

We thank the two reviewers for their constructive comments. We propose to carefully revise the manuscript and we respond to each point below. The reviewer's comments are in black font, while our answers, numbered 1 to 45, are in blue font. In the revised version all the changes introduced in the manuscript and discussed below will be clearly indicated.

Reviewer #1

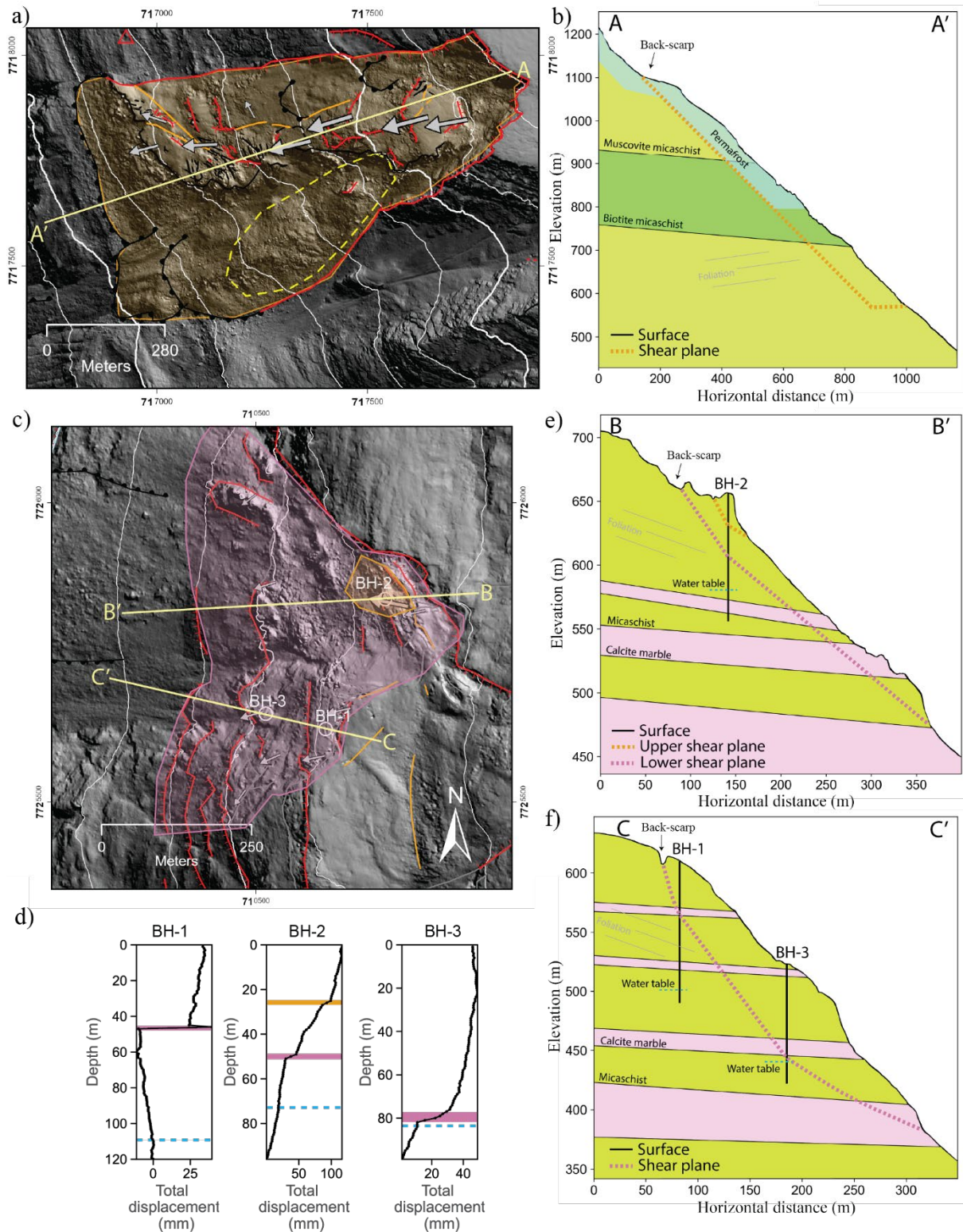
Comment: The submitted manuscript investigates processes driving arctic/permafrost-affected rock slope failures. By analyzing monitoring data (6 to 12 years collection periods) of two well-researched landslides in Norway, the authors discuss the role of climatic influences (rain, snow, permafrost degradation) on failure kinematics, and provide insights into seasonal controls and deformation behaviour of rock slope beyond millions of cubic meters. The strength of the manuscript lies in the fusion of datasets (meteorological, seismic, surface and subsurface displacement & velocities, hydrogeological data) and their combined interpretation, and the data itself (scarcity of in-situ data). Moreover, the authors demonstrate the use of an Auto Regressive Distributed Lags model in predicting sliding velocity on the basis of past sliding velocity and exogenous parameters (precipitation, temperature, relative seismic velocity change). The model was used to backcalculate the individual share of the parameters in contributing to overall deformation. With a detailed analysis of in-situ records, this manuscript substantially contributes to deciphering the complex driving mechanism of arctic rock slope deformations.

The data, data processing, and methodology are precisely described and sound. Overall manuscript quality is good. I have minor remarks on the figures and the discussion section. Literature is adequate.

I suggest publication after minor revisions:

Comment: Discussion: A "synthesis" section or "synoptic discussion" would strengthen understanding of the interwoven process. An illustrated conceptual model (i.e., a figure showing two cross-sections) of both landslides could be really nice to highlight the differences and similarities. I.e., permafrost distribution, geological structures, shear planes/zones, and location/degree of fractured rock mass, water availability/height of pressure head, differential kinematics, and reaction time lag. You demonstrated multiple relationships effectively, but a clear synthesis of the core insights would strengthen the overall message. The writing can be somewhat technical at times (many numbers), making some aspects difficult to read, though it is generally acceptable given the type of analysis presented.

Answer 1: This might indeed be very useful to illustrate our analysis. We propose to write a synthesis of the discussion, and add a conceptual model of the two landslides such that comparison is eased. This conceptual model displays cross-sections of the unstable slopes integrating all available observations (geological, kinematical, geophysical, permafrost) available on each site. The new figure is provided below.



Caption: Schematic representation of the Gámanjuni and Jettan landslides. a) Map view of Gámanjuni and profile A-A' , and c) map view of Jettan showing profiles B-B' and C-C' , together with their associated boreholes (BH-1 to BH-3). b,e, and f) Cross-sections illustrating lithology, foliation, interpreted shear zone, permafrost extent, borehole locations, and hydraulic head. d) Borehole data showing displacement, indicating the presence of one or two distinct shear zones. Displacement is measured along the slip direction and represents the maximum recorded value in each borehole. Axis scales differ among plots. Orange and pink lines denote the interpreted upper

and lower shear zone, respectively, and the dashed blue line indicates the maximum water level. Shear zones are interpreted by projecting borehole data onto the profiles and extrapolating toward mapped morphological features. Proposed sliding scenarios are shown on the map with semi-transparent orange and pink polygons.

Comment: Strong discussion in general. Focus is yet specific on the studied sites. A designated section on transferability, intercomparability to other arctic/permafrost rock slope studies would be appreciated. Were the same seasonal shifts in hydrogeology or in slope velocities and their magnitudes reported elsewhere in comparable settings?

Maybe compare your results to Scandroglio et al. (2025) – long-term joint seepage record in sporadic permafrost terrain; Offer et al. also (2025) show seasonal permeability changes through ERT, and piezometric records in mountain permafrost.

Scandroglio, R., Weber, S., Rehm, T., and Krautblatter, M.: Decadal in Situ Hydrological Observations and Empirical Modeling of Pressure Head in a High-Alpine, Fractured Calcareous Rock Slope, *Earth Surface Dynamics*, 13, 295–314, <https://doi.org/10.5194/esurf-13-295-2025>, 2025.

Offer, M., Weber, S., Krautblatter, M., Hartmeyer, I., and Keuschnig, M.: Pressurised water flow in fractured permafrost rocks revealed by borehole temperature, electrical resistivity tomography, and piezometric pressure, *The Cryosphere*, 19, 485–506, <https://doi.org/10.5194/tc-19-485-2025>, 2025.

Answer 2: We find this question difficult to answer as the comparison with other areas in similar contexts are either inexistent or very specific. For instance, the two proposed studies explored the Zugspitze landslide, which is a rockslope that failed 3700 years ago and Kitzsteinhorn had a minor failure of a few hundred cubic meters, which is a rockfall, i.e., not on the same scale as a rockslide of millions of cubic meters studied here. Furthermore, both studies focus on water transport and not on the kinematics of the slopes. We would have liked to compare our results with studies similar to our, but could not find any with such details.

