Editor Comments:

The revised submission is much improved and both reviewers recognise the work done in this regard. The reviewers remain concerned about the novelty, especially in relation to the inundation modelling. Recommendations regarding publication are split, so I have additionally reviewed the manuscript myself and include some comments below. I share the reviewers concern around the novelty of the inundation methods and don't believe the modelling method per say is novel. However, three of the four conclusions relate to the big data architecture and implementation of the modelling methods within this. In that regard I think it is possible for a novel contribution when looking at the work as a whole, with some further revisions to the manuscript needed to bring this out. Please respond to all of the comments below and those from both reviews.

Comments:

1. Line 60-67 need references for examples of these approaches.

The section is revised to include references. The relevant text (Lines 47-55) and references are copied below

"Flood inundation modelling approaches can be broadly divided into three model classes: empirical (Schumann et al., 2009; Smith, 1997); hydrodynamic (Brunner, 2016, DHI, 2012); and simplified/conceptual (L'homme et al., 2008, Néelz & Pender, 2010). Empirical methods entail direct observation through methods such as remote sensing, measurements, and surveying, and have since evolved into statistical methods informed by fitting relationships to empirical data. Hydrodynamic models, incorporating three subclasses, viz; one-dimensional (Brunner, 2016; DHI, 2003), two-dimensional (DHI, 2012; Moulinec et. al., 2011), and three-dimensional (Prakash et. al., 2014; Vacondio et. al., 2011), consider fluid motion in terms of physical laws to derive and solve equations."

Brunner, G. W. (2016). HEC-RAS River Analysis System 2D Modelling User's Manual Version 5.0. (Report Number CPD-68A). US Army Corps of Engineers Hydrologic Engineering Center.

DHI, 2003. MIKE 11-A Modelling System for Rivers and Channels - User Guide. DHI, p. 430.

DHI. (2012). MIKE 21-2D Modelling of Coast and Sea. DHI Water & Environment Pty Ltd.

L'homme J., P. Sayers, B. Gouldby, P. Samuels, M. Wills, J. (2008) Mulet-Marti Recent development and application of a rapid flood spreading method P. Samuels, S. Huntington, W. Allsop, J. Harrop (Eds.), Flood Risk Management: Research and Practice, Taylor & Francis Group, London, UK, Moulinec, C., Denis, C., Pham, C.T., Rouge, D., Hervouet, J.M., (2011). TELEMAC: an efficient hydrodynamics suite for massively parallel architectures. Comput. Fluids 51 (1), 30e34.

Prakash, M., Rothauge, K., Cleary, P.W. (2014). Modelling the impact of dam failure scenarios on flood inundation using SPH. Appl. Math. Model. 38 (23), 5515e5534.

Schumann, G., Bates, P.D., Horritt, M.S., Matgen, P., Pappenberger, F., 2009. Progress in integration of remote sensing-derived flood extent and stage data and hydraulic models. Rev. Geophys. 47 (4), RG4001.

Smith, L.C., 1997. Satellite remote sensing of river inundation area, stage, and discharge: a review. Hydrol. Process. 11 (10), 1427e1439

Vacondio, R., Rogers, B., Stansby, P., Mignosa, P., (2011). SPH modeling of shallow flow with open boundaries for practical flood simulation. J. Hydraul. Eng. 138 (6), 530e541

2. Line 67: needs evidence to support the view that the majority of recent developments for large scale studies are of the simple conceptual type models.

A reference has been included in the sentence to support the claim. The relevant text (Lines 55-57) and reference are copied below:

“The third model class, simple conceptual, has become increasingly well-known in the contexts of large study areas, data scarcity, and/or stochastic modeling and encompasses the majority of recent developments in inundation modelling practices (Teng et. al. 2017).”

Teng, J., Jakeman, A. J., Vaze, J., Croke, B. F. W., Dutta, D., & Kim, S. (2017). Flood inundation modelling: A review of methods, recent advances and uncertainty analysis. Environmental Modelling and Software, 90, 201–216. https://doi.org/10.1016/j.envsoft.2017.01.006

3. Line 70: What do you mean by a class of mode in this sentence, please be specific. “A class of model which uses the output of a more complex model as a means of calibrating a relatively simpler model is also gaining popularity (Oubennaceur et al., 2019).”

