Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-468-RC2, 2021

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Space variability of hydrological responses of Nature-Based Solutions and the resulting uncertainty" *by* Yangzi Qiu et al.

Ruben Imhoff (Referee)

ruben.imhoff@deltares.nl

Received and published: 27 January 2021

1 Summary

The manuscript describes the effect of both the space variability of rainfall and the (spatial) implementation of different nature-based solutions (NBS) on the hydrological response of an urban catchment southwest of Paris. This is done for three rainfall events using a physically-based, spatially distributed hydrological model, which was derived on a 10-m spatial and 3-min temporal resolution. The authors clearly show the impact of taking into account, or not, the spatial variability of rainfall. To a lesser extent, they found this for the spatial implementation of the NBS too. The authors then touch upon the effect of the combination of both effects, which may lead to insightful informa-

tion for urban water managers. I found this a relevant urban test case, especially the focus on the added value of a high-resolution, fully-distributed focus in urban hydrology, and I have read it with great interest. From that perspective, I think this manuscript may be considered for publication in HESS. I have, however, also some questions and suggestions to further improve the manuscript. Some are more major than others, but I hope that it helps improving the manuscript.

Below I describe some general comments, followed by more detailed comments line by line.

2 General comments

2.1 Title

The title gives the impression that the space variability of the hydrological response is studied, while the authors have studied the effect of the space variability of rainfall and NBS on the hydrological response. In addition, the authors present a very interesting case study, so perhaps it is good to mention this in title too (e.g. case study for Guyancourt).

2.2 Space-time resolution of the hydrological model

The authors state that the model has a spatial resolution of 10 m, but a DEM with a resolution of 25 m was used. Could the authors explain how this coarser DEM was used to derive the model topography on a 10 m spatial resolution? Moreover, the model was run with a temporal resolution of 3 min, whereas the temporal resolution of the rainfall data for 3.4 minutes. Could the authors indicate how they have downscaled the temporal resolution of the rainfall data? Hydrological model parametrization and

reproducibility of the NBS implementations The used hydrological model Multi-Hydro is shortly introduced in Section 2.3. In Section 3.1, the authors introduce the implementation of the different land use classes and the model parameterization. Although the authors do not have to introduce the entire model (the given references suffice), I would recommend more elaborately introducing the used model parameters and the effect they have, including the different land use classes, on the model. Hence, what can we expect from the given parameterization and land uses classes (i.e, differences in evapotranspiration, interception, storage capacity, etc.)? The same holds for the NBS scenarios in the model and their parameterization (for the green roofs, this was already done by the authors). For the NBS scenarios, also describe how they are implemented and what their expected storage increase, or effect, is. Why are the NBS scenarios applied in the way they are applied (in space, but also the density of the application)? Is this a feasible application or have you chosen this purely as a synthetic experiment? A more elaborate description would highly increase the reproducibility of this study, also with other hydrological models.

2.3 Experimental setup

I think the authors chose for an elegant application by using the multifractal approach. The authors write: “For instance, both ‘hot spots’ (extremes) of the rainfall and NBS are scarce and therefore could rarely coincide, i.e., rainfall spikes may fall more often elsewhere than on NBS. Similar questions can occur for less extreme events. The effective NBS performance could be therefore biased with respect to their potential performance due to this problem of intersection between rainfall intensity and NBS”. This make me wonder if a synthetic experiment would have worked too. In such an experiment, you can exactly define where, how much and when the rainfall falls and the same for the location and density of the NBS applications. This would make it possible to do a larger sample analysis of the effects (the authors point on the need for this in the conclusion). Could the authors comment on this and if they agree that

something like that – or another approach – is possible, write something about it in the discussion?

2.4 Verification metrics

In the methods and results, the percent error is used. However, the authors compare different scenarios and approaches with each other. The percent error is generally used to compare a result with the true value or at least the theoretical value. It gives the feeling of an error, whereas you are not sure which one is right or wrong. Hence, using the percent difference throughout the manuscript may be a better choice.

2.5 The results in a larger perspective

In the last section of the results and the conclusions, I miss a bit the significance of the results. How does this relate to other literature and are the intersection results of importance when compared to the large effect of uniform or non-uniform rainfall inputs? In addition, what can the authors say about the effects of the chosen events on the results? This is of course a case study, but I would encourage the authors to put the results a bit into perspective.

3 Specific comments

Page 1, line 13: “of their hydrological responses sensitively depends”. Do you mean that the hydrological model results depend on the mentioned two processes?

Page 1, line 24: It is not more pronounced, but there is a somewhat significant effect for the two NBS scenarios the authors mention.

Page 1, lines 26 – 27: What kind of flooding do you mean (i.e., flash, coastal, urban, fluvial and/or pluvial floods)? In case of just urban flooding, the sentence is fine like this. Otherwise, there are more essential drivers, such as land subsidence in deltas, deforestation, etc. (add some extra references in that case). Nevertheless, I would suggest to change "The increasing of extreme flooding risks" into "The increase of extreme flood risk" (possibly with an indication of what kind of flood risk).

Page 2, line 37: "parallel concepts". Perhaps I am not familiar enough with the concepts, but what kind of parallel concepts are meant here?

Page 2, line 44 – 45: For completeness, also write why the fully-distributed models are rarely used.

Page 2, line 49: What do you mean by complicated urban catchments?

Page 2, line 54: "Indeed, such models should better assess the hydrological performances of NBS on a smaller scale." Although I agree with the need for such models, be careful with this statement. It stands or falls with the presence of reliable high-resolution model forcing and parameterization (among others).

