Dear Dr. Bair,

thank you very much for commenting and providing helpful suggestions on the manuscript. Below we have pasted your comments in blue, our point-by-point responses are given in black.

Line 10: "is" b.c. information is singular.

Thanks, for the correction.

Line 19: Not grammatically correct. Could be something like "The high frame rates allowed us to obtain better resolved time derivatives of velocity..."

We will change to: " The high frame rates enabled us to calculate time derivatives to obtain velocity and acceleration fields."

Line 29: citation?

We will cite: Pudasaini and Hutter (2007) (ISBN 978-3-540-32687-8) and Schweizer et al. (2021) (ISBN 978-0-12-817129-5).

Line 44: This definition isn't accurate as the weak layer is always in contact with the slab since, as the authors state in the discussion, the slab is never in free fall.

Thanks, for pointing out. We will rewrite to:" Quantities of particular interest during self-sustained crack propagation are the speed of the propagating crack, the touchdown distance, which is the length from the crack tip to the trailing point where the slab rests on the crushed weak layer, and the specific fracture energy of the weak layer (e.g., Schweizer et al., 2011; van Herwijnen et al., 2016b; van Herwijnen et al., 2010)"

Line 48: why not abbreviate as PST starting here?

We will follow your suggestion and abbreviate at this point.

Line 57: no space
Line 73: These high fracture energies are comparable to solid ice (0.3-2 J m^-2) as pointed out by Dave McClung in a review of van Herwijnen et al. (2016) and Reuter et al (2019). That suggests that either: 1) there's something wrong with the E and wf measurements or 2) there's quite a bit of dissipation of that energy.
Rosendahl and Weissgraeber (2020, p 126) have a nice discussion about this and suggest that the compressive fracture toughness (which can be related to the sp. fracture energy with E) for snow should be significantly higher than the tensile fracture toughness for ice because of dissipative processes involved in the crushing of the weak layer.
I don't expect the authors to provide a definitive answer, but more context on these specific fracture energies is needed.

We are aware that this point was raised previously. We completely agree with the arguments of Rosendahl and Weissgraeber (2020). Tensile failure and compressive failure are very different. That holds for strength of materials approaches (tensile strength and compressive strength are different properties) as well as for fracture mechanic approaches (specific fracture energy in tensile is not the same as specific fracture energy in compression). We therefore think that the remark, initially raised by Dave McClung, is comparing two different properties, and is therefore not adequate to claim the contradiction of too high specific fracture energies for snow. We will include a discussion of this "contradiction" in the revised manuscript.

Line 75: should be "derivation of". I suggest an English language service and won't make further grammatical corrections.

Thanks for the suggestion.

Line 123: define t as time. In seconds I assume?

We will define time (t) one line above

Line 170: Might want to mention that accounting for material properties of the weak layer is the major difference between RW model and the Heierli model, which assumes a slab in free fall.

We will mention the difference by writing:
"Their model [RW method] consists of a Timoshenko beam sitting on a weak layer represented by smeared springs, in contrast to the model of Heierli et al. (2008a), where the weak layer is rigid."

Line 223: The slab is resting on the weak layer prior to failure so it's always in contact with the weak layer and whether or not the weak layer is crushed is a qualitative description at best. And as the authors mention in the discussion, the slab is never in free fall.
What the authors are describing is the section of the slab that is experiencing (positive) slope normal movement.

We agree, "making contact again" is not correct. We will rewrite to:
"As the crack propagates through the PST beam, the slab subsides before it comes to rest on the crushed weak layer."

Line 223: Looks like there are some edge effects to address, i.e. velocity is highest closest the far edge of the beam.

Yes, the downward velocity of subsets which are close to the far end of the beam increases. We address this edge effects later in the discussion around line 395.

Figure 7: This key is not intuitive and make the melt freeze layer look very thick at first glance. Maybe just have the symbols with arrows pointing towards each layer?

Thanks for the suggestion. We think the many arrows will make the graph messy. Instead we will modify the graph by plotting the grain type legend more separated.

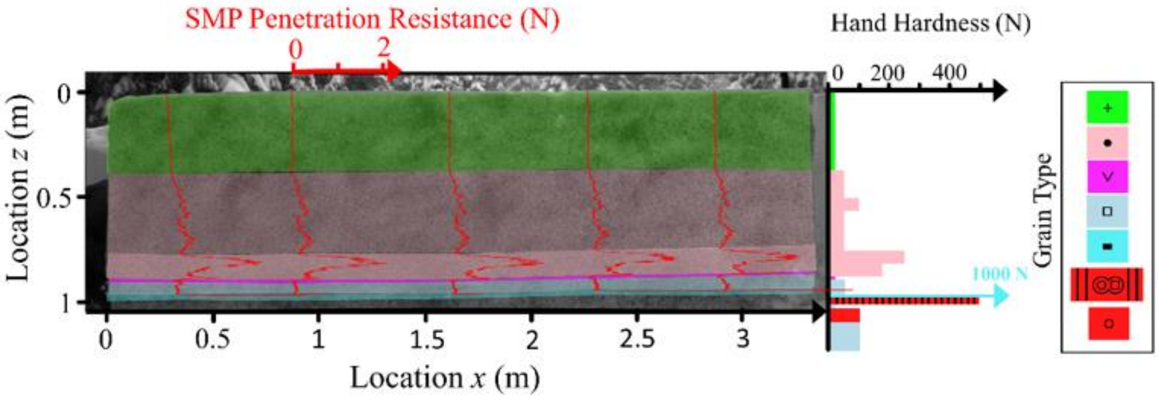

Table 1: Where is the slope angle? That's important.