By a class of model, we mean the type of models which by construct are simplistic in nature but the data used for calibration of these models comes from more complex modeling studies. For example, Oubennaceur et. al. 2019 used simple power law relationship between discharge and flood depth in any cell. However, the inundation depth is calibrated based on complex H2D2 model.

4. Line 81: You are missing a class of parallelisation that uses shared memory threading on CPU's.

The line has been modified to include the shared memory threading class. The relevant text (Lines 69-72) is copied below:

"With respect to 2D/3D hydrodynamic model code parallelization, Vacondio et al. (2017) listed two approaches: classical (multi-treading or Open Multi-Processing and Message Passing Interface) and Graphics Processing Units (GPUs)."

5. Line 88-92: very long sentence, consider splitting

The sentence has been reworked as follows (Lines 91-95):

"Such simple conceptual inundation models offer another potential avenue to handle limitations such as computation requirements and data scarcity. In turn, areas and scales poorly served by standard hydrodynamic modelling may be provided with up-to date flood extent maps. Platforms through which the public can view and interact with the flood extent maps may also be developed (Tavares da Costa, 2019). "

6. Line 92-101: "One such simple conceptual inundation model…" Given that you are implementing a simple conceptual model this section is very brief. The logic in the introduction breaks down here because you initially state that the simple conceptual models have seen the most progress in recent years and then suggest that conceptual flood models have remained largely uninvestigated. For example why not mention GLOFRIS
Assessing flood risk at the global scale: model setup, results, and sensitivity - IOPscience
HESS - A framework for global river flood risk assessments (copernicus.org)
Or
A rapid urban flood inundation and damage assessment model - ScienceDirect
I think this is why the reviewers are disputing the novelty claimed here. In my opinion there is a lot of work on simple inundation models that can inform the approach taken here.

Thanks for the suggestion, we have referenced both of these models in this section as follows in order highlight how our approach is distinct from these. The relevant text (Lines 80-91) is copied below:

"Several studies have introduced generic modelling frameworks that aim to provide robust flood risk estimates with relatively little configuration. Winsemius et al. (2013) for example developed GLOFRIS, a global-scale flood risk modelling framework comprised of global forcing data, a global hydrological model, a flood routing model, and an inundation downscaling model. While capable of providing flood risk at virtually any location on earth, the modelling framework is fixed to the existing datasets and models used, which have significant uncertainty at the scales considered. At a more local scale, Jamali et al. (2018) introduces a flexible flood inundation model that integrates a 1D hydraulic model with a simple GIS-based flood inundation approach. However, this loosely coupled approach still requires specification of a standalone hydraulic model for each location at which it is implemented. There has been a recent stream of research aiming to develop simple conceptual inundation models that preserve both the generality of GLOFIS and the specificity of more local-scale models."

7. Line 140: As a non-expert in alternative spatial data models could you state what the traditional models would be in this context. The novelty here was lost on the reviewers and I'm still unsure how significant this is.

The following text (Lines 143-146) has been added to clarify this and is copied below:

"uses an integer-based addressing system which makes it orders of magnitude more efficient than that of other, more traditional spatial data models (i.e, raster, vector) (Mahdavi-Amiri et. al. 2015; Li & Stefanakis, 2020; Robertson et. al. 2020)."

We have also added the following line to the paragraph where we introduce DGGS (Lines 118-121)

"The Open Geospatial Consortium adopted a DGGS Abstract Specification in late 2017 and work is currently underway to develop standards for DGGS specification as a core geospatial data model (OGC 2017). This is the first use of a DGGS for flood modelling we are aware of."

8. Line 241: Is it channel or floodplain manning's n being determined? Are both the same? Obviously the link to n is more direct for the floodplain. Channel n is often lower than the floodplain and more associated with channel morphology than adjacent landcover.

We agree the channel Manning's n can be much lower than the floodplain Manning's n (0.02-0.03). However, the majority of the design flood will inundate the floodplain so we used the floodplain specific LULC dependant Manning's n. Furthermore, for the water pixels on the channel a nominal value of 0.04 has been used.