Comment: Concerning hydrogeological flow paths: It would be interesting to show how piezometric measurements in boreholes correlate with snowmelt and rainfall. Maybe add a paragraph or figure on the connection between surface water availability and subsurface water or infiltration times. As I understood, slope kinematics are mainly explained by surface water (rain/snow) rather than recorded borehole hydraulic heads (except BH-2 upper; Figure A2). Typically, transient hydrostatic pressure buildup in the rock mass may explain kinematics. I am aware you noted “...because open boreholes can misrepresent subsurface water flow (Aspaas et al., 2024),” in line 460. Still, it might be interesting to see!

Answer 3: We could add such a figure in the results section. However, what we point to here is not whether water levels are correlated to displacement, but rather ask whether water pressure or seepage forces are the mechanism causing accelerations. We propose to clarify this point in Section 4.3 and to modify the text into “However, because open boreholes may not reliably capture in situ subsurface flow paths or pore-pressure conditions (Aspaas et.al., 2024), it is not possible here to distinguish between seepage forces and pore-pressure effects, and these hydromechanical processes are therefore not analyzed further.”.

In addition, we have added the proposed figure in section 3.3 and replaced the reference to Figure A2 in the text with the new figure.

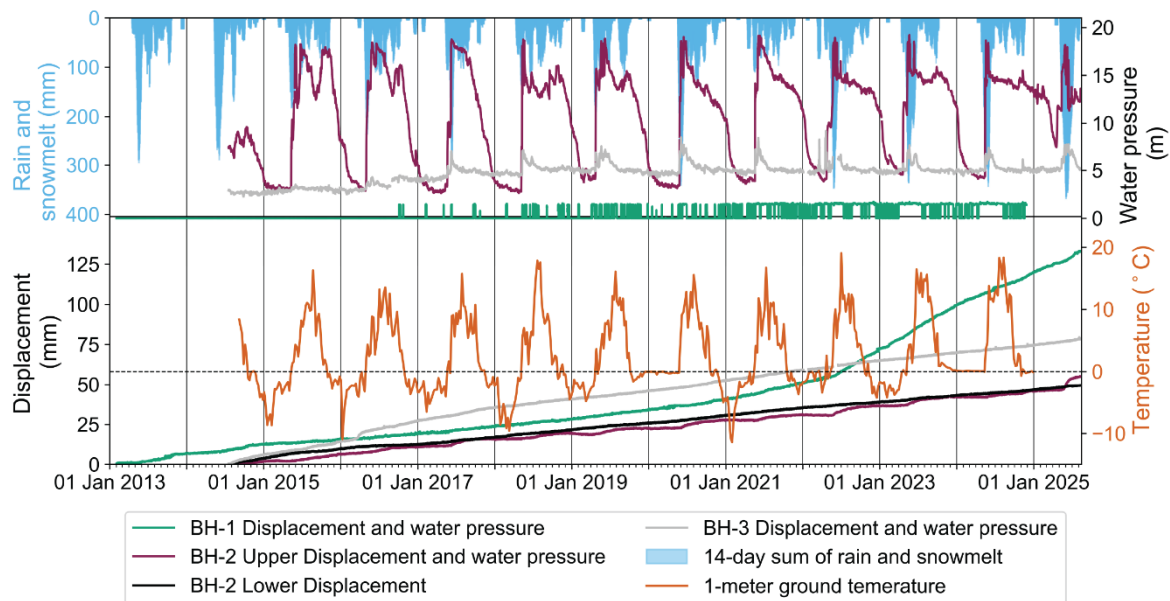


Figure 7: Time series of displacement in the shear zone and water pressure measured in boreholes BH-1 (mint green), BH-2 upper shear zone (purple) and lower shear zone (black), and BH-3 (orange), together with 14-day cumulative rainfall, snowmelt, and air temperature at the Jettan landslide.

% Minor Remarks

Comment: “Catastrophic landslide” is used throughout the text. Please clarify what you mean. You study slow-moving gravitational rock slope deformations. I guess you mean rapid rock detachment evolving into highly mobile mass movements? Catastrophic is related to damage to humans/infrastructure and not to the process itself.

Answer 4: We have clarified the terminology in the text by avoiding the word “catastrophic” and rather used different versions of sudden failure.

Comment: Ln21: How are they highlighted? More specifically.

Answer 5: We have changed the sentence to: 'In these papers, the roles of global warming and permafrost degradation are highlighted through observations of landslides originating in permafrost-affected slopes, where warming and thaw-related weakening are interpreted as key preconditioning factors.'

Comment: Ln27: Not only permafrost thaw, but also warming permafrost temperatures may be relevant in the final detachment phase (Mamot et al. 2018).

Mamot, P., Weber, S., Schröder, T., and Krautblatter, M.: A Temperature- and Stress-Controlled Failure Criterion for Ice-Filled Permafrost Rock Joints, *The Cryosphere*, 12, 3333–3353, <https://doi.org/10.5194/tc-12-3333-2018>, 2018.

Answer 6: We added warming permafrost in the revised version: 'The processes linking global warming to landslide activity are complex (Patton et al., 2019), involving a combination of ice loading history (Gramiger et al., 2017; Lacroix et al., 2022; Walden, 2025), rapid snowmelt or extreme rainfall events (Ben et al., 2022), increased permafrost thaw (Donnini et al., 2023; Cardinali et al., 2000; Gruber et al., 2007; Hilger et al., 2021), and warming permafrost (Krautblatter et al., 2013; Mamot et al., 2018).'

Comment: Ln27ff: Rather than singular individual drivers, the coupling of processes typically leads to enhanced progressive failure (Grämiger et al., 2020) or may peak in final detachment (Pfluger et al., 2025): Polythermal shifts in glacier or rock slope regimes impact hydrogeology – previous impermeable areas are fed with infiltrating meltwater. Hydrogeology impacts deformation and thermal conditions in turn (hydro-thermo-mechanical feedbacks). Often impossible to decipher individual factors for destabilization.

Grämiger, L. M., Moore, J. R., Gischig, V. S., Loew, S., Funk, M., and Limpach, P.: Hydromechanical Rock Slope Damage During Late Pleistocene and Holocene Glacial Cycles in an Alpine Valley, *Journal of Geophysical Research: Earth Surface*, 125, <https://doi.org/10.1029/2019JF005494>, 2020.

Pfluger, F., Weber, S., Steinhauser, J., Zangerl, C., Fey, C., Fürst, J., and Krautblatter, M.: Massive Permafrost Rock Slide under a Warming Polythermal Glacier Deciphered through Mechanical Modeling (Bliggspitze, Austria), *Earth Surface Dynamics*, 13, 41–70, <https://doi.org/10.5194/esurf-13-41-2025>, 2025.

Answer 7: We understand this comment as a recommendation to include additional references in this paragraph. However, in our view, the inclusion of the two suggested papers may not substantially enhance this paragraph, as the key aspects are already addressed in the manuscript. The difference in hydrological pathways in a frozen versus thawed rock is already mentioned in the discussion section 4.1 on line 385-390 referring to Pogrebiskiy and Chernyshev (1977).

Comment: Table 1: *Sloping Local Base Level (SLBL) -> Reference to calculation, publication, or the model is missing.

Answer 8: We have added a reference to the publication Böhme et al., 2016.

Böhme, M., Bunkholt, H., Dehls, J., Oppikofer, T., Hermanns, R., Dalsegg, E., Kristensen, L., Lauknes, T. R., and Eriksen, H. Ø.: Geologisk modell og fare- og risikoklassifisering av det ustabile fjellpartiet Gamanjuni 3 i Manndalen, Troms, 2016a.

Comment: Figure 2: Figures and subfigures should be labeled uniformly throughout the manuscript (a),(b),...

I would recommend each subfigure with its own legend for clarity.

Highlight location for displayed monitoring records (b), (c) in the map (a).

Answer 9: Thank you for the suggestion. We have labeled all the figures uniformly throughout the manuscript. We have not made each plot into its own figure as we find it cleaner to display it as it is. However, we do see the possible confusion of GPS-4 and Air temperature and have therefore changed one color. The location of b and c on map a is already highlighted. For c it is more complex as the data is modeled by the Meteorological Institute and seNorge, but the underlying data is from the meteorological stations displayed in a. The figure text is slightly changed to accommodate these changes.

Comment: Figure 2. Caption. Borehole displacement given for what depth/location? Specify. Please give a reference altitude for the temperature. Was the temperature/precipitation measured at the local site or from seNorge? (same for Figure 3).

