Response to review of "Observations of seasonal and diurnal glacier velocities at Mount Rainier, Washington using terrestrial radar interferometry"

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We thank the reviewers for their comments. We have incorporated most of their suggested changes and explain the specifics in response to each comment below. The vast majority of the comments from both reviewers were regarding the modeling, which was mainly included to add additional interpretation of the observations, it was not the main focus of the paper. We recognize that we needed to modify how we incorporated modeling so that it didn't distract from the main point of the study but still contributed. Therefore, we removed the sliding model, which was too simple and problematic in the eyes of both reviewers and did not add much to the paper scientifically anyway, and we introduce uncertainty estimates for the deformation model to address the reviewers' comments regarding that model. We also made some minor modifications to the text to further improve clarity.

Response to comments:
Original comments in black, author response and changes in red

Response to M. Luthi comments

On page 4074, line 11, it is stated that the interferograms were created from MLIs. They are not (since MLI are just signal strength without phase information) but they are created from the SLC data. I would have assumed that this is a typo, but then in Figure A4 the same statement reappears, and is even illustrated. This looks like a serious misunderstanding of the radar data analysis process. In the Gamma software the call signature of the program creating an interferogram is SLC intf <SLC-1> <SLC-2R> ."
Thank you for catching this. We have updated the lines you reference to now say: "Interferograms were generated from single-look complex SLC products with a time separation of 6 minutes, though sometimes longer if acquisition was interrupted (for example images, see Fig. A4). Interferograms were multi-looked by 15 samples in the range direction to reduce noise."

And we changed the caption of Figure A4 to: "Pair of multi-look intensity (MLI) radar images from ROI viewpoint (left and center) generated from original single-look complex (SLC) images multi-looked by 15 samples in range and multi-looked interferogram generated from the SLC images (right)."

The noise correction with interpolation from bedrock looks interesting, but how robust is it? Atmospheric disturbances are often blob-like and not linear with distance, so it is not immediately clear how useful the method is to reduce noise. It would be interesting to elaborate somewhat more in this.
Indeed, the atmospheric noise is often "blob-like", we see this in the data from Mount Rainier. We spent time looking at the atmospheric noise characteristics (which could be a study on its own), and determined that, qualitatively, the "blobs" are usually larger in scale than the width of Nisqually glacier (~500-900 m across). Bedrock points on either side are typically at distances smaller than the scale of the "blobs" and so the geometry is well-suited for our noise removal method. The geometry isn't quite as favorable for all of the Emmons glacier (~700-2100 m wide), but still acceptable, due to ridges of exposed rock in the middle of the upper Emmons. Our results are quite robust, the velocities of the median stack for each sampling period were very similar whether or not we applied the atmospheric noise correction. The main improvement of the correction was to significantly reduce the uncertainties (reflected as the median confidence interval width - Table 2) and reduce the noise over regions with slow velocities.

We addressed this comment by adding the following to the description of the atmospheric noise correction methods: "Even though atmospheric noise is not necessarily linear with distance, the scale of the atmospheric noise features we observed in the data were typically much wider than the width of the glaciers so we expect the method we use does a reasonable job of approximating the atmospheric noise directly over the glaciers."

And we also added a few sentences to the first paragraph of the Results section, which now reads: "Stacking alone was very effective; the velocities of the mean and median stacks with and without the atmospheric noise correction were very similar. The main benefit of the extra step of using stable rock points to subtract an estimate of the atmospheric noise was to significantly reduce the uncertainties and to reduce the noise where velocities are slow. The uncertainties before and after atmospheric correction are compared on Table 2."

The section 5.3 (p 4084) on flow modeling should be split, with the introductory part moved into the "Methods" section, and the results in the "Results" section. Here one would expect only the discussion of the model results.
We made this change.

The authors use a SIA model which is not well suited for the problem at hand (steep geometry). The authors are fully aware of the problem and even cite three papers using better methods, but do not rely on them at all. Full models in glaciology have been used since the 1980s (e.g. Iken, Echelmeyer, Gudmundsson etc) and have become very easy to use nowadays. Writing this section which sounds like an excuse probably has taken longer than just installing Elmer and modifying one of their examples for the investigated glacier (not that I am advocating a specific code here).
We could have used Elmer here, but we did not feel it was appropriate to use a more complex, full 3D model. The uncertainty in ice thickness would be problematic regardless of model complexity, so we decided to employ a simple model. Furthermore, this is not a modeling paper, it is an observational paper and we invoke the modeling only to aid in interpretion of observed results.

