Responses to Reviewer 1 Comments

General comment 1

The authors provide a comparative review of three alternative approaches for assessing the local compound flooding probability. They implement the approaches for studying compound flooding in the estuary of the Swan River in Western Australia. Such an application provides a basis for discussing the advantages and limitations of the three approaches. Overall, I did find the study very interesting and timely. I recommend revising the manuscript based on my comments prior to publishing the manuscript.

Response:

Thank you for your overall positive comments on our paper. Please find responses to your detailed comments below.

General comment 2

In general, I found the introduction pleasant to read, but I would recommend improving the presentation of the methodology, especially of Method 3. In fact, I found it particularly difficult to understand some components of Method 3.

Response:

This section on method 3 has been revised: a brief introduction has been added at the beginning to outline all of the steps involved; and the description of each step is also revised to include details of how each step is carried out (e.g. in relation to the case study used). Please also see detailed responses on relevant comments below.

General comment 3

The discussion of the advantages and limitations include a part regarding the limitations/advantages that the approaches have for assessing the climate change effect on compound flooding probability. The idea of discussing this topic is certainly interesting, but it requires some revision in my view. For example, the authors mainly refer to the possibility of including changes in the dependence between the drivers through method 3, but it is not discussed the relevance of the change in the marginal distributions, which is fundamental. In particular, I understand that the authors state that method 2 is difficult to be considered for assessing climate change as it requires time series of storm tide and precipitation for the future. This would be an issue also for method 3, despite the fact that they claim that method 3 can consider climate change effects easily. See comments related on this topic below.

Response:

We agree with the reviewer that changes in the marginal distributions as a result of climate change are of fundamental importance, and have ensured that this is clearly articulated in the manuscript.

However we maintain that there are several key advantages of using method 3 that make it much easier to apply for climate change applications in practice compared to method 2.

Of these, the most important is that the nature of the decomposition between the marginal and joint distribution in method 3 means that it is relatively straight forward to capitalise on existing information and agreed approaches (including those embedded in flood risk estimation manuals that are available in various countries around the world) that allow uplift of the ‘marginal distributions’. For example in Australia, there is guidance on increasing Intensity-Duration-Frequency curves (which characterise the marginal distribution of rainfall) by 5% per degree (range 2%-10%) (Ball et al. 2019) as a result of climate change. Similar approaches to uplift factors exist in many other parts of the world. In relation to sea level rise and storm surge, one could also elect to increase the marginal distributions by ‘factors’ representing changes in those components. The capacity to align with standard flood estimation approaches (e.g. the Australian Rainfall and Runoff and the UK flood guidance) is more than a pragmatic advantage, as it also enables seamless interface between estuarine floods and upstream flood risk estimates, since both would be driven by the same changes to IDF curves (the same example could be used for transitioning between estuarine and coastal floods).

A secondary benefit is that, under certain conditions, the method capitalises on existing hydrodynamic model runs rather than requiring these to be repeated, since it would be possible to use changes in both the marginal and joint distribution to recalculate the probabilities of the simulated flood levels (in other words, the simulated water levels would stay the same, but the exceedance probabilities ascribed to those levels would change).

This is contrasted to method 2, which would require the full joint timeseries of the boundary conditions (e.g. sub-daily time series of wind and pressure needed as a boundary condition to storm surge, plus rainfall needed for inland catchment processes) and thus is a much more involved problem. Indeed, to our knowledge, we are not aware of any examples where this has been achieved in practice or published in the literature. Of course it could be argued that one can just apply similar scaling factors to historical time series; however in the case of rainfall, it’s well known that the averages will change in a very different way to the extremes, so this scaling would be difficult in practice. As a result, solving this is a much more challenging problem and unlikely to be practically viable except for very research-heavy applications.

