Response to reviewers for:

Intersecting Near-Real Time Fluvial and Pluvial Inundation Estimates with Sociodemographic Vulnerability to Quantify a Household Flood Impact Index

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Reviewer 1 (R1):

We thank the reviewer for taking the time to review our paper and for the minor comments to both improve the clarity of our work as well as its presentation and organization. We have made the following adjustments to the manuscript based on your suggestions:

(R1.1) *The sections of Introduction and Background are long-winded. For example, section 2.3 can be removed; it could be just mentioned that the hydrodynamic models are time-consuming and highly data required in the introduction. The paragraph on 'depression' in the introduction could be moved to methodology section.*

The paragraph on depressions (originally starting at line 33) was eliminated, and pertinent information that defines depressions was incorporated elsewhere in the introduction (new line 34) and in the methodology section (new line 96). Section 2.3 on recent advancements on compound flood mapping was significantly reduced. We believe that acknowledging recent advancements in this area is still important for establishing the context in which our model was developed.

(R1.2) *Figure 7, 8 and table 3 needs to be explained in detail in the text.*

Table 3 is discussed in more detail in the paragraph that begins at new line 338. Figure 7 is expanded upon in the results section to better describe what is being shown (please refer to line 453 in the new draft). Figure 8 is also briefly expanded upon in the results section to better describe what is being shown (please refer to line 456 in the new draft).

(R1.3) *In the discussion, it would be better to stress that how this methodology can be applied to other area and what could be the limitations.*

This expansion was added into the conclusion section (please refer to line 614 in the new draft).

Reviewer 2 (R2):

We thank the reviewer for taking the time to review our paper and provide some discussion points as well as minor comments to improve the clarity and presentation of our work. We address your comments as follows:

(R2.1) *My main concern is about vulnerability and resilience. The vulnerability variables used in this study can be regarded as resilience variables. Is it reasonable? And, the impact index is a very complex issue in the evaluation of the risks. I think you should be careful about the selection of Variables. Some variables maybe not that representative or maybe there is relationship between variables which make these variables used more than once in the computation of the flood index.*

The reviewer brings up a great point in that vulnerability and resiliency represent two related but different concepts. To further complicate it, there is also the concept of adaptive capacity; there are entire papers dedicated to discussing the interrelated nature of some or all of these three concepts (Gallopin, 2006; Miller et al., 2010). Being able to directly estimate any of these concepts is a difficult, if not impossible, process, which inspired the creation of the first Social Vulnerability Index, or SVI, (Cutter et al., 2003), from which our paper draws motivation. The SVI serves as a proxy for these more complex concepts because we are able to estimate it using widely available Census data. For this paper we are using the IPCC definition of vulnerability (please refer to the paragraph starting at line 209), in that vulnerability is the degree to which a system is susceptible to, or unable to cope with, the adverse effects of a hazard. Trends in the 29 Census variables that can potentially be in the SVI calculation have been shown to reflect either an increase or decrease in an individual's vulnerability. The authors refer the reviewer to Cutter et al., (2003) for how different concepts of socioeconomic status, gender, race, ethnicity, age, employment, education, etc. influence vulnerability.

The reviewer is also correct in that some variables are highly related, which is what leads to similar variables being grouped into components. For example, the variable "Median housing value" and "Percent households earning over $200,000 annually" are most assuredly related. There continues to be an ongoing debate on the usefulness of specific vulnerability indices, as we discuss in section 6.4. Sensitivity analyses of the specific SVI methodology used have shown that different configurations of the parameters still yield fairly stable results (Schmidtlein et al., 2008). While some papers have found substantial problems with SVIs, they also recognize that SVIs serve a role for being able to estimate a complex concept quickly (Spielman et al., 2020). As we state in section 6.4, our proposed methodology for estimating a near-real time flood impact index is robust because any SVI estimate can easily replace our own based on the user's goals, preferences, and expert opinion.

(R2.2**)** *Since you provide effective near real-time estimates providing dynamic information for householders or others, did you consider that variables should be updated frequently to make sure the efficiency of the evaluation? This is a very important issue for practical use.*

This is an excellent point and highlights one of the advantages of using a social vulnerability estimate built on Census data. As stated in section 3, for this study we used information from the 2017 American Community Survey (ACS) 5-Year Estimate, an aggregate of 5 years' worth of survey data. We chose this year because it might best represent the socio-economic conditions of 2015, as it is the midpoint of the years used to determine the 2017 estimates. For more recent events, the most recent ACS 5-Year Estimate should be used to calculate the SVI, which can easily be recalculated. For a widely applicable flood impact index, yearly updates to socio-economic data are sufficient as there are no other more frequently updated socio-economic datasets, and changes within a single year are often not drastic enough to have an impact on estimates.

