Response to all reviewers
Climate and impact attribution of compound flooding induced by tropical cyclone Idai in Mozambique

We thank all three anonymous reviewers for their elaborate comments and suggestions on how to improve the manuscript. We address each point raised by the reviewers below (in blue). Line numbers refer to the revised documents (manuscript or supplement) with tracked changes.

Given the alignment of the comments of all three reviewers, we would like to clarify the scope of the paper, and justify the validity of our approach. The aim of the paper is to showcase the applicability of a storyline attribution framework for tropical cyclones using global data only, thereby exploring an approach that could be implemented operationally, particularly in data-scarce contexts such as Mozambique. In this novel storyline attribution framework, we make significant steps for climate and impact attribution of compound flooding from tropical cyclones. We have clarified in the revised version of the manuscript that this framework provides a *conditional* attribution assessment of the observed event under a range of *plausible* climate trends. This framework focuses on changes in TC flood drivers for which there is robust scientific evidence.

For the attribution experiments within our paper, we utilize well established storyline attribution principles (Lloyd and Shepherd, 2021; Shepherd, 2016; Sillmann et al., 2021). For event-based attribution of tropical cyclones (TCs), assumptions and benchmark choices are necessary. For instance, keeping the original TC track reduces the number of degrees of freedom in choosing counterfactual scenarios necessary for practical application of the attribution approach, which is a typical example of a conditional attribution assessment (Mester et al., 2023; Strauss et al., 2021). Constructing counterfactuals per definition violates consistencies as it puts an event into a different environment, which implies assuming an infinite number of unchanged impacts that could have unfolded differently in the real world. A conditional attribution experiment should be considered a thought experiment, not a physical reconstruction where all physical forces and budgets are closed. Consequently, counterfactual scenarios are designed to study plausible what-if scenarios of the effect of climate change on the event (Lloyd and Shepherd, 2020). Such what-if scenarios are especially relevant for low-likelihood high-impact events, such as tropical cyclones (Sillmann et al., 2021). Inherently, our experimental setup relies on assumptions, which are kept constant between scenarios. We argue that some of the sensitivity experiments requested by the reviewers go beyond the essential reasoning of the intent of a storyline attribution experiment. Including uncertainties in all modelling parameters will blow-up uncertainty levels that will bury useful information, especially for data-scarce regions with many unknowns (where some uncertainty might not even be quantifiable), as is the case for Mozambique. We rely on the best available global datasets to allow global applicability of our framework, also in data-scarce environments, and use state-of-the-art or conservative estimates for model parameter uncertainty.

We agree with the reviewers' suggestions to include uncertainty quantification of the effect of climate change on the flood drivers and have therefore provided our results as a range of plausible values for the conditional attribution statement. We have included additional counterfactual scenarios with plausible values for the climate trend in the flood drivers of tropical cyclone Idai (see Table 1 and updated section 2.3.2 in manuscript) based on best available literature. We now present the results including a low, medium, and high plausible climate trends for different driver combinations (22 combinations; see Table below):

- **Low, medium and high counterfactual values for TC rainfall (-4%, -8% and -16%)**, following Clausius Clapeyron for the medium counterfactual scenario (-8%; Knutson et al., 2020), the super-Clausius Clapeyron principle for the high counterfactual scenario (-16%; Guzman and Jiang, 2021; Liu et al., 2019; Patricola and Wehner, 2018) and a plausible TC rainfall reduction by negative ocean-atmosphere feedback for the low counterfactual scenario (-4%; Tu et al., 2022).

- **Low, medium and high counterfactual values for TC wind speed (-1%, -5%, and -10%)**, according to the findings from Knutson et al. (2020) for the low and medium counterfactual scenarios (-1%, -5%), as suggested by reviewer 1. The high counterfactual scenario (-10%) is based on the TC wind speed trend from IBTrACS for the southern Indian Ocean from Mester et al. (2023).
- **Low, medium and high counterfactual values for SLR (-0.05 m, -0.10 m, and -0.15 m)**. Our best-estimate SLR is based on Treu et al. (2024) and is the mean of all stations along the coast of Mozambique, within our D-Flow FM domain, resulting in a plausible -0.10 m value for the medium counterfactual scenario. We add a plausible low and high counterfactual SLR scenario of ±0.05 m based on Strauss et al. (2021), resulting in -0.05 m and -0.15 m. For our best-estimate SLR analysis, we have switched from the geocentric water level to the water level dataset of Treu et al., which includes vertical land movement, as it is considered to be more plausible data for the effect of SLR on flooding.

