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Natural Hazards and Earth System Sciences
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Interactive comment on "Quantification and analysis of geomorphic processes on a recultivated iron ore mine on the Italian island Elba using long-time ground-based LIDAR and photogrammetric data by an UAV" *by* F. Haas et al.

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The authors want to thank the three anonymous referees for their work and their mainly constructive critique! In the following we will try to answer the questions and comments of all referees:

Anonymous Referee #1

Referees comment 1: The authors provide an overview about the issue they are analysing, however I feel like the introduction is too focused on fluvial erosion. Since

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Discussion Paper

the journal is Natural Hazards and Earth System Sciences, I would suggest to consider a wider description of the issues related to mining landscapes, and on why this research offers a useful contribution in the field. I believe that the authors should provide an idea of the overall processes on mined/reclaimed hillslopes, not only focusing on pollutants but specifically on erosion, since the latter is the focus of their quantitative analysis. Answer of the authors: The authors agree with the referee and we will try to implement the suggested aspects into the manuscript.

Referees comment 2: High-resolution datasets in general. I would suggest to consider the paper review by Tarolli (2014) that provides a sound overview also about the potentiality of photogrammetry derived topography as well as LiDAR-derived one in numerous environments. Answer of the authors: The authors agree and will try to consider the work of Tarolli (2014). We also will implement the work of Smith & Vericat (2015) recently published in ESP&L.

Referees comment 3: Mining landscape and high-resolution datasets. I think the readers might find helpful knowing about other study cases showing the potentiality of high-resolution datasets (e.g. from UAV or Lidar) in mining landscapes, see for example Francioni et al. (2015), Chen et al. (2015) and Hanckock et al. (2015). Answer of the authors: The authors agree and will try to implement the suggested other studies, also including the work of Smith & Vericat (2015)

Referees comment 4: What version of photoscan was used? Answer of the authors: We are not sure, as we make regularly updates, but we believe it was photoscan professional Version 1.1.6

Referees comment 5: Point cloud thinning: what was the selected extent for the neighbourhood considered for the filtering? (L. 5-6, p 6280). Is this an important parameter influencing the quality of the final dataset? I think a reader that is unfamiliar with the technique might wonder about this point. Answer of the authors: Yes, it is an important parameter, but we only tried to organise a consistent point cloud regarding the point

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density for the whole AOI. This latter is much more important for the LiDAR data as for the UAV data. We are sure, that we did not influence the quality with this processing step, but make the data set easier to handle and better comparable. Nevertheless the mean point density after the filtering lies between ∼288 and ∼418 points per m2 and thus quite high. As values for the filtering in the LIS Tools we have choosen (values are in map units):

Segmentation by Planes (PC) âĂć NN Search âĂć Segmentation o Robust Plane Fitting: true o Maximum Distance: 0.4 o Minimum Percentage: 0 o Method Search Point: if search point is not included skip point âĂć Region Growing o Search Radius: 1 o Minimum Segment Size: 200 o Maximum Segment Size: 100000 o Maximum Normal Difference: 20 o Maximum Plane Offset: 0.3 o Normal Recalculation Interval: 100000 âĂć Output o Segment Size: true o Normal Vector: true o Slope: true o Aspect: false o Quality of fit: false Segmentation of the Point clouds via robust fitting of planes. Points that are not assigned to planes are likely to not represent ground points. This first classification serves as input information for the following steps. Create Ground Seeds (PC) âĂć Maximum Segment Slope: 55 âĂć Normal Tolerance: 50 âĂć Init cellsize: automatically âĂć Cutoff Cellsize: 1 âĂć Complete Ground Segments: true Ground seed are determined from the point cloud. Seeds are forced to be part of a valid segment determined in the preceeding step. Region Growing (PC) âĂć NN Search o radius o Search Radius: 1.5 âĂć Region Growing o Minimum Segment Size: 300 o Maximum Segment Size: 100000 o Recalculation Interval: 100000000 o Tolerance (Normal): 25 o Maximum Plane Offset: 0.4 âĂć Output: o Segment Class: 2 o Segment Class Undefined: 1 Final classification of Points in ground vs. non-ground.

