Hydrol. Earth Syst. Sci. Discuss., 10, C5220–C5225, 2013
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Hydrology and Earth System Sciences Discussions
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Interactive comment on "Water displacement by sewer infrastructure in the Grote Nete catchment, Belgium, and its hydrological regime effects" *by* D. Vrebos et al.

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Received and published: 27 September 2013

We would like to thank Anonymous Referee #2 for the helpful comments.

General comments

We agree with the referee that the study has its limitations. Both model based and empirical data have their disadvantages. Measured data allow us to compare different discharges in detail. But it is difficult to situate these flows in a broader context with other hydrological processes. Models allow us to integrate these processes and test different scenarios, but have to compromise in detail and accuracy. For that reason, a combined approach was chosen where we could combine both strengths and analyze the impact of the sewer system in different ways. We agree with the reviewer that other approaches would be possible but with other types of limitations. We will add these considerations and limitations of the study to the discussion section of the paper.

To make the analysis better understandable a scheme of the different steps (see next in specific comments) will be added to the 'Material and methods' section, to better clarify the methodology. Special attention will be given to the study area description, TIA and EIA design, model design and how the different analysis steps fit together. Figures will be revised based on the comments of both referees. Conclusions will be rewritten to better incorporate recent research and discuss more the limitations of the study (see also our reply to referee 1).

Specific comments

Paragraph 1. This is a valid point. The limitations of EIA selection in the study and will be addressed by additional discussion in the article. Information on SUDS will be provided in the study area description and implications will be integrated in the discussion. As also replied to referee #1, because of the uncertainties in the estimation of the EIA, only 1 decimal place will be used as both referees suggested. Impervious cover was defined by a selection out of the 47 land use classes. Areas like gravel areas and gravel roads are not included in the category 'imperviousness'. The table describing the different land cover classes and categories might be incorporated in the article.

Paragraph 2. A figure based on the provided sketch (Figure 1) will better explain the step-wise methodology followed and will be added to the revised paper (also in reply to the general comments of the reviewer). The results will be rewritten to limit the numerical details.

Paragraph 3. Peak and low flows are selected following the methodology of Willems et al. (2009). Nearly independent peak flows were selected from the flow series based on criteria for the inter-event time, the inter-event low flow discharge and the peak height. Using a similar procedure, but using a recession constant for baseflow instead, nearly independent base flows can be defined. Truly extreme events are not incorporated in the study as they did not take place during the assessed time period.

Paragraph 4. The spatial configuration and location of impervious areas is integrated in the study by reducing the model rainfall input in a spatially variable way based on the % of EIA in each grid-cell. The study specifically wanted to the hypothesis in relation to FDPMs. The spatial resolution of the model was based on the maximum cells the model environment can handle. Higher resolutions were not possible, while lower resolutions would insufficiently represent the fragmented land use of the catchment.

Paragraph 5. Observations of sewer flows to the WWTPs are available. However evaluation of these with respect to sewer infiltration is more difficult. Figure 4 aims to illustrate the increased impact of WWTPs during high and low flows. Directly linking this to water intrusion in the sewer system is however difficult. The comments on water intrusion in sewers in the catchment are based on local knowledge.

Paragraph 6. Note that the previous section in the discussion of the paper discusses limitations and provides motivation for the approach.

Paragraph 7. The model impact results discussion will be rewritten to better convey its message and differentiate between hypothetical and real impacts. Results of flow reduction as a consequence of rainfall reduction were indeed expected in order of magnitude, but we hypothesized whether the empirical and model-based results are consistent, given that this gives trust to the impact results. We believe that the model-based approach is sound as motivated before. However, discussion on the model approach will be added (see our replies to previous comments).

Paragraph 9. The referee's comment regarding the overestimation of TIA will be integrated into the discussion. Limitations of the study, such as the high resolution data (land cover, sewer system...) required or the limited integration of sewer processes, are indeed missing in the conclusions and will be integrated.

Paragraph 10. The Box-Cox transformation aims to obtain homoscedastic errors. Because the equation does not help much, we will shortly explain its aim and also provide a reference (e.g. Willems, 2009).

