Response to review by Martin Truffer

We want to thank Martin for his thoughtful review. While we obviously disagree with his conclusion, we are grateful for the opportunity to clarify some important points and to address concerns that might have been raised by other readers as well. Given the strong conclusion drawn by the review, we feel it is important to address each of Martin's points in both broad terms and in significant detail. We apologize for the length of our response relative to the length of the review.

The essence of our response is that (a) we are well aware of the mathematical underpinnings of ill-posed problems, (b) volume-area scaling is ill-posed (non-unique solutions but stable), and (c) Martin's primary reasoning that the problem can't be ill-posed is misleading; he notes that "small changes in area result in small changes in volume", so it is stable. In fact, this example makes precisely our point. Volume-area scaling is stable because it is a long-spatial-wavelength solution to an ill-posed problem.

We address each point in the order that it is presented in Martin's review. Point (5) addresses the heart of the discussion.

(1) *"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)."*

This statement in the review is only intended as a summary of our work, but we disagree with the assessment or at least with the choice of words. We would not conclude that volume-area scaling is "safer," because this has an unfortunate connotation that scaling might be better. We actually believe that both scaling and numerical inversions are equally valuable. The methods of Faronotti et al, McNabb et al, Clarke et al, and others are incredibly promising and invaluable additions to the literature. The ability to model all of the world's glaciers simultaneously is going to open up some very interesting possibilities.

So instead we would only conclude that numerical inversions need to exercise special caution regarding short-spatial-wavelength results. As noted in our manuscript, each of the referenced numerical models already filters or manages these short wavelengths by some technique. We wish only to note that volume-area scaling is on an equal footing in this respect and that it automatically eliminates short wavelength instabilities. Do we wish that numerical models would address the potential short wavelength instabilities more carefully? Yes, in many instances. But we have also seen scaling applied poorly and with very questionable results. Neither scaling nor a numerical inversion is inherently safer and both can be abused. When used properly we are proponents of both approaches, and we would not want any reader to conclude otherwise.

We will certainly review the text of our paper and see where we can make this clearer.

(2) *"First, ill-posedness is not something that should just be assigned to a physical system, such as a glacier."*

We do understand this, of course. A quick review of Bahr et al (1994) will confirm our understanding of the mathematical underpinnings of ill-posed problems which hinge on the solution's behavior rather than on the physical system. In this manuscript we are very careful to associate ill-posed with the calculation and solution behavior. We note this many times.

For example, on page 5408, line 9,

> "*The theoretical implications are that information is lost exponentially as a calculation progresses deeper into a glacier and becomes increasingly unstable…*"

And on page 5408, line 11,

> "*…appropriate control of the calculation can damp (though not eliminate) the instability and can lead to reasonable and approximate (though non-unique and less precise) solutions*."

And on page 5408, line 23,

> "*As pointed out by Lliboutry (1987, p. 180), the best that we can do is say that a particular solution has been derived by a model or a calculation; we cannot claim that the sole solution has been derived*."

And on page 5411, line 23,

> "*numerical techniques have differing strategies for removing the unphysical short wavelength oscillations*."

All of these statements (and more) make it clear that we are talking about the calculation and solution and not the physical system. The last statement even notes that the oscillations are "unphysical." Page 5411, line 24 also refers to the oscillations as "unphysical."

We will review the text and see if we need to make this clearer, but this is not a flaw in our logic. It is only a potential flaw in our presentation.

Ironically, the following two sentences were removed from the final draft of our manuscript because we felt that the point was self-evident. (We will now consider adding these sentences back into the text.)

> "*Note, however, that the glaciers themselves are not physically unstable. Only the inversion calculation is inherently unstable.*"

(3) *"Instabilities associated with inverting surface velocities for basal motion (as discussed in Bahr et al., 1994) do not automatically apply to other problems."*

Yes, we agree. Bahr et al (1994) specifically discusses the instabilities associated with inverting stresses and velocities. Most of the literature cited in the manuscript also discusses instabilities associated with velocities and stresses.

However, much of the cited literature discusses the instabilities associated with inverting for the bed geometry. Knowledge of the bed geometry (inferred from surface data) is the same as inferring the ice thickness. For example, Gudmundsson and Raymond (2008) find that basal geometry perturbations can be resolved down to wavelengths of approximately one ice thickness, but not less. As noted in Zhdanov (2002), the form of these instabilities is typically exponential and is dependent on the spatial frequency, as is also found by many of the other references cited in the manuscript. We therefore use the frequency dependent and exponential form from Bahr et al (1994) as a reasonable starting point to discuss the errors and instabilities associated with calculations of thickness.

