Reply to the Reviewers

Re: Manuscript ID Preprint egusphere-2025-4892
“Pan-European assessment of coastal flood hazards”
Camila Cotrim, Alexandra Toimil, Iñigo J. Losada, Sara Novo, and Iria Suárez
Natural Hazards and Earth System Sciences – NHESS

We would like to thank the Reviewers for their careful revision of our manuscript and for the thoughtful and constructive comments provided. We appreciate the time and effort dedicated to the review process, and we believe the suggestions have significantly improved the quality and clarity of the study. Below, we provide a list of main relevant changes made to the manuscript and detailed, point-by-point response to each comment, outlining the corresponding revisions made in the manuscript.

Relevant changes made to the manuscript

As suggested by both reviewers, the main changes made to the manuscript include:

- More emphasis on the results of the validation of the methodology.
- An extended description of the marine boundary conditions used in the coastal flooding simulations.

Additionally, two of the control cases have been updated with more recent reference flood maps.

Response to Dominik Paprotny

Review Report

The manuscript "Pan-European assessment of coastal flood hazard" introduces a new, high-resolution analysis of coastal flood hazard. The strength of the manuscript is an extensive sensitivity analysis and validation, which definitely increases the understanding of the limitations of coastal flood mapping. The work is comprehensive and solid, so my comments are only minor, in order of appearance:

Abstract: "The framework not only advances scientific understanding of large-scale coastal flooding but also provides actionable evidence to support the EU Floods Directive, adaptation planning, and climate risk management in the finance and insurance sectors." I suggest to remove this, as the paper shows still serious limitations of the approach (out of 12 cases, 5 miss most flooding and in 5 most of the flood zone is overestimation). The data is not available publicly, so it can't be easily reused. The actionability is reduced substantially by lack of future projections. The authors should mention the validation results, which are not mentioned here at all.

The last sentence has been removed and details have been included concerning the validation results. Lines 10 – 12: "The validation results confirmed the robustness of the large-scale methodology while highlighting the strong dependence of the results on the resolution and vertical accuracy of the underlying digital elevation model (DEM)."

L72, L484: Unfortunately, an error crept into Groenemeijer et al. (2016) report, as in reality a resampled version of the original DEM in 100 meter resolution was used to calculate the coastal inundation. This was not made explicit as it should have been (I was the author of the data and that part of the report).

We appreciate the reviewer's clarification and transparency. This information has been corrected in the revised manuscript as well as in Table S1.

L108: "each located at a relative depth of 0.1." – what does this mean?

Clarified. Lines 132 – 133: "CTPs were located at a relative depth of 0.1 (h/L; where h represents the water depth and L the corresponding wavelength of the wave peak period with an exceedance probability of 0.1)."

L135: Corine Land Cover does not cover Russia either, so what data was there?

We thank the reviewer for pointing this out. Indeed, Corine Land Cover was not used in this study. This information has been corrected in the manuscript. Lines 159 – 160: "In areas not covered by ODSE-LULC, such as Russia and western Turkey, the 100 m Global Land Cover dataset (Copernicus, 2019b) was used instead."

L153-158: this description is very vague, e.g. what historical storms were used, what was the calibration here, why only two events per year, what was the timespan of the historical data used or how the mean storm shape was derived.

The detailed description of the method adopted to design TWL hydrographs is presented in a separate study (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2025EF006545). Nevertheless, we have expanded the description included in the manuscript. Lines 175 – 191: "Following Cotrim et al. (2025), marine boundary conditions were represented by TWL hydrographs constructed for each CTP and composed of three elements: peak, duration, and shape.

Peak water levels were estimated through extreme value analysis (EVA), using an exponential model fitted to extreme events identified in the 37-year TWL hindcast. Events were selected using the peak-over-threshold (POT) method, with a spatially variable threshold yielding on average two events per year and requiring a minimum 72-hour interval between events. Sensitivity analyses on the POT threshold and distribution fits indicated the EVA method selected to provide the most appropriate balance of accuracy, robustness, and sample size. For more details on the TWL reconstruction method and the EVA, please refer to Cotrim et al. (2025). Hydrograph duration was derived from a storm duration function calibrated with historic storms (i.e., all events identified by POT) that combined a storm type (ST) classification with individual storm duration estimates. The storm classification system was based on hydrograph shape through Manhattan Dynamic Time Warping and reflected the dominant TWL drivers at peak conditions. Four storm types were identified (Table 1): ST A (surge-dominated), ST B (tide-dominated), ST C (mixed, tide-leaning), and ST D (mixed, surge-leaning). Individual storm duration estimates were then associated to peak TWL by adjusting the sample to a ST-specific function (shifted power function, exponential fit, or simple average) enabling the extrapolation of storm durations to return period events. Hydrograph shapes were based on the mean storm shape by averaging all events identified by POT, resulting in smooth-shaped and location-specific hydrographs. Hydrographs were constructed for each storm type at each CTP, as well as for a combined scenario weighted by the relative frequency of occurrence of each storm type."

L179: where did the Manning value come from and how were they linked in the specific land cover data used by the authors?

More information was provided in the manuscript and the Supplementary Material.

Lines 209 – 211: "Within each mesh, land cover classes were attributed to impact zones based on dominant land-use type, and Manning's roughness coefficients were assigned accordingly, following van der Sande et al. (2003) (see Table S2)."