NR	RUN NAME	RAIN [%]	WIND [%]	SLR [m]
1	Factual	0	0	0
2	CF_all_low	-4	-1	-0.05
3	CF_all_medium	-8	-5	-0.10
4	CF_all_high	-16	-10	-0.15
5	CF_rain_low	-4	0	0
6	CF_rain_medium	-8	0	0
7	CF_rain_high	-16	0	0
8	CF_wind_low	0	-1	0
9	CF_wind_medium	0	-5	0
10	CF_wind_high	0	-10	0
11	CF_SLR_low	0	0	-0.05
12	CF_SLR_medium	0	0	-0.10
13	CF_SLR_high	0	0	-0.15
14	CF_rainwind_low	-4	-1	0
15	CF_rainwind_medium	-8	-5	0
16	CF_rainwind_high	-16	-10	0
17	CF_rainSLR_low	-4	0	-0.05
18	CF_rainSLR_medium	-8	0	-0.10
19	CF_rainSLR_high	-16	0	-0.15
20	CF_windSLR_low	0	-1	-0.05
21	CF_windSLR_medium	0	-5	-0.10
22	CF_windSLR_high	0	-10	-0.15

Also worth noting is that we removed Table S2 from the supplement, as Table 2 in the manuscript is now updated to include a range of plausible counterfactual values for the considered scenarios. Moreover, we have updated the land cover dataset to Vito 2019 compared to the earlier used Vito 2015 for consistency but this had a negligible impact on the factual flooding values.

Reply to reviewer 1

This manuscript addresses an important gap in attribution science by developing a storyline framework for tropical cyclone-induced compound flooding in data-sparse regions. The authors demonstrate technical sophistication in coupling multiple state-of-the-art models (SFINCS, wflow, D-Flow FM) to resolve all flood drivers dynamically. The application to TC Idai in Mozambique is particularly valuable given the underrepresentation of African cyclones in attribution literature. The work makes meaningful contributions to understanding how climate signals propagate from hazard to impact through nonlinear damage relationships.

However, the manuscript requires strengthening in several critical areas before publication. The counterfactual design lacks sufficient scientific justification for key parameter choices, the validation strategy needs refinement given data limitations, and the uncertainty quantification is inadequate for the compounding uncertainties inherent in this multi-model framework.

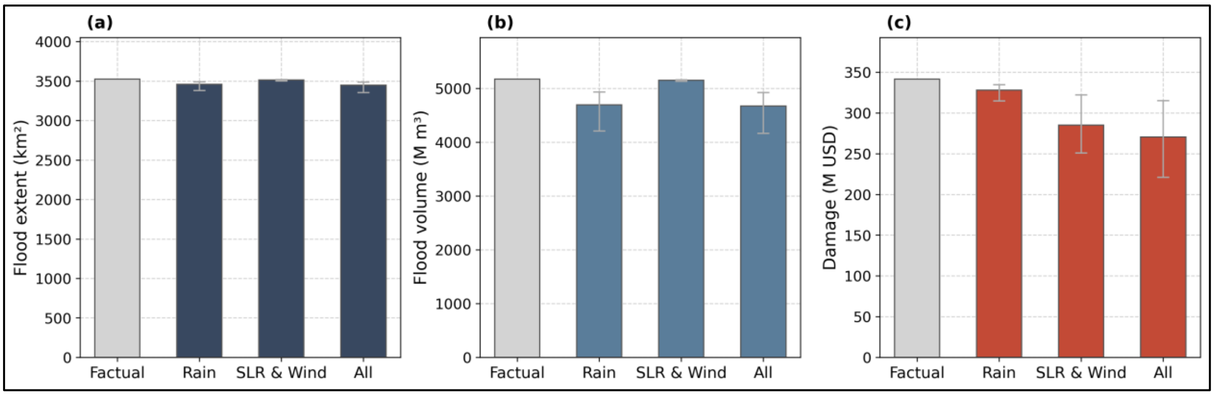
We thank the reviewer for the detailed comments and feedback they have provided. As explained in the general response section, quantifying all uncertainties and propagating these through the whole framework is not in the scope of our study and would also reduce the framework's applicability for possible future operational applications in data-scarce regions. We have made significant revisions to the manuscript to ensure that the results are interpreted within their limitations, presenting plausible and informative attribution statements that advance current attribution science. We agree that the description of those inherent uncertainties, limitations and inconsistencies lacked in the submitted manuscript, which has now been revised.

