Review of the paper by P. Bartelt et al., entitled "Avalanche Impact Pressures on Structures with Upstream Pile-Up/Accumulation Zones of Compacted Snow"

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1 General comment

The topic addressed by the authors is of utmost importance. The calculation of the impact force of avalanches on obstacles when a fluid-to-solid transition occurs, thus forming stagnant (quasi-static) zone upstream of obstacles and traveling jumps, is a challenging question. Although a number of significant advances were made in the recent years (most of them are available along my report below), there remains a lot to do because of the complicated physics which takes place during dense flow/obstacle interaction. The present paper proposes an approach based on a (simple) *"energy approach"*.

I read in detail the ideas developed by the authors. I must say that I have a number of major concerns about the theoretical part proposed by Perry Bartelt et al. The main reasons are (at least) the following:

The paper is –first of all– not free of misconceptions:

- the approach proposed starts with some equations that are pulled out of a hat (Eq. (3) for instance) or even wrong (Eq. (4)); this poses a serious problem because all the results presented in the rest of paper (virtual cases and practical case) depend on those confusing or flawed equations stated at the beginning of the description of the model.
- I also found a couple of misleading statements (see the section **specific comments** below).

Moreover, I must say that the present study does ignore a number of important works done before on the topic. I thus fully agree with the short comment earlier proposed by Peter Gauer on this weak point of the paper.

Unlike referees 1 and 2, I can not provide a positive feedback on the present study (see section **Recommendation** at the end of the present report).

I have taken time to write a rather detailed review in order to explain where the outcomes of the previous studies were relevant and could (not to say should) have been considered. I really invite the authors to read and consider the efforts made in the recent years by other researchers on the topic. In particular, they should demonstrate that their energy approach (ONCE CORRECTED) is superior to previous approaches mostly based on momentum conservation equation. At this stage, I did not go through the details of the examples/applications (section 4) because some equations used to draw the different plots and presented at the beginning of the paper are flawed.

2 Specific comments

- abstract: *"Existing methods to calculate snow avalanche impact pressures on rigid obstacles are based on the assumption of no upslope pile-up of snow behind the structure at impact."* This sentence shall help the author to promot their model but it must be removed because this is a wrong statement! There are methods—already published—that make effort to address carefully the problem and the authors should not ignore them: see the references cited along my review comments below.

- page 1, lines 18-19: why using this term "shape coefficient" for C_D? In fluid mechanics C_D is the "drag coefficient". Here, the EU handbook edited by Tomas Johannesson et al. (2009) would merit citation. In particular, table 12.1 (page 107) provides recommendations for the values of C_D.

- page 2, lines 3-5: it is well-established that when granular flows impact walls, the formation of dead zones upstream of the obstacle is a key process that we need to include into the impact force calculation in order to predict the correct impact forces. There are a number of published papers on the problem that would merit to be mentioned here: please see (Faug et al., Phys. Rev E 2009) for walls overtopped by a steady granular flows, see (Chanut et al., Phys. Rev. E 2010; Faug et al., Phys. Rev. E 2011) for walls overtopped by a transient granular flows, and more recently Albaba et al. (Phys. Rev. E 2018) for semi-infinite rigid walls (no overtopping). Moreover, it is also a well-established fact for snow avalanches/obstacle interaction problems that dead zones and shock-waves traveling upstream are important physical processes: see, and please cite, the EU Handbook 2009 (already mentioned above), and Faug et al. (Ann. Glaciol 2010). Also some other papers on the topic by B. Sovilla and co-workers, as well as by Shiva Pudasaini and co-workers would merit much more attention.

- page 2, line 3 again: note that "cohesive avalanches" is not a necessary condition. Dry (cohesionless) granular flows also produce dead zones at the impact with wide obstacles (see the literature mentioned above).

- figure 1: notation used here and all along the paper is weird... your drawing is nothing else than a traveling jump that is classically observed in water and granular flows (and snow avalanches) when those flows transition from a supercritical to a subcritical flow regime (for instance when those flows impact a wall). The difference between water flows and granular (or snow) flows is the fact that the granular (and snow) jumps may be compressible and accompanied by a shock in density, in addition to both velocity and height discontinuities. In general, in fluid mechanics the notation used is h_1 and h_2 for the heights before and after the jump, respectively (the same for the velocities and densities). I'm left with the (bad) impression that using another notation may allow you to promote your approach but the approach is not so original at the end. Again, please see Gray et al. (J. Fluid Mechanics 2003), Hakonardottir and Hogg (Phys of Fluids 2005), Gauer and Johannesson (Handbook 2009, Chapter 11), Faug (Phys Rev E 2015) and Albaba et al. (Phys. Rev E 2018).

