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.

We appreciate the time and effort exerted by the reviewer on our manuscript. We found their questions and suggestions very helpful, as they have highlighted areas where we can further clarify our writing and explanations. 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 #1:

1. Abstract, line 12: I don't think these foreshores have in many cases been incorporated in the design.

   The reviewer is correct that neither effects of foreshore vegetation nor the impact on IG waves are currently considered. However, the effects of the foreshore bathymetry on incoming wind-sea and swell waves is indeed taken into account. This is considered the standard practice in the Netherlands and is mentioned in the subsection "Foreshore Scenarios" in Section 2.2.4 (lines 303 to 304).

2. Abstract, line 12: overtopping volume, so not binary

   Yes, it would be more accurate to say "reduce the likelihood and volume of waves overtopping...". We will make this change in the revised manuscript.

3. Abstract, line 22: not position relative to the inlets and channels?

   That is correct that there is correlation with the inlets, since areas behind inlets are exposed to larger waves than areas behind the islands. We will amend the text as follows:

   "locations with larger offshore waves-such as those behind tidal inlets--and shallower water depths."

4. Line 30: I would say "the foreshore contributes to a reduced load and therefore reduced likelihood of failure"

   We thank the reviewer for their suggestion. We will amend the text to match.

5. Line 31: ref Dalrymple, mendez and Losada.

   We thank the reviewer for the suggested references. We will add the following:

   Dalrymple, R. A., Kirby, J. T., and Hwang, P. A. (1984). "Wave Diffraction Due to Areas of Energy Dissipation." Journal of Waterway, Port, Coastal, and Ocean Engineering, 110(1), 67-79

Mendez, F. J., and Losada, I. J. (2004). "An empirical model to estimate the propagation of random breaking and nonbreaking waves over vegetation fields." Coastal Engineering, 51(2), 103-118.

6. Lines 44-45: maybe not explicitly, but implicitly they have been in empirical formulas which relate the offshore forcing parameters to overtopping, and thereby include the intermediate processes.

The reviewer is correct that empirical formulae based on offshore forcing parameters would implicitly account for the intermediate processes. Therefore, risk assessments that make use of such formulae would indeed account or the effects of infragravity (IG) waves.

The formulae used in the Netherlands (and the wider Europe, see EurOtop Manual) are based on parameters at the toe. For conditions with shallow foreshores, these formulae e.g. Van Gent (1999) would indeed already account for IG motions. However, it is standard practice to use a phase-averaged wave model, like SWAN, to obtain the boundary conditions at the dike toe. As these models tend to exclude IG wave dynamics, these waves are often not considered in the analysis.

We will amend the text to make it clear that empirical formulae based on offshore forcing parameters would indeed account for these processes.

7. Line 46: but it is corrected for if empirical formulas are used.

The reviewer is correct. In this manuscript, we make use of such an empirical formula to correct or the infragravity waves.

8. Line 60: i am sure there is a classical reference.

We will add the following reference:

Van Gent, M. (1999). "Physical model investigations on coastal structures with shallow foreshores: 2D model tests with single and double-peaked wave energy spectra." Hydraulic Engineering Reports, Delft.. ”

9. Line 68: ref.

We will add the following reference:

Roelvink, D., Reniers, A., Van Dongeren, A., Van Thiel de Vries, J., McCall, R., and Lescinski, J. (2009). "Modelling storm impacts on beaches, dunes and barrier islands." Coastal Engineering, 56(11-12), 1133-1152.

10. Lines 70 -72: was the problem the "case" (ie. combination of profile and/or forcing conditions, or the "method" ie using xbeach.

Our findings suggest that the problem was "method" specific; not due to the use of XBeach Surfbeat (numerical model) but due to the use of inappropriate empirical formulae to estimate the actual overtopping discharge. This is discussed in detail in Section 4.2 along with the findings when different overtopping formulae are applied.

