RESPONSE TO REVIEW #1

We highly appreciate and are very thankful for the time and effort that was invested in reviewing our manuscript. Thank you for initiating this fruitful discussion. After carefully studying the constructive queries and comments, and following lengthy discussions among the co-authors, we have thoroughly revised our manuscript in an attempt to refine our key motivation and messages: to derive a simple but reliable methodology for localizing urban inundation hotspots by means of a numerical model, which makes best use of open access (geo) data, and a new and easy-to-apply flood severity index. Please find our responses (blue) and revised text blocks (*blue, italic*) below each review comment (**black, bold**).

General Comments:

Part I:

The authors present some kind of methodology to build a 2D hydraulic model based on freely available data. The objective is praiseworthy but since these data appears to be of relatively low quality and having in mind the sensitivity of a hydraulic model to the DEM for flood simulation, it seems pointless, even dangerous. Indeed, an urban flood model have to be of high quality (DEM, hydraulic calibration and validation) having in mind the repercussion of modeling results. The authors present an interesting discussion on the DEM uncertainties based on freely available data. They should try to propagate these uncertainties using the numerical model; it could lead to any kind of results. Most institutions or insurance companies will use flood maps provided by scientists as a truth. If the model is not properly calibrated nor validated, it may lead to very problematic situation for people living in these areas. Clearly here, for the case of the River Sai Gon next to Ho-Chi-Minh-City (very flat system largely influenced by tide, complex system of canals, heavy rains, etc.), a numerical model of the city needs data of much better quality for the construction and validation of the model. Is it reasonable to have a DEM resolution of 30 m or more with a vertical uncertainty up to 1 m to build a 2D numerical model? Eventually, the proposed model is not really calibrated nor validated.

We are thankful for receiving this constructive feedback and are reassured in our motivation to disseminate our findings, given that the reviewer sees the objective as praiseworthy, too. This opinion confirms the added value of communicating the presented methodology for building urban surface runoff models based on open-access data to a wider audience. Upon carefully examining this general comment, we came to the conclusion that the purpose of our methodology was not communicated as clearly as intended. To clarify our overarching motivation, changes were made to the manuscript starting off with the title which now reads: *"Uncovering Inundation Hotspots through a Normalized Flood Severity Index: The Potential of Open-access Data for Flood Estimations in Ho Chi Minh City, Vietnam"*

This should reduce the misleading impression that open-access models could be the "be-all and end-all" instrument for producing highly accurate flood maps. In contrast, the title now emphasizes the inherent uncertainty and limitations introduced by using open-access data for this purpose. Furthermore, the abstract was modified to better reflect the main objective of the presented work and now reads as follows (ll. 18-21):

"(…) To help alleviate this problem, this paper explores the usability and reliability of flood models built on open-access data in regions where highly-resolved (geo)data (e.g., from LiDAR campaigns, bathymetric surveys or hydrological data acquisition) are either unavailable or difficult to access, yet evaluation of risk from flooding is crucial. To that end, the example of Ho Chi Minh City, Vietnam, is taken to describe a comprehensive methodology for obtaining, processing and applying the necessary open-access data (topography, bathymetry, tidal water level, river flow and precipitation time series) to the fullest. The goal is to produce preliminary flood maps that provide first insights and estimations about potential flooding hotspots that demand closer attention in subsequent, more detailed flood risk analyses. As a key novelty of the paper, a normalized flood severity index (I_{NFS}) that combines flood depth and flood duration is proposed. The index serves as an indicator that further narrows down the focus to areas of significant flooding. It helps to uncover elements at risk, where particular scientific or practical attention is needed, be it in terms of precautionary relief efforts or training to prepare in advance to cope with flood risks (…)"

Furthermore, the wording of the introduction (ll. 45-46) has also been modified to better articulate our intentions regarding the cost and time intensive character of data acquisition and processing of on-site procured high resolution data. It now reads as follows:

"(…) which complicates numerical studies, especially for independent parties. Furthermore, when made available, not only are these data sets prohibitively costly, but they also often lack the necessary spatial and temporal coverage needed for proper derivation of boundary conditions and model set-up."