The following discussions (and additional references) have been added to improve clarity:

> “A range of multivariate approaches have been applied to compound flood estimation problems, including Vine copula (Bevacqua et al., 2017), standard copulas (Muñoz et al., 2020), unit Fréchet transformations (Zheng et al., 2014), regression type models (Serafin et al., 2019) and conditional exceedance models (Jane et al., 2020). The use of copulas or equivalent formulations (e.g. unit Fréchet transformations) enables the factorisation of multivariate distributions into a set of marginal distributions and a dependence structure (i.e. a joint probability distribution). This joint probability distribution captures the defining features of the variables of interest and their interaction. ”
>
> “…,it becomes easier to incorporate the effects of future changes. This is particularly the case if one is able to assume that the dependencies between variables are either not greatly affected by climate change or that changes in dependencies produce second-order effects on flood probability compared to changes in the marginal distributions. Under these conditions, the method can capitalise on published information on uplift factors to changes

in the key marginal distributions (e.g. scaling factors for IDF curves, or for peak ocean levels), which are becoming increasingly commonly available as part of engineering flood guidance in many parts of the world (Wasko et al, in press). A further advantage is that under the assumption that the relative timing of different flood drivers is not considered (see discussion in the paragraph below), the flood surface produced using hydrodynamic models will not change under climate change; rather it is how the flood surface is converted into flood probability based on the dependence model that will change. Indeed, by separating the flood estimation problem into the two components indicated above, it could be possible under certain conditions to estimate the impact of future changes such as climate change on estuarine flooding without additional hydrodynamic simulations, simply by re-calculating the probabilities of the flood drivers and their dependence structure under changed future conditions."

In addition, the following changes have been made to section 2.4 to improve clarity:

- The 'two steps' in this section has been change to 'two components' to differentiate form the steps required when implementing each method.
- Component 2 is revised to "the estimation of the flood magnitude (i.e. water levels) for each combination of boundary conditions, using what is often referred to as a 'structure variable' or 'boundary function'."

Specific comment 1

L47, I would cite the paper from Wahl at the end of the sentence (already mentioned in the manuscript).

Response:

The reference has been added.

Specific comment 2

L64, please, consider merging this sentence with the last sentence of the previous paragraph (on the same topic).

Response:

Thank you for this suggestion. The first sentence is on the joint impact of different flood drivers. The second one is on the impact of future climate conditions on the joint impact of different flood drivers. Therefore, we felt it best that these be kept as distinct ideas.

Specific comment 3

L76 I suggest that produce an inverse barometric effect and on-shore winds, which in turn leads to storm surges and waves

Response:

Thank you for the suggestion. It is changed to “that produce on-shore winds and an inverse barometric effect, which in turn leads to storm surges and waves”.

Specific comment 4

L78 water -> oceanic water level (to make clear you are referring to the sea component only in this sentence).

Response:

It has been revised as suggested.

Specific comment 5

L116, typology

Response:

Thank you for the observation. It has been corrected.

Specific comment 6

L128 “and considered here” after identified

Response:

Thank you for the suggestion. It has been added.

Specific comment 7

L 129-130, Consider using "compound flood" here and elsewhere when referring to the compound flooding water level, such to make clear that you are referring to the resulting water level from the two drivers. For example, in the caption of Fig 2. I can certainly say that this would have made my reading easier.

Response:

Added, as suggested.

Specific comment 8

L 146 “numerical” is fine here? In method 2 you may not need a numeric model (i.e., hydrodynamical model) rather use an e.g., statistical model (personally, I do not see that as numerical). I see that in your case you use numerical modelling, but this part of the manuscript appear of a more general nature.

Response:

The wording has been changed to “numerical or statistical modelling”.

Specific comment 9

L 166 Similarly to the above, doesn't dynamically refer to something that is not statistical? Anyway, I would modify, to make clear that such modelling can also be purely statistical.

Response:

“Numerical modelling” is not referred here, so no changes are made.

Specific comment 10

L 159 Consider adding Approach 1,2,3, also earlier on, to give a better orientation to the reader.

Response:

Added at the beginning of section 2.1.

Specific comment 11

L 174, do you mean 30 years of data to estimate the 1-in-100 years return level? Anyway, you may want to qualify "estimate", anything can be estimated, but would that estimate be too uncertain or not?