11. Line 82: contrasting?

    We chose the word "inconclusive" with reference to the work of Oosterlo et al. (2018) that suggested that further work was needed before the influence of the IG waves could be described with confidence. We will add this citation to the text:

    Oosterlo, P., McCall, R., Vuik, V., Hofland, B., Van der Meer, J., and Jonkman, S. (2018). "Probabilistic Assessment of Overtopping of Sea Dikes with Foreshores including Infragravity Waves and Morphological Changes: Westkapelle Case Study." Journal of Marine Science and Engineering, 6(2), 48.

12. Figure 2: aren't SS parameters and vaklodingen input into the empirical formulas?

    We thank the reviewer for highlighting this. We will adjust the figure so that inputs are correctly described.

13. Line 129: plus: storms occur in the winter season with little vegetation present

    That is correct. We will add this to the text.

14. Figure 3: what is there to reverse shoal on etabar?

    We thank the reviewer for highlighting this. We will move the etabar for clarity.

15. Figure 3: typo

    We thank the reviewer for spotting this!

16. Figure 3: write out the caption, it should be understandable without having to read the text.

    For clarity, we will expand the caption to describe each of the parameters.

17. Line 141: ref

    We will add the following reference:
    Booij, N., Ris, R. C., and Holthuijsen, L. H. (1999). "A third-generation wave model for coastal regions - 1. Model description and validation." J Geophys Res-Oceans, 104(C4), 7649-7666:

18. Line 143: in this paper? why? you add an empirical relation which should cost no comp effort.

    The reviewer is correct. The added empirical relation did not have a significant impact on the computational demand. However, the probabilistic model itself (FORM) does require computation effort--around 10 minutes per dike section. This is discussed in Section 4.1 (lines 478 to 483).

In the revised manuscript, we will describe this computational demand briefly here.

19. Line 144: but if the waves at the location of the hydra output were already breaking, you underestimate the offshore wave height by simply reverse shoaling with linear theory.

The reviewer is correct and this is acknowledged in the manuscript in lines 149 to 151. This approach is considered to be an approximate estimate here.

20. Lines 148 – 149: so you are comparing output at two locations?

Yes, that is correct. We compare estimates at the dike toe to estimates approximately 1 km offshore (obtained by reverse shoaling). We will amend the text so that this is clearly described.

21. Table 1: m

We thank the reviewer for spotting this!

22. Lines 162-163: no? obliqueness is a function of the orientation of the contour lines, so for dike sections that are not perpendicular to NW-SE you still get refraction

The reviewer is correct. However, our approach assumes a perpendicular dike orientation. This results in a milder slope than if the transect was considered from North to South. This approach is considered comparable (in terms of accuracy) to assuming obliqueness and using a "correction" factor in the empirical overtopping model.

23. Line 164: if your transect is not perpendicular to the contour lines you will get artificially milder slopes

The reviewer is correct. See our response to the comment 22 above. We have taken this into account in our average dike slope calculation. We will add the following text to describe this:

"It should be noted that assuming a NW to SW orientation results in an artificially milder dike slope for dikes that are not perpendicular to this transect. This is taken into account in the average dike slope calculation.".

24. Line 166: what is this based on?

This is based on the value used by Vuik et. al (2018) to represent the spatial and temporal variations in the bathymetric data of the Dutch Wadden Sea. We will add the following citation:

Vuik, V., Van Vuren, S., Borsje, B. W., van Wesenbeeck, B. K., and Jonkman, S. N. (2018). "Assessing safety of nature-based flood defenses: Dealing with extremes and uncertainties." Coastal Engineering, 139, 47-64.

25. Line 168: what this exceeded in the data? Ie where there milder slopes?

The calculated foreshore slopes ranged from -1:2350 to 1/124 with an average of 1/720; where a negative slope indicates a slight downward slope towards the dike.

