

Dear Editor and Reviewers,

We are grateful to the anonymous referees for the useful comments and suggestions. I am pleased to upload the authors' answer to the referees. In the following we copy-paste the referees' comments and add the authors' answers (in bold) with the initial label A.#.§, where # is the referee (1 or 2) and § is a sequential integer indicating the ID of the answer.

Additionally, the authors' general short comment posted during the open discussion (nhess-2017-337-SC1) is reported in the final part of this document. This part will be referred to in order to support the specific authors' answers (labelled A.#.§).

Given the major changes proposed by the authors in order to address the reviewers' comments and suggestions we also propose to change the title and the order of the authors, as follows:

"Integrating Regional Protocols for Post-Event Assessments with Local UAV-based Surveys: the Emilia-Romagna (Italy) case study" by Duo, E., Trembanis A. C., Dohner, S., Grottoli, E. and Ciavola, P.

We are looking forward to receive the final decision of the Editor about this manuscript.

Best regards,

Arthur C. Trembanis, on behalf of the authors

REFeree #1 (nhess-2017-337-RC1 and nhess-2017-337-RC1-supplement)

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 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.

A.1.1 We thank the referee #1 for this general comment. The highlighted issues and suggestions were very useful. We propose restructuring of the manuscript on the basis of the information contained in nhess-2017-337-SC1 (short comment from the authors to reviewers). In particular, (i) the local scale survey will be contextualized within the regional protocol for post-storm assessment; (ii) the title will be modified; (iii) the interviews will be included; (iv) the photogrammetric process and UAV-derived outcomes will be reviewed and improved.

The manuscript does not aim at comparing the survey techniques, demonstrating that UAV are better than GPS or Lidar-based methodologies. The capacity of UAV for beach survey was already demonstrated by several studies (that are included in the manuscript). The reason why we present UAV-derived data is for demonstration purposes, showing the level of detailed data (i.e. orthomosaic and digital elevation model) that this methodology can provide for rapid post-event surveys, damage assessment, and flooding extent. This approach aids regional managers by integrating large scale assessments with local, fine scale approaches, where necessary.

Manual flights were completed during all UAV surveys due to a lack of automated mission planning software available for the DJI Phantom Vision 2+ at the time of these surveys. Software companies released updates to include “follow me”, “waypoint”, and “hover” capabilities in March of 2015 but no systematic mapping option. Thus manual flights were conducted to the best of the authors’ abilities with available objects on the beach as GCPs. At the time of this study, recommended GCPs per area varied wildly and was left to the discretion of the surveyors. The debris on the beach was not always visible in the resulting aerial images, therefore DSM error varied greatly from one flight to another. This caused the authors to choose a section of the study site where the available GCPs created a reasonable DSM with acceptable errors to compare to GPS data. Minor point cloud editing occurred over the selected DSM to remove major outliers but surface object such as umbrellas and beach chairs were kept. The choice to use beach debris stemmed from the authors’ goal to show rapid data collection following the extreme storm event where controlled GCPs such as painted boards and tarps are not always feasible from a time, effort, or economic standpoint for local authorities. Therefore, the suggestions in Section 6.2 are aimed at readers using readily available objects as GCPs rather than systematic seeding of the study site with standardized GCPs.

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.

A.1.2 The introduction will be reviewed and improved (see nhess-2017-337-SC1). We will stress on the importance of local scale assessments in support of regional ones. In order to quickly survey local hotspots and provide accurate data, minimizing the costs, UAVs are preferred for this type of studies.

We will add these recent studies to improve introduction and discussion:

Moloney, J.G.; Hilton, M.J.; Sirguey, P., and Simons-Smith, T., 0000. Coastal dune surveying using a low-cost remotely piloted aerial system (RPAS). Journal of Coastal Research, 00(0), 000–000. Coconut Creek (Florida), ISSN 0749-0208

Seymour, A.C.; Ridge, J.T.; Rodriguez, A.B.; Newton, E.; Dale, J., and Johnston, D.W., 0000. Deploying fixed wing unoccupied aerial systems (UAS) for coastal morphology assessment and management, Journal of Coastal Research, <https://doi.org/10.2112/JCOASTRES-D-17-00088.1>

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.