9. Line 232: "Regional hydrological frequency analysis at ungauged sites is also studied by few researchers" Is this true? Prediction in ungauged basins is one of the more fundamental and widely studies areas of hydrology. Perhaps this is the case in Canada or there is some more nuance to this point?

Several studies of the regional hydrological frequency analysis in Canadian context have been done by Prof. D. H. Burn and his group. However, there is in general a scarcity of recent studies on this topic over Canada.

10. Line 283: check wording of sentence.

The extraneous “used in” has been removed. The remaining sentence (Lines 290-293) is copied below:

“We took the logarithm of Equation (1) on both sides - a procedure noted in Hailegeorgis & Alfredsen (2017) as used in Eaton, Church, & Ham (2002) - yielding a linear relationship which was solved using the Ordinary Least Squares approach (Haddad et al. (2011).”

11. Line 294: This is a very long sentence, consider if this can be split and reordered for readability.

The text has been edited for clarity and is copied below (Lines 302-305):

“A fundamental step of the analysis process is the selection of a suitable probability distribution model, a common tool in hydrologic modelling studies. The model should account for changes to the flow’s extreme value characteristics in response to such factors as urbanization, agriculture, resource extraction, or the operation of dams and weirs.”

12. Around line 359: OK 1D hydraulic models require cross-section to be defined. But HAND would also need channel bathymetry to capture the channel flow components of the discharge I assume. How is channel conveyance represented in your HAND method? Is the bathymetry in the DEM and if it was I assume that’s been interpolated from cross section data or estimated from a design discharge? So, I agree that on the floodplain HAND avoids the problem of defining cross sections (as a 2D hydrodynamic model also would), but for the channel it will depend on what has been done here and this is not clear to me. I think you oversell the benefits of HAND here.

We agree the use of bathymetry would improve the reliability of the flood simulation significantly. However, even if bathymetry is presented the 1D model would still need to decide the specific orientation of the cross-section which would have an effect on the simulation results.

We used the interpolated DEM values as the bathymetry of the channel.

We added the following sentence for better clarity in the specific section (Lines 364-370):

“Even though the use of DEM-interpolated bathymetry, as used by our method, induces error in the modelling of flood inundation, it is a necessity in the absence of bathymetry data. There are several instances in literature (Sanders, 2007) where the DEM-interpolated bathymetry has been tested in place of actual bathymetry for hydrodynamic flood modelling. Furthermore, the requirement of the cross-section being perpendicular to the flow direction makes it an implicit problem and also dependent on the choice of cross-section position as well as the distance at which the points are taken on the cross-section.”

Sanders, B. F. (2007), Evaluation of on-line DEMs for flood inundation modeling, Advances in Water Resources, Volume 30, Issue 8, 2007, Pages 1831-1843, ISSN 0309-1708, https://doi.org/10.1016/j.advwatres.2007.02.005.

13. Line 368: Backwater effects are not only due to flood mitigation structures but will occur on all rivers with subcritical flows conditions (which will probably include the vast majority of those with significant floodplains). The issue is more ubiquitous than suggested here.

We agree the high flow depth and small flow velocities in the natural rivers can cause backwater effect in very far upstream. We revised (Lines 378-382) to include this perspective as follows:

"Furthermore, the large flood depth and low flow velocity in the natural rivers makes the river subcritical on many occasions, specifically for large floodplains where the water slows down significantly. This causes the backwater effect very far upstream of the flooding locations which is not simulated in HAND based methods."

14. Line 685, sentence needs to be specific regarding what the "direct comparisons" are. I know its obvious from the earlier text but the sentence on its own is truncated in my view.

The modified sentence reads as follows (Lines 700-702):

"Overall, the results indicated that the current iteration of the InundatEd flood model was reasonably successful on the basis of moderate-high MCC values and direct comparisons against the observed flooding extents."

15. Line 694: Perhaps I missed it, but I didn't get a good understanding of what a traditional raster based methods are in this context. This links back to earlier comments around line 140 where I didn't really appreciate what the innovation being brought by DGGS was reference to.

