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The Cryosphere Discussions

Interactive comment on "Glacier volume estimation as an ill-posed boundary value problem" *by* D. B. Bahr et al.

M. Truffer (Referee)

truffer@gi.alaska.edu

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This paper treats the determination of ice thickness from observables at the surface as an ill-posed problem. It then applies lessons about spatial resolution learned in earlier work (Bahr et al., 1994) to derive limits on what spatial scales make sense for ice thickness resolution. They treat volume-area scaling as the extreme case of low resolution (only the mean ice thickness is derived) and conclude that this is a 'safer' method of calculating ice volume then higher resolution methods, such as those of Farinotti et al (J. Glac., 2009) or, more recently, of McNabb et al. (J. Glac., 2012). There are several fundamental issues with the reasoning in this paper. First, ill-posedness is not some-

thing that should just be assigned to a physical system, such as a glacier. Instabilities associated with inverting surface velocities for basal motion (as discussed in Bahr et al., 1994) do not automatically apply to other problems. Ill-posedness has a mathematical definition that applies to an equation (or system of equations) and that goes back to Hadamard. It involves existence, uniqueness, and stability of a solution. Of particular interest in the geophysical context is stability, which assesses the sensitivity to small changes in input. In that sense, volume area scaling is clearly not an ill-posed problem: For each area there exists a unique volume, and small changes in area result in small changes in volume. The paper does not show that other methods of finding ice thickness are ill-posed. My expectation is that methods based on integrating the continuity equation are not. This is because, loosely speaking, integration is stable, and derivation is not. One can think of taking a derivative as a simple ill-posed problem. Ultimately that's what makes velocity inversions ill-posed. But ice thickness determination will not suffer from having to take two derivatives. There are other issues with methods that integrate in some way, and generally they involve the effects of biases or systematic errors. If one integrates a surface mass balance field, random errors will behave nicely, but biases will lead to systematic errors that could be large. Finally, volume-area scaling has other issues that are not addressed here and that have nothing to do with stability of the solution. First, the determination of the scaling 'constants' is based on a relatively small sample of glaciers and has to be extrapolated to a large population. Second, fundamentally, area volume scaling assumes a unique relationship between the two variables, which is bound to lead to some errors in rapidly changing systems.

My recommendation is that this paper should be rejected in its present form. If the authors wish to make an argument about ill-posedness, they should examine the equations that are being solved and make the argument mathematically.

Martin Truffer

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