These amendments to the manuscript should make it clearer for any reader that the open-access data flood model of HCMC does not promise to deliver results that can be considered as truth, but rather estimations that open up opportunities to gain insights for subsequent decision-making processes regarding more detailed modeling for critical areas. The presented methodology can also be seen as an orientation for city planners and authorities from the developing world, helping them to readily estimate where hotspots with particularly high damage potential are located in a first flood risk assessment. Furthermore, it is not uncommon to find regional flood studies of HCMC that rely on coarse terrain data. For example, Scussolini et al. (2017) used a terrain mesh that ranged from 100 m to 500 m for their regional flood model, while Nhut Duy et al. (2019) relied on a 1-D model with 1000 data points on a 15 m grid for the river network and 28600 points on a 15 m grid for built-up areas. Undoubtedly, Progress has been made in flood modeling in recent years, yet highly resolved (geo)data is neither readily available nor always accessible to independent users. The presented manuscript deals with those situations, where highly resolved data is missing or inaccessible.

Lastly, we would like to address the impression of the reviewer that the model was not properly calibrated nor validated. In our opinion, this comment is not fully justified but we acknowledge the lack of emphasis in the presentation of calibration results and therefore added the following table to section 2.1.3 to avoid any misunderstanding:

Model calibration for different Manning friction coefficients focusing on reported inundations during three rain events (left column) and corresponding RMSE, NSE and PBIAS values

	n = 0.08 s/m$^{1/3}$			*n = 0.10 s/m$^{1/3}$*			*n = 0.12 s/m$^{1/3}$*		
Calibration Events	*RMSE*	*NSE*	*PBIAS*	*RMSE*	*NSE*	*PBIAS*	*RMSE*	*NSE*	*PBIAS*
Event 1 Date: 01/07/2010 P = 79 mm HWL = 1.10 m 23 Observations	*0.02*	*-5.25*	*37.5*	*0.01*	*0.50*	*5*	*0.02*	*-1.75*	*-25.6*
Event 2 Date: 09/07/2012 P = 58 mm HWL = 1.12 m 19 Observations	*0.03*	*0.14*	*21.4*	*0.02*	*0.64*	*10.7*	*0.03*	*0.29*	*-15.3*
Event 3 Date: 01/10/2012 P = 74 mm HWL = 1.15 m 18 Observations	*0.04*	*-3.23*	*33.7*	*0.03*	*0.52*	*6.2*	*0.05*	*-1.42*	*-17.9*

The table shows that a Manning friction coefficient of 0.10 s/m$^{1/3}$ does indeed provide the best results for all three statistical parameters. Albeit far from 100% accuracy, a NSE value of

0.5 to 0.64 is satisfactory for first flood estimates and is sufficient for a model whose goal is to determine inundation hotspots rather than quantitatively predict flood depths. This is especially valid when compared to the flood model by Hoa Binh et al. (2019) which relied on non-open-access 2 m resolution LiDAR data and still achieved NSE values of 0.51 to 0.89. Last but not least, our method of calibration is better designated for rain events as it relies on flood depths measured within the city and not on discharge and tidal gauges that are remote from the affected urban areas. Considering our results against the backdrop of comparable models, we are confident about the robustness of the presented approach.

Part II:

Results in Fig. 5 are correct but there are many unclear assumptions behind. And main results presented in Fig. 7 are quite poor.

We have made special efforts to edit Fig. 5 so that the assumptions are clearer. Furthermore, a table was composed, where flood depths observed at scattered locations and their corresponding simulated flood depths can be compared. The geolocations are numbered and supplemented by street names that are depicted on a map that can be part of the Supplementary Material. In regards to Figure 7, it is worth noting that this figure is meant to exemplify the application of the I_{NFS} by highlighting the differences between the maximum flood depth, inundation duration over threshold and their combination in the form of the I_{NFS}. Furthermore, this figure is best understood when simultaneously looking at the results in Table 4, which clearly highlight how accurate and trustworthy the I_{NFS} was in covering the locations of reported inundation as opposed to the other two flood indicators. Nevertheless, we edited the text to better define the goal of this section.

