Dear Professor Margreth Keiler, Editor

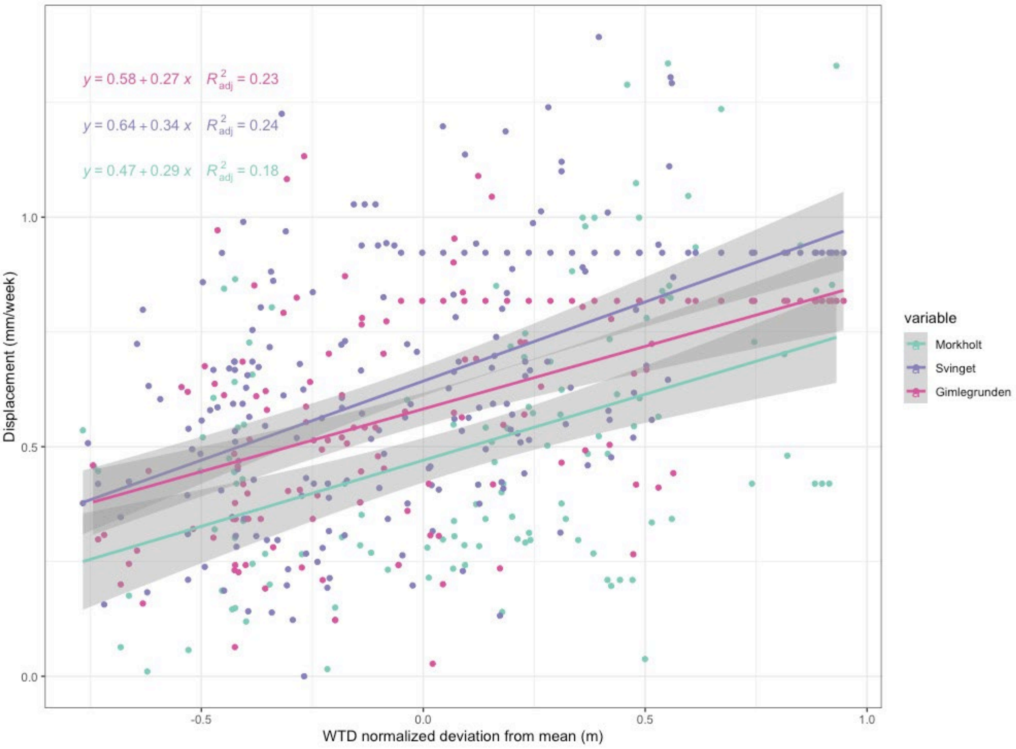
We are pleased to submit our revised version of manuscript nhess-2023-68, now titled "Assessing the impact of climate change to landslides at Vejle Denmark, using public data". We have addressed the comments from the three reviewers and from you to the best of our ability and made substantial changes to the manuscript. We believe it has clearly improved our work, especially sharpening the novelty of our research. To our knowledge there is a clear national and international novelty argument, i.e. first study to thoroughly combine InSAR, water table depth (WTD) and DEM data on Danish landslides, the first multivariate landslide study using only publicly available remote data. Both points forward with respect to further research in Denmark and internationally, where the amount of publicly available data is ever growing.

We have thoroughly revised the introduction and methodology, as suggested by reviewer 1. Regarding reviewer #1 and #2's comments to expand the correlation analysis, we have expanded our analysis using the suggested methodology and achieved the same result as presented in the original submission. Specifically, the WTD accounts for 18 to 24% of the variation in weekly landslide movement at the three investigated landslides (see fig below). We argue that this correlation supports the same relationship between WTD and landslide movement that is also shown in the original submission. Since the new analysis did not contribute to any additional findings, we have not included it in the revised manuscript.

We appreciate your consideration of our revised manuscript and look forward to your decision.

Sincerely, on behalf of the authors

Kristian Svennevig



Dear Dr. J. Pfeiffer

Thank you very much for your careful and thorough revision of our manuscript. We have addressed all your comment in the revised manuscript and elaborated on the changes in the answers to reviewers below here. We hope you will find these changes satisfactory.

Kind regards, on behalf of the authors

Kristian Svennevig

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 bet of our ability answered r1's other questions and hope to have addressed these methodological concerns in doing so.
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.
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 have elaborated 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 have made 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 argumentation suffers from issues within the applied statistical approach.	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 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 have made 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 have expanded 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	We have redrafted the abstract to focus more on the outcome of the study and quantify these.

