

Interactive comment on “A methodology based on numerical models for enhancing the resilience to flooding induced by levee breaches in lowland areas” by Alessia Ferrari et al.

Anonymous Referee #2

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The paper presents an interesting contribution to the journal, offering a novel approach to improving resilience to flooding and increasing preparedness to face levee breach-induced inundations. However, I have some major concerns related to the current version of the manuscript. Overall, some parts lack clarity, and this might cause some confusion while reading the article. Also, English needs polishing before the paper is ready for publication.

As the paper deals with lowlands and human modifications (levees), it should be important to include some contextual works on the role of levees and embankments to contributing to flooding (i.e. Black 2008; Munoz et al., 2018) and on the importance

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of the artificial drainage network and landscape changes in contributing to floods (i.e. Wohl 2019a,b; Pijl, Brauer, Sofia, Teuling, & Tarolli, 2018; Sofia et al., 2019). The discussion of the results should also be framed into this wider context. Currently, it is much focussed on the technical domain (computer requirements, time for simulation etc), but the paper would benefit a wider audience if the results were framed into the larger picture of lowlands and flood risk.

A greater concern emerges for the paper structure. In my opinion, the paper structure is very confused, and the chapters are currently disorganized proposing a mixture of literature review, method description, and results altogether. There are a lot of references to what should or should not be done, according to a literature review, rather than a focus on the novelty of the proposed approach, and this makes the text hard to follow. The paper should at first describe what the RESILIENCE project is (beginning of chapter 3) and then describe the methodology proposed in this paper (i.e. ParFlood and why it is novel/Accurate), and further proceed to describe the setting for the current simulation. Currently, much of the description is about previous works and all possible approaches, but this 'distract' from the description of the actual method proposed. The authors should consider rewording the text, so that it is clear what are the novelties and strengths of this work, as compared to past ones.

Within the methods, also, a lot of parameters are case-specific and it is not clear how they should be 'tuned' for further application of this approach in different study areas. For example, it is not clear what the 'hydrological scenario' are. Do they come from simulated flows? do they come from actual data? if they come from simulated flows, how are these accomplished? Also, the choice of the return period for inflows A and B is not clear. was this return period previously analyzed and identified? how? [this latter confusion probably emerge from some lack of clarity in the manuscript]. Should the parameters be optimized for future studies, if so how?

A further issue is that the authors state that 'Compared to previous studies on flooding induced by levee breaches, the proposed methodology benefits from the adop-

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tions of an accurate and fast numerical model and of high-resolution meshes', but the manuscript does not present any actual comparison with previous studies, but it only showcases a literature review on them.

I believe addressing these issues would add value to the paper, and would make this work useful to a wider scientific audience.

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