Also see point (7) below, where we illustrate the fundamental and monotonically increasing linear relationship between (unstable) stress and thickness. The well-established instabilities in stress are linearly transferred to instabilities in thickness. As noted in Bahr et al (1994, pg. 514, paragraph 5), the linear relationship means that the Lyapunov exponent is unchanged. In other words, the rate of information loss and instability is unchanged. Therefore, equation (1) of the manuscript is the proper exponential form of the thickness instability.

We will note the above points more explicitly in the text of our revised paper.

(4) *"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."*

We agree. This is an important point, because it is the entire system of equations that are relevant. In the case of a temperate glacier, that system is composed of the equations of motion (force balance), the constitutive equation, and the continuity equation. Each of these equations is important, and none of these can be neglected or discarded. Discarding any of these would mean that we are no longer modeling (or finding a solution for) the full behavior of a glacier. By discarding any equation, we would be ignoring some or all aspects of the velocities, stresses, geometry or other parameters. This will be relevant to our discussion at item (8) below.

As defined by Hadamard, a problem is ill-posed if the solution is (a) not unique, or (b) if the solution is not stable (continuously dependent on the data), or (c) if the solution does not exist. Importantly, we do not have to show that each of (a), (b), and (c) are true. We only need to show that any one (or more) of (a), (b), and (c) are true. Generally geophysicists focus on stability, but for our arguments we will focus on both stability and uniqueness. (For more information on the definition of ill-posed along with its history and application, we particularly like the discussion in Zhdanov (2002) which nicely summarizes well-posed and ill-posed geophysical inverse problems.)

With the above in mind, an unbalanced placement of boundary conditions is a notorious way to create unstable solutions and an ill-posed problem (e.g., Courant and Hilbert, 1966, pp. 227-30). Specifying all of the boundary conditions at the surface is the source of the calculation instability within the previously specified set of glaciological equations (Lliboutry, 1987). If we could instead specify velocity data at all of the ice surfaces (including the glacier bed), then there would be no calculation instability.

However, on rereading our text, we agree with some of the sentiment and spirit of Martin's review – we have been unclear about the meaning of ill-posed, particularly in paragraph 2 on page 5407. We have conflated ill-posed with unstable, when really we mean that unstable implies ill-posed. We have also conflated the lopsided boundary conditions with being ill-posed. What we really mean is that for the system of equations that describe a glacier (continuity, force balance, and constitutive), placing all of the boundary conditions at the surface creates an instability and therefore an ill-posed problem (e.g., Lliboutry, 1987). Furthermore, we have not discussed the issue of non-unique solutions; although non-uniqueness should be clear from the context of scaling theory (see discussion below), it should not have been glossed over in the context of this paper. However, these are problems with our prose and are not problems with our underlying logic. We will certainly rewrite paragraph 2 on page 5407, and we will very carefully comb the text to make sure that we correct any similarly misleading prose.

Note: Martin brings up the discussion of the mathematical definition to (in part) suggest that volume-area scaling is not ill-posed. We discuss this further in point (6) where we show that volume-area scaling is ill-posed because it does not give a unique solution.

(5) *"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."*

Actually, this makes our point precisely! Volume-area scaling is a long-spatial-wavelength solution to an ill-posed problem. Therefore, as demonstrated in our paper, it is a stable calculation. As noted by Martin, when using volume-area scaling we do not expect to see sensitivity of the volume to small changes in the area. (By sensitivity, we mean exponential divergence of the solutions as defined with a Lyapunov exponent – see Bahr et al, 1994. This is a common way to measure stability.) In other words, the problem is unstable, and volume-area scaling gives one way to control that instability.

As a counter argument to Martin's point, consider the similar scaling relationship between average glacier velocity and area. This velocity-area scaling relationship exists, as shown in Bahr (1997a). This relationship shows that a small change in area will result in small changes in velocity. However, we cannot claim that the apparent stability of this velocity-area scaling relationship implies that velocity calculations are not ill-posed. In fact, we already know that inverting for englacial velocities is definitely unstable and therefore ill-posed (see citations in manuscript). So why is the velocity-area scaling relationship stable? It's because velocity-area scaling is a long-spatial-wavelength solution. The unstable short wavelengths have been eliminated.

In other words, a calculation of the average glacier velocity (long spatial wavelengths and stable) is very different from a calculation of short-spatial-wavelength velocities (unstable). Similarly the average glacier thickness (and volume) is a stable calculation, but an inversion calculation of the thickness at every point in a glacier is a short-spatial-wavelength solution and is subject to instabilities.

