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The Cryosphere Discussions

Interactive comment on "Observations of seasonal and diurnal glacier velocities at Mount Rainier, Washington using terrestrial radar interferometry" *by* K. E. Allstadt et al.

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Observations of seasonal and diurnal glacier velocities at Mount Rainier, Washington using terrestrial radar inetrferometry K. Allstadt et al.

This manuscript presents a detailed analysis of spatial and temporal variations in glacier flow at the case of Mount Rainier. The novel technique of terrestrial Interferometry is successfully used to derive continuous surface velocity fields for summer (melt) and Winter situation for the glaciers at Mount Rainier. This allows a very interesting and detailed investigation of spatial distribution and temporal evolution of ice flow. The comprehensive and novel datasets presented are impressive and give some very in-

teresting insights into seasonal and spatial variations. In general the results are mostly well presented and carefully analysed and discussed and substantial conclusions are reached that are relevant from an ice dynamics and an observation technique perspective. However, there are a few points mainly relating to the modelling analysis and interpretation of basal sliding that are in my view rather speculative and need addressing before publication.

General issues/comments I am a bit critical about the method and consequently the results regarding the quantification of basal sliding, in particular in relation to ice deformation and I think the derived ratios are subject to very large uncertainties that should be better discussed. The above 90% sliding to ice deformation ratio seems to me a very high estimate and could well be lower. I briefly outline my points below: 1. Ice deformation is highly dependent (linearly) on the rate factor A which itself is (for isotropic ice) dependent on ice temperature, water content and impurities and is in general not that well know. Even for ice at 0 degrees (temperate ice) literature values vary by a factor of 2 (higher than used here, see also Paterson) and impurities and high water content (probably to expect for a relatively warm and moist climate regime) may lead to even higher rate factors. This means the ice deformation could easily be a factor 2 to maybe 3 bigger which results in substantially lower sliding ratios (factor 2-3 higher ice deformation). I agree that the chosen value for A is probably the best guess but it is not in stone.

2. Bed topography and therefore ice thickness are not that well known (as clearly stated on p. 4092 line 1-2) which potentially impacts very strongly on the inferred ice deformation velocities. In particular in areas without radioechosounding data, which I assume includes that fast flowing areas of ice falls, thicknesses are interpolated and may well be off by more than the given +/-11m RMSE. Even if we assume just 11m uncertainty in thickness for this relatively thin glacier of 30m to 80m we get thickness uncertainties of 25% to 12% which (due to the non-linearity between ice flow and thickness) result in and over- or under-estimation of ice flow by a factor 5 (30m) to 1.8 (80). I guess for

the thin ice fall regions uncertainties in ice thickness likely will be higher, and as the ice is thin there it will turn into even higher uncertainties in ice flow estimates (more than factor 5). This means the calculated velocities due to ice deformation and in particular the spatial variations will be strongly affected by uncertainties in bed topography and consequently weaken the conclusions on basal sliding and its spatial patterns.

3. Further the used DEM is from 2008 and thinning (in average) from 2003-2011 is 8m. Has this been taken into account? If not, thicknesses to calculate flow may in places well be overestimated by about 4m which actually overestimate ice flow due to deformation (which is in favour of the conclusion of flow dominated by sliding) between a factor of 2 (for 30m) and 1.3 (for 80m).

4. The approach to calculate velocity fields for ice deformation (using the shallow-ice-approximation) is also questionable, in particular in areas of large changes in surface (bed) gradients such as around ice falls. The spatial smoothing (Echelmeyer method) certainly improves results compared to pure SIA, but I still think large uncertainties remain which are currently just assumed to be basal sliding (residuals packed into basal sliding). I agree that not too much modelling effort should be done if the bed (and ice thickness) are not well known, but in such a case maybe one should rather not try to derive accurate basal sliding rates at all and keep the modelling and interpretation on sliding simple.

Thus, overall the basal sliding analysis/modelling part (and its spatial variation) seems to suffer from over-interpretation in particular regarding the large uncertainties attached to the modelling. I would expect a less narrow consideration of these modelling results (% in sliding ratios) and that modelling uncertainties related to flow parameters, model choice and geometry data are taken into account and communicated. This would actually strengthen the case. Rather than exact sliding ratios, tendencies could be communicated in the conclusions Doing a modelling inversion is hard and certainly was time consuming but I think the details (peff and exact sliding %) currently do not add that much. Maybe the modelling part can be simplified and reduced as the outcomes are

due to the large uncertainties rather speculative.