Table S2 Description and correspondence of land use classes from ODSE-LULC (Witjes et al., 2022) and Global Land Cover dataset (Copernicus, 2019b)**, accompanied by the respective Manning roughness coefficients, following (van der Sande et al., 2003).**

Group	Description	ODSE-LULC	Global Land Cover	Manning roughness coefficient
1	Urban areas	1	50	0.150
2	Other urban areas	2 – 8	–	0.200
3	Rural areas	9 – 15	20 / 40	0.127
4	Natural vegetation	16 – 21	111 – 126	0.100
5	Bare areas (beaches, sand), dunes)	22 – 26	30 / 60 / 70 / 100	0.120
6	Waterbodies	27 – 33	80 / 90 / 200	0.050

Table 2: some better naming scheme of the case studies needs to be applied, as it makes reading the text and analysing the graphs rather difficult.

Case studies have been renamed and updated across the entire section 4 of the manuscript including Figures 3 and 5 and Tables 2 and 3. The new nomenclature was also included in Figure 2 to facilitate the understanding of the study.

The updated nomenclature is as follows:

Previous nomenclature	Updated nomenclature
CFCC02	UK01
CFCC03	DE02
CFCC04	DE03
CFCC05	PT04
CFCC08	FR05
CFCC09	ES06

CFCC10	ES07
CFCC11	IT08
CFCC12	IT09
CFCC13	ES10
CFCC14	DE11
CFCC15	PT12

L214: one additional important source of underestimation is the issue that higher resolution of DEM enables capturing some coastal defences, but still does not allow for defence failure mechanisms other than overtopping. In past coastal floods, dikes have failed without water levels reaching their crests, which is very difficult to represent in flood models (https://www.hkv.nl/wp-content/uploads/2020/07/Applications_of_VNK2_a_fully_probabilistic_risk_analyses_BM.pdf)

This limitation has been included in the manuscript. Lines 265 – 267: "However, even if coastal defenses were included in the DEM, their representation in a flood model presents its own challenges as defense failure mechanisms could in fact result in a larger flooded area than its neglect in the input data."

L437: datasets such as COASTPRO-EU rely on nominal or official protections levels, which are often much lower in practice due to e.g. inadequate maintenance or change in extreme water level probability (https://doi.org/10.1007/s11069-024-07039-5). This causes very large sensitivity of flood maps to protection level assumptions overshadowing all other (https://doi.org/10.1007/s11069-024-07039-5).

We agree that datasets such as COASTPRO-EU typically report nominal/official standards of protection, which may substantially differ from the effective level of protection. This discrepancy can lead to systematic bias when translating protection standards into flood extents. We also agree that assumed protection levels can be a dominant driver of uncertainty when producing flood maps. This challenge has been included in the limitations section of the Discussion. Lines 565 – 570: "However, available coastal protection databases remain coarse, outdated, and inconsistent with the resolution of flood maps, as they provide information only at the NUTS2 level. Such datasets (e.g., COASTPRO-EU) typically reflect nominal or official standards of protection, which can be substantially lower in practice due to inadequate maintenance, deterioration, or changes in the probability of extreme water levels (Paprotny et al., 2025). As a result, flood extents can be highly sensitive to protection-level assumptions and in some cases dominate the overall uncertainty in large-scale flood mapping.”

Table 6: it should be highlighted that the MFA results are not fully comparable across studies. The data in Paprotny et al. (2018) and Groenemeijer et al. (2016) are pretty much the same, but the figure from the former refers to a smaller number of countries than the latter.

The reviewer makes a good observation. We agree that MFA figures are not strictly comparable across studies because the underlying spatial coverage and country sets differ. We have therefore strengthened the disclaimer below Table 6 to make this explicit. Lines 613 – 615: “Note. MFA results should be interpreted with caution when compared across studies, as differences in spatial domain and underlying data coverage can affect the reported values; comparisons are therefore indicative rather than directly comparable.”

L534-536: same remark as regarding the abstract.

Sentence removed.

Response to Reviewer 2

Review Report

General comment

The manuscript ‘Pan-European assessment of coastal flood hazards’ presents a European scale assessment of coastal flooding featuring state-of-the-art methods and, to my knowledge, for the first time a comprehensive cross validation of flood extents for a multitude of different regions in Europe. I would like to congratulate the authors for submitting such a detailed, rich, and comprehensive study. In my opinion, the manuscript is of overall good scientific quality and significance and my comments are therefore mostly of minor nature. My main comments are about methodological clarifications (vertical datum, marine boundary conditions) and a clearer communication of when dikes were included in the simulations and when not. In addition, I suggest to enrich the discussion by emphasizing current weaknesses in broad-scale coastal flooding assessments (including coastal morphodynamics, compound flooding) beyond the ones analysed by the authors and previous studies, and outlining ways forward in this scientific field.

Please find my detailed comments below.

Specific comments

Abstract

1. L13-14 (hydrograph variability and storm type variability affect floods differently):

This is a nice and new finding on such a large spatial scale.

We thank the reviewer for this positive comment and for recognizing the novelty and large-scale relevance of this finding.

2. You should stress more the effort you made in validating your European wide assessment for so many cases across the continent. In my opinion, this is a crucial step forward of your work!

More information has been included concerning the validation results. Lines 10 – 12: “The validation results confirmed the robustness of the large-scale methodology while highlighting the strong dependence of the results on the resolution and vertical accuracy of the underlying digital elevation model (DEM).”