Page 2, line 55: "lack of high-resolution rainfall data". What is the desired resolution the authors are looking for? I.e., are rainfall estimates from X-band radars, commercial microwave links or personal weather stations a solution? I am aware of the challenge of (reliable, high-resolution) rainfall estimation in urban areas, but be a bit more specific about what the limitation is.

Page 2, lines 59 – 61: This is the first time you introduce the intersection between the spatial variability of rainfall and the NBS. Although I do understand what you mean, I would suggest devoting one sentence here to explain what is meant by that. In addition, what about the time variability of rainfall. What can you say about the importance of that?

Page 3, line 68: "Two different types of rainfall data". The data source is the same, so

I would rather suggest calling it two different rainfall processing approaches (gridded and catchment-averaged).

Page 4, lines 104 – 105: What is the rainfall amount that corresponds to this return period? That would make it easier to relate the studied events to the drainage capacity.

Page 4, lines 113 – 115: Could you tell a little bit more about how this tool works?

Page 8, lines 245 – 251: This is a clear explanation, which would be even more valuable when placed at the start of the section. Page 11, lines 317 – 324: It would be good to place a reference to Nash & Sutcliffe (1970) here.

Page 11, lines 328 – 338: The authors already describe some model results here. This fits better in the results, e.g. as first subsection. Side note, the model indeed performs well for the given study area.

Page 11, lines 339 – 340: Although I do agree with this conclusion, can the authors say something about the model performance (regarding simulated fluxes and/or states) on the grid level or at the sub-catchment scale? On the used high spatial resolution and in an urban setting, I know this is challenging. However, it would further support your conclusions, especially because you are focussing on spatial variability in the results.

Page 12, lines 361 – 363: This is exactly what you expect for the baseline situation. The difference in total runoff volume should not be too different, because the total rainfall volume should be the same for the gridded and uniform rainfall inputs. The small differences are an effect of differences on the grid scale (storage capacity, evapotranspiration, etc.), which are differently modelled when the input is uniform or non-uniform.

Page 14, lines 433 – 437: I think this is one of the most interesting (and important) results of the study. However, it is not very easy for the reader to make the comparison based on the figures (e.g. figures 14 and 16). Could the authors add a subplot to figure 16 indicating the differences between the two scenarios (so the difference between uniform and non-uniform rainfall plus the difference between the scenarios) and tell

somewhat more about it?

Page 15, conclusions: The abbreviations used throughout the text, are also directly used in the conclusions. If you read the entire text, this is clear, but for readers who quickly skim through the abstract and conclusions, I would suggest to (re-)introduce the meaning of these abbreviations.

Page 16, lines 479 – 480: “However, the RG scenarios appear to be less affected by the intersection effects, with a difference lower than 3% on peak flow and lower than 1 % on total runoff volume.” This is indeed supported by the results, but in the results, you also discuss the reason for this small effect on the peak flow and runoff volume. It would be good to include that here too.

Page 16, lines 481 – 485: I fully agree with the authors that this hints towards using fully distributed hydrological models over semi-distributed or lumped models, but that is not exactly what is shown in the results. The authors do not benchmark the result with semi-distributed or lumped models, but rather focus on the rainfall variability and NBS variability on the discharge response. I would like to ask the authors to rephrase this paragraph a bit. Figures overall – Make sure the font size is readable and approximately the same font size is used for all the figures in the manuscript.

4 Technical corrections

Page 1, lines 17 – 19: For readability, I would suggest making two separate sentences out of this one.

Page 1, line 21: “, which is more pronounced than those of the total runoff volume.” Do you mean, “, which is a stronger effect than the effect on the total runoff volume.”?

Page 2, line 30: “results in rainfall transfer into runoff rapidly” becomes "result in a rapid transfer of rainfall into runoff".

Page 2, line 31: “The approach of expanded and upgraded the capacity of the existing drainage system” becomes "Expanding and upgrading the capacity of the existing drainage system".

Page 3, line 62: “such mentioned” becomes "the mentioned" and “over higher spatial resolutions” becomes “for higher spatial model resolutions”.

Page 4, line 109: “a clear tendency towards growing number of somewhat shorter, but much heavier rainfall events, was perceived for this region” Suggested change: “A clear tendency towards a growing number of shorter duration, but higher intensity rainfall events is perceived for this region”.

Page 6, line 184: Remove the ‘)’ before the end of the sentence.

Page 7, line 201: “simulated under both different types of rainfall” becomes “simulated for both rainfall inputs”.

Page 18 and 19, lines 559 – 567: The references are not alphabetical here. Perhaps also at other lines.

Figure 2: I may be wrong, but it seems that the legend colour for forest is not exactly the same as the colour used in the map. Figure 3: The rainbow colour map is not always intuitive, also here with respect to the rainfall amounts. I recommend using a more intuitive colour map. Some explanations and inspiration can be found in Crameri et al. (2020).

Figure 10: The authors refer in the caption to (a) EV1, (b) EV2, (c) EV3, but the letters (a) – (c) are not shown in the figure.

Figure 12c: It would be better to show the ratio on a logarithmic axis.

Figure 14: I spotted a minor typo in the figure title (Percenatge instead of percentage).

5 References

Crameri, F., Shephard, G.E. Heron, P.J (2020). The misuse of colour in science communication. Nature Communications, 11, 5444 (2020). https://doi.org/10.1038/s41467-020-19160-7.

Nash, J. E., Sutcliffe, J. V. (1970). River flow forecasting through conceptual models part I - A discussion of principles. Journal of Hydrology, 10(3), 282–290. https://doi.org/10.1016/0022-1694(70)90255-6.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-468, 2020.