However, our approach is in-line with the standard assumption (e.g. CLASH database (Steendam et al., 2004) and Keimer et al., 2021) that slopes milder than or equal to 1/1000 are all considered (near) flat/horizontal. We will add the following text to clarify this:

"It should be noted that the calculated foreshore slopes ranged from -1:2350 to 1/124 with an average of 1/720; where a negative slope indicates a slight downward slope towards the dike. However, here we consider all foreshore slopes <= 1/1000 to be (near) flat, in-line with the approaches of Steendam et al., 2004 and Keimer et al., 2021."

References:

Keimer, K., Schürenkamp, D., Miescke, F., Kosmalla, V., Lojek, O., and Goseberg, N. (2021). "Ecohydraulics of Surrogate Salt Marshes for Coastal Protection: Wave-Vegetation Interaction and Related Hydrodynamics on Vegetated Foreshores at Sea Dikes." 147(6), 04021035

Steendam, G. J., Van der Meer, J., Verhaeghe, H., Besley, P., Franco, L., and Van Gent, M. (2004). "The International Database on Wave Overtopping." Coastal Engineering 2004, 4301-4313.

26. Figure 4 caption: typo

We thank the reviewer for spotting this!

27. Figure 4 caption: how is the average slope determined?

The average slope is determined as a best-fit line (linear regression) of the foreshore elevation points from the dike toe to 1km offshore. We will add text to clarify this.

28. Lines 180-182: but that fact would lower the significance of including IG waves. So you should reflect on this in the discussion

The reviewer has a good point here. Actually, increasing the dike crest actually increases the influence of the IG waves, since the higher load needed for failure is reached earlier when IG waves are included. This is indeed discussed in Section 3.2.2 in our analysis of the effect of different crest levels.

29. Line 185: ref Booij et al 1999

We will add the following reference:

Booij, N., Ris, R. C., and Holthuijsen, L. H. (1999). "A third-generation wave model for coastal regions - 1. Model description and validation." J Geophys Res-Oceans, 104(C4), 7649-7666.

30. Equation 2: I thought it upgraded to Baldock formulations?

The Battjes and Janssen formulations remain the default in SWAN version 41.31A (used in this study) according to the user manual obtained here:

http://swanmodel.sourceforge.net/download/zip/swanuse.pdf.

31. Line 197: sqrt missing

We thank the reviewer for spotting this; we will correct it in the revised manuscript.

32. Line 198: and a mean of?

The mean value is determined using Equation 4 above. We will amend the text as follows for clarity:

"...standard deviation of 0.05 and mean value determined using Equation 4."

33. Equation 8: why these pre-determined frequency ranges? I would let that depend on the peakfrequency and then let the IG range go to 0.5*fp. see also your figure 16 which shows that the split frequency should be at f=0.1 Hz.

We agree with the reviewer that for conditions with a single visible high-frequency peak, that 0.5*fp is the more accurate separation frequency. However, in the Dutch Wadden Sea, the wave spectra typically show multiple peaks. In order to avoid contaminating the infragravity signal with that of swell (around 10 s or 0.1 Hz), a separation of 0.05 Hz is typically used.

In the revised manuscript, we will include an example of a measured wave spectrum to highlight this in the Appendix..

34. Line 215: true, but for design conditions, the peak period increases so the split freq will lower

As mentioned in our reply to the comment 33 above, we agree that the separation frequency should be dependent on the high-frequency peak for uni-modal spectra; however, for the Wadden Sea where spectra typically have multiple peaks, it is considered more accurate to have a fixed separation frequency. This is to avoid mixing swell and infragravity waves.

35. Equation 10: what is the effect of neglecting the variance above 1 Hz?

There is little to no variance above 1 Hz. Therefore, there is no impact on the calculation. We will add measured wave spectra to the Appendix to demonstrate this.