(R2.3) *Furthermore, the proposed method is relatively efficient when compared with conventional approaches. But the reasonability and accuracy are still the fundamental issue of the impact map should be addressed. So as the uncertainty.*

The reviewer is correct in that this methodology trades a potential decrease in accuracy for an increase in speed. While literature exists to support that emergency managers are willing to accept a decrease in accuracy for an increase in speed (McCarthy et al., 2007), we must still produce a result that is reasonable and accurate as well as be able to recognize the sources of uncertainty. To assert the reasonability and accuracy of our methodology, we compared our results to those obtained from a full hydrodynamic model, because the overarching goal is to be able to produce a comparable impact map in a fraction of the time. We therefore take the full hydrodynamic model to be the absolute truth and define accuracy as how well our methodology creates a similar impact estimate. As we discuss in section 5.1, our methodology produced an inundation extent that was only 31% accurate. However, when we use our reclassification scheme at the residential parcels, we classify 92% of residential parcels similarly. And of those parcels that are misclassified, 94.4% are misclassified by only one impact level. By reporting impact at the parcel level and by using hazard classes for flood depths, a common practice in flood communication research (Eq. 3), we achieve our goal and produce comparable impact maps. This impact map serves as an estimate for residential parcels that are going to be the most affected, and similar to many social vulnerability indices, should still rely on a certain level of local expert opinion and objective interpretation.

(R2.4) The authors give us a background in a very detailed way. A very nice Review. maybe it is too wordy for this study. I'm not sure the background section needs so much description, I suggest cutting some down.

Some material has been eliminated from the background section, as in response to the first reviewer's comments.

(R2.5) I am curious about the application scope of fill-spill-merge. Is there any requirement for topographic difference or complexity of surface landscape? Would it be more helpful for reader to add some terrain information through DEM and the number or location of depressions in Figure 1?

The authors explored showing the extent of depressions, but due to their nature of being a few meters in size and highly dispersed, it was difficult to render a figure that meaningfully conveyed that information. We refer the reviewer and readers to figure 4 (figure 5 in the original manuscript), specifically the insets A2 and B2 to examine the nature of depressions in two different representative locations in the study area.

***(R2.6*)** In Line 593-595: "Our pluvial hazard estimate was shown to be accurate in determining the parcel level impact index 94.4% of the time when compared to a full hydrodynamic model". It was confusing for me to understand how to define the "accurate in determining the parcel level impact index"? Is the drainage network from FSM and hydrodynamic model compared here are both out of consideration, differing only at the depressions? Setup differences between the two methods should be made clear.

Clarification was added to expand upon this accuracy in section 5.1 (please refer to line 407 in the new draft). We also make this clarification in response to your previous comment, R2.3.

***(R2.7*)** Figure 1: To those readers who are not in the US, they have no idea where Austin is. It is better to add a figure about the general location of the study region in the US.

An inset map was added to show Austin in the context of the US.

***(R2.8*)** The residential area is the main focus of this study, it should be shown on figure 1.

To avoid overcrowding, residential parcels are not shown on this map and we refer the reviewer and readers to Figure 4 (Figure 5 in the original manuscript). Figure 1's intended purpose is to highlight the watershed basins in the study area.

***(R2.9*)** Table 1: The basic information about these watersheds, such as the size and imperviousness, should be added.

The area of each watershed is conveyed in Table 4. We decided against including impervious surface information in any table because land cover characteristics are not included in our simplified methodology.

***(R2.10*)** Figure 3 is not needful. The process is quite straightforward and has been documented clearly in the text.

Figure 3 was removed.

***(R2.11*)** Line 420: the full stop after "sources" should be deleted

This typo was corrected.

References:

Cutter, S., Boruff, B., & Shirley, L. (2003). Social Vulnerability and Environmental Hazards. Social Science Quarterly, 84(2). https://doi.org/10.1111/1540-6237.8402002

Gallopin, G. (2006). Linkages between vulnerability, resilience, and adaptive capacity. Global Environmental Change, 16(3). https://doi.org/10.1016/j.gloenvcha.2006.02.004

McCarthy, S., Tunstall, S., Parker, D., Faulkner, H., and Howe, J. (2007). Risk communication in emergency response to a simulated extreme flood. Environmental Hazards, 7. https://doi.org/10.1016/j.envhaz.2007.06.003

Miller, F., Osbahr, H., Boyd, E., Thomalla, F., Bharwani, S., Ziervogel, G., Walker, B., Birkmann, J., van der Leeuw, S., Rockström, J., Hinkel, J., Downing, T., Folke, C., & Nelson, D. (2010). Resilience and Vulnerability: Complementary or Conflicting Concepts? Ecology and Society, 15(3). http://www.jstor.org/stable/26268184