Part III:

In general, although the manuscript is well written, many technical details are missing. It is often difficult to understand how the bathymetry and boundary conditions are built.

Regarding the bathymetry, section 2.1.2 entitled “Bathymetric Data” is completely dedicated to explaining and discussing how the bathymetric data was acquired and integrated into our model. Admittedly, this methodology was proposed mainly because of the lack of comprehensive bathymetric data for the model area even after consultation with local partners. Unfortunately, it is not always the case that local institutions or authorities have knowledge about or are mandated to grant access to available geodata. Nevertheless, we developed an additional figure for the bathymetry that complements our original explanations.

Part IV:

The authors introduce a new index to evaluate the flood risk (normalized flood severity index), which can be interesting. However, they should verify if the normalization with a maximum value cannot bias the result in case of numerical divergence. Also, since the results of the model are quite poor, it appears difficult to validate the use of the index here. The index should be discussed for a case, which is much better described and a numerical model that is of higher quality.

We are very pleased that the reviewer regards the original idea and derived concept of the I_{NFS} as valuable, given that its proof-of-concept was one of the primary motivations for submitting this manuscript. Yet, we acknowledge the concerns regarding result bias due to false maximum values in case of numerical divergence. In fact, we thoroughly examined the simulation results in order to exclude any divergence, artifacts or outliers, which in our case were not found. Based on this comment, we decided to increase the robustness of the I_{NFS} against divergence and outliers by relying on quantiles of flood depth and duration for normalization. Through this method, the maximum flood depth is capped to the 95th quantile, keeping the value of the I_{NFS} between 0 and 100, while eliminating potential artifacts due to numerical divergence.

Part V:

Flood hazard assessment of pedestrian often combine water depth and flow velocity (Musolino et al., 2020). Since this criteria is based on results from a 2D model, it could be interesting to introduce a second index based on velocity and duration. Anyway, this part of the paper appears a little bit off-topic.

The combination of water depth and velocity is definitely an interesting prospect to determine risk to pedestrians in an urban environment and needs to be examined in more detail. The decision to neglect the velocity component from integration into the index was based on its negligible impact on flood damage modeling attempts (Amadio et al., 2019; Wagenaar et al., 2017, Kreibich et al., 2007) in low-elevation coastal zones (LECZ), where urban or rural terrain is rather flat, putting economic damage rather than pedestrian casualties in focus. This argument holds true when considering the high economic damage caused by less severe but much more frequent urban floods that Ho Chi Minh City regularly suffers from (ADB, 2010). Furthermore, the nature of the presented surface runoff model, where barriers such as buildings and vegetation cannot be easily represented, does not allow for the computation of peak flow velocities due to changes in cross-section. The proposed combination could be useful for a more detailed model for certain areas or districts where a surface elevation model with a fine resolution (5 m or lower) can be built. Through our proposed methodology, the areas of greater risk (hotspots) can be identified where more detailed simulations are worthwhile.

Minor Comments:

1- **L32: For a list of reference, use the chronological order**

Thank you for this comment. References with multiple entries were changed accordingly.

2- **L34: next decades, L40: skil "C.R", Fig 2. Use (a), (b), etc. Instead of (A), (B): 3×3 instead of 3x3 (times and not x-letter)**

Thank you for these corrections, the text was changed accordingly

3- **L75 (Figure 1) I do not see any step of calibration and validation of the model**

This is a valid point and we amended said figure accordingly.

4- **L79: What do you mean by "similar sources"?**

We have taken this comment into consideration and changed the wording of this sentence that now reads as follows:

"Generally, the search priority of terrain data, as well as hydro-meteorological data, follows the same path, with official sources at the top, followed by global repositories, peer reviewed literature, grey literature (i.e. publicly available reports and assessments), and finally regional and global models."

5- **L137: Please detail the characteristics of the LiDAR data**

A sentence was added that lists the corresponding characteristics.

