nature portfolio

Peer Review File

Overlooking probabilistic mapping renders urban flood risk management inequitable

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

23rd Mar 23

Dear Professor Bodoque,

Your manuscript titled "Overlooking probabilistic mapping potentially renders urban flood risk management inequitable" has now been seen by 2 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We hope you will find the reviewers' comments useful as you decide how to proceed. For publication of a revised manuscript in Communications Earth & Environment to be appropriate, we would need you to:
• Discuss the effects of roughness on flood modeling in more detail.
• Improve the communication of methods, notably convergence analysis, and explanation of results based on Reviewer’s #2 comments.
• Provide a detailed discussion of the calibration and assessment of the models.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees’ comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren’t able to submit it within this time so that we can discuss how best to proceed. If we don’t hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Rahim Barzegar, PhD
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Communications Earth & Environment
orcid.org/0000-0002-1941-2991

Joe Aslin
Senior Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
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All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
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If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Overlooking probabilistic mapping potentially renders urban flood risk management inequitable

The paper provides a comparison of Probabilistic versus Deterministic modelling approaches in determining urban flood risk. The paper provides a strong rationale indicating the significant economic damage caused annually by urban flooding. Improved approaches that can accurately map flood risk are needed. The study provides a strong spatial analysis of flood risk, and identifies factors such has spatial roughness coefficient and flood quantiles introducing the greatest levels of uncertainty.

As roughness appears to be one of the key sources of uncertainty, I would like to see more discussion in the paper considering roughness parametrisation and spatial roughness determination. An old study Chow (1959) is used to parametrise the roughness in the modelling. This may well be a benchmark paper, but I think the authors perhaps should be using more novel approaches to characterise the roughness spatially. How is Mannings n roughness attained for urban areas with complex structures and buildings? What about forested riparian areas with different vegetation heights, structures and flexibility in flowing water? How is Mannings n varied with flood depth in these forested areas, or other vertical objects (buildings) for that matter (see Antonarakis and Milan, 2020)?

A 500 year RI flood is simulated. I think the authors should be clear that there is also limited confidence in using past flow records to estimate recurrence intervals due to no-stationarity in the

discharge record related to climatic trends and perturbations (e.g. Chen et al., 2021). Indeed, more could possibly be said considering potential future flood risk scenarios influenced by climate change.

I suggest publication after minor revision.

Other points:
Line 17 Is this a US directive - international audience may not know
Line 30, I 120 billion USD a global figure?
Line 83, 315, 398, How is the 500 yr flood estimated? what about non-stationarity in historic records?
Line 86, a study location map should surely be referred to here
Line 150/51, it would be intyerest8ingt to know exactly how roughness was estimated and also whether it was spatially distributed or uniform?
Line 178, spelling: “ware”? should this be “were”?
Line 243, Fig. 5 , It is not clear what the pe charts are showing. More explanation needed.
Line 290, remove the phrase “meander-like channels”, suggest using :”the river exhibits a meandering morphology” may be worth stating further whether the channel is alluvial, or bedrock influenced, and also something about the hydrological regime. To what degree is the flood hazard influenced by flow regime over the hydrological year and wat factors affect this, e.g. snowmelt, drought, landuse, flow regulation etc. What is the hydrograph character is it flashy? This point is potentially significant when considering risk, for example the flashiness of the hydrograph.
Line 303, a 1-m bathymetry DEM is probably quite unusual to have. How was this data acquired?
Line 307/8, was the model validated against real field measurements?
Line 324, more information needed on the Mannings n roughness coefficient. What values used and was spatial variability considered? I do express concern that Chow 1959 is being used to parameterise Mannings n. this is a potential flaw, as one of the major factors inducing uncertainty in model outputs is variability in roughness.
Line 373, Can the authors refer to other studies that have accounted for spatial error in DSMs (e.g. Milan et al., 2011)? How does the approach use here compare?
Line 389, Chow (1959), seems quite dated – considering roughness is concluded to be one of the main uncertainties in the modelling.
Line 672, should units not be m/sec on the Fig legend S1 2?
Line 711, should the inset graphs have axes labelled and units in the figure S1 8?

