

## Review of NHESS-preprint egosphere-2025-6361 by Perk et al.

In their study the authors tackle the computational challenge of simulating large flooding events with detailed 2d-hydraulic models. They present two approaches to reduce computational requirements. First is the flow-based subdivision of the model domain in smaller - partly independent subdomains, allowing (at least partly) the parallel processing of the subdomains and hence potentially lead to a reduction of the computation time estimates of the simulations. The second approach is a grid refinement technique allowing to only use fine grid structures in regions of importance (e.g. preferential flow pathways or the surroundings of buildings) while other regions can be simulated in a much coarser resolution. Finally, they apply both approaches in a combined way for simulating flood events on a mesoscale region where they use the gain in computational efficiency to conduct a series of sensitivity studies on the impact of simulation setup as well as variation in precipitation fields on the resulting inundation depth.

Generally the topic of the study is very relevant and certainly of interest to readers of NHESS. Especially in the light of real-time forecast of inundated areas in case of pluvial flood events computationally efficient and fast (real-time forecast) detailed simulations of flooded areas are key. Also in the light of flood vulnerability assessments, large scale (on district, state or country level – or even beyond) information of pluvial flood hazards are required. Hence, smart ways to parallelize model domains as well as methods to generally reduce the computational needs are required.

Nevertheless, the present version of the manuscript is in my opinion not sufficient and has to be substantially improved to match the quality criteria of NHESS. Below I raise my major concerns related to the presented manuscript. I think they are serious enough to recommend, that the manuscript needs a major work-over.

### General comments:

- General structure of the paper – in my opinion the manuscript lacks a clear structure. After the introduction the authors describe the study area. Then they include a section on the basics of 1/2d-hydraulic models basically presenting text book knowledge which I would question if this is needed in the detail presented. This section is then followed by a section which could be called a method section. Afterwards some results are presented, but also details on the simulation setup are given. This is then followed by a section on stability – again partly methods, partly results. And finally there are some summaries, although they are called discussion and conclusions.  
So I would suggest to introduce a method section as well as a section clearly defining what kind of simulations have been conducted (including setup, input data etc) and for what purpose. I additionally would strongly recommend to streamline the simulation setup, since in the current version of the manuscript basically each part uses different precipitation characteristics. And finally please introduce a section where the results are presented and not a mix of method, simulation setup and results.
- Introduction – this section certainly needs to be streamlined to be more focused on the target of the study. In large parts it reads like a series of more or less random quotes of a set of papers without a clear link between them.
- Presentation of results – there is a general need to improve the description and presentation of the findings of the conducted analysis. For most of the figures, I have a hard time to see any differences between the simulations (e.g. Fig 4, lower left panel, Fig 5, upper panels, Fig 7, Fig 8 almost all panels, Fig A1). I would suggest to rethink if maps depicting the absolute/relative difference are of any use considering the scale of the model domains.

- Discussion of results – the discussion section is not a discussion section but a summary (actually followed by a further summary named conclusion). This definitely needs to be improved. The study is neither the first study working with the parallelization of model subdomains nor the first study using local grid refinement. Hence, there is a need to place the outcome of this study in the context of the findings of already existing studies. So to make it short – the outcome of the discussion section should give a clear answer to what actually the ‘breakthrough’ is, which the authors claim that the present study has made and which was not already there before.

### More detailed comments:

- Lines 134 ff: What is the added value of mentioning mean climate characteristics of the study domain. I do not think that this information is of any relevance for the presented study, hence I would remove it completely.
- Lines 143 ff: Again, what is the added value of the CatRaRe events? All simulations are conducted based on relatively short (3h) design storm events. What information does the reader gain knowing that there has been a series of events with a duration of 24h or even 48h – if these events are not within the scope of the study.
- Lines 155 ff: I have the feeling that eq 1 and 2 are identical, describing the flow in X direction. I actually anyway would have only expected two equations.
- Line 186: Although I know what SCALGO is, there is a need for a reference and a short description here.
- Line 310: 50 mm in 3h is about a 20 yr event when using KOSTRA (V 2020) as baseline. What was the motivation for this rain amount and duration? Would the deviations in the model results increase with increasing precipitation?
- Lines 345 ff: Here 30 mm in 3 h (~3yr event) are used. Why this number and not again 50 mm in 3h or why not always using a 100yr event like it is used later? Again, what would be the impact of the input on the deviations in the model simulations?
- Fig 5 - upper panels - maybe scatter plots would be better. In section 4.2 it is mentioned that the mesh refinement was conducted on streams, roads, buildings, culverts and model boundaries – why only refinement of streets and streams is included in the figure? Additionally, what do the authors refer to when they state as panel heading: ‘Lower is better’? (actually also included in almost all other figures). If we look at the computing time it certainly seems that the runs with the lower (coarser) resolution are performing better – but I doubt that this is the case for the other results.
- Lines 457 ff: The description of simulation setup and inputs is a complete mess. According to KOSTRA, a 100yr event for 3h roughly is about 65 mm. Where do the other inputs (up to 150 mm/3h) come from. What is actually the temporal assumption of the rainfall input. Additionally, what about the role of soils. Looking at the large scale of the simulations (as well as the substantial part of the domain covered by forests and agricultural land) what were actually the assumptions of infiltration. And how is the variation on infiltration ability of soils due to different preconditions reflected in the simulation setup?

- Fig 8 – Looking at the upper right panel, I have the feeling that the largest rain amounts (yellow color) are leading to the least flooded areas. Does this in anyway make sense or what actually is shown in the respective panel? Bottom row – left panel: Are the regions the same as the regions identified by the subdivision process? Right panel – if I understood the setup correctly, 150mm/3h were applied to different subregions. This is a huge amount, hence I would expect to see many flooded areas marked in blue/turquoise color - where the precipitation input was provided over almost all of the domain – why is this not the case?
- Lines 484ff and Fig 9 – Here the authors use 50 mm over 4h. Why was this chosen and how would the result differ, if the simulation setup would be similar to the previous setups. Additionally Fig 9 indicates only one critical point when using base resolution 8 and 1m, respectively. Is this the only critical point in the domain? What, if a grid refinement method would have been used (which actually was the scope of the study) – would the result be the same as for the 1m base resolution case?
- I do not provide detailed comments on the discussion and conclusion sections, since both should be completely worked over.