Fig 1. The two shades show the distinction between the parts of the waste water treatment zones that are located within and outside the natural catchment boundary. This distinction will be better explained in the figure caption.

Fig 5 & 6 Figures show the variance of the change in upstream area. The revised article and figure caption will better explain this. The two shades of grey will be removed.

Fig 7 – Figure 7 will be changed to provide averaged data over a larger time step. In this way the figure will still give an overview of measured and modeled flow and illustrate the difference in flow between seasons and years.

Fig 8 – Regarding the BC, as replied before, the Box-Cox transformation will be explained in the materials and methods section. The BC transformation aims to reach homoscedastic errors; which means standard deviation of model residuals independent of the flow values. That's why the +/- standard deviation lines can be shown parallel to the bisector. The same approach is followed for the mean residual. It is visualized by shifting the bisector (1:1 line) with the mean residual. Because it is so small, it almost follows the 1:1 line.

Fig 9. This type of figure has been used in recent articles on hydrological model performance (e.g. (Willems, 2009). However, considering the large amount of figures in the article and the limited amount of information it provides, Figure 9 will be removed from the article.

Fig 11. The axis title should indeed be 'contribution' instead of 'distribution'. We doubt whether it is a good idea to add the river flow series to the plot because this will overload the plot with information also duplicate information from Figure 3.

Fig 12-13. The return periods are calculated empirically as the total length of the available time series (in years) over the peak flow rank (1 for highest, 2 for second highest...). This information will added to the 'materials and methods' section. The scale of the y-axes will be changed to make the plot clearer.

References

Willems, P. (2009). A time series tool to support the multi-criteria performance evaluation of rainfall-runoff models. [Article]. Environmental Modelling & Software, 24(3), 311-321. doi: 10.1016/j.envsoft.2008.09.005

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 7425, 2013.

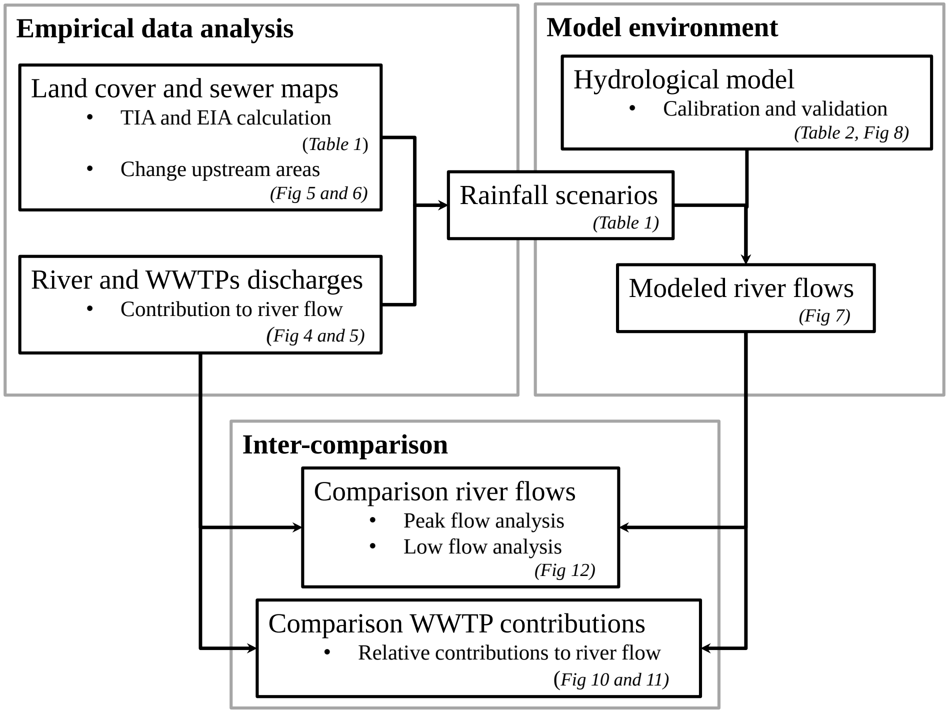

Fig. 1.

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