Major Comments

1. Counterfactual Design and Scientific Justification

The manuscript's attribution conclusions depend entirely on robust counterfactual scenarios, yet the justification for key parameters is insufficient:

For all of the points below, Sections 2.3.3 and S1.6 have been updated in the revised manuscript and supplementary material. We have included uncertainty bounds in Fig. 5 and Table 2 of the manuscript. The earlier described medium counterfactual scenarios are used for Fig. 4 and 6. We have also added more emphasis on the plausibility of these values, to better convey our confidence in the conditional attribution assessment. Since Fig. 5 only shows relative changes, we added a figure below for reference showing the uncertainty bounds of the different plausible climate trends on the absolute values for flood extent, flood volume and flood damage.



- **Rainfall reduction** (8%): While the Clausius-Clapeyron relationship is cited, the manuscript should explicitly demonstrate this calculation rather than only asserting it. Recent literature supports 8% reductions for ~1.1°C warming, but this warrants a dedicated methods subsection showing this applied to the case here and a discussion of uncertainties in this approach, particularly for tropical cyclones, where dynamic effects may deviate from thermodynamic expectations.

Following the suggestion of the reviewer, we have added additional counterfactual rainfall scenarios of -4% (lower than Clausius–Clapeyron due to possible enhanced cooling from slower TC translation speeds in a warmer climate; Tu et al., 2022) and -16% (super Clausius–Clapeyron; Guzman and Jiang, 2021; Liu et al., 2019; Patricola and Wehner, 2018), and added scientific support for these values in L195-202. We have also explicitly added the Clausius–Clapeyron relationship used for the calculation of the counterfactual rainfall values in the method Section 2.3.2 in L197. Lines L195-202 are presented below for convenience:

"*The plausible climate trend of TC rainfall, maximum wind speed and SLR is based on best available literature and global datasets. For the climate change effect on TC rainfall, some studies find that the trend is in line with the Clausius– Clapeyron relationship (7 %/°C of warming; Knutson et al., 2020), while other studies show trends higher than Clausius–Clapeyron (Guzman and Jiang, 2021; Liu et al., 2019a; Patricola and Wehner, 2018), referred to as super Clausius–Clapeyron (14 %/°C of warming), and lower due to enhanced cooling from slower TC translation speeds in a warmer climate (Tu et al., 2022). As Idai took place in a ~1.1 °C warmer world, we adopt plausible reductions of rainfall of 4 %, 8 % and 16 % for the low, medium and high counterfactual scenarios, respectively.* "

- **Wind speed reduction** (10%): This is more problematic. Knutson et al. (2020) report median projections of 1-10% intensity increases for 2°C warming, suggesting 0.5-5% for current ~1.1°C warming. A 10% reduction appears to overestimate the counterfactual change, potentially inflating the attributed impact from wind-driven processes. The manuscript cites Mester et al. (2023), who used regional observed trends, but doesn't establish why this particular value is appropriate.

Following the suggestion of the reviewer, we have added additional counterfactual wind speed scenarios of -1% and -5%, both based on Knutson et al. (2020) and rounding to integers (L202-206), to capture uncertainty in the climate change effect on TC wind speed:

"*For the climate change effect on TC maximum wind speeds, we use plausible reductions of a 1%, 5% and 10 % for the low, medium and high counterfactual scenarios, respectively. The low and medium scenarios are based on the likely range of 1–5 % per °C of warming for the Southern Indian ocean from climate models (Knutson et al., 2020), and the high scenario of a 10 % wind speed reduction is based on regional trends from observed TCs (Mester et al., 2023).*"

- **Sea level rise component**: The methodology for SLR estimation needs clarification:
    - Authors use the Treu et al. (2024) dataset but this contains systematic biases. The manuscript must explicitly address whether such biases affect factual and counterfactual equally (canceling in differences) or differently (amplifying attribution error)

