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Interactive comment on "A note on the water budget of temperate glaciers" *by* J. Oerlemans

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1 General Comments

In 'A note on the water budget of temperate glaciers,' the author considers the calculation of meltwater in temperate glaciers from the perspective of mass and energy conservation. He discusses the necessary amount of meltwater generation simply resulting from the flux of precipitation through a glacier under the assumption of steady state conditions, and then proceeds to discuss meltwater generation resulting from changes in geometry. This latter point is interpreted in the context of glacier geometry changes associated with surges. The paper finds that meltwater production by the 'extra' dissipation of potential energy during a surge is an order of magnitude greater than the steady state rate of meltwater production. The contribution due to surface

melt infiltrating the intra-glacier hydrologic system is at least as great as either dissipative source. The above conclusions are supported by examples from a few temperate glaciers in various locations.

The paper is particularly admirable in its capacity for circumventing the devilish complexity associated with characterizing the intra- and subglacial drainage systems by utilizing a spatially averaged approach. When we try to explicitly incorporate spatial dependence into our treatment of hydrologic routing, we often lose the ability to predict basic quantities like average water volume and discharge. This method provides a clear method for estimating these quantities, under a limited set of assumptions. Also, I think that using conservation of mass and energy as building blocks for drawing more nuanced conclusions is both intuitive and unassailable in its correctness.

By the same token, the use of wholly averaged quantities presents fundamental limitations on the types of conclusions that can be drawn (as the author specifically mentions). Namely, the procedures outlined in this paper do little to inform water routing in a glacier. Because of this lack of specificity, it is important to note that the discussion of the role of increased meltwater as a feedback mechanism in a surge or as a driver in basal sliding processes is mostly speculative, particularly given that the relationship between added meltwater and ice speed-up is not yet well-constrained. Perhaps one of the best uses for this type of procedure in the future will be as a check on more spatially explicit models of glacial hydrology; do the results of these models correspond well to the constraints set forth by conservation of energy?

From a stylistic perspective, I found that the basic algebra steps involved in deriving the relevant equations were overdone. I think that for a published work, it is sufficient to state which equations are being substituted, and to simply state the result that is most pertinent to the remainder of the paper. More precisely, three lines of equations containing five equal signs, all communicating the same mathematical relationship is superflous, and the same is true for derivations throughout the paper.

2 Specific Comments

- P. 2681, L. 25: It might be worthwhile to note that while most glaciers in the world are likely temperate (ennumeratively), most glacial ice (volumetrically) is cold.
- P. 2682, L. 6: I do not know what 'ten times easier' means, although I do agree that ignoring the strain rate tensor makes this analysis simpler.
- P. 2682, L. 12: The paper just stated that the approach is not valid for glaciers exhibiting high levels of sublimation. Thus, the statement 'generally valid' is false.
- P. 2682, L. 16: How does this theory apply to glaciers that are not in steady state, but are also not surging? This is an important class of glaciers that is not addressed by this theory.
- P. 2683, L. 8: Many readers might find the volumetric interpretation of the storage equation to be more intuitive, which is to say that $m^3 s^{-1}$ might be more appropriate than kgs^{-1}.
- P. 2683, L. 13: If the basic idea is to see if meltwater production can be calculated from energy considerations, some effort should be made to determine if the method actually calculates realistic meltwater generation rates, e.g. comparison with the discharge of water at the terminus. If this kind of analysis is not possible, validation of the theory should at least be addressed.
- P. 2685, L. 12: The statement that most kinetic energy associated with running water is dissipated could use some clarification (although I believe it to be true). Same for the neglect of kinetic energy of water near the terminus.
- P. 2686, L. 4: The term h_l is undefined. If it were defined like b_L is defined, then would it be surface height at the terminus?

- P. 2686, L. 6: Precipitation is not constant, and is not considered constant in the analysis, so including this extra bit of math is not necessary.
- P. 2686, L. 13: I believe that this means that mean ice thickness is now stated as a function of slope. If this interpretation is correct, please clarify this in the text.
- P. 2687, L. 4: Is there any guarantee then that the chosen values of L are reasonably realistic ones for a given precipitation rate?
- P. 2687, L. 10: The inconsistency in γ (lapse rate) is difficult to manage. Why not use just one set of units for it, particularly the one that yields congruence with the units of $\dot{p}$.
- P. 2688, L. 3: It isn't clear how and if this procedure would differ from the results presented in Table 1. As long as the average quantities are the same, would rough estimates of geometric characteristics not yield the same results as whatever procedure this paragraph suggests?
- P. 2688, L. 15: 'Continentality of the climatic setting' can be changed to 'climate'.
- P. 2688, L. 25: Yes, but see Harper (2003, J. Glac.) for an example of when this 'more water, more sliding' paradigm doesn't seem to hold.
- P. 2691, L. 21: Other possibilities exist, see above comment. Without an understanding of the explicit distribution of this meltwater generation, it is really difficult to rule out possibilities like the reconfiguration of subglacial drainage networks, or even the movement of extra water into storage.
- P. 2692, L. 13: This may be obvious to some, but I think that an important and interesting result here is also that the 'normal' dissipative meltwater production is insignificant compared to the influx of surface water.

- P. 2693, L. 8: This is already being done to some extent. Expressing the loss of potential energy to viscous and frictional dissipation is the core of variational methods in ice sheet modelling (see Dukowicz, 2010, J. Glac. and Schoof, 2006, Journal of Fluid Mechanics.). Simultaneously, spatially distributed meltwater production due to excess strain and frictional heating is standard procedure in numerical modelling, and is handled particularly elegantly by enthalpy methods (e.g. Aschwanden 2012, JGR). The calculation of the source terms is not an issue; routing is the difficult part.

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