6- **Tab. 2: An error of one meter for a DEM is huge! How accurate can you be for hydrodynamic calculations?**

We agree with the reviewer that differences of one meter are significant for a DEM, which is why special emphasis was put on the discussion of the differences in Section 4. However, the use of difference plots as described in 2.3.1 counteracts these uncertainties, which is confirmed by the model calibration and validation. Furthermore, it is important to measure the amplitude of the bias of the proposed DEM with regards to other open-access DEMs (SRTM, ALOS, ASTER, COPERNICUS). The positive bias of these traditional satellite DEMs can reach up to 13 m vis-à-vis the LiDAR data samples, rendering them completely unreliable for flood modeling purposes. This corroborates the conclusion made by Hawker et al. (2018) on the global scale in regards to the usability of the existing global DEMs. In this regard, the proposed DEM of this manuscript is far more reliable than any other open-access DEM and can confidently be used in preliminary flood estimations.

7- **In many countries such as in Vietnam, bathymetric data exist and could be obtained through collaborations or by paying for it**

Thank you for this comment. According to our knowledge and local networks, no open-access data exists for the Sai Gon River, while open-access bathymetric data for the Dong Nai River stems from US Army Corps of Engineers maps created in 1965 (Gugliotta et al., 2020). It is also correct that bathymetric data is available for sale. However, it is mostly provided in deep sections (e.g. river mouths) for transportation purposes and in hard-copy only. Accordingly, access and use of data from HCMC, if available, would be limited by commercial interests. This fact again underlines the need for the utilization of open-access data in flood modeling, which is overarching the objective of our manuscript.

8- **L158: Again, such data base provides very rough estimations of the bathymetry. How accurate will be the model using such data?**

We are pleased that the reviewer raises this point. This is exactly why we did the sensitivity analysis, whose results are presented in Section 3.2, showing that even a depth change of +80% of the river bed influences urban flood depths by only a few centimeters (7 to 12 cm).

9- **L172: What is the reference here? How do you set the bed level of the canal? Is this average depth a tidal-average depth?**

The canal depths are given relative to mean sea level. This detail was incorporated in the revised manuscript.

10- **L180: “expedient” is maybe a little bit strong. For the moment, the model construction seems very crude, especially for a complex and very flat system such**

as the Ho Chi Minh City Area

Thank you for this comment. We agree with the reviewer's opinion on the wording and have omitted the word "expedient" from line 180.

11- **Fig. 3: Please provide a proper figure caption and not a discussion of the figure. Also, most of the legend has no clear meaning (i.e. difference, exemplary colours, etc. ?). What do A, B and C red squares mean? I guess they correspond to the LiDAR samples**

Thank you for pointing out these mistakes and unclarities. We fixed the figure according to your comments to enhance its readability.

12- **L190: I'm not sure I understood. Are buildings represented as non-flowing area? Or is an equivalent Manning friction coefficient used to represent build effects on the average flow velocity?**

We agree that this sentence might have caused misinterpretations. Therefore, the sentence (ll 190-195) was adjusted and now reads as follows:

"Buildings and extensive vegetation that significantly reduce the available cross-section for water routing are not represented in the final DEM. Furthermore, given the 1 arc second spatial resolution, structural footprints of buildings cannot be represented as no-flow areas. Instead, an equivalent Manning friction coefficient was adjusted accordingly to compensate for no-flow areas that would otherwise be hydraulically misattributed."

:L195: The Manning coefficient has a unit; don't use the term "roughness coefficient" while talking about the Manning friction coefficient

Thank you for this valuable comment, the text was adjusted accordingly by substituting the term "roughness coefficient" with "Manning coefficient" and by adding its corresponding unit.

13- **L197: is a unique roughness coefficient used for the whole model? ($n=0.1\ s/m^{1/3}$)? What about canals and main channels (Sai Gon and Dong Nai Rivers)?**

We are aware that the wording might have caused misinterpretation in reference to the application of Manning friction coefficients. Therefore, the wording of this sentence was changed and now reads as follows:

"Following this approach, the best results are obtained for a Manning coefficient of 0.1 s/m1/3 uniformly applied across the whole modeling domain, (…)"

14- L222: The Sai Gon water discharge is mostly influenced by tide (Camenen et al., 2021). They provide some estimation of the net discharge for years 2017-2018

Thank you for sharing this reference. We have examined the given net discharges of 30 and 65 m3/s for the years 2017 and 2018, respectively, and are satisfied that our chosen net discharge of 54 m3/s, which is the long-term net discharge according to Trang Ngoc et al. (2016), falls within this range. Nevertheless, we now mention these additional values in our manuscript as part of Section 2.2.1.

