Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2020-242-RC1, 2020

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "An efficient modelling approach for probabilistic assessments of present-day and future fluvial flooding" *by* Hieu Ngo et al.

Anonymous Referee #1

Received and published: 2 November 2020

The authors present a study investigating the flood hazard for Can Tho city, the largest city in the Mekong Delta (MKD). The authors consider the hydrodynamic setting of the delta by linked 1D-2D models, in order to simulate the flood propagation within the city. The 2D inundation model is driven by water level boundaries derived from a simplified large scale hydraulic model of the MKD. This large scale model is itself driven by discharge time series derived by a stochastic streamflow generator. Additionally the impacts of climate change on discharge and thus water level, sea level rise and land subsidence on flood hazard were estimated. The manuscript is generally well written and structured, but here are some substantial issues that need to be dealt with. I list my major general concerns in the following. More comments are included in the attached

annotated manuscript.

1. The proposed method is neither new nor innovative. The combination of 1D for large scale flow dynamics with 2D detailed inundation dynamics has been used in many other studies worldwide, e.g. Falter et al. (2016); Metin et al. (2018); Vorogushyn et al. (2010), the cited Davidsen et al. (2017) and many more, but also in the MKD: Apel et al. (2016). The simplification of the 1D model to gain faster simulation times is not sufficient to claim a new modelling concept as the title suggests. This simplification has also some drawbacks like the very likely insufficient performance for the many smaller channels and the floodplains in the MKD. This has not been shown here, but the neglect of these features in the simplified model will inevitably cause this. The simplified model is thus a model that is tailored for this particularly purpose only, i.e. the simulation of water levels along the main channels of the MKD. This has not been mentioned in the MS, but should be.

2. The authors fall short in properly citing and discussing the available literature. Apel et al. (2016) performed an almost identical study in the MKD, for the same city, even the very same district of the city, with a similar model setup (combination of a 1D model for the whole delta and a 2D model for the city), and using in parts even the same data (DEM). The study was even published in the same journal as this discussion paper. Under this circumstances neglecting this study is a serious breach of proper scientific conduct and cannot be accepted. The authors need to address the previous study and highlight the scientific advances made by their study, or provide a comparison of the results, or address any weaknesses in the previous study, if they see any. Comparing both studies and considering comment 1, I suggest that the authors focus on the estimation of the changes in flood hazard by climate change, sea level rise and land subsidence, which is not performed in Apel et al. (2016).

3. I have serious concerns about the statistics used in the study. Firstly, the authors use an input (Kratie) discharge time series of 7 year only. This is clearly not sufficient for a statistical evaluation of return periods. In this context it has to be noted that for Kratie

almost 100 years of daily discharge values (since 1924) are publicly available through the Mekong River Commission. Why did the authors did not use this valuable data source? This is not comprehensible. The authors claim that the insufficient time series for extreme value statistics is compensated by the (stochastic?) synthetic streamflow generator. I have serious doubts that this is the case. Any streamflow generator has to make some assumptions on the discharge statistics in order to provide information about extreme events not contained in the time series used. But these statistics will be highly uncertain due to the shortness of the time series length. And if the streamflow generator if based on resampling of 7 years of data only, the extreme events will surely be highly underestimated. In any case the authors have to provide more details about the streamflow generator and the applied methods and assumptions, and provide prove or convincing arguments that the generated synthetic time series are statistically representative for the "real" time series and discharge variability at Kratie. Tis is crucial for the validity of the results. Moreover, I urge the authors to use the full length of time series of Kratie. Using this will enhance the statistical soundness of the analysis, and enables an estimation of the representativeness of the generated 1000-year time series based on shorter time series, and if this approach would be comparable or even superior to the bivariate extreme value statistics by (Dung et al., 2015) used in Apel et al. (2016). Another aspect concerning statistics is the poor fit of the Gumbel function to the synthetic water level time series at Can Tho shown in Figure 6. There is a large mismatch between the empirical quantiles and the distribution quantiles. If such a long synthetic time series is used, the distribution function should model the time series almost perfectly. Otherwise the empirical quantiles should be used. The use of the Gumbel distribution function for estimating quantiles is in this case a loss of information. See also my comment in the annotated manuscript. To sum all these comments up, I have serious doubts that the probabilities associated to the discharges (Kratie) and water levels Can Tho) are robust.

4. The authors try to validate the 2D hydraulic model by two water depths in manholes only. I am missing a validation or at least a plausibility check of the spatial inunda-

tion simulation. The performance of the spatial inundation simulation is crucial for the hazard analysis, thus some analysis or at least arguments should be provided. As mentioned above, there are more (and mostly more specific) comments are provided in the annotated manuscript.

References

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Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2020-242/nhess-2020-242-RC1-supplement.pdf

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-242, 2020.