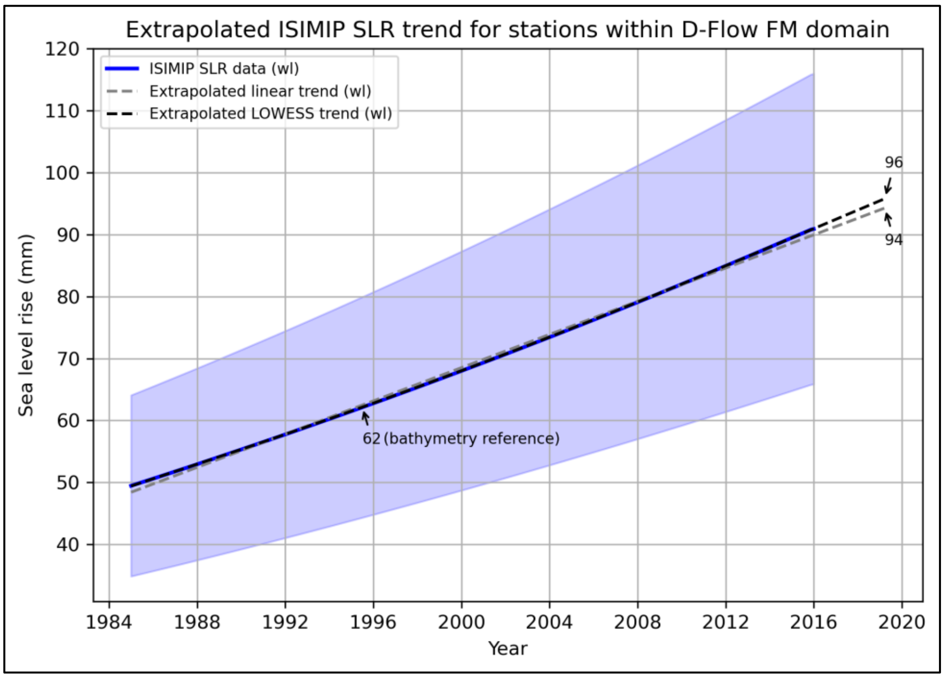
To capture uncertainty in the climate change effect on SLR, we have added two additional counterfactual SLR scenarios. As mentioned in the general response section, we have switched from the geocentric water level to the water level dataset from Treu et al. (2024), which includes vertical land movement. This is also incorporated in our correction for the vertical datum of the bathymetry (see Section 1.6). Analyzing the water level dataset from Treu et al. (2024) for the stations in Mozambique within our D-Flow FM domain leads to a mean SLR of +0.10 m at the time of Idai since 1901 (see updated Sections 2.3.2 and S1.6). For our medium counterfactual scenario, we remove this best estimate of SLR (-0.10 m). For the low and high counterfactual scenarios, we take ±0.05 m from the medium counterfactual SLR scenario based on Strauss et al. (2021), which results in values of -0.05 m and -0.15 m for the low and high scenarios (L204-210):

"*For the climate change effect on SLR, we use plausible reductions of 5, 10 and 15 cm for the low, medium and high counterfactual scenario, respectively. The medium scenario is based on the dataset by Treu et al. (2024), used to estimate the SLR between the time of the event and pre-industrial levels, and the low and high scenarios are based on uncertainty bounds from Strauss et al. (2021)*"

The dataset of True et al. (2024) is derived by combining different state-of-the-art global datasets. While there may be biases (as with any dataset), we now include additional counterfactual scenarios to account for uncertainty in our attribution results (see Table 2 in the revised manuscript). As such, we consider the dataset by Treu et al. (2024) to be suitable for the type of application that we present here.

- Figure S10 shows only 2015 data, yet the authors extrapolate to 2019, assuming linear trends.

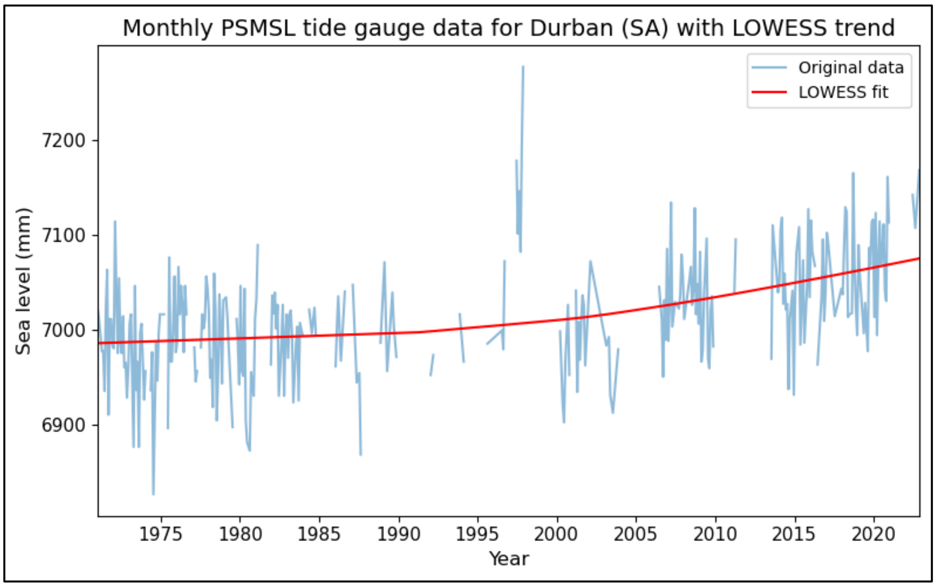
Instead of extrapolating the 2015 SLR data from Treu et al. (2024), we have now extrapolated the data based on 30 years of data and used both linear and LOWESS extrapolation, see figure below. The difference between these extrapolation methods is minimal (1.6%), but the LOWESS-based extrapolation fits slightly better and is therefore adopted (updated figure S10 in Supplement).



