general comments

The manuscript entitled "Assessing the impact of climate change to landslides using public data, a case study from Vejle, Denmark" exploits publicly available monitoring and modelling data of surface deformation, water table depth and precipitation in order to investigate the effect of climate change on the behaviour of deep-seated landslides in a coastal area in Denmark. This manuscript provides insights into a highly relevant field of research. Although the manuscript is well-structured and written in an understandable manner, some methodological concerns arise.

The authors use publically available data and state-of the art analysis tools in a rather conventional workflow missing innovative aspects. Furthermore, I see a huge unused potential in the work presented and encourage to include some of the following aspects:

- The authors propose their workflow to be replicable and applicable to other case studies. I think this it is a missed opportunity to really proof it's applicability at other landslides. Since the data is already available, I think this would have been an easy but highly profitable task.
- I can't really follow why the study has only used the far climate projection data for the period 2071-2100. In my opinion the period from today to 2070 is at least equally (if not more) relevant.
- The specified model uncertainties of the groundwater model are :<1m (L220). On the other hand, one of the main findings shows that climate change will increase the WTD by +0.7m (Fig 7 and L351, 395). In addition, the 0.7m increase represents the upper limit of the 95% confidence interval which is by far higher than the median increase (which regarding to figure 7 is in the order of +0.2m for RCP 8.5). This discrepancy between model uncertainty and predicted changes of WTD needs in-depth argumentation and check for significance. Overall, I get the feeling that the argumentation suffers from issues within the applied statistical approach.
- Reading the research questions in the Introduction "…*With this increasing availability of new public data in mind, we set out to answer the question: How will large coastal landslides respond to future climate change? And how far can we get towards answering this question using freely and publicly available data?..."* and comparing it with the content of discussion or conclusion I am missing more detailed answers and discussion of the initially stated questions. Especially in section 4.3 ("*Limitations and benefits …"*) I would have expected more details, particularly when it comes to transferring your approach to other case studies I assume there are way more limitations than listed. (e.g. InSAR limitations regarding geometry and LOS issues, vegetation, snow-cover, displacement rates exceeding wavelength associated thresholds…). It would be great to tell the reader how your worklflow was able to tackle these issues (e.g. by using DoD) and what limitations are still unsolved.

Specific comments

Abstract. Clear and quantitative statements are missing. It would be great to provide the reader clear and concise outcomes of your study in terms of numbers. By this I do not mean the WTD elevation and how it will change in future (since this is already contained in the public data) but more the outcomes from your own workflow and the combination of WTD and EGMS/DoD data. In my opinion the main interest is on how will the landslide activity behave in future.

L16 The 0.7m represent the upper 95% confidence interval (CI). In my opinion this is not the right measure to be provided here. At least you should state both (upper and lower) CI limits. From my point of view, the specification of a median and a measure of variability (e.g. Standard Deviation) is mandatory in this context.

For example in L398 the authors argument based on their findings of an climate change-induced increased in WTD : "…This will overall lead to increased seasonal landslide activity." What I am missing here is a more detailed determination on how the expected increase would change the landslide's kinematics. Since there have been relations elaborated between landslide deformation and WTD (e.g. Figure 6 and section 3.2) I would recommend to at least visualise this correlations (e.g. in a scatterplot X axis: WTD and y-axis: landslide displacement) or use the correlations for estimating the potential effects of future WTD on future landslide deformations. I think this would be a valuable information in better understanding the correlation coefficients. Furthermore, the ability for fitting statistical models by regression analysis could have been exploited and further used to determine potential landslide activities based on the climate projection WTD data.

I think it is a great idea to integrate LiDAR derived DoDs with InSAR time series. What generally is missing, are the different characteristics of the datasets. They have different advantages and disadvantages and the approach to integrate both data sets is not an easy task. In the manuscript this is somehow missing. Whenever the authors present deformations of both datasets in mm per year. their meaning is totally different. This issue and opportunity at the same time could be discussed in more detail.

technical corrections

L14 abbreviation needs to be explained at their first occurrence. Please check the whole document

L24 pronounced topography?

L27 some other studies have also shown that landslide activity could be reduced due to climate change e.g. Malet et al. 2005; Coe 2012, Zieher et al 2022

- Malet J-P, van Asch TWJ, van Beek R, Maquaire O (2005) Forecasting the behaviour of complex landslides with a spatially distributed hydrological model. Nat Hazard 5(1):71–85. https://doi.org/10.5194/nhess-5-71-2005
- Coe JA (2012) Regional moisture balance control of landslide motion: implications for landslide forecasting in a changing climate. Geology 40(4):323–326. https://doi.org/10.1130/G32897.1
- Zieher, T., Gallotti, G., Rianna, G. et al. Exploring the effects of climate change on the water balance of a continuously moving deep-seated landslide. Nat Hazards 115, 357–387 (2023). https://doi.org/10.1007/s11069-022-05558-7

L28 "…but relatively few studies have quantified the extent of this effect." It would be valuable to cite and briefly describe some of these studies

L32 suggest to use either fluctuations or dynamics

L33 What are thresholds for landslide movement in this context? I think this needs a more detailed explanation. Especially in the context of continuous and slow-moving landslides.

