## Review: tc-2015-109 Allstadt & al

Dear colleagues,

The manuscript "Observations of seasonal and diurnal glacier velocities at Mount Rainier, Washington using terrestrial radar interferometry" by Allstadt et al. presents TRI measurements with long baseline from three campaigns on several glaciers on Mount Rainier. The manuscript is well-written, and presents some interesting results. I recommend publication after some points, detailed below, have been addressed.

Sincerely,

Martin Lüthi

### General 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

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SLC_intf <SLC-1> <SLC-2R> ...
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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.

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.

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).

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

$$u_b = C\,\tau_b \tag{1}$$

with a spatially and temporally varying slipperiness $C$? This would also alleviate the problem with negative $N_{\text{eff}}$ which are probably not as unphysical as the authors think, especially given the serious limitation of the code (no surface evolution, no full stresses).

The discussion of velocity changes (p 4087, l 20ff) is over-simplistic. It seems to be based on the assumption that $N_{\text{eff}}$ is somehow directly related to meltwater supply, and that basal motion is somehow directly controlled by $N_{\text{eff}}$. There are some hard-bed 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.

**Specific comments**

4084, 3 The model is “plane-strain”, not “plan-view”.

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

4084, 15 models: use singular?

4090, 2 A better reference for the SIA would be Hutter (1983) or Greve and Blatter (2009).

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).

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).

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 $N_{\text{eff}}$ is different.

4090, 11 longitudinal stretching cannot be simulated with SIA, also not by smoothing surface topography.

Fig 11 the symbols are too small.

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.

## References

Clarke, G. C. K. (2005). Subglacial processes. *Annual Review of Earth and Planetary Sciences*, 33:247–276.

Clarke, G. K. C. (1987). Subglacial till: a physical framework for its properties and processes. *Journal of Geophysical Research*, 92(B9):9023–9036.

Greve, R. and Blatter, H. (2009). *Dynamics of Ice Sheets and Glaciers*. Springer-Verlag Heidelberg. ISBN 978-3-642-03414-5.

Hutter, K. (1983). *Theoretical glaciology; material science of ice and the mechanics of glaciers and ice sheets*. D. Reidel Publishing Company/Tokyo, Terra Scientific Publishing Company.