The paragraph is reworded as follows (Lines 710-714):

"There is a distinct contrast of runtimes between the DGGS method and those using a traditional, raster-based method for sub-catchments within the Grand River Watershed (n= 306 for each method) during the generation of respective RP 100 flood maps. The DGGS based storing and processing method is an order of magnitude faster than processing the HAND and catchment boundaries using raster and vector format."

16. Line 717: I'm happy with principal behind the approach taken to the inundation modelling, but I'm not convinced the use of catchment integrated Manning's is really a novelty of this work. The other novelties listed are stronger in my opinion. Rather than reinvent HAND as novel, would it be more accurate to say an existing method has been

implemented with a new data set and/or location and computation framework. I don't know what has been done previously in the region, but in my opinion it is the application rather than the inundation modelling that is more likely to be novel. Furthermore, the results show how well the inundation model reproduces historical events and design extents, at no point do you analysis the physical process directly, so I would not claim that the inundation extents are physically justified but instead point to the accuracy of the extents.

The second novelty point is reworked as follows (Lines 735-738);

"Second, the computational framework has been implemented using a regional dataset over locations and at scales which have not been studied before. We successfully demonstrated the merit of the HAND-based inundation modelling to emulate the observed flooding extent for several historical and design floods."

Referee 1 Comments:

The authors are to be commended for their responses to the original reviews and have provided an analysis of model behaviour and set-up that is more robust than the first iteration. I'm sorry to say, however, that the novelty of their contribution is still extremely limited scientifically, and does not significantly further understanding in our field. RFFA+HAND is a model of boundary condition generation and inundation prediction that has been extensively published elsewhere, and this paper does not provide further insight into it. Stated novelties are confined to the model deployment and its architecture, rather than its scientific contribution. The use of LULC data to define Manning's n is not novel. Novelty aside, the modelling process and validation is largely scientifically sound. My only comment is to be careful when comparing skill scores (e.g. CSI) across different models, time, space, and contexts. These are not objective measures of performance, and vary depending on the size of the flood, catchment properties, and quality of the benchmark data. See Mason et al., 2009 (https://doi.org/10.1016/j.jhydrol.2009.02.034); Stephens et al., 2014 (https://doi.org/10.1002/hyp.9979); Wing et al., 2021 (https://doi.org/10.5194/nhess-21-559-2021) for further elaboration on this. The comparison to Wing et al. (2017), for instance, states CSIs of ~0.5 – which isn't strictly true as the coverage of the benchmark data was poor, resulting in a lot of 'non-genuine' overprediction, and thus biasing the results. Metrics in Bates et al. (2021; https://doi.org/10.1029/2020WR028673) account for this, and so should be used for a more reliable CSI comparison.

The paper introduces the first DGGS-based implementation of a HAND flood inundation model. This is implemented completely in-database which has the following benefits:

- Speed improvement compared to raster-based HAND model .
- On-the-fly computation only requiring the following data inputs: DEM, LULC and stage-discharge curve of the input.

- Scalability - computations done in-database meaning transport of data to application software not required, opening up potential for truly global scale high resolution flood inundation modelling
- Multiscale properties – given the nested nature of DGGS cells visualization of model outputs can be immediately rescaled to any resolution dynamically and/or aggregated as inputs

We have edited the manuscript to highlight its novel contributions throughout.

We have revised the comparison as follow (Lines 616-623)

"Bates et al. (2021) achieved CSI values of 0.69 and 0.82 for a 100-year return period flood model of the conterminous United States at a 30m resolution. It must be noted that direct comparisons between the works listed here and this study must be viewed with caution, due to differences in methodologies, assumptions, data sources, data availability, and return periods between the studies. Furthermore, the extent comparison scores are not necessarily objective measures of performance of the simulation model. They can vary depending on the severity of the flood, catchment characteristics, and quality of the benchmark data (Mason et. al. 2009, Stephens et al., 2014, Wing et. al. 2021)."