Answer 10: Thank you for pointing out this. The borehole displacement referred to is measured on the shear zone. We specify this information now in the caption of the figure. For the altitude of the modeled meteorological variables, we added the information under the section 'Meteorological data' as we find it the most logical place to put it. We modified this sentence: 'The modeled variables were therefore extracted at the elevation of each landslide (557 and 820 m a.s.l. for Jettan and Gámanjunni, respectively), allowing elevation-consistent comparison while providing snow variables unavailable from in situ observations.' The meteorological data are acquired at each site by the meteorological stations, and then the seNorge model utilizes all meteorological stations in vicinity of the landslide to interpolate and weight the grid cell used, as we explain in the same paragraph.

Comment: Figure 4. The figure appears before the notion in the text. For what period is the displacement shown, add in the figure or caption.

Answer 11: The location of this figure has been changed and moved closer to the corresponding paragraph. The period is added in the figure caption.

Comment: Figure 5. Both temperatures look almost the same; It would be good to have a reference altitude for the temperature. i.e., use the location of the head scarp, as the two landslides vary distinctly in elevation range.

Answer 12: The reference altitude is mentioned in the comment above. The temperature is quite similar, but not identical, and it is not possible to spot the difference in these two plots, because differences are small compared to the large seasonal variability of each temperature time-series and because they appear on two distinct subplots. However, looking at the freeze index, the reader can see there is a difference in the two temperature graphs. The figure text is changed to help the reader: ‘Time series of the air temperature and normalized freeze index, precipitation, and rain and snowmelt are displayed in (a) and (c) for the Gámanjunni and Jettan landslides, respectively. The transparent blue and yellow background highlights cold and warm periods based on the freeze index, facilitating comparison of these periods between the two landslides. The time series of the mean velocity of all GNSS receivers for the Jettan and Gámanjunni landslides (except GPS-3 for Jettan) are shown in (b). All time series have been smoothed by down-sampling to a 30-day median or summation for precipitation and rain and snowmelt.’

Comment: Ln310: If a central driver is water pressure, show the plot also in the main manuscript rather than the appendix. Best add it into one and the same with snow/rainfall data to show if there is a link in the records.

Answer 13: The new figure is added to the manuscript. The figure incorporates the information from A2 as well as the suggestion of showing rain and snowmelt in a figure together with the borehole water pressure sensors. See answer 3.

Comment: Section 3.4.2: Correlations: Please use a table to show the correlations of tested pairs. Visually, it is easier to compare in a table than written in text form.

Answer 14: We have added a table and a new sentence in this section. ‘Borehole slip acceleration, hydrological variables, and seismic velocity changes in 2022 exhibit strong to very strong correlations, with systematic lags between forcing and response (Table 2).’

Table 2. Correlations among tested variable pairs for the year 2022

Variable X	Variable Y	Pearson's r	Time lag
Borehole BH-2 upper shear zone 30-day slip velocity	Borehole BH-2 water pressure	0.74	5 days
Borehole BH-2 upper shear zone 30-day slip velocity	14-day cumulated rain and snowmelt	0.79	4 days
Borehole BH-2 upper shear zone 30-day slip velocity	apparent surface velocity variations (dv/v)	0.90	68 days
Borehole BH-2 water pressure	apparent surface velocity variations (dv/v)	0.86	60 days
Borehole BH-1 30-day slip velocity	14-day cumulated rain and snowmelt	0.36	23 days
Borehole BH-1 30-day slip velocity	apparent surface velocity variations (dv/v)	0.80	30 days
14-day cumulated rain and snowmelt	apparent surface velocity variations (dv/v)	0.74	75 days
Ground temperature (1 meter depth)	apparent surface velocity variations (dv/v)	0.98	49 days

Comment: Ln366: This first paragraph is a bit generic. Rather than using it as an introduction (as it was anyway in section 1), I would skip it here, and emphasize your own subsections on comparability and transferability of your results.

Answer 15: The paragraph is now removed.

Comment: Ln498: Please refer to the Figures accordingly.

Answer 16: We have referred to the figures in the text.

Comment: ff. For me, it is unclear what dv/v decrease is associated with shear plane wetting and what is associated with localized/volumetric deformation. The impact of deformation is likely higher, but both processes affect the measured data at the same time. Can you estimate orders of magnitude for the dv/v reduction impact of deformation and of water availability?

Answer 17: We clarified this point in the discussion section 4.4 by adding expected percentage increase or decrease in dv/v related to the mechanisms in addition to modifying this paragraph: 'Cross-correlation analysis shows strong negative relationships between dv/v and shallow ground temperature (1 m depth), the 14-day sum of rain and snowmelt, and borehole BH-1 velocity (Table 2). The long-term cyclicality of dv/v suggests a similarly fluctuating controlling mechanism. From 2013 to 2020, borehole BH-1 displacement exhibited a predominantly linear, non-seasonal trend, indicating that dv/v does not directly reflect shear zone stability (Figure 6). While both shallow temperature and water availability display seasonal signals, only rain and snowmelt exhibit two distinct annual peaks (Figure 7). Previous studies have shown a dv/v decreases with wetting (causing stiffness reduction) and increases with drying of the medium (Liu et al., 2024; Borgeat et al., 2025). At the Åknes landslide (fractured gneiss), Bogner et al. (2026) reported dv/v drops between 2.6 and 3.9 % associated with water-level increases of several meters at approximately 80 m depth. Here, the first drop in dv/v (between day 8 and 9 in Figure 8b) coincides with peak velocities in GNSS and borehole shear zones, whereas the second drop (day 10 in Figure 8b) aligns with GNSS acceleration but not with borehole acceleration. This pattern suggests that spring acceleration reflects localized shear deformation, while autumn acceleration represents volumetric deformation of the landslide body. Collectively, these observations support an interpretation of an annual rheological transition from rigid to compliant behavior driven by progressive wetting of the landslide'.

Using ambient noise interferometry, dv/v time series reflect changes in ground hydromechanical characteristics. The subsurface is affected by both periodic/reversible and irreversible effects caused by environmental parameters and landslides reactivation/failure, respectively.

The main periodic and/or reversible effects are controlled by temperature (thermo-elastic coupling), pore pressure and water content, loading by snowfall (compaction), and icing/de-icing.

A first order amplitude from recent literature reviews (Tables 1 and 2) shows that temperature and water content/pore pressure are generally linearly correlated with dv/v variations with amplitudes of up to few percents (typically in the range $\pm 2\%$) for depths of a very few tens of m. Recently, Bogner et al. (2026) reported dv/v decreases of 2.6-3.9% that they attributed to a reduction of stiffness itself caused by increase of the water level of a few m at around 80 m depth.

Considering mechanical loading by snow, dv/v has been observed to be either positively or negatively correlated with snow height, with dv/v variations of up to several percents (Guillemot et

al., 2021). Finally, few reports have been made so far regarding icing effects. Bièvre et al. (2018) and Steinman et al. (2020) reported dv/v increases between 0.5 and 2% during potential icing periods.

Regarding dv/v drops caused by landslide failure or reactivation, few reports are available so far but observations show drops of 4 to 7% in fine-grained landslides (Mainsant et al., 2012; Bontemps et al., 2020) and slates (Chen et al., 2026).

Table 1: Long-term (monthly, yearly, and multiple years) environmental variations of seismic velocity reported in the literature. It corresponds to Table 2 in Le Breton et al. (2021)