Response:

Yes this is correct. The sentence has been revised to “to ensure sufficient accuracy in flood estimates, with a typical rule-of-thumb being the requirement of at least 30 years to estimate flood levels corresponding to probabilities up to the 1% annual exceedance probability (Ball et al., 2019)”.

In addition, a statement on the uncertainty of the results obtained using this method is added in the results section:

“The confidence intervals become increasingly wide with increasing return period, and it is important to note that return periods have been calculated based on only 22 years of historical water level data.”

Specific comment 12

L 185, During a discussion among colleagues, it was hypothesised that this may be related to the fact that often there is interest in measuring either the sea level or the river discharge and therefore no stations are collocated at the interface between the two. What do you think about this? Discuss it if you think that this is relevant. I guess that this appears also discussed/hypothesised in Paprotny et al. ("Compound flood potential in Europe").

Response:

Thank you for this suggestion, the following comment has been added.

"The lack of gauges within estuaries are likely to be at least in part due to the fact that there has historically been greater interest in measuring either the sea level or the river discharge and therefore there is less interest to place stations at the interface between the two (Paprotny et al., 2018)."

Specific comment 13

L 188, please, make it clear that you are referring to the need of transforming flow into the water level

Response:

The sentence has been revised to

"…, which can be problematic in estuarine regions where flows can be bidirectional and water levels are influenced by both upstream and downstream processes."

Specific comment 14

L 205, Do you have a reference? Not sure if this was given earlier.

Response:

The following references have been added:

Boughton, W. and Droop, O.: Continuous simulation for design flood estimation—a review, Environmental Modelling & Software, 18, 309-318, 2003.

Sopelana, J., Cea, L., and Ruano, S.: A continuous simulation approach for the estimation of extreme flood inundation in coastal river reaches affected by meso- and macrotides, Natural Hazards, 93, 1337-1358, 2018.

Specific comment 15

L 227, "Although…". Consider moving this to the beginning of the next paragraph. In general, regarding sections 2.2-2.4, 230-241, I believe that the reader would benefit from finding dome additional references to works where similar approaches have been used. In my view, this can help, especially in a work like that aims at reviewing available methods.

Response:

Thank you for this suggestion. The paragraph has been revised as suggested. The following references have been added.

Hasan, H. H., Mohd Razali, S. F., Ahmad Zaki, A. Z., and Mohamad Hamzah, F.: Integrated Hydrological-Hydraulic Model for Flood Simulation in Tropical Urban Catchment, Sustainability, 11, 2019.

Heavens, N. G., Ward, D. S. & Natalie, M. M. 4: Studying and Projecting Climate Change with Earth System Models., Nature Education Knowledge, 4, 4, 2013, 4(5):

Zaehle, S., Prentice, C., and Cornell, S.: The evaluation of Earth System Models: discussion summary, Procedia Environmental Sciences, 6, 216-221, 2011.

Specific comment 16

L230-241, I find this part a bit too strong in the statements. Hydrodynamical modelling works based on oceanic and streamflow input that is available from climate models. I see that there are uncertainties and that storm tide and river flow need to be obtained based on computationally expensive modelling. But some data are available out there that can be helpful to assess the climate change impact on compound flooding even with Approach 2. I would suggest discussing this topic more.

Response:

I agree that this is a topic that different people may feel differently and worth discussion. The authors believe that by extending modelling boundary the model will generally become more complex and additional errors will be introduced. It is important to recognise this challenge when considering extending model boundaries. However, it is also important to recognise that some datasets already exist as boundary conditions, which are helpful to assess the climate change impact on compound flooding even with Approach 2. This section has been revised in the manuscript:

"Widening the modelling chain to explicitly represent an ever-increasing set of time-varying processes is certainly an attractive means to explicitly address non-stationarity of key flood generating processes. This is especially the case considering that some datasets from climate models already exist as boundary conditions for hydrodynamical modelling runs (e.g. Kanamitsu et al. (2002) and Naughton (2016)), which are helpful to assess climate change impact on compound flooding with Approach 2. However, it is important to recognise that widening the modelling chain can also lead to evermore complex models, with greater possibility of inducing biases and other forms of modelling errors into the results (Zaehle et al., 2011)."