Referees comment 6: How much is the final density of the UAV derived point cloud? i might have missed it in the text. Answer of the authors: Point densities can be found in Table 1 for all data sets. We still referred to this table within the text.

Referees comment 7: SPI: I wonder if the authors considered the evaluation of the SPI based on the DInfinity method (Tarboton, 1997) rather than on the multiflow SCA

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(Quinn et al. 1991). C2088 The Quinn method might be too dispersive on hillslopes, so I wonder if the use of the d-infinity SCA might better highlights areas of potential erosion. Answer of the authors: We do not agree with this. Due to the very short slopes and partly sheet flow conditions, we think, that the multiflow SCA seems to be better for this study and the result shows both sheet flow and channel flow regions quite good. Additionally, this work has not its main focus on this aspect. We only tried to highlight changes in "erodibility" on the slopes and make these changes visible with figure 10 using SPI.

Referees comment 8: Chapter 4.4. I would consider to split this chapter, and move the section 4.4.1 as a single chapter at the beginning of the results. As the authors recognize in line 13-16 p. 6290, the comparison of the two datasets is critical to understand the differences in the estimated volumes, as well as to understand the quality of the analysis of the SPI. To this point, for the comparison between the datasets, I suggest a deeper assessment of the differences, rather than just approaching to the point density analysis. It would be worth to analyse the differences in the distribution of elevation values (median value? Mean value? Distribution shape?), and the presence/absence of outliers, see for example the analysis provided by Prosdocimi et al. (2015) based on the work of Höhle and Höhle (2009). This analysis would help in understanding also the results currently described in chapt. 4.4.2. I wonder if it would be possible to analyse the difference in elevation values for the reservoir and the hillslopes, to better understand also the difference in erosion budget, and to highlight better what technique might be more appropriate for either context as a general surveying technique. The authors already hint to this point in their discussion (lines 13 to 24 page 6292). Furthermore, section 4.4.1 is in my opinion critical and really important, and it deserves to be a chapter on its own, following chapt. 4.1 and 4.2 (and preceding the SPI analysis). Answer of the authors: We agree and will try to split this section. We already compared LiDAR and UAV results in the Figures 13 & 14 and tried to make them visible. We can try to analyse slope and reservoir seperately and integrate this analysis in this part of the work.

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Referees comment 8a: Differences in estimated volumes. The overall differences in volumes are really high, and the authors highlight this point in detail throughout the text. Would it be possible to analyse the erosion/deposition budgets also for specific sub-areas? Aside from offering a comparison of the DEM values between the UAV DEM and the TLS DEM (as I suggested above), it would be worth to see and quantify erosion and deposition volumes for the hillslope and for the reservoir separately, and to assess these results. This would help to better understand what technique might be more appropriate for either context for the specific task of multitemporal surveys to monitor erosion. Answer of the authors: We can highlight special areas in our work for the analyses of single areas regarding the differences between LiDAR and UAV and integrate this in Figure 13. Because we think an additional Figure will increase the number of pages and the manuscript is still quite long?!

Referees comment 8b: The authors also speak about the limitation in the representation of (among others) the constructed channel. Would the overall quantification of erosions/deposition carried out masking the 'critical' features be more similar between the TLS and the UAV? Answer of the authors: We do not agree, as there are elevation changes (mostly erosion or subsidence) in this region. By masking this area, we would loose this information. Filter operation don't work there. So we decided to keep them for the analysis. Beside this, most of the differences between LiDAR and UAV can be seen at the gully structure. So masking of the artificial channel won't considerably change the results.

Technical comments Please be consistent with the acronyms (e.g. AOI or AoI for Area of Interest). Some sentences are not clear, and should be rephrased (e.g. L. 25 p 6284: 'quantitative and qualitative analyzing': I would change it to 'quantitative and qualitative analysis'; Line 12 p. 6285 'seems to bee': change to 'seems to be'.) Sometimes the authors use exclamation points to highlight out some elements, I think they are not needed. Answer of the authors: We will consider these comments.

References Chen J, Li K, Chang KJ, Sofia G, Tarolli P. 2015. Open-pit mining ge-

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omorphic feature characterisation. Int. J. Appl. Earth Obs. Geoinf. 42, 76–86. doi:10.1016/j.jag.2015.05.001. . . . . Answer of the authors: We will try to include the given references!

