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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.

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The authors gratefully acknowledge the positive and constructive review of the anonymous Referee. In this document the comments provided by the Referee are reported in italic, whereas the authors' response and indications about the original paper modifications are marked in bold fonts.

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

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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.

We thank the Reviewer for his comment. The entire manuscript will be carefully revised.

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 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.

We agree that flooding events represent a crucial task for different research branches. Therefore, in the revised manuscript, we will reword both the introduction and the discussion section in order to better discuss the role exerted by levees on the flooding of lowlands. Particularly, we will deepen the description of the levee-effect problem and we will add references to the mentioned contributions.
As regards the drainage network, we will add some discussion about the fact that the drainage networks only influence the flood dynamics at a local scale, for example by defining preferential pathways. However, in the scenarios here considered, the flood volume (in the order of 10^7 m^3) largely exceeds the discharge capacity of the drainage systems. It is also relevant to notice that

most of the minor channels are equipped with gates that are kept closed at the passage of huge flood waves in the river, and hence they do not contribute to the drainage of the flooded volume until the end of the event. As a result, these networks are not expected to significantly contribute to the flood dynamics, and hence they were neglected in the terrain description, also to avoid the excessive increase in the number of computational cells.

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.

We appreciate this suggestion and accordingly we will modify the structure of the manuscript in order to clearly distinguish among literature review, the presentation of the RESILIENCE project, and the results obtained by applying the proposed methodology to the study area, and to highlight the novelties of the approach.
In particular, we will start by presenting the RESILIENCE project and the methodology in general, and then we will describe the application to the case study, which will also allow us to further discuss the strengths of this work.

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?

We thank the Referee for this useful comment that allows us to clarify some further aspects of the proposed methodology. In the revised paper we will better stress that the methodology is general and that it can be applied to any leveed river. Therefore, we will explain that the values of the breach parameters adopted for the pilot area (i.e. location point, width, evolution time) are provided as examples, and this does not prevent the extension of the RESILIENCE project to different areas.
In this context, we will also clarify the role of the hydrological scenarios (A and B in Sect. 3.2) in the creation of the database of flooding scenarios. In short, the discharge hydrographs adopted as upstream boundary condition are simply synthetic design hydrographs with assigned return period, derived from previous hydrological studies, which in our case were provided by the Po River Basin Authority. Please notice that for most rivers such hydrographs are already available, often employed for creating flood hazard/risk maps according to the EU Floods Directive. We performed preliminary simulations of the propagation of floods with different return periods (e.g. 20, 50, 100, 200 years), and identified the one corresponding to incipient overtopping: this was inflow A. For example, inflow A was the 50 years-hydrograph for the Secchia River, and the 100 years-

one for the Panaro River. Then, a lower return period hydrograph (20 years) was chosen in order to consider levee collapses due to piping or other mechanisms, during a flood event that does not induce overtopping: this was inflow B.

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 adoptions 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.

The Referee is right. The paper does not compare the results of the RESILIENCE methodology with those of previous studies, and the sentence "compared to previous studies .." was adopted to refer to literature studies. We agree with the Reviewer that this can lead to misunderstanding, hence we will correct this sentence in the revised paper accordingly.

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

The authors wish to thank the anonymous Referee for his positive overview about the manuscript.

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