Specific comment 17

L252, Isn't it what you produce the value of the structural variable rather than the variable itself? You use a "function" to convert a given bivariate event into a water level.

Response:

It is correct that the second component of this method is to produce the water levels. This statement is revised to:

"2) the estimation of the flood magnitude (i.e. water levels) for each combination of boundary conditions, using what is often referred to as a 'structure variable' or 'boundary function'."

Specific comment 18

L 262, "condition." Please, provide a reference, where this method is described

Response:

The following reference is added.

Ball, J., Babister, M., Nathan, R., Weeks, W., Weinmann, E., Retallick, M., and Testoni, I. (Eds.): Australian Rainfall and Runoff: A Guide to Flood Estimation, Commonwealth of Australia, 2019.

Specific comment 19

L 266, how to select the design events? Multiple pairs in the bivariate space can have the same probability to occur, i.e. return period. Therefore the selection is not as easy as in the univariate case. This is discussed in the paper of Moftakhari et al. (2019). A brief discussion (2/3 sentences) on these issues is welcome.

Response:

It is true that multiple pairs of drivers in the bivariate space can have the same probability to occur. However, this is expected and not considered an issue. In fact the method is design to deal with this, as on the flood surface (e.g. Figure 8), the estimated flood contour highlights how the same flood level can occur for different combinations of both flood drivers. The key of flood event selection is to have flood drivers with a return period much longer than that of estimated flood levels, as pointed out in section 5.3.

The discussion on the selection of flood events is now included in the second paragraph of revised section 4.3.

"In the first step, compound flood events caused by different flood drivers, such as storm tide and river discharge (i.e. combinations of boundary conditions with different return periods) need to be selected for simulation. Flood levels generated from these flood events will be interpolated to form flood surfaces or response surfaces with different flood magnitudes. The DVM only requires the simulation of a limited number of 'flood events' (often on a regular grid, e.g. 10 by 10 flood events generated from combinations of flood drivers with different return levels) to produce a reasonable cover of the bivariate probability surface formed by two flood drivers (Zheng et al., 2015a; Zheng et al., 2014). In this study, both historical and synthetic flood events on an irregular grid are used to ensure flood events from drivers with significantly longer return period than the estimated flood required are included. This is recommended in order to have reasonable confidence in the estimates (Zheng et al., 2014)."

Specific comment 20

L 268, please, clarify this sentence.

Response:

The following explanation has been added:

"This is particularly the case if one is able to assume that the dependencies between variables are either not greatly affected by climate change or that changes in dependencies produce second-order effects on flood probability compared to changes in the marginal distributions. Under these conditions, the method can capitalise on published information on uplift factors to changes in the key marginal distributions (e.g. scaling factors for IDF curves, or for peak ocean levels), which are becoming increasingly commonly available as part of engineering flood guidance in many parts of the world (Wasko et al, in press). A further advantage is that under the assumption that the relative timing of different flood drivers is not considered (see discussion in the paragraph below), the flood surface produced using hydrodynamic models will not change under climate change; rather it is how the flood surface is converted into flood probability based on the dependence model that will change."

Specific comment 21

L 298 "Due to...complexity", Or is it that none has rally tried to develop one?

Response:

This is correct. There is currently no hydrological model exist for the entire catchment, mainly due to the size and complexity of the catchment.

Specific comment 22

L368, Authors tend to oppose GPD and GEV as alternative approaches. Do you expect any differences in terms of uncertainties? Also, you use the GPD to estimate return periods/level. Shouldn't you also provide an equation for that?

Response:

The difference in the estimation outcomes from GPD vs GEV is out of the scope of this paper. The equation for the GPD is included in section 4.1.

Specific comment 23

L 390, Please, refer explicitly to the fact that extreme H may also be driven by non-extreme conditions of either of the drivers, therefore this should be taken into account when defining the threshold for Q and T.

Response:

The following additional comment has been added in the revised manuscript.

"For example, extreme water levels H may also be driven by non-extreme conditions of either of the flood drivers."

