

Comment	Answer
Anonymous Referee #2	
<p>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.</p>	<p>We thank the reviewer for the positive and thorough review. With regards to the novelty of our research we believe that it lies in the integration of publically available data in a novel way so we can quantify the consequences of climate change on landslides. Furthermore the results adds to the growing body of evidence that climate change will result in increased landslide activity.</p>
<p>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.</p>	<p>We have tried to address this around line 149 in section 2.1. We will further expand this in the revised manuscript to address the issues raised.</p>
<p>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”.</p>	<p>We generally follow a protocol of climate change impact analysis. Our analysis is focused on mean values as the applied HIP model has not been designed to adequately capture extreme events. It has been set up and calibrated to represent average conditions.</p> <p>We make a selection of simulation grids (N=16), due to local heterogeneities, mainly related to the hydrogeology and artifacts of coastal gridcells. The model has been set up at national scale and its application at single grids scale has not been evaluate before, therefore such heterogeneities are to be expected.</p>
<p>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.</p>	<p>We will reconsult our data and moderate the statement in line 332-333 and in the discussion.</p> <p>However, rainfall is quite constant throughout the year in DK which may pose challenges in correlating it with the InSAR movement which is clearly seasonal.</p>
<p>LL415-420 This statement is quite daring. I would be more cautious about this extrapolation of current behavior for the future.</p>	<p>We will moderate the statement – However, we believe it scientifically sound 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. Furthermore it is important to be able to</p>

	communicate a case in addition to our “purely numbers” results for future outreach – especially when we have such a nice one.
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 simulation 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, instead we combine these data in a novel way.
The discussion section presents various conceptual repetitions	We will go through the discussion for repetitions.
Technical corrections L71 correct “dynmaic”. L221 we first normalize -> we have first normalized. 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 will amend the suggested technical corrections.

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.