Reviewer 2 Comments:

1. The authors have provided detailed responses to reviewers' comments and have made substantial revisions to the paper. The main issue (identified both by myself and the other reviewer) was the lack of novelty of the work. They have clarified the novelty of the proposed approach, which has more to do with managing efficiently big data than with improving large-scale flood modelling per se. Perhaps the title of the paper should be changed to reflect this, i.e. "Dealing more efficiently with big data through the use of Discrete Global Grid System Framework: a case study on flood risk modelling".

We have revised the title of the paper as follows (Lines 1-2):

InundatEd-v1.0: A HAND-based flood risk modeling system using a Discrete Global Grid System

2. There are currently several initiatives in Canada that will result in revised flood maps for very large territories (for example Info-Crue in Quebec which started in 2018 and aims to provide flood maps based on hydraulic modelling for over 25,000 km of rivers by 2023), so it is not clear how this DGGS would be used in practice. Could it serve to store the catalogue of flood simulations that would be produced by each province? HAND flood simulations are useful for visualization purposes, but it is not obvious that they can be described as "reliable flood risk maps" (p. 6, line 153), i.e. the type of maps that can be used in legislation for land-use planning. Furthermore, making flood risk information "more accessible" (p. 6, line 159) is also already achieved in many European countries (e.g.

https://flood-warninginformation.service.gov.uk/long-term-flood-risk/map) so it is not clear why DGGS is needed to convey this information for the general public. I therefore remain not entirely convinced that there is sufficient novelty to justify a publication.

There are indeed renewed initiatives to address the dearth of up-to-date flood risk maps in Canada currently underway, including a large amount of work stemming from the Global Water Futures project. However, the type of product as was linked to showing long term flood risk in the UK does not exist in Canada nor will it be the outcome of current efforts to improve flood risk mapping. Further, these maps show precomputed static flood properties, rather than dynamically updated / interactive outputs generated by our system. The novel computational framework we describe enables interactive, generic, large scale implementation of flood risk, something we have not seen elsewhere.

3. The Introduction could be shortened by removing detailed information on the impacts of floods and general statements on flood modelling (first two paragraphs). Note that the text specified in the answer to my comments is not the same as what appears in the revised manuscript. For example, on p. 5, line 127, it is stated that "the novelty of this study is twofold", whereas in the response to reviewers, it is stated that "the novelty of this study is threefold". This gives the impression that it was not obvious for the authors to determine what were the novelties in this study…

To reduce the length of the Introduction, we have removed the first paragraph and reduced the length of the second paragraph (Lines 39-46, copied below):

"The practice of flood modelling, which aims to understand, quantify, and represent the characteristics and impacts of flood events across a range of spatial and temporal scales, has long informed the sustainable management of watersheds and water resources including flood risk management (Handmer, 1980; Stevens & Hanschka, 2014; Teng et al., 2017, 2019; Towe et al., 2020). Flood modelling research has increased in response to such factors as predicted climate change impacts (Wilby & Keenan, 2012) and advancements in computer, GIS (Geographic Information Systems), and remote sensing technologies, among others (Kalyanapu, Shankar, Pardyjak, Judi, & Burian, 2011; Vojtek & Vojteková, 2016; Wang & Cheng, 2007)."

Regarding the discrepancy between the manuscript and reply, we apologise for the incorrect update of the reply. We combined the first two point which involves both DGGS and big-data architecture into a single point in the final manuscript. However, we made a mistake when we updated the reply document.

Comments:

4. p. 4, line 95: Afshari et al. should be 2018, not 2017. The reference (p. 30, line 945) lists this paper incorrectly in the alphabetical order as it starts with the first name (Shahab) instead of the surname (Afshari).

Corrected in the revised manuscript.

5. p. 6, line 133: Define acronyms the first time they are used (here, RFFA). It is not entirely clear what you mean by "without sacrificing the consistency of the framework". Why would other types of large-scale modelling approach become "inconsistent" if they used either RFFA or HAND (or alternative models)?

The acronym of Regional Flood Frequency Analysis (RFFA) has been updated (Line 138).

By "consistency" we meant that the inundation modeling modules doesn't depend on the earlier discharge estimation modules and vice verse. Its possible to replace with another model without breaking the entire framework. In other methods, such as 1-D shallow water equation, the equation can be solved as a routing method for discharge inside the discharge estimation module. However, that type of approach makes it very hard to separate out one module from another.