Specific comment 24

L 390, It is not clear to me why you need to account for the low water level periods through the resampling approach, given that you will fit the GPD only to the extremes. I understand that is necessary to be aware of the time in between the peaks to estimate the return periods, but why simulating it?

Response:

One important reason that flood data during low water level periods are also 'simulated' using the resampling approach is because the actual threshold values that will be used to fit the GPD is not known a priori. The resampling approach will provide a reasonable transition of flood levels between 'flood periods' and 'low water level periods' compared to just using zero values and makes sure reasonable flood level estimates will be used for flood probability estimation.

Specific comment 25

L412, Are you simulating also a fraction only of the low water level and then using such a short simulation to fill a longer part of the time series? Please, explain better.

Response:

This is correct. The following comments is added:

"In other words, only a fraction of the low water level periods is simulated and resampling with replacement is used to fill in flood data across the entire low water level periods."

Specific comment 26

L 426, This is shorter than then 31years, which correspond to about 271560hours. I would highlight this explicitly as it is relevant as you implicitly suggest.

Response:

Thank you for this suggestion. The following comment is added:

"…, which is approximately 10% the entire 31 year period under consideration."

Specific comment 27

L 436, “conditions”, refer to variables to guide the reader (storm tide and river discharge)

Response:

Thank you for the suggestion. The following wording has been added:

“i.e. flood drivers, such as storm tide and river discharge”

Specific comment 28

L437, Introduce the “grid” or make it clear what the grid is here in this context.

Response:

The following wording has been added to improve clarity:

“e.g. 10 by 10 flood events generated from combinations of flood drivers with different return levels”

Specific comment 29

L 442, “250 years”, what return period? The univariate of the individual drivers? This is unclear. Also, you may need to clarify what type of data are you using for the boundary conditions. You seem to have 22 years of data of storm tide only, how did you estimate the 250 year return period without massive uncertainties?

Response:

The return period has been changed to “1 in 250 years”. As indicated in the sentence, the return period refers to that of flood drivers (i.e. univariate).

As described in both section 2.4 and section 4.3, Method 3 under Approach 3 is event based and no continuous flood data are used. There are in total 28 flood events are used. These events have univariate flood drivers with return periods up to 1 in 250 years, in order to estimate compound flood levels up to 1 in 100 year return period. A summary of these flood events is provided in the supporting material. The locations of these 28 event in the flood surfaces are indicated by the black dots in Figure 28.

The 22 years of data are observed water level data at a tide gauge near location Sw10. They are used for Method 1 only. Additional comment has been added in section 5.1 on results from Method 1 to emphasise this.

“The confidence intervals become increasingly wide with increasing return period, and it is important to note that return periods have been calculated based on only 22 years of historical water level data.”

Specific comment 30

L 445, The dependence between the drivers within the 28 events? Please, clarify.

Response:

The dependence here refers to the dependence between the flood drivers, which are not estimated using the 28 flood events. The dependence structure between the flood divers can be estimated using observed or simulated data. In this study, it was estimated using observed flood driver data. The following comment has been added to improve clarity.

"For the case study, the dependence between flood drivers are estimated using observed data of storm tide and river discharge."

Specific comment 31

L458, I see that you use MIKE21 for Methods 1 and MIKE FLOOD for method 3. Can this be responsible for the differences in the results based on the two methods? Please, discuss.

Response:

Sorry for the confusion. MIKE21 is one module in MIKEFLOOD. The same hydrodynamic model is used for Method 2 and Method 3. MIKEFLOOD has been changed to MIKE21 in the manuscript, except in Acknowledgements, where MIKEFLOOD license is mentioned.

Specific comment 32

L 459, step 1 was not introduced formally.

Response:

Section 4.3 on Method 3 has been revised to improve clarity. The writing in this section has been changed.

Specific comment 33

L460, this small paragraph is not clear to me. Please, explain better for people who are not familiar with the method.

Response:

This is the final step of estimating flood probability by integrating the generated flood surface in step 3 and estimated dependence model in step 2 using an integration method. The details of the integration method are out of the scope of this paper. A reference has been provided for readers who are interested in the details. In addition, this paragraph has been rewritten to improve clarity.

