Thesis title:

REAL-TIME CONTROL OF STORMWATER BASINS FOR SUSTAINABLE AND ADAPTIVE MANAGEMENT OF URBAN STORMWATER

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Answers to jury's comments on the thesis report:

I wish to thank the jury committee and my supervisors for their patience in reading the thesis and their valuable comments. All comments were tried to get integrated in the revised version. The references were carefully cited and the arguments in the introduction were justified by the related references. There were some concerns and questions mostly on the methodology and how the integrated optimization and rule-based approach works most of which have been discussed during the thesis presentations. Some more detailed explanations were also added in the Introduction Chapter to make the proposed approach clearer for the readers. The equations numbers and sections of the published papers are modified based on this report numbering. More details about the modifications are given below.

Answers to Dr. Cherqui:

Answer to comments in detailed report:

- The first comment of Dr. Cherqui, in introduction Section and in the detailed report, is about the inefficiency of the traditional approach and the potential benefits of the dynamic approach. As stated above, the introduction of the thesis was modified to take this comment into account.
- The second comment of prof. Cherqui, in its detailed report and in the Introduction Section, was about the RTC and how it can be beneficial in the context of stormwater management in urban areas. This comment has been

considered by modifying the related discussion in the introduction and adding a reference to justify it.

- The next comment of Dr. Cherqui is related to the definition of the rolling horizon. Considering this, control horizon and planning horizon are defined in page 89 line 18. Actually based in these definitions “Control horizon” is the period over which the dynamic outflow scheduling is planned with respect to several physical and hydrological limitations. It means that at each time-step when the integrated optimization and rule-based approach is run, the outflow rate set-point is generated for all the time-steps over this control horizon. This makes the approach, predictive real-time as it decides for the next few time-steps ahead. While the planning horizon is the period over which this control will be continued which can be either finite or infinite. More explanations are added in page 11 to clarify how the proposed dynamic approach is run at each time-step.
- Regarding Dr. Cherqui’s comment on using the precipitation data of May-November and not the whole year, in Quebec due to the cold winters, the rainfall series are taken into account from May to November, and over other months of the year, normally there is snow and only a little rain.
- The comment related to the possibility of having better outflow rates in figures 3-9 and 3-14, as discussed during the thesis defence, the rule-based approach doesn’t provide the optimal set-point while providing a significant improvement in the performance of the system. Thus, we can imagine some better responses such as earlier discharge of water or having lower rates outflows etc.
- Regarding the comment of Dr. Cherqui on applicability of the proposed approach on stormwater structures other than detention basin, the answer is yes. This approach can be applied on any system operates based on the mass balance while considering some modifications to the mathematical model to adapt to the physical characteristics of the new system.
- Comment on giving more details on the difficulty of the global approach: The main difficulty of the global approach was in the quality control rules that don’t provide

optimal outflow rates. The optimization part considers the minimization of the total outflow rate so that the sequential outflow discharge is generated while in quality control rules it was more challenging to have sequential discharge. Thus we have modified the control rules for the global control approach, as explained in detail on page 94, in such a way that they are run in a N-iteration loop (N is the number of basins) for each basin and by looking at $\boldsymbol{t_{next\,rain}}$, $\boldsymbol{V}_{i,req}$ and the emptying time of the basin ($\boldsymbol{t_{i+1,e}}$), the discharging process from the basins are set either sequentially starting from the basin in need for higher percentage of capacity for the upcoming rainfall or simultaneously, depending upon the $t_{next\,rain} - t_i > t_{i+1,e}$ condition.

