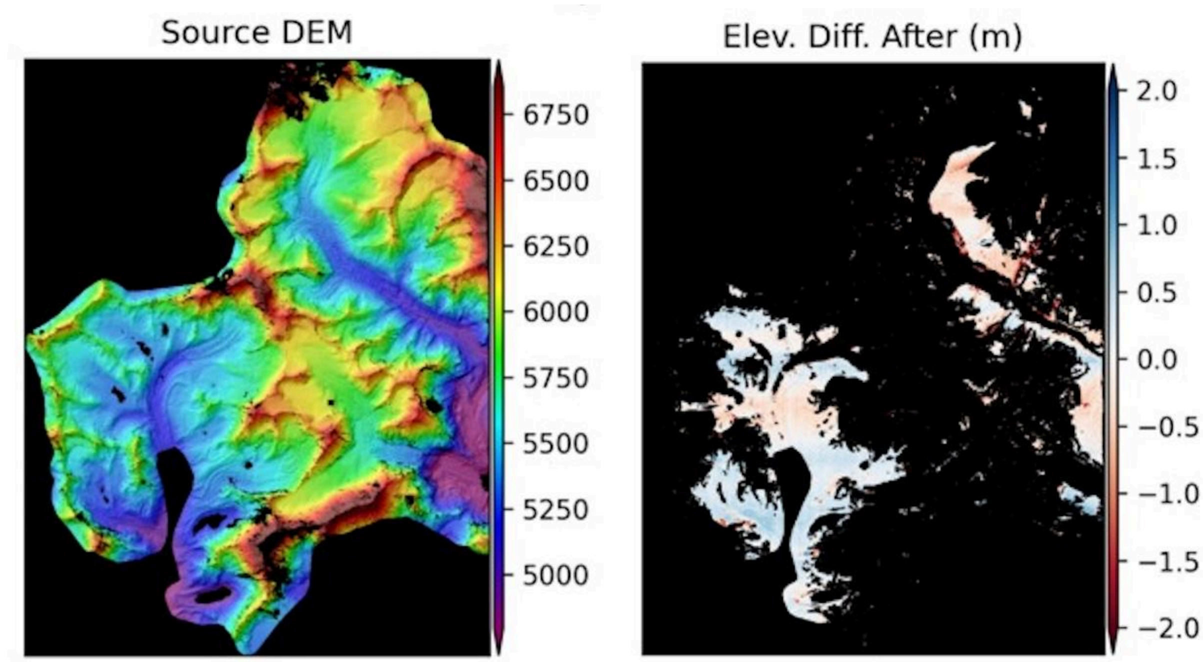


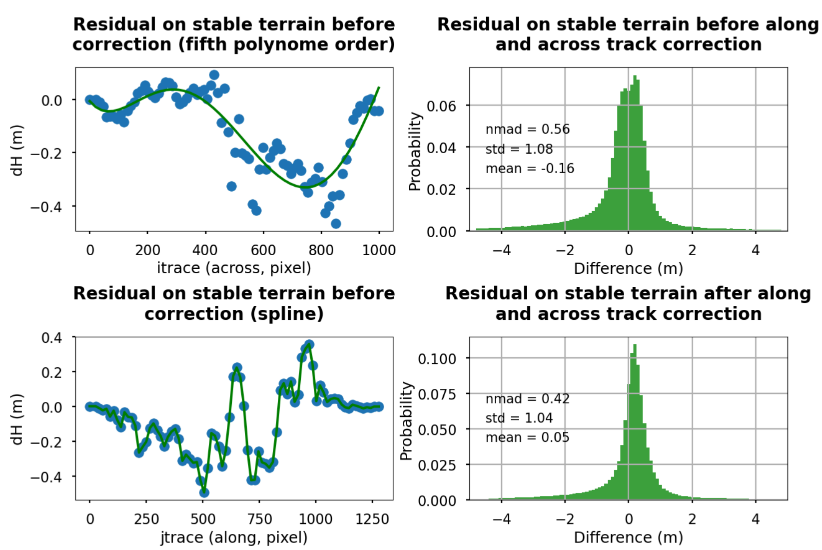
Figure R1. Three examples where a 5th order polynomial fit to the residuals off glacier was applied to reduce the uncertainties in the PGO DEM.
The examples are for product Gulkana_ALA_2016-09-01_2128501_2021-09-09_2122269_SGM_2m (upper left), Lombardy_CEU_2017-08-01_1032561_2022-08-21_1033056_SGM_2m (upper right) and Olsen_GRL_2017-07-17_1358160_2022-07-22_1413096_SGM_2m (lower left).