Study	Frequency (Hz)	Amplitude (%)	Context	Suggested governing influence (correlation sign, lag time)
Sens-Schönfelder and Wegler (2006)	Broadband >0.5	4	Volcano, Merapi, Indonesia	Groundwater level (-)
Sens-Schönfelder and Larose (2008)	6-11	0.15	Lunar soil, Apollo 17	Temperature (+, 7 d)
Meier et al. (2010); Tsai (2011)	0.1-2	0.1	Basin, Los Angeles, USA	Groundwater level (-)
Voisin et al. (2016)	6-8	2.2	Landslide, Utiku	Groundwater level (-)
Bièvre et al. (2018); Larose et al. (2015)	3-20	6	Landslide, Pont-Bourquin, Switzerland	Rainfall (-), temperature (+, 40 d), frost (+)
Wang et al. (2017)	8-12	3	Landslide, Pont-Bourquin, Switzerland	Rainfall (-), temperature (+, 40 d), frost (+)
Miao et al. (2018)	0.15-0.9	0.03	Whole Japan, Hi-net	Pore pressure (-), snow depth (+), sea level/tide (+), Temperature (+, 40 d)
Lecocq et al. (2017)	Broadband <30	0.3 ms	East Japan, Kik-net	Rainfall (-)
	0.1-0.8	0.04	Ground water storage, Gräfenberg Array, Germany	Temperature (+) and groundwater level (-)
Clements and Denolle (2018)	0.5-2	0.3	Groundwater basin, San Gabriel Valley, California	Groundwater level (-)
Hillers et al. (2015a)	0.5-2	0.2	San Jancinto Fault, California, USA	Temperature (+, 20 d)
Hillers et al. (2015b)	2-8	0.3	Mountain, San Jancinto Fault Area, Piñon Flat Observatory, California, USA	Tidal strain (-)
Hillers et al. (2014)	1-4	0.3	Chelungpu fault, Taiwan	Precipitations/groundwater level (-)
Richter et al. (2014)	4-6	0.29	Northern Chile	Temperature (+, 33 d)
	9-11	2		
Mordret et al. (2016)	0.1-0.3	0.1	Ice sheet, western Greenland	Ice mass (-, 2 months)
Preiswerk and Walter (2018a)	2-8	10 dB SNR	Glacier, Aletsch, Switzerland	Source variation
	4-16	CC drop	Glacier, Plaine Morte, Switzerland	Source variation due to drainage/filling of a glacier-dammed lake
James et al. (2017)	13-17	6	Permafrost, Poker Flat Research Range, Alaska	Soil freezing (+)/thawing (-)
Guillemot et al. (2020)	4-10	3	Rock glacier, Gugla, Switzerland	Soil freezing (+)/thawing (-)
Gassenmeier et al. (2015a, b)	1.5-3	0.8	CO ₂ storage site, Ketzin, Germany	Groundwater level (-), Frost (+)
Rivet et al. (2014)	0.1-2	0.50	Volcano, Piton de la Fournaise	-
Hobiger et al. (2016)	0.25-0.5	0.02-0.04	Earthquakes in Japan	Curve fitting with a sinusoidal function
	0.5-1	0.04-0.12		
	1-2	0.08		
Bontemps et al. (2020)	3-8	2	Landslide, Maca	Rainfall and earthquakes
Renalier et al. (2010b, 2010a)	1.3-3.5	1	Landslide, Avignonnet	-
Valentin (2018)	10-12	5.00	Landslide, Char d'Osset	-

Table 2: Literature review of studies that have measured dv/v and compared with groundwater levels (Table 1 in Denolle *et al.* (2025)).

Region	Wave type	Network	Freq (Hz)	Δ GWL (m)	Hydro estimate method	dv/v (%)	Reference
Southern CA, USA	Coda	Array	0.2–2	16	Well	0.10	Mao, Lecointre, et al. (2022)
CA, USA	Coda	Single	4–8	6	Rainfall	1.15	Clements and Denolle (2023)
San Gabriel, CA	Coda	Array	2–5	25	Well	0.15	Clements and Denolle (2018)
New Zealand	Coda	Small array	6–8	2	Piezo, seismometers	2.00	Voisin, Garambois, et al. (2016)
Lyon, France	Coda	Small Array	2–5	2	Piezo	2.50	Gaubert-Bastide et al. (2022)
Lyon, France	Coda	Small array	3–20	3	Piezo	2.50	Voisin, Guzmán, et al. (2017)
Ketzin, Germany	Coda	Array	1.4–3	0.6	Well	0.60	Gassenmeier et al. (2014)
Texas, USA	Coda	Single	0.01–8	25	Well	2.00	Kim and Lekic (2019)
Germany	Coda	Array	0.1–0.8	0.1	Well, water balance	0.01	Lecocq et al. (2017) (*)
Malta	Coda	Single	3–10	0.5	Well, rainfall	1.00	Laudi et al. (2023)
CA, USA	Coda	Small array	4–15	1.5	River gage	2.50	Rodríguez Tribaldos and Ajo-Franklin (2021)
Australia	Coda	Small array	20		0.3	2.00	Olivier et al. (2017)
Kyushu, Japan	Coda	Array	0.1–0.39	0.01	Rainfall	0.02	Q.-Y. Wang, Brenguier, et al. (2017) (*)
OK, USA	Coda	Array	0.1–1	0.15	GRACE	0.02	Zhang et al. (2023) (***)
Greece	Late coda	Single	0.3–1	2–5	Wells	0.03	Delouche and Stehly (2023)
Italy	Coda	Array	0.1–0.9	100	Geodetic inversion	0.10	Almagro Vidal et al. (2021)(**)
Germany	Late coda	Small array	1.5–3	0.4	Well	0.20	Gassenmeier et al. (2014)
Lyon, FR	Surface waves	Small array	3–40	2.5	Piezo and wells	1.50	Garambois et al. (2019)
Mojave, CA	Coda	Array	0.1–2	12	Well	0.10	Tsai (2011) and Meier et al. (2010)
Taiwan	Coda	Single	0.1–0.9	7	Well	0.40	Feng, Huang, Hsu, et al. (2021)

Comment: Ln519: Conclusion: First paragraph is repetitive; I would not cite literature in the conclusion; it is about your work. For my taste this paragraph can be skipped. I.e. merge the last sentence with the first of line 525.

Answer 18: Thank you for the suggestion. The first paragraph is removed.

Comment: Ln537: Be specific. How does it improve hazard assessment and monitoring strategies? It is easy to say through more data (i.e., surface and subsurface observations), but how many boreholes do we have at such locations? I would put the emphasis more on the processes.

Answer 19: The conclusion is now more specific towards how our work can improve hazard assessment and monitoring strategies. We now write: Distinguishing between hydrologically driven seasonal accelerations and deformation associated with permafrost thaw enhances landslide hazard assessments by clarifying which slope instabilities are primarily governed by transient hydrological forcing and which may exhibit elevated long-term deformation. Furthermore, these results inform monitoring strategies in warming polar regions by highlighting the need to account for both permafrost conditions and subsurface groundwater dynamics within unstable slopes. In this context, seismic monitoring offers a cost-effective complement to borehole investigations, providing spatially distributed constraints on rock mass mechanical properties rather than relying solely on discrete point measurements.'

Comment: I was happy reviewing the manuscript. Detailed insights into spatio-temporal deformation processes of permafrost/arctic rock slopes. Great contribution!

Reviewer #2

Comment: This paper investigates the kinematics, mechanisms and controls of two rockslides in the arctic region of Norway (Jettan and Gàmanjunni), leveraging time series (2015–2025 for Jettan; 2021–2025 for Gàmanjunni) of surface displacements and hydro-meteorological data (both landslides), as well as borehole displacements, temperature, pore pressure and seismic data (Jettan only). The authors perform statistical analyses aimed at establishing correlations between seasonal and interannual displacements and different variables, representing possible landslide triggers/controls (rainfall, snowmelt, pore pressures, temperature) or landslide internal damage (seismic dv/v). The authors also attempt to understand the temporal evolution of the landslide mechanisms, particularly for Jettan, where high-quality borehole data are available.

Improving the understanding of landslide mechanisms and controls in high-mountain environments is important for enhancing landslide forecasting capacities in the context of climate change. The topic is relevant and suitable for the journal. The authors benefit from a wealth of high-quality data collected over many years at two well-characterised landslides and apply sound statistical analysis methods. Nevertheless, the quality of the data and analyses is not fully reflected in the paper's narrative and discussion of results. In particular (see also the detailed comments below):

The paper appears to pursue two distinct objectives: a) understanding the controls of different environmental factors (including permafrost) on the dynamics of two landslides located above and below the permafrost limit; and b) describing the mechanisms of the Jettan landslide specifically, as supported by borehole data. For this reason, the discussion is not straightforward and the take-home messages are somewhat unclear.

The authors performed sophisticated analyses of excellent data, but the results are presented in an extremely descriptive — albeit very detailed — manner, a bit disconnected from their interpretation, which in turn is not framed within robust geological models of the studied landslides. As a result, the conclusions appear rather generic and do not seem to contribute substantially to current landslide forecasting capabilities. I suggest that more details on the geological-structural model of the studied landslides would support a more convincing interpretation and discussion of the results. See, for example, the paper by Etzelmuller et al. (2022) in *ESurf*, of which some of the authors of the manuscript under review are co-authors.

[Answer 20: We understand the more detailed geological-structural model as a request to add a cross-section of Gàmanjunni, and to add more detail to the current cross-section of Jettan. This has also been addressed by the first reviewer. See answer 1 for a proposed new figure.](#)

Comment: Still regarding the geological reference model: the simplified landslide cross-sections of Jettan suggest strong lateral variations in landslide thickness. Could the authors elaborate on the possible effects of this complex geometry on the observed landslide behaviour? Could these characteristics account for the differences between the behaviours of Jettan and Gàmanjunni without invoking permafrost controls? This represents another major issue in the manuscript: the authors present very interesting data and a thorough description of the kinematics of these

landslides, yet the interpretation of the controls on the observed behaviour does not appear to be fully supported by the data analysis.