Anonymous Referee #2

Referees comments 1: This manuscript presents a full processing workflow of terrestrial laser scanner and photogrammetric derived 3D data to quantify erosion and deposition patterns over a slope. The topography of the area of interest has been reconstructed at different years using TLS derived point clouds and photogrammetric data obtained by processing UAVcollected photographs using a Structure-from-Motion approach. The authors adopted due care in ensuring high quality of data by measuring the surface from different position to reduce shadowed area, by post-processing and filtering the results to enhance their quality, and by the use of an error propagation framework to set a limit of detection threshold. The article is well written and pleasant to read. The scientific approach is rigorous and described in detail. The debate between the advantages/disadvantages between TLS and SfM applications (especially using UAV devices) is a hot topic in geomorphology, and I think that this contribution would be of great interest for many readers. I have a number of minor suggestions that could hopefully improve the already good quality of the paper. The only major one rises from the observation of the volume estimations. These are very different between TSL and UAV data, and the authors highlight this fact and discuss it in detail. Looking at figure 13, we clearly see the underestimation of the TLS of slope erosion and of AUV data of deposited volumes. Why this happens for the "cut" part is shown in figure 12. However, the "fill" difference is very high despite good densities. This is partially justified by the UAV LoD. But one could wonder if it is only because of that, and the doubt would be justified given the high differences. The authors argue that there is no systematic error detectable, and I agree with them on the basis of their results. Nevertheless, would it be possible to produce an image that compares TLS and UAV DEMs for the same year (e.g. Apr 2013 or Apr 2015)? It is clear that even if this highlights a systematic

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difference between TLS and AUV that could mean nothing, because the two would still be relatively well registered and so restitute the good DEMs of Difference visible in figure 13. However, if no systematic error is detected, then logically we would observe the differences in the same locations as the differences between figure 13a and 13b, and in some way confirm the observation made in the text. In conclusion, I think that this is the major and more puzzling result of the paper and it is worth investigating a little bit more, since one might wonder how a result for either method could be trusted if even with such rigorous approaches discrepancies are so large. This is not easy though, since as I mentioned the authors made a great effort in optimizing the data, so it is definitely possible that these high differences are solely due to point densities and LoD thresholds. Answer of the authors: We will try to integrate such an analysis in one figure (Figure 14 would be a good place) and try to add the results to the discussion. I think it could be too much to create a new figure. As we discussed in our answers to referee #1, the paper is still quite long. But we agree that such an additional figure could improve the paper and support our discussion section.

Referees comments 2: My minor suggestions (please take with care my suggestion about the English, as I am not a native speaker and could be wrong): Abstract: write out and define UAV as you did with TLS Abstract line 6 and in other places: I would incline towards putting process dynamic in plural. Page 6273, L 18: could you please provide a reference for natural landscape as well? Page 6273, L 18 and 19: repetition of “conditions” Page 6273 L24: this is a very long sentence, and I suggest splitting it. Page 6274 L28: I would delete “in this study” and “the methods” Page 6275 L11: by leaving ! and left? Page 6275 L18: Mine ! mine , delete “not or” Page 6276 L14: I do not think that it is necessary to mention that it was done with students. Page 6276 L21-23: I suggest deleting this sentence and inserting a small paragraphs describing the chapters at the end of the Introduction section instead. Page 6277 L11 and 21: I would move the (3-D scanner software) at line 11. Page 6277 L23: Are the authors sure that we are talking about accuracy here? In my experience, Riscan Pro provide a standard deviation value in the MSA module, and I would therefore talk about precision

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here (as in the next line). Page 6281, L3-4: I would delete “in fact”. I would also delete the “!” here and in all other instances, I do not think that they are necessary to stress your point. Page 6286 L 12: typos: bee ! be Page 6286 L18-19: I would delete “the reservoir: : : and as a consequence”, as it is already clear and implied and it causes only the sentence to be longer without need. Page 6289 L1: delete “in fact” Page 6290 L 13: would it not be sect. 4.1 as well? Page 6294 L 6: could you mention some advantages/disadvantages of airborne LIDAR in one sentence as well here? Page 6294 L17: “if this would lead..” ? Table 5 is a bit redundant, as the same information is shown in table 6. I think it would be better to leave table 6 only, and refer to the first line of it instead of table 5 where necessary. DEMs of Difference images: I would insert a small white/grey bar near the 0 m value in the DEMs of Difference legend. This would make more clear that values between –LoD and +LoD are not in colour. Figure 7: specify in the caption that these results are from TLS data. Figure 13: add a short sentence in the caption explaining what could be observed in the two squares. Answer of the authors: We will consider all these technical comments.