Specific comments Abstract lines 12+13: I am a bit critical about these sliding ratio numbers, the method behind and think there are very high uncertainties attached to these numbers (could well be smaller…).

p. 4068 line 25: this is a very general statement but the references refer to the very specific glaciers if this study.

p. 4070 line 16: rather a remark: excuse me my ingnorance but I was initially surprised about this statement of 'among best studied glaciers', as I did not know much about them. After reading the paper I agree that they are well researched but maybe 'best-studied' is another league.

p. 4074, line 2: but before (introduction 1min minimum repeat intervals are mentioned and later for this study 3min are chosen (and as far as I know 1min is minimum given by the gamma-make used here). So why not mention thes actulally used intervals od 3 min.

p. 4074, line 1: but I guess snow compaction was not measured the targeted glacier surface, so my questions is if this snow compaction can really be ignored.

p. 4075, line 5-6: I do not quite follow this what 'interpolated result' is meant here

p. 4075, line 7-8: maybe this stacking needs to be explained a bit further, for non-TRI experts this is maybe not clear.

p. 4076, line 22: specify here from when DEM is: '…an existing DEM from 2008 to …'

p. 4080 line 23: here, the uncertainties mostly refer to 'atmospheric variations' I assume.

p. 4081 section 4.4 and figure 8: I think here this comparison of velocities could quantified better by just comparing absolute line of sight (LOS) values (project all data in LOS direction). The figure is useful as a visual comparison but maybe a comparison of summary measures (Mean, SDT,. . .) would be useful.

p. 4082 line 15: interesting this increase in velocity from July to winter at the ice fall and certainkly good to discuss this. But maybe worth saying that it is a 'slight' increase. To be positive, I think even if velocity do not change there this is interesting.

p. 4082 line 18-20: a note following on the point just above: according the the kinematic wave theory applied for glaciers (Nye 1961, 1963, 1965, also in Vanderveen book Fundamentals of Glacier Dynamics 2nd edition, p301ff)) the along-flow propagation of changes in thickness/flux is related to flow speed and the inverse of slope, which implies changes in ice thickness/speed struggle to propagate over steep ice falls. Although this paper does not deal with thickness changes the theory also applies to flux changes (including velocity) so may be relevant here.

p. 4083, line 5: just a note: given the large diurnal variation in air temperature (and potentially atmospheric conditions I am quite surprised that the interferometric results are not affected more by atmosphere. I guess the stacking and corrections take care of that.

p. 4084, section 5.3 flow modelling: if the modelling remains a central part of the analysis I would move the brief model description (with a celar and early reference to the details in the appendix) already in the METHODS section.

p. 4084 line 17/18: it is crucial to refer to the Appendix here for model details (at the end of this section is in my mind too late) and I would specify here what ratefactor (A) is used e.g. '. . .using an ice rheology corresponding to temperate ice (see Appendix. . .). This is crucial as firstly the choice of A introduces relatively large uncertainties (which should be communicated) (see also main comments).

p. 4084 line 24: how is 'weak' spatial dependence done? Is it partly a consequence of the length coupling (weighting) of the ice deformation calculation. If such a peff inversion has been done (although I think given the data available this may overdo

(see main comments)) I would be interested to see the resulting peff variations with space. Or is it basically spatially constant, then I guess such an inversion dos not add too much anyway.

p. 4085 line 8: based on the given data (and modelling analysis) I do not quite agree with this conclusion of almost all flow by basal sliding. The uncertainties from rate factor, bed topography (thickness), etc. are pretty high (several fold) (as explained in detail in main comments), so these sliding ratios could well be quite different (in both directions but with a tendency to be rather smaller). Thus, I would not take these sliding % numbers as too narrow. Certainly, the uncertainties in these numbers should be discussed and communicated and maybe to conclusions be softened up a bit (e.-g. according to this modelling analysis, flow is likely to be dominated by basal sliding). Similar for the spatial variations in sliding I would be a bit more vague, the uncertainties in bed topography and type of model used will for some areas likely dominate the signal.

p. 4086 line 1: again, the poor fit may well point to the large uncertainties in the modelling approach (parameter, model, datasets,. . .).

p. 4086 lines 11: I would rather say '. . .are consistent with. . .' or '. . . can likely be attributed with . . .' as apart from velocity chages there are virtually no further data supporting this claim. Most of the discussion on related basal hydrology changes are based on general understanding from elsewhere. Although I welcome an integration into the general/existing understanding I think the discussion and interpretation could maybe rely a bit more and clearer on collected data/evidence. Maybe in this paragraph the inversed peff (if it really is useful) could be linked in as well.

p. 4087 line 23-25: again if Neff is really inverted and shows something, I would like to see it here (and how it varies in space).

p. 4088 line 11-12: near the tongue the decrease in velocity is simply because the glacier retreated (and at the terminus it should be close to zero!!!).

p. 4089 line 14-15: again I struggle with these very narrow sliding ration numbers, maybe soften the numbers a bit, take into account uncertainties and use a more vague formulation (tendencies).

p. 4091: lines 12 . . .: an assessment of uncertainties in A on U_deformation would be useful. . .

Figure Fig. 1: the dark green for the arrows is not an ideal color choice, appears almost as black, maybe change color to something more distinct.

Fig. 3: caption: change to '. . . slope-parallel TRI velocity for. . .'

Fig. 4: the legend/colorbars here are very small that I could hardly read the numbers, actually similar for other figures (9/10).

Fig. 6: it would be nice to have some idea about uncertainties of these velocity data. i agree that the graph should not be cluttered too much but maybe a rough uncertainty bar somewhere would help, or simply put it in text in caption. Should it for the profile location not refer to Fig 4 instead of Fig. 5 in the caption?

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