"In the fourth and final step, the probability of different compound flood levels simulated in Step 3 can be derived based on the bivariate dependence model developed in Step 2 using the bivariate integration method introduced by Zheng (2015a). More details of this integration method can be found in Zheng et al. (2015b)."

Specific comment 34

472, In the methods, please mention how you retrieved the uncertainties in the estimate (based on the uncertainties in the fitted parameters).

Response:

The following phrase has been added to improve clarity.

"…(estimated using a bootstrap method)"

Specific comment 35

488, I suggest highlighting that this location was used in Method 1 (so to allow for a comparison).

Response:

Thank you for the suggestion. "(i.e. this is where the results of Method 1 and Method 2 can be directly compared)" has been added.

Specific comment 36

L 498, Aren't you also for Method 2 using the MRL plot applied to the H water level? Please specify if not done already and include a discussion on this within this sentence (one may expect that MRL to define a thresholds such result in similar uncertainties at all locations.

Response:

It is correct that MRL plots are used in Method 2. This part of discussion is still under Method 2.

Specific comment 37

L 509, Why do you use the values maximising the dependence? Understanding when the dependence is maximised provides interesting information on the physical system, however, the dependence values that are relevant from a point of view of the impact is that between the variables at the same time. In fact, the storm tide and the river flow interact at the same time in the real world.

Response:

This is because in Method 3 the information on the temporal dynamics (i.e. relative timing) of storm surges and astronomical tides is discarded and only the peaks of flood drivers are considered via the use of a static tail water level, as discussed in section 2.4. This is one of the limitations of Approach 3 and thus, Method 3. The following statement has been added to improve clarity:

"This is because in this method the information on the temporal dynamics of storm surges and astronomical tides is discarded and only the only the peaks of flood drivers and their joint dependence are considered, as discussed in section 2.4."

Specific comment 38

Fig 8, The 2D simulations receive as input time series of T and Q, therefore a question arises: which is the value of the time series that you consider as that to be reported on the x and y axes?

The plots, e.g., panel c, suggests that for a given 10 year return level of Q, when T becomes larger (from 0 to 1-year return period), H decreases. This is physically inconsistent. Such inconsistent behaviours seem to occur in the range of T AND Q below 1-year return levels. Do you have an explanation for that? If the explanation is convincing, one would then consider not showing values in this bivariate range (up to 1-year return level for both variables).

Response:

This variation is potentially caused by the interpolation method used. Additional discussion has been added in the revised manuscript to explain this.

"It can also be observed in **Error! Reference source not found.** that there are some variations in estimates of flood levels with very short return periods (e.g. return periods of 1 in 1 year or below), with the increase in one flood driver leading to decreased compound flood levels. Careful inspection of the results shows that this feature does not apply to any of the simulated data points, in the sense that simulation points with larger values of the boundary conditions always yield larger flood levels. Rather, the 'inflection' only occurs in a sparsely sampled region of the plot, and is thus suggestive of the limitations of using a log-linear interpolation scheme in this region. This therefore highlights the importance of carefully considering the sampling scheme as part of the analysis."

Specific comment 39

L 524, How do you estimate the case of complete dependence/independent variables? I understand that you get the water level based on 2d simulation with input T and Q observed time series. If they were not time series, I could see the concept of independence, but in this context, I find it unclear. This comment is related to that on the general explanation of method 3.

Response:

The case of complete dependence/independence can be estimated by using different alpha values representing complete dependence (i.e. alpha = 0) and complete impendence (i.e. alpha = 1). This is discussed in section 4.3. Additional reference to section 4.3 has been added here to improve clarity.

Specific comment 40

L 550 I would reverse the sentence, highlighting the result based on method 2 and 3 compared to 1, the observation-based method. Hence, "2 and 3 lead to lower estimates than 1…"

Response:

Thank you for this suggestion. As this part focuses on results of Method 1, the way the results are discussed is not changed.

Specific comment 41

L 565 can the comparison be affected also by the difference model type used in method 2 and 3?

Response:

The same model is used for Method 2 and Method 3. See response to Specific Comment 31 above.