- Are there one or more optimal answers for the outflow rates? When the outflow rate is generated by optimization, it is an optimal answer. However when the rules generate the outflow rates, there may be better answers too. This is now discussed in the thesis.
- Discuter de l'hypothèse qu'il n'y a pas délai en cours d'eau entre les exutoires de chaque bassin: We simulate the rainfall-runoff process by the simulation model SWMM and use the inflow rates from this model in our dynamic algorithm. Looking at the data obtained using SWMM, we can see that the inflows don't arrive always at the same time and there are some delays between the inflows of the basins. However this delay can be only a few time-steps and not properly clear from the presented graphs, but they are not necessarily simultaneous. But you are right that our model does not take into account the time of the flow in the river between the various outflows, because, in our case study, the four outflows were close to each other. For other case studies in which outflows are no so close, the delay in the river could be taken into account in the control approach by adding a simple routing function. This is now discussed in the thesis in first paragraph of page 132.
- Discuter des conséquences d'utiliser des séries de pluie différentes pour chaque bassin: Even if the rainfall series used for this study was the same for all the basins,

the dynamic approach is flexible and can be employed when there are different rainfall series for each basin. Since the rainfall data is not directly fed in the dynamic approach and it is the simulation model that uses this rainfall data to simulate the inflow rates for the dynamic approach, different rainfall data would not affect the proposed approach.

- Pourquoi avoir choisi 85%/15% des données pour l'apprentissage et les tests du modèle OVNN: This is a common percentage for dividing the data in training and testing sets in artificial Neural Network algorithms. Besides we have performed a trial-and-test to verify this division and concluded that this results in accurate predictions.
- Pourquoi les erreurs de modèle OVNN sont-elles plus importantes pour les intensités élevées pour le bassin #1: Because there is not enough data to train the model for the higher volume rates and more than 96% of the data are related to the flow rates less than 1.3 m^3/s specially for the basin 1 which makes it more difficult to properly predict higher rates flows. This implies the importance of the qualified input data in the ability of the model forecaster to estimate properly the outputs (explained in 2 last lines of page 125).
- Ajouter les références qui manquent: All the cited references now appear in the list of references.

Answer to comments in the thesis report:

- Considering that the English abbreviations like GPRTC are used as the name of the proposed dynamic approach, Dr. Cherqui's comments on changing the English abbreviations in French sections have not been considered.
- Regarding the definition of resiliency "the capacity of the system to bounce back from a failure" and the fact that in our case, "Overflow" is a basin failure, the discussion on the impacts of prediction errors, remains the same.
- Page vii: the prediction data is a commonly used term in English that can be interchangeably used with forecast data.

- Some of the tables have been modified to be more readable based on the jury's comments.
- Some typo in the equations have been revised especially in chapter 4.
- Page 1 paragraph 1: The sentence is modified to "the traditional stormwater management systems need to be modified based on these new emerging challenges, more than ever."
- Page 2: Figure 3 is modified.
- Page 4 figure 1-2: This figure is taken exactly as it is from the California Office of Environment website that generally shows the runoff hydrographs before and after urbanization where the x-axis represents the time in hours. As the goal of this figure is only to show how urbanization can affect the runoff hydrograph, it doesn't include some detailed information and it should preferably be simple.
- Page 6 lines 5-6: As suggested retention is changed to detention and "and/or storage" is added to the volume control approaches.
- Page 6 lines 15-16: "One of the most used" and "Store" are added.
- Page 8 lines 24-25: "Sustainability in SWM Systems" is added to be more precise regarding the given comment.
- Page 9 second paragraph: The fact that traditional stormwater management systems are inefficient facing with current emerging challenges like climate change has already been justified by other studies in the literature so I respectfully do not think that we should again test the traditional systems to verify this fact. Actually this is not the objective of this study (as we know that there is a problem based on the literature), however the objective is to propose a modern control approach that improve the performance of traditional systems facing with these evolving challenges which is proved in this study.
- Page 10 first paragraph: the reference is modified and "Advances" is removed.
- Page 16: Travis and Mays 2008 studied "Retention" basins.
- Page 17: Reference added.
- Page 18: The reference of the Figure 2-1 is modified as suggested.