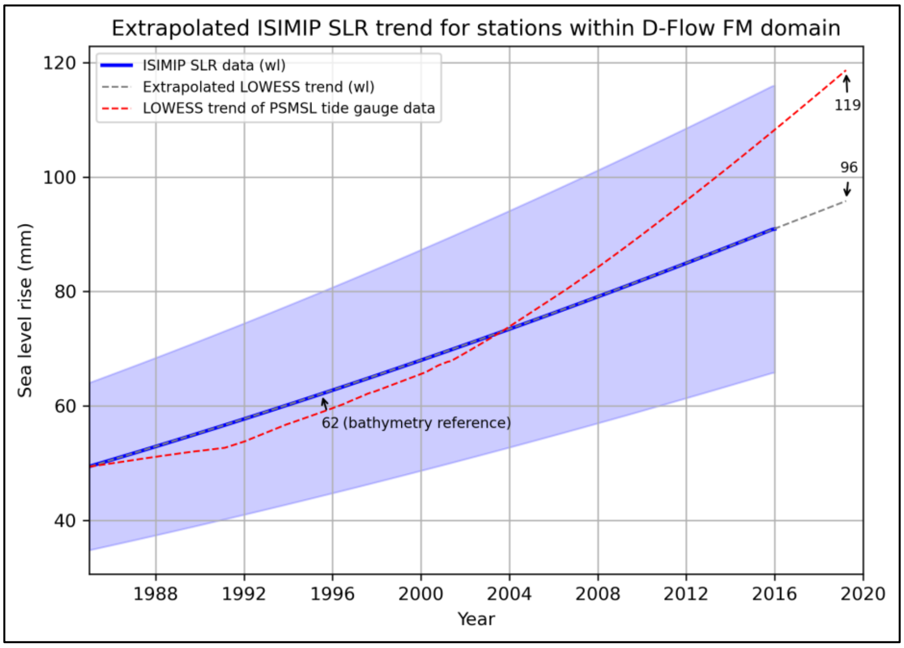
- Long-term tide gauge observations from the region should be incorporated to validate the 14 cm estimate

There is no tide gauge data available along the coast of Mozambique, but we did a trend analysis for the nearest tide gauge station with sufficient data, located in Durban, South Africa, using Permanent Service for Mean Sea Level (PSMSL) data (Holgate et al., 2013; Permanent Service for Mean Sea Level

(PSMSL), 2026). Similar to the long-term SLR data from True et al., we have fitted a LOWESS trend to the tide gauge data:



For our comparison, we assume equal SLR of the tide gauge data as at the start of the 30-year timeseries from Treu et al. (2024). Comparing the SLR at the time of event, we find the tide gauge data to result in a SLR of 119 mm compared to our SLR estimate of 96 mm. The tide gauge SLR value of 119 mm falls within the additionally included plausible range for SLR (150 mm).



- Bankfull discharge assumption: The 2-year return period assumption for bankfull discharge should scale between scenarios. If climate change increases discharge, the effective channel capacity may differ between factual and counterfactual. Holding this constant creates an inconsistent comparison -the counterfactual world would have had different equilibrium channel geometries. This deserves explicit discussion or sensitivity testing.

We thank the reviewer for the comment about possible realistic changes of climate change on river bathymetry due to increased precipitation, but argue that this is beyond the scope of this study. As explained in the general response section, we assume similar river bathymetry for the purpose of this storyline attribution assessment. We have made this assumption explicit in L192-193: "*We assume no change of non-flood drivers, such as exposure and vulnerability.*" and L225-226: "*The attributable change is conditional on the considered counterfactual flood drivers, and on the assumptions made in the model schematizations and input datasets.* "

2. Methodological Concerns

- **Model simulation duration**: How long were the simulations run? The manuscript doesn't specify the total simulation period or spin-up time. For compound flood modeling, the synchronization of multiple drivers is critical. Eilander et al. (2023) found surge peaked 3-5 days before discharge for TC Idai, producing limited compound interaction. Does this framework capture such timing effects?

Yes, we do capture timing effects, see Fig. S13b-c. The total simulation period was mentioned in line 91 *"(9 to 25 March 2019)*") but has been made more explicit in the revised version of the manuscript *"(simulated for 9 to 25 March 2019)*" (L93). We refer to the next response for comments on the spin-up time.

