Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-597-RC1, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Performance of automated flood inundation mapping methods in a context of flash floods: a comparison of three methods based either on the Height Above Nearest Drainage (HAND) concept, or on 1D/2D shallow water equations" *by* Nabil Hocini et al.

Francesco Dottori (Referee)

francesco.dottori@ec.europa.eu

Received and published: 22 December 2020

The manuscript describes the application of three different modelling approaches to map flash flood hazard in three case studies in South France. The topic is undoubtedly worth of interest, considering the potential for near-real-time applications and the possibility to include flash flood impacts in future applications. The manuscript is well structured and reasonably well written. The authors perform a detailed analysis of the

model results, including the main types of error found, and this gives the reader a comprehensive overview of the strengths and limitations of each method. In my opinion, the paper will be ready for publications after a moderate revision to correct a few issues.

Main points

L203: "The simulations are all run in steady state regime based on estimated flood peak discharges for each river reach. This leads to neglect the possible dynamic effects related to the inundation of floodplains occurring in unsteady flow regimes. This assumption is considered here as reasonable since the width of the floodplains do not exceed several hundred meters, and the volumes of the floodplains remain limited if compared to the volumes of the floods." I think that the limitations due to this modelling choice need be better explained. Based on the results, this seems indeed a reasonable assumption given that there is no general overestimation for the 1D and 2D models. Still, simulating a steady flow regime using peak flow implies an overestimation of total flood volumes, compared to a real flood wave with increasing and receding limbs. As such, this point should be mentioned in the discussion because it might originate errors in case of flood events where flood volumes are small compared to the floodplain extent. Moreover, steady flow simulations have limitations when modelling the interaction of flood waves at confluences. The underlying assumption is that flood peaks are occurring at the same time (a sort of worst-case scenario), while in reality peaks might occur at different times. This point should also be mentioned.

Section 5.3 . The presentation of run times would be even more informative if the authors could make a more quantitative comparison with run-time required to actually set up a real-time flood simulation. Often, reliable weather forecasts of flash flood events are available only few hours in adavance, meaning that a real-time simulation should be available to emergency responders in ,say, 2-3 hours to be effective and helpful. Considering the usual speed-up attainable for 2D hydraulic models (see for instance Neal et al., 2018, https://doi.org/10.1016/j.envsoft.2018.05.011) this seems to be feasible objective, provided that the Floodos model can be parallelized in a similar

way. Could you please elaborate a bit on this?

Conclusions: I suggest to elaborate a bit more the discussion on real-time applications, given its importance. In my opinion, real-time applications are meaningful only with the 1D or 2D hydraulic models, which are both able to simulate flood waves in unsteady flow conditions, including the interaction of flood waves with different timings at confluences. On the contrary, if the steady-state approach is deemed appropriate, then an off-line catalogue (similarly to what done by Dottori et al., 2017, https://doi.org/10.5194/nhess-17-1111-2017) would probably be enough. I would be interested in reading the opinion of the authors on this point. In addition, I suggest to mention the possibility of using the described methods to evaluate flash flood impacts (see the recent works by Merz et al., 2020, https://doi.org/10.1029/2020RG000704; and Ritter et al., 2020, https://doi.org/10.1016/j.envint.2019.105375)

Minor issues

The Title is maybe a bit redundant, consider shortening , e.g.: "Performance of automated methods for mapping flash flood hazard: a comparison of hydrodynamic and geomorphologic methods" or something similar

Abstract L13-14: "With these methods, the inundated areas are overall well retrieved..." Here I would suggest replacing the qualitative evaluation with some quantitative metrics, as done for the water levels

L 19 "Flash floods represent a significant part of flood related damages worldwide". Do you have a quantitative assessment of the share of flash flood damages, for instance in France? You might for instance look at the HANZE dataset by Paprotny et al (2018, https://doi.org/10.5194/essd-10-565-2018)

L 37: "For instance, in France it is estimated that a river network of about 100.000 km should be documented for a comprehensive coverage of the small streams". Is there a reference for this statement?

L62-69: This paragraph doesn't read well due to many references and lists of models. Please try to rearrange the information (e.g. I would put first the sentence "All these methods determine a local discharge/height relationship from..." and then "These methods are applied either directly from the DTM for the AutoRoute method...")

L83: "A significant evaluation and validation effort is proposed..." Maybe better rewrite as "A comprehensive evaluation and validation exercise is proposed..."

L88-90 Please replace "first section", "second section" etc with "Section 2", Section 3" etc

Title of Section 2: I'd rather use "description" than "presentation".

L103-104: "A conventionnal Dinf approach is used here instead of the Geonet approach used in GeoFlood." Could you please either specify the difference or provide references for the two approaches? Also, correct the typo (conventional)

Section 2.2: I suggest renaming the approach as CaRtino-1D HECRAS ,given that HECRAS is the actual hydraulic model applied.

L116: please provide a reference for the Mascaret model here (or remove the mention if not relevant for the study)

L125-126: "Its main limits, already identified in previous works, lie in the 1D scheme which may not be adapted in areas with complex hydraulic features". Please name some of these works here.

L 140: "The model has been compared with the widely used 2D LISFLOOD-FP model (Bates et al., 2010), showing equivalent results and faster computation times." Were these tests performed by Davy et al. as well? Please specify also the reduction in computational time as compared with LISFLOOD-FP.

Section 4 L243-248: This paragraph and Figure 4 might be better placed in a separate subsection after subsection 4.1

Figure 4: it is not clear where these two areas are located within the study area, Please add a smaller map of the study area showing the location of the two boxes

Figure 8: Is it simulated water level in panels b-d?

Figures 8 and 9: I assume that you are using Floodos simulations here right? Please specify this in the text and captions

Section 5.2.1: Accounting for protection structures is indeed a major challenge in any large-scale flood risk assessment. Could you tell how much of the study areas is protected by dykes or other defence structures?

L358 typo: feasibility of reasonably accurate

L375 "The sensitivity to roughness values has also to be further investigated for an appropriate representation of uncertainties". Using variable roughness values according to land cover could be an option for future studies. This is actually a standard practice for large-scale flood models (see Sampson et al., 2015, https://doi.org/10.1002/2015WR016954; and Dottori et al., 2016, https://doi.org/10.1016/j.advwatres.2016.05.002)

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-597, 2020.