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Interactive comment on "Ice velocity of Jakobshavn Isbræ, Petermann Glacier, Nioghalvfjerdsfjorden and Zachariæ Isstrøm, 2015–2017, from Sentinel 1-a/b SAR imagery" *by* Adriano Lemos et al.

Anonymous Referee #1

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Review of "Ice velocity of Jakobshavn Isbræ, Petermann Glacier, Nioghalvfjerdsfjorden and Zachariæ Isstrøm, 2015-2017, from Sentinel 1-a/b SAR imagery" by Lemos et al.

MS : tc-2017-251

Summary: The authors present velocity data from Sentinel 1-a/b from four major Greenland Ice Sheet outlet glaciers. The data provide higher temporal resolution than existing datasets and extend the existing temporal record by several years. The authors document multi-annual and inttra-annnual speed changes.

General comments

1) The paper could be significantly improved by expanding on the implications of observations for better understanding ice dynamics. Right now, to me it reads as a lot of results without much discussion of significance or implications. For this work to be published in The Cryosphere, it seems it should increase our understanding of glacier mechanics/dynamics, not just describing what we see. For example, why has Jakobshavn Isbrae (JI) begun slowing? Why did the amplitude of seasonal velocity change increase on JI at the same time?

2) It seems to me that you could better exploit the novelty of this high temporal resolution dataset to investigate processes at "the timescales over which glacier dynamics evolve". Resolving multi-annual trends in velocity doesn't require 6 or 12 day repeat times. But investigating seasonal dynamics hugely benefits from this increased temporal resolution. I believe your paper could be strengthened (in its ability to demonstrate capability of new generation radar satellites) by digging in deeper to these processes that are more difficult to resolve with existing datasets.

3) The introduction could use more substance by referencing relevant existing work. See "specific comments" for some suggested references.

4) I wonder if your method for characterizing uncertainty may overestimate your error. It seems that by using SNR (where SNR = (mean velocity)/(standard deviation of velocity) within your window, correct?) would be higher where velocity is spatially variable. In this case, SNR would be high, but not because of bad data – because of physically meaningful velocity variation. It seems like the margins/shear zones typically have high uncertainty – could this just be because there are very different (physically meaningful) velocities across these regions?

For these reasons, and the specific comments listed below, I recommend the manuscript requires Major Revision before publication.

Specific comments

Page 1 L28: Your definition of mass balance appears to ignore negative terms in SMB (i.e., only mentions mass input)

L39: See below for relevant papers for this idea that should be cited – e.g.,

Felikson, D., Bartholomaus, T. C., Catania, G. A., Korsgaard, N. J., Kjær, K. H., Morlighem, M., ... Nash, J. D. (2017). Inland thinning on the Greenland ice sheet controlled by outlet glacier geometry. Nature Geoscience, 10, 366–369. https://doi.org/10.1038/ngeo2934

Durkin, W. J., Bartholomaus, T. C., Willis, M. J., Pritchard, M. E. (2017). Dynamic changes at Yahtse Glacier, the most rapidly advancing tidewater glacier in Alaska. Frontiers in Earth Science, 5(March), 1–13. https://doi.org/10.3389/feart.2017.00021

Page 2 L7: Should cite modern landsat efforts mapping glacier velocity velocity change at large scale – e.g.,

Armstrong, W. H., Anderson, R. S., Fahnestock, M. A. (2017). Spatial patterns of summer speedup on south central Alaska glaciers. Geophysical Research Letters, 44. https://doi.org/10.1002/2017GL074370

Burgess, E. W., Forster, R. R., Larsen, C. F. (2013a). Flow velocities of Alaskan glaciers. Nature Communications, 4, 2146. https://doi.org/ 10.1038/ncomms3146

Dehecq, A., Gourmelen, N., Trouve, E. (2015). Deriving large-scale glacier velocities from a complete satellite archive: Application to the Pamir-Karakoram-Himalaya. Remote Sensing of Environment, 162, 55–66. https://doi.org/10.1016/j.rse.2015.01.031

Fahnestock, M., Scambos, T., Moon, T., Gardner, A., Haran, T., Klinger, M. (2016). Rapid large-area mapping of ice flow using Landsat 8. Remote Sensing of Environment, 185, 84–94. https://doi.org/10.1016/j.rse.2015.11.023

Jeong, S., Howat, I. M. (2015). Performance of Landsat 8 Operational Land Im-

ager for mapping ice sheet velocity. Remote Sensing of Environment, 170(8), 90–101. https://doi.org/10.1016/j.rse.2015.08.023

L27-28: Many authors have shown that high melt years actually correspond with smaller net annual displacements – e.g.,