- Page 20: GA is defined and all the references were added.
- Page 28: The section numbers were modified.
- Page 29: The parentheses were added.
- Pages 33-34: Table is justified.
- Page 39 last line: No, as discussed before we did not perform a comprehensive uncertainty analysis to achieve more reliable model. However we verified the impacts of prediction error on the performance of our dynamic approach. The explanation brought here and in the first paragraph of the page 40 is a part of the literature discussion in our published Review paper.
- Page 52: references added.
- Page 55: The definition of Static control approach is given in page 36 and the last paragraph of the page 35. Besides in the Introduction section page 7, there are more explanations of static control approach and specifically the definition of static control in this thesis.
- Page 57: please refer to the first paragraph of the page 2 of the present document.
- Page 58: The figure is modified.
- Page 59: The definition of ppt and qqt is modified. Thank you for noticing.
- Page 60: The font size is reduced as suggested.
- Page 61: We need to predict the rainfall for a few hours more than 40 hours to have enough time for emptying the basin after 40 hours. If the rain is going to start exactly in 40 hours, the rules will decide on a detention time less than 40 hours (between 20 and 40 hours). So we need to consider 48 hours for the prediction horizon which allows proper decision-making on the detention time.
- Page 63: 30 min is the control horizon that is rolled over the planning horizon. Which means that at the start of each control horizon the system parameters are updated to generated set-points for all the time-steps over the control horizon.
- Page 65: The planning horizon is the time that planning is continued (in our case the dynamic control algorithms will be run) which can be either forever or for a limited time according to the studied system.

- Page 65: the answer to the question "why May-Nov?" can be found in page 2 paragraph 3 of the present document.
- Page 66: we considered the year 2013 because it shows to have "total highest volume" among the years 2000-2017 and as you mentioned it is different from the highest intensity rain events. In fact, we considered the total volume, because we wanted to verify the performance of our model in critical situations when for example several rainfalls occur continuously. This way we could validate better how the dynamic control model performs.
- Page 66: We have added 15% to the volume of all the events.
- Page 66 Table 3-3: Yes I have all the characteristics of the years 2000-2017 but it is out of the scope of this paper to bring that much details.
- Page 67: Scenarios sequence is modified.
- Page 68 Table 3-4: It is mentioned that we consider outflow variation "minimization" and from all the context of the study, it is obvious that we want to "maximize" the detention time.
- Page 68: Totally agree that it would be a better idea to consider the number of overflows and not the percentage of volume capacity of the basin that was used. However, this indicator also can provide an appropriate imperative for decision-makers for basin sizing and how much capacity is needed to manage runoffs in rainy years like 2013.
- Page 68: As mentioned in the table, N is the number of time-steps. Also, ppt and qqt do not provide a clear understanding of the outflow variations so we have defined this performance criteria.
- Page 70: As the paper is long, we tried to avoid any repetition.
- Page 71: Explained in paragraph 6 of the present page.
- Page 72, 73 and 77: Explained in paragraph 4 page 2 of the present document.
- Page 88: 15% added to the volume of all the events over the year 2013 (May-Nov).
- Page 89: The prediction horizon in the present study is different from the one that you defined. The proposed dynamic control algorithm does not run over the

prediction horizon, however it produces the outflow set points based on this prediction horizon. Also, we do not have "Sampling interval", yet we have time-steps which are the small periods of time that the algorithm is run (defined in page 89). All these concepts are already defined in different parts of the thesis (page 89, 62, 57).

- Page 89: The time series is used as it is but we needed to identify different rain events to compute the performance criteria, so we considered a 6 hours inter-event duration and 1.2 mm/h for dividing the rain-series into events.
- Page 91: Legend added.
- Page 92: Discussed in page 3 paragraph 3 of this document. Also, regarding the basins type, we have developed the formulations for parallel basins and there is no flow sharing between the basins. However, the formulations can be modified for basins in series too. To clarify more, some explanations were added to the page 12 of Introduction section of the thesis report.
- Page 92: The equation numbers are added.
- Page 92: The same rainfall but not necessarily the same inflows as explained in page 3 paragraph 3.
- Page 93: Emptying to zero volume. It is easy to calculate it as: $\frac{V_{i,available}}{Q_{i,t}}$ (this formulation is added to the text).
- Page 94: Discussed in paragraph 1 page 3 of the present document.
- Page 94 last paragraph: Because the predictions are for one month period.
- Page 95: We tried to bring a generalized figure so it is more preferable not to give the exact hours.
- Page 96: (Service Atmosphérique Environnement Canada) is added.
- Page 97: The river is added to the figure 4-5.
- Pages 97-98: The equations are modified as suggested.
- Page 98: Fixed position does not mean fix outflow however the maximal outflow is respected.

