Reviewer 2	Location	Answer
General comments		
If one wants to use the relative indicies for early warning purposes, the landslide needs to be known before the failure occurs. While large slow moving landslides often known (especially in the vicinity of urban environments), it is not always known which part actually moves. However, this is required (at least approximately) if relative indices are used, otherwise the landslide specific signal mixes with the non-landslide reference signal and thus the ratio gets less pronounced.		Thank you for this comment. Indeed, this is true. We have refocused the manuscript on characterizing landslide activity and not early warning. However, we do touch on the topic of how these indicies could be used for detection in the discussion and conclusion.
To analyze the relative ratio and its changes over time requires same or at leastvery similar conditions (e.g. land cover/use) for both regions. Otherwise thechanges in the ratio can occur e.g. due to different use of the land. In manycases (at least that I have experienced) the land cover is actually different, e.g.agriculture, pasture in the surrounding and natural vegetation on the slow moving landslide, especially when the knowledge that a slow moving landslide existsat this place (wich is a requirement to apply the proposed approach) preventspeople to use this site as agricultural area, because of the potential risk.		Thank you for this interesting point that we had not considered previously. We have elaborated on the topic in the discussion.
Alternatively if the conditions are not the same the temporal behavior of the indicesbetween these two areas need to be known (e.g. from a long-term referenceperiod), and then a relative change to the typical behavior can be analyzedin regard to potential landslide acceleration. However, this can also only be appliedif the reference behavior is similar throughout the years.		This is an interesting and promising approach that we have also included in our discussion. Thank you for suggesting it.
The discussion and analysis for relation between coherence and NDVI needs to be elaborated in more detail. Assuming a decrease in NDVI is related to vegetation decrease, as done in this paper, we expect more coherence in the period related to NDVI decrease (please see Bai et al. (2020), Scientific Reports volume 10, Article number: 6749 (2020); https://www.nature.com/articles/s41598-020-63560-0). But this is not the case here and the coherence is decreasing in the period when we see a declining trend in NDVI. This is not surprising as several studies (e.g. Van doninck et al. (2012), Hydrol. Earth Syst. Sci., 16, 773–786, 2012, www.hydrol-earth-syst-sci.net/16/773/2012/ doi:10.5194/hess-16-773-2012) have shown that changes in NDVI can also be related to changes in soil moisture depending on the elevation (See Fig. 5 and other figures in Van doninck et al. (2012)). It seems that the NDVI changes that the authors observe here is more related to soil moisture change rather than vegetation change. Please elaborate more on this in the paper.		Indeed, we have significantly expanded and restructured the discussion, in the hopes to better address the various aspects. We do not agree, however, that a reduction of vegetation should necessarily lead to increased coherence. While this is certainly true for seasonal cycles, it would not hold true if there is a continuous removal of vegetation. Indeed the Bai paper shows two distinct clusters of vegetation vs. coherence, and though they suggest a linear relationship between the two, there is no data to back this up. While it is possible that there is an influence of soil moisture on NDVI (and coherence!), it hardly explains the whole trend. VanDoninck et al. link soil moisture to radar backscatter and NDVI, but find little, or only temporally offset, correlation between NDVI and soil moisture. This is not surpising since vegetation would need some time to respond to the increased soil moisture. In our case, vegetation is decreasing when there is a suspected increase in soil moisture. We have added an extra figure in the results section adressing the spatial patterns of NDVI changes prior to the failure and will interpret those results with regard to both the vegetation and soil moisture changes, as well as discussing potential impacts on NDVI.
This is not surprising as theascending geometry suffers from foreshortening. Due to this geometrical distortionin the ascending data, I would not focus on ascending data. Rather, pleaseuse amplitude time-series from both polarimetry channel. If soil moisture is themain contributor to the coherence decrease, we would probably be able to see itby comparing the backscattering between different polarization that we have forS1. The authors have mentioned this in the paper, but have not investigated thisin detail.	Line 220	We believe that expanding our analysis to include polarimetric data is beyond the scope of this paper. However, we will address the impacts of foreshortening and will normalize our amplitude data for the varying incidence angles, to get a better representation of what the ongoing processes are and how intensity data can inform these.