Referees comment 3: Congratulation, this is very interesting and rigorous work! Answer of the authors: Thank you very much for this feedback!

Anonymous Referee #3

General referees comments: The authors propose a study of erosion processes on a recultivated mine based on repeated (from 2009 to 2015) 3D measurements with different techniques (terrestrial LiDAR and aerial photogrammetry), the last two dates being investigated simultaneously by both techniques. The subject and the data are very interesting and probably worth being published. However, it must be acknowledged that parts of the document, in particular the material and methods section, suffer from remaining inconsistency and unbalanced level of details, hence hampering from taking full benefit of the results and discussion section. On the other hand, authors already provided great and appreciated effort to describe the data and its processing, which allows the reader for an overall understanding of the scientific and technical work done.

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More, with these investigations, the authors could detect ongoing processes of erosion and sediment accumulation. The authors even experimented estimation of sediment balance for the whole area, and discussed relative capacities of both methods. For all of these reasons, this kind of paper is of a highest interest in the field. However, in the present condition, the paper shows unexpected results (in particular, the differing sediment balance estimated by two independent methods), that are associated to remaining vagueness and missing information in the methods. For these reasons, I would suggest the editor to accept the paper only after the appropriate revisions would have been done. Answer of the authors: As the general comments are specified in the specific comments, the authors will answer them step by step

Specific referee comments: Referee comment 1: Site presentation : Minimum and maximum altitude of the site are needed. The shadowed DEM used as background does not allow for full interpretation of the global topography (and hence, expected drainage network), all the more so as there is no ground picture of the full site. This is especially disturbing for the interpretation of the shifting channels. Some figures, at very least the second one, would benefit from having rough contour lines and/or drainage network superimposed, or every other information that would help the interpretation of the topography and expected flows. Answer of the authors: We partly agree and we will integrate the values min and max altitude in the text and the figure 2 (section study area). And we can put in a picture of the study site (view from the reservoir with the gully structure visible) if required in Figure 3. But we are sure, that contour lines won't help, as they will make it much more confusing. We tried it and we think that the shaded relief is enough for the presentation of the topography (what is meant by global topography?).

Referee comment 2: TLS : It would have been very interesting to see the individual TLS DoDs on the whole area at least as an additional material to the article. Answer of the authors: Wow! That would mean at least two or three more figures. Beside this, we still discussed in the text, that surface changes on the slopes are quite low (especially

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at the locations with sheet erosion). As a consequence erosion on the slopes is mostly under the LoD for the single time steps. As a consequence we would have a lot of figures showing mostly nothing more than the bare shaded relief on the slopes. Only the long period makes surface changes on those areas visible (Figure 6). For the reservoir, the surface changes are mostly over the LoD for the single epochs. Thus we already integrated the high resolution analysis of the single epochs only for this area (Figure 7).

Referee comment 3: what are the positions of the 6 permanent targets ? Answer of the authors: See figure 2c and 3. But we can mark the permanent targets also in Figure 2 c. But you can still see the reflectors on the picture.

Referee comment 4: Why are stable areas only around a centerline of the AoI ? Answer of the authors: Because these are the only stable areas.

Referee comment 5: Figure 3 may make think that permanent targets are close to each other relatively to the whole test site area and might be in a vicinity of a straight line. This can possibly lead to global tipping of each assembled point cloud. If so, computing the LoD near to the centerline may lead to an underestimation of the true LoD, especially around the borders. Answer of the authors: Yes, they are close to each other and in a vicinity of a straight line. This is the only possibility to do it. We described it the methods section our workflow in detail: Following the data acquisition in the field, the software Riscan Pro (3D scanner software) was used for the postprocessing of the raw data. The single point clouds of each time step were registered globally using the scanned tie points. All scan positions of each single scanning epoch were referenced to one “master scan” which provided these global coordinates. All other scan positions were well distributed over the slope in order to ensure enough overlap of the single point clouds for the following referencing procedure using an ICP-based algorithm (MSA tool in Riscan Pro). The values for the referencing accuracy lie between 0.007 m and 0.013 m. Table 1 shows the number of the scan positions on the slope, the referencing accuracy as mean value, the resulting number of points and the point den-