Answer 21: It is difficult to ascribe the movement to structural geometry alone when we see a seasonal signal corresponding to permafrost thaw at Gåmanjuni. We do have several indirect measurements of permafrost as shown in the paper, and direct measurements from bore-wells are needed to confirm this hypothesis. On the other hand, the landslide geometry could be the primary control of the movement, if permafrost is not present, this is our preferred interpretation. We will add the cross sections displayed in the Answer 1, that help clarify our analysis.

Comment: A major issue: what criterion was used to identify the outlined "acceleration periods" (e.g. creep bursts)? In attempting to correlate acceleration events with the measured (or calculated) environmental variables, an objective criterion is required to define such events. In Figure 6, while the seasonal accelerations are clearly identifiable, creep bursts occurring in the shear zones in boreholes are less obvious and resemble other similar signals that were not outlined as acceleration periods. How were these identified? Clarifying this point is essential to make all subsequent correlations credible to the reader.

Answer 22: These acceleration periods were visually identified by a trend change from a linear line laid over the displacement time series. We have now instead updated the plot with the objective STA-LTA algorithm used in Aspaas et al. (2024). With this method we miss smaller events, so the catalogue is incomplete below some event size. The method will be clarified in the methods section 2.2.5 with reference to Aspaas et al. (2024).

Aspaas, A., Lacroix, P., Sena, C., Kristensen, L., Langet, N., & Renard, F. (2024). Creep bursts on the Åknes landslide, Norway: Detection, characterization and possible mechanisms. *Journal of Geophysical Research: Earth Surface*, 129, e2024JF007733. <https://doi.org/10.1029/2024JF007733>

DETAILED COMMENTS

Comment: Abstract: A few additional words on the basic geological features of the two studied landslides (including their differences) would be useful here to properly frame the proposed results.

Answer 23: We propose to incorporate basic geological features (lithology, foliation, and sliding kinematics) and the differences to frame the results, while ensuring the length of the abstract complies with the recommendations of the journal.

Comment: Page 2, line 23: "millimetres to metres per year": is there a reference for this definition of "slow-moving landslides"? Does this definition apply to landslides in all materials (soil and rock) or specifically to rockslides?

Answer 24: We use the definition of slow-moving landslides of Lacroix et al., (2020) that combines the previous classification of very slow to moderate landslides of Cruden and Varnes (1996). A slow moving landslide actually refers to a sufficiently low velocity motion to be able to monitor the evolution of the physical properties of the landslides. We clarify this point.

Comment: Page 2, lines 33–36: Aside from the indispensable work by Iverson, the references here are limited to work by the authors and some US researchers collaborating with them. Several other citable references on this topic exist from different landslide research groups in Europe and beyond.

Answer 25: We propose to add the following references.

Fiolleau, S., Uhlemann, S., Wielandt, S., & Dafflon, B. (2023). Understanding slow-moving landslide triggering processes using low-cost passive seismic and inclinometer monitoring. *Journal of Applied Geophysics*, 215, 105090.

Watlet, A., Whiteley, J., Dashwood, B., Morgan, D., Lane, V., Finch, L., ... & Chambers, J. (2026). Seismic response of a slow-moving landslide: exploring data from two years of seismic monitoring at the Hollin Hill Landslide Observatory (UK). *Seismica*, 5(1).

Liu, Z., Liang, C., Sens-Schönfelder, C., Hu, W., Sun, X., Zhang, T., ... & Jiang, H. (2024). Monitoring crack opening via seismic velocity variation to assess that fatal effect of precipitation for landslide motion. *Earth and Planetary Science Letters*, 644, 118922.

Bièvre, G., Joseph, A., & Bertrand, C. (2018). Preferential Water Infiltration Path in a Slow-Moving Clayey Earthslide Evidenced by Cross-Correlation of Hydrometeorological Time Series (Charlaix Landslide, French Western Alps). *Geofluids*, 2018(1), 9593267.

Comment: Page 2, lines 41–42: "the objective of this study": the objectives appear twofold and somewhat unclear. See the General Comments above and further comments below.

Answer 26: We understand the comment as a request to add a second objective in addition to the current objective: 'analyze the driving mechanisms responsible for acceleration phases and, consequently, the potential triggers of rapid collapses in Arctic landslides situated near the permafrost boundary'. The current objective is achieved by examining two landslides that both have surface measurements that show similar kinematic behavior. Therefore, we explore the mechanism at the origin of this behavior using the data on Jettan that are much more numerous with subsurface observations. The new geologic cross-sections (see Answer 1) show the similarities and differences of the two landslides for a better analysis of the data for the common objective.

We do not understand the point that investigating additional data of one landslide warrant two objectives as analyzing borehole and seismic data at Jettan is performed to understand the driving mechanism of the landslide.

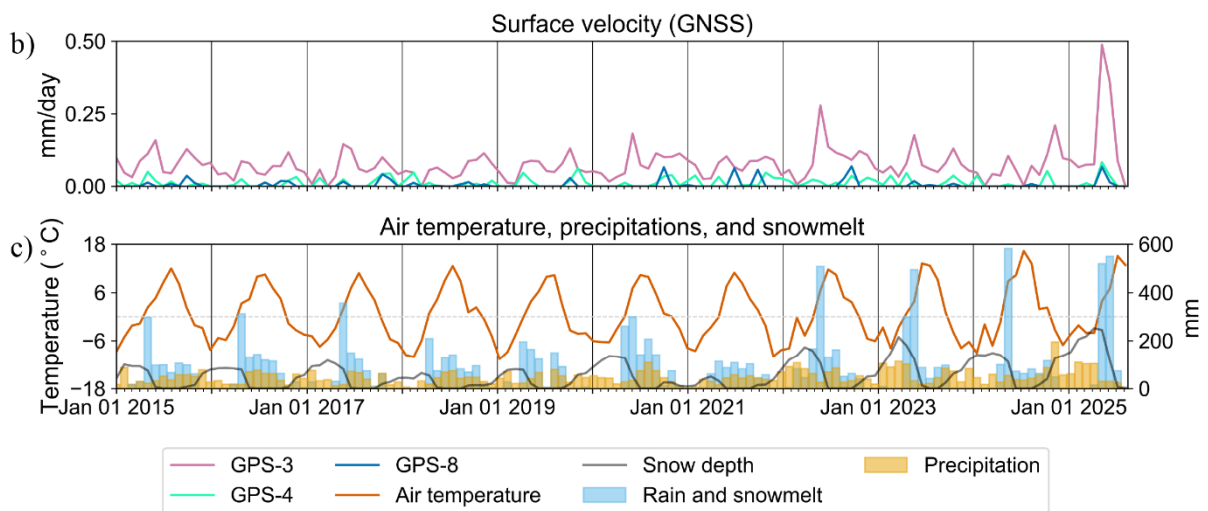
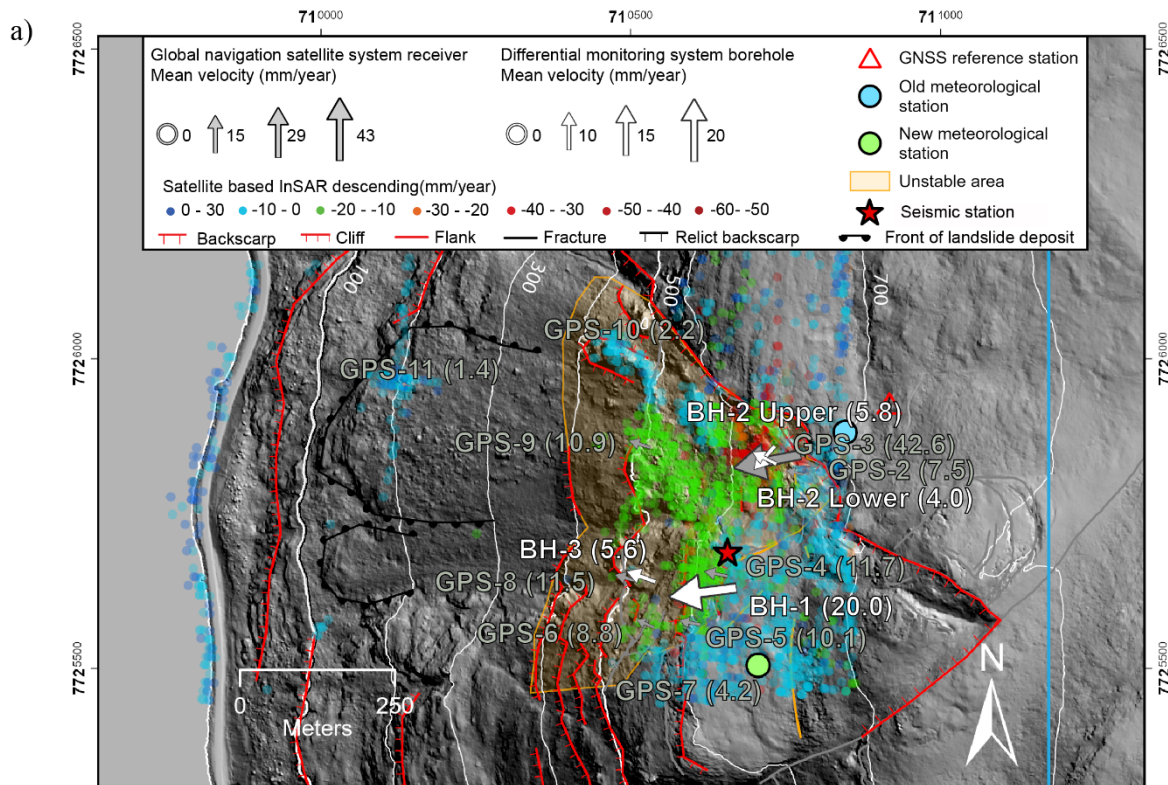
Comment: Page 3, Table 1: "Sloping Local Base Level (SLBL)": this concept may be unfamiliar to some readers, yet it is important. Please include adequate references and an explanation in the caption.