- **Boundary condition consistency**: How are the multiple drivers synchronized temporally across model domains? The wflow warm-up (365 days) is mentioned, but what about SFINCS, D-Flow FM initialization? Are antecedent soil moisture conditions consistent between scenarios? How is infiltration handled in SFINCS relative to wflow?

The D-Flow FM model has an initialization spin-up adjusted to the models' inherent timescale, which is 3 days. For SFINCS, the model starts 5 days before TC Idai hits the case study area, i.e. 5 days before the effect of the TC becomes visible in the flood model forcing (including discharge, precipitation or coastal surge). During this period no flooding occurs, and can therefore be considered spin-up time. All this information can be found in the model building scripts, but is too detailed to be added in the manuscript.

The antecedent soil moisture conditions are kept constant between scenarios, which is in line with our storyline experiment. Within wflow, the available water for infiltration is taken as throughfall and stemflow (and snow runoff and glacier melt, if applicable). The actual infiltration depends on the vertical saturated conductivity and the soil moisture capacity. As SFINCS does not have a soil component, for the infiltration the simpler Curve Number method is used, where a fraction of the precipitation is assumed to be infiltrated. Curve Number values vary spatially based on land use, taken from the global dataset by Jaafar and Ahmad (2019).

- **Validation metrics**: The validation should report separate performance metrics for: coastal zones (surge-dominated), fluvial zones (discharge-dominated) and compound zones (driver interaction). This would strengthen confidence that the model captures the different flooding mechanisms appropriately.

We like the suggestion of the reviewer but argue it is beyond the scope of this paper. Additional runs would be necessary to define these separate zones. Moreover, with the considerable uncertainty in the satellite products, we argue that providing zone-specific hit rates will not provide a lot of new information to the notion that the lack of consistence between these satellite products do challenge any verification, which is already reported.

- **Meteorological forcing quality**: ERA5 underestimates TC intensity (appropriately addressed by Holland parametric winds), but the manuscript should clarify: Was the TC parametric wind model used throughout the entire modeling chain? If so, how do biases propagate? The Holland model performs well when fitted to observations or additional empirical relationships, but "out of the box" applications can have significant errors and miss asymmetric TC shapes. The 0.75 TC radius merging also needs explanation—how sensitive are results to this choice?

We used a parametric tropical cyclone model to generate wind and pressure fields for both D-Flow FM and SFINCS to ensure consistent forcing, particularly important for coastal areas. This was done according to the Holland model (Holland, 2008), fitted to TC Idai parameters from the IBTrACS database. The wind and pressure fields were thus defined over a large area around the TC track, extending well beyond the R34 radius of the TC (area of extreme winds), using the 'spiderweb' schematization, commonly applied for tropical cyclone modelling in D-Flow FM (Deltares, 2026, pp. 234–235). To ensure realistic meteorological conditions outside of the TC area of influence (e.g. during model spin up, before the TC enters the model domain), the parametric fields were merged with ERA5 reanalysis data at 0.75 of the 'spiderweb' forcing field radius (merge fraction parameter). This, however, does not affect the coastal surge generation in our area of interest, because the TC track passes directly through the case study region and surge is generated by winds well within the 0.75 of the TC forcing field radius. Because the 0.75 merge fraction does not influence the coastal water levels (and hence the attribution results), we removed it from the manuscript (L178-179); this technical detail remains documented in the published scripts.

3. Wave Setup and Coastal Process Assumption

- The **assumption that wave setup remains constant across counterfactual scenarios** is scientifically invalid and potentially introduces substantial error since wave setup scales with wave breaking intensity. A 10% wind reduction would generate lower wave heights, producing correspondingly lower setup—potentially 15-20% reduction if setup scales as Hs^2.

For similar reasons as given before, we consider this element beyond the scope of our study. Including a counterfactual dynamical wave scenario would require setting up and validating an additional model (deep-wave model forced by the counterfactual TC parametric wind model), as a simple assumption on the fractional effect of change in wind speed on wave height would not cover this uncertainty properly. However, we agree with the reviewer that including a counterfactual wave scenarios would be very interesting, and could cause additional impacts that can be potentially attributed to climate change. Therefore, we encourage future studies to include a counterfactual for this driver to our framework, as mentioned in L401 of the manuscript. We have also removed the word “all” from TC flood drivers in L87.

- Also, the **SnapWave approach over transects** is problematic since wave conditions aren't properly downscaled (ERA5 offshore waves are coarse), transects are poorly connected to actual water levels, and local nearshore processes (refraction, shoaling, breaking) may be inadequately represented in a transect approach

Apologies for the unclear description of our wave modelling approach. In our study, SnapWave is not used as a stand-alone transect model, nor are offshore ERA5 wave conditions directly imposed at the coastline. Instead, we employ a fully 2D flow–wave coupling through SFINCS–SnapWave to calculate wave-induced setup (Fig. 2 in manuscript), in which SnapWave is explicitly used to downscale offshore

ERA5 wave forcing and resolve local nearshore wave processes, including refraction, shoaling, and depth-limited breaking.

