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The Cryosphere Discussions

Interactive comment on "Evidence of meltwater retention within the Greenland ice sheet" *by* A. K. Rennermalm et al.

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This paper touches on a major difficulty in our current understanding of ice sheet surface mass balance: the fact that actual runoff is almost never directly measured. Losses by climatically-forced surface mass balance (i.e. melt) are almost invariably estimated by measuring (or more commonly, modeling) surface ablation rates and then modeling (or assuming) that some fraction, or all, of the surface melt actually escapes from the system. In this study the authors model surface melt rates by the usual methods, but are also able to measure the stream flux exiting the system – and find that the two quantities are nowhere close to agreement, not just for one season, but cumulatively, for 3 years running. The authors consider various possible means of tucking away that much water, but nothing obvious jumps out.

Like Mauri Pelto, who has also posted a comment, I find myself mostly wondering whether the numbers are right rather than where the water might be hiding. The first thing I checked was the treatment of infiltration and refreezing – but since the study catchment is entirely below the ELA (well below), there's no multi-year storage capacity for refreezing (i.e. the excess runoff can't be frozen in firn for long since we're in the ablation zone). However, as Mauri points out, below the ELA, firn that becomes superimposed ice (water refrozen at the ablation surface) and infiltration ice (water refrozen in firn above the ablation surface) gets melted twice before running off, so if that isn't accounted for in the modeling, the energy expenditure on surface melt will indicate a lot more water leaving the system than actually occurs. If the authors account for this, they don't say so; they simply refer us to Greull and Konzelmann (1994) for details on percolation and refreezing. Gruell and Konzelmann's model may actually handle superimposed ice (they discuss it rather obliquely), but the real question is whether and how it's handled here. This may or may not be a problem, but if superimposed ice were not accounted for in the modeling of runoff, it would create a discrepancy that acts in this direction.

There's one other possible problem that I'd like to see the authors discuss: how well do they know the catchment area? It's obvious that an error in the catchment size would produce an error in the modeled runoff, but what's surprising is that a fairly small error in the catchment has a rather large effect. If the 64.2 km2 catchment discussed here were approximated as a circular area, it would have a radius of 4.5 km; reducing the catchment area to 51.4 km2 makes the runoff R_W (assumed constant over area) equal to the discharge QAK4, and the corresponding radius drops only to 4.0 km. In other words, an uncertainty in the position of the catchment boundary roughly equal to or less than one ice thickness can explain the difference between modeled runoff and measured discharge. That's probably a pretty reasonable uncertainty, given that the ice surface topography, while being the dominant determinant of the sub/englacial catchment boundary, isn't the sole determinant - the bed does play a role.

Again, this may or may not be problem, and if it is the problem, there's perhaps not a lot the authors can do about it – it may simply be the case that you can't hope to make runoff and discharge agree on a such a small catchment where the potential uncertainty in area is such a large percentage of the total area.

Interactive comment on The Cryosphere Discuss., 6, 3369, 2012.