Answer 27: This comment has also been addressed by reviewer 1, and we have added the reference to the publication of Böhme et al. (2016).

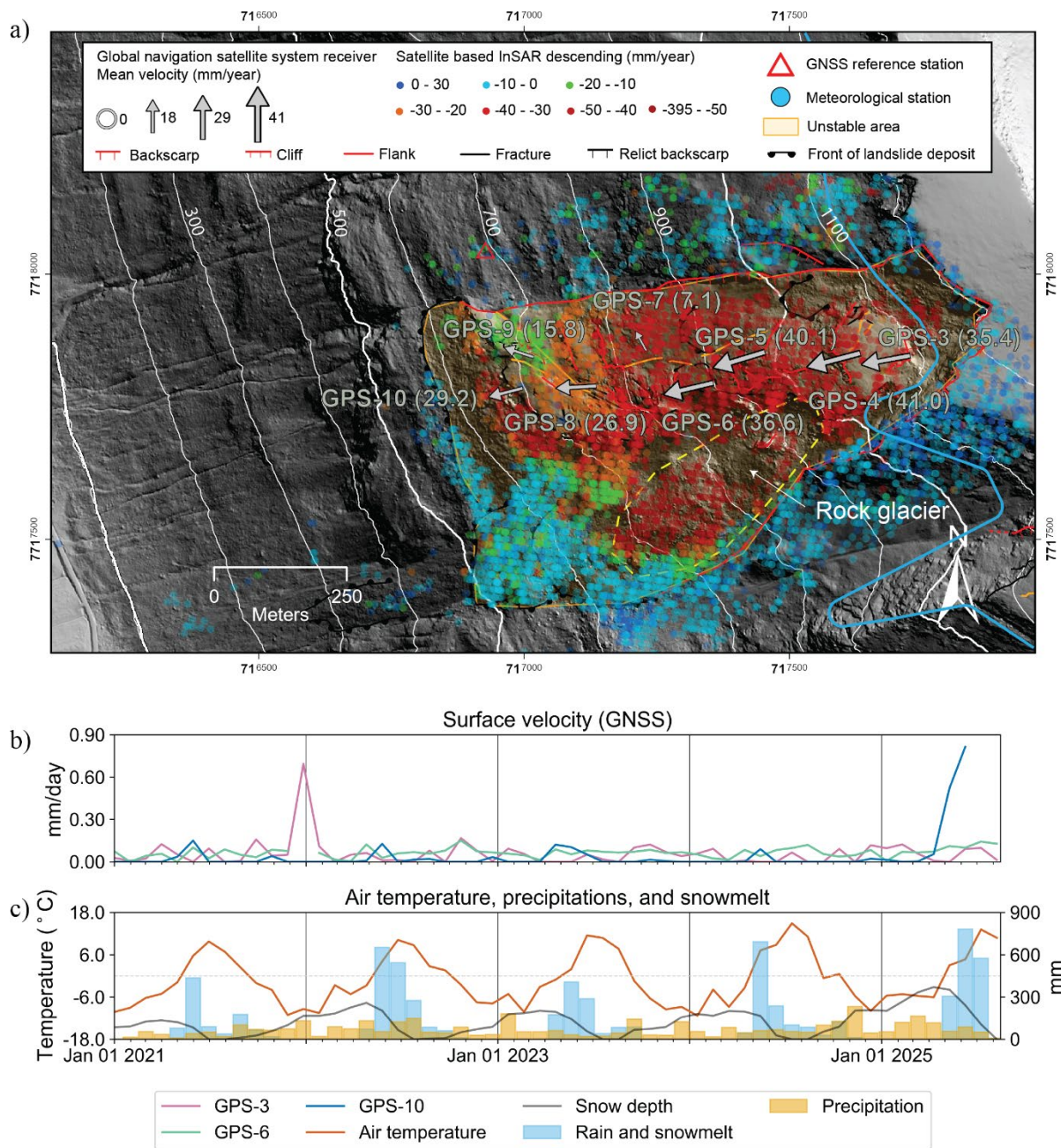
Böhme, M., Bunkholt, H., Dehls, J., Oppikofer, T., Hermanns, R., Dalsegg, E., Kristensen, L., Lauknes, T. R., and Eriksen, H. Ø.: Geologisk modell og fare-og risikoklassifisering av det ustabile fjellpartiet Gamanjunni 3 i Manndalen, Troms, 2016a.

Comment: Figure 1: I assume the authors have access to a permafrost map or an APIM-like dataset. Would it be possible to overlay the likely permafrost extent to show the relative positions of the two landslides?

Answer 28: We understand the interest of overlaying a permafrost model. For the readability of the figures, we decided not to add a colormap of the permafrost probability. We propose to add a line showing the limit of high probability of permafrost in the revised figure. Detailed view of Jettan can be found in Magnin et al., 2019, Figure 12 a) and of Gamanjunni in Etzelmüller et al., 2022 Figure 1 b). To clarify this point, we also added this information to the caption of Figure 2 and 3. We have also added the potential permafrost extent inferred from geophysics in Figure 4 (see answer 1).



Caption: Overview of the Jettan landslide. a) Map showing key morphological features and instrument locations. Gray arrows represent mean surface velocities (mm/yr) derived from GNSS data, while white arrows indicate borehole shear zone displacements from the differential monitoring system (BH), both averaged over the period from 01.01.2021 to 12.08.2025. Arrow dimensions are scaled proportionally to velocity magnitude, with values annotated in parentheses. Borehole BH-2 displays two arrows corresponding to distinct shear zones. The blue solid line indicates the elevation above which permafrost is highly probable (Magnin et al., 2019). White contour lines denote 100-meter elevation intervals. Hillshade based on Høydedata (Kartverket). Time series plots include b) GNSS-derived surface velocities and meteorological parameters over the past decade, smoothed using a 30-day median for GNSS and c) 30-day cumulative rainfall, snowmelt, and total precipitation.



Caption: Overview of the Gámanjuni landslide. a) Map illustrating key morphological features and instrument locations. Gray arrows denote mean GNSS-derived surface velocities (mm/yr) calculated for the period from 01.01.2021 to 12.08.2025, with arrow size scaled to velocity magnitude and annotated values in parentheses. The blue solid line indicates the elevation above which permafrost is highly probable (Etzelmüller et al., 2022). The yellow dashed outline delineates the active rock glacier extent. Elevation contours are shown in white at 100-meter intervals Hillshade based on Høydedata (Kartverket). Accompanying plots display a) GNSS velocity and b-c) meteorological time series over the past five years, smoothed using 30-day median for GNSS and 30-day cumulative rainfall, snowmelt, and total precipitation.

Comment: Figures 2 and 3: Delineating landslide boundaries and their main features (e.g, back-cracks, scarps) would greatly help the reader to understand the processes below the monitoring data.

Answer 29: Morphological features such as back-scarps, frontal lobe, cliffs, flanks, and fractures are shown in Figure 3. The geologic map made by the Norwegian Geologic Survey (NGU) did not specify these features for Jettan (Figure 2). However, we have added these features to the revised figure based on mapping from NGU.