We modified the explanation here to sound less like an excuse, and added uncertainty estimates of sliding percent by assuming a wide range of uncertainty in the thickness and ice softness estimates that go into the deformation model (+-25% thickness and 2x ice softness). Even with these large uncertainties, the deformation for Nisqually still contributes <10% - deformation was so much smaller than the observed velocities in most places that even doubling or tripling deformation didn't change the median percentages much. The possible range for Emmons is much wider than for Nisqually - when we account for the range of uncertainties in the inputs, we get sliding contributions of 60 to 97%.

The implementation of sliding seems cumbersome. Since nothing is known about the process anyway, why formulate it like Equation (B3), and not just formulate it as ub = C τb (1) with a spatially and temporally varying slipperiness C? This would also alleviate the problem with negative Neff which are probably not as unphysical as the authors think, especially given the serious limitation of the code (no surface evolution, no full stresses).
We agree with the reviewer's suggestion here, but this is no longer an issue because we have decided to remove the sliding model from the paper. The fit to the data was poor and the model perhaps too simple, for many of the reasons discussed both in our text and by the reviewers, and as a result it did not add much scientifically.

The discussion of velocity changes (p 4087, l 20ff) is oversimplistic. It seems to be based on the assumption that Neff is somehow directly related to meltwater supply, and that basal motion is somehow directly controlled by Neff. There are some hardbed sliding theories where these assumptions might hold true, but given that the glaciers reside on a volcano it is likely that their beds consist of sediment, which has a very different rheology and dynamics. With the given data it is impossible to discern between different sliding regimes, but papers like e.g. Clarke (1987) and Clarke (2005) give an idea of the complexity and nonlinearity of possible processes.

We removed the sliding model so this paragraph is no longer in the paper.

4084, 3 The model is "planestrain", not "planview".
We made this change.

4084, 9 Ice thickness and bedrock topography are basically the same (if the surface is known).
We deleted "bedrock topography."

4090, 2 A better reference for the SIA would be Hutter (1983) or Greve and Blatter (2009).
This suggestion may be due to the reviewer having more familiarity with European authors, but we added Greve and Blatter (2009) and keep the citation for Cuffey and Paterson.

4090, 2ff In the formulation of the problem it is very important to consistently specify the coordinate system. Is z pointing vertically up, or perpendicular to mean slope? According to Equation (B1) it is the latter (given the sin term), but then H has to be measured accordingly (i.e. not vertically).
We added "The coordinate system is vertically aligned"

4090,3 In glaciology only the Stokes equations are usually considered, since all acceleration and momentum advection terms are vanishingly small (as proven by scaling arguments).
We changed this to Stokes instead of Navier-Stokes.

4081, 10 It is very important to be clear about the coordinate system (is z vertical up, or perpendicular to mean slope). Depending on this the calculation of overburden stress and Ne_ is different.
It seems the reviewer is referring actually to 4091, 10? We addressed this in a previous response by stating that the coordinate system is vertical.

4090, 11 longitudinal stretching cannot be simulated with SIA, also not by smoothing surface topography.
As described in the manuscript, we follow the approach of Kamb and Echelmeyer (1986), which demonstrates that this is, in fact, possible.

Fig 11 the symbols are too small.
We increased the size of the symbols on the plot and the font size of the stake location labels. The stakes are too close together to increase their symbol size.

Fig A1 , A2, A3: What do we see here? I see mountains with some snow-covered areas. It would be very helpful to mark the glacier outlines with red lines.
Good suggestion, we added rough outlines and labels of each glacier.

Response to A Vieli 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.

This is a good point and we have taken your suggestion and reran the deformation model for the maximum and minimum thicknesses and the maximum realistic ice softness parameter. Actually, even accounting for the maximum uncertainty of +-25% thickness and an ice softness parameter 2x higher, the sliding percentage for Nisqually glacier is still above 90% because the sliding is so much greater than the deformation in most places that even a several-fold increase in deformation doesn't change the percentages much. When we perform a similar test for Emmons glacier, however, the sliding contribution can be as low as 60%, so this was a valuable addition to the paper.

