Response to Prof T.J. Hughes reply in The Cryosphere Discussions, 31 January 2017.

It is a tad disappointing that Prof. Hughes' reply to my note in The Cryosphere [Van der Veen, 2016] is so superficial and riddled with mistakes and apparent misconceptions about my more conventional approach. This suggests that the geometric force balance advocated by Prof. Hughes is, indeed, indefensible, as argued in my note. I will limit this response to the Section entitled "Problems with Van der Veen (2016)" – the following Section is a mere cut-and-paste from earlier work, and the "Concluding remarks" read like a lament on the half century of misunderstanding that Prof. Hughes apparently has suffered from reviewers and the scientific community. This may be of some interest from a historical perspective, but is irrelevant for the present discussion.

Prof. Hughes states that my equations (1), (2), (7) and (15) "can be confusing because they employ open parenthesis) instead of closed parentheses () so terms are not properly separated." I have read this rather perplexing statement multiple times and still fail to understand its meaning or implication. Perhaps Prof. Hughes should check his pdf-reader to ensure it reproduces equations correctly on the screen or in print.

Equation (1) in my note is simply the definition of stress deviators; Equation (2) is the essential equation of my rebuttal and expresses force balance along a flowband or flowline. For sake of further discussion, this equation reads

$$\tau_{dx} = \tau_{bx} - \frac{\partial}{\partial x}\left(H\tilde{R}_{xx}\right) - \frac{\partial}{\partial y}\left(H\tilde{R}_{xy}\right) \quad . \qquad (1)$$

For Prof. Hughes benefit let me reiterate the meaning of the various terms in this equation. The left-hand side represents the gravitational driving stress, responsible for making the glacier flow in the downslope direction. The three terms on the right-hand side represent drag at the glacier base, τ_{bx}, resistance associated with gradients in longitudinal stress, also called longitudinal stress gradients, $\frac{\partial}{\partial x}\left(H\tilde{R}_{xx}\right)$, and resistance associated with lateral drag, or friction originating at fjord walls or slower-moving ice, $\frac{\partial}{\partial y}\left(H\tilde{R}_{xy}\right)$. The tilde (~) denotes depth-averaged values of the resistive stresses, R_{xx} and R_{xy}. I am puzzled as to why this equation, which has been derived and applied many times before, can be confusing. But Prof. Hughes following comment makes it clear that he fails to understand this simple force-balance equation.

According to Prof. Hughes, my "Figure 2 compares apples and oranges, a longitudinal stress gradient with basal and side drag stresses and a gravitational stress. A stress is not the same as a stress gradient." Really? My Figure 2 shows the four force-balance terms as expressed by the above equation. Both the longitudinal stress gradient term, and the lateral drag term, are estimated from measured map-view *gradients* in the corresponding resistive stresses, R_{xx} and R_{xy}. So, contrary to what Prof. Hughes claims, my Figure 2 is correct and compared apples to apples (or oranges to oranges).

It might be instructive here to quote Turcotte and Schubert on the difference between Body Forces and Surface Forces [Turcotte and Schubert, 2002, p. 73].

"Body forces acts throughout the volume of the solid. The magnitude of the body force on an element is thus directly proportional to its volume or mass. An example is the downward force of gravity, that is, the weight of an element, which is the product of its mass and the acceleration of gravity g. [….] Surface forces act on the surface area bounding an element of volume. They arise from interatomic forces exerted by material on one side of the surface onto material on the opposite side. The magnitude of the surface force is directly proportional to the area of the surface on which it acts. It also depends on the orientation of the surface."

In Section 2-3, Turcotte and Schubert [2002] proceed to consider the surface forces acting on a small rectangular element with dimensions δx, δy, and δz. This, of course, is the same derivation as I have given previously [Van der Veen, 2013, Section 3.1], and leads to the general balance of forces in the x-direction:

$$\frac{\partial \sigma_{xx}}{\partial x} + \frac{\partial \sigma_{yx}}{\partial y} + \frac{\partial \sigma_{yz}}{\partial z} = 0 \quad . \qquad (2)$$

Similar equations hold for the other directions, along the y- and z-axes. This fundamental form of the equation describing balance of forces can be used to derive the force-balance equation (1). Most importantly for the present discussion, it demonstrates that the concept of balance of forces as applied to glaciers has only physical meaning when applied to finite volumes of ice. Flow of glaciers is driven by *gradients* in the gravitational lithostatic stress that can only be estimated over a certain horizontal distance. The terms opposing this stress (the terms on the right-hand side of equation (1)) correspondingly apply to the same horizontal distance. Also, note how in equation (2) the balance of forces is expressed in terms of stress gradients.

The considerations given above do not imply that at any location in the ice sheet balance of forces is not satisfied. Of course it is, but this balance becomes rather meaningless with the lithostatic stress at any location balanced by an equal but opposite lithostatic stress, as shown in Figure 4a in Van der Veen [2016] – no matter how many times Prof. Hughes repeats assertions to the contrary. At the calving front of a glacier, where the (depth-averaged) lithostatic stress in the ice has to be balanced by the (depth-averaged) pressure exerted by the sea water, the familiar boundary condition used in numerical models is found [Van der Veen, 2013, eqs. (9.69) and (9.70)].

It is always a good idea not to put words in someone else's mouth. Case in point is the statement by Prof. Hughes, where referring to my Figure 4a, "if it were true there would be no thinning of a flat ice shelf or at ice divides of an ice sheet because neither has a surface slope. Yet thinning of both occurs. For ice shelves the correct analytical solution was provided by Weertman (1957)." The reference to Weertman's solution for an ice shelf is somewhat surprising in this context because Weertman's Figure 1 clearly shows an ice shelf with zero surface slope, and Weertman explicitly states that "we now make the assumption that at a position far from the edge of the ice shelf all the stress components must be independent of the" two horizontal directions. As I have shown in the section on ice-shelf spreading [Van der Veen, 2013, sect. 4.5] this assumption of no surface slope or stresses independent of the flow direction, need not be made to arrive at the same solution as Weertman did.

More importantly is the contention by Prof. Hughes that no thinning will occur if the driving stress is zero, as on flat ice shelves or at ice divides. This need not be the case and I have never suggested such. An ice divide, or better, a flow divide, represents the vertical plane through which no transfer of mass takes place. Indeed, the local driving stress is zero and the horizontal velocity at that location is zero at all depths. There is still a vertical component of velocity and a vertical strain rate, however. Incompressibility requires the vertical strain rate to be balanced by a horizontal strain rate, or a horizontal velocity gradient. Consequently, there is local flow divergence, and for this condition to be in steady state, the surface mass balance has to be such that it compensates for this flow divergence. A similar line of argument can be applied to a flat ice shelf, in which the stretching rate, or velocity gradient, is constant in the flow direction.

As I stated at the beginning of this brief response, Prof. Hughes has failed to provide convincing or, for that matter, valid arguments to demonstrate that my derivation is wrong or seriously flawed. I have to conclude, therefore, that the concept of "pulling stress" as introduced and advocated by Prof. Hughes is nothing more than a red herring that, apparently, has obfuscated the thinking of Prof. Hughes. One must conclude that Weertman's "bones" proved correct after all.

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