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Discussions

Interactive comment on "Evaluating health hazard of bathing waters affected by combined sewer overflows" *by* Luca Locatelli et al.

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The manuscript titled "Evaluating health hazard of bathing waters affected by combined sewer overflows" introduces the use of new health hazard indicators for bathing water, correlating the rainfall volume with the duration of the bathing water contamination. These indicators are applied in a real world application.

For this reason, the authors coupled an urban drainage model using the Infoworks software with a sea water quality model using the MOHID software. The manuscript is well-written, well organized, well structured and is scientifically consistent. The authors provided all the required information and assumptions and did not hide the "weak" approximations. Their method is practical (and still consistent) and can be applied

directly to other case studies. I suggest to be published after some minor revisions.

My remarks:

1) In Table 2 there is the variable T in the brackets and I suppose is the return period calculated using the available IDF curves and rainfall duration. Regarding the rainfall intensity, I am a little bit surprised to see such small values (from less than year to 6 years). The authors shall double check these values.

2) Which is the way for calibrating the urban drainage model? The authors automatized Infoworks and used an optimization algorithm and if yes, which is the algorithm.

3) Which is the objective function for the calibration of the urban drainage model? The sum of the RMSEs?

4) The authors obtained a rather small value for the Manning coefficient at impervious areas (0.013 s/mˆ1.3). Although these areas are mad from asphalt or concrete and are characterized by small roughness, however in real world there are several obstacles in surface, increasing the roughness. Did the authors use constraints for keeping a physical meaning at the variables which were calibrated, or the parameters are considered as black-box parameters?

5) Except of the mentioned parameters at the urban drainage model which were calibrated, what did the authors with the rest, such as parameters for the hydraulic structure of the CSO? These are the parameters which were manually calibrated?

6) Regarding the sea water quality calibration the authors might provide a magnitude of the computational time (hours, days?) in order to support their decision for manual calibration. Besides they could provide some additional references about the use of Machine Learning in such cases, when a highly computationally demanding software requires calibration. Finally, taking into account the nature of the observations (some points vs. a dynamic time-series), the process followed by the authors is more a plausible check than a proper calibration and they should discuss about that.

7) The authors classified the rainfall volume to bins (e.g. Fig 11) but the range between 8-10 mm is not appearing. Is there a reason for that?

8) Can the authors support bibliographically their decision to assume that the evolution of E. Coli concentration is done with this linear way or it is an assumption for practical reasons (this does not reduce the importance of their work).

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