Introduction

3. L36-37 ('local thresholds'):

Please specify 'local thresholds'. In fact, you mean coastal protection infrastructure and/or natural flood barriers like beach berms, dunes and cliffs, right?

Specified. Lines 36 – 38: "Flooding occurs when extreme TWL exceeds local thresholds such as natural topographic barriers or coastal protection infrastructures (van de Wal et al., 2024), inundating low-lying coastal areas typically represented by digital elevation models (DEMs)."

4. L54 ('wave setup'):

With a broader readership in mind, I suggest you very briefly explain the term and also differentiate it from wave run up. It would help the reader if you could further explain why not the latter was used in your assessment, as also wave run up can contribute to flooding. This would also help the reader further down in the manuscript, for instance L112, where you explain the marine forcing.

The wave contribution description has been extended. Lines 55 – 69: "Yet, large-scale studies often omit or oversimplify some of these components, particularly regarding the wave contribution which is frequently neglected (Muis et al., 2016; Paprotny et al., 2018). When included, the wave contribution to TWL is commonly represented as wave setup, i.e., a quasi-steady (time-averaged) elevation of the mean coastal water surface at the shoreline induced by wave breaking. Wave setup should be distinguished from wave runup, which describes the oscillatory vertical excursion of the shoreline driven by swash motions and typically includes both infragravity and incident components (Stockdon et al., 2006). In this study, we represent the wave contribution via setup rather than runup because the large-scale estimation of infragravity swash remains highly uncertain and would introduce poorly constrained spatial variability. In addition, the incident swash component primarily manifests as intermittent uprush ("splashing") at the shoreline; while it can contribute to localized overtopping, it typically does not translate into sustained water-level elevations or flood volumes that generate persistent inundation footprints and damages at the spatial and temporal scales targeted here."

5. L73-74 (DEM's and resolutions):

As it is indeed crucial background information to your assessment, I suggest you add some info on currently highest resolution global DEMS, featuring fabdem, deltadtm (for coasts), and the one you are using in comparison. They are summarised in resolution and vertical accuracy here: https://www.nature.com/articles/s41597-024-03091-9

Information added. Lines 83 – 85: "As remote sensing and lidar-derived topography data continue to improve, high-resolution DEMs such as EU-DEM (Copernicus, 2019a), FABDEM (Hawker et al., 2022), and DeltaDTM (Pronk et al., 2024) allow for more accurate flood maps to be produced."

6. L78-80 (marine forcing data):

Here I think the Global Tide and Surge Model should be featured, as it provides global tide, surge and total water levels at resolutions even higher than 2.5 km (1.25 km for Europe) (https://www.frontiersin.org/journals/marine-science/articles/10.3389/fmars.2020.00263/full).

The study mentioned by the reviewer has been included. Lines 90 – 92: "When neglecting the wave contribution, these inputs are available at 2.5 km resolution globally and 1.25 km in Europe (Muis et al., 2020). When including the wave contribution, studies have achieved ~70 km resolution globally (Kirezci et al., 2020) and at continental-scale up to 2.5 km resolution (Le Gal et al., 2023)."

7. L85 (model validation):

You should specify here that you are speaking about validating both coastal water levels used to force the hydrodynamic model (when they are taken from other models like GTSM), and the validation of the flood extent. The latter is much more difficult and often only applied on local scales, even if the study simulates floods on much broader scales. The flood extent is crucial because that's the output used to calculate risk. Therefore a more comprehensive flood extent validation is needed, which is exactly what you nicely do in your paper. Therefore I believe it could be more pronounced to demonstrate that your paper is a real advancement.

Specified. Lines 97 – 104: "While the validation of boundary conditions has been widely explored (e.g., Dalinghaus et al., 2025; Kirezci et al., 2020; Treu et al., 2024; Vousdoukas et al., 2018), the validation of the flood extent remains a challenge at the large scale due to the scarcity of observational records. This limitation is critical because flood extent is the primary output used to quantify exposure and damages, and thus underpins risk estimates. Comprehensive inundation-footprint validation is therefore essential, yet it is still rarely undertaken beyond local case studies. Here, we address this gap by validating reconstructed flood extents against multiple historical events across diverse coastlines, thereby strengthening confidence in the risk-relevant outputs. In addition to the validation, it is equally important to understand the reasons behind its results as they can indicate potential sources of uncertainty or opportunities for methodological improvement."

8. L87-89 (systematic large-scale validation):

However, I believe it is worth while mentioning and acknowledging the following effort in the validation of sfincs for river floods on a global scale in a systematic manner. Link to preprint: https://egusphere.copernicus.org/preprints/2025/egusphere-2025-4387/

Mention included. Lines 107 – 111: "Although systematic global-scale flood-extent validation has recently been demonstrated for riverine flooding (Sadana et al., 2025), to the best of our knowledge comparable large-scale coastal flood methodologies have rarely been validated in a systematic manner against high-resolution, local-scale inundation observations across multiple historical events and diverse coastal settings, including the analyses needed to benchmark alternative input datasets and methodological choices (Supplementary Table S1)."

9. L98-99 (study aims):

You could stress even more the novelty of your validation, as the overall approach (although admittedly with lower resolution input data) has been applied in variations elsewhere (e.g. Vousdoukas et al., 2016).

The novelty of the study has been highlighted. Lines 119 – 122: "Although similar large-scale coastal flood modeling approaches have been presented and validated for Europe (e.g., Le Gal et al., 2023; Vousdoukas et al., 2016), though with lower resolutions and different input data, the present study is supported by a series of sensitivity analyses used to quantify the influence of key assumptions within the modeling chain."

