The authors are very grateful to the Editors and Associate Editors for the kind consideration and possible publication of our article in the Natural Hazards and Earth System Sciences. The authors would like to thank all reviewers for suggesting improvements for the manuscript. Point-wise reply/answer to each comment is provided below (comments are shown in ***green bold font****, answers are shown as black font). All suggestions have been addressed, but still, the authors are open for further explanations and cooperation in term so manuscript improvement. Furthermore, the authors appreciate the editors and reviewers for the timely handling of the review process. After evaluation of this reviews we have the clear idea how to correct our manuscript to be publishable in NHESS. This mainly includes:*

- *Rewriting the introduction to highlight the novel aspect of the manuscript because in the present form, the novelty of the work is not clearly visible (rewriting according to the reviewer 2 suggestion)*
- *The literature review will be moved into material and methods sections (according to the reviewers 2 suggestion, shorten a little however will be kept because as the reviewer 3 stated this is a strong part of the introduction)*
- *According to the reviewer 2 suggestion, time series of the PS points will be presented together with the discussion about presented deformation mechanism*
- *According to the reviewer 1, some sentences will be rewritten to improve readably and some confusing aspect (thresholding, slope reprojection, PS technique limitation etc.) will be deeply discussed.*

Authors

REVIEWER 2

POINT The authors have the data, but do not take a good advantage of them. In particular, they should show InSAR displacement time series to better demonstrate the state of activity of the studied landslides.

This will be added to the revised manuscript.

What is new in the study with respect to many (tens or even hundreds) other similar studies that used InSAR for landslide assessment? Why is your study important? The authors closely follow some papers published several years ago, largely outdated. There is nothing new in that, unless the authors would like to focus on the problems and provide a critical assessment of these kind of simplistic approaches. However, this requires considerable landslide expertize and good knowledge of local slope processes, which, I'm afraid, the author may not have. So, I think, to bring out some novelty they need to re-focus on the application limitations and on some potentially interesting aspects of the study (see below).

You are right that this methodology is applied by many authors. However, in the presented paper we would like to grab all various aspects in one comprehensive study.

Firstly, some authors use only one geometry of SAR images (ascending/descending) but here we used both. Additionally, in this study, we showed that using one SAR geometry is not appropriate approach due to the specific SAR geometry and terrain characteristic (slope/aspect). In some cases, ascending geometry is appropriate to investigate specific landslide and, in some cases descending images are required. Therefore, there is a need to investigate both geometries.

Moreover, in this study, we present some advantages of both Sentinel-1A/B data processing with 6-days revisiting time. This allows to increase the coherence and therefore increase the PS points density. This is a novel aspect of the paper, previously not presented- comparison between Sentinel-1A and Sentinel-1A/B data processing in terms of PS points density and therefore landslide activity state updating.

Furthermore, even though some paper presented already utilization of PSI for activity state updating, this is the first paper which shows landslide activity state updating in a region in Poland. This region is specific in comparison to study areas investigated in other papers (rural areas with extensive agriculture). This specificity will be emphasized in the revised version of the manuscript. Therefore, the results which are presented for another and characteristic study case may be interesting and beneficial for scientific community. Moreover, this is a first presentation of Sentinel-1 data application to estimate the landslide activity state in Polish Carpathians. The PS interferometry technique has been previously applied in monitoring Carpathian landslides by Perski et al., 2010, 2011. However, in different study area but due to the high temporal decorrelation, resulting from vegetation cover and short wavelength (X-band), it was only partially successful (Perski et al., 2009; Perski et al., 2011). This is mostly related to low PS density due to the TerraSAR-X data application. Therefore, exploitation of C-band (Sentinel-1) and L- band (ALOS PALSAR) data bring more advantages, especially in rural areas of Carpathians mountains and in our opinion this finding should be presented for scientific community. Moreover, inspired by reviewers, we will provide, in the revisited version, also critical discussion of the method used supported by accuracy analysis.

Perhaps we did not appropriately underline all novel aspects in introduction section of our manuscript. We will improve it while revising this manuscript.