15- L238: So, as far as I understood, you had access to Nha Be data

This is correct. We had access to both tidal water level data for Nha Be and rainfall data for the Tan Son Hoa rain station from our local partner. We used this data to critically evaluate the reliability of the open-access data and have specified the results of the comparison for both tidal water levels and rainfall data in Section 2.2.2 and 2.2.3.

16- L239: It would be interesting to present a plot showing these results

We found this idea very interesting as well and developed a graph that summarizes these results. Given that parts of this information are inherently contained in the publication by Gugliotta et al. (2017), we are deliberating, whether the illustration should be included in the revised manuscript or the Supplemental Material.

17- L255: variables in italic: *n*=28, Eq. 1; functions in roman: *n*=28; define all variables introduced in this equation, L263: The variable *n* is already introduced for a number of years, Eq. 2: this is not an equation; to be written within the text

Thank you for these comments, we have corrected the manuscript accordingly.

18- L260: α?

The value of α serves to indicate the goodness of fit of a theoretical distribution to the actual data as developed by Dyck (1980). This should show that the Gumbel distribution delivers a very good fit for the rain data set made available by NOAA for the Tan Son Hoa rain station.

19- **L269: Do you mean ß=0.854 for the Ho Chi Minh City area?**

Correct. We have added the reference Ho Chi Minh City to avoid confusion.

20- **L294: Arguable**

We understand the reviewer's concern regarding our conclusion. The question to be asked here is whether these derived boundary conditions are of sufficient quality to be used for the generation of qualitative flood risk estimates (as opposed to quantitative and highly-detailed results). The comparison between the open-access rainfall data and the rainfall data provided by local partners for Tan Son Hoa shows that there are indeed notable differences as can be seen from Table 3. However, these differences are reasonable especially for return periods of 5 years and less, which are the focus of this study.

21- **L304: this is not a proper argument. If there is some protection measure, there won't be any flow toward some of the lowest elevations. These zones may be eventually flooded but for other reasons (rain, groundwater, etc.) and so with a different dynamic.**

We share the reviewer's concern regarding this point. However, the argument greatly depends on the nature of the protection measure being approximated. Larger-scale, above-ground measures (e.g., dikes, pumping stations, detention/retention ponds) could still be accurately represented in this DEM, especially in the case where the impact of adaptation measures is evaluated relative to a no-adaption base case (see Scheiber et al. (Preprint)). In contrast, underground stormwater drainage systems are harder to represent, especially when relying on open-access data. However, there is significant evidence for the ineffectiveness of the storm water drainage system in the case of Ho Chi Minh City (Le Phu et al, 2021, Q.T. Nguyen, 2016), so that a worst-case scenario modeling is conceivable (Scussolini et al, 2017). These drainage systems are not well maintained and have limited functionality, so that their capacity is heavily diminished (Nguyen et al., 2019). This warrants a conservative engineering approach, in which they are deliberately omitted in the modelling scheme.

22- **L308: It would be interesting to present this reference. And this methodology is also arguable. If this reference is not realistic compared to observed flooded zones, how can we trust simulations with more extreme conditions?**

We understand the concern of the reviewer in regards to this chosen reference. This reference is indeed pivotal to the results of our flood simulation. If the reference, is not correctly defined, then the model would not perform as intended. In the case of our study, this reference is a theoretical inundation layer from mean tidal and fluvial conditions which do not lead to inundations in reality. It was defined based on flooding threshold values that were determined through joint work with local partners as well as information from grey literature like the JICA reports (JICA, 2001; URL: https://openjicareport.jica.go.jp/618/618_123.html) as well as different media articles accessible at the following URLs:

https://www.c40.org/case-studies/mitigate-urban-flooding-in-ho-chi-minh-city-phase-1/

https://global.royalhaskoningdhv.com/projects/flood-management-in-ho-chi-minh-city-vietnam

https://borneobulletin.com.bn/poor-urban-development-cause-of-flooding-congestion-in-ho-chi-minh-say-experts/

https://e.vnexpress.net/news/news/major-anti-flooding-project-in-hcmc-misses-deadline-for-four-years-4444006.html

https://www.channelnewsasia.com/cnainsider/siege-climate-man-made-problems-sinking-ho-chi-minh-city-floods-2052231