Suggested references:
Antonarakis, A.S. and Milan, D.J., 2020. Uncertainty in parameterizing floodplain forest friction for natural flood management, using remote sensing. Remote Sensing, 12(11), p.1799.
Chen, M., Papadikis, K. and Jun, C., 2021. An investigation on the non-stationarity of flood frequency across the UK. Journal of Hydrology, 597, p.126309.
Milan, D.J., Heritage, G.L., Large, A.R. and Fuller, I.C., 2011. Filtering spatial error from DEMs: Implications for morphological change estimation. Geomorphology, 125(1), pp.160-171.

Reviewer #2 (Remarks to the Author):

This manuscript focuses upon a very important and significant component of numerical flood

modelling: the uncertainty associated with predictions that are typically presented as “crisp” or in/out of the flood zone polygons, in both scientific research and flood risk management applications. I agree with the authors that the implications of their findings are societally significant, since practice that incorporates uncertainty is less likely to cause inequality due to the imprecision of model predictions not being characterised. The investigation appears to have been undertaken with rigour but there are issues with the communication of the methods, particularly convergence analysis, and the explanation of results where the prose needs to be more precise when using some technical terms that may not be understood by all readers and/or mis-interpreted by readers with different backgrounds. Some figures (5 and 6) need to be improved to enable clearer communication of results. Overall, I recommend major revision.

- “Potentially” in title – I find this rather cautious; a clearer, more precise assertion is needed in the title
- Can you contextualise the need for this approach, and it’s implications, in an international policy/practise context that extends beyond European legislation/practice?
- The authors focus upon a single case study, whilst this is acceptable for the scope of the paper, the conclusions that are drawn in terms of the impact of the uncertainty analysis need to be contextualised further within the topography of the case study; is the case study typical of a topographically confined, partly confined, or unconfined valley bottom and associated floodplain/s (see L209 where topographic shape is discussed)?
- There is a significant emphasis on “model convergence”. However, this concept is not clearly described in either the abstract, or main body, which results in this approach being confusing for the reader. Indeed, although my expertise is in numerical modelling I have not previously seen this application in flood modelling and whilst I was able to delve into the methods section, the manuscript’s approach to communicating this key component of the methodology needs to be much clearer.
- Similar to the above comment, more detail is needed on the central limit theorem (L93) within the main body of the manuscript.
- L100 Are flood quantiles a parameter? I’d argue they’re an output. I think outputs / variables / parameters are also incorrect / unclear / mis-classified elsewhere e.g. L115, L126.
- L102-3 “stochastic model converged” – this needs to be explained and quantified, potentially linking to the methods
- L111 – “Quantification of convergence” – Too much prior knowledge assumed here
- L136 – Urban “centres” or urban “objects?”
- L169 – delete “a”
- L178 – use “being” not “were”
- There are some awkward / complex / unclear sentences and undefined (and potential unclear) concepts that would benefit from rewording, this is especially pertinent in the results section e.g. L32-34, L125-6 (what does “locally” mean here?), L134 (what does “not well defined geometrically mean here; can it be quantified?), L126 (doubtful variable) L127 (“questionable variable”), L128 (“flood quantile)
- Section 5 - What are the implications for how we calibrate and assess models from observed data? I think this is really important and not focused upon.
- L264. How are these results communicated? This is key at the social scale? But many users of maps may not know what to do with uncertainty. I think Figure 6 needs to be improved – I can’t really understand what the legend means!
- Methods. What engine was used in HEC-RAS (SWE?) and what turbulence closure was used? I found Figure 7 difficult to follow, it needs to be improved to communicate the workflow more

clearly. L357 “true positives” – are these between the deterministic and probabilistic? L375 – How many points / what’s the distribution? How does resolution relate to topographic variation?