As stated in line 91, all PSTs were performed in the flat. We will add this to the caption of Table 1.

Line 264: This is interesting. I think stress intensification from the far edge is playing a big role here and the secondary crack may be an artifact of that. Could the second crack going in the opposite direction be seen visually, or only using DIC? It's pertinent because there aren't many field PST observations (without high speed measurements) where this occurs.

Indeed, the far edge plays an important role. The secondary crack is initiated by the impact of the stronger negative acceleration the slab experiences at the far end of the beam when sitting down on the crushed weak layer. We did not observe the secondary weak layer cracking nor the slab fractures in the field or in the videos. In a revised version we will highlight this by writing:
“While in the field we classified PST3 as END, the displacement and strain data clearly show that the crack propagation dynamics were more intricate, and a combination of END, SF and ARR. This unexpected result was not recognized in the field.”

Line 348: 26 This is an overstatement. This is not the first time strain fields have been measured in snow at high speed & high resolution, e.g. Reweiger and Schweizer 2013.

We agree, to be more precise, and to distinguish from the work done by Reiweger and Schweizer 2013, we will rewrite to: *"For the first time, we were able to measure strain fields in PSTs, showing strain concentrations in the area of the weak layer (Figure 8) as well as in the slab in experiments with slab fractures."*

Line 355: Ah, this should be stated in the results.

We will also mention this in the Results section (see above).

Line 359: Again, difficult to rule out artifacts from edge effect

See reply on comment line 264. We also suggest that the secondary crack is caused by an edge effect. However, we think that further discussion about edge effects would distract the reader at this point. That’s why we refer to Appendix A.

Line 373: Not sure the grain scale measurements would be helpful as especially with something like large surface hoar crystals, you'd see many different fracture modes as the grain get blown apart during the collapsing and shearing of the slab.

We agree and already discuss this in lines 367-374.

Line 409: This whole idea of steady-state crack propagation may be a red herring. It's never observed in controlled experiments. And we know that spatial variability is the rule with snow in the mountains, which is why we see wild looking river markings on crown faces, even in new snow (e.g. Fig 4 in Bair et al 2016), suggesting cracks traveling at different speeds as they encounter snow with different properties.
Bair, E.H., Gaume, J. and van Herwijnen, A. (2016). The role of collapse in avalanche release: review and implications for practitioners and future research, Proceedings of the 2016 International Snow Science Workshop, Breckenridge, CO USA.

Our experiments come close to controlled experiments. Laterally very homogenous snowpack, flat field (cf. Fig. 7), and the beam length of PST #3 was larger than the touchdown distance. We therefore think that it is worth mentioning that a theoretically predicted steady-state for such conditions was not (yet) observed. That spatial variability most likely causes the crack speed to adapt along its path is thus not called into question.
We will somewhat reword this sentence. Instead of writing 'we were not able to observe', which suggests that we failed to observe something we assume is there, we will write 'we did not observe'.

Line 412: I believe these collapse wave speed measurements, but they are slower than speeds measured from real avalanches (Hamre et al. 2014), or slope scale simulations (Game et al, 2019). Thus some discussion about measuring collapse wave speeds from PSTs and how they relate to avalanches is warranted. I assume the PSTs were conducted on low angle slopes, but slope angle measurements are not provided in Table 1.
Gaume, J., van Herwijnen, A., Gast, T., Teran, J., & Jiang, C. (2019). Investigating the release and flow of snow avalanches at the slope-scale using a unified model based on the material point method. Cold Regions Science and Technology, 168, 102847. https://doi.org/https://doi.org/10.1016/ j.coldregions.2019.102847
Hamre, D., Simenhois, R., & Birkeland, K. (2014). Fracture speeds of triggered avalanches. Presented at the International Snow Science Workshop, Banff.

Indeed, our experiments were performed in the flat. We agree that different circumstances, e.g. slope angle > 30°, may lead to different crack propagation modes and therefore to much higher crack speeds and will address this in the Introduction section of the revised manuscript.

Line 420: And because of the edge effects, that maximum is probably greater than what you'd see in an avalanche or whumpf in the field with the same slab/weak layer.

We agree, but we do not think this assumption is relevant enough to be mentioned in this context.

Line 420: Is there a practical takeaway here? Are there implications for practitioners using PSTs?

The long touchdown lengths, in addition to edge effects at both beam ends, make it impossible to assess the propensity for self-sustained crack propagation with normal-sized PSTs. We will mention this in a revised version with:
“As a practical implication, our results show the need to rethink the predictive power of normal-sized PST experiments. That the measured touchdown distance is longer than a typical PST beam length once more emphasizes that normal-sized PSTs cannot be used to assess the propensity for self-sustained crack propagation.”

Line 494: Not compliant. Either make the videos freely available or explain why they are not see: https://www.the-cryosphere.net/policies/data_policy.html#data_availability

We will refer to WSL’s data repository www.envidat.ch in the revised manuscript and make the data available on acceptance of the manuscript.