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sity (points / m2) for every single epoch. Thus we had a very precise global registration of the master scans of the single epochs by using these stable fix points. As these stable points are in the centre of this master scan position, we didn't detect a dipping for the master scan between the single epochs. As all following scans are registered by using the ICP (separately for all single epochs) and big overlapping areas of the single scans, a dipping could be avoided. Anyway, we don't have any other chance, as there are no other stable areas around. Referee comment 6: UAV : Presented information does not allow the reader for a full understanding of how did authors actually manage with internal orientation / lens distortion / autocalibration. This is especially puzzling because authors mention first that camera is calibrated (6278-10) and then argue that better distributed GCPs would have minimized a bowl effect (6293-5). Answer of the authors: We used a calibrated camera with a fixed lens (autofocus is deactivated and the lens is additionally fixed by an adhesive tape). The calibration was done by the technical college of Karlsruhe (Prof. Hell) using a measurement grid. The calibration values were integrated into the workflow of Agisoft Photoscan Professional (camera calibration parameters). There are a lot of discussions about the bowl effect in the Agisoft forum. We made a lot of tests (not at Elba, but close to Eichstaett) using only a few GCPs in the centre of the captured area. Result was that Photoscan tends to bend the point cloud optimising the result by using GCPs even if calibration parameters are integrated. This bending becomes less by distributing the GCPs over the AOI even in the border areas.

Referee comment 7: Readers need answers to the following questions : What focal length had been used ? (and thus, what is the average ground sampling distance?) Answer of the authors: We can integrate this information in the text, but is it essential to understand the presented workflow and the results? If yes, we have no problem. Ground sampling distance lies between 50 to 80 m (as it is written in the text). Depending on the relief and the manual control, we do not have a constant sampling distance. Do you want to read the mean height of all pictures?

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Referee comment 8: How was actually managed internal orientation / lens distortion / (auto-)focus ? Where are the GCPs (could be shown on fig 8a) ? Were the markers pointed manually or detected in all images possible ? Answer of the authors: See answer to comment 6. Markers were set manually and they were detectable in all prospect pictures (if this is meant by this question; we are not sure if we understand the question right).

Referee comment 9: Workflow & sediment balancing : The three sections 3.2, 3.3 and 3.4 have to be reviewed and rewritten by the authors. The overall quality of these sections (including English quality, especially for 3.2 and 3.3) is lower than the rest of the document (details in the section 'technical corrections') and figure 5 has to be redrawn. Answer of the authors: We will try to improve the quality of this section

Referee comment 10: Showing a workflow as identical as possible for both techniques does not seem relevant seeing that (i) technologies do not rely on the same physical principles and (ii) workflows are indeed very similar, but not totally identical. Answer of the authors: Different physical principles, for sure! But we do not agree with this. I think the figure is very important to visualize the workflow for both techniques and should help the reader understanding the single steps. For us it is essential to have it and to make clear, that we had to do a lot of steps processing and analysing the data.

Referee comment 11: Authors use the same procedure for vegetation filtering of photogrammetric data and TLS data whereas laser beams are expected to penetrate vegetation, contrarily to the "surface sensing" capacity provided by images/photogrammetry. Answer of the authors: We do not agree! It is right, that laser beams are expected to penetrate vegetation. But this is only true for sparse vegetation. Under sparse vegetation we get both ground points (penetrating laser beams) and vegetation points (or a mix of vegetation and ground as we have no full waveform information!). In the case of dense vegetation we don't get ground points at all, thus we have to filter the complete vegetation area. The filtering of vegetation is thus identical for UAV and LiDAR as we use the colour information (Hue-value). We describe this in detail in our text! In the

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case of sparse vegetation, the described region growing algorithm was successful and can be used for both UAV and LiDAR data because: (i) LiDAR partly penetrates vegetation and thus produces both vegetation and ground points. (ii) UAV/photogrammetric show also both, vegetation and ground points. Because normally we have the vegetation not only captured from above, but also from the side (looking under the vegetation if it is sparse) from different side perspectives. Thus we get also ground and vegetation points. As a consequence we can use the workflow for both data sets and most parts of the vegetation could be eliminated doing so.