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Dear Reviewers,

We are submitting a revised copy of our manuscript, "Overlooking probabilistic mapping potentially renders urban flood risk management inequitable" (ID: COMMSENV-23-0147A). We greatly appreciate your insightful comments on our manuscript, and we have considered all of your suggestions. To facilitate the review, we have modified the manuscript by highlighting all changes in blue. Regarding reviewer 1's comments, we have explained why we used the approach proposed by Chow (1959) to characterize roughness, but we also highlight the desirability of applying more robust methods in order to reduce uncertainty in roughness characterization, especially in river channels and riparian areas. In accordance with reviewer 2, we have described the concept of "convergence" in flood modeling and gone into greater detail about what its analysis entails. We have also revised the wording of the sections of the manuscript noted by reviewer 2 to make them easier for readers to grasp. In accordance with reviewer 2, we have revised figures 5, 6, and 7 as well as the accompanying figure captions to make them more self-explanatory. Likewise, we also answer here the question raised by reviewer 2 about the calibration of our model. Finally, a total of six new references have been added to the revised version of the manuscript.

Responses to the reviewer 1' comments				
No.	**Comment**	**Location in the submitted paper**	**Location in the revised paper**	**Amendment**
1	Line 17 Is this a US directive - international audience may not know	Page 1, Line 17	Page 1, Lines 9 and 16-17	We were referring to the European Floods Directive. To avoid misinterpretation, we have made this explicit in the text.
2	Line 30, I 120 billion USD a global figure?	Page 1, Line 30	Page 1, Lines 29 and 30	This figure was approximated on a global scale. This has been clarified in the text.
3	Line 83, 315, 398, How is the 500 yr flood estimated? what about non-stationarity in historic records?	Page 3, Line 83; Page 13, Line 315; and page 17, Line 398	Page 13, Line 334; Page 30, Lines 733-748 and Page 31, Lines 749-766	We computed flood frequency analysis based on the Pearson type III distribution with a logarithmic transformation. For the estimation of the parameters of this distribution, the Expected Moments Algorithm (EMA) was employed based on the generalized method of moments procedure. Prior to using the approach described above, we statistically examined the data for conformity with the underlying assumptions (i.e., stationarity, homogeneity, and independence) when doing flood frequency analysis. The supplementary information to this publication offers a thorough description of the procedure used

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				(Supplementary SI 4: Page 30, Lines 733-748 and Page 31, Lines 749-766).
4	Line 86, a study location map should surely be referred to here	Page 3, Line 86	Page 3, Line 86; Page 12, Line 300	The location map of the study area is supplied in the manuscript's supplementary information section (Supplementary SI 1). As suggested by the reviewer, we have included a reference to this figure in the text.
5	Line 150/51, it would be interesting to know exactly how roughness was estimated and also whether it was spatially distributed or uniform?	Page 6, Lines 150-151	Page 17, Lines 421-429	Roughness was characterized spatially distributed by connecting the predominant land uses / land cover (obtained from CORINE Land Cover database) with the possible range of Manning's n values. See page 17, Lines 423-431, for more details.
6	Line 178, spelling: “ware”? should this be “were”?	Page 7, Line 178	Page 7, Line 186	We have corrected the typo detected by the reviewer.
7	Line 243, Fig. 5, It is not clear what the pe charts are showing. More explanation needed.	Page 10, Line 243	Page 10, Line 247	Figure 5 has been improved to be self-explanatory.
8	Line 290, remove the phrase “meander-like channels”, suggest using: "the river exhibits a meandering morphology” may be worth stating further whether the channel is alluvial, or bedrock influenced, and also something about the hydrological regime. To what degree is the flood hazard influenced by flow regime over the hydrological year and wat factors affect this, e.g. snowmelt, drought, landuse, flow regulation etc. What is the hydrograph character is it flashy? This point is potentially significant when considering risk, for example the flashiness of the hydrograph.	Page 12, Line 290	Page 12, Lines 302-311	We have accepted the correction proposed by the reviewer and, in agreement with him/her, have explained in greater detail the geomorphological characteristics and hydrological response of the examined reach of the Duero River.
9	Line 303, a 1-m bathymetry DEM is probably quite unusual to have. How was this data acquired?	Page 13, Line 303	Page 33, Lines 808-816; Page 34, Lines 817-825	The Digital Batimetric Model (DBM) was built by taking cross sections with a single-beam echosounder embedded in an aquatic drone, considering man-made structures and the geomorphological configuration of the reach. A survey for bathymetric error estimation was conducted using a single-beam echosounder, obtaining about 10,000 bathymetric points along the river reach under study. The supplementary information section has a more detailed description