Methods

10. L122 (coastal slope dataset for intertidal zones):

Your study area also includes coastlines with no to very little intertidal zones. How did you deal with that? Also, there is a much more recent dataset of coastal slopes across the globe (https://essd.copernicus.org/articles/16/3433/2024/). As coastlines are morphologically highly dynamic environments, I consider the 1984 dataset outdated. Please consider using the new coastal slope dataset or elaborate a bit further why the 1984 one was used instead.

Indeed, the study area includes a variety of coastlines and the application of the Sunamura (1984) approximation in this study did not differentiate limited or absent intertidal zones as the methodology was applied consistently across the entire study area. Yet, the Sunamura (1984) approximation has been successfully validated in this study as presented in section 4.3.3. The adoption of a foreshore slope parametrization instead of observational data represents an important limitation of the methodology and has been included in the limitations of the study. Lines 574 – 575: "For example, the foreshore slope approximation used does not differentiate limited or absent intertidal zones".

Concerning the dataset mentioned by the reviewer, unfortunately, the present study was initiated and largely completed prior to the public availability of Athanasiou et al. (2024). Replacing the slope data at this stage would require a full reapplication of the methodology and a repetition of its validation. Considering the limited influence the slope has on the resulting TWL and flood extent, such a repetition of the modeling chain is not justifiable.

Nevertheless, a comparison between the Sunamura (1984) approximation, the observed slopes extracted from DEM profiles with the traditional method, and the data provided by Athanasiou et al. (2024) has been performed. Figure X1 presents the results for each control case. This comparison indicates that both Sunamura (1984) and Athanasiou et al. (2024) show comparable performances with neither providing perfect matches to the observations across all cases and each performing well under certain conditions.

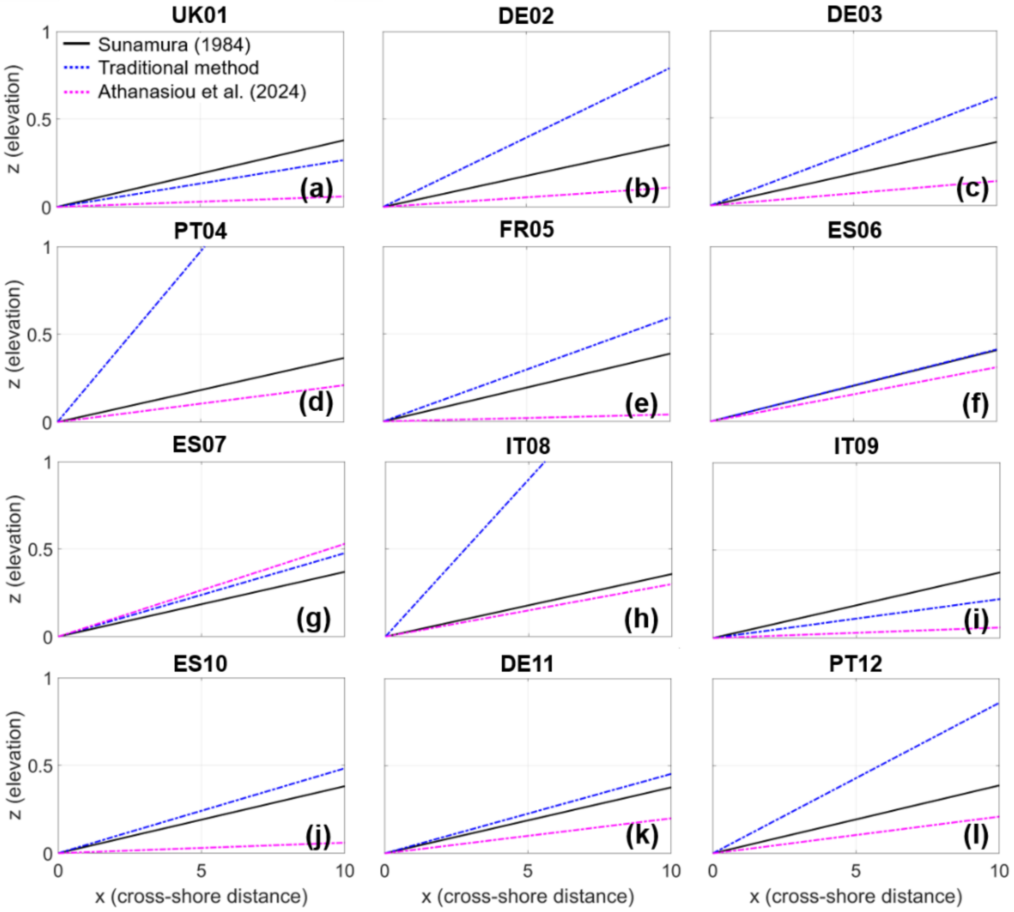

Figure X1. Comparison of mean foreshore slopes resulting from the Sunamura (1984) approach, the traditional method, and the global coastal slope dataset by Athanasiou et al. (2024) per control case (a – l).

11. L133-134 (land cover dataset):

Not everyone is familiar with this dataset. Can you give examples of the most relevant classes in that data for flood modelling? Are urban/developed areas included as well as forests (broad leafed, needle leafed), and (coastal) wetlands?