Referee comment 12: Several different AoI seem to be introduced (6280-9, 6281-15, 6281-16) and used (fig 6, fig 10). These imprecisions have to be corrected into a single coherent framework.

Answer of the authors: We do not agree with this comment. Different AOI are necessary, as we wrote in the text. This is the consequence of including the parameter overlapping into the workflow of the UAV processing. Thus AOI of the UAV analysis had to be smaller than the AOI of the LiDAR survey. For the comparison of UAV and LiDAR we reduced the LiDAR AOI to the UAV AOI. So it is not an imprecision or an error but intended. But perhaps we will try to make this clearer.

Referee comment 13: What considerations (scientific, technical, software) are behind the choice of a 0.2m ground sampling distance for the DEM ? Answer of the authors: We have chosen this grid resolution, as a higher resolution would increase no data areas within the AOI mainly in the LiDAR data set. Due to the inhomogeneous point densities of the LiDAR data (we discussed this in the manuscript) is predetermined mainly by the LiDAR resolution. So grid resolution has a technical and a scientific reason and not because of software or compute power.

Referee comment 14: The 3.3 section suffer from an unbalanced use of references and equations, both within the section itself (LoD or _DoD ?) and between the section and the rest of the document (rDEM1DEM2 notation neither explained nor used). Ref-

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erences in this section seem to be whether misused, whether not clearly introduced. Notations introduced at 6283-10 are not consistent with the (Brasington, 2010) reference. This is all the more prejudicial that LoD is at the end estimated by statistical analysis of DoD values on stable areas, that do not really need any of the introduced equations (6284-5-7). I would advise to drastically reduce this section, explaining that statistics of DEM differences on stable areas are used to evaluate which magnitudes of DEM differences can be accounted for error and which can be accounted for true terrain change. Answer of the authors: We agree and will try to rework this section.

Referee comment 14: section 3,4 : TLS and UAV DEM comparison announced (6284-19-20) but not shown, although of the highest interest for the paper Answer of the authors: Of course we did. We have an analysis of the UAV and the LiDAR point densities (sect. 4.4.1), which is important for our discussion section and a comparison of the UAV and LiDAR balancing (4.4.2). We tried to visualize this with Figures12, 13 and 14. But indeed we have no comparison of DEM to DEM. If you think this is necessary, we can try to implement such a discussion in the manuscript and try to visualize this with a figure (map of differences and a statistical analysis). We agree that this could be a good additional analysis, as we have clearly differences in the volume calculation. So we will try to implement such a section and the necessary figures.

Technical corrections 1: Considering remaining questions for the material&methods section, pieces of information (remarks, questions) that would have constituted specific comments for the results& discussion section have been directly postponed into the “Technical corrections” section below without synthesis as guiding material for the authors. Remove all “!” signs. 6276-5 : add max&min height of the 13800m2 domain Answer of the authors: We will consider these comments.

Technical corrections 2: Fig 2 : put artificial drainage network and/or rough level curves onto the orthophoto would help the understanding of this highly artificialised area ; for instance : is the water flow upstream the bare slope collected so that it would flow in this artificial channel ? Putting rough level curves on figs 6, 8, 10, 13 would be very

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helpful as well. Answer of the authors: We do not agree, as we are quite sure, that contour lines make it more confusing. (see also answer to referee comment 1).

Technical corrections 3: 6276-15 : “three more” or “four” missions Answer of the authors: No, we have 5 campaigns in total, thus since 2009 we have four more. If this was the question. . .

Technical corrections 3: 6277-21-22 : sentence is unclear : is the “tie points” terminology the good one ? Answer of the authors: It is the terminology used in Riscan Pro, so it makes sense to use it.

Technical corrections 4: Fig 5 : Are ortho-images really done after alignment and markers ? this had probably been done after dense cloud Answer of the authors: You are right, we will change this.

Technical corrections 4: To Fig 5:Interior orientation step missing Answer of the authors: If you think this is necessary, we can implement it.