A.1.3 The Sections 2 and 3 will be reviewed and improved as suggested (see nhess-2017-337-SC1). Additionally, the new section will include a review of the Emilia-Romagna alerting and monitoring protocol. The subsection related to the regional setting and case study site will be reviewed and lightened. The subsection related to the storm event will be enriched with information on the activation of the regional protocol during and after the emergency.

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.

A.1.4 The requested results from the SfM processing will be provided. With respect to the point clouds, major outliers were removed before surface processing however, the authors preferred to maintain as much data as possible and then chose to refine the surfaces via MATLAB processing. This is acceptable for point cloud pruning as well as GCP error calculations.

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?

A.1.5 We will answer to these questions in the text also including information collected through the interviews (see also A.2.5). About the "secondary inundations", the observations were validated by the team that documented with on the ground pictures. Potential mitigation measures were identified in the framework of the EU FP7 RISCKIT project, through specific interviews performed months before the event and the survey here reported. However, it is beyond the aim of this manuscript to include that analysis.

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.

A.1.6 It is the DSM and the authors will update to the correct terms through the manuscript. We also agree that the manuscript would benefit from adding the proposed comparison on unaltered areas. We will look for suitable areas to conduct such a comparison.

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.

A.1.7 GCP limitations from this study will be included in the methods section. However, GCPs associated with high error were discarded in point cloud processing and therefore have no influence upon outputted surfaces. These choices will be discussed within the methods sections so readers are more aware of the authors' reasoning behind processing choices and resulting data. The suggestions for choosing GCPs is a valuable section for readers wishing to utilize in-situ objects as GCPs and thus the authors feel this is a valuable contribution to the paper and the readers.

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 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.

A.1.8 We will address all these comments.

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>

A.1.9 We thank the referee #1 for the valuable comments in the supplement material. We will address them in the revision of the manuscript.

REFeree #2 (nhess-2017-337-RC2 and nhess-2017-337-RC2-supplement)

General comments: The general topic and assessment procedure presented for the manuscript entitled Quick Response Assessment of the Impact of an Extreme Storm Combining Aerial Drone and RTK GPS by Trembanis et al. possesses potential, particularly for practical efforts on the ground to improve recovery in coastal areas facing extreme weather conditions. This is particularly the case for storms producing flooding in local communities. However, substantial revision is needed to make this a solid article. The main issues that need to be addressed are: 1) the lack of clarification in the text specifically with regard to the Quick Response Protocol (QRP) explanation; 2) the need for clarification of texts in a fair number of other instances (see the continued contents of this report for details in addition to the supplemental material); 3) the need to completely overhaul the explanation of the qualitative component of the research as it pertains to the interviews conducted with local stakeholders; and 4) a revision of the general verbiage and sentence restructuring throughout the text (again, please see the rest of this report and supplemental material for reference).

This reviewer also agrees, generally, with the comments made within the first submitted report from Anonymous Referee #1. The following sections of this report provide specific comments organized by manuscript sections, while the supplemental PDF provides both comments in their textual reference as well as technical corrections. The technical corrections provided in the PDF also contain suggestions for improving the text.

A.2.1 We thank the referee #2 for the general comments. Again, the highlighted limitations and suggestions were very useful and we believe that the proposal included in nhess-2017-337-SC1 will address them.

Abstract: There is a need for greater specificity with what is meant by the “comprehensive approach” and the “timing information” that supported the activities of the research. What exactly makes this approach comprehensive? The authors later in the body of the main text make brief mention of the qualitative component of the research. Is the combination of the two what makes this comprehensive? If so, that part of the approach should be mentioned within the abstract text. The official name of the regional EWS should also be used. The abstract would also benefit from having at least one or two of the actual findings provided in the end of the abstract. As it stands, the content contains primarily method description. Providing results or content framed as some kind of key findings would greatly strengthen this abstract.