Based on the text, it sounds like this 5th order polynomial cross-track correction is performed first, then the spline along-track correction is performed second. **The order of these corrections is important, so if this is not the case, please clearly state the order in the text.** The order stated in the PGO paper (cross-track then along-track) is different than Gardelle et al (2013) “Then, we compute the elevation differences along and across the satellite track on stable areas and when necessary correct the bias using a 5th order polynomial fit.”

The PGO sample I downloaded (Makalu_ASE_2018-10-16_0500119_2023-10-23_0500156) illustrates the problem…

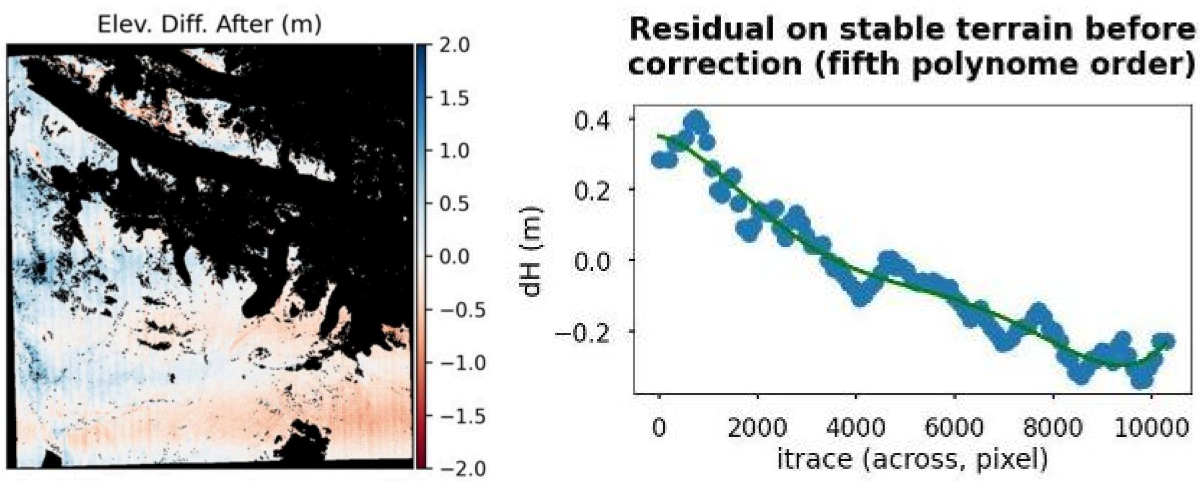


The right plot shows the non-uniform, anisotropic distribution of “stable” surfaces, and the clear residual along-track jitter artifacts over these surfaces. By first computing a cross-track correction, the non-uniform spatial distribution of the static control surfaces leads to preferential sampling of the jitter bias (which dominates the residuals), so the area to the west appears to have slightly positive bias, and the area to the east appears to have negative bias. Fitting the cross-track correction to these residuals, before the along-track jitter correction, biases some columns by over 0.3 m in this case, which will propagate to all glacier surfaces in those columns.