The reference was thus set according to tidal time series for Nha Be presented in Fig. 6, which represents the employed flooding threshold for Ho Chi Minh City. The confirmation of this reference method is further reinforced by the calibration and validation results.

23- L321: Flow depth is often not sufficient to evaluate risk for people. One also needs the flow velocity (Which can be provided by a 2D model of properly calibrated)

The reviewer's argument is valid. 2D models that have a high spatial resolution to properly reflect changes in flow cross sections for urban environments can definitely deliver flow velocities relevant to evaluate risk for people. This, however, does not belong to the scope of our study, especially because our model is not appropriate for such a purpose and because high flow velocities do not pose a significant threat in a flat LECZ such as HCMC as explained in Section 4.

24- L334: This sentence should appear after the introduction of Eq. 3

Thank you for this clarification, we have changed the text accordingly.

25- **Eq. 3: even if this error is very common, it is not correct to introduce a variable made of multiple letters, i.e. NFSI = N×F×S×I. I would suggest to write:**

$$\boldsymbol{I_{NFS}(x,y) = \frac{z_{max}(x,y) \times D_o(x,y)}{max(z_{max}(x,y)) \times max(D_o(x,y))}}$$

Isn't it a problem to use the maximal flood depth and duration as a reference/ If the model provides some local unrealistic values for z_{max} and or D_o, it would significantly affect the results.

We find this comment very valuable and have implemented the suggested change. In regards to the effect of using the maximal flood depth and duration, we would like to refer to our answer to the general comments Part IV on the use of 95th quantiles.

26- **L346: This is a significant issue. In many cases, institutions or insurance companies will use such flood maps as truth. If the model is not properly calibrated nor validated, it may lead to very problematic situation for people living in these areas.**

We agree with the reviewer's comment. A model that is meant to deliver highly accurate flood depths, durations and velocities that can be considered as truth should definitely be set-up, calibrated and operated on data with higher resolution and little uncertainty. However, the goal of this study is to assess the performance of open-access data (in cases, where that highly resolved information is inaccessible) in delivering first estimates of potential flooding as well as gaining understanding of underlying flood mechanisms. An open-access model built for regions where data is scarce can hardly be the "be-all and end-all" instrument for producing highly accurate flood maps but can still be very valuable for specific use cases, like determining inundation hotspots, especially when combined with the use of the INFS.

27- **L352: What about calibration?**

To alleviate the concerns of the reviewer, we have revised our explanations regarding model calibration and added a table under Section 2.1.3. Furthermore, we changed the title of the section which now reads as follows:

"*Hydraulic Roughness Coefficient and Model Calibration*"

28- **L354: What about discharge and water level (tidal) conditions on the River Sai Gon?**

The reviewer's question surely warrants a closer examination of the discharge and tidal conditions of the River Sai Gon. However, there is a considerable lack of data in regards to this particular waterway especially in open-access data, so that we are afraid nothing can be done in this regard.

29- **L357: Is this specific event representative of all events occurring on the HCMC area? Are there some cases with higher discharges for The River Sai Gon and/or strong tidal effects for which the model could also be validated?**

We have chosen the 14/06/2010 event for model validation, firstly, because the corresponding boundary conditions are known and, secondly, because flood depths were measured at different locations in HCMC during this event which is a pre-requisite for proper validation. Furthermore, our focus is on rain-induced flooding in Ho Chi Minh City and the special backwater effect caused by high tide, which is epitomized by this event (P=73 mm, WL=1.15 m). In regard to the discharges of the Sai Gon River, we have assumed that the reservoir mitigates higher discharge values associated with extreme river discharges. As for stronger tidal effects, such events can surely be simulated, but were not the focus of our study, which addresses frequent and disruptive rather than extreme flood events.