The results of this pilot study show high potential to get early indicators of landslide failure. However, since it is not quite clear (as authors state by themselves) what the underlying processes are that allow to distinguish between normal accelerations and accelerations towards the failure it is absolutely not clear if this methodology is transferable to other landslides. Do you have any other examples to test your findings? If so, this would definitely strengthen your findings and raise the need to publish this manuscript. In your conclusion at L246f you also mention this need ": : : a more in-depth analysis of NDVI ratio and coherence ratio at multiple landslide sites is necessary to assess their full value."		We currently do not have other sites where we have performed a similar analysis. However, we have adressed the question of what types of landslides might show this kind of signal in the discussion section.
Specific comments		
delete "the"	Line 20	Thanks for catching this, we have corrected as suggested.

This statement is quite general. Do you have anyreferences for that statement? Since this is a major motivation why you propose a different approach than InSAR, please elaborate this part a bit more. What are thechallenges? Are these challenges, which are challenging but can be solved or dothese challenges really impede the use of InSAR in analyzing slow moving landslideaccelerations.	Line 23	We have added a paragraph describing the challenges of InSAR in the introduction: *Radar, while able to image the ground surface during all lighting and weather conditions, can be rendered useless in areas of steep topography due to its oblique viewing geometry and the resulting layover (the compression of a large area into only few image pixels) and shadowing effects (Wasowski & Bovenga, 2014; Hansen 2001). The amount of measurable ground deformation is also dependent on the viewing geometry, since radar instruments only measure the component of motion in line of sight (Massonet & Feigl, 1998). Further difficulties include the relative nature of radar measurements, making it necessary to know or assume a stable location where there is no deformation (Wasowski & Bovenga, 2014), as well as the fact that radar measurements are 2 pi wrapped, limiting the unambiguously measurable displacement to one quarter of the radar wavelength. The wrapped nature of the data requires that radar measurements are unwrapped to derive the actual displacement (in meters rather than radians; (Massonnet & Feigl, 1998; Chen & Zebker, 2002). This process is computationally expensive and phase unwrapping errors can mask the full displacement (Wasowski & Bovenga, 2014). Additionally, in order to reliably measure ground displacements, the wave scattering properties of ground targets must remain stable between two radar measurements. This similarity is expressed with the coherence metric (Zebker & Villasenor, 1992).*
This whole paragraph describes landslide inventory mapping (in terms of mappingpost failure landslides), which is not related to the scope of this manuscript. Moreover,the selection of your references seems a bit arbitrary (for (semi-)automated landslidemapping you cite a single study out of many available studies evolved in the lasttwo decades). Please cite review articles or maybe better skip this part, because asmentioned is not related to the scope of the manuscript.	Line 25 ff	Thank you for this input. We do believe that the various uses of coherence metrics to identify landslides - while aimed at mapping and not at characterizing the dynamics prior to the failure - are still an important framework for - and part inspiration - for our work. We will re-formulate this paragraph to reflect this more clearly and include additional references.
Why do you indicate “the area the area from which the May 2017 failureoriginated” by a rectangle. Wouldn’t the outline of the landslide as given in Figure 3 bemore meaningful if you refer to the landslide area?	Figure 4	Thank you for highlighting this. We have changed the plot to show the reference slope and the slide in the first plot and then left all outlines off the image. We decided not to plot the outline in each panel becasue it obscures the data to some extent.
It might be good to include the reference point also in the right plot. Thiswould give a better insight on how the stable slope behaves in comparison to the otherpoints.	Figure 5	We have plotted the mean of the 9x9 cells around the chosen reference point in the displacement plot.
“The premise is that general environmental and atmospheric changes (e.g.,vegetation cycles, meteorologic storms, ionospheric disturbances) affecting InSAR coherenceshould not vary between a landslide and its surrounding slopes.” This is truefor the latter two, but as mentioned above vegetation cycles may vary between landslidebody and surrounding.	Line 212	Thank you again for pointing this out, we have included this point in the discussion.
Please elaborate more on the steps that you have done for calculating amplitude time-series. It is not clear if sigma in Formula 5 is just the simple mean of amplitude data or the mean value after corrections (calibration, speckle filter, terrain correction etc...)	Equation 5	As presented in the paper, these are just simple means of amplitude. However, we will re-generate the time series after normalizing the amplitude data for the incidence angle.