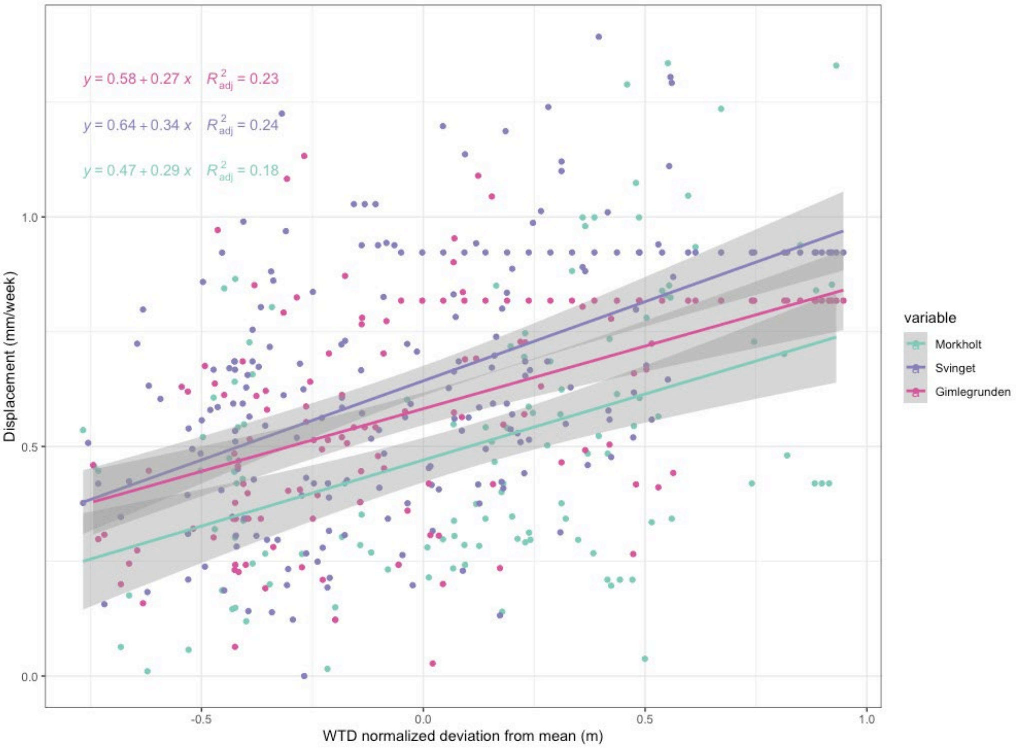
outcomes from your own workflow and the combination of WTD and EGMS/DoD data. In my opinion the main interest is on how will the landslide activity behave in future.	
L16 The 0.7m represent the upper 95% confidence interval (CI). In my opinion this is not the right measure to be provided here. At least 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.	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 have made 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 have elaborated 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 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.	We have worked along the lines of thought of the reviewer but the time series of the publicly available data is not long enough for a good correlation analysis. We have added a figure (scatterplot) below to demonstrate this. This plot ads little to the plot already in the ms so we've decided not to include it in the ms.
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)	All the technical corrections have been addressed in the revised manuscript.

	Details of some of these are discussed below
Line 354 Could this be reported more precise? what do you mean with rapid here?	Unfortunately we have not been able to quantify this historic event any further as the reconstruction is based on eye witness accounts.
Line 404 R1 Figure 6: why are there no InSAR data points for Svinget landslide between end of 2015 and beginning of 2016?	This is already described in line 204 "Data are lacking for Gimlegrunden and Svinget landslides in the winter 2015/2016 due to an acquisition error observed in all 117A tracks across northern Europe"
Line 440 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?	The hydrological model is a coupled surface-subsurface model. Infiltration in the unsaturated zone is simulated 1D whereas 3D flow is simulated in the subsurface. The hydrogeological model is based on the national borehole database and geophysical data. The model is calibrated and evaluated against groundwater head and river discharge. In the calibration process, key parameters like hydraulic conductivity are estimated. A concise sentence is added to the manuscript to offer more details to the readers
Line 445 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	We have downtoned our formulation in section 1.1 Our point was to underline that there is no dry and wet season in the Danish climate. Yes, precipitation varies, but there is no clear rainfall seasonality. The statement here is still valid. The temporal dynamics of WTD are primarily driven by the seasonality of temperature and potential evapotranspiration. The seasonality of temperature is much stronger than the precipitation variability
Line 455 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	We argue that landslides are triggered by extreme high WTD. This may not happen every year and therefore we highlight mean climate change impact + 2*std (upper 95% confidence interval) in our results and discussion. The straight line at zero (historic reference) will be removed in the updated figures.

statistical analysis and statements based on that. You could check your statistical analysis for significance.	
Line 465 R1 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	We have worked along the lines of thought of the reviewer but the time series of the publicly available data is not long enough for a good correlation analysis. We have added a figure (scatterplot) below to demonstrate this. This plot ads little to the plot already in the ms so we've decided not to include it in the ms.
Line 503frequency 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.	The specific use here refers to how it is used in the reference so we prefer not to change it.
Lien 503 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.	An ensemble of 22 climate model was used in the analysis.
Line 520: 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.	Thank you for the suggestion. This is a good point. We have expanded with a paragraph on this at the ed of section 4.3

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*.