Burgess, E. W., Larsen, C. F., Forster, R. R. (2013b). Summer melt regulates winter glacier flow speeds throughout Alaska. Geophysical Research Letters, 40, 6160–6164. https://doi.org/10.1002/2013GL058228

Tedstone AJ and 6 others (2013) Greenland ice sheet motion insensitive to exceptional meltwater forcing. Proc.Natl. Acad. Sci.U. S. A., 110(49), 19719–19724 (doi: 10.1073/pnas.1315843110)

Van De Wal, R. S. W., Smeets, C. J. P. P., Boot, W., Stoffelen, M., Van Kampen, R., Doyle, S., ... Hubbard, A. (2015). Self-regulation of ice flow varies across the ablation area in south-west Greenland. Cryosphere, 9(2), 603–611. https://doi.org/10.5194/tc-9-603-2015

Page 3 L35: I believe you are talking about 5 cm position uncertainty in the satellite. What is your uncertainty in the ground coordinates of each pixel?

L36-37: I am not a SAR person – do you have to model or account for atmospheric delays? Seems like you would have to do that before isolating glacier displacement.

L41-42: What year was the data used for the GIMP DEM collected? How would error in the GIMP DEM (or elevation change between its acquisition and your time series) affect your velocity estimates?

Page 4 L4-6: What labelling algorithm do you use? This bit is unclear and would be difficult to repeat.

L10-13: It seems like this approach would lump actual physical spatial velocity variability with spurious non-physical velocities. Could this be why your glacier margins/shear zones on Figure 3 are always high uncertainty? There actually is high spatial variability in velocity there, so SNR would be low, but not because of bad data. It seems like this method may overestimate error in some regions.

L17: What is your “airborne estimate of elevation change”? What constitutes an “extreme case” (i.e., what is the magnitude “large” of surface lowering? Needs more detail.

L15-24: Are these errors systematic or random? Seems like tidal forcing may be random (sometimes surface is higher, other times surface is lower than you think) but thinning would be systematic (always lower than you think).

L26-27: It seems that using one SAR estimate of velocity to check another SAR estimate of velocity is vulnerable to errors that would effect both in the same way. Do you have any known velocities from GPS or optical image correlation to compare against?

L26-27: Should also specify that TSX is based off phase change (InSAR) and you are using feature tracking of SAR imagery (amplitude based) if that is indeed the case. This provides more evidence that using it is a robust check on your data because it is a different method.

Page 5

L 7-9: How does this “highlight the importance of resolving glacier velocities within their near terminus regions”? And resolve for what purposes?

L25: What station is this 5 year speed change calculated at? Jif? Looks like JI1/2 are relatively stable over this time. What would magnitude be if calculated there?

Page 6

L1 - This is interesting that you find different relationship between speedup and retreat at different glaciers. But seems similar to findings by Moon et al., that find some glaciers terminus-forced, others see more land-terminating style of meltwater-forced. If dL = u$_{ice}$ −

$-u_calve, thenwouldexpectlengtheningifcalvingcan'tkeepupwithfastericeflowinsummer.$

L20-24 – How could Munchon et al. [2016] have used data from 2016/2017? And seems like comparing apples to oranges to compare speedup calculated over different time spans. That seems to tell more about glacier dynamical change than measurement accuracy.

Figure 1 – difficult to see lines. Please thicken all lines, but especially green. I also think the overlay makes the image harder to interpret. Maybe just overlay on a grayscale hillshdaded DEM that would provide topographic data but not confuse velocity information.

Figure 2 – This figure would be clearer to me if you labeled the lines in at least panel a

Figure 5 – Could you show earlier data (from a different source) to put this plot in context of longer-term JI velocity evolution?

Figure 6 – How are portions of data for fitting red and black lines decided? Don’t think either is the 2009-2011 or 2012-2017 fit lines mentioned in the text?

Technical corrections – typographical errors

Page 1

L33: language is a little sloppy/unclear – marine terminating glaciers still have SMB – should specify this 30

L34-35: “erosion of their termini” → replace with “terminus retreat” or “submarine melting”; I can’t tell what you mean and “erosion” could be confused for subglacial bedrock lowering

L38: “high frequency variability” → “high spatial variability” if this is what you mean

Page 2

L8: “Polar Regions” → “polar regions”

L26: “over the past few years” implies the JI speedup is ongoing (which you show it is not)

L33-35: Sentence starting with "Therefore..." is confusing. You are saying -10 km2/a is not a big area change? Could just reword this sentence to be positive (e.g., "glacier area has remained relatively constant") instead of negative (e.g., "glacier area has not changed an unusually large amount")

Page 3

L1: migration of what? Would call this "terminus retreat rate" if that is indeed what it is.

L8: Sentence starting with "Although located..." could be reworded to use fewer commas.

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