Review: "How exposed is a mountain road to snow avalanches? A corridor-scale assessment in the southern Andes" Latorre, Schaefer and Oesterle

General comments

A central difficulty I have with the manuscript is that the overall modelling framework represents a **rather standard deterministic avalanche-dynamics workflow**, while many of the physically important processes that currently define the difficult questions in avalanche science **are either prescribed externally** or **omitted entirely**. The paper combines predefined release areas, return-period-based fracture depths, a depth-averaged dense-flow model, and an empirical friction calibration to generate corridor-scale exposure maps. Such an approach can certainly be useful operationally, particularly in a data-scarce region, **but in my view it does not constitute a methodological advance**. Rather, it is an application of existing open-source software to a new geographical setting, with several important limitations in the physical formulation.

The main issue is that the model chain bypasses most of the processes that connect meteorological loading, avalanche release, and subsequent flow dynamics. The release areas are prescribed, the fracture depths are imposed from ΔHS_3-based scenarios, the initial volume is computed from those assumptions, and this volume is then used to assign an effective friction class. The subsequent motion is simulated without snow entrainment and without an evolving thermodynamic or flow-regime description. Consequently, the **paper treats avalanche volume as a central explanatory variable** while neither the formation of that initial volume nor the evolution of avalanche mass and flow state are represented in a physically complete way.

The problem can therefore be summarized as a sequence of coupled omissions:

no storm-history representation → no physically evolving snow state → no entrainment → no flow-regime evolution → effective friction represented mainly by V_{release}

These aspects cannot be separated cleanly. If the model cannot represent the changing flow state caused by snow properties and entrainment, then the calibrated friction coefficient inevitably becomes an effective parameter absorbing processes that are missing from the model. A fitted value such as $\mu = 0.20$ therefore cannot straightforwardly be interpreted as an intrinsic friction coefficient characteristic of "large avalanches".

I see four major consequences of this formulation.

1. Calibration and the interpretation of Section 5.1

The calibration procedure and its interpretation in Section 5.1 require substantial revision. In the present form, the manuscript appears to infer a general volume-dependent friction relationship **from a very limited and partly circular calibration exercise**.

For the Campanario 2011 event, neither the release depth nor the release volume is independently constrained. Table 1 identifies both quantities as back-analysed, and Section 3.5 states that the authors vary both the Coulomb friction coefficient μ and the fracture depth, corresponding to release volumes of approximately $11{,}000–108{,}000\ \mathrm{m}^3$, until the observed deposition and runout are reproduced. The preferred solution of approximately μ =0.20 and $V \approx 65{,}000\ \mathrm{m}^3$ therefore does not represent an independent friction calibration at a known release volume. Release volume and friction are fitted simultaneously and may compensate for one another. This is a classical equifinality problem.

The later interpretation of the resulting pair (V, μ) as evidence for a volume-dependent friction relationship is therefore partly circular. The quantity used to explain the friction coefficient is itself one of the fitted quantities used to obtain that coefficient.

The comparison between Campanario and La Biblioteca does not resolve this problem. The two paths differ not only in release volume but also dramatically in path length, vertical drop, geometry, confinement, snow conditions, and likely flow regime. Campanario has a path length of roughly 2 km and approximately 1000 m of vertical drop, whereas La Biblioteca is only of order 100 m long. The available data therefore establish two path-specific effective friction values, but they do not demonstrate that the difference between $\mu \approx 0.20$ and $\mu \approx 0.31$ is caused by avalanche volume.

The Campanario 2022 validation event is particularly problematic for the proposed classification. Its release volume is estimated at approximately $9{,}100\ \mathrm{m}^3$, which places it below the manuscript's $10{,}000\ \mathrm{m}^3$ threshold and would therefore assign it to the $\mu = 0.31$ class. However, the authors explicitly report that the event is best reproduced by μ =0.18–0.22, essentially the same friction as the much larger Campanario 2011 event. This appears to contradict the proposed class boundary rather than validate it.