Seidenfaden, Ida K., et al. "Quantification of climate change sensitivity of shallow and deep groundwater in Denmark." Journal of Hydrology: Regional Studies 41 (2022): 101100.



Dear reviewer 2

Thank you very much for your thorough revision of our manuscript. We have addressed all your comment in the revised manuscript and elaborated on the changes in the answers to reviewers below here. We hope you will find these changes satisfactory.

Kind regards, on behalf of the authors

Kristian Svennevig

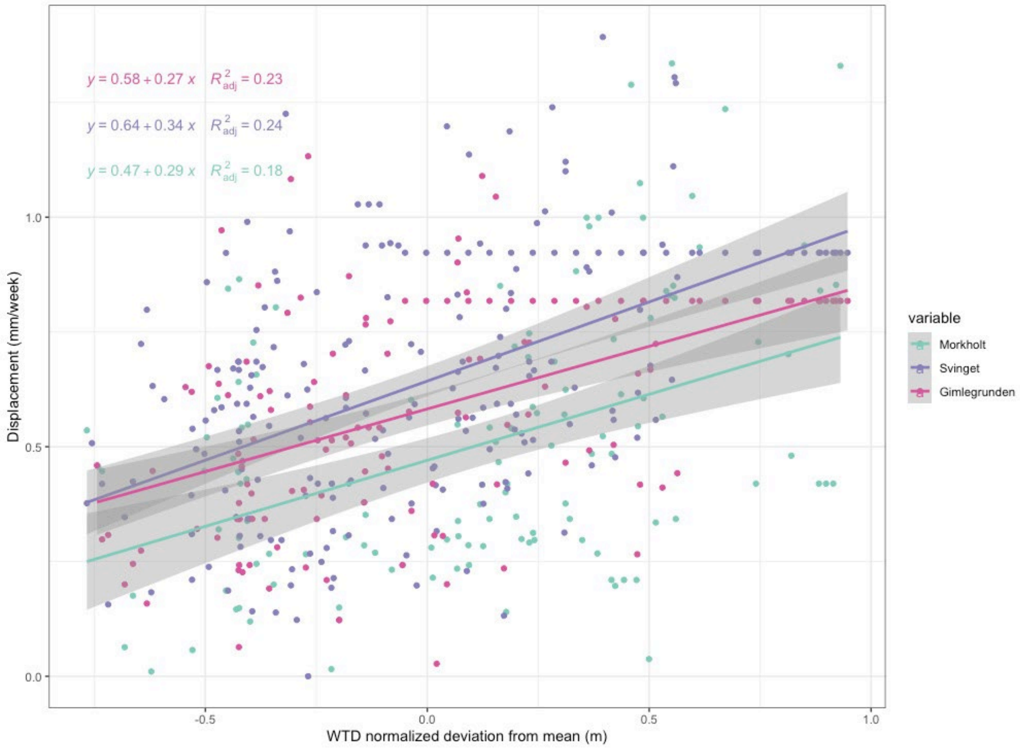
Comment	Answer
The manuscript is well written and the topic is within the scope of the journal. In my opinion the manuscript has moderate scientific novelty. Also some points may be better discussed and framed, for instance the low correlation between displacement and rainfall, and climate change modeling.	We have elaborated on the novel aspects of the work throughout the manuscript
L 147: Here you state that by DoD you were able to evaluate the vertical change in elevation for landslides. But how is the procedure accurate? In other words, what happens for the areas outside the landslides? Are there changes in elevation even there? Which is their order of magnitude respect to landslide areas? More details on this should be added in my opinion to the manuscript.	We have tried to address this around line 171 in section 2.1. "accuracy of 1.4 cm between the two acquisition dates"
LL217-233: Here you describe the post-processing of the water table depth (WTD) data originally provided by the DK-HIP model. It is unclear if and how this post-processing might have affected your analysis, both in the historical and in the future periods. This issue should be further explored. For instance, what would have been the variations between future and control scenarios with the original data taken from the DK-HIP model? Would they be way more different of the results you presented? I am imagine that in spite of the bias in the WTD data, this assessment could be still done, as you would be comparing future and control scenarios have similar "biases".	We generally follow a protocol of climate change impact analysis (Seidenfaden et al 2022). The DK-HIP data used in this study has the actual WTD data for the historic period. For the Future period the mean climate change impact per month is reported. This impact is the difference between the reference model run and the climate change model run. The individual climate change model runs are not available via the DK-HIP portal. What is available are the mean impact per month across the applied climate models. The standard deviation of the impact is also available. Therefor we do not expect that the results would have been different if we would have used the original climate change runs from the DK-HIP model. We agree with the observation of the reviewer, since we are only investigating differences, the comparison is less senstive to the