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				of the approach employed (Supplementary SI 10: High resolution DBM -Page 33, Lines 808-816; Page 34, Lines 817-825).
10	Line 307/8, was the model validated against real field measurements?	Page 13, Lines 307-30		The model was not validated against real field measurements.
11	Line 324, more information needed on the Mannings n roughness coefficient. What values used and was spatial variability considered? I do express concern that Chow 1959 is being used to parameterise Mannings n. this is a potential flaw, as one of the major factors inducing uncertainty in model outputs is variability in roughness.	Page 14, Line 324	Page 7, Lines 178-183; Page 17, Lines 423-425; Page 24, Lines 642-645; Page 25, Lines 674-678	We used the method proposed by Chow in 1959 because it is widely utilized, both in the scientific literature (Ming et al., 2022, *Journal of Hydrology*; Kohanpur et al., 2023, *Water Resources Research*, are just two examples of studies published recently in top-leading journals) and at the applied level, being the method recommended by the community of practitioners dedicated to the study of floods in countries such as Japan (JSCE, 1989; *Japan Society of Civil Engineers*) or Spain (MMA, 2011; *Spanish Ministry of the Environment*). This method is popular due to its simplicity of application, as it is founded on look-up tables that relate Manning's n to land use/land cover. In our study, Manning's n values have been linked to the land use classifications published in the Corine Land Cover for the study area. The Manning's n values utilized were not those published by Chow in 1959 but rather those contained in the 2011 application guide published by the Spanish Ministry of Environment. In the revised version of the manuscript, the corresponding citation has been added. However, the results of our analysis indicate that roughness is a highly sensitive input data, particularly in areas with limited data, such as the river channel. In the results and discussion section, we have emphasized, as also claimed by the reviewer, the benefit of utilizing more rigorous methods to estimate roughness, particularly in the river channel and riparian areas.
12	Line 373, Can the authors refer to other studies that have accounted for spatial error in DSMs (e.g. Milan et al., 2011)? How does the approach use here compare?	Page 16, Line 373	Page 16, Lines 403-405	In agreement with the reviewer, we alluded to previous authors who have considered spatial error while developing DSMs, placing their work in context with the approach we adopted in our research.

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13	Line 389, Chow (1959), seems quite dated – considering roughness is concluded to be one of the main uncertainties in the modelling.	Page 17, Line 389		See amendment to comment 11.
14	Line 672, should units not be m/sec on the Fig legend S1 2?	Page 28, Line 672	Page 30, Line 730	In accordance with the reviewer's request, we have amended the legend for figure SI 3 (current version of the manuscript).
15	Line 711, should the inset graphs have axes labelled and units in the figure S1 8?	Page 30, Line 711	Page 33, Line 806-807	Figure SI 9 (current version of the manuscript) has been improved as required by the reviewer.

Responses to the reviewer 2' comments				
No.	**Comment**	**Location in the submitted paper**	**Location in the revised paper**	**Amendment**
1	"Potentially" in title – I find this rather cautious; a clearer, more precise assertion is needed in the title	Page 1, Line 1; Page 26, Line 666	Page 1, Line 1; Page 27, Line 722	In accordance with the reviewer's request, the paper's title has been modified.
2	Can you contextualise the need for this approach, and it's implications, in an international policy/practise context that extends beyond European legislation/practice?		Page 2, Lines 41-53	The approach provided in this research is valuable for improving the implementation of the European Floods Directive as well as any other national or international strategy or policy in which risk reduction is supported by flood hazard mapping. The National Flood Insurance Program in the United States is another instance that illustrates this approach (see Stephens and Blesoe, 2020, *Anthropocene*). The above is expanded upon in the introduction part of the manuscript (see Page 2, Lines 41–53).
3	The authors focus upon a single case study, whilst this is acceptable for the scope of the paper, the conclusions that are drawn in terms of the impact of the uncertainty analysis need to be contextualised further within the topography of the case study; is the case study typical of a topographically confined, partly confined, or unconfined valley bottom and associated floodplain/s	Page 9, Line 209	Page 9, Line 214	We have placed the geomorphology of the studied river-floodplain system into context, as suggested by the reviewer.