Firstly, the information regarding the land use data used for parts of the study area has been corrected in the manuscript. Lines 159 – 160: “In areas not covered by ODSE-LULC, such as Russia and western Turkey, the 100 m Global Land Cover dataset (Copernicus, 2019b) was used instead.”

Secondly, more information was provided in the Supplementary Material concerning the land use classes considered.

Table S2 Description and correspondence of land use classes from ODSE-LULC (Witjes et al., 2022) and Global Land Cover dataset (Copernicus, 2019b), accompanied by the respective Manning roughness coefficients, following (van der Sande et al., 2003).

Group	Description	ODSE-LULC	Global Land Cover	Manning roughness coefficient
1	Urban areas	1	50	0.150
2	Other urban areas	2 – 8	–	0.200
3	Rural areas	9 – 15	20 / 40	0.127
4	Natural vegetation	16 – 21	111 – 126	0.100
5	Bare areas (beaches, sand), dunes)	22 – 26	30 / 60 / 70 / 100	0.120
6	Waterbodies	27 – 33	80 / 90 / 200	0.050

12. Section 3.1 (marine boundary conditions):

This section could be better explained and justified using existing literature. For instance, the choice of the threshold used, the distribution applied (GEV/POT) and other aspects such as the number of events used to fit the distribution have important implications on the extrapolated extreme sea levels.

I think you should also mention whether and how the marine boundary conditions were validated. I assume this was done in previous studies, but it would help the reader interpreting the flood maps, if information on the accuracy of the total water levels would be presented.

The marine boundary condition inputs used in this study are provided by Cotrim et al. (2025), which include a description of the decisions taken in the extreme value analysis in its Supplementary Material. In addition to describing the hydrographs in more details in section 3.1, we also included a mention to the source of the marine boundary conditions in the manuscript. Lines 181 – 182: “For more details on the TWL reconstruction method and the EVA, please refer to Cotrim et al. (2025).”

13. L159-160 (hydrograph generation):

Can you please specify a bit further on how hydrographs were created for those return events? Did you take the average surge shape, combined it with peak or average tidal signals? I assume they come from here (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2025EF006545), but how and if this method was applied needs further explanation.

Yes, the detailed description of the method adopted to design TWL hydrographs is presented in a separate study (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2025EF006545). Nevertheless, we have expanded the description included in the manuscript. Lines 175 – 191: “Following Cotrim et al. (2025), marine boundary conditions were represented by TWL hydrographs constructed for each CTP and composed of three elements: peak, duration, and shape.

Peak water levels were estimated through extreme value analysis (EVA), using an exponential model fitted to extreme events identified in the 37-year TWL hindcast. Events were selected using the peak-over-threshold (POT) method, with a spatially variable threshold yielding on average two events per year and requiring a minimum 72-hour interval between events. Sensitivity analyses on the POT threshold and distribution fits indicated the EVA method selected to provide the most appropriate balance of accuracy, robustness, and sample size. For more details on the TWL reconstruction method and the EVA, please refer to Cotrim et al. (2025). Hydrograph duration was derived from a storm duration function calibrated with historic storms (i.e., all events identified by POT) that combined a storm type (ST) classification with individual storm duration estimates. The storm classification system was based on hydrograph shape through Manhattan Dynamic Time Warping and reflected the dominant TWL drivers at peak conditions. Four storm types were identified (Table 1): ST A (surge-dominated), ST B (tide-dominated), ST C (mixed, tide-leaning), and ST D (mixed, surge-leaning). Individual storm duration estimates were then associated to peak TWL by adjusting the sample to a ST-specific function (shifted power function, exponential fit, or simple average) enabling the extrapolation of storm durations to return period events. Hydrograph shapes were based on the mean storm shape by averaging all events identified by POT, resulting in smooth-shaped and location-specific hydrographs. Hydrographs were constructed for each storm type at each CTP, as well as for a combined scenario weighted by the relative frequency of occurrence of each storm type.”

14. L221-222 (difference in flood extent with cases Schlei fjord and Wismar attributed to wave set up):

True, but the case of Wismar shows that waves are likely a subordinate cause of the difference you observe. Wismar is sheltered and thus not wave exposed. Wave setup alone can therefore not explain the difference. The same is true for areas inside the Schlei fjord (check also https://doi.org/10.1017/cft.2024.11). Here and in Wismar, it is most likely the dikeline that is not sufficiently well resolved in your model. Check https://www.nature.com/articles/s43247-

023-01100-0 for a map of the German Baltic dikeline (Figure 4). I think this should be made clearer here.

Clarified. Lines 260 – 267: "Differences between our results and these references can be explained by key input assumptions. On the one hand, DE02 and DE03 did not include wave setup in their reference maps, whereas our study did. Given the low exposure of both sites to wave action, wave setup is likely not the sole reason for such result. On the other hand, all three reference maps accounted for existing coastal defenses, leading to a smaller observed extent. Overestimation results in UK01, DE02, DE03, and DE11 are similarly explained by the absence of coastal defenses in our model (Kiesel et al., 2023; Kiesel et al., 2024; Koks et al., 2023; Wadey et al., 2012). However, even if coastal defenses were included in the DEM their representation in a flood model presents its own challenges as defense failure mechanisms could in fact result in a larger flooded area than its neglect in the input data."

15. Table 3:

I would also refer to it in section 4.2, as you discuss the quantitative validation results here. It also links to Figure 3, right?