InSAR results look OK. However, the assumptions regarding their use need to be better justified, limitations clearly acknowledged. The use of some rather arbitrary "thresholds" appears improper. Therefore, some of the interpretations and conclusions are questionable.

Thresholds which we have used in presented paper are widely applied in scientific community. We did not select these thresholds arbitrary. We followed many works with the same approach/thresholding for example: Bianchini et al. (2012); Bianchini et al., (2013) Cigna et al., (2013) Del Ventisette et al., (2014) or Barra et al., (2016) etc.

The manuscript is poorly organized, includes repetitions (for example, between part 1 and 2). Sloppy, even the sections and subsections of the manuscript are incorrectly numbered. Citations thrown in a haphazard fashion, often not representative or useful. Poor English.

Repetitions of some subsection has been made accidentally when we were adjusting our manuscript into the Copernicus NHESS template. This of course will be corrected in revised version of the manuscript. In our opinion, citations have been selected correctly however we will check one by one each citation to meet your suggestion. English has been checked by native spear before submission, however if reviewer stated that it is not appropriate, we will try to improved English in revised version of the manuscript and we will check it by native speaker again.

Detailed comments

Abstract – should improve. What are the relevant scientific questions you are trying to address??

Abstract as well as introduction will be rewritten and reorganized to underline novel aspects of this study.

1 Introduction This part includes some information about PSI technique that should be moved to part 2 Materials and Methods

This will be corrected to the revised manuscript. However, another reviewer stated that this is a strong part of the introduction, thus we will try to keep this comprehensive literature review in our manuscript.

At line 51-72 In recent decades, new methods for updating landslide inventory maps – too long, too many and outdated references.

This will be corrected of course. However, another reviewer stated that the advantage of the introduction is wide literature review. Thus, we will try to face both requirement and find the balance between these two reviews.

At line 65 Commonly used methodology in the abovementioned papers is the PSI matrix approach with diverse SAR sensors, where specific thresholding of the landslide velocity, acquired from specific PSI processing, are performed. – OK, but this belongs to Materials and Methods

This can be corrected to the revised manuscript.

Objectives – I suggest to focus more on the limitations and some potentially most interesting aspects of the study, that is: Evaluating the effect of SAR geometry delivered from ascending and descending orbits from ALOS PALSAR and Sentinel 1 and the sensitivity to measure deformation over the study area;

Evaluating the difference in landslide activity updated from three diverse data stacks, namely: L-band (ALOS), C-band with one satellite (Sentinel 1A, with a revisit interval of 12 days) and C-band with two satellites (Sentinel 1A and 1B with a revisit time of 6 days), respectively; â (New) Exploiting displacement time series for landslide activity/intensity assessment.

Thank you for your suggestion. While evaluating the reviews, we think that we should totally rewrite introduction because some novel and interesting aspects were not properly highlighted, thus they were not clearly noticed. Thank you for noticing these aspects and for your recommendation. Based on this review, we concluded that we should focus on introduction improvement. Thus, for sure these aspects with SAR geometry of various sensors, time series of various landslides (active vs. not active or extremely slow vs very slow etc.) difference in landslide can be narrowed in introduction. This will be corrected to the revised manuscript.

2 Materials and Methods This part mixes the description of the study area with the methods and includes unnecessary information like: This region is rural and, thanks to the breathtaking landscapes, the population has grown rapidly in recent decades. Moreover, it is also attractive to tourists.

This will be removed from revised version of the manuscript.

At Line 106: landslide distribution within the study area as well their predefined activity states. – what do you mean by predefined?

Predefined means activity updated in 2010 by geologist during the field trip, when they were creating the official landslide database. Afterwards activity state has not been updated. An clarification to this issue will be added in revised manuscript.

2.1.1 Geological and hydrological settings of the study area No hydrological information is included, while different names of formations and units are thrown in and these are nowhere shown.