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	(see L209 where topographic shape is discussed)?			
4	There is a significant emphasis on "model convergence". However, this concept is not clearly described in either the abstract, or main body, which results in this approach being confusing for the reader. Indeed, although my expertise is in numerical modelling I have not previously seen this application in flood modelling and whilst I was able to delve into the methods section, the manuscript's approach to communicating this key component of the methodology needs to be much clearer. - Similar to the above comment, more detail is needed on the central limit theorem (L93) within the main body of the manuscript.		Page 19, Lines 491-496	We have expanded the section on convergence in the materials and methods section to make it easier to understand. Essentially, in the approach deployed in this manuscript, we analyzed the convergence of the Monte Carlo-based ensemble's first moment for the outputs flooded area, water depth, and flow velocity, and we devised a criterion to consistently end the Monte Carlo process. The proposed criterion is based on the Central Limit Theorem –CLT (see Yang, 2011, *Environmental Modelling & Software*), which seeks a band of a given width (provided by the maximum acceptable variance for the output of interest) and length (the minimum number of simulations) such that the probability of the Monte Carlo samples falling outside of this band is virtually null.
5	L100 Are flood quantiles a parameter? I'd argue they're an output. I think outputs / variables / parameters are also incorrect / unclear / mis-classified elsewhere e.g. L115, L126.	Page 4, Line 100; Page 4, Line 115; Page 4, Line 126	Page 2, Line 45; Page 4, Lines 99, 115; Page 7, Lines 165-167 and Line 195; Page 9, Line 215; Page 11, Line 262; Page 13, Line 321; Page 14, Line 349; Page 15, Line 353; Page 16, Lines 394, 397; Page 19, Line 478; Page 20, Lines 503, 506; Page 33, Lines 801-802	The reviewer is right when claims the term "parameter" is inconsistently used across the manuscript. We thoroughly reviewed it, and we corrected it when necessary.

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6	L102-3 "stochastic model converged" – this needs to be explained and quantified, potentially linking to the methods		Page 19, Lines 491-496	See amendments to the reviewer comments 4 and 7.
7	L111 – "Quantification of convergence" – Too much prior knowledge assumed here		Page 19, Lines 491-496	Typically, the stability of the computed moments is evaluated by plotting the sample moments of the target variable (e.g., flooded area) and evaluating them based on points as a function of the number of Monte Carlo simulations executed. These graphs are usually displayed on a natural scale, and the qualitative convergence of results is determined by whether the sample moment curve has reached a more or less flat shape. The confidence limit theorem –CLT (see Yang, 2011, *Environmental Modelling & Software*) is used in our approach to quantitatively determine when the computed moment is stable, based on finding a convergence band of such width and length that the probability of the Monte Carlo sample mean falling outside this band is negligible.
8	L136 – Urban "centres" or urban "objects?"	Page 5, Line 136	Page 5, Line 135	To avoid misinterpretation, we have replaced "urban centres" to "urban areas".
9	L169 – delete "a"	Page 6, Line 169	Page 7, Line 169	Done.
10	L178 – use "being" not "were"	Page 7, Line 178	Page 7, Line 186	Done.
11	There are some awkward / complex / unclear sentences and undefined (and potential unclear) concepts that would benefit from rewording, this is especially pertinent in the results section e.g. L32-34, L125-6 (what does "locally" mean here?), L134 (what does "not well defined geometrically mean here; can it be quantified?), L126 (doubtful variable) L127 ("questionable variable"), L128 ("flood quantile)	Page 2, Lines 32-34; Page 4, Line 126; Page 5, Line 127.	Page 2, Lines 32-34; Page 4, Lines 125-128; Page 5, Line 133-134.	The reviewer is right when claims that some parts of the manuscript are unclear. We have redrafted them, aiming to facilitate reading and understanding. In lines 125-126, "locally" means at the cell level. Line 126 ("doubtful variable") and line 127 ("questionable variable") means that the referred hydraulic outputs did not converge. Moreover, we have deleted the text "not well defined geometrically" as it is redundant. It refers to the natural and man-made features mentioned on lines 132–133 of the revised manuscript.
12	Section 5 - What are the implications for how we calibrate and assess models from			We recognize the significance of calibrating flood maps to reduce their uncertainty. In the first phase of this research, we considered two options: i) calibrating our model for the 500-year flood using