Yes, a mention to Table 3 was added to section 4.2. Lines 244 – 245: "**Error! Reference source not found.** presents the CSI for each case with higher values indicating better agreement between the simulations and the observations."

16. Up from Section 4.2:

I suggest to move those to the results section, as the whole validation and sensitivity study is in my opinion a major outcome of your work and you are in fact describing results.

The reviewer suggests an interesting reorganization of the manuscript, which we initially considered through a similar structure. However, it resulted in an unbalanced manuscript, with such an extensive section of Results. Additionally, given the novelty and relevance of the validation and the associated sensitivity analyses, we believed it deserved its own section focused entirely on the control cases.

Nevertheless, following the comments by the reviewer, we have emphasized the validation exercise throughout the manuscript as shown below.

Abstract. Lines 10 – 12: “The validation results confirmed the robustness of the large-scale methodology while highlighting the strong dependence of the results on the resolution and vertical accuracy of the underlying digital elevation model (DEM).”

Introduction. Lines 119 – 122: “Although similar large-scale coastal flood modeling approaches have been presented and validated for Europe (e.g., Le Gal et al., 2023; Vousdoukas et al., 2016), though with lower resolutions and different input data, the present study is supported by a series of sensitivity analyses used to quantify the influence of key assumptions within the modeling chain.”

Discussion and Conclusions. Lines 514 – 516: “The methodology was validated through the reconstruction of 12 historical flood events using local-scale control cases. Although the validation exercise did not produce equally strong results across all cases, it allowed us to explain the observed performance, identify key sources of uncertainty, and highlight methodological steps with potential for improvement.”

17. L292-294 (sentence clarification):

This sentence needs rephrasing or clarification. I would not agree that Norway is ‘flat’, as it mostly lacks large floodplains in contrast to the Bay of Biscay, The Netherlands, and Belgium, which are further characterised by meso-tidal regimes.

Rephrased. Lines 334 – 337: “The highest confidence values are observed in tide-dominated areas with relatively simple coastal configurations, such as the Bay of Biscay, and sections of the Norwegian coast. The lowest values appear in the North Sea, including parts of the UK, Belgium, and the Netherlands, as well as in the southeastern Mediterranean, where more complex hydrodynamic regimes and irregular coastlines dominate.”

Results

18. L314-315 (hydrographs):

This is where more information on the hydrograph creation would be useful. You should remind the reader what 'smooth' means and add more detail in the methods section what ‘mean storm shape’ refers to. Is it the mean surge shape, but how was it combined with the tidal signal then? Or is it the average total water level hydrograph of a given storm surge event?

More information on the hydrograph shape definition was included in section 3.1. Lines 188 – 189: “Hydrograph shapes were based on the mean storm shape by averaging all events identified by POT, resulting in smooth-shaped and location-specific hydrographs.”

A reminder of what “smooth” means has been included in the section mentioned by the reviewer. Lines 356 – 358: “Figure 7 shows the MFA resulting from smooth-shaped hydrographs, based on the location-specific average TWL storm shape, applied to the dynamic flood model RFSM-EDA for a 100-year TWL combined storm type event, without considering coastal defenses.”

19. L318-319 (dikes?):

But the southern North Sea is highly protected by dikes. I could not follow whether this is or is not considered in the results you present here. If not, it should be clarified.

Clarified. Lines 357 – 359: “Figure 7 shows the MFA resulting from smooth-shaped hydrographs, based on the location-specific average TWL storm shape, applied to the dynamic flood model RFSM-EDA for a 100-year TWL combined storm type event, without considering coastal defenses.”

20. L444-449 (comparison with Vousdoukas et al., 2016):

It could also have to do with how Vousdoukas et al., 2016 have incorporated the coastal protection standards, as you have used a very recent database (van Maanen), which in 2016 was not yet available. Can you elaborate on that as well?

Elaborated. Lines 491 – 496: “Besides the use of a more recent and updated dataset of coastal protection standards (i.e., COASTPRO-EU vs FLOPROS of Scussolini et al., 2016), these differences likely reflect both the higher resolution of our analysis (25 m vs 90 m flood map resolution) and our characterization of wave setup. While Vousdoukas et al. (2016) applied a uniform wave setup of 0.2Hs with offshore wave conditions, we used a semi-empirical formulation based on nearshore wave conditions and spatially varying foreshore slopes, which provides more detailed boundary conditions.”

Discussion/conclusion

21. L483 (‘flood map generation’):

maybe better: affect the results of hydrodynamic flood simulations.

Replaced. Lines 532 – 533: “Third, DEM resolution affects the results of hydrodynamic flood simulations, but its influence is not straightforward.”

22. L488-489 (drivers of uncertainty):

I would also think you should mention the incorporation of dikes here, as its really important, which you also show.

As the incorporation of coastal defenses does not compose the proposed methodology, it was not included as a driver in the uncertainty quantification analysis, which focuses solely on components of the methodology itself. However, its influence on flood extent was indeed assessed in a sensitivity analysis allowing its attribution to flood map uncertainty to be estimated. This information has now been included in the paragraph focused on coastal defenses. Lines 562 – 565: “Flood extent varies between 9% and 22.9% of the European floodplain depending on the assumed level of coastal protection, with the inclusion of defenses reducing flood extent by 40 – 61% relative to the undefended case and corresponding to an uncertainty range of 13.9% attributed solely to the treatment of coastal defenses.”