A.2.2 We will completely review the abstract on the basis of the new version of the manuscript (see nhess-2017-337-SC1).

Introduction: The introduction also focuses very generally on method, providing little mention or explanation of the protocol presented in the research. The introduction also does not address the qualitative part of the research, namely interviews that were conducted (this is not described). A general reframing of the introduction is needed to better set up the structure of what will be presented in the sections that follow.

Some points of clarification in the text are also needed. Particularly in line 25, “to ensure appropriate plans are enacted”, using “plans” is vague. Would this pertain to general land use plans, coastal territorial plans, or developmental plans? This section would benefit from better connection to the kinds of planning tools that the information presented in this research supports. This may also help the research reach a broader audience. Another point is found in line 29, “to assess the impacts to the coastline after the storm,”. Please be more specific. How long after the storm? And why? Would one wait until authorities deem it safe to survey and ensure impacts are assessed as soon as possible to prevent loss of data? Stating “before either natural or human induced recovery process begin...” is not specific enough (the authors should at least state “as soon as possible” prior to these recovery processes). The reason I stress this point is in connection to the practical application of the presented methods. More specifically, how long would a scientist or practitioner wait post-storm to perform these methods and how long is too long a wait (or is this so context specific that there is no way to give a general indication).

Although the introduction makes an attempt to connect to the research to planning and coastal management. This connection can and should be strengthened by providing more elaboration on potential uses of this information and also include mention of particular types of stakeholders such as governmental entities and NGOs.

A.2.3 The introduction will be reviewed and improved (see nhess-2017-337-SC1). Additionally, the specific comments will be addressed and the requested information will be included. See also answer A.1.2.

Section 2: I agree with the previous referee that this section should be renamed as “Case study” rather than “Study area”. I also agree that the information in Section 3 “Storm event” should be part of the case study description in Section 2. The event itself is part of the case study setting and the selection criteria for this location of study. Within the “Case study site and target area” section, there is also no mention of why this particular target area was selected as opposed to others. Why was this particular portion of the coast chosen? If there are unique geophysical characteristics that make this a “unique case” with regard to your selection criteria, this should be stated.

It would also be beneficial to have greater specificity as to what kinds of tourist facilities exist in the case study in order to understand the kind of land usage and potential for damage and general economic impact. The next sentence tries to address this in part, but does not address the types of tourist facilities (or types of residential). E.g. are these high density establishments? Very little demographic or land use information is given in demonstrating an understanding of the case study.

Section 3: This section requires some sentence revision and several points of clarification. Suggestions and elaboration of these points are found in the supplemental material submitted with this review.

A.2.4 Please, see nhess-2017-337-SC1 about the new proposed section "Case study". The requested information on the tourist facilities will be included. See also answer A.1.3.

Section 4: The first sentence of this section requires major revision. One of the most critical issues in this section (and indeed the manuscript as a whole) is the complete lack of explanation for the collection and analysis of data from stakeholder interviews.

The importance and purpose of the interview method should be provided much earlier than in section 4. This should have been a part of the abstract and introduction, and especially should have

had more elaboration in the latter. Important questions that should have been addressed with a proper explanation of the method include: What kinds of interviews? (E.g. structured, semi-structured with open or closed questions?) Were these individual or group interviews? What was the purpose of these interviews? What type of data was collected? What kinds of "local" stakeholders did you interview? And how were they selected? What questions were asked?

The way this is presented, the reader has no idea whether the authors simply walked around the area asking random questions to random people (the brief inexhaustive list in the first bullet is not sufficient explanation). There is no scientific process presented and no explanation or transparency in communicating how this field method was used. Either fix this, or remove this qualitative component altogether from the manuscript. As is, the explanation of this method brings into question the scientific rigor and general quality of the research.