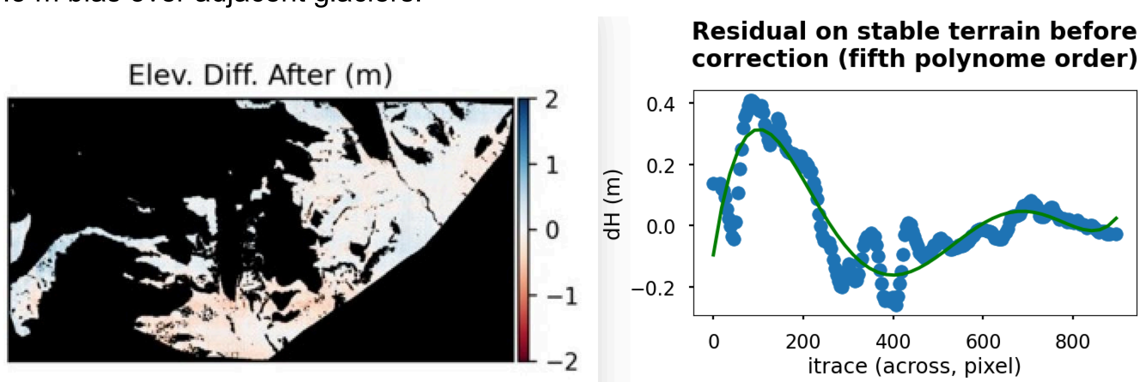


The along-track jitter correction should be performed first, as this captures most of the observed residual variability. I maintain that lower-order polynomials (planar, at most 2nd order) are sufficient to capture expected cross-track artifacts for linescan stereo sensors like Pleiades (assuming the detector array geometry is good, which it is for PHR1A and 1B).

Following along-track correction, the Gulkana residuals (below, mentioned in the response doc) could be modeled with a linear correction.



The Olsen residuals (also cited in the response doc) suffer from the same issues as Makalu above, with limited stable terrain, and non-uniform sampling of the jitter artifacts leading to apparent cross-track artifacts. The current PGO correction is unnecessarily introducing +/-0.2 to 0.3 m bias over adjacent glaciers.



Again, this is not a “must do” for this paper, but please consider this carefully for future processing efforts and future PGO product versions.

264-267 - Thanks for the detailed response regarding the systematic geolocation bias with latitude. Interesting to see the plots in the response document. Hopefully the source of this issue becomes clear. Personally, I would exercise caution during interpretations for different sensors, as there is considerable spread in the translations at all latitudes.

I appreciate the tests with different reference DEMs. Just to confirm, this was all done by first transforming the reference DEM to match the local UTM zone of the PGO DEM?

While it shouldn't be an issue within a UTM zone, I'm still wondering about projection distortion issue with latitude. Distance, direction, and pixel area are all important properties for Nuth and Kaab, and these will be distorted by different amounts depending on projection choice and latitude. One way to test this is to repeat the co-registration using a local planar projection (like orthographic, or stereographic with clat,clon defined by center of DEM).

267-272 and responses doc, regarding the 2.4 m vertical bias and refraction/aberration correction

> While some of the systematic 2.4 m vertical error could be due issues around penetration through snow in the GLO-30 products, I'm suspicious that this could be systematic error in the ASP v3.0 output (note similar vertical bias magnitude for WV/GE documented in Shean et al., 2016 and Fig 4 in Shean et al (2019) PIG paper in TC). This could likely be related to the (implementation and/or lack of) atmospheric refraction and velocity aberration correction for the version of ASP you are running, and/or some of the 2.0 m uncertainty associated with definition of the WGS84 Ensemble (EPSG:4326) vs a specific ITRF/WGS84 realization (e.g., https://spatialreference.org/ref/epsg/7664/ - see WKT2). Note that the latest ASP includes new, validated refraction/aberration corrections using the USGS Community Sensor Model, and support for PROJ9 and unambiguous output product 3D CRS definitions using WKT2.
>
> **We computed one of the DEMs using the last version of ASP at the time of writing (v3.4) and found almost no changes (within 1-2 cm in height). We computed the mean vertical error for Pléiades 1A and 1B and found it was more negative (-3.9 m) for 1B than for 1A (-1.1 m). Hence, a**
>
>
>
> **sensor specific correction may be partly responsible for the systematic vertical error and this is also under investigation at CNES. See revised section 3.1.1.**

Glad you tried this, but unfortunately, the refraction and aberration corrections were not enabled by default for Pleiades. See release notes:
https://stereopipeline.readthedocs.io/en/latest/news.html#release-3-4-0-june-19-2024

WorldView (DigitalGlobe) cameras (Section 5):

- The WorldView linescan model got moved to a CSM implementation. The transitional option `--dg-use-csm` was removed. The new implementation is about 5x faster for ground-to-image projections.
- Re-enabled correcting velocity aberration and atmospheric refraction. These corrections are now implemented in the CSM camera model, and, unlike before, play nicely with bundle adjustment (Section 5.9).
- The options `--enable-correct-velocity-aberration` and `--enable-correct-atmospheric-refraction` got removed.
- Non-DG cameras do not use these corrections, as a case for that has not been made.