The coupled SFINCS–SnapWave simulations produce a wave-induced water level component, which is calculated as the difference between simulations with and without SnapWave, resulting in the targeted wave setup component. For practicality and to maintain computational consistency within our existing hydrodynamic modelling framework, the wave setup timeseries is subsequently extracted along coastline perpendicular transects and superimposed onto the nearest tide–surge water levels at Delft3D-FM output points (see S1.4). We have revised L145-150:

"*We combine the tide and surge with dynamically modelled wave setup, calculated from a coupled SFINCS-SnapWave simulation. The 2D SFINCS-SnapWave model has a spatially-varying grid with a resolution of 400 m offshore to 50 m at the coast, covering an area of 5400 km^2 (yellow domain in Fig. 2). The D-Flow FM output is generated around the 5-meter depth contour within the SFINCS domain at a 10-minute temporal resolution (Fig. S1). The wave setup output is also generated at a 10-minutes temporal resolution and saved at coastal transects*"

- I strongly recommend: (1) fully integrate SnapWave 2D within SFINCS for dynamic wave-surge coupling in both scenarios, OR (2) remove the wave coupling component entirely, acknowledge this as a limitation, and note it may introduce ~10 to 20% uncertainty in coastal flood depths. The current approach undermines the compound flooding framework's credibility.

While fully acknowledging that the current approach could be further improved, dynamically accounting for the wave contribution in a globally applicable approach is already a large advancement over current globally applicable approaches that tend to reply on simplistic parametric estimates or empirical estimates (Hinkel et al., 2021). SnapWave is very recently developed software (Roelvink et al., 2025), and at the time of our framework development, fully integrating SnapWave within SFINCS combined with all compound flood drivers was technically not yet possible (L78-81 in Supplement). We argue that having a better estimate of our factual scenario by including wave setup from SFINCS-SnapWave still adds value as it provides higher confidence in the simulated flooding and therefore in the attribution assessment. For this reason, we have chosen a hybrid approach by including a credible application to assess the contribution of waves on the event, without a full integration of the climate change impact on this contribution.

4. Flood Damage Modeling

- Depth-damage curve validation: The continental curves from Huizinga et al. (2017) assume European-style construction. Snel et al. (2019) showed Ethiopian traditional buildings experience 100% damage at 2m depth versus 5m for concrete structures. Mozambique's post-Idai assessments reported 111,163 completely destroyed houses, but no published studies validate these damage functions against actual losses. I recommend: 1) comparing aggregate model damage against reported sector-specific losses, 2) conducting sensitivity analyses with alternative damage curves for informal/traditional construction and 3) discussing this as a major uncertainty source.

We agree with the reviewer that these simplified depth-damage curves are uncertain but would like to stress that we work with the best available global data and state-of-the-art methods, allowing global applicability of our framework in data-scarce contexts. In response to this and the other comments, we have made our assumption explicit in L225-226, and made it more explicit that these damage curves are a major source of uncertainty in L411 of the discussion. Comparing our damage estimates with sector specific losses is too detailed for the scope of our study.

L225-226: "*The attributable change is conditional on the considered counterfactual flood drivers, and on the assumptions made in the model schematizations and input datasets.* "

5. Missing Elements: Uncertainty Quantification

This is the review's most critical concern. Every component carries substantial uncertainty:

- Counterfactual parameter choices (rainfall: ±2-3%, wind: ±5%, SLR: ±5cm)
- Meteorological forcing (ERA5 vs. parametric TC model inconsistencies)
- Hydrological model calibration
- Missing river bathymetry (bankfull approximation)
- Wave setup assumptions
- Exposure data completeness
- Damage function transferability

These uncertainties compound multiplicatively, not additively. The total framework uncertainty is substantial.

<u>The manuscript MUST include:</u>

1. Uncertainty quantification at each modeling step
2. Formal uncertainty propagation through the attribution framework
3. Comprehensive sensitivity analysis on key assumptions
4. Probabilistic framing of attribution statements with confidence intervals

Recent attribution papers explicitly report uncertainty ranges. Does climate change contribute 5-15% or 25-35% to damages? Without uncertainty bounds, readers cannot properly interpret the "31% damage attributable to climate change" conclusion.