36. Line 219: ref

We will add the following reference:

Smit, P., Stelling, G., Roelvink, D., van Thiel de Vries, J., McCall, R., van Dongeren, A., Zwinkels, C., and Jacobs, R. (2010). "XBeach: Non-hydrostatic model." Report, Delft University of Technology and Deltares, Delft, The Netherlands.

37. Equation 11: the factor H_IG should be dimensionless

The reviewer is correct. H_IG is indeed dimensionless. The factor (0.36) has the dimension m^-1, which we forgot to state clearly. We thank the reviewer for spotting this oversight; we will correct the description in the revised manuscript.

38. Equation 11: is this the incoming component only?

This could be either the incoming component alone or incoming+reflected. It depends on the gamma_d factor which accounts for reflection depending on the structure slope.

We will add a statement to reflect this is the revised manuscript.

39. Equation 12: so \tilde{H_IG} has a dimension m^{1/2}?

It is dimensionless. See our response to comment 37 above.

40. Line 225: what are the values of these factors?

These factors can have values ranging from 0 to 2.5 depending on the various environmental conditions. In the revised manuscript we will describe/present how each factor is calculated.

41. Line 240: how much more gentle?

We will modify the text as follows:

"..typically 1:500 to (near) flat...".

42. Equation 13: dimensionless?

Yes, it is dimensionless.

43. Line 299: what about bottom dissipation?

Bottom dissipation is indeed present; however, this is expected to be minor compared to wave breaking. For clarity, we will modify the description so that dissipation by bottom roughness is included.

44. Figure 7: H_ig IS equation 12. what is the difference in input between the open circle and the solid?

We thank the reviewer for highlighting this. We agree that the current description is confusing and will adjust the manuscript to improve clarity.

H_IG is a stand-in for H_m0,IG,toe/H_m0,SS,toe, which can also be derived from the measurements (Equation 11 for definition). It may also be estimated empirically using Equation 12. We will correct this. We will adjust Equation 11 and its description to better reflect this.

45. Figure 7: how can \tilde{Hig} be measured? Does it take the observed wave heights? What about the gamma factors? how are they observed?

See our response to comment 44 above. We will adjust the text to clearly describe how H_IG may either be derived from the measurements or estimated using the empirical equation (Equation 12).

46. Figure 9: why a histogram and not a table?

Since the failure probability is typically quoted in powers of 10 (e.g. 10^-3 or 10^-6), we felt it would be easier to visualize the differences if plotted on a logarithmic scale.

47. Figure 9: what about SS+veg?

This was not originally assessed but we will add SS+veg to the plot for comparison.

48. Figure 11: this figure undermines your thesis that IG waves are important factors that are missing as the alpha values are small. This would be the takehome for me, so please correct the reader for drawing this conclusion

We thank the reviewer for this comment. The sensitivity factors only describe how sensitive the calculation is the uncertainty in the parameters (described by their standard deviations). It does not, however, account for the sensitivity of the results to the mean value.

Since the plot may distract the reader from the main results, we will move this to the Appendix.

49. Line 411: parentheses

We thank the reviewer for spotting this typo.

50. Line 418: see my earlier comment. repeat this statement above

We will adjust the text above to reflect the impact of the inlets.

51. Line 420: then why is the pf higher in the extreme west?

Points in the west are exposed to higher levels of wave forcing. This is made visible in Figure 13a where points with higher Pf have higher values of H_m0,deep^2 x T_m-1,0,deep.

We will mention this clearly in the text.

52. Figure 12: why is it dark red here?

The influence of the IG waves depends on several factors, as shown in Equation 12. The dark red area shown to the West in Figure 12 corresponds to two areas exposed to higher offshore waves (>2.4 m, while most of the other areas typically have waves <2 m) and very shallow water depths at the dike toe (>1.4 m, while most of the other areas have water depths > 3 m). This water depth is a function of the still water level and the (high) bed level at this location.

This discussed broadly from lines 441 to 463 but we discuss this specific area clearly in the revised manuscript.