I reviewed appendix C in the Pleiades user guide (https://content.satimagingcorp.com/media/pdf/User_Guide_Pleiades.pdf) and it appears that we need to enable these corrections for the rigorous model in the Pleiades primary products (and perhaps also the RPCs). "The value of the atmospheric refraction correction depends on the incidence (null when viewing at nadir, **approximately 2m when incidence equals 15 degrees**)." - the magnitude is consistent with your observed vertical bias, and the sensor source (1A vs. 1B) may be less relevant than the relative geometry of each pair.

I raised this issue with ASP devs, and we will revisit. The PGO team can also repeat the ASP v3.4.0 test the CSM linescan model implementation for Pleiades with these corrections enabled.

319: lidar data source

280-281 - Must provide citations for these ALS data (with information on how they were collected and processed), and ideally links to open data archive for the specific products/versions used. I believe these were all ACO products in Canada, not sure about Norway. Details on instrument, altitude, etc will help interpret the observed differences with PGO DEMs over surfaces with variable roughness (e.g., crevasses). While most ALS data should be significantly more accurate/detailed than PGO DEMs, there is a huge range in ALS data quality, often related to instrument specs and processing approach.

The equipment and processing steps used to produce the ALS data are the same as those described in Pelto et al. (2019), now cited in the paper. Work is in progress to release the entire ALS data obtained over glaciers in western Canada but that manuscript is not yet finished.

Thanks for including Pelto reference for the BC lidar surveys. It is disappointing to see that **no corresponding information was included about the Norway lidar collections in the text or the responses**. According to the author contributions section, presumably co-authors JMCB and LMA provided the airborne lidar data for Norway and can track this down? Hopefully someone can include citations or some text with information about those surveys, even if the lidar data are not publicly available. As a community, we need to be more careful assuming "lidar is truth" and "all airborne lidar data have the same accuracy."

324-327 - regarding lidar data preparation and vegetation masking

327-328: While ALS does obtain returns from beneath the canopy, if you are not isolating first/only or ground returns in the ALS point clouds, then ASP is going to produce some weighted average of all available ALS point elevations in each grid cell. You will not end up with a DTM (and I'm not sure that a citation to Piermattei should be included here).

My recommendation would be to mask the vegetation, as this affects your stats, and you need to create a DSM, and need to deal with height percentiles (e.g., 90th percentile) when gridding lidar points for comparison with stereo products. This is tricky, and an important gap that requires more attention from our community in my opinion - ALS is great, but it needs to be carefully processed when used as "truth" for stereo evaluations.

We think it is actually important to show the influence that vegetation can have on two different types of measurements (stereo vs. Lidar). It is maybe obvious for specialists of Lidar but probably not to all users of the PGO database. Even near-contemporaneous acquisitions can exhibit spatially structured biases due to different interactions with the surface.

I think the authors missed my point about the vegetation. The lidar point clouds should be filtered to preserve first returns to produce a DSM, or vegetation should be masked before co-registration.