Schmidtlein, M., Deutsch, R., Piegorsch, W., & Cutter, S. (2008). A Sensitivity Analysis of the Social Vulnerability Index. Risk Analysis, 28(4). https://doi.org/10.1111/j.1539-6924.2008.01072.x

Spielman, S.E., Tuccillo, J., Folch, D.C., Schweikert, A., Davies, R., Wood, N., & Tate, E. (2020). Evaluating social vulnerability indicators: criteria and their application to the Social Vulnerability Index. Natural Hazards, 100. https://doi.org/10.1007/s11069-019-03820-z

We thank you for taking the time to review our paper and provide some insightful discussion points to improve the transparency and clarity of our work. We address your comments as follows:

C1: *One important assumption of this study is that the rainfall depth equals the runoff depth, which implies a 100% runoff coefficient. It should be noted that such an assumption is probably always invalid. Even a highly urbanized area can hardly reach a 80%+ runoff coefficient during a 5-hour storm event, since the actual impervious coverage could only be as high as 70%. A higher (90%+) runoff coefficient could possibly result from a prolonged storm duration, but such scenario doesn't seem permitted in this study due to the reset mechanism (see the next comment for more). Granted that this approach targets a 'worst-case' scenario, an 'impossible' estimate would not be very meaningful, would it?*

You are correct, in that our equating rainfall depth to runoff depth is an overestimate. However, we believe that our choice here is justified by the following three points:

First, as stated in section 4.1 (and expanded upon in the new manuscript at line 296), the nature of the local weather before the storm event favors the assumption of near saturated conditions. The previous five days leading up to the storm event under investigation all recorded some level of precipitation. Furthermore, the storm itself exhibited flash flood characteristics, with 80% of the precipitation (over 10-cm) falling within two hours. This intensity coupled with already wet soils signals that a significant portion of rainfall will be converted to runoff. We agree that while a 100% runoff coefficient is likely impossible, we believe that it is not unrealistic to assume many areas approached a relatively high runoff coefficient. When our methodology is applied to other areas, it is fully possible and feasible to take a fraction of the rainfall to assume some level of abstraction based on the local characteristics (impervious cover, potential soil conditions, rainfall patterns, etc.), but we believe for our scenario it would be better to slightly overestimate the amount of runoff rather than risk an underestimation.

Second, in a near-real time scenario, when the only available precipitation data is likely a set of point depths measurements, these estimates will continually change and grow as the storm develops. Rather than trying to estimate a proper abstraction value as it continues to rain and the amount of runoff only continues to increase, it would be timelier to assume it is all converted to runoff, especially for emergency response purposes.

Our third and final reason relates to the overarching goal of the paper to create a near-real time impact index from an inundation estimate, and then the question becomes does a 30% reduction in the total precipitation change the impact estimate. We found that the extent of pluvial inundation is virtually the same when comparing a full conversion of rainfall to runoff to 70% conversion. The change in depths also had a negligible impact on the final impact index. This is because changes in pluvial flood depths occur predominantly in areas that ponded water collects at larger depths including underpasses, intersections, and open spaces (parks, culverts, parking lots, etc.) and not on residential parcels.

As previously mentioned, you are correct in that the assumption that 100% percent of the rainfall being converted to runoff is unlikely. However, we believe that for this storm event under the given circumstances that this assumption would fall within a real-world application of the proposed methodology, and it can be adjusted accordingly when applied to other study areas or other storm events. For example, previous studies examining water ponding over a landscape have made simple adjustments to inputs depths to consider runoff abstraction and this could easily be incorporated in future applications (Shaw et al., 2012).

C2: *There is a reset mechanism in the inundation mapping approach with a 5-hour frequency in the case study. This setting seems to consider the time of concentration at some spatial scale as well as the storm duration, but the rationale behind this setting is ambiguous in the manuscript. I recommend give some clear guidance on this setting to help expand the application over other areas. The essential problem with this setting is that pluvial inundation is set to be strongly driven by the rainfall accumulation within the 5 hours regardless of the initial condition i.e., how much water is ponded initially. I have concerns over the implication of the inundation maps produced in the middle of a long storm event (say, 12 hours) in reality.*

One of the known limitations of topographic based inundation models is that they lack a timing mechanism and can therefore only be used to show a single state of inundation (Bulti and Abebe, 2020; Fritsch et al. 2016; Lhomme et al., 2009). For our study, the storm event lasted 5 hours, therefore using the cumulative rainfall/runoff depth of those 5 hours is representative of the final inundation state given the assumption that there is no initial ponded water**.** As mentioned in response to **C1,** while there was recorded rainfall for the five days leading up to the storm event, the precipitation amounts were unlikely to cause a significant amount of ponding as they were relatively low daily amounts (less than an inch per day). Using the cumulative value for whatever time step the user is interested in, whether it is at the completion of a 5-hour storm or halfway through a 12-hour storm, that is the best assumption that can be made when using a topographic based model. Initial conditions could easily be applied using this methodology as well, by routing a volume/depth through the topography before any additional runoff is routed. limitation and its justification was added to section 6.4.