- Page 98: The frequency of discharge can be interpreted by the number of time steps when there is a discharge Q>0 divided by the total number of time steps.
- Page 98: the mean is already mentioned under “performance criteria”.
- Page 100: This page was not printed well in previous version. It is added in this new version.
- Page 101: In basin C like other basins we have constant dynamic outflow rates.
- Page 103: The headlines of the table 4-4 are modified to “Perfect rainfall predictions” and “With errors on rainfall predictions” as suggested. And the total number of overflows is removed. But we kept the “Total” as it is, since the “mean” is already mentioned under “performance criteria” and having “total” helps the reader to understand that this value is for the total outflow rate to the receiving water.
- Page 103 the response to the question “Does it mean that the imperfect forecast is better than the perfect forecast for overflow control” is given in the lines 4-9 at page 103.
- Page 104 figure 4-9: If the discharge started earlier, there would not be any overflow in the basin, and probably the dynamic control approach would decide on a lower outflow rate to preserve the river from the high rate outflows and also avoid any overflow in the basin.
- Page 107: SEA was defined.
- Page 108: no, this paper is not submitted yet.
- Page 109 line 18: Mean absolute error is added.
- Page 112: Because there is not enough data to train the model.
- Page 113: The rainfall series related to the year 2013 in Figure 5-1 has been replaced with a clearer hyetograph.
- Page 115: Some details were added to the caption. The legend is added.
- Page 118 first paragraph: Text is modified.
- Page 122: discussed on page 4 paragraph 2 of the present document.
- Page 123: Table 5-5 is modified as suggested.

Answers to Dr. Pleau:

Answer to comments in detailed report:

- Utilisation dans le problème d'optimisation d'un modèle hydraulique très simplifié: As discussed, this is an original optimization algorithm that has been developed to optimize the performance of the stormwater basins. Although it is simplified, it helps the stormwater basins to have an enhanced performance in terms of quality and quantity. For sure, this algorithm can be extended to consider more details on hydraulic/hydrologic characteristics of the problem in future studies.
- Applicable qu'à des bassins parallèles localisés à l'exutoire des réseaux : discussed in paragraph 3 page 8 of this document and some explanations were added to the introduction part of the thesis.
- Équations parfois mal formulées et mal expliquées : All the errors in equations were modified as suggested.
- Aucune quantification de l'amélioration de la qualité des eaux (% de réduction des matières en suspension déchargées dans le milieu récepteur) : As a further study, we can consider the calculation of TSS reduction via simulation or field measurement. However, in this study we only relied on the strong relation between the detention time and TSS removal. This now discussed in the Conclusion.
- Aucune étude comparative démontrant les avantages d'une approche intégrée par rapport à une approche locale par bassin de rétention : We have shown that the global control approach and local control approached have enhanced the performance of the basins, and we did not study the comparison of local approach and global approach. However, we can say that considering local approach would result in an improved performance at local scale but will result in a lower performance at global scale, since the basins would most probably by emptied around the same time, which would lead to higher total outflows in the river.

- Plusieurs références dans le texte qui ne se trouvent pas à la Section 7 – References : All the references were revised and the missing ones were added to the reference chapter.