23. L508-509 (vertical datum):

Was the vertical datum known and, if needed, corrected for the baseline simulations here (25 m EU DEM)? This can be a crucial source of error and should be clearly communicated in the manuscript.

Throughout this study vertical datum corrections were applied to the hydrographs prior to the application of the flood model. This was the case for both the continental and the local-scale applications, as well as the different DEM resolutions and flood models.

The information concerning the continental-scale methodology has been clarified. Lines 192 – 194: “Because hydrographs were referenced to local mean sea level, while the EU-DEM elevations are referenced to the geoid, a vertical correction was applied using the AVISO database, which provides differences between both vertical datums from satellite altimetry (NCAR, 2022).”

The information concerning the control cases was also clarified. Lines 228 – 231: “As the source of the high-resolution DEM data used in the control cases varied so did the vertical datum. In cases where the datum information was explicit (e.g., Germany, Portugal, Spain, and France), the appropriate corrections were applied in each case. Otherwise, the geoid was considered as the default reference in order for a vertical correction to be applied."

24.

The discussion could benefit from adding a paragraph on ways forward in regional to global scale studies on coastal flooding. The general parameters of uncertainty like dikes, DEM, total water levels, and modelling approach have been nicely discussed and are furthermore content of previous assessments, which you also cite. However, your paper can also discuss and mention the factors not yet sufficiently well represented in comparative studies, such as morphodynamic responses of the shoreline to extreme sea levels like dune, berm and dike breaches. Also, your study area covers major estuaries, yielding potential for compound floods. The potential effect of neglecting these important processes and reasons for their current underrepresentation as well as ways forward could nicely be featured as well and would give the reader more context of the presented work.

A paragraph on ways forwards has been included in the Discussion. Lines 581 – 598: "Beyond the previously discussed sources of uncertainty and considering the limitations identified in the study, future research directions are identified for large-scale coastal flood assessment methodologies. First, even though this study treats the entire study area in a systematic way focused on overwashing, the processes that influence coastal flooding vary. For example, dune erosion, berm overtopping, or dike breaching can significantly alter the flood propagation and extent (Nicholls et al., 2015). Therefore, further efforts are needed to advance the understanding of morphodynamic responses of the shoreline to extreme sea levels, particularly beach responses such as sediment accretion or erosion (Toimil et al., 2023). Second, the widespread presence of estuaries and rivers, typical of large-scale studies, emphasizes the potential for compound coastal flooding due to the confluence of coastal flooding with pluvial and riverine flooding as a result of extreme precipitation and/or river discharge (Eilander et al., 2023; Wing et al., 2024). Such process poses an elevated risk to coastal communities due to the high concentration of people and assets in these areas. Third, an improved representation of coastal defenses accompanied by their spatial variability, structural properties, and failure mechanisms is needed to better capture uncertainty in flood projections. The limited inclusion of morphodynamic responses, compound flooding, and coastal defenses in continental-scale studies reflects the challenge of representing local-scale processes within broad-scale frameworks besides the scarcity of homogeneous data and computational constraints. Even though conterminous frameworks are limited, recent research have focused on proposing scalable frameworks which deal with the varied complexity of marine dynamics as well as coastal morphology (e.g., Benito et al., 2025). Lastly, as research advances and computational capabilities continue to expand, the availability, quality, and resolution of relevant datasets also improve, highlighting the importance of revisiting methodologies such as the one proposed here and enabling more accurate assessments."

Technical corrections

1. L145/Figure 1 (wording):

… coastal flood modelling with *these* three steps:...

Figure 1 has been modified.