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.
See response to previous comment.

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).
This is lumped in the uncertainty of thickness uncertainty of 25%. Given the uncertainties involved with the bed and deformation model, we feel that using the 2008 surface is appropriate. We added a clarifying sentence.

4. The approach to calculate velocity fields for ice deformation (using the shallow-iceapproximation) 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.
As described in responses to reviewer #1, the sliding model was removed and we estimated uncertainties on the deformation model.

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: : :).

We estimated uncertainties for sliding % and report those in the abstract as well as elsewhere in the paper, in addition to our best estimates. The updated sentence in the abstract reads: ". Simple 2D ice flow modeling using TRI velocities suggests that sliding accounts for 91% and 99% of the July velocity field for the Emmons and Nisqually glaciers with possible ranges of 60 - 97% and 93 - 99.5%, respectively, considering ice thickness and ice softness uncertainties."

p. 4068 line 25: this is a very general statement but the references refer to the very specific glaciers of this study.
Yes, but they also happened to be studies with point sparse measurements, so they are used as examples here.

p. 4069 line 16: rather a remark: excuse me my ignorance 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 'beststudied' is another league.
These glaciers have a very long history of continuous and on-going study (led now by the National Park Service), and are very well-studied compared to most glaciers, but we tone this statement down a little since this isn't an important point and we don't want it to distract. It now says "Though Rainier's glaciers are among the best-studied alpine glaciers in the U.S.…."

p. 4073, 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.
We changed this to: "The interval between acquisitions can be as short as ~1 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.
We mean under the instrument, not on the glaciers, as implied by the context of the previous sentence, but we clarified this point anyway.

p. 4075, line 5-6: I do not quite follow this what 'interpolated result' is meant here
This is explained in the previous sentence ("we interpolated apparent displacement values over static control surfaces…"), but we do agree that the sentence wording here is a little confusing so we clarified this in the text.

p. 4075, line 7-8: maybe this stacking needs to be explained a bit further, for non-TRI experts this is maybe not clear.
Stacking is a pretty standard concept in many fields (e.g., seismology), but for additional clarification, we added "To stack, we take all the images for a given time period and compute the mean or median at each pixel, this has the effect of augmenting signal and canceling out noise. The median is less affected by outliers and is our preferred result."

p. 4076, line 22: specify here from when DEM is: ': : :an existing DEM from 2008 to…
We specified the 2007/2008 DEM.

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.
We added a summary table, Table 4, and changed this section to say "In general, the velocity magnitudes are similar, with the overall mean of the Walkup et al. (2013) measurements slightly higher on average but often falling between the 7 July and 2 November GPRI magnitudes, as would be expected of a mean

velocity spanning approximately the same period. The velocity directions are also relatively consistent, with a median difference of 12°."

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.
We added the slight qualifier to this sentence.

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 change
This is an interesting note, and is certainly consistent with our observations. Since we do not have thickness change data, we would prefer to avoid speculation about flux variations. We will keep this point in mind as we pursue future studies of simultaneous velocity and elevation change data.

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.
We agree.

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.
This change was made.

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).
We made this change.

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.
The sliding model was removed.

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.
As explained in responses to earlier comments, we now provide a possible range of sliding % based on the uncertainties in ice thickness and ice softness.

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

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.
We added the word likely, however, it is hard to come up with other explanations for such a large seasonal change in velocity.

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).
The sliding model was removed.

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!!!).
This is already reflected in the existing text at the end of section 5.5.

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).
We now take into account uncertainties when computing the sliding percentage as explained in response to earlier comments.

p. 4091: lines 12 : : :: an assessment of uncertainties in A on U_deformation would be useful: : :
We now consider that A can be up to twice as high and use this to estimate deformation uncertainties.

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.
The arrows are the only arrows on the plot so we didn’t think changing the color was necessary but removed the word “green”.

Fig. 3: caption: change to ‘: : : slope-parallel TRI velocity for: : :’
Added “derived from TRI”

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

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?
We feel that this would make the plot too cluttered. Uncertainties are clearly shown on Fig. 5 and also summarized for the each study period on Figure 2. We added this note to the caption. Good catch on the incorrect reference figure for the profile line. This was fixed.