Using the term of "hydrological condition", we meant that Rożnów lake and Dunajec river undercut the slope which is the main reason of many landslide activation in this area. We will clarify this issue in the revised version of the manuscript

Different names of geological formation are presented in supplementary materials (it was placed in the appendix to increase readability of the manuscript)

2.1.2 Landslide types and distribution (note that this section is repeated twice)

The landslide activity in the study area is mostly associated with hydro-geological conditions such as rock stratification and precipitation. These conditions created favorable conditions for landslide activation. – poorly written

Repetition will be of course removed. This was included when adjusting style according to the NHESS requirements. English will be improved. We will include information about precipitation in the study area and we will try to correlate it with the activity state obtained using PSI based method. When revising our manuscript we will try to improve English and style.

Most of the landslide scarps within the study area lead down to valley floors – not clear

This will be described more precise and clearer in revised version

2.1 Radar data and PSI processing (page 6)

At line 177 Therefore, exploitation of C-band (Sentinel-1) and L- band (ALOS PALSAR) data can bring more advantages, especially in rural areas (Lu et al., 2018). – seems like an incorrect reference as the work is about land deformation in Changzhou city and uses InSAR data sets from 2006 to 2012, that is before Sentinel-1

Thank you for this comment. Reference Lu et al., 2018 will be corrected in revised version.

2.2 PS post-processing phase (page 7)

After PSI processing, all results for the five diverse data sets have been post-processed in order to retrieve the most adequate displacement information – not clear what you mean by the most adequate, for what?

Adequate means that we performed SAR geometry analysis, reprojection into the deepest slope etc. This aspects allow us to remove some possibly not reliable PS points. This will be clarified in the revised version.

2.2.1 PS suitability analysis (page 7) – need to shorten as the information is already available in the literature

This will be shortened while revising the manuscript

At line 204 Therefore, conversion of LOS deformation into the most probable direction (direction of maximum slope), by assuming a pure translational movement mechanism, is commonly used (Bianchini et al., 2012). – hopefully not so commonly, as, for example you also have rotational or combined rock-debris slides in your study area. This need to be explained.

Yes, you are right however, many authors applied this simplified reprojection (Cigna et al., 2013). From our point of view, we can used LOS velocities, but then we cannot merge ascending and descending results together into the one database. Additionally, we can reproject it into the vertical and horizontal movement but real practice shows significant difficulties in mountainous and hilly areas where the chances of retrieving radar targets in both geometries (ascending and descending) are significantly lower due to geometrical visibility and distortions. Thus, using v_{slope} seems a reasonable solution. Of course, this is as simplified transformation. We will provide an additional explanation when revising manuscript.

2.2.2 PS velocity projection along the steepest slope (page 8)

At line 232 Despite the great advantage of the motion represented in the slope direction, this projection has some limitations. First, when =90°, Vslope goes into infinity. Here we followed Herrera et al. (2013) and selected an absolute maximum value of - dangerous to talk about great advantage. Then, why should the approach by Herrera et al be good in your case?

It has the advantage that we can merge ascending and descending results together. Of course, in contrary this approach has disadvantage from the mathematical point of view.This approach was already applied by Bianchini et al. (2013) Kalia (2018). The C coefficient is the fraction of the 3D displacement that can be measured by PS targets and β the angle between the steepest slope and the LOS direction. Several limitations of this method must be taken into account: when β is almost 90°, C is close to 0 and V_{SLOPE} tends to infinity. Following the work done by Herrera et al. 2013, an absolute maximum value of β = 72° corresponding to cos β = 0.3 is fixed and, as a result, V_{SLOPE} cannot be higher than 3.33 times the V_{LOS}. This threshold corresponds to the condition number of 15 proposed by Cascini et al. 2010 as the number for the inversion matrix solving the algebraic system used for the projection process. In order to reduce any V_{SLOPE} exaggeration, it is assumed $C = -0.3$ when $-0.3 < C < 0$ and $C = 0.3$ when $0 < C < 0.3$. Whereas PS V_{SLOPE} values turn positive ($V_{SLOPE} > 0$) they are discarded. This is because a positive

V_{SLOPE} would represent uphill movement. However, thank you for your comment, we will try to rewrite this sentence when revising our manuscript.

2.2.3 Velocity thresholding for activity state estimation - PSI based matrix approach (page 8)

At line 247 For the LOS velocity, distribution is almost normal (Gaussian), while for SLOPE is second negatively skewed as a result of the PS reduction (Bianchini et al., 2013). Therefore, for activity state estimation, we applied 5 mm/yr as the Vslope threshold. – not very clear. Why 5mm/yr? What is your measurement precision?