L45 associated to what? Climate change?

L47 I am not a 100% convinced by this general statement. I would recommend to be more precise. What specifically are the changes in seasonal precipitation regime and for what kind of landslide type is this valid? I would assume there is a difference for example between delayed responding, large and deep-seated landslides compared to fast responding, smaller, shallower and rapid landslides

L65 I would strongly recommend to not use the word 'good' in this context. E.g. you could use 'appropriate' instead

L69 "… firstly measure weekly …" I suggest to rephrase this, since I assume the InSAR measurements were not directly done by yourself?

L71 lead to accelerated landslide movement

L81 Figure 1d: It would be great to have a legend explaining the colour gradient. I assume it's according to the elevation? In general, the figure captions within the whole manuscript could be shortened by including more legends and descriptions directly in the figure.

L95 is this a monthly mean? Or calculated for a certain period? I would also suggest to add a reference from where the data was retrieved.

L96 It could be mentioned if snowfall and delayed snowmelt plays a role in the context of the explained hydro-climatological setting

L100 landslides hosting houses sound a little bit strange to me. Could re-reformulated.

L103 Looking at Figure 1d I guess a DTM (Digital terrain model) was used in order to avoid the influence of Vegetation and infrastructure? Please specify. In addition, I also think that for the morphological mapping a shaded relief image retrieved from the DTM was used. Please specify in case.

L105 the indications of recent activity could specified

L113 in the shaded relief image derived from the DEM

L119 makes it possible sounds unusual for me. I suggest to use: enable OR aims at, in this case

L122-123 please highlight in more detail what you mean with novel. This should also be part of the introduction. In there, it should be made very clear what was already done in other studies and what is the novelty of your study. For example correlating InSAR landslide displacement time series with hydrological timeseries (either modelled or monitored) alone was done more than a decade ago (e.g.: Ambrosi and Crosta 2006 doi:10.1016/j.enggeo.2005.06.031; Frattini et al. 2018 DOI 10.1007/s10346-017-0940-6).

L126 The term threshold requires more explanation. Please describe what you mean by climatic thresholds.

L 125 I miss a description of the workflow in the manuscript. I think besides of just referring to figure 2, a comprehensive summary of individual steps accomplished within the framework should be given in this section. In this context a concise and more detailed information about your novelty and improvements could be mentioned. Although it is mention in L123 which steps are novel and which are considered "standard", I think this demands further descriptions and explanations what exactly the novel part of your study is and how it fits in existing literature.

Figure 2: In my opinion the font size needs to be increased

L 141 Could you specify which classification approach you used to remove above ground objects?

L 142 Since vegetation and buildings have been removed, you could specify the digital elevation model as a digital terrain model (DTM)

L143 I assume you used a shaded relief image derived from the DEM/DTM to map morphological features (as it is shown in Figure 1d and 3). This and the approach you used for deriving a shaded relief image could be mentioned.

L148-149 Slope parallel transport determination is not possible with the DoD. But with the same data availability other methods could be applied to detect slope parallel or even 3D transport, for example by using image correlation or point cloud-based approaches: Dille et al. 2021 (doi: https://doi.org/10.1016/j.rse.2021.112402), Pfeiffer et al. 2018 (doi: https://doi.org/10.3390/rs10111688).

L151 Also here please provide information how you determined the precision, Please also check for using the right terminology (precision vs accuracy)

L154 In case PS (persistent scatterer) is understood by "point data " I would make this more clear here.

L154 instead of "validate" you could use "compare"

155 InSAR anomalies could be explained

L 160 it is worth mentioning that these "points" represent persistent scatterer

L171 is the direction of 70°-80° equal to east-north-east ENE? Could be included for better coprehensibility.

L 172 "of the actual movement" please define which landslide is meant

L 175 in my opinion data smoothing and especially the window-size of 90 days requires justification

L180-L185 This paragraph is a bit out of context here. I would expect it at the beginning of the section. Although describing the general functionality of PSI is not necessary in my opinion. In fact, I think it is more important to give profound information of what is possible and what stays undetected within the EGMS data in regard of landslides.