Answer to comments in the thesis report:

- Page 1: The references were added when required.
- Page 2: Figure 3 modified to figure 1-2.
- Page 6: "Can be" was deleted.
- Page 7: This figure is designed by myself.
- Page 7 line 10: Since the water level and flow set-points cannot be defined in the context of static control, this line is modified.
- Page 16: Text was modified.
- Page 19: Reference was added.
- Page 29: The sections are modified to be in-phase with the thesis report.
- Page 49 line 9: The energy consumption minimization objective for the Csoft is removed as suggested.
- Page 57: This is the definition of wet period which shows that whenever there is an inflow we consider it is the wet period.
- Page 58: This figure is not referenced because it is designed by the author.
- Page 59 line 11: Even in large rain events this constraint is met and no overflow occurs.
- Page 59: As discussed during the presentation, since by minimizing the total outflow rates we can achieve a peak flow mitigation (we showed this by calculating the "peak-flow reduction" performance criteria), we decided to consider this objective function.
- Page 59: The proposed mathematical formulation is a new approach for operational real-time control of stormwater basins and can be further developed/extended to a MIP or non-linear programming to better represent the

hydraulic/hydrological characteristics of the system. This is now discussed in the Conclusion.

- Page 59 last line: Your concern is totally understandable however even in the small basin scenario we have never had overflow (infeasible optimization). Since the capacity of the basin should be considered in the constraints the presence of this formulation is necessary here.
- Page 60: based on the comment on V_0 the following sentence is added with a foot note explanation "The value of V_0 here is updated at each time-step based on the generated set-points of the previous control horizon".
- Page 60: This is another way of considering the maximum allowable outflow rate that can be taken into account in future studies. The actual equation has met the requirements of this stage optimization model.
- Page 60 "A better objective is to minimize the peak flow discharged over the control horizon. However, to do so, it would have been required to use a Mixed Integer Linear Programming or a Nonlinear" is answered in lines 15-22 of page 11 in the present document.
- Page 61 equation 9: Since $t_{next\ rain}$ is defined as the "time until the next predicted storm event starts", we do not need to calculate the interval between previous and next rains.
- Page 61 equation 10: I do agree with your comment however, as discussed during the thesis defense, the rule-based approach doesn't provide the optimal set-point while providing a significant improvement in the performance of the system. This is now discussed in the Conclusion.
- Page 62: the definition for V_{req} is provided.
- Page 62 the response to the comment "How would we determine this value in real-time if there is no rainfall predicted over the forecasting horizon": In this case we consider that the next rain will be started in 48 hours, which makes the rules keep the water for 40 hours and release it after the sedimentation is realized.

- Page 62 the response to the comment “How is this value determined”: By simulating the next coming inflow which is obtained by running the simulation model based on the rainfall series data.
- Page 62: All t s are defined based on time step number and not the continuous time.
- Page 62: Integrating the rules into the optimization model and having a unique optimization model can be considered as a further study. This is now discussed in the Conclusion.
- Page 63: answered in line 15 page 11 of the present document.
- Page 64: The value of maximum allowable outflow is changed to 50 L/s.ha.
- Page 68: Detention time is exactly calculated as suggested. Some explanations were added in the thesis.
- Page 72: Showing the volume under static control made the figure too detailed and we did not aim at analyzing the storage volume under static control.
- Page 76: As explained before, the rules do not produce optimal set-points, so for sure there is possibility of having even better set-points than the one generated. This is now discussed in the Conclusion.
- Page 78 line 19: As a further study, we can consider the calculation of TSS reduction via simulation or field measurement. However, in this study we only relied on the strong relation between the detention time and TSS removal. This is now discussed in the Conclusion.
- Page 78 line 25: That is why an analysis is done in the third paper to show the impacts of error on the performance of the system and its probable failure.
- Page 79 and 80 line 1: Although, both definitions are the same but not stated exactly the same way, an additional sentence with a footnote has been added.
- Page 92: The typo in the $t_{i,e}$ equation is revised. Thank you for this comment. It was quite important.
- Page 92: As discussed before in page 8 paragraph 3 of the present document, this model