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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.

Anonymous Referee #3

Received and published: 13 December 2015

*****General 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

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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. 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.

***Specific comments

*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.

*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. Then, rather questions than comments about individual scans co-registration and global referencing :

-what are the positions of the 6 permanent targets ?

-why are stable areas only around a centerline of the Aol ?

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

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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.

*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).

Readers need answers to the following questions :

What focal length had been used ? (and thus, what is the average ground sampling distance ?)

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 ?

*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.

-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.

-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.

-Several different Aoi seem to be introduced (6280-9, 6281-15, 6281-16) and used (fig

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6, fig 10). These imprecisions have to be corrected into a single coherent framework.

-What considerations (scientific, technical, software) are behind the choice of a 0.2m ground sampling distance for the DEM ?

-The 3.3 section suffer from an unbalanced use of references and equations, both within the section itself (LoD or δ DoD ?) and between the section and the rest of the document (rDEM1DEM2 notation neither explained nor used). References 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.

*section 3,4 : TLS and UAV DEM comparison announced (6284-19-20) but not shown, although of the highest interest for the paper

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.

***Technical corrections :

Remove all “!” signs.

6276-5 : add max&min height of the 13800m² domain

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 helpful as well.

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6276-15 : “three more” or “four” missions

6277-21-22 : sentence is unclear : is the “tie points” terminology the good one ?

Fig 5 : Are ortho-images really done after alignment and markers ? this had probably been done after dense cloud

Interior orientation step missing

Detail PC=Point Cloud

“alignement of all epochs” under Riscan Pro is unclear to me

vertical writings are uncomfortable for reading

6279-28 : put the reference to LIS at the first occurrence of this acronym.

6280-5 : filter based on variability : too vague ; give figures

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 shown in white, which does not help to dissipate the confusion between different Aols and filtered/masked areas.

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

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).

6281-20 is “consistent” the right term ? “all single epochs” or “each epoch” ?

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

6282-5-7 “by two lines of cells after the gridding to remove these artefacts” to be replaced by “by two pixels”.

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.”.

6282-10 : why not having 2012 ?

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

6282-15 : explain in few words what stream power index is supposed to reflect

6282-17 : Error assessment

6282-22 time step=epoch ? Avoid the use of “then” at the beginning of the paragraph (same at 6282-2)

6282-23 variable variations ?

6282-22 to 6283-4 sentence needs a complete rewriting

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.

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) ?

6285-8 : western or eastern ?

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 (= $f(\text{slope area} * \text{LoD})$? ; figures to be added to table 3?)

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 ?) ?

6289-12 : between 2009 and 2015, as shown in text and within figure, or between 2009 and 2013 as written in figure caption ?

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 ?)

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 ?

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) ?

6290-13-16 : This sentence, dealing with both UAV and TLS data, is misplaced in a section that deals only with TLS data.

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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.

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 ?

6293-4-7 : not usable if not knowing what internal orientation has been done

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

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