- page 2, lines 19-20: *"Because we predict the speed of the compaction front, and therefore the loading duration as a function of the incoming avalanche velocity, the method facilitates the use of dynamic magnification factors in structural analysis."*. There already exist relevant models, based on the correct equations (the shock-wave equations: see another comment below, on your eq. (4) which looks to be wrong by the way), to predict the speed of the traveling shock-wave, as proposed ealier for snow avalanches (see Chapter 11 of the EU Handbook (2009)) or studied in detail for granular flows impacting walls by Faug (Phys. Rev. E 2015) and Albaba et al. (Phys. Rev. E 2018), or also proposed for landslides interacting with dams (Iversion J Geophys Res 2016).

- Eq. (3): this equation needs more explanation. Should reads:

$$\frac{\mathrm{dK}_\Phi}{\mathrm{dt}} = \frac{\mathrm{d}}{\mathrm{dt}}(M_\Phi V_\Phi^2) = M_\Phi V_\Phi \dot{V}_\Phi + \frac{1}{2}\dot{M}_\Phi V_\Phi^2 \tag{1}$$

Could you explain why the first term is neglected? You are dealing with time-varying incoming flow conditions ($\dot{V}_\Phi$ should vanish under steady-state conditions only).

-page 4, lines 5-6: *"The difference between... is a measure of cohesion"*... What do you mean? Without any cohesion (in a dry granular flow) you also have a difference (=a jump).

- Eq. (4) is wrong: this equation proposed by the authors does violate the mass conservation across the shock-wave. Let us use some more standard notation in fluid dynamics: $\mathbf{U}$ is the speed of the traveling wave, and f_1 and f_2 for any variable before the shock and after the shock, respectively, and $[[f]] = f_2 - f_1$ the difference between the enclosed function f on the forward and rearward sides of the singular shock surface. The correct equation in its depth-averaged form is:

$$[[\rho h(\mathbf{u} - \mathbf{U}) \cdot \mathbf{n}]] = 0 \tag{2}$$

This yields ($U < 0$, and $u_2 = 0$ in the dead zone against the wall):

$$U = -\frac{u_1}{X - 1}, \tag{3}$$

if we note $X = \frac{\rho_2 h_2}{\rho_1 h_1}$. Your Eq. (4) gives $U = -\dot{S}_\Phi = -\frac{u_1}{X}$, which does not give the correct jump condition.

- Eq. (6): why this 1/2 factor? This equation is false. Either it is a factor one and a difference in the brackets (sign $-$) or a factor 1/2 and a sum (sign $+$).

- from now on, I'm a bit worried because Eq.(3), (4) and (5) are wrong or pulled out of a hat... Several equations in the rest of the paper should be corrupted by the mistakes made... and the plots (virtual cases shown and practical application) may be false too.

- (see previous comment) in particular, Eq. (10) clearly produces a drag coefficient which is wrong and does not satisfy the shock-wave conditions (see for instance Albaba et al. Phys. Rev E 2018).

- figure 2, caption: this is weird to state $h_2 = h1$ while you have a jump (you are talking about pile-up; see also a comment by Referee 1). Note that the density ratios used should merit some discussion. This is not an easy question in practice.

- figure 3: are you sure that you get V_Φ (the incoming flow velocity upstream of the obstacle) at the two outgoing sections downstream of the obstacle? You should write mass conservation before infering such a statement.

- page 8, line 9: why this (very personal) comment into brackets? Please remove it. I am sure there are scientists who are much more optimistic and will find a way of measuring this one day!

- page 8, lines 11-12: I agree that more data on snow avalanches and interaction with obstacles should be measured, about dead zone dynamics (compaction, length and shape evolution). BUT, please, refer/not ignore the information already available from the literature about granular flow/wall interaction: see for instance the recent paper by Albaba et al. (Phys. Rev. E 2018) where the shock-theory is compared to the dead zone dynamics measured in very informative numerical simulations on dry granular flows. In want of any precise measurements with snow, this information is rich.

- pages 13, lines 26-30, Eq. (17): I do not understand this statement at all. It should be removed. If I use your Eq. (16) and your Eq. (14) (recalling the latter is false) and neglect the "tractive" force term (as you do along your example), I exactly get that Eq. (17). Again, please check Albaba et al. (Phys Rev E 2018). The model proposed based on shock wave theory (mass and momentum conservation equations) gives also this form but the terms C_D and A are not constant and (morever) coupled: such a model is correct and powerful.

- conclusions: if the authors are able to correct the flaws in their equations, they should explain why an energy approach is superior to already existing formula based on shock-wave theory relying on mass and momentum conservations.

3 Recommendation

For all the reasons given above, I conclude that the paper in its present form is far from being suitable for publication to NHESS. It seems to me that referees 1 and 2 both provided rather positive reports, however. Unlike both referees 1 and 2, I have no other choice but to provide a negative feedback on the current paper proposed by Perry Bartelt et al. That said, I would be happy to read in the future a revised paper if the authors can make an effort to :

- (i) correct the wrong equations,
- (ii) better explain their assumptions,
- and (iii) demonstrate that their energy approach is superior to existing methods (a cross-comparison would be needed and not only the plots from the energy approach by the authors) if points (i) and (ii) are carefully addressed, first.

With my best regards,
Thierry FAUG