Technical corrections 4: Detail PC=Point Cloud Answer of the authors: We will explain the abbreviation.

Technical corrections 5: “alignement of all epochs” under Riscan Pro is unclear to me vertical writings are uncomfortable for reading Answer of the authors: The figure is only a visualisation of the detailed process written in the methods section. The alignement is described in detail in section 3.1.1.

Technical corrections 5: 6279-28 : put the reference to LIS at the first occurrence of this acronym. Answer of the authors: We will consider this comment.

Technical corrections 6: 6280-5 : filter based on variability : too vague ; give figures Answer of the authors: We will consider this comment.

Technical corrections 7: 6280-8-10 : show the manually mapped AOI on the fig 2 ; on the figure 6 and later, AOI and filtered/masked area are merged and both

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shown in white, which does not help to dissipate the confusion between different AoIs and filtered/masked areas. Answer of the authors: Sorry, we don't understand this comment. . .

Technical corrections 8: 6280-11 : 2D-block filtering : Was it applied to UAV ? Does not appear in the text but appear in fig 5. Which method is used ? Reference ? 625 pts/m2 in the text and 635 pts/m2 on fig 12 Answer of the authors: We will make this clearer.

Technical corrections 9: 6281-8-9 : unclear : is the "N_ overlap tif" georeferenced on the base of the ortho ? There is here a considerable (and probably unnecessary) amount of detail on this step, compared to other ones (where more detail would have been welcomed). Answer of the authors: We used the UAV GCPs for the referencing. We will make this clearer. I think this step is new and Photoscan offers no possibility to export the information overlapping. So we think it is worth being described how we did it. It could be interesting for other researchers including this paprameter. . .

Technical corrections 10: 6281-20 is "consistent" the right term ? "all single epochs" or "each epoch" ? Answer of the authors: We will check this.

Technical corrections 10: 6282-2 : the plane fit method is a bit unclear. Is this plane horizontal ? Is the value chosen the mean of the 12 neighboring values or is the plane fit done with minimization of least squares ? The actual process may be simpler than its current written explanation Answer of the authors: Starting with the point cloud, a grid is places over all the points. Next, for each grid cell center the twelve points closest to the cell centers in 2D space are chosen. Where no twelve points are located in a grid cell, the algorithm is allowed to search for points up to a distance of 1.5 the grid cell length from the cell center. Now, least squares 2D regression minimizing vertical distances to the plane (as opposed to orthogonal distances) is applied to fit a plane that represents the selected points within the cell. In other words, a function of x and y is fit to the measurement points. Finally, the z value on the regression plane at the xy

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location of the grid cell center is used as DEM cell value."

Comment to reviewer: It should be obvious that a "plane fit" is not the same thing as caclulating the mean of points

Technical corrections 11: 6282-5-7 “by two lines of cells after the gridding to remove these artefacts” to be replaced by “by two pixels”. Answer of the authors: We will consider this comment.

Technical corrections 12: 6282-8 : “The spatial analysis was done within the framework of SAGA GIS (version 2.2.0) by using the integrated terrain analysis tools.” to be replaced by “The spatial analysis was done with terrain analysis tools of SAGA GIS 2.2.0.”. Answer of the authors: We will consider this comment.

Technical corrections 13: 6282-10 : why not having 2012 ? Answer of the authors: No significant changes between 2012 and 2013. Most changes happened between the periods 2009 to 2013 and 2013 to 2015.

Technical corrections 14: 6282-11 Moore et al 1999 in the text and Moore et al 1991 in the references. Moreover, notations chosen are the one of Saga GIS and not the ones of Moore nor the ones of Quinn Answer of the authors: We will consider this comment.

Technical corrections 15: 6282-15 : explain in few words what stream power index is supposed to reflect Answer of the authors: We will consider this comment.

Technical corrections 16: 6282-17 : Error assessment Answer of the authors: We will consider this comment.

Technical corrections 17: 6282-22 time step=epoch ? Avoid the use of “then” at the beginning of the paragraph (same at 6282-2) 6282-23 variable variations ? Answer of the authors: We will consider this comment.

Technical corrections 18: 6282-22 to 6283-4 sentence needs a complete rewriting Answer of the authors: We will consider this comment.