The comparison in Section 5.1 with previous Chilean studies is also over-interpreted. The authors correctly acknowledge that friction coefficients from SAMOS-AT, RAMMS, Aval-1D, and temperature-dependent RAMMS formulations are not directly transferable but then use numerical similarities among the respective μ values as support for the proposed volume dependence. At most this provides a qualitative plausibility check.

I am also not convinced by the comparison with the earlier Dirección de Vialidad assessment. Differences in predicted pressure are attributed to friction, density, release-depth estimation and calibration strategy, but these factors are not isolated in a controlled experiment. The two modelling chains differ simultaneously in release geometry, terrain representation, dynamics formulation, friction law and post-processing. It is therefore not possible to attribute a factor-of-several pressure difference to any particular factor or conclude that the previous design basis is systematically biased low. The argument is further weakened by the fact that the manuscript invokes the previous study's use of $\rho = 250\ \mathrm{kg\,m^{-3}}$ as a possible reason for lower pressures, although its own baseline simulations use the same value.

Section 5.1 should therefore be reformulated much more cautiously. The current results support two path-specific effective calibrations that are subsequently extrapolated to the corridor. They do not establish a validated volume–friction law.

2. Neglect of entrainment

The neglect of entrainment is, in my view, a major physical limitation rather than a secondary caveat.

For the Campanario 2011 event, the model reproduces the observed runout and road deposits by jointly adjusting the initial release volume and friction while keeping avalanche mass constant during motion. In a real long-path avalanche, however, the mass reaching the road may be substantially larger than the initially released slab mass due to erosion and snow entrainment. If entrainment occurred, the no-entrainment model necessarily compensates for this missing mass evolution through some combination of increased prescribed release volume and altered effective friction.

Consequently, the fitted release volume of approximately $65{,}000\ \mathrm{m}^3$ and the fitted $\mu \approx 0.20$ cannot be regarded as fully independent physical properties of the Campanario event. They may partly be effective parameters compensating for omitted entrainment.

This is particularly important because Campanario is the principal calibration anchor for the large-volume friction class. The model therefore potentially uses an entrainment-compensated release volume to define a volume-dependent friction parameterization and then extrapolates that relationship to the entire corridor.

The contrast with La Biblioteca is especially relevant. Campanario provides a long track over which substantial entrainment can occur, whereas La Biblioteca is extremely short. Thus, the apparent difference between $\mu \approx 0.20$and $\mu \approx 0.31$could be influenced not only by volume and path geometry but also by very different opportunities for mass entrainment.

McClung's field-based analysis explicitly cautions against equating initial slab mass with total avalanche mass for large events. He notes that entrainment can increase total mass very substantially, in extreme cases approaching an order of magnitude beyond the initial slab mass. This distinction is central here because the manuscript uses initial release volume as the controlling variable for friction.

There are in fact at least three different quantities:

$$V_{\text{release}},\ V_{\text{flow}}(x,t),\ V_{\text{deposit}}.$$

They are not interchangeable.

Yet the manuscript effectively uses

$$V_{\text{release}} \to \mu$$

while omitting the evolution of V_{flow}. This makes the physical interpretation of “avalanche volume” particularly problematic.

The sensitivity analysis does not resolve this issue. Perturbing μ and density while keeping release geometry and the no-entrainment assumption fixed quantifies numerical model sensitivity, but it does not explore what may be one of the dominant missing physical processes.

3. Release geometry, probable release areas, and the meaning of avalanche volume

A further concern is the treatment of avalanche release itself. The paper gives considerable importance to avalanche volume because volume determines the friction class, yet the release volume is constructed from predefined polygons and prescribed fracture depths rather than from a probabilistic or mechanically constrained release model.

The 208 release areas are inherited from the earlier engineering study and were delineated using expert geomorphological interpretation, slope criteria, wind loading, imagery and field observations. This is entirely reasonable for deterministic engineering scenarios. However, it means the analysis answers the question:

What happens if these predefined polygons release with the prescribed depth?