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	observed data? I think this is really important and not focused upon.			satellite-derived flood maps (e.g., Baldassarre et al., 2009, *Journal of Hydrology*); and ii) using point observations to infer flood extent (e.g., Brázdil et al., 2006, *Hydrological Sciences Journal*). In our study, we did not calibrate our model using remote sensing because no events representative of the 500-year flood were captured in the available time series of imagery. Regarding point observations, the city of Zamora provides flood marks from the second part of the nineteenth century and the early twentieth century. However, their use to calibrate our model of the 500-year flood is highly questionable given that the current bathymetric configuration of the studied river reach has nothing to do with the bathymetry that existed in the latter half of the 19th century and the early 20th century, as the river bed is alluvial in nature.
13	L264. How are these results communicated? This is key at the social scale? But many users of maps may not know what to do with uncertainty. I think Figure 6 needs to be improved – I can't really understand what the legend means!	Page 11, Line 264	Page 12, Lines 291-297	To make figure 6 understandable, we have provided a more detailed explanation of its caption.
14	What engine was used in HEC-RAS (SWE?) and what turbulence closure was used?		Page 13, Line 319	We used diffusion wave equations. On the other hand, we have not examined turbulence, as our analysis has been focused on the floodplain area flooded by the 500-year flood.
15	I found Figure 7 difficult to follow, it needs to be improved to communicate the workflow more clearly	Page 14, Line 318	Page 14, Lines 339-342	Figure 7 has been modified to make it easier to understand. Moreover, we have provided a more detailed explanation of its caption.
16	L357 "true positives" – are these between the deterministic and probabilistic?	Page 15, Line 357	Page 16, Lines 381-387	We have clarified in the text the meaning of "true positives", "true negatives", "false positives" and "false negatives".
17	L375 – How many points / what's the distribution? How does resolution relate to topographic variation?	Page 16, Line 375	Page 33, Lines 808-816 and page 34 Lines 817-835	The High-Resolution Digital Model (HRDM) was obtained by coupling the Digital Surface Model (DSM) with the Digital Bathymetric model (DBM). To estimate the error in the DSM, we conducted a digital global positioning system (DGPS) survey that

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				delivered 3,412 elevation control points. These control points were largely placed in urban areas, but also in periurban zones and the entire floodplain. Regarding the DBM, a survey for bathymetric error assessment was conducted using a single-beam echosounder device embedded in an aquatic drone, which allowed us to gather over 10,000 bathymetric control points along the studied river reach. A more detailed explanation of the procedure followed can be found in the supplementary information section (Supplementary SI 10: High resolution DBM and Supplementary SI 11: High resolution DSM, Page 33, Lines 808-816 and page 34 Lines 817-835.

6th Jun 23

Dear Professor Bodoque,

Your manuscript titled "Overlooking probabilistic mapping potentially renders urban flood risk management inequitable" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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Best regards,

Rahim Barzegar
Editorial Board Member
Communications Earth & Environment

Joe Aslin
Senior Editor,
Communications Earth & Environment
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I am looked through the suggestions/edits that reccomended in my initial review, and I am satisfied with the authors response. I think the manuscript has been improved and would recommend publication.

Reviewer #2 (Remarks to the Author):

I have reviewed the author's table and the revised manuscript. They have undertaken a thorough revision of the manuscript and justified their choices when they have chosen not to modify the manuscript. I recommend that this is accepted.