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Technical corrections 19: 6283-5-6 : “follow a normal distribution” 6283-15 : rDEM1DEM2 ? Not defined 6283-19 : Delete “in” 6284-2 : this is not an equation, there is only one term. Moreover, it seems not to be used. 6284-5 : repeat => repeated ; to => too 6284-9 : all detected changes on these areas can be seen Table 3 : An indicator of the DoD quality misses for the 2009-2015 time period, which is at the end the only one shown entirely. Answer of the authors: We will rework this part, as we mentioned in our answer to comment 14.

Technical corrections 20: Consider having the “Epoch” line as a first line to help the reading (the last column with TLS between parenthesis is not clear with the “Method” line as a first line. Table 3 caption should be “Standard deviation of DEM difference and LoD (...)” What about DEM difference at the same date (between UAV and TLS) ? Answer of the authors: We will consider this comment.

Technical corrections 21: 6285-8 : western or eastern ? Answer of the authors: We don’t understand the question regarding this line of the manuscript.

Technical corrections 22: 6287-10 : this has to be discussed relatively to an estimation of the non-detectable (under the LoD) sheet erosion volume over the whole slope (= f(slope area * LoD) ? ; figures to be added to table 3?) Answer of the authors: We are quite sure, that this won’t make sense. We define everything under the LoD as not detectable or as measurement error. We can not discus anything related to something unknown..

Technical corrections 23: 6289-11 : here SPI change is announced to be a proxy for the erosion potential, whereas in 6282-11-12, it was expected to simply use the “single-date” SPI ? What has been done actually (and why ?) ? Answer of the authors: As mentioned above, we will add a sentence at 6282-11-12

Technical corrections 24: 6289-12 : between 2009 and 2015, as shown in text and within figure, or between 2009 and 2013 as written in figure caption ? Answer of the authors: Between 2009 and 2015. We will correct it.

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Technical corrections 25: Figure 10 : why is this figure not showing the same masked and shadowed areas than figure 6 (deep gullies not shadowed) ? Moreover, the clipping is not the same than other figures (why ?) Answer of the authors: Only for the visualisation we used the UAV DEM of 2015 for the hillshade.

Technical corrections 26: Figure 11 : The changes seen in the SPI distributions (6289-15) are not clear. What are the significances of these changes ? Is the middle value of the boxplot a mean or a median ? Answer of the authors: What do you mean with significances? It is explained on the right side of the figure, it is the mean.

Technical corrections 27: 6289-18 : Comparing maximum values seems fragile especially as this value start to decrease between 2009 and 2013 and then increases. Have the processes on the whole area followed the same trend (decrease then increase) ? Answer of the authors: We think that max values can be an indicator for the increase of erosion areas. But this is not a quantitative analysis. It is only meant as qualitative analysis to support our conclusion regarding the reworking of the slopes..

Technical corrections 28: 6290-13-16 : This sentence, dealing with both UAV and TLS data, is misplaced in a section that deals only with TLS data. Answer of the authors: It is only a conclusion of the whole analysis in the sections above and should be a connecting passage to the next section. We think, that this is necessary.

Technical corrections 29: 6290-19 : Precise information on the filtering algorithm (especially the 2D-block point cloud thinning) is needed in order to properly examine the comparison of the resulting point cloud densities. Answer of the authors: As mentioned above, we will try to integrate all the settings we used for the filtering process.

Technical corrections 30: 6293-2-4 : The figure (with very few values over the LoD) do not serve these affirmations (by the way, shifting is only one type of systematic error). Maybe some other figures among the following would have helped more : map with all DoD values (possibly with some local spatial filtering) ? scatterplot DEM-UAV-2015 = f(DEM-UAV-2013) ? histogram of DoD ? Answer of the authors: We know, that there

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are more possible systematic errors, thus we wrote (e.g. shifting). Shouls we call all possible?? We will try to give a better visualisation of the data in order to check possible errors. But we don't understand what you mean with some local filtering. Perhaps scatterplot is a good suggestion.

Technical corrections 31: 6293-4-7 : not usable if not knowing what internal orientation has been done Answer of the authors: As mentioned above, we will implement this in the methods section.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 6271, 2015.