30- **Fig. 5: Do not add a linear regression when comparing simulation to observation; I see only 14 points on the plot whereas 25 are shown on the map. As far as I understood, the simulated water depths correspond to a difference between simulation results and results of the simulation for the 3h1y rain event with mean tide and mean river discharge. How sensitive are the results to this choice?**

We have taken the reviewer's comment into consideration and have removed the linear regression. Instead, we now provide a table, where the observed and simulated depth according to street name are given, and another table, which presents the results of the validation in addition to the graph. The simulated water depths are the difference between the results of the validation event (P=73 cm and HWL=1.15 m) on the day of the event (14/06/2010) along with mean river discharge and the results of the "reference" case which characterized by mean tidal conditions and mean discharge, but no rain, for which no flooding occurs. Both data collections can be provided either in the revised manuscript or as Supplementary Material.

31- L363: Just to be sure I understood, you increased the Sai Gon bed level from +8.4 m (above sea level?!) to 14.8 m (Fig. 6). Is it realistic? Anyway, I'm amazed that such variations don't affect the results. How deep is the River Sai Gon for normal flows?

We would like to thank the reviewer for this valuable comment. We have changed the text and Fig.6 so as to make it clear that the depth was varied from -8.4 to -14.8 m above mean sea level (MSL) in the context of our sensitivity analysis. This variation in depth of the Sai Gon River was done in order to determine the impact of this parameter on the simulated flood depth. We don't have robust information pertaining to the depth of the Sai Gon River, they were solely derived using the official navigational depth maintained for access to the ports along the river. The results of this sensitivity analysis show that a flood model could indeed be built on such an assumption without largely interfering with the robustness of the results when the goal is to generate flood risk estimates in an environment where data is scarce.

32- :365: How were selected these three points?

Point C was selected because it represents the outlet of the Ben Nghe canal, and Point B represents the intersection between the Ben Nghe and the Tau Hu canals, where frequent flooding occurs, while point A is a known inundation hotspot at the endpoint of the Tau Hu canal. The goal is to show the impact of the change in the depth of the Sai Gon on the maximum water levels at different distances upstream from the outlet of the Ben Nghe canal to the Sai Gon River. Point C is located directly at the Sai Gon River, Point B is located 3 Km upstream from Point C, while Point A is located 13 Km upstream from Point C.

33- Fig. 6: Define the location of the pints where sensitivity analysis is provided on the map Fig. 3 (use other letters since A, B, and C corresponds to other areas) and present plots only. Add a proper scale with axis legend for the three plots (or 4 if you include Nha Be water level time series)

Thank you for these valuable comments, we revised the figure accordingly.

34- L375: There is not Fig. 6a and b. If you're talking about the plots in Fig. 6, it is not clear for me how you evaluation mFD and DoT from these plots.

Thank you for pointing out this mistake, Fig. 7 was actually meant here. These figures serve to show the difference in spatial extent as well as spatial variability of mFD and DoT. The main argument is that areas with high mFD do not necessarily translate to areas with high DoT.

35- L377: How do you explain this behaviour? Is it based on observations from the field or from the numerical results?

Interesting question. This behavior can be explained by the fact that these areas have smaller catchments with lower concentration times and are thus drained more quickly. Furthermore, their proximity to the Sai Gon River also plays a role in the drainage behavior.

36- L380: What do you mean by "highlights previously hidden inundation hotspots"? Again, if the model is not really validated (at least not everywhere in the studied area), how sure are you about such results?

Good question. This sentence was meant to set aside the inherent bias of the spatiality of the reported inundations. However, after careful reviewing this paragraph, we have decided to omit this sentence. We cannot say for sure whether these results 100% reflect the truth for unvalidated areas but it would definitely be highly interesting to validate them accordingly. Unfortunately, no inundations were reported in the depicted areas.

37- L381: "considerable spatial overlapping"! I'm not that enthusiastic. Most of the reported inundation points do not overlap with the zones with a NFSI>0! What about all the zones with a high NFSI value? I can understand there is also a bias in the reported inundation points but you cannot say here that results are good.