53. Line 432: why is this, as \bar{eta} was the largest contributor?

The sensitivity factors only describe how sensitive the probabilistic calculation is to the uncertainty in the parameters. It does not describe the correlation between the mean values and the resulting failure probabilities. We note that the discussion on the alpha values distracts from the main focus of the paper; so we will move it to the Appendix.

54. Line 436: what are the parametric differences between saltmarshes and mudflats?

The characteristic differences are that: 1) saltmarshes are typically found in areas with less exposure (lower offshore wave heights and periods); and 2) saltmarshes typically have higher bed levels, which may translate to lower water depths at the dike toe, depending on the still water level.

55. Figure 13 caption: and what are the colors?

The colors represent the mudflats vs saltmarshes, as listed in the legend of the Figure.

56. Line 447: why not show this in a similarly styles plot as figure 14a?

We will add this as a third subplot in Figure 14.

57. Line 456: please go into details about some of the large and small values, and separate out batyy from forcing

Yes, we will describe the specific "hot spots" in more detail. See our response to the reviewer's earlier comment (#52) on Figure 12 b.

58. Line 461: relative, not in absolute sense

The reviewer is correct. We will modify the text as follows:

"areas with low water depths at the toe compared to the offshore wave heights are expected...".

59. Line 497: And it takes the boundary conditions at the toe, so the transformation is not included

The laboratory experiments used to derive the equations for shallow foreshores (Van Gent 1999) did include the transformation. The problem comes when models that do not correctly account for the transformation are used to obtain the boundary conditions at the toe.

60. Line 501: I would leave this part off. It is not essential for your takehome and message

We acknowledge the reviewer’s suggestion; however, the authors consider this discussion critical, since the choice of overtopping model significantly affects the calculated failure probability and explains the differences found here, compared to that of Oosterlo et. al (2018).

61. Line 541: I don't understand. H_IG should account for the bottom slope as through equation 12.

H_IG does indeed account for the bottom slope's influence on the height of IG waves compared to that of the SS waves. However, Tm-1,0, by definition, gives a disproportionate amount of weight to energy at lower frequencies and is very dependent on the spectral shape. Therefore, while the ratio of the wave heights may be the same for different foreshore slopes, the spectral shape varies (Figure 16) and so too does the value of Tm-1,0 (Table 4). This is also described in the Appendix.

In the revised manuscript, we will refer to the Appendix in-text here.

62. Lines 568-570: this needs more ink. was it case (as in site/forcing) specific or method specific or both? I would suggest to spend an entire subsection on the comparison with Oosterlo.

We agree with the reviewer. This comparison is actually done in Section 4.2; though, admittedly, without direct reference to Oosterlo's approach. In the revised manuscript, we will make it clearer that the differences between Oosterlo's findings and ours here are due to the method used, which is discussed in detail in Section 4.2.

63. Line 578: I would argue it from the other side. Given a crest level of 8 m, in both scenarios the dike does not fail. But with what increase sea level does it start to fail under either scenario? Remarking that HIG decreases relatively when the water depth increases. So don't look at a hypothetical crest height but the actual (higher) one.

As shown in Figure 10a, the influence of the IG waves on the failure probability actually increases for higher crest heights; since the larger forcing (increase in period and, to a lesser extent, the wave height) required to result in failure is achieved sooner when IG waves are considered. We will add text to Section 3.2.2 to make this clearer.

Please note that effects of sea-level rise were not considered within the scope of the current work.

64. Line 602: Make this is stronger statement. What has been found? A lot of readers only read the abstract and conclusions.

This paragraph serves as a brief summary of the work before stating the main conclusions (the following paragraphs).

65. Line 611: this is the conclusion

We agree with the reviewer.

66. Line 622: combined with this statement

We agree with the reviewer.

67. Line 634: these are not conclusions but recommendations

That is correct. We felt it necessary to end the conclusion by acknowledging the study's limitations and making recommendations or future work.