Comment	Answer
J. Pfeiffer, Referee #1	
This manuscript provides insights into a highly relevant field of research.	We thank the reviewer for the positive and thorough review
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.	It is not clear from r#1's comments what innovative aspects he is suggesting we explore. We have, to the best of our abilities tried to answer all r#1's question below.
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.	It would of course be beneficial to expand with an auxiliary site but it is beyond the scope of our present manuscript to expand the study. Luckily, we have very few geohazardous landslides in Denmark (Svennevig et al 2020, Luetzenberg et al 2022) and we do not know of any other sites where this approach would be suitable in Denmark.
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 projected increase in precipitation is larger for the far future than for the near future (Pasten-Zapata et al 2019). This makes the expected impact on rising groundwater levels most significant for far future conditions: i.e.: the signal is stronger. Nevertheless, we agree that climate change adaptation should also consider the near future perspective. In the revised manuscript we will elaborate more on the near future and far future projections of precipitation for Denmark and their implications for groundwater level rise and landslide risk.
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).	We agree, the model uncertainty must be considered for impact analyses. However, here we are presenting relative differences of a reference run for a historic period and a future impact simulation and we expect that the model error behaves similar in both simulations which leads us to the conclusion that errors cancel each other out. We will make this clearer in the revised manuscript
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	Landslides are triggered by extreme WTD, not a rise in the mean WTD that is why we chose to use the 95% confidence interval. The applied HIP model has not been designed to adequately capture extreme events. It has been set up and calibrated to

argumentation suffers from issues within the applied statistical approach.	represent average conditions. A model tuned to represent extreme WTD is required to follow the reviewer's suggestion and is beyond the scope of this paper since we only focus on open and already existing data. We will make this clearer in the revised manuscript
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.	We will expand section 4.3 to address these issues
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.	We will redraft the abstract to focus more on the outcome of the study and quantify these.
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.	Landslides are triggered by extreme WTD, not a rise in the mean WTD that is why we chose to use the 95% confidence interval. We will make this clearer in the revised manuscript
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.	The expected increase in WTD will lead to lower friction on the basal surface of rupture causing the rotational landslide to adjust to these new conditions by increased landslide activity. We will work towards elaborating this in the revised manuscript
Since there have been relations elaborated between landslide deformation and WTD (e.g. Figure 6 and section 3.2) I would	We have worked along the lines of thought of the reviewer but the time series of the

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.	publicly available data is not long enough for a good correlation analysis. We will add a figure (scatterplot) to the review to show this.
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.	We will expand the integration of InSAR and DoD throughout the manuscript specifically in the method section and in section 4.3
technical corrections (see pdf)	We will address all the technical corrections in the revised manuscript

Luetzenburg, G., Svennevig, K., Bjørk, A.A., Keiding, M., Kroon, A., 2021. A national landslide inventory of Denmark. Earth Syst. Sci. Data Discuss. 2021, 1–13. https://doi.org/10.5194/essd-2021-414

Pasten-Zapata, E., Sonnenborg, T. O., & Refsgaard, J. C. (2019). Climate change: Sources of uncertainty in precipitation and temperature projections for Denmark. *GEUS Bulletin*, *43*.

Svennevig, K., Luetzenburg, G., Keiding, M.K., Pedersen, S.A.S., Asbjørn, S., Pedersen, S.A.S., 2020. Preliminary landslide mapping in Denmark indicates an underestimated geohazard. GEUS Bull. 44, 1–6. https://doi.org/https://doi.org/10.34194/geusb.v44.5302