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Interactive comment on "InundatEd: A Large-scale Flood Risk Modeling System on a Big-data – Discrete Global Grid System Framework" *by* Chiranjib Chaudhuri et al.

Anonymous Referee #2

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The authors present a detailed paper on the coupling of Regional Flood Frequency Analysis (RFFA) to a simple Manning's-based Height Above Nearest Drainage (HAND) conceptual model. While the efforts of the authors with respect to computer science are likely commendable, there are a number of issues with the hydrologic science that should preclude publication in its present form. I sincerely hope the below feedback is a valuable tool in the reformulation of this analysis and its write-up.

1. The paper contains much extraneous detail and a number of unnecessary figures, creating a long paper that is difficult to follow in places. Consider which information the reader requires to understand your model, how it works, and how it performs. For

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Discussion paper

instance, equations 2 and 8, figure 6, parts of section 2.2.2 and 3.1.

2. The novel aspects of this framework either do not exist or are inadequately emphasised. The presented RFFA does not appear to be much different to Hailegeorgis & Alfredsen (2017). Much of the Canadian RFFA literature by the likes of Taha Ouarda and Donald Burn is omitted. Advances in large-scale RFFA have been presented in, for instance, Faulkner et al. (2016, doi:10.1080/07011784.2016.1141665) or Smith et al. (2015, doi:10.1002/2014WR015814) and so the authors should be clear about what is novel about their approach. Similarly, the use of HAND in flood inundation prediction is well documented and so the authors must make clearer what is novel about their approach. Again, key literature on this front such as Afshari et al. (2018, doi:10.1016/j.jhydrol.2017.11.036), Liu et al. (2018, doi:10.1111/1752-1688.12660), and Zheng et al. (2018, doi:10.1111/1752-1688.12661) is missing.

3. The limitations on the functionality of the presented model are inadequately discussed. How does the requirement for quality river gauge data with long records impair the ability to deploy this model at large scales elsewhere? The limitations and inaccuracies of 'planar' models such as HAND are well known, but this is not discussed to any meaningful degree. The suggested literature above, amongst others, shows how physics-lite modelling approaches often correspond poorly with observations of flood inundation.

4. The inferences made arising from model validation results are often unsupported. For instance, the wild overprediction in Figure 11b is not unusual for models not grounded in a derivation of the shallow water equations. Where the benchmark flood is not valley filling and takes place in a wide, flat floodplains – as seems the case in this panel – the failure to simulate the flow of water can often lead to overprediction. Instead, the authors suggest grid resolution may be the issue. I suggest a more in-depth analysis in this section with evidence for the conclusions drawn.

5. The lack of a requirement for channel geometry is not clear to me. An understanding

of how much flow remains in-channel, which would have no meaningful representation in the DEM, would surely create a much more accurate model. Indeed, I do not know how one can hope to simulate floods such as 1.25, 1.5, 2.0, 2.33 year recurrence (most of which would presumably remain in-bank) without understanding channel conveyance. I think this needs to be further unpacked.

6. Damage computation is mentioned, but not demonstrated or tested. Consider dropping this component or illustrating a use case – as presently there is no scientific contribution on this front.

7. Validation results require much further explanation and contextualisation (grounding in literature). For instance, I have no idea what to take from lines 472-477. The Matthews Correlation Coefficient is not widely used and so makes contextualisation impossible. The reader needs more help in understanding what the validation results mean. Equally, it is unclear whether the authors have made errors in their computation or whether the MCC is unfit for purpose: any metric which rewards figure 11b higher than 12b is clearly not doing its job. Consider a more widely used metric so the reader can understand, to some extent, how your model compares to others in this field. Secondly, consider the purpose of the validation in the context of the purpose of the model. What is the point of your model? What is it meant for? How good does it therefore need to be? If you are computing damages, your benchmark may be higher as this requires accurate depths – if so, test how well the model replicates depths.

8. Some of the (necessary) figures require improvement. The colour scales on figures 9 and 10 make it difficult to discriminate 'good' from 'bad'. There is no key on the depth grid for figures 11 and 12, but should just be made a single block colour anyway as this is a binary comparison.

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