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Earth Surface Dynamics Discussions

Interactive comment on "Automated Terrestrial Laser Scanning with Near Real-Time Change Detection – Monitoring of the Séchilienne Landslide" *by* Ryan A. Kromer et al.

Anonymous Referee #2

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GENERAL COMMMENTS:

This paper describes the setup of a terrestrial laser scanning based monitoring system, including the installation of the hardware, pre-processing of the acquired data (e.g. removal of outliers, atmospheric correction, etc.) and also some first analyses (i.e. deformation quantification) and visualization of changes. As test scenario a landslide in France is chosen. This contribution is dedicated to a very innovative and important topic and it is definitely a pioneer study in the field of automatic 4D monitoring of Earth surface dynamics.

The current manuscript reads like a scientific/technical report with clear focus on the documentation of what has been done in this study. The aim is to present the specific

TLS-based monitoring solution. I wished to see more science-driven facts and arguments in the paper: It is not clear what parts of the workflow are general facts and are valid to any TLS-monitoring (also other phenomena or LiDAR systems) and which parts/settings/algorithms/etc. are specific for this study site or only for landslides? This is an important fact for all readers because we want to learn how to set up our own system in our research without re-inventing the wheel. This could be improved by making this separation between general findings and the use case more evident in the manuscript.

You are working on landslides. What are the specific "user requirements" for your landslide use case from a process monitoring perspective - regarding hardware, scan acquisition settings (e.g. temporal and spatial resolutions), data processing times, etc.? For example, what is "real-time" and "near real-time" in your case (note that you do not use it consistently in the manuscript)? It would help much if you could describe all requirements that determine how you set up the hardware, software and also analysis methods.

Furthermore, from a scientific perspective I strongly suggest to publish also your tools and scripts along with this paper. You used many open source frameworks - thus it is straightforward to publish it also open source. This would help the readers to follow your workflow much easier and to follow your work in a transparent manner.

A critical aspect that needs to be revised and explained is the presented alignment of point clouds. If you assume deformations in your data, you should only use data parts (i.e. areas) that do not change (cf. Wujanz et al. 2016: http://onlinelibrary.wiley.com/doi/10.1111/phor.12152/abstract). You write on Page 13/ Line 4: "In the fine alignment stage, we use all of the points in the point cloud to optimize the alignment.". I could not find any solution to this fundamental issue of deformation analysis in your paper.

Another critical aspect for improvement is the confidence interval calculation in Sect.

3.2.5. First, I need to mention that you somehow use "accuracy" synonymously to your LoD value (e.g., P15/L5, P19/L9-L14), which pretends that your system quantifies changes or distances with 2-10 mm at 1000 m range. Following the scanner datasheet specification, your scanner has a range precision of 7 mm at 100 m (1 sigma) and thus it might be worse at 1000 m distance. Second, there is no validation with independent reference data. On page 22/last paragraph you state one value comparison (with an extensometer), which is not sufficient to evaluate your results. Without further (mathematical) proof and explanation of your spatio-temporal confidence interval, I cannot approve what you present in this section. Some issue to consider regarding your main assumptions: You do not repeat a single LiDAR measurement if you average over time because you measure something (slightly) different with each scan (i.e. epoch). Your measurements are not independent because you use spatial averaging with neighbors for your cloud-to-cloud distance calculation (Sect. 3.2.4). At the current stage this section is not plausible for me, but I am looking forward for indepth clarification.

In Sect. 3.3 you finally list some of the settings that you used in your case study. However, many settings are not mentioned, mainly of the processing steps (e.g. threshold for outlier removal, etc.). To be able to reproduce your results, it is necessary to know all the major settings that you applied, which could be put into an appendix section.

SPECIFIC COMMENTS:

- P1/L14: Keywords: "Near real-time" instead of "real-time". - P4/L8: ..."of the valley"? - P5/L4: -85% compared to which value? - P6/L6: -> "ILRIS" in capital letters (applies also for other occurences in manuscript). - P6/L20: -> "RAM" - P7/L6: "Software design": The word "software design" is misleading because you do not actually design a software (as computer scientists would see it); you "use" existing software in a processing chain/workflow. - P7/Fig.3: I cannot see a pan tilt. Either mark it or add picture. - P10/Sect. 3.2.3: I do not really see the point of your initial alignment processing for each dataset because you use a static scanning setting. Of course, you need to determine an initial alignment when you change the scanner position completely - but why

processing it every time and not only if something changes? Please explain better. See also my general comment above. - P10/L27: positon -> position - P10/L28-29: These are speculations. I would remove such speculations from the paper and replace it with some hard facts. - P11/first paragraph: This might be only necessary for "slow" scanners. Newer generation scanners are quite fast and such full scans could also be splitted and treated separately because we know the timestamps for each laser point. - P13/Sect. 3.2.4: A sketch figure explaining the 4D distance calculation would increase comprehensibility of the method in this paper. - P14/24: pi is known and must not be explained. - P16/Sect. 4.1.: is a valuable section! - P17/9: You use "surface saturation" or "slope saturation level" to describe the changing reflectance of the surface when it gets wet. From a physical perspective the changing reflectance of the surface is the main issue. I think "saturation" does not really fit to explain this issue because it has nothing to do with saturation of soil/material etc. -> reflectance of surface/target in the laser's wavelength. - P21/Fig. 8: Difficult to see anything. Too small. Rework figure to be larger and you could mark the processes that you identified in this figure. - P23/Fig. 9.: Add infos/description about points also in figure caption. - P27/10: You did not evaluate the accuracy of the system. - P27/19: This statement is not generally valid because other LiDAR systems can penetrate water/rain/etc. much better due to their different wavelength (e.g. green wavelength TLS). For your specific system it is valid, but you should not draw general conclusions from it. - P27/22: affect -> effect. - P28/6: must be Eitel et al. (2016) - P28/12: "We were also...". Is there something missing in this sentence? Difficult to understand.

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