	biases in the reference model run, because we expect that biases are systematic and thus also affecting the climate change model run.
LL332-333 you state that “no correlation was found between the accumulated weekly precipitation and the InSAR movement”. That is an issue of processing the data. I think that some correlation may be spotted if you consider the cumulative rainfall, i.e. by cumulating all the weekly precipitation from a starting time up to any given time. Of course a certain lag is present between cumulative rainfall and displacement as some time is needed for rainfall to infiltrate. This statement and the related discussion (cf. LL386-391) needs to be revised with a more sound interpretation of the analyses.	Rainfall is quite constant throughout the year in DK which may pose challenges in correlating it with the InSAR movement which is clearly seasonal. Correlation between accumulated rainfall and movement can be expected to be strong. Both variables are monotonically increasing. Precipitation is quite constant throughout the year in Denmark, but groundwater recharge is mostly taking place during winter when evapotranspiration is low. Again, this is the motivation behind using simulation results from a numerical groundwater model. From the movement data, we can observe that largest movement is taking place in winter. The related processes will not be conveyed by correlating against accumulated precipitation.
LL415-420 This statement is quite daring. I would be more cautious about this extrapolation of current behavior for the future.	We will moderate the statement – However, we believe that a future with WTD exceeding past levels will lead to higher landslide activity and the unique thing about the Vejle case is that we have a historical case for this.
Section 3.3 It is unclear how climate model data were used within DK-HIP. As far as I understand you just took that data provided by the public service and made some analyses of changes. It this is correct, I think that presenting just an exploratory analysis of model data available from other authors is quite limited in terms of novelty for a research paper. Please explain.	Climate data (Precip, Temp and Potential ET) where bias corrected from CORDEX (Pasten-Zapata et al 2019). The hydrological impact simulatiosn and the related post-processing (std across impact simulations and mean change of WTD) has been carried out outside this work and is publicly available data. Again the novelty of this paper is not moving beyond state the art within hydrological modeling, InSAR analysis. It is the combination of these public data in a novel way. It is correct that we utilize data from existing public databases. However, we are

	not convinced that this should limit the novelty of the research, since the available simulation results are generated using state of the art techniques and this study is the first to utilize these data to assess landslide activity in Denmark.
The discussion section presents various conceptual repetitions	It is unclear which repetitions are meant. We have rewritten parts of the discussion and hope this addresses the comment.
Technical corrections L71 correct “dynmaic". L221 we first normalize -> we have first normalized.	We will amend the suggested technical corrections.
Fig. 6 I would suggest showing the scatter plots instead of only the Spearman correlation values; this would give a clearer insight of what’s going on.	We have worked along the lines of thought of the reviewer but the time series of the publicly available data is not long enough for a good correlation analysis. We have added a figure (scatterplot) below to demonstrate this. This plot ads little to the plot already in the ms so we’ve decided not to include it in the ms.

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*.

Seidenfaden, Ida K., et al. "Quantification of climate change sensitivity of shallow and deep groundwater in Denmark." Journal of Hydrology: Regional Studies 41 (2022): 101100.



$y = 0.58 + 0.27\,x \quad R^2_{adj} = 0.23$
$y = 0.64 + 0.34\,x \quad R^2_{adj} = 0.24$
$y = 0.47 + 0.29\,x \quad R^2_{adj} = 0.18$
Displacement (mm/week)
WTD normalized deviation from mean (m)
variable
Morkholt
Svinget
Gimlegrunden

Dear Daniel Ben-Yehoshua

Thank you very much for your thorough revision of our manuscript. We have addressed all your comment in the revised manuscript and elaborated on some of the changes in the answers to reviewers below here. We hope you will find these changes satisfactory.

Kind regards, on behalf of the authors

Kristian Svennevig

Comment	**Answer**
Overall, I believe that the manuscript fulfills the journal's scientific standards and I recommend accepting this manuscript with minor revisions.	We thank the reviewer for the positive review.
Technical corrections (See pdf and revised manuscript)	We will amend the technical corrections suggested.
Line 491 does the sea level control the WTD to some extent? is this factor included in the model?	Yes. Sea level rise has been considered in the hydrological model.
Line 508 I feel like coastal landslides in general deserve a bit more attention/credit here. Many countries in the world have areas affected by these types of landslides and most of the world population lives close to the sea. Your work shows very well that coastal landslides seem to be strongly controlled by increased ground water table rather than by short term precipitation events. Can you make a statement about what your result mean for coastal slides in general? At least for coastlines with a similar geologic settings.	Thanks for the comments. Our workflow is not limited to coastal landslides so we have chosen to keep this statement general.