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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Quick Response Assessment of the Impact of an Extreme Storm Combining Aerial Drone and RTK GPS" *by* Arthur C. Trembanis et al.

Anonymous Referee #1

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The manuscript entitled Quick Response Assessment of the Impact of an Extreme Storm Combining Aerial Drone and RTK GPS by Trembanis et al. illustrates a rapid deployment of RTK GPS and UAV survey after a storm that produced floods in the nearby communities. The study explores the potential application of UAVs as a rapid response to evaluate the extent of an event.

The main limitation of this manuscript is the structure of the text, which does not flow well and is complicated by logical flaws. For instance, the introduction does not have a leading thread, and all points do not support well the general direction of the manuscript. Similarly, Section 2 presents various aspects in a random order, and does

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not support a solid understanding of the background of the study area. Additionally, logical problems also occur within sentences, some of which are unnecessarily long, use very imprecise words and non-scientific wording (e.g. "The authors present . . ."). The manuscript would therefore highly benefit from a complete reworking of the structure and, in some instances, re-writing.

Conceptually, the manuscript presents a few important flaws. Firstly, in few instances the authors present this study as a potential basis for a Quick Response Protocol (QRP), but the study itself mostly illustrates one application of such a deployment. Although it is mentioned that this study took place in the context of a EU project, no background is given and the reader is left to wonder what is the broader context and about the nature of the relationship with the Early Warning System as well as the local policy makers. I recommend removing any mention of a "protocol" and focus on the application at one case study. Second, the title of the manuscript contains impact assessment, which implies a quantification of the impact due to the storm either on the built or natural environments. No quantification as such is presented in the manuscript, and only parts of the changes of the morphology of the beach is qualitatively investigated. Thirdly, it is mentioned in the method and in the discussion that interview with residents were performed. However, no detail is given on the procedure or the purpose, and at no point any attempt is made to include (or even mention) the results of such interviews in the more global result. Why? Finally, the authors present the result as a potential benchmark to assess the discrepancies between RTK GPS, UAV and LiDAR-derived topographic products, which is not the case. A global comparison should include statistically robust tests and the transparency of the data. The present manuscript lacks critical information such as the quality report obtained from Pix4D, the error on the GCPs (amongst other) required for a comprehensive comparison. Additionally, i) only in the last section are the flaws on the GCPs presented, which have a first-order control on the accuracy of UAV-derived DSMs and ii) no benchmark area was identified to estimate the error between the three datasets where no change has occurred.

Scientifically, my main is related to the application of the UAV survey, which lacks important steps to assess and reduce the uncertainty. First and foremost, considerations regarding GCPs made in Section 6.2 are typically made before the deployment, any many options for designing and placing efficient GCPs exist, most of them being thoroughly presented in the user manuals of the most common SfM softwares. As a result, limitations presented in Section 6.2 should be presented in the methodology section along with a quality report of the error on the GCPs, as this step has a first order importance in the accuracy of the results. Second, it is difficult to understand why a manual flight plan was preferred over an automatized one, which provides consistency on the overlap of images. Thirdly, the workflow presented in Fig. 5 only shows the automatic workflow implemented in Pix4D, but an important step, namely the manual cleaning of the point cloud, has been ignored. This step is critical to reduce the noise of the densified point cloud, which greatly influences the accuracy of the resulting DSM. Fourthly, the error of all UAV-derived products, particularly when it comes to change detection, should be critically assessed and reported based on such outputs as distortion or point density maps. The authors could use supplementary material to provide this information. Finally, no real scientific results are presented on the impacts, and only some qualitative descriptions of the changes on the beach morphology are reported.

As a result, the present manuscript is hard to judge. On one side, the manuscript promises global conclusions (i.e. protocol, impact assessment), but results suggest that the manuscript should rather focus on the application to one case study. On the other side, most results and conclusions focus on the method, which is not as constrained compared to photogrammetric studies published in the literature, and the true science that could be derived from the method is mostly neglected. In this context, I must mention that I understand the complications associated with UAV surveys and RTK GPS ground-truth, and the limitations of the accuracy of the method should not be a factor preventing the publication of such a study, for as long as i) limitations are thoroughly and transparently presented from the beginning and ii) the method is used to support science. Therefore, I feel that this manuscript would deserve to be published once i) objectives are toned-down to consider the application to one case study rather than pretending to serve as a basis for a protocol and ii) more quantitative science is put forward based on the result of the UAV survey. For these reasons, I recommend major revisions and a possible resubmission.

Please find below some general comments. Other comments are also included in the annotated PDF file.

Introduction Too general, does not really frame the project. The introduction of UAVs mainly builds upon the limitation of RTK GPS. It needs a stronger, clearer logical workflow.

Section 2 & 3 Both sections should be merged into a generic “Case study” section. I have a problem with the logic used in the presentation of the background data. For instance, Section 2 provides elements of the physical geography and morphology of the study area at various scales, the history of feedback between urbanization and response on the natural systems, previous projects, policy and management, climate and classification of storms in a random order that is hard to follow. In particular, the classification section illustrates parts of the illogical ordering of the manuscript: first, the classification scheme is barely used throughout the paper and could be summarized in a Table; second, the final sentence of the last paragraph of Section 2 classifies the studied storm, even before its presentation in Section 3.

Section 5.1 Please describe the results of the SfM algorithm in a table (i.e. number of images used, number of images validated, overlap, errors on GCPs etc… i.e. Pix4D report). Additionally, point clouds are usually manually cleaned before generating the DSM in order to reduce the noise. Subjective steps, such as outlier removal and curve smoothing, are mentioned in the text, which probably wouldn’t be required if the dense point cloud had been cleaned

Section 5.2 This section is weak as it only presents a 2D validation of the UAV-derived orthomosaic (which is usually more reliable than the 3D geo-referencing), whereas other potential research questions are ignored. What is the maximum water height required to inundate the farthest point observed? How does such an estimated height compare to observed floodmarks? Additionally, Fig 7 suggests that the so-called "secondary inundations" all occur in private properties. Were these observations validated by interviews? Have potential mitigation measures been identified in the field?

Section 5.3 There is a confusion between DSM, DTM and DEM. If the UAV-derived product has not been treated and contains elements on the beach, then it is indeed a DSM (as is the output of Pix4D). Please clarify.

Additionally, it would be useful to identify zones unaltered by the storm in order to compare the alignment of the LiDAR and UAV datasets.

Section 6.2 The points listed in Section 6.2 are important limitations to the presented approach and subsequent results and should be presented in the methodology section. Figure 9 demonstrates that many GCPs are potentially misleading, i.e. too small, round features, or objects that are easy to move (Fig. 9B). It is therefore difficult to trust the result of the UAV-derived DSM when such considerations are made after rather than before the field deployment. It is therefore necessary to assess the quality of all GCPs in the target area and show it on Figure 1 (i.e. colormap showing the quality of the GCPs). Additionally, these aspects make it even more important to report the error on the GCPs in a table.

Other - The manuscript contains many long sentences - There is often poor logic in the construction of sections, paragraphs and sentences. Adverbs such as "however" or "notably" are often misused and complicate the understanding of the sentences - The use of "the authors" should be... should not be! Caesar died more than 2000 years ago, time to move on! Use an impersonal form if possible - There should be consistency in the way you refer to drones. UAV is the most frequently used denomination - There is a frequent use of very general and unconstrained terms namely "data" or "wide". Be as specific as possible - Always add a space

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between values and units - Please double check any reference to DSM vs DEM. I think there is confusion there. Unless filtered, the point cloud of Pix4D produces a DSM

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-337/nhess-2017-337-RC1-supplement.pdf

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