We are aware that is it not very easy to determine the spatial overlapping using the color scheme presented in Fig. 7, which is exactly why we created Table 4, where it is obvious that the INFS map is 4 times as accurate in matching the locations of reported floods when compared to a random area with the same size. Nevertheless, we agree with the reviewer in this regard and intensely discussed whether the results can be presented in a way that effective spatial overlapping becomes clearer in Fig. 7 itself.

38- Fig. 7c: It is not very consistent to compare the flood severity index with reported inundation. A reported inundation corresponds to a water depth; so, these points should be compared to the modelled maximum flood depth (Fig. 7a). Again, do not provide comments of the figure in the figure caption (redundant with the text)

We understand the concern of the reviewer in this regard. However, the reported inundations that were used in this figure do not only correspond to places were high flood depths were recorder but also places that are persistently flooded over a relatively long time. What we wanted to highlight here is that the INFS is better at identifying these areas as opposed to the flood depth, which does indeed cover more reported inundation, but performs worse relative to size and thus does not serve the purpose of the methodology, which is to better pinpoint areas that require closer attention and where the procurement of higher quality data is worthwhile.

39- **L437: Due to the limitation of data to calibrated/validate the model, it is logical to use a single Manning friction coefficient for the whole domain. However, in reality, this coefficient should vary spatially depending on the city structure (presence of vegetation or not, porosity of the system, etc.)**

We agree with the reviewer on this point. The spatial variation of the Manning friction coefficient can be and actually was considered. We left this out of the scope of this paper because the land use classes were not available in open-access. One might argue that remote sensing could be used to the determination of land use classes and then using that to determine the Manning friction coefficient of some parts of the city, but that would be out of the scope of this paper.

40- **L474: True but the velocity is important in term of flood hazard for pedestrian (Musolino et al., 2020)**

We thank the reviewer for this insightful comment. We may consider this caveat in our text but as stated in the responses to Part IV of the general comments of the reviewer, our model is not designed to forecast precise flow velocities, which also play a secondary role in a flat, low-lying system such as HCMC.

41- **L479: True but you need a robust and well calibrated model**

We agree that a more robust and model that is calibrated for more events can deliver more robust and trustworthy results, but we don't insist on this argumentation since the overall objective is led by another intention as previously pointed out in the answers to Part I of the general comments.

42- **L491: I'm not sure such model can be used to simulate flood drivers, even partially.**

It is an important fact that the provided methodology was calibrated and validated for rain events only. The aforementioned explanations regarding the scope and limitations of our modelling scheme, hopefully, add some context and robustness to the highlighted statement. In any case, we revised this sentence in the new version of the manuscript.

43- **L528: Use European convention for dates: 12/06/2018**

Thank you for this and the following plethora of comments and corrections, which is very helpful to ensure the correctness of this paper. The invested efforts are much appreciated. The bibliography was changed accordingly.

44- L531: Use "doi:" instead of the full link "https/: doi.org/"

45- L541: De Andrés, M.; be homogeneous with journal title (abbreviated or not)

46- L546: Use capital letters for acronyms only, i.e. Bennghe Port Company Limited

47- L554: Initials for first names after the name

48- L557: Add all authors (instead of "et al."), initials of authors

49- L567: reference?!

50- L574: date, doi

51- L595: Use capital letters for acronyms only, i.e. Go Fair

52- L602: Skip "available at…"

53- L608: Skip "available at…"

54- L614: NGO?!

55- L615: Journal?!

56- L630: Explain the acronym JICA

57- L638: de Moel, H.

58- L672: Add all authors (instead of "et al."), initials of authors

59- L685: Explain all acronyms

60- L689: Don't use capital letters for the title and journal (International Journal of Geomate), some co-authors are missing

61- L701: Skip "available at…"

62- **L716: Add all authors (instead of "et al."), initials of authors; Don't use capital letters for the journal name (?); Add (in Vietnamese)**

63- **L726: Don't use capital letters for the author name**

64- **L740: Skip references in review**

65- **L776: Don't use capital letters for the author name**

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