As mentioned in the general response section and individual response to the reviewers comments, we have improved the manuscript as follows:

- Refined the scope of our study in L77-79, L87-90
- Clarified uncertainties and assumptions made within the scope of our study (L225-226).
- Throughout the manuscript improved the wording of our attribution assessment as being conditional, and using plausible climate trends for the constructions of counterfactual scenarios.
- Include climate trend uncertainty propagated in our counterfactual scenarios through the whole modelling chain for a low, middle and high scenario, and presenting our results as a

range of plausible values for the conditional attribution statement (Table 1 and 2 in the revised manuscript).

- Additional wflow hydrological model validation using GloFAS discharge data (Section S1.3)
- Additional SLR analysis and comparison with PSMSL data for the closest tide gauge (this rebuttal document).

As a result, these clarifications and additional experiments have helped to improve the interpretability of our results for the reader, leading to changes throughout the manuscript and in the abstract (L8-23).

Minor Comments

Exposure and Vulnerability Treatment (L285-290)

The manuscript states that exposure/vulnerability is held constant, but this critical assumption deserves more prominent discussion. The counterfactual answers: "What would 2019's exposed population experience under a pre-industrial climate?" not "What would the pre-industrial population experience?" This is correct for physical attribution, but should be explicitly stated in Section 2.3.2 rather than buried in the discussion.

We have explicitly included references to our assumptions on constant exposure and vulnerability between scenarios as part of Section 2.3.2 (L192-193) "*We assume no change of non-flood drivers, such as exposure and vulnerability, in the counterfactual scenarios*", per suggestion of the reviewer. We would like to take this opportunity to share that we are working on extending the framework to include a counterfactual exposure scenario in an upcoming paper.

Supplementary Figure S2 (Return Period Analysis)

The disconnection around 1-2 years is striking and unexplained. How was the fit performed? Was this a standard GEV/Gumbel distribution? The discontinuity suggests potential issues with the extreme value analysis or the underlying wflow discharge distribution. Please add a methods subsection describing the EVA approach and discuss this feature.

The extreme value analysis was done using the pyextremes Python package (Bocharov, 2023) that relies on standard, well-known fitting approaches to define extremes events (i.e. block maxima or peaks-over-threshold methods) and to estimate distribution parameters. Here, we selected yearly maxima from the time series and then fitted a GEV distribution, in accordance with the extreme value theorem that the GEV distribution is the limit distribution of independent and identically distributed block maxima samples (here, yearly maxima). In cases where the shape parameter is 0, this resulted in a Gumbel distribution. The selection between Gumbel or GEV was based on the Akaike Information Criteria (AIC) goodness-of-fit metric. The discontinuity observed is the result of a few years being less extreme than others and is mainly the result of the limited length (30 years) of the time series. As suggested by the reviewer, we have added more details on the EVA approach in section S1.2.

Line 175 (Holland Model Implementation)

"linearly fading the data at 0.75 fraction of the TC radius" - Why 0.75? This appears arbitrary and could significantly affect results. Show sensitivity or cite precedent. Also, how is the TC eye resolved in terms of rainfall distribution? The Holland wind model has asymmetric components—were these included?

The 0.75 merge fraction is explained in an earlier response. For asymmetry in the Holland wind model, the TC asymmetry between different quadrants was defined using Schwerdt et al. (1979), as common in literature (e.g., Leijnse et al., 2021). While more advanced tropical cyclone wind models exist, data availability is often a limiting factor, and in our view, this makes application on a global and operational context infeasible. Considering the validation of the model, which is reported in section 3.2, S1.3 and S1.5, we consider our approach valid for the scope of our study.

Recommendations

Despite the substantial revisions required, this manuscript represents important and novel work. The technical execution is sophisticated, the application to Mozambique addresses a critical gap, and the compound flooding attribution framework is genuinely innovative. With careful attention to the major comments—particularly uncertainty quantification, counterfactual justification, and wave setup treatment—this can become a strong contribution to NHESS and the broader attribution literature.

Priority actions:

1. Add comprehensive uncertainty analysis (Monte Carlo or ensemble approaches)
2. Revise or remove the wave setup coupling
3. Strengthen counterfactual justifications with sensitivity analyses
4. Improve damage model validation against reported losses

The open-source, globally-applicable framework you've developed has significant potential for advancing attribution science in vulnerable, data-poor regions. I look forward to seeing the revised manuscript.

We would again like to sincerely thank the reviewer for their time to provide the detailed and elaborate comments on our manuscript. We believe these have strengthened our work significantly. Please refer to our earlier response to individual comments and the general response for the actions we have taken to include the reviewers suggestions.

References

Bocharov, G.: pyextremes (Version 2.3.3), https://github.com/georgebv/pyextremes, 2023.

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