It does not answer:

What are the possible avalanche releases along this corridor, with what plausible dimensions and probabilities?

That distinction is important.

The McClung (2009) field data demonstrate that dry slab release geometry has a clear statistical structure. Slab thickness D, downslope length L, and width B show very large variability but approximately lognormal distributions, with characteristic median ratios of order

$$L/D \sim 100,\ B/D \sim 100,$$

and B/L of order unity for mixed confined and unconfined terrain.

This means that a specified fracture depth should not automatically be multiplied by an arbitrarily delineated full polygon and interpreted as a unique release volume. For a given D, there is a distribution of plausible release lengths and widths.

A more defensible large-scale framework would first identify probable or potential release terrain independently of the specific avalanches already known. One could then generate plausible release realizations according to a relationship such as

$$P(L, B, D, \rho, terrain)$$

leading to a distribution

$$P(V_{\mathrm{release}}).$$

This is particularly important for avalanche width. Width is not simply a scalar multiplying volume. A wide release can feed several gullies or avalanche tracks simultaneously, whereas a narrower release from the same general release terrain may remain confined to one branch. Thus, uncertainty in B can alter the spatial pattern of road exposure itself.

This is especially relevant to the paper's use of "sub-path" counts as an exposure metric. Where many sub-paths are assigned to a single broad catchment, it is not obvious whether these should be interpreted as statistically independent avalanche sources or as alternative branches of a larger possible release. The relationship between release width, subdivision of the source area, and multi-track flow therefore requires much more consideration.

The treatment of density introduces a related issue. McClung reports an empirical relationship between mean slab density and thickness,

$$\rho \approx 225 D^{0.24}\ \mathrm{kg\,m^{-3}},$$

with considerable scatter. Thus, fracture depth and density are not necessarily independent variables. Yet the manuscript prescribes $\rho_0 = 250\ \mathrm{kg\,m^{-3}}$for all corridor scenarios and later varies density independently between 100 and $400\ \mathrm{kg\,m^{-3}}$.

Again, this is a legitimate sensitivity test of the numerical model, but it is not the same thing as a physically consistent treatment of release uncertainty.

The manuscript therefore gives "release volume" much more physical certainty than is warranted. Since volume subsequently determines friction, this uncertainty propagates directly into the dynamics.

4. Storm history, snow state, and flow regime

The final and perhaps most general concern is the physical meaning of the Moderate, Severe, and Extreme snow-loading scenarios.

The manuscript follows the established practice of deriving fracture depth from the return period of three-day snow-height increase, ΔHS_3. I recognize that this is widely used in Swiss avalanche-hazard practice and provides a transparent engineering convention. **However, ΔHS_3 is not a physical description of the meteorological and snowpack history that produces a large avalanche.**

Natural avalanche cycles may develop over substantially longer periods. A ten-day storm period, for example, may consist of several precipitation pulses separated by periods of settlement, wind redistribution, temporary stabilization, temperature changes, and the formation and burial of weak interfaces. Failure may occur within storm snow or on a persistent weak layer beneath it.

Consequently, increasing fracture depth does not simply correspond to accumulating "more of the same snow".

A deeper slab associated with a prolonged storm may be denser, warmer and more strongly settled than a shallower recently deposited slab. It may contain several internal layers. Conversely, a deep cold slab over a persistent weak layer may retain very low temperatures and produce strong fragmentation and substantial entrainment. These two scenarios can have similar initial release volumes while generating very different avalanche behaviour.

This distinction is particularly important because snow state affects flow regime.