This threshold has been selected based on literature mentioned before. In order to directly evaluate the accuracy we need to have another data (e.g. levelling). Nevertheless, according to the internal accuracy estimation provided by SarScape software, PS velocity estimated by Sentinel-1 data is around 1mm/year and for ALOS is 3mm/year. We will add the information connected with accuracy estimation in the revised manuscript as follows:

Internal reliability assessment

Because lack of other data such as levelling etc., it was not possible to assess the accuracy of PSI approach. The effective estimation of displacement, atmospheric and other components of DInSAR, mainly depends on number of used acquisitions. Obviously, when a broad set of data stack is used, then more precise estimation of the results. Therefore, we used interior accuracy assessment based on Andrea Monti Guarnieri (2009)

Guarnieri, A.M. (2009) Accuratezza nella stima del rate di subsidenza. Politecnico di Milano

Assuming that we have N images, with temporal baselines Bt(i) with i = 1…N and normal baseline, Bn(i), we can define the following parameters:

$$k_v = \frac{4\pi}{1000 * \lambda} \quad [\frac{rad}{mm}]$$

$$k_q = \frac{4\pi}{\lambda} * \frac{1}{Rsin\theta} \quad [\frac{rad}{m}]$$

Where: λ - wavelength of the sensor, θ - incidence angle of scenes center, R - distance

Then, let define the matrix A

$$A = \begin{bmatrix} 1 & k_q B_n(1) & k_v \frac{B_t(1)}{365.24} \\ 1 & k_q B_n(2) & k_v \frac{B_t(2)}{365.24} \\ \cdots & \cdots & \cdots \\ 1 & k_q B_n(N) & k_v \frac{B_t(N)}{365.24} \end{bmatrix}$$

where the term 365.24 represents the average number of days in a year. Then we can evaluate the following matrix:

$$C = (A^t A)^{-1}$$

Note, if $(A^t A)$ is singular, or almost singular, it estimation of the velocity is impossible.

Standard deviation of the velocity estimation is:

$\sigma_v \simeq \sqrt{c_{3,3}} * \sigma_\phi$ *[mm/yr]*

And σ_ϕ is calculated as:

$$\sigma_\phi = \sqrt{\frac{\frac{4\pi^2}{\lambda^2}\sigma_r^2 - 2\log\gamma_S}{N_{PS}} - 2\log\gamma_P}$$

Where

N_{PS} – *the average number of PS points per sq/km (in presented study, we assumed 200)*

γ_P-*coherence of the target whose accuracy is to be assessed*

γ_S- *the average coherence of the scene (typically* $\gamma_S = 0.7$*)*

σ_r^2- *the standard deviation od two-way atmospheric disturbances (typically* σ_r^2*=(0.015)2)*

For the final estimation of altitude precision of PS, we instead use the following formula:

$$\sigma_v \simeq \sqrt{c_{2,2}} * \sigma_\phi \ [mm/yr]$$

Presented formulas do not take into account global error such as phase unwrapping or orbital errors. For all these reasons, the formula can be assumed as the standard action of the best estimate, considering that in the real case this value it can get worse.

At line 251 Four diverse activity states have been determined (Fig. 5): (1) reactivated = active after being inactive, (2) active continuous = currently moving, (3) dormant =inactive, but possible to be reactivated and (4) stabilized = not active anymore. – You have InSAR data and should show displacement time series to clearly demonstrate the state of activity of your landslides. For example, are they continuously moving?? I doubt. Don't some of them stop in a dry season or winter?

You are right, in this region the driving force of landslide activity is precipitation. Since we performed our processing for time spans: 2014-2016 and year 2017, we estimated the activity for this time. Of course we will add time series deformation in revised manuscript and we will discussed this issue. Thank you for your suggestion.

2.2.4 Landslide intensity estimation (page 9)

3. Results (page 9) 3.1 Landslide activity state and intensity map generation – part of this section belongs to the previous one (2 Methodology).