As noted above, the stability of volume-area scaling does not rule out being ill-posed because the solution can still be non-unique; we discuss this in the next section.

(6) *"For each area there exists a unique volume, and small changes in area result in small changes in volume."*

We disagree with the assertion that for each glacier area there exists a unique volume. This is neither true in theory nor in reality. This is particularly important because non-unique solutions show by definition that the problem is ill-posed (see above). As correctly implied by Martin in his review, one of the most significant and common abuses of volume-area scaling is a misapplication of the scaling parameter "c" (see equation 4 of the manuscript). While "c" is often incorrectly treated as a constant, "c" actually has a distribution of possible values (page 5414, line 3). See for example, the distribution derived from data in the appendix of Bahr (1997a). Therefore, if we select a single glacier area, then scaling theory tells us that there is a distribution of possible values for the associated glacier volume (due to the distribution of "c"). The volume distribution has a well-defined mean, which indicates that the glacier volume can be reasonably approximated as the mean. But the volume is not uniquely determined for any given area.

Similarly, we can consider all of the world's glaciers that have a specified area within plus or minus some arbitrarily small tolerance. With great certainty, we know that these glaciers will not have the same volume because they are each on different basal topography in different local climates, etc. For this specified glacier area (plus or minus the tolerance), there will be a distribution of associated glacier volumes. Again, the distribution may be small and have a well-defined mean; but the calculated volume cannot be unique.

Incidentally, this non-uniqueness argument applies both to volume-area scaling and to any other method that inverts the area (without other data) for the volume.

Again, in the spirit of Martin's review, we believe that we can be clearer about this in our text. We already mention the distribution of values of "c" (page 5414, line 3), but we will now add a short discussion of the non-uniqueness of volume solutions to help clarify that the volume calculation is indeed ill-posed. However, for better or worse, geophysicists typically associate ill-posed with unstable solutions rather than non-unique solutions. Therefore, after explaining that volume-area scaling can be cast as an ill-posed problem, we will minimize our references to ill-posed. Instead we will simply note that scaling provides a stable alternative solution. This should help to avoid unnecessary confusion.

(7) *"The paper does not show that other methods of finding ice thickness are ill-posed."*

Actually, we do. In addition to the above discussion of non-uniqueness, see page 5408, line 14.

> "*For example, in an approach presented by Huss and Farinotti (2012), a numerical inversion uses surface topography to extract the basal shear stress. In turn, this allows an iterative solution for the glacier thickness. When the thickness is calculated at many locations, the glacier volume can be estimated. Therefore, the glacier volume solution depends entirely on ill-posed estimations of the basal shear stress derived from surface parameters and is thus non-unique.*"

In essence, we already know that the calculation of basal shear from surface parameters is unstable and ill-posed. Therefore, if a numerical model uses the inverted and unstable basal shear stress to calculate thickness, then there will also be instabilities associated with that calculated thickness.

More exactly, a dimensional analysis shows that the fundamental relationship between stress and thickness is linear and monotonically increasing. In particular, for all i and j, $\sigma_{ij} = k \rho g h$ for some dimensionless constant k. Therefore, at the most fundamental level, instabilities in stress will be transferred to thickness. In other words, the thickness calculation/solution will not be stable. (Note that in this context, σ_{ij} refers to full non-deviatoric stress, and that all stress components are, to first order, small deviations from the overburden ($\rho g h$). This first-order relationship to the overburden is most obvious when partitioning stresses into lithostatic and non-lithostatic components (e.g., Van der Veen and Whillans, 1989; Cuffey and Paterson, 2010, pg. 321).)

A dimensional analysis does not divide the variables' contributions into their particular roles in separate equations (e.g., their role in the continuity equation versus the constitutive equation versus the equations of motion). Instead, because the dimensional analysis has no a priori knowledge of the equations, it only considers each variable's holistic role with respect to all equations considered simultaneously. There is no confusion about the source of the equation or whether or not integrals and derivatives are involved – those details play no part in a dimensional analysis. Fundamental relationships between variables are revealed. Therefore, thickness and stress are fundamentally related in a linearly increasing manner, and the well-established instabilities in stress will be transferred to instabilities in thickness.

Incidentally, the vertically integrated force balance equations also show the linear relationship between thickness and stress. See for example equations (8.56) and (8.58) in Cuffey and Paterson (2010, pg. 325).

We will add this more detailed explanation to our paper.

