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Interactive comment on "Brief communication: Comparing top-down and bottom-up paradigms for global flood hazard mapping" *by* Giuliano Di Baldassarre et al.

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This Brief Communication proposes an useful comparison of two main paradigms for global flood risk modelling, and it will probably raise the interest of researchers and practitioners working on the topic. The manuscript has a clear scope and it is generally well structured, event though some sections could be improved. At the same time, while I understand the constraints of the manuscript format, I believe that some statements and conclusions need a bit more discussion and should be supported with adequate references.

General comments

Printer-friendly version

Discussion paper

1) I'm not fully convinced by the proposal of using the terms "top-down" (TD from now on) and "bottom-up" (BU from now on) to define the two modelling approaches. Is it a novel definition by the Authors, or does it come from previous literature? I'm not in principle against such definitions, but I would like the Authors to bring more compelling reasons for their use. Considering how these two terms are used in other contexts (e.g. in decision-making processes), the analogy proposed seems in my opinion misleading. On the one hand, all methods that delineate floodplains based on topograhy still require assumptions and elaborations which need to be consistent everywhere (i.e. the "top" side). At the same time, results of TD models are heavily influenced by topography, perhaps more that by hydrology (i.e the "bottom" side). Therefore, why not use the definitions of "hydrological" and "hydrogeomorphic" paradigms respectively? This would clearly indicate the main driver of each approach and is possibily more consistent with the terminologiy adopted so far in literature.

2) The present descriptions of the two paradigms (Sections 2-3) are not balanced: BU description has a large number of references (possibly not all of them necessary), with some sentences not so relevant in the context of this manuscript (see some specific comments below). In addition, it does not mention any drawback, for TD methods there are some lines on the uncertainty of estimating synthetic flood events. Given that pros and cons of the two approaches are listed in Table 1, my suggestion is to remove them and tighten up the two descriptions.

Specific comments

- Please specify in the abstract and introduction that the paper is about inland flooding only (coastal flooding seem not to be considered here).

- abstract: "resulting in a plethora of freely available products". To my best knowledge, most global scale models, are not freely available at global scale (with the excepotion of the JRC and GFPLAIN models). Are there more BU or TU models applied at global (or at least continental) scale,? If not, please consider rephrasing this sentence.

- Surprisingly, climate change is not mentioned after the introduction, yet it is a crucial field of application for global TD models

- line 26-27: I would write "...a range of synthetic events" instead of "a synthetic event", given that all global models use a range of flood magnitudes to evaluate flood hazard.

- Figure 1. The inset is not clear, it is hard to understand whether urban areas overlap with flood maps. I would suggest to avoid the color scale for water depth and just represent flood extent, possibly with a different color for representing the intersection of flood maps with impervious areas. Also, please put a scale in the inset and add a reference for the GFPLAIN dataset in the caption.

- lines 31: "while hydrodynamic models have been successful in reproducing historical events..." the reference provided is for a local-scale model, which is not so relevant in thix context (it is well understood that a hydraulic model is fit-for-purpose when applied at local scale). I would rather refer to results of large scale models such as in Schumann et al. (2013) and Wing et al (2019).

- line 49 "... identifies flood-prone areas across diverse climatic regimes with varying parameterizations." I think this sentence is not so relevant and could be removed. First, this is true not only for BU models but also for TD models. Second, it seems to partially contradict the statement in Table 1 that "Scaling laws have limitations in dry climates". Looking at Figure 1, my impression is that GFPLAIN is identifying ancient floodplains which are not anymore flood-prone areas, therefore suggesting that topography is not representative of present conditions. Maybe the Authors could add a reference to previous studies describing the performance of BU models in arid and semi-arid climates

- line 59-61 "International development banks (...) humanitarian response use these global maps in data-poor regions for mapping risk hotspots and flood-prone areas (Ward et al., 2015)". To my best knowledge, the paper by Ward et al only makes reference to global TD models . Are there references about the use of BU models in these contexts?

- Table 1: Please add references for the suggested pros and cons, where possible

- Table 1: "variable over time" is listed as a cons for TD models. However, this is also a strong pros, because models can simulate changing conditions (e.g. climate change scenarios, change in hydraulic structures...).

- Table 1: besides the design of risk reduction measures, TD models can be used for risk assessment in general.

- Table 1: I'm wondering whether BU models can somehow estimate flood magnitude. In my understanding, BU models identify a sort of residual risk, i.e. they map all the areas which may be affected by flooding, but withot an explicit representation of hazard frequency and magnitude. I woyld be happy to hear the opinion of the Authors on this point.

- Table 1: "more sensitive to scales": this is not much clear, do you mean that BU approaches have low skill over the minor drainage network? In my view, an advantage of BU methods is their feasibility of application over very fine scales (e.g. minor branches of any river network), especially conidering that the vast majority of global models are limited to major rivers because of data and computational constraints. If it's not the case, please add some further explanation or references.

Conclusions: "flood risk hotspots" should be changed to "flood prone areas" or similar definition, as according to the authors themselves BU models cannot quantify risk

Conclusions: "In this context, this means the identification of flood risk hotspots in data-poor areas should consider both flood inundation areas derived by the two paradigms as depicted in the insert of Figure 1." I agree in principle with such statement, which is in line the idea of using ensemble of models to better represent (at least in part) the overall uncertainty of model estimated. However, such conclusion should be better framed considering the existing literature. I'm listing some recent works below, my suggestion is to incorporate at least some of them (and possibly others, if manuscript constraints allow) in the elaboration of conclusions:

- Trigg et al. (2016) compared flood maps produced by six different TD models in Africa and found more disagreement than agreement, thus suggesting large and mostly unexplained uncertainty in TD model structures.

- Wing et al., (2017) observed that global TD models have rarely undergone testing against high-quality data of commensurate coverage

- Bernhofen et al. (2018) observed that ensembles based on a range TD models had skill in reproducing three past flood events (but adding all models increased noise without increasing accuracy).

- Wing et al. (2019) evaluated the flood maps produced by large scale TD model against a BU model (HAND tool , NOAA 2018) and found significant lower performances by the BU model.

Conclusions: a personal opinion on the precautionary principle (up to the authors whether to use it or not): in my view, the precautionary principle should call for using all existing models, rather than one sample from each of the two paradigms. This is of course valid in an ideal world of free and open datasets, whereas in the real world the selection of global flood maps is in fact limited by data availability.

References

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- Wing et al (2019) flood inundation forecast of Hurricane Harvey using a continental-scale 2D hydrodynamic model. Journal of Hydrology X 4, 100039

- Schumann et al , 2013 A first large-scale flood inundation forecasting model, Water Resources Research, https://doi.org/10.1002/wrcr.20521

- NOAA National Water Center, Boghici, E., Arctur, D., 2018, NOAA NWC – Harvey NWM-HAND Flood Extents (HydroShare). doi:10.4211/hs.fe85a680d0144e79b39e8c483dc1e5aa.

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