The first three critical tasks listed in bullet points on page 6 are not adequately described in their procedure or parameters. For the first bullet point related to the stakeholders interviewed, the following questions should be addressed in the text: Why these stakeholders? What is (or was) the saturation point or parameter for sufficient representation of stakeholder types?

In general for the QRP, there really needs to be a more structure presented with regard to the sequence of the protocol steps with more elaboration. The sequence of steps could also benefit from a visual illustration (e.g. at least a workflow diagram with minimal explanation).

For line 24 on page 6 that reads "In this study, 7 days were sufficient to complete the aforementioned tasks", not enough detailed is provided. What were the parameters for sufficiency of each of these steps, and how were they met? Within that same paragraph, for the "error analysis and data comparison", what is the significance of these numbers? How do they contribute to the robustness of your research design and execution?

For line 1 on page 7, stating that "The integrated information will help to understand the overall impact of the storm in the surveyed area", is a big promise. However, there is little explanation of how this is achieved. Stating that interviews were conducted with a vague explanation of purpose and nearly zero method description does not automatically mean that the data collected from these interviews was integrated into the broader research pursuit. How exactly was this data analyzed? It needs to be very clear how this qualitative data was used, and prior to this, what type of data was collected in the first place. (E.g. What questions were asked? Did this enable gathering data on risk perception? Identifying priority areas?)

A.2.5 The new reviewed version of the manuscript will include specific information on the performed interviews (methods, results and discussion; see nhess-2017-337-SC1) and how the collected information supported the local survey. In particular, the interviews were performed asking informal questions to a limited number of people (10 stakeholders: residents, shop or tourist activities owners, fisherman, fireman) focusing on what happened the night of the peak of the event, how they behaved, what damages did they get to properties, etc. The information was mainly used to better organize the fieldwork but some interesting insights on the regional alerting and response chain were highlighted. Additionally, the local protocol will be reviewed and, as proposed in nhess-2017-337-SC1, it will be contextualized within the regional protocol for post-storm assessment.

Section 5: This section needs some sentence revision (see supplemental attachment). The results and findings that have been integrated from the interviews is a missing component to the content of this section.

A.2.6 Please, see nhess-2017-337-SC1. We will revise the text in order to rephrase the sentences, as suggested.

Section 6: In reference to line 6 on page 11, it should be made very clear what kind of time and other resource saving is made possible and/or enhanced using this protocol and the described methods. There are also several instances that would greatly benefit from more specification (e.g. see instances were “increase”, “really detailed”, and “prolonged” are used).

A.2.8 We will address this comment adding the requested information.

Within section 6.2, for the last bullet point guideline on page 12, please explain why this was done and how these points would have influenced your results. Line 9 on page 12 also mentions that the team was divided into thematic groups. Explanation is needed for what these thematic groups were and how they were determined.

A.2.9 The Section 6.2 does not include information on what was done. It includes suggestions on how to improve the efficiency of the local survey. It will be reviewed as proposed in nhess-2017-337-SC1 and more specific information will be added.

Section 6.3 needs further elaboration on what kinds of further analyses should be performed and what kinds of deeper investigations would create a more robust outcome. The statement at the end of this section on potential uses of the protocol is good. However, this use potential and connections to practical application should be made in the beginning of the manuscript as well. The text prior to this section does not provide adequate detail into use potential and connections to practical application.

A.2.10 This section will be revisited as proposed in nhess-2017-337-SC1 and merged in a single section with suggestions for improvements.

Section 7: The sentence in line 28 beginning with “Limitations of the application” is what one would call a “cookie-cutter” sentence that can be copy and pasted into any manuscript as it contains no specificity or uniqueness to the research presented. The sentence needs revision with some hint at the specific limitations and recommendations provided for this research.

A.2.11 We will completely review the conclusions on the basis of the new proposed version of the manuscript (see nhess-2017-337-SC1).