C3: *The current method also nullifies heterogeneous effects of land cover on runoff generation, leaving the pluvial inundation dictated by topology. Besides the obvious uncertainty introduced, it undermines the hyper-resolution enabled by the DEM data. I understand 'computational efficiency' is the key word here, but I can also see more realistic alternatives than assuming a uniformly impervious coverage. For instance, land use land cover data combined with curve number method seem to be efficient and well-suited for estimating runoff within the current framework.*

There are certainly simplified topographic based inundation methods that incorporate land use information including such factors as infiltration or friction (Chu et al., 2013; Appels et al., 2011; Antoine et al., 2009; Lhomme et al., 2009). However, a major difference between the application of those methods and ours is the study area size and resolution of the underlying DEM. A goal of this research was to utilize 1-meter elevation data because of its increased prevalence within the US when it comes to inundation mapping and its ability to identify realistic urban pluvial flooding locations. Our DEM contains approximately 502 million cells, representing an area over 250 km^2 at a 1-meter horizontal resolution; a DEM size that far exceeds the previously listed studies. To meet the goal of near-real time at this resolution requires an oversimplification of processes, which is common for simplified inundation models. For example, the Rapid Flood Spreading Model

(RFSM) only requires an input flood volume, elevation, and surface roughness (Lhomme et al., 2009; Bernini & Franchini, 2013). Even with this simplification, this method is approximately 3.8 times slower than the Fill-Spill-Merge method utilized on our study (calculated by comparing the ratio of DEM cells to computational time in our study to theirs, being 502 million cells in 28 minutes with our method and 387 thousand cells in 5 seconds with theirs). In a near-real time scenario when the entire storm event occurs in less than 5 hours, the difference between a computational time of 28 minutes (our computational time) and 106 minutes (our computational time multiplied by 3.8, the estimated speed ratio of RFSM to Fill-Spill-Merge) is substantial. This information was incorporated into section 6.4 in the new draft.

Land use and land cover data combined with the curve number method are certainly alternatives to the topographic method we utilized, and the robustness of our impact index is that alternative inundation methods can be applied based on the end users' preferences and data/computational power available to them.

References:

Antoine, M., Javaux, N., & Bielders, C. (2009). What indicators can capture runoff-relevant connectivity properties of the micro-topography at the plot scale? Advances in Water Resources, 32(8). https://doi.org/10.1016/j.advwatres.2009.05.006

Appels, W., Bogaart, P., van der Zee, S. (2011). Influence of spatial variations of microtopography and infiltration on surface runoff and field scale hydrological connectivity. Advances in Water Resources, 43(2). https://doi.org/10.1016/j.advwatres.2010.12.003

Bernini, A., & Franchini, M. (2013). A Rapid Model for Delimiting Flooded Areas. Water Resources Management, 27. https://doi.org/10.1007/s11269-013-0383-3

Bulti, D., & Adebe, B. (2020). A review of flood modeling methods for urban pluvial flood application. Modeling Earth Systems and Environment, 6. https://doi.org/10.1007/s40808-020-00803-z

Chu, X., Yang, J., Chi, Y., & Zhang, J. (2013). Dynamic puddle delineation and modeling of puddle-to-puddle filling-spilling-merging-splitting overland flow processes. Water Resource Research 49(6). https://doi.org/10.1002/wrcr.20286

Fritsch, K., Assmann, A., Tyrna, B. (2016). Long-term experiences with pluvial flood risk management. FLOOD risk 3rd European conference on flood risk management. https://doi.org/10.1051/e3sconf/20160704017

Lhomme, J., Sayers, P., Goudby, B., Samuels, P., Wills, M., Mulet-Marti, J. (2009). Recent developments and application of a rapid flood spreading method. Flood Risk Management: Research and Practice. http://eprints.hrwallingford.com/id/eprint/695

Shaw, D., Pietroniro, A., & Martz, L. (2012). Topographic analysis for the prairie pothole region of Western Canada. Hydrological Processes, 27(22). https://doi.org/10.1002/hyp.9409