Specific comment 42

Table 2,

Disadvantages for method 2, "Difficult to assess future conditions…": Why isn't this the case also for case 3?

The advantage for method 3 about future conditions: This does not seem to me as simple as stated. Please discuss. By the way, also changes in the marginals should be included, which appears to be the most relevant for future changes and at least the change for which we have the highest confidence (the confidence on the changes in the dependence is small). I would suggest discussing this taking into account the following papers (at least):

– About changes in the dependence: Wahl et al. (Nature Climate Change)highlights a

change in the dependence in the past, Bevacque et al (https://eartharxiv.org/repository/view/293/) highlights the changes in the dependence are uncertain, and Ganguli and Merz (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019GL084220) also discuss changes in the dependence for the past.

– Moftakhari et al.,(PNAS) and Bevacque et al (above) about projected changes in the marginals (i.e. Storm surge, precipitation, and sea level rise).

Response:

For response to the comment on advantages of Method 3, please refer to response to General Comment 3 above.

In addition, the end of discussion on Approach 2 in section 6 has been revised to:

"Although, these high-resolution and temporally consistent data are at present not widely available under future climate scenarios, they can potentially be developed in the future allowing Approach 2 to be used to assess compound flood probability under future changes."

The following paper has been added in the revised manuscript.

Ganguli, P. and Merz, B.: Trends in Compound Flooding in Northwestern Europe During 1901–2014, Geophysical Research Letters, 46, 10810-10820, 2019.

The Bevacque et al. paper (https://eartharxiv.org/repository/view/293/) is not peer reviewed yet and is therefore not included.

Specific comment 43

L 590 “stationarity”, add “in the estuarine characteristics”. You are referring not to the meteorological conditions here, so make it clear, please.

Response:

Thank you for this suggestion. It has been changed.

Specific comment 44

L598 Could you clarify/discuss why should accounting for the dependence explicitly be an advantage (compared to method 1)? Thanks.

Response:

By accounting for dependence implicitly, the method has one less variables to estimate, which simplifies the estimation process and reduces chance of introducing additional error/uncertainty.

Specific comment 45

L602, Personally, I would add something along this line. “incorporated in the modelling framework”, add: “through considering the most recent bathymetry characteristic of the estuary when interested in the present-day estimate of the flooding probability”.

Response:

There are multiple ways long-term driver data can be incorporated. The authors believe that it is better to leave it open rather than suggesting a specific way how the data are incorporated. For example, in order to address future changes, future projections of changes in the estuarine regions need to be used. Therefore, no changes have been made here.

Specific comment 46

L 609, There is high-resolution data of sea level (storm surge/waves) and precipitation available, though I understand that especially for sea level, these are rare and in general can be uncertain. There are climate models. How can they be used to solve the issue? The fact that data is not widely available at high resolution does not mean, I think, that this is something to negatively judge this method given that I am not sure about what would a better alternative be.

Response:

The statement referred to is “These high-resolution and temporally consistent data are at present not widely available under future climate scenarios.”

This statements here simply points out the fact the high resolution data required for this method may not be readily available. There is no judging of this method involved.

Specific comment 47

L612, “updating”, Are you referring to update with respect to changes due to climate change? If not, please discuss the climate change issue, as this is done in the other two cases. If yes, please clarify. In addition, I do not understand how you would estimate the changes in the dependence. Do not we have the same issue as in method 2? Also, we have the problem that we need to estimate changes in the marginals, not only in the dependence. See comments above regarding this topic too.

Response:

The dependence model includes the marginal distributions for individual flood drivers and the dependence structure, and can be estimated under future conditions, considering but not limited to climate change. The advantage of method 3 is that the hydrodynamic runs required to produce the flood surface will not need to be repeated. For detailed response, please refer to response to General Comment 3.

Specific comment 48

L649, See comment above about climate change. This needs to be discussed carefully.

Response:

See response to Specific Comment 47 and General Comment 3 above.

Specific comment 49

L 655, “Implementation of each approach available” -> “approaches available”

Response:

This sentence emphasis that there are multiple implementations of each approach available. Therefore, it has not been changed.