6. p. 6, line 139: A reference is needed to support the statement that "the IDEAS framework uses an integer-based addressing system which makes it orders of magnitude more efficient than that of other, more traditional spatial data models."

We have added 3 references to support this claim. The relevant text (Lines 143-146) and references are copied below:

"In terms of the tradeoff between model complexity and computation power, the IDEAS framework uses an integer-based addressing system which makes it orders of magnitude more efficient than that of other, more traditional spatial data models (i.e, raster, vector) (Mahdavi-Amiri et. al. 2015; Li & Stefanakis, 2020; Robertson et al., 2020)."

Mahdavi-Amiri, A., Alderson, T., & Samavati, F. (2015) , A Survey of Digital Earth, Computers & Graphics, Volume 53, Part B, Pages 95-117, ISSN 0097-8493, https://doi.org/10.1016/j.cag.2015.08.005.

Li, M., Stefanakis, E. (2020) Geospatial Operations of Discrete Global Grid Systems—a Comparison with Traditional GIS. *J geovis spat anal* **4,** 26. https://doi.org/10.1007/s41651-020-00066-3

Robertson, C., Chaudhuri, C., Hojati, M., & Roberts, S. (2020). An integrated environmental analytics system (IDEAS) based on a DGGS. *ISPRS Journal of Photogrammetry and Remote Sensing, 162,* 214-228.

7. p. 8, line 194: "The vertical accuracy of the DEM is 0.34 m ± 6.22 m, i.e., 10 m at the 90% confidence level". Where do these values come from? A reference is needed, as the reported value appears underestimated since it is significantly smaller than what is stated in other publications on SRTM DEMs (e.g. RMSE of 17.76 m in Mukherjee et al., 2013; 13.25 m in Yap et al. 2019). This is important as later you indicate that the vertical

uncertainty is "small enough to not affect our large-scale flood modelling simulations". Since LiDAR data are available in several parts of the Ottawa watershed, it would be straightforward to run tests on slope estimated from the SRTM in certain reaches to see how they compare with LiDAR estimates.

The vertical accuracy values are sourced jointly from the DEM metadata, the source webpage at the open.canada.ca website (Canada Centre for Mapping and Earth Observation (2015)) and the publication Beaulieu & Clavet (2007) on behalf of Natural Resources Canada (the data provider). The latter has been added to the manuscript. The revised text (Lines 201-202) and new reference are copied below:

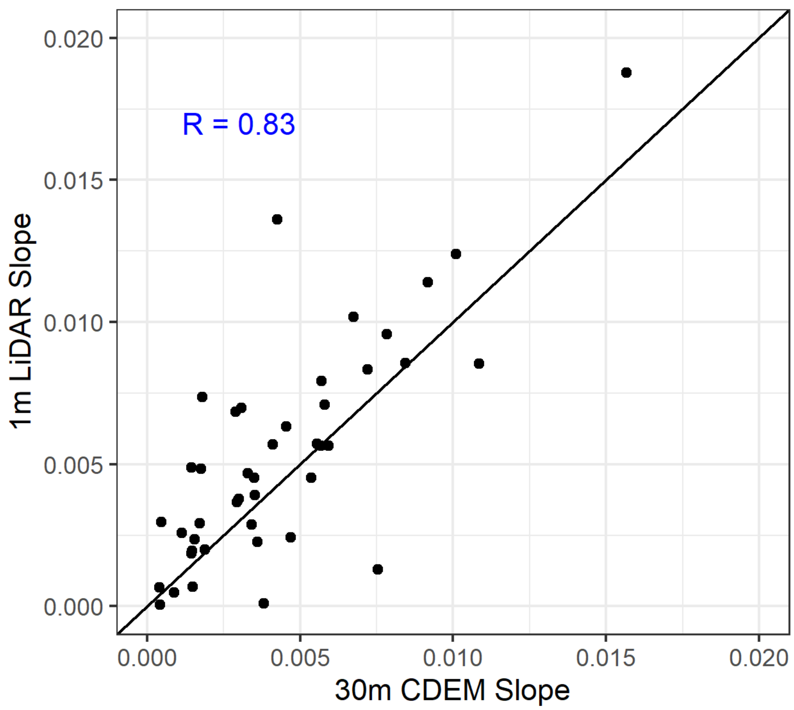
"The vertical accuracy of the DEM is 0.34 m ± 6.22 m, i.e., 10 m at the 90% confidence level (Beaulieu & Clavet, 2007)."