Comment: Page 6, line 106: "Gàmanjunni landslide dataset is limited to surface displacement and meteorological data": this is an important issue. According to the paper's title and abstract, the manuscript focuses on comparing the seasonal and multi-annual behaviour of two nearby landslides located below and above the permafrost limit. However, the two landslides differ considerably in their characteristics and available data, limiting their comparison. At the same time, the authors present a more in-depth analysis of the mechanisms of the Jettan landslide, supported by the availability of both surface and borehole data. While this analysis is very interesting, it raises the question: what is the actual scope of the paper?

Answer 30: The scope of the paper is not to perform a symmetric comparison of two landslides with identical data availability, but rather to investigate how landslides located below and above the permafrost limit express seasonal and multi-annual dynamics under contrasting thermal conditions. The Jettan landslide benefits from a more comprehensive monitoring setup, including borehole measurements, which allows a more detailed interpretation of subsurface processes and driving mechanisms. Rather than limiting the study, this difference in data availability allows a qualitative and conceptual comparison, focusing on similarities and differences in timing, sensitivity to meteorological forcing, and long-term dynamics across the permafrost boundary where the Jettan data are used as a reference for interpreting Gàmanjunni. This general strategy is explained in the abstract, in the introduction, and in the conclusion.

Comment: Page 6, line 124: "Geoengineering Service Center": the company is Italian and named "Centro Servizi di Geingegneria (CSG)". Translating a company name should be avoided; I suggest referring to it as "CSG".

Answer 31: We have implemented this modification in the manuscript.

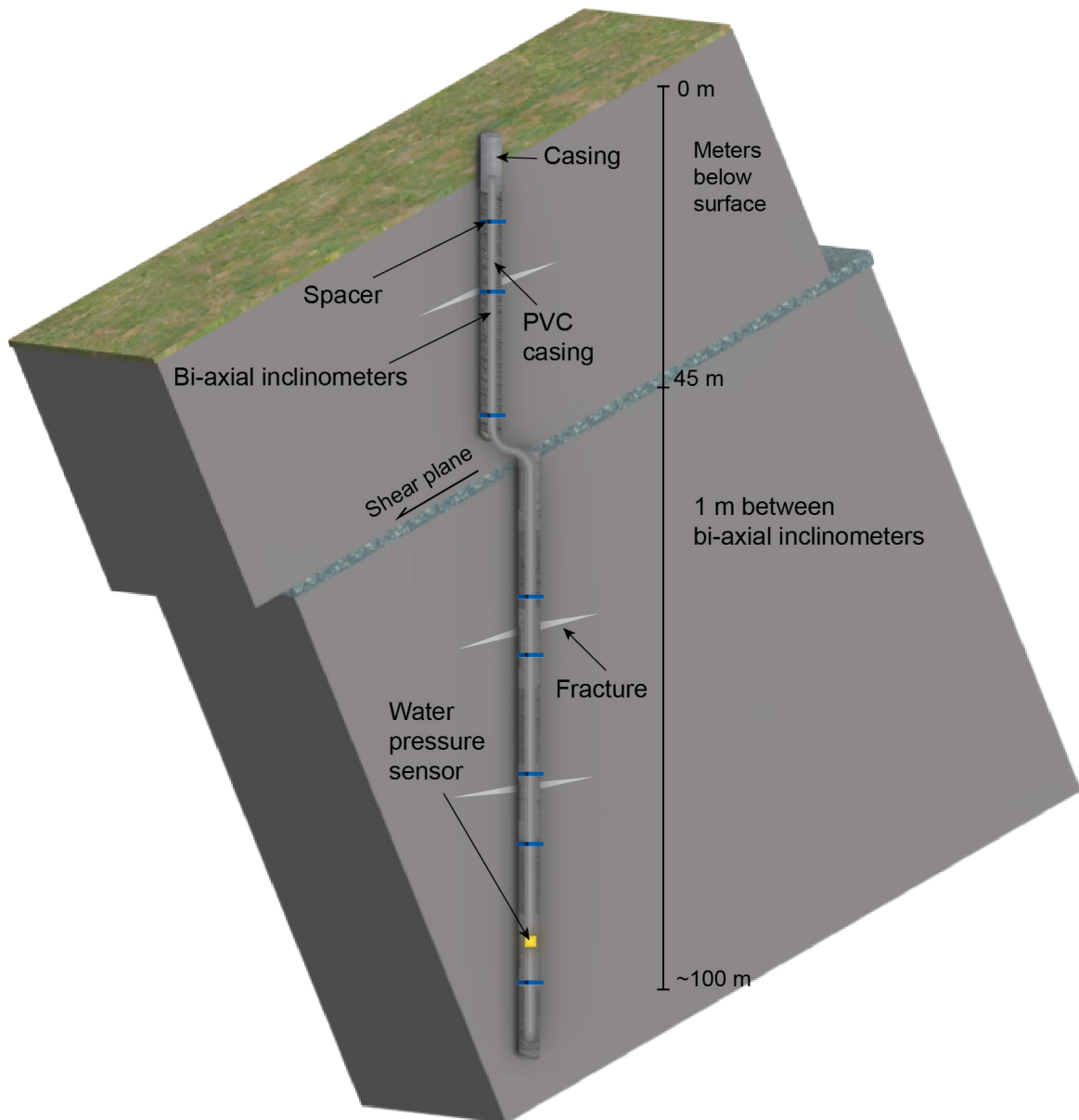
Comment: Figures 2 and 3 (time series): What averaging time windows were used to calculate the velocities of the two landslides? Are the same windows applied to both case studies? How were they selected? This is important for understanding how correlations between triggering factors and landslide response are evaluated.

Answer 32: In both figure captions it is stated that the GNSS velocity is smoothed using a 30-day median. We see that this was not sufficiently clear. We propose to add: "The velocity is calculated

by a linear regression over one day and then smoothed by down-sampling to 30-days median intervals.' The 30-day window was selected based on trial and error testing of different smoothing lengths with the aim of emphasizing seasonal and multi-annual signals rather than short-term variability. We chose 30 days to provide a suitable compromise that preserves the timing and amplitude of seasonal signals while allowing a clear visual and interpretative comparison of long-term trends between landslides and forcing variables.

Comment: Page 7, bottom of page (line numbers missing): "a piezometer was placed near the bottom of each borehole": how was this setup decided? Is there a single, fully interconnected aquifer within the landslide? Can the authors confirm that the pore pressures measured here (below the main sliding surfaces) are representative of conditions at or above the sliding surface? The presentation and interpretation of the data would benefit from a clear explanation of this.

Answer 33: We see how this can be confusing to the reader. We will add a sketch of the instrument to ease understanding as well as a better geological-structural model of both landslides. The boreholes were installed as one instrument with a piezo-meter at the end of each instrument (approximately 4 meters before the end of the instrument string). It is very difficult to demonstrate that the landslide has a single fully connected aquifer. Based on previous experience with instruments made to capture water pathways as in situ, we would suggest that there is not one single connected aquifer. Open boreholes do not let us confirm whether the measured water pressure is representative of any sliding zone as it captures water from all fractures along the borehole. We will make this point clearer in the revised version of the manuscript.



Caption: Conceptual sketch (not to scale) of borehole BH-1 instrumentation. The borehole intersects a landslide shear surface and extends into the underlying bedrock. The installation comprises a chain of 100 biaxial inclinometers and a water pressure sensor positioned at the base of the open borehole, measuring hydraulic head. The instrument is installed within a PVC casing.

Comment: Page 8, Figure 4: The landslide cross-sections suggest strong lateral variations in landslide thickness. Could the authors elaborate on the possible effects of this complex (3D) geometry on the measured kinematics? Could these characteristics account for the differences in behaviour between Jettan and Gàmanjunni without invoking permafrost controls? As noted above, this represents a major issue in the manuscript: the authors present very interesting data and a thorough description of the kinematics of these landslides, yet the interpretation of the controls on the observed behaviour does not appear to be fully supported by the data analysis.

Answer 34: As mentioned earlier (see our Answers 1 and 20), we will add cross-sections with more detailed geologic-structural models. The boreholes aiding the interpretation of the sliding zones are point measures. Cross-section B-B' has two boreholes, and A-A' has one. If there was a lower borehole along A-A' the interpreted shear plane could be equally deep, but we do not have that datapoint and therefore extrapolate a line from the data we have towards the surface. However, related to the variation in landslide thickness, this will invoke larger driving stress with increasing depth and could promote division into two separate blocks such that BH-2 represents two scenarios and BH-1 and BH-3 represents a third scenario. We have re-written text in section 4.1 at two places. 'Lithological heterogeneity at Jettan introduces multiple potential weakness zones, whereas the more homogeneous mica schist at Gámanjunni favors the development of a single dominant shear zone. Accordingly, Gámanjunni deforms as a fractured yet largely coherent rock mass along a curved failure plane (Böhme et al., 2016), in contrast to the Jettan landslide, which exhibits a compartmentalized structure with blocks moving along listric failure planes (Braathen et al., 2004; Blikra et al., 2015). In addition, landslide thickness at Jettan likely shows greater spatial variability than at Gámanjunni, further promoting differential movement (see Figures 2 and 3).'

