Dear Editor and Reviewer,

Please find below, our response to the reviewer's comments regarding the manuscript entitled, "The Influence of Infragravity Waves on the Safety of Coastal Defences: A Case Study of the Dutch Wadden Sea" for publication in Natural Hazards and Earth System Sciences.

Firstly, we would like to thank the reviewer for their very positive and encouraging feedback on the manuscript. We found their suggestions for improvement very helpful and we were pleased to see that both reviewers shared similar concerns. The areas identified will be adequately addressed and improved in the updated manuscript.

In the following paragraphs, we detail our position (in blue) regarding the modifications we will make to the manuscript in response to the reviewer's comments (in black).

Comments from Reviewer #2:

1. General: In my opinion the manuscript fits well with NHESS and addresses relevant scientific questions for the journal audience and even wider readers. This manuscript clearly provides a relevant contribution to natural hazards and their study, namely by analysing the impact of infragravity waves in coastal structures.

   We thank the reviewer for this comment.

2. General: The scientific quality is in my opinion excellent. I have followed the discussion between the fellow reviewer and the authors. I was very pleased to see that all the issues raised by my colleague were quickly address and that the authors recognized many of the limitations point out. Nevertheless, I must say I found the scientific approach correct. It is multidisciplinary and despite relying mostly on numerical modelling approaches, the field validation guarantees reliability of results presented. The fact that the manuscript is superb in English and structurally helps the reader a lot. Furthermore, I also think the figures are of good quality. In a sentence, the results are presented in a clear, concise, and well-structured way. Figure number and quality are suitable and very informative.

   We thank the reviewer for this comment.

3. Discussion: Having said this, I believe the case for the relevance of infragravity waves is not sufficiently stressed and more conclusive and clear evidences are missing. This was also noted by the other reviewer and it is a crucial aspect of this work. It is stressed in the title, in the abstract, etc. but results do not seem to so clearly demonstrate the reasoning forward. I believe the authors must be less enthusiastic and more cautious when writing the discussion. It is crucial that they address the shortcomings and discuss reasons for the poor discrimination made (for example on Figure 10). I am also curious about the error associated with the models and would like to see that clearly mentioned in the methods. The approach used is a succession of different model data and I am wondering if the sum of errors is not above 20%... I am mentioning this because in the abstract you describe increases of 1.1.to 1.6…

   We thank the reviewer for highlighting this. To account for the error in the empirical models, the estimates are multiplied by normally distributed factors with mean values and standard

deviations to represent the bias and scatter (errors) associated with each model. This is already described in Section 2.3.2 (Methods) and summarized in Table A.2 (Appendix). Likewise, the error (uncertainty) in the SWAN numerical model is captured in the breaker parameter (Equation 4) which is treated as a stochastic parameter with a standard deviation of 0.05 (Section 2.3.1). We will add a discussion on the error (uncertainty) associated with each model and how they are accounted for in the probabilistic framework to Section 4.1 (Discussion), which is dedicated to the modelling approach taken.

It should be noted that the errors (uncertainty) associated with each of the numerical and empirical models are shown in Figures 6 to 8 as error bars.

The combined error (or uncertainty) may be expressed using a combined coefficient of variation, which is equal to the combined standard deviation normalized by the combined mean. If we consider the means and standard deviations of Equations 4, 12, 13 and 14, the combined coefficient of variation or uncertainty is 0.15 or 15%. We will also make a note that the influence of the infragravity waves at locations where the factor increase in failure probability was less than or equal to 1.15 may be considered minor given that the combined uncertainty in the models applied is of the same magnitude.

4. Discussion: Furthermore, roughness is never mentioned and I think it is a crucial physical aspect when we are discussing overtopping. On line 564 you state: "the influence of saltmarsh vegetation on coastal safety under extreme forcing remains an important issue for future research." I was somewhat disappointed that this theme was not further discussed as it deserves. So, my suggestion is to add a paragraph further discussing this topic after line 600, for example.

   We thank the reviewer for highlighting this. Bottom roughness is indeed a wave dissipation process to be considered here. We will modify the description of the scenarios (Section 2.2.4) so that dissipation by bottom roughness is also mentioned. We agree with the reviewer that wave overtopping is sensitive to slope roughness; however, here we do not consider roughness elements and the dikes at each location are assumed to be smooth. This assumption will be clearly stated in Section 2.2.1 where the dike-foreshore characteristics are described.

   We will also include a review of literature that addresses the effect of dike roughness on overtopping.

   The influence of saltmarsh vegetation is discussed in Section 4.4, lines 553 to 565. We agree with the reviewer that the theme is important and deserves further discussion and research. However, more measurements of wave-vegetation interaction under very severe storms is needed before further conclusions can be drawn. Here, we discuss our vegetation results based on measurements taken during two storms (with exceedance probabilities of 1/ per year); but given the uncertainty surrounding vegetation and whether or not it will remain standing under more severe conditions, we are unable to expand on this further.

5. Materials and Methods: Finally, I am worried with the limited number of events analysed and with the narrow spatial distribution studied. To support some of the bolder statements regarding the relevance of infragravity waves the authors should have provided a more extensive database. Despite this, I feel this is a very good contribution to this scientific subject and deserves to be published on NHESS after some minor changes are made.

Here, we apply numerical and empirical model tools that were previously validated using physical model tests and now validated here against field data. Nonetheless, we agree with the reviewer that a larger database of field measurements would improve the manuscript. As field measurements under actual storm conditions are difficult to obtain, we made use of the most appropriate dataset available for the Dutch Wadden Sea, to-date. In our manuscript (last paragraph of the Conclusions), we recommend that additional field campaigns focused on measuring infragravity waves be carried out in the Dutch Wadden Sea to provide the data necessary to further validate and support the conclusions drawn here.

6. Conclusions: Another aspect is the extention of the Conclusions. They must be more focused and a couple of paragraphs could be deleted as they are very generic.

   We thank the reviewer for highlighting this. We will modify the conclusion so that is more concise and less generic.

7. References: A final note on some self-citation and what I consider to be an average reference list. There are a few classic papers missing….

   We will add more classic references to the manuscript where appropriate. This work brings together several tools and methods that were developed in previous works by the authors; hence, the apparent self-citation. We will minimize this where possible.

8. General: Case presented is not sufficient to prove major importance of infragravity waves.

   Our findings indicate that neglecting infragravity waves results in an overestimation of safety. We also highlight that the magnitude of the impact will differ per case, as it is dependent on local conditions (forcing and bathymetry). For the case considered here, the change in failure probability was by a factor of 1.1 to 1.6 times. This may be considered minor but it should not be assumed that IG waves would have a similar impact at other coastlines. We will make a note of this in the abstract and conclusions.

9. General: Error associated with models and poor discrimination.

   See our response to Comment #3 above.

10. General: Roughness.

    See our response to Comment #4 above.

11. Conclusion must be shortened.

    See our response to Comment #6 above.