Hydrology and Earth System Sciences Discussions

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Comment on hess-2022-330

Anonymous Referee #3

Referee comment on "Impact of urban geology on model simulations of shallow groundwater levels and flow paths" by Ane LaBianca et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-330-RC3, 2022

This manuscript deals with the modelling of shallow groundwater flows and levels in urbanized catchments, and highlights the impact of both the urban geology description and the spatial resolution used in the distributed model. This topic is of high interest, because the interactions between groundwater and underground constructions are important in urban soils whose features are very variable, and we need to improve our ability to simulate these complex hydrological behaviours.

The study is based on an integrated hydrological model using MIKE SHE code and this model allows a detailed representation of groundwater levels and flows. Velocity fields and then travel times may be deduced from the model; this is a real added value of this modelling application : this type of result is quite rare in the field of urban groundwater modelling and it has to be noticed. The impact of urban infrastructures in the shallow groundwater flows and level is proved through this study and this is a step forward in the urban hydrology behaviour knowledge.

The structure of the paper is basic and clear, with a first introduction section presenting the main issues related with this topic and a short state of the art dealing with urban shallow groundwater modelling, and a focus on the importance of the soil and geology description. The second section includes the case study presentation. The Geological models and the main modelling methodology adopted here is presented then and the data- modelling- and evaluation methodology adopted here. The last sections are usual, with results, discussion and conclusion.

General opinion and minor comments

This manuscript is devoted to the sensitivity of an integrated hydrological model to the urban geology, and uses 3 different representations (i.e. 3 geological models) with various consideration of the specific urban soil features. The sensitivity of the model to the spatial resolution is analysed too. For this last factor, I wonder if only two grid sizes is enough for the study of the effect of the spatial resolution.

The overall manuscript, including methods and results, is relevant and well-prepared and written. However, I have a few minor comments that could be into account in order to improve the quality of the manuscript and help the reader.

First of all, I noticed a lack of justification, especially in the Methods section. The authors did not always argue their assumptions :

- p5 l 118 : "… *concrete pavement , which have an imperviousness of 75%"* . How was this value estimated? Traditionnally, this kind of surface is considered as totally impervious. But I aknowledge that it may be partially pervious. But that should be explained.

- p7 l 183 : " *… and additional data on soil material in the top 5 meters*". As the modelling application is quite sensitive to the soil configuration, especially in the first meters, one can wonder where this "additional data" comes from! What kind of additional data? From drilling data? From infiltration tests?

P8 l 207-209 " *the location of roads and pipes (…) were used as proxies for the presence of excavations and trenches*" What is the relevance of this assumption? Did you assess this assumption? Did you compare this proxies methodology to real data? Is it valuable only in this study case or could it be transposed in any urban catchment?

P8 214-220 – Why the SHE model was chosen here? We can understand that it is the model used by the research team, but could the authors argue why this model is appropriate to do this study? Are there any equivalent modelling tools/methods that could have been considered for this type of modelling study? Is SHE model the only one that allows to achieve the objectives of this study?

- p9 l 245. What is this surface-subsurface leakage coefficient? A parameter of the SHE model? Does it take into account the leakage in pipes, ot only the leakage from surface-subsurface? How could it be estimated?

Then, the methods section could have been improved with a graphical scheme helping the reader to understand the chosen parametrizations. This is especially needed in the 3.2.2 paragraph, because the list of the presentation of the parametrization and boundary conditions is quite long, and a scheme would be more efficient and more easy for the reader.

Finally, I have a short comment about one element of discussion : l 535-543. The sewers renovation could be a way to reduce the soil-sewer interactions and the infiltration of groundwater in sewers. As discussed by the authors, the preferential flow paths would still be present in the pipe trenches. However, I wonder if having a full renovated sewer system is not an utopy… To my opinion, there will still be some defects in the sewer system and then, as the preferential flow in the trenches remains present, the water will always find a way to penetrate in the sewers. I have the impression that this type of sewer renovation (or "non leaking pipes assumption") is only a "modelling dream"; I am not sure it would be feasible in reality.. (especially in a economical point of view). I would appreciate that the authors re-consider this paragraph.

References

Several mistakes should be corrected :

- l57 Boukhemacha et al (2051) and Epting et al. (2008) are missing in the list of references

- l115 / l 633 : Danish Geodata Agency ?

- l197 Kristensen et al (2015) is missing in the list of references

- l 227 DHI 2017 is missing

- l 260 is specified in Fejl ! ... Like fundet / to be corrected