Please also note the supplement to this comment: <https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-337/nhess-2017-337-RC2-supplement.pdf>

A.2.12 We thank the referee #2 for the valuable comments in the supplement material. We will address them in the future version of the manuscript.

Authors' answer to referees #1 and #2 - Proposed changes - General overview (nhess-2017-337-SC1)

The authors of the manuscript “Quick Response Assessment of the Impact of an Extreme Storm Combining Aerial Drone and RTK GPS” by Trembanis et al. (nhess-2017- 337) are grateful to the reviewers for their useful comments and suggestions. The authors are willing to improve the manuscript, addressing their comments.

On the basis of the two reviews we propose a restructuring of the manuscript, addressing the main limitations highlighted.

The proposed changes will indeed:

- i. better contextualize the local survey implemented at Lido degli Estensi within the regional protocol implemented by the regional authorities;
- ii. address the lack of information on the local interviews and the collected qualitative information.

In the following we summarize the main changes that we propose to implement:

- a) the title will be changed in "Integrating Regional Protocols for Post-Event Assessments with Local UAV-based Surveys: the Emilia-Romagna (Italy) case study";
- b) the "Abstract" will be completely revisited;
- c) the "Introduction" will be revisited and specific paragraphs will be added focusing (i) on the existing regional protocol for emergency and post-event assessment and (ii) on the importance of local stakeholders' involvement for coastal studies;
- d) the section "Study Area" will be renamed as "Case study" and will be revised; the new section will include (i) the regional setting and the description of the local case study, revised on the basis of the reviews; (ii) a review of the protocol for coastal alert and monitoring of the Emilia-Romagna Region with focus on the importance of the regional EWS and the methods for post-event assessments; (iii) a thorough description of the February 2015 event, including the regional implementation of the protocol for coastal alert and monitoring described in the paper (in italian) by Perini et al. (2015b);
- e) the "Methods" section will be revised: the current section 4.2 will be deleted; the local protocol description will be restructured and additional information on the performed interviews will be added; the description of the photogrammetric process will be reviewed; minor changes will occur in the other subsections, addressing the specific comments;
- f) the "Results" section will be improved adding the qualitative information collected through the local community, minor changes will be applied in the other subsections;
- g) the "Discussion" section will be enriched by adding a discussion of the outcomes of the interviews including references to standard protocols for stakeholder involvements that can improve the quality and reliability of the collected information; moreover, a discussion of the outcomes of the local survey will be added focusing on how the local assessment could be integrated in the regional scale assessment; the existing text will be revisited;
- h) "Practical and general recommendations" will be reviewed, better contextualized and merged in a separate section named "Suggestions for possible improvements";
- i) "Conclusions" will be reviewed accordingly with the new manuscript structure with emphasis on the possible improvement of the existing regional protocol with local assessments based on the proposed methodology;
- j) logic flaws and writing will be improved, as suggested, and all the other specific comments will be addressed.

Thus, the new structure will be as follows:

- 1. Introduction
- 2. Case study
 - 2.1. Regional settings and case study site
 - 2.2. Coastal alerts and monitoring in Emilia-Romagna
 - 2.3. Storm event
- 3. Methods
 - 3.1. Quick Response Protocol
 - 3.2. Stakeholder interviews
 - 3.3. Ground GPS survey

3.4. AUV survey and Ground Control Points

4. Results

4.1. Summary of the interviews

4.2. Topographic profiles and Digital Elevation Model surface

4.3. Coastal flooding

4.4. Erosion and sedimentation patterns

5. Discussion

6. Suggestions for possible improvements

7. Conclusions

We hope that the proposed changes will meet the Editor's and Reviewers' expectations. We are looking forward to receive the Editor's decision on the further handling of the manuscript.

Best regards,

Arthur C. Trembanis, Enrico Duo, Stephanie Dohner, Edoardo Grottoli and Paolo Ciavola