L203 -212 I get a bit confused by the different models and discrepancy of resolutions. There are model results in 100m (L203), 10m (L209) and 500m (212) available. Looking at table 1 there is only the 100m resolution data set.

211 Climate change impact simulations- Please specify, simulations of what? WTD?

L214-L216 This sentence could be at the beginning of the section. Reading the above paragraph, I got confused by all the different WTD model data. I would suggest to rephrase it in a clear and structured way. For example: first stating where the data was retrieved from and then for which periods and timesteps you have hindcast, forecast data in which spatial resolution and how it was modelled.

L218 Please provide information about the aggregation method used (mean? median?)

L220 a brief information about model validation strategy could be given to better understand the model's quality/performance rather than only specifying a mean error for an unknown validation period

L221 not sure if I get the right message here. What do you mean with: " the model was not intended to be used at a 100 m grid scale?" I think this needs more information.

L224 “.. obtain a robust and realistic time series …“please explain why a normalized and aggregated time series is more robust and realistic than the "raw" data. There are essential numbers/methods missing for the reader to understand what was done here.

L224- L227 This is not comprehensible for me

L234 why only far-future situation and not also the 2041-2070 time frame as provided by the DK-HIP model data?

L262 This is quite a magnitude and worth mentioning that this can only be determined using DoDs and not InSAR. Also when looking at Figure 1b I assume this impressive subsidence shown happened in a relatively short period of time (maybe even within two individual InSAR acquisitions). How can you be sure, that 650mm subsidence prevail per year when you have a time period of 4 years between LiDAR acquisitions?

L 264 please specify what kind of movement (horizontal, vertical, along LOS) you mean here

L279: again you have ALS acquisitions from 2014 and 2018. Specifying a displacement rate per year is not a solid assumption since it is not known if there have been years with fast or slow movement in between. I would suggest to rephrase this

L280 I would use persistent scatterer instead of InSAR points

L289 Could this be reported more precise? what do you mean with rapid here?

L293 “…bar of clay…” - I can't follow what you want to tell the reader here

L305 for better comprehensibility, I would recommend to specify the period of long-term

L306 “… c. …” what does this mean?

L307 so 2m variation determines if the groundwater level is shallow or deep?

L311 could you specify the differences in uncertainty using statistical measures?

L332 have you checked if there is a time-delayed correlation between landslide velocity and cumulated precipitation?

Figure 6: why are there no InSAR data points for Svinget landslide between end of 2015 and beginning of 2016?

Figure 7a shouldn’t the red asterisk be more on the left? And also what is not clear for me, are the confidence intervals shown in Figure 7a referring to the historic period 1990-2019? Is it a coincidence that the red and blue asterisks indicating the maximum WTD values within RCP4.5 and 8.5 are placed exactly on the interval limits? I would suggest to change the color for the mean. It could be misunderstood with the same-coloured horizontal line at 0.

L 360 period 2071-2100

L371 no correlation maybe because of an unconsidered time delay between rainfall and landslide acceleration

L372 Although you have used already existing model results. I think it would be great to give more details about model parametrisation and validation strategy that could be discussed here. In my opinion this would be of high interest for the readers and will also give a better comprehensibility of the model's quality. which is important in case your workflow will be applied to other case studies

Hydrogeological processes can be very complex. especially in the setting of deep-seated landslides typically featuring hydraulic heterogeneities at a local scale. Could you give more details how and in which detail this is considered in the modelling framework?

376 I do not agree that precipitation is evenly distributed throughout the year while examining figure 6a. I see cumulated rainfall especially during winter whereas during summers there is generally less precipitation Also in the study area description you stated: "Mean annual precipitation in Vejle is 766 mm/y which is distributed across the year but more intense in the fall season." This is also not in line with your discussion statement set here

L384-L390 This is out of context in the discussion here - I would either expect this paragraph in the Introduction or as a point of discussion in regard to the findings of your study. This I think is missing here.

L395 weak statement. not convinced by this. so +0.7m is the upper level of the 95% confidence level. looking at figure 7c. the lower confidence level (5%) is somewhere at -0.5m meter. So I agree that you observe a general shift towards positive changes in WTD but your confidence region says that you can still have negative changes. The historic reference in figure 7b and 7c is specified as a single line. Why do you not also show the confidence intervals of the historic period? Please double check your statistical analysis and statements based on that. You could check your statistical analysis for significance.

L432 frequency of occurrence? I find it always difficult to apply frequency of occurrence to slow moving landslides. Since once they occur they can accelerate or decelerate. The frequency of occurrence of reaching a certain velocity threshold for example could be used as an indicator.

L439 The usage of different climate models will also result in different predictions of future precipitation. It could be mentioned and discussed, that in your study only one climate model was used.

L448 remotely sensed and modelling data