Geoscientific Instrumentation Methods and Data Systems Discussions

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Comment on gi-2021-5

Anonymous Referee #2

Referee comment on "Observation of the rock slope thermal regime, coupled with crackmeter stability monitoring: initial results from three different sites in Czechia (central Europe)" by Ondřej Racek et al., Geosci. Instrum. Method. Data Syst. Discuss., https://doi.org/10.5194/gi-2021-5-RC2, 2021

The paper entitled « Observation of the rock slope thermal regime, coupled with crack meter stability monitoring » presents a monitoring system of the thermal and rheological behaviour at the surface and near-surface of three rock wall sites in Czech Republic.

The paper is overall well-written and easy to follow, the monitoring approach is interesting but significant improvements are required before publication.

My main concern is that the authors claims that the monitoring system is original and innovative because of its completeness and affordability. While this is true that the system is affordable, it is, in my opinion, not unique and not so innovative : many sites are equiped with both ground temperature measurements in shallow boreholes and crackmeters (e.g. Weber et al., 2019, Ewald et al., 2019, Hasler et al., 2012, Gischig et al., 2011, in addition of those already referred). More explanations would be needed to really understand how novel is the system: how (precisely) the data will contribute to improve the understanding of rock fall preparation and triggering that the other system do not allow ? Will it provide data for thermo-mechanical models ? How such improvements can be made ? Which paramaters, which process in the models ? (See more detailed comments).

Moreover, many data are presented without being discussed nor used for research perspectives: e.g. Table 2, 3, Figure 3, … This makes the paper quite long without improving its impact. I suggest to rework the paper top make sure that the data that are presented are clearly used for the results, discussion or explanation of research strategy.

Other major comments:

- Manuscript content and objectives: they are very vague, specific research questions and a clear research strategy should be cleary explained and detailed.
- Title: the title should report at least the study area (Czech Republic, 3 sites) and environnemental settings
- The abstract needs to be rewritten. It is vague and general. It should be more precise: how many sites intrumented, which environemental settings (elevation range, rock type, etc.), where are these sites, when did the record start, how long are the type series, what type of differences are measured…
- Introduction: It is generally quite long, lines 47-74 are a long list of some of the existing instrumentation to monitor and detect rock slope deformation and failure. It is too long and too detailed and I didn't get the purpose of such a long list which is somehow summarized line 73-74 but is not convincing. I am not convinced that approach presented in this pape ris so differentthan many other sites, except maybe that instrumentation is relatively affordable. In the introduction, the hydrological processes are not considered at all while they represent a major external forcing in rock falls (see for example Krautblatter and Moser, 2009). I suggest largely rewritting the introduction to make it more concise and better introduce the approach presneted in the paper in order that the reader understand why it is so different than the others. In my opinion, the quetsion related to the choice of the fractures and blocks to instrument with crackmeters is still open, and many other studies combined such point-scale geotechnical observation with geodesy data to apprehend rock deformation at a larger scale as well, which is after all, a more complete approach than only the geotechnical approach.
- Section 2.3: the same concern arises. Words such as « complex monitoring », « innovarive » are used but I still do not understand why the approach is so innonvative. Many other studies also combines shallow borehole temperature measurements with crackmeters.
- It would be interesting for the reader to know if any instrument was calibrated or not.
- Another concern : the instrumentation do provide any data about the temperature and the mechanical behaviour of the failure pla, which, in my opinion, strongly limits the interest to understand failure mechanisms.
- The paper is long with lot of tables and figures that are not necessarily relevant for understanding the approach. I suggest to create supplementary material or to barely remove information that are not a direct relevance to understand the strategy behind the instrumentation (see detaield comments).

Detailed comments :

- Line 33: permafrost doesn't melt, it thaws.
- Line 38: see also the PERMOS reports from the Swiss permafrost monitoring system.
- Line 43: « Unfortunately… » : such formulation is not appropriate in a scientific paper
- Line 55-56: there is something wrong with this sentence, rephrase.
- Line 83: what is a « 2D environment »?
- Line 95: is teh monitoring system the same for each site?
- Table 1: why some items have o price?
- Figure 2: remove « so far » from the caption, it is not appropriate

- Table 5: over which period are the radiation measurments available?
- Line 268: number of the figure is missing
- Line 269: the operating time is not relevant here as all sensors seem to record the greatest amplitude within this period on Fig. 5. If not, clarify.
- Line 271: I do not understand this statement: the temperature amplitude decrease with depth is related to the thermal diffusivity, not the albedo.
- Line 272: how sure is this statement ? Please refer your interpretation to facts/proper observations or do not interpret.
- Figure 6: explain teh statistics displayed with the boxplot in the caption: median… ?
- Line 280: what is a significant opening? Does « significant » have a scientific meaning or definition in this context?
- Line 290: same question with « relatively high »
- Line 293-294: this si very speculative as the time series is very short. Please, base your interpretation on facts rather than speculation. The interpretation might change completly with a longer time series.
- Line 295: how does a block destabilization trend is expected to look?
- Line 313 ff: the present forcings are not the only factors of rock stability. Past conditions and events (climate change, former rockfall, …) have to be accounted for as well. In addition, the hydrological processes must be considered.
- Line 323: the automatic discontinuity extraction approach needs some detailed explanation.
- Line 335-340: this is part of the method, not the discussion !
- Table 7: I also wonder if this is relevant in the discussion. This would be better to explain why the monitoring system of this study is so innovative.
- Table 8: This is part of the results.
- Figure 8: idem.
- Table 9. Idem, and I do not really see the relevance of giving so much details.
- Line 366-367: explain this statement and appropriate references.
- Line 372: explain the concept of « rock disintegration » and how temperature change act for such process.
- Line 376-377: to determine the place of the potential rock failure, quantitative understanding of the mechanical properties of the failure plan would be required. Similarly, the measured temperature in compact rock is not representative of the temperature in fractures. See Hasler et al., 2011 for example.
- L 390: the sentence is difficult to understand. Please rephrase.
- Line 412: this statement is ehre again very speculative in the absence of proper comparison of climate data.
- L 440: these perspectives should be cleraly detailed in the discussion. The reader needs to understand the research strategy and how the data will be used for the implementation of the perspective.

References

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