Cold, dry, low-density snow may fragment strongly, entrain additional snow and develop highly fluidized dense flow and a powder cloud. Warmer snow may behave more cohesively, while wet snow belongs to a fundamentally different rheological regime. **Flow resistance therefore cannot necessarily be parameterized by release volume alone.**

The calibration events themselves illustrate exactly this problem. Campanario 2011 is described as a wet full-depth avalanche. Campanario 2022 had a dense core accompanied by a substantial powder component. The La Biblioteca events were associated with pronounced warming. Yet all of these events are represented within the same dense-flow framework and are then used to infer a volume-based friction scaling.

It is therefore impossible to know whether the fitted variation in μ primarily reflects volume or whether it is absorbing differences in snow temperature, moisture, fragmentation, entrainment, flow regime and path characteristics.

The neglect of entrainment and the lack of a flow-regime description are therefore tightly connected. Entrainment does not merely increase mass. The avalanche incorporates snow having its own density, temperature, bonding and moisture state, thereby altering momentum, fragmentation, fluidization and deposition. If those processes are absent, the fitted basal friction necessarily becomes an effective compensation parameter.

The problem can therefore be represented more completely as

storm history → snow state → release geometry and mass → entrainment → flow regime → effective resistance

whereas the manuscript reduces much of this chain to

$$V_{\mathrm{release}} \rightarrow \mu.$$

I think this issue becomes particularly important for the Extreme scenario. A rare avalanche scenario should represent a physically coherent extreme event, not simply a larger fracture depth applied to an otherwise unchanged conceptual snowpack. If the extreme scenario corresponds to deeper, warmer, denser or differently stratified snow, then the relevant flow regime may change as well. A volume-only friction classification cannot represent this.

This does not mean that the traditional ΔHS_3 method is useless. It remains a practical and reproducible engineering convention**. However, its limitations should be explicitly recognized.** As avalanche-dynamics modelling increasingly considers entrainment, snow thermodynamics, fragmentation and mixed-flow behaviour, extreme scenarios will need to be defined in a way that preserves physical consistency between storm history, slab formation, release geometry, entrainable snow and the resulting flow regime.

Taken together, these issues are also central to my assessment of novelty. **The study performs a conventional deterministic corridor-scale calculation based on predefined release areas, design fracture depths and a depth-averaged dense-flow model. Similar approaches have been used for many years with RAMMS and related models. The use of AvaFrame, its open-source character, and the application to a poorly studied Chilean corridor are useful, but these aspects do not in themselves constitute a scientific advance.**

Indeed, the present formulation omits several processes that are now recognized as central to large avalanche dynamics: entrainment, evolving flow regime, snow temperature and thermodynamic state, and probabilistic uncertainty in release geometry. **I therefore see the study primarily as a regional deterministic exposure application rather than an advancement of avalanche-science methodology.**

More generally, I see a **missing physical chain between avalanche formation and avalanche dynamics**. A more complete large-scale framework would ideally proceed from independently identified probable release terrain, through physically and probabilistically plausible release dimensions and snow properties, to the snow available for entrainment and finally to a dynamics model capable of representing the corresponding flow state:

$$\boxed{\text{PRA} \rightarrow P(D, L, B, \rho, T, \ldots) \rightarrow \text{entrainable snow} \rightarrow \text{flow regime} \rightarrow \text{dynamic simulation}}$$

The present manuscript largely begins at the final part of this chain. Release areas and fracture depths are prescribed, their product defines an initial release volume, this volume determines the friction class, and a no-entrainment dense-flow model is then applied. Consequently, both the formation of the initial avalanche and the subsequent evolution of avalanche mass and flow state are outside the model, even though “volume” is treated as the principal quantity controlling mobility.

For these reasons, I do not currently see the manuscript as a methodological advancement. I see it as an opportunistic application of available open-source software to a new corridor, with severe physical limitations that are not adequately reflected in the interpretation of the results. If the authors wish to retain the broader methodological claims, the treatment of release uncertainty, calibration, entrainment, snow state and flow regime would need to be substantially strengthened. Otherwise, the paper should be repositioned more modestly as a deterministic regional exposure study whose main contribution is geographical coverage and reproducibility rather than methodological novelty.