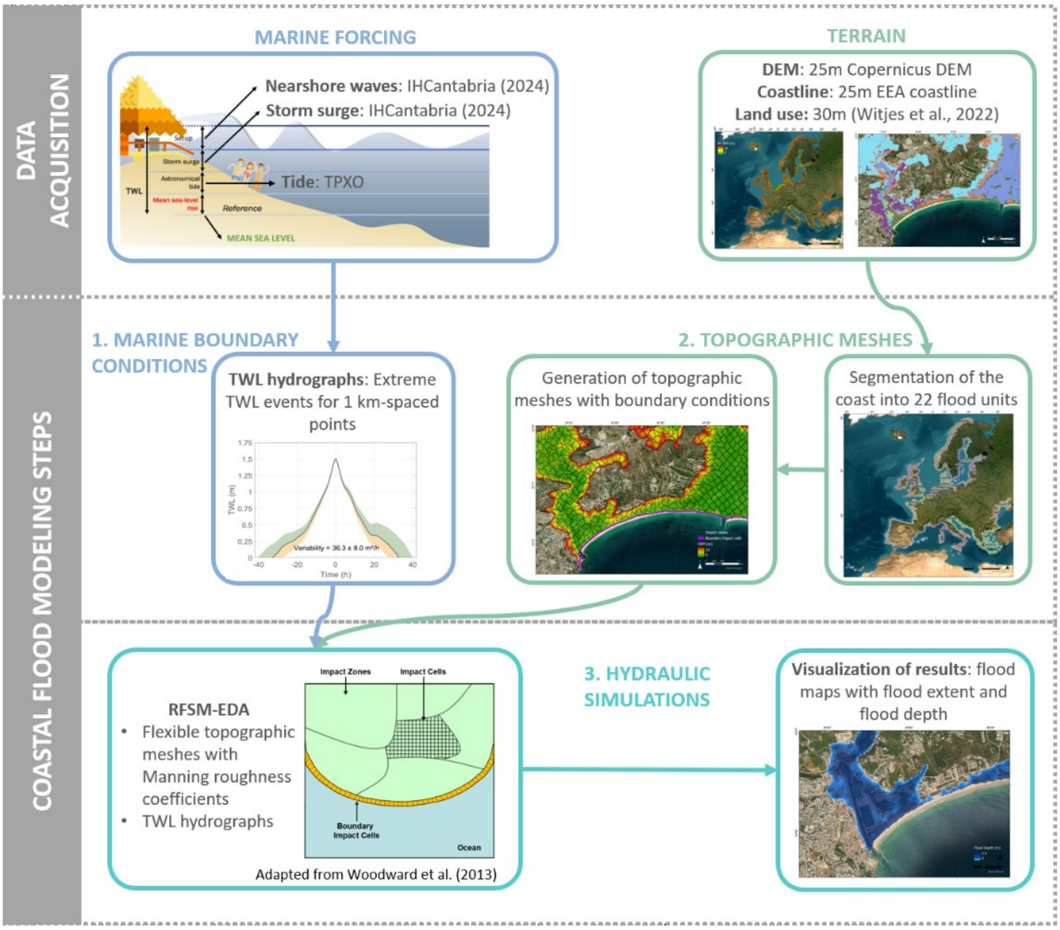

Figure 1. Continental-scale coastal flood modeling methodology applied. The methodology is divided into data acquisition and coastal flooding modeling with tis three steps: (1) preparation of marine boundary conditions; (2) generation of topographic meshes; and (3) hydraulic simulations. Including adapted diagram by Woodward et al. (2013).

2. Table 2 (wording):

'*Schlei* fjord': if additional validation 'test power' is needed: this study (DOI: https://doi.org/10.1017/cft.2024.11) has run Delft3d-Flow in the Schlei fjord for the 2019 event and provided a flood extent validation in Figure S2 of the Supplementary Material.

Corrected. We appreciate the reviewer bringing to our attention a more recent study on the 2019 flood event. This additional validation 'test power' provided (Kiesel et al., 2024) has been tested as an alternative for the validation of DE03 (Schlei Fjord, DE), previously performed with a flood map provided by Kiesel et al. (2023). The critical success index (CSI) of the four cases tested (RFSM low and high-resolution and SFINCS low and high-resolution) have improved indicating a better agreement between the simulation performed here and the published reference flood maps, further supporting the validation exercise presented in the manuscript.

An update has been carried out with ES10 as well (Ebro Delta, ES). The comparison of the simulations performed here with the recently published flood map by Caballero et al. (2024) showed better agreement than with the previously reference flood map used (Copernicus, 2025). The improved result may be related to differences in satellite source and date: the flood map by Copernicus (2025) was provided by Copernicus Emergency Service using SPOT satellite from 26th of January 2020, whereas Caballero et al. (2024) used Sentinel-2 imagery from the 23rd of January 2020, a date closer to the actual flood event.

Therefore, the results for DE03 and ES10 have been updated in Figure 3 as well as the corresponding information and results in Tables 2 and 3 and associated descriptions. Figure 6 has also been updated although with minor changes. Overall, these updates do not alter the interpretation of the results, except for the validation CSI themselves. The relative comparisons between flood models, DEM resolutions, and degrees of confidence index improvement remain unchanged.

Table 3. Critical success index (CSI) comparing the simulated results in the present study against observations. Results are shown for both DEM resolutions and flood models applied in each local-scale control case.

DEM resolution	Low resolution		High-resolution	
Flood model	RFSM-EDA	SFINCS	RFSM-EDA	SFINCS
UK01	0.41	0.41	0.28	0.26
DE02	0.20	0.19	0.43	0.41
DE03	0.22	0.18	0.48	0.52
PT04	0.01	0.01	0.01	0.01

FR05	0.59	0.60	0.10	0.07
ES06	0.44	0.29	0.44	0.42
ES07	0.13	0.14	0.19	0.23
IT08	0.00	0.00	0.24	0.24
IT09	0.43	0.43	0.42	0.43
ES10	0.35	0.34	0.004	0.01
DE11	0.03	0.03	0.07	0.16
PT12	0.00	0.00	0.49	0.43

3. Table 2 (wording):

‘Elbe Estuary’

Corrected.

4. L274 (wording):

Figure 5~~m~~

Corrected.

5. L338 (reminder):

This is a good spot to remind the reader that the ‘floodplain’ refers to all areas below 15 m hydrologically connected to the sea. 37/23% of it flooded is substantial. I assume that in this assessment, dikes are not taken into account, right? I think this should be clarified.

A reminder of the floodplain definition used has been included. Lines 382 – 384: “For the European coastline as a whole, 36.8% of the floodplain (i.e., area below 15 m of elevation and hydrologically connected to the sea) is inundated with the static model, whereas the dynamic model RFSM-EDA, driven by smooth hydrographs, reduces the relative MFA to 22.9%.”

Yes, the inclusion of coastal defenses is not part of the proposed methodology. Therefore, coastal defenses (including dikes) are not included in this analysis and are only considered in sections 4.4 (confidence index) and 5.5 (sensitivity analysis of the influence of coastal defenses).

A disclaimer has been included in the methodology. Lines 220 – 222: “The representation of coastal defenses lies outside of the scope of the proposed methodology. However, given their role as an important uncertainty source in flood modeling, their influence is evaluated separately in a dedicated sensitivity analysis.”

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