At line 271 However, the activity state has been presented only for landslides where sufficient PS points have been found. At least four PS points within a landslide body were set up as the threshold. – why just four PS? I know that some authors you cite have indicated and used (also their followers) this arbitrary threshold, but is it scientifically sound? You may not justify its use.

For instance:

4 points within landslide body → Bianchini et al. (2013); Cigna et al. (2013)

3 points within landslide body → Cascini et al. (2013)

In the revised version we will implement the following improvement. Based on at least 4 points we will calculate the trimmed mean, removing two values; the smallest and the largest one. This will allow us to reduce outliers influence.

3.2. Possible hazard assessment (page 10) - part of this section belongs to the previous one (2 Methodology). However, why do you call it hazard assessment??? You are trying to assess possible damage. Moreover, please check the thresholds proposed by Mansour et al. (2011) really apply in your case and explain the limitations.

We will replace the word of hazard into the possible damage/destruction. This will be explained in revised version.

At line 304 Landslide with velocity below 10mm we classified as landslide with minor

expected damages. – questionable use of the threshold that can lead to dangerous interpretations. What if your landslide accelerates? And if t moves 9 mm/yr for 10 or more years?

This actually very challenging topic with boundary values, which we meet very often in classification or everyday life. In case of classification the issue can be minimize by using natural jenks/breaks classification which allows us to increase difference between classes. Nevertheless, in this case the threshold has been assess by Monsour et al. (2011) based on correlation between destruction identify in field and movement.

3.3 Field validation (page 11) – again, part of this section belongs to the previous one (2 Methodology). However, what do you validate in the field? You make some inferences based only on some simple observations. Moreover, you when to the filed 10 years after the ALOS data were acquired, and 1-few years after the acquisition of Sentinel data. Need to explain.

Not really, field images were taken in various time. Unfortunately, we did not specify the dates of the photographs. We have tried to be in the field immediately after the time span of PS processing, see table below. We will include this information when revising manuscript.

	Sensor used for activity evaluation in presented figure	*Time span of analyzed data*	*Data of the field photos taken*
Figure 11	*ALOS*	*31/01/2008-27/12/2010*	*9/29/2010*
Figure 12	*Sentinel-1A+B*	*2/01/2017-31/12/2017*	*03/07/2018*
Figure 13	*Sentinel-1A+B*	*2/01/2017-31/12/2017*	*03/07/2018*
Figure 14	*ALOS*	*31/01/2008-27/12/2010*	*25/03/2011*
Figure 15	*Sentinel-1A+B*	*2/01/2017-31/12/2017*	*03/07/2018*

Figures Fig 1 (and others) – lack of coordinates; incomplete caption

Caption and coordinates will be corrected when revising manuscript

Fig 2 - > 60 geological units with no explanation, sending the reader to a supplementary material? You need to group them by similar lithology, with reference to the susceptibility to landsliding

The geological unit will be merged together in revised version of the manuscript.

Figs 4-6 Who needs to see these figures?

I thought that it will be better to imagine how this PSI based matrix approach works,

Fig 6 – Present or historical PSI data? Not clear what you mean.

We will remove this figures, however we think that some aspects are better represented as an image rather than text. Similarly, other works present the PSI based matrix together with colors. This helps reader to connect specific PSI thresholding with specific activity. We will leave PSI velocity without word of "present or historical PSI data". Using this word, we thought that this velocity can be estimated by various PSI processing (actual sensors or past sensors). But of course we can remove it.

C5 Fig 7 – overlapping colors, hard to see what is going on.

It will be corrected however, the resolution and quality of the images has been degraded during pdf generation. We submitted also original images with high quality as a .zip package. Thus hopefully, images will be in better quality in final version of the manuscript.

Fig 8 – Landslide intensity scale or simply velocity scale?

Based on Cigna et al. (2013) who deeply discussed PSI-based activity and intensity estimation approach, we used this terminology. Generally, intensity is already accepted term of movement speed or as you said velocity scale.

Fig 11 – wrong choice of color for the landslide area, as some PS have the same or very similar color

I will be corrected in the revised version of the manuscript