Comments to Assessing fragility of a reinforced concrete element to snow avalanches using a non-linear dynamic mass-spring model by Favier et al.

In general the paper is much improved from the original version. I am thankful that the authors introduced in section 2 and in the conclusions more "snow avalanche dynamics".

Line 5, Page 2: "Hence, in order to find a compromise between simplified but time-efficient models and refined but time consuming models, RC structures can be described using Single-Degree-of-Freedom (SDOF) models (Biggs, 1964) where the structure is modeled by an equivalent mass and an equivalent spring. This approach has been largely used and validated in the field of structures subjected to blast loads (Ngo et al., 2007; Jones et al., 2009; Carta and Stochino, 2013), but has still to be used for snow avalanches or any other mass flow."

- ➔ I believe the traditional term for SDOF models is the Rayleigh's Method, at least in the United States. The goal is to reduce a complex structure to a single degree of freedom system. The method has been applied to snow avalanches, specifically snow avalanche blasts and forest destruction, see "Dynamic magnification factors for tree blow-down by posed snow avalanche air blasts" NHESS, Vol 18(3) by Bartelt et al. (2018).

Line 11, Page 2, "civil engineering abacuses" is not clear to me.

Line 15, "ignor" – ignore.

Line 15, The end of the this paragraph is not quite clear to me: is it really only "quasi-static"? Does not the "mass and spring" system mean that you account for the dynamic impulsive loads and therefore the method is "dynamic"? (I read on and you appear to solve the second order equation with Newmark's method – why to you insist to call the procedure "quasi-static").

Section 2 "Avalanche dynamics and *measured* pressure signal". There are some misspellings in this section (e.g. avalance). Please check spelling. I found this section interesting, simply because the authors try to overview the present state of knowledge on impact pressures. I would be a little more critical with the avalanche community – they have measurements but are unable to identify underlying mechanisms, especially in the real scale field measurements. This is why there is such a large variation in the measurements. I would stress two things: 1) Measurements in the runout zone are rare (!) and therefore much is based on back-calculations, which is extremely difficult and 2) There are cases where the standard V^2 formula work extremely well, cases where it doesn't. Basically, it is a mystery. The loading rate of 0.1 kPa/s appears to me to be way too low, especially for the initial hit. I would suggest that impact loading rates of 2000 kPa/s are more appropriate – but this is all speculation, and not the problem of the paper. I would modify the text to express the uncertainty of the measurements, and the difficulties of gaining information from case studies (only one sentence is needed).

Section 3.1. The opening of section 3.1 I find somewhat awkward. Why don't you keep the geometry open and undefined. "We consider a simply support wall with length L, width b and thickness h". The method you develop is completely general. In the examples you state L=8m, somewhere. Merge section 3.1.1 into section 3.1.2.