Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-522-AC2, 2017

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Coupling urban drainage–wastewater systems and electric smart grids during dry periods: a gain/loss framework using the relative economic value with ensemble flow forecasts to predict the switch" *by* Vianney Courdent et al.

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Received and published: 17 January 2017

Dear reviewer,

We greatly appreciate the review and acknowledge that the comments and suggestions will lead to an improved paper. Our reply to the general comments:

The problem is exposed as a dynamic one (e.g. P4 L25-26, P9 L27-30, P14 L21-22).However it is solved as a static one: the relative economic value is presented as a

function of the gain/loss ratio [1]. The decision threshold (i.e. the fraction of members of the ensemble of predicted discharge exceeding a discharge threshold) beyond which the manager decides to switch from energy optimization to safety of the system is deduced from the envelope of separate curves. As for the choice of a method for the post-processing of numerical weather prediction model the maximum threat method extends significantly the range towards low gain/loss ratios resulting in positive relative economic values compared with the aerial overlap method. Having these results, the methodology has still to be proven in real dynamic situation i.e. where the decision to be taken at a given time depends on the decisions already taken. What is missing in order to that? An order of magnitude of the losses in case of combined sewer overflow, a hydraulic model able to reflect the management actions? The authors are asked to make clear the scope of the paper and either add new results or add comments in the outlooks [2].

[1] Thank you to point this out, you are right, we need to clarify the use of the term "dynamic". The referenced articles on the REV ((Richardson, 2000; Roulin, 2006) are using fixed alpha values (the cost of mitigation measures and their benefits are assumed fixed). In our case the alpha value varies with time, the gain depends directly on the variation of the energy market. Hence the incentive to optimize the IUDWS for energy consumption changes with time. In this sense the alpha ratio is dynamic. But, indeed for at a given time, for given NWP forecast the alpha is fixed and the problem solved as a static one. We will make this distinction clearer.

[2] We agree that further information on results and performance would be appreciated and we are working towards it. Indeed, two large pipes will be constructed just before the inlet to the Damhusåen WWTP with the primary purpose to reduce CSO to cope with the new regulation. Those 2 pipes can contain a volume corresponding to one day of dry weather flow and would nicely fit the concept developed in this paper and in the paper by R. Halvgaard et al. R. Halvgaard, L. Vezzaro, P. S. Mikkelsen, M. Grum, T. Munk-Nielsen, P. Tychsen, H. Madsen: Integrated Model Predictive Control of

Wastewater Treatment Plants and Sewer Systems in a Smart Grid (In Review Process).

I missed information regarding the methodology. No lead time is specified with the results. Are all the ensembles (2 years x 4 issue hours) used at hourly time step to the forecast horizon of 54 hours (P5 L14-18)? Or 2 days (P3 L5, P12 L11-26)? How the scores are computed regarding both the issue time and the lead time? In case the forecast horizon is 2 days, how do the authors deal with the decreasing skill scores or relative economic value of the predictions with the lead time? [3]

[3] Indeed a decrease in the performance skill is observed with increasing lead time as was documented in (Courdent, 2016). The bids and offers on the daily energy market are made up to 36 hours in advance. Therefore, the different lead times are aggregated in the results. We will clarify this point in the methodology section.

Please find appended to this reply our point to point responses to the received comments displayed as a Table in pdf format. We will make changes to the paper that accommodate the technical comments by the reviewer, including careful proofreading. We would like to express our sincere thanks to the reviewers for their constructive comments and identification of areas in the manuscript which needed clarification.

On behalf of all the authors,

Vianney Courdent

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-522, 2016.

Reply to the specific comments of reviewer 2.

Referee # 2	
Specific comments	
1. P3 L26 "spatial" instead of "special". Do you have a reference for this assertion?	Indeed it is a typo, we meant "spatial".
2. P4 L1 "The radius of the neighbouring area included is used as a parameter during the decision making, in addition to fEM." should move from introduction to the methodology section (2 Material: : : : 2.2). P11 L18, Figure 7 Results considering a radius of 6 grid cells are presented. This radius has been optimized on REV? What is the sensitivity to this parameter?	The radius was not specifically optimised on REV, the selected radius was based on the previous article (Courdent et al., 2016), which describes this method further. We will make this clearer.
3. P5 L14 UTC	Yes, UTC will be added to the NWP generation time.
4. P6 L18 and other occurrences of "forecasted" should be "forecast". "summing up to a total of n event assessments" : this part of the description methodology should be made much clearer (see general comments).	The occurrences of "forecasted" will be corrected to "forecast". We will clarify the computation of the skill score as mentioned in the reply to the general comment [3].
5. P7 L4 "Methodology" : section 2 involves also description of the methodology. Section 3 is more related with validation.	This comment is similar to the reviewer 1 specific comment [8]. The NWP post-processing methods described in section 2.2 are developed in the previous manuscript (Courdent et al. 2016) and are used to generate input data to the model for this article. Despite being a method, it was decided to develop it under the data section to distinguish it from the core methods of this paper. This section will be modified to make this clearer.
6. P10 L6 and other occurrences (e.g. Table 5, Figure 5, 8) "always optimize", "never optimize" sounds strange because the paper is all about optimization. Find a short reference to the two objectives (like "always energy objective"). ½½	We agree the text on the figure a will be changed for more clarity.
7. P11 L20-29, Figure 7b, Table 4 The slightly better upper bound provided by the areal overlap method can't be seen on the figure. How the complementarity of both post-processing approaches can be used in a real situation? Through the gain/loss ratio and the decision threshold?	The complementarity of the 2 approaches is more visible on the ROC diagram Figure 7a. The catchment aerial overlap provides valuable information for low PoFD, which are not covered by Maximal threat approach. This is not especially visible on the REV diagram

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Fig. 1.