(8) *"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."*

No matter how we massage the equations that describe a glacier, we can only find one out of many possible solutions, and we can never claim to have the sole solution. Integration may be more stable than a derivative, but integration cannot transform an ill-posed problem into a well-posed problem. For example, we could instead integrate the equations of motion (integrated force balance). This has been done in various ways (e.g., Van der Veen and Whillans, 1989), but it does not provide a unique or stable solution as proven elsewhere (e.g., Bahr et al, 1994). Integration, however, might hide the instabilities by finding a reasonable (though non-unique) solution.

Also, if we only use the continuity equation, then we are ignoring important glacier behaviors (as discussed above). It is important to include the *entire* system of equations (as we discuss in our response to item (4)). If an integration of the continuity equation did happen to provide a stable and unique thickness solution, then it's only because we neglected the equations of motion which we already know will produce calculation instabilities. Again, in that case, we could only claim that the integration of the continuity equation provided "a possible solution" and not "the unique solution."

(9) *"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."*

Yes, absolutely! We agree, and we say so twice in the manuscript. On page 5409, line 17:

> *"Furthermore, each method for estimating glacier volume will have additional sources of error that are separate from the ill-posed instability, such as errors in data, errors from numerical instabilities, and errors in specified parameters (like the scaling constant, or flow law exponent, or sliding law). The impact of these additional errors on the variance of the total volume must be evaluated separately from the ill-posed boundary value problem. However, unless special precautions are taken (as described in the following section), it is the exponentially large variance from the ill-posed instability that has the greatest potential to overwhelm an estimate of total GIC volume."*

And on page 5417, line 3:

> *"Other significant sources of error exist, but unless proper spatial filtering (or another equivalent error suppression technique) is applied, the ill-posed boundary value instability can grow exponentially and quickly swamp all other errors."*

In these statements we are both acknowledging these other potential sources of error and we are noting that their impact is unlikely to be as large as the exponential error associated with an unstable inversion.

Note that our statements about other sources of error are not restricted to scaling. In the context of our manuscript, these statements also apply to tunable parameters and other sources of error in numerical models. Common tunable parameters in numerical models include the rate factor in the constitutive relationship, the sliding law parameters, shape factors, etc. In other words, numerical models have many uncertainties as well. For example, in Huss and Farinotti (2012), equation (4) has four tunable parameters, equation (5) has three tunable parameters, equation (6) has one tunable parameter, etc. These are all potential sources of error, just as the scaling parameter "c" is certainly a source of error. However, we stand by our statement that these other sources of error (tunable parameters, inaccurate and imprecise data, etc.) are unlikely to produce errors as large as the exponentially huge instabilities associated with the inversion. For example, errors in scaled volume grow only linearly with errors in the scaling parameter "c", and this is wholly irrelevant compared to the exponential growth rates of errors in an inversion calculation. (For a discussion and comparison of growth rates, see Shaffer (2001, Chapter 3).) Therefore, one of our goals is to bring attention to the often neglected but large inversion errors.

As we have stated repeatedly, both approaches (scaling and numeric) have advantages and disadvantages. It's simply outside the scope and intent of this paper to discuss the pros and cons of other sources of errors in either the scaling or the numerical techniques.

(10) *"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."*

Yes, rapidly changing systems can lead to other errors. In that case, a dimensional analysis demonstrates that response time scaling is also necessary in addition to volume-area scaling (see for example the approach used by Marzeione et al, 2012). This discussion of other errors is important but outside the scope of this paper. And as we note in the manuscript, unless carefully controlled, the exponential errors associated with an unstable inversion calculation is likely to swamp all of these other considerations.

Again, volume-area scaling does not assume a unique relationship between the two variables. This is a common but incorrect assumption. See discussion above.

References:

Cuffey K. M. and Paterson W. S. B.: The Physics of Glaciers, 4th edition. Elsevier, Burlington, 693 pp., 2010.

Marzeion, B., Jarosch, A. H., and Hofer, M.: Past and future sea-level change from the surface mass balance of glaciers, The Cryosphere, 6, 1295–1322, doi:10.5194/tc-6-1295-2012, 2012.

Shaffer, C. A.: A Practical Introduction to Data Structures and Algorithm Analysis: Third Edition (C++ Version). Prentice Hall, Up Saddle River, 512 pp., 2010.

Van der Veen C. J. and Whillans, I. M.: Force budget: I. Theory and numerical methods, J. Glaciol., 35(119), 61-67, 1989.

All other references are in the original manuscript.