And in the concluding paragraph: 'In contrast, spatial variability in landslide thickness at Jettan, together with listric sliding surfaces, likely enhances kinematic segmentation and internal deformation, thereby reducing slip rates.'

Regarding structural control and permafrost, this is addressed in answer 21.

Comment: Page 10, line 195: "W": how many days does W represent? The averaging time window used to calculate velocity is generally critical when attempting to correlate changes (accelerations/decelerations, i.e. "bursts") with environmental variables and should therefore be clearly stated.

Answer 36: W is 1 day for the velocity in all plots in this manuscript. We have added this information in the Methods section.

Comment: Page 11, lines 199–202: This is not entirely clear: what do the authors mean by "temporal aggregation of the time series"?

Answer 37: We have extended the explanation of temporal aggregation in the manuscript: 'Temporal aggregation of the time series was performed by resampling the data to coarser temporal resolutions using the method resample method (Pandas Developers, 2025a). Within each resampling interval, values were aggregated using either the sum or the median, depending on the data type: precipitation variables (rain and snow) were summed, while temperature and displacement data were aggregated using the median. This procedure resulted in regularized time series at daily, weekly, or monthly resolution, depending on the requirements of the specific analysis.'

Comment: Page 11, lines 203–209: This is a valuable component of the analysis, technically described in the appendices, but not fully exploited to provide an integrated understanding of the relationships between rockslide mechanisms, kinematics, and environmental controls. As noted in

the general comments, this seems to be a limitation of the study: the authors performed sophisticated analyses of excellent data, but the interpretation is not framed within robust geological/technical models of the studied landslides, resulting in rather speculative conclusions that do not appear to contribute substantially to current landslide forecasting capabilities.

Answer 38: We thank the reviewer for recognizing the technical quality and value of the datasets and analyses presented in this study. We respectfully disagree with the implication that the conclusions are speculative in the sense of being weakly supported or insufficiently grounded in geological or mechanical understanding. The interpretations presented are based on consistent kinematic patterns with systematic relationship to environmental forcing. The aim of the manuscript is not to exhaustively explore all possible mechanical processes or to develop operational forecasting tools, but to interpret a unique and comprehensive monitoring dataset at an appropriate level of synthesis. Given the complexity of natural landslide systems, and the unavoidable limitations in subsurface constraints for Gàmánjunni, any interpretation necessarily involves a degree of inference. We view this as an inherent aspect of landslide science rather than a shortcoming of the study. However, the comment of the reviewer might tell that clarifications are required to separate clearly the results from the discussions. For this reason, we have separated the observations (Result section, ground truth) from the discussion (Discussion section, possible explanations with the available data), and also added several new Figures including integrated cross-sections.

Comment: Page 11, line 219: "the model allows predicting": if I understand correctly, the aim of the ARDL regression model is to quantify the relative contributions of different controlling variables (e.g. rainfall, snowmelt, temperature) to the measured displacements, rather than to make predictions about future landslide behaviour. If so, I would suggest the authors state this clearly to avoid confusion. Could the phrasing be changed to "the model allows explaining a time series..." instead of "predicting"?

Answer 39: We have implemented this change and the reviewer has understood correctly that we wanted this model to quantify relative contributions rather than predicting. The prediction was meant to show the validity of the quantified contributions.

Comment: Page 12, lines 236–238: "since the process is computationally intensive, we down-sampled the time series to weekly intervals...": I understand it. However, did the authors perform an a priori verification of the typical response time of the landslides to the triggers? Is a weekly sampling rate sufficient to capture the observed processes? There is a risk of feeding a sophisticated model with data at an inadequate temporal resolution. Could the authors elaborate on this point?

Answer 40: We have compared a smaller sample (40% of the data ~2.5 years) of daily data to the identical dataset down-sampled to weekly intervals. The variables used were borehole displacement, borehole water pressure, and air temperature. The contributions in both models are similar, where the daily data showed a lag of 5 days for borehole displacement, and 0 day lag for the temperature, and the weekly data showed 1 week lag for borehole displacement and 0 week

lag for temperature. In general, the preparation process before landslide failure can last for years, and the Norwegian authority for landslide warning (NVE) expects to increase the hazard warning weeks before an expected failure. So, based on current knowledge and practice, we consider a weekly sampling would be sufficient, however, it would be more robust with daily data, but the model cannot handle it over such long time series. We clarify this point in the manuscript.

Comment: Page 12, line 262: "7.8 mm/yr": what is the accuracy of this velocity estimate? Lines 287–289 on page 14 suggest an error of 1 mm/yr, in which case it would be more appropriate to round the value to, e.g., "8 mm/yr". Please consider this here and elsewhere throughout the manuscript.

Answer 41: Indeed, the uncertainty of the GNSS measurements is around 1 mm/yr. We have adjusted these values throughout the manuscript.

Comment: Page 15, Figure 6: What criterion was used to identify the outlined "acceleration periods"? As noted above, an objective criterion is required to define such events when attempting to correlate them with measured (or simulated) environmental variables. In this figure, while seasonal accelerations at shallower levels are clearly visible, those in borehole shear zones are less distinct and resemble other similar signals that were not identified as acceleration periods. How were these selected? Clarifying this point is essential for the correlations proposed in Section 3.4 to be credible to the reader.

Answer 42: This comment is addressed in answer 22.

Comment: Page 17, lines 344–345: "how shear zone slip velocity responds to borehole measurements" — what does this mean? Is not shear zone slip velocity itself the quantity measured in the borehole? I suggest the authors clarify this. As already noted, the presentation of results is somehow difficult to follow, being descriptive and lacking a clear framing within a geological landslide reference model. Furthermore, the same analyses are not consistently available for both Jettan and Gàmanjunni (no borehole measurements at the latter), which hampers a complete comparison.

Answer 43: 'lines 344–345: "how shear zone slip velocity responds to borehole measurements" — what does this mean?' We agree that this sentence can be confusing. Indeed, borehole measurements consist of inclinometers (used to measure the slip velocity) but also the groundwater pressure. Hence, we have re-written the sentence into 'how shear zone slip velocity responds to borehole groundwater pressure measurements and meteorological factors'.

NVE would also like to obtain data from boreholes at Gàmanjunni, but as of now, the landslide has not yet been drilled due to difficult access and huge costs.

Comment: Section 4.1, pages 19–21: As discussed above, I am not convinced that comparing the displacement patterns of the two landslides and their correlations with the considered

environmental variables can yield conclusive evidence regarding the influence of permafrost on landslide behaviour. Despite their proximity and generally similar geological settings, the two landslides differ significantly in geometry and structure (as the authors themselves acknowledge on page 20, lines 396–413), and these differences could account for the observed contrasts in behaviour. Moreover, permafrost occurrence at Gàmajunni is inferred rather than measured. In this context, the interpretation that "a combination of a well-developed shear plane, coherent rock mass movement, and thawing permafrost inside the landslide could be the key factors driving Gàmajunni's faster displacements" is reasonable but remains rather generic and poorly conclusive. I suggest the authors elaborate carefully on this and better support their conclusions with data.

Answer 44: We agree that the presence of permafrost in Gàmajunni is not confirmed and that drilling boreholes would be needed to solve this question. However, simulations show that the presence of permafrost is possible. This is why it is difficult to conclude firmly on the main control parameter. We have kept this paragraph as it is and added a sentence. "To conclude on the role of permafrost in Gàmajunni, boreholes should be drilled, and it could be the topic of future studies."

Comment: Sections 4.2, 4.3 and 4.4: I found these sections very interesting. They provide a rare example of a description of rockslide deformation mechanisms in space and time, based on high-quality multi-parametric monitoring data, and are well supported by data — except for a few statements that appear somewhat speculative (e.g. page 22, lines 475–476). Nevertheless, this part could serve as the subject of a separate paper, as it is dedicated exclusively to the Jettan landslide (where borehole data are available) and diverges from the main narrative and scope of the paper as established in the preceding sections. I encourage the authors to try to reconcile and harmonise the different parts of the manuscript within a single, coherent narrative framework.

Answer 45: We prefer to keep the 'speculative statement' on line 475-476. This sentence is an hypothesis and formulated as this. We also prefer to keep these sections because they allow us to discuss landslide deformation in a broader context. We also mention clearly that an important difference between Jettan and Gammanjuni is the amount of data (and instruments) available at each site.