Beaulieu, A., & Clavet, D. (2007). Accuracy Assessment of Canadian Digital Elevation Data using ICESat. *Photogrammetric Engineering & Remote Sensing, 75(1),* 81-86.

We did some comparison of river slope of 44 catchments against the 1m lidar redrived DTM. The comparison is reasonably consistent with a correlation of 0.83 (please see the figures below).

Figure 1: The location of the tested catchments.

Figure 2: Comparison of the river slope from 30m CDEM and derived from 1m LiDAR data.



8. p. 8, line 204: As indicated above, comparing slopes obtained by LiDAR and SRTM DEMs over a few reaches would have been relatively simple to do. In fact, vertical accuracy could have been significantly improved by working with a 30-m aggregated LiDAR DEM (where available).

We understand that use of high-resolution DEM or aggregated high-resolution DEM can potentially improve the quality of the simulations. However, merging two differently processed DEM creates additional problems during delineation. Also, we wanted to be consistent in our entire method with a single dataset. Therefore, we used the CDEM 30m dataset.

9. p. 8, line 215: The reference cited here (Comber and Wulder, 2019) doesn't mention Manning's n and thus does not seem appropriate to justify the statement that each pixel is attributed a Manning's n value based on land use/land cover attributes. Considering the uncertainty with HAND, it is not obvious that using a spatially varied Manning's n in the floodplain provides a major advantage over approaches using constant values (e.g. n = 0.035 in the channel, n = 0.1 in the floodplain, Fleishmann et al., 2019).

The reference (Line 223) is replaced with

Brunner, G. W. (2016). *HEC-RAS River Analysis System 2D Modelling User's Manual Version 5.0.* (Report Number CPD-68A). US Army Corps of Engineers Hydrologic Engineering Center.

We agree that the use of spatially varied Manning's n would have marginal effect on the simulation quality in contrast to use static Manning' n values for both channels and flood plain.

However, the forests, which account for the majority of the Ottawa River watershed's land (~73% on the Quebec side) (Table 1, Environment and Climate Change Canada, 2019), may have higher roughness than 0.1 (we have used 0.16). In a large watershed it may be worthwhile to use the spatially varied Manning' n. Fleishmann et al., 2019 have used parameterized rectangular cross-section but in our case we have used natural channel geometry itself. It may not be an exact comparison.

10. p. 19, line 531: Are there really braided rivers in the Ottawa watershed? Do you mean in cases where there are islands, resulting in anabranch channels?

Yes, we meant at the downstream of the Ottawa watershed where there are several islands and anabranches in the channel.

11. p. 21, line 612: Afshari et al. (2018) instead of Afshari (2017).

The reference is corrected in the revised manuscript.

12. p. 24, line 693 : Figure S6 (instead of S7). I don't think a figure is needed for this – the fact you had to add 4 seconds to the DGGS makes this figure particularly confusing. The main interest of the proposed methodology is clearly in its efficiency in managing big data, rather than in modelling accurately flood zones, as was pointed out on p. 23 ("InundatEd model allows for the "swapping" of various flood modelling methods, and thus could easily accommodate, for instance, shallow water equations").

The Figure S6 has been removed from the supporting material. The associated text has been edited to describe the runtime comparison without reference to Figure S6 and is copied below (Lines 710-716):

"There is a distinct contrast of runtimes between the DGGS method and those using a traditional, raster-based method for sub-catchments within the Grand River Watershed (n= 306 for each method) during the generation of respective RP 100 flood maps. The DGGS based storing and processing method is order of magnitude faster than processing the HAND and catchment boundaries using raster and vector format. The mean runtime using the DGGS method (0.23 seconds) was significantly lower than the mean runtime using the raster-based method (3.98 seconds) at both the 99% confidence intervals ($p < 2.2e-16$)."