

Institute for Marine and Atmospheric research
Utrecht University, the Netherlands

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Editorial office of The cryosphere
Bahnhofsallee 1e
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Dear dr. Kang Yang:

Attached you find an improved manuscript, which has your suggestions implemented of the second round of review.

We hope you find the corrections sufficient to start the publishing process.

Thank you for your time and
consideration. Kind regards,

A handwritten signature in black ink, appearing to be 'Bas Altena' with a stylized flourish underneath.

Bas Altena, Andreas Kääb & Bert Wouters

encl: response to Reviewer 1

Anonymous reviewer 1, from 22 Feb 2022

Overall, the authors have done a good job addressing reviewer concerns. I especially commend the authors for going through the trouble of implementing the AmpCorr algorithm and the workflow of Joughin, 2002 for comparing the different uncertainty metrics. This is no easy task, and hopefully this more detailed comparison is illuminating for readers who are willing to get into the weeds. I only have a few minor comments remaining for typos and clarifying points (note: line numbers are referenced to the revised manuscript).

- Line 103: "directly relate the correlation peak" → "directly model the correlation peak".
sentence has been corrected.

- Figure 2: The caption and the title for maps (b) and (c) indicate variance, but units of mday are used. Do the authors mean standard deviation instead? Or are the units (mday)²?

This is correct, but standard deviation (σ) instead of variance (σ^2) is given, hence this is now adjusted in the figure.

- Figure 2: As mentioned in the response when comparing to ROI-PAC, the south-western side has high uncertainties, and the authors mentioned "lack of structure", but the ROI-PAC uncertainties are low, which seems inconsistent to me, but I'm sure there's an explanation for this. The authors alluded to the explanation in Line 179 ("where crevasses occur"), but didn't state explicitly the orientation of the crevasses relative to the flow direction. I would suggest a few short words on this for the readers' benefit.

Concerning the first point, an intermediate step in our argumentation is not given, which might have caused the difference between both estimates; As there is a lack of structure results a low signal to noise ratio (SNR) is present. Instead of a single pronounced peak in the correlation function, a noisy landscape will be present. This has an effect on the fitting, especially for our proposed method, as it uses more of the surrounding than the ROI-PAC method.

Concerning the second point, we have now more clearly stated more clearly what kind of crevassing occurs: i.e.: sheared.

- Line 179: "south-eastern part". Do you mean south-western?
This suggestion has been implemented.

- Figure 3: Might want to place a white box behind the Greek symbols on the map for easier viewing.
A white background is now included in the figure.

- Line 242: From my original comment and the authors' response, it seems like we have slightly different definitions of peak-locking. From what I can find in the literature (PIV and stereo vision), peak-locking is not unique to frequency domain methods and can be present in discrete methods in the spatial domain as well. Therefore, if the authors have a reference for peak-locking as it pertains to frequency domain matching, please include it here.
A reference is included in the new manuscript.

It is a very interesting aspect that the reviewer is raising, but if no references are given it is a bit difficult to respond.

Nevertheless, to us it is not a surprise that the effect of peak-locking might be present in spatial methods. The underlying cause might be rooted from the same origin, since many if not all spatial correlation methods use the discrete Fourier transform under the hood. This speeds-up the convolution, but as a by effect might introduce a integer preference.

- Line 246: I would merge the two sentences, e.g. "...such estimates seem too optimistic" → "such estimates can be highly influenced by sample statistics where a large amount of pixels in a template cause..."

Sentences are merged now.

- Line 278: *Frequency domain methods produce sharp peaks where? In the correlation surface that's transformed back into the spatial domain? Please specify here.*

It is now better specified in the text.

Also, as the authors mentioned, spatial domain methods that do not take into account affine transformations within the image template also assume rigid translation, so there's a distinction there that should be briefly mentioned here.

Theoretically, this might be the case, but in practice spatial domain methods are less sensitive (see Figure 4.10 in [Altena 2018]). While Fourier methods have a distinct separation into a phase and amplitude domain (see e.g.: [Altena and Wouters, 2021]). An eloboration into this domain might not be of interest to the reader.

- Line 346: "metodology" → "methodology"

Correction is done.

References

[Altena 2018] Observing change in glacier flow by using optical satellites

[Altena and Wouters, 2021] Shadow cast tracking for deduction of elevation data through affine matching methods on optical satellite imagery