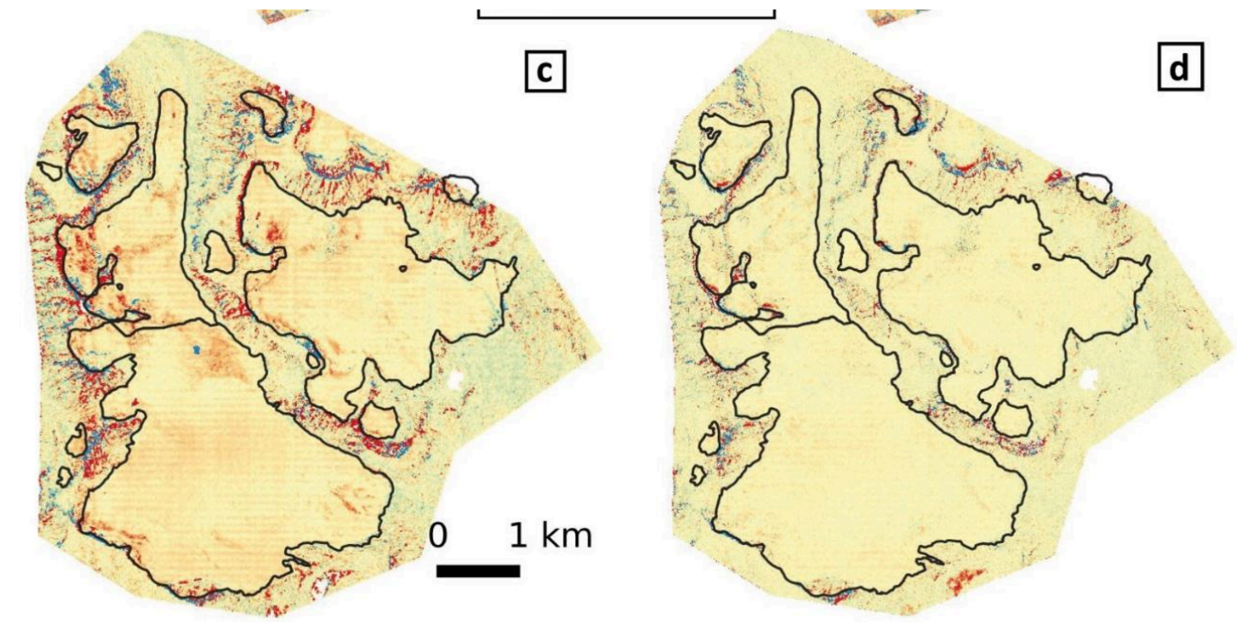
I agree that showing and analyzing offsets over vegetation is valuable, but the current approach will potentially lead to incorrect co-registration and bias output DEMs.

When creating a DEM using ASP's point DEM, you are using points from all lidar returns (top of canopy, canopy, understory, ground). This will lead to systematic bias in the gridded values over vegetated areas - the final point2dem weighted average elevation for the output grid cell is not the top of canopy and not the ground, but somewhere in between (excatly where depends on point density).

The example in Figure 7 A/B shows vegetation bias in blue with relatively small areal coverage (maybe 5%), so it shouldn't impact the robust co-registration approaches used (automatically eliminate outliers). But using the same strategy will have a much bigger impact for other sites with greater vegetation cover (>20-30%).

I'm not suggesting reprocessing everything, but please be careful of this in the future - either isolate first returns from the lidar point cloud to create a DSM, or mask vegetation during co-registration. You can then perform the same analysis using the unmasked products, which will show the important dh difference over vegetation. Hopefully that makes sense.

Fig 7 - NMAD for BM and SGM compared to lidar for Hellstugubreen



I'm a little surprised by the observed differences between c and d if the only difference is the input PGO DEM using BM (c) vs. SGM (d). This potentially indicates that the independent co-registration approach (co-registering BM to lidar and independently co-registering SGM to lidar) is introducing bias that could be incorrectly interpreted as due to the correlation approach (or terrain properties). **We observe many local artefacts (Figure 7). However the bias (see column "Median Dh on glac**

16

(m)" in Table 3) differ by only 0.05 m between the two correlation approaches so we do not really the point of the referee.

I think the authors misunderstood my earlier comment. It appears that there is larger residual horizontal geolocation error in the BM product compared to the SGM product, likely introduced during the independent co-registration process for each. Please check the translation vectors for the dh maps used to produce 7c and 7d - I can't do this because the products involving lidar data are not available (which is fine, but limits reproducibility).

I'm having a hard time understanding how the on-glacier NMAD values in Table 3 for the Hellstugubreen **BM** DEM (0.12 m, Fig 7c) is lower than the on-glacier NMAD for the

Hellstugubreen **SGM** DEM (0.15 m, Fig 7d). Just looking at these difference maps (with identical color ramp), there appears to be more spread in the BM dh product than the SGM dh product (reproduced above for clarity). I'm wondering if there was potentially a mistake in the analysis/plotting code or labeling in the figure? Maybe double-check, and if necessary, modify the statement on linked 364-366:

> the uncertainty on glaciers is a conservative approach. Interestingly the choice of the correlation algorithm (BM or SGM) has a different influence on and off glaciers. SGM results in lower NMAD ~~is superior~~ off glaciers whereas using BM leads to reduced NMAD on glaciers.

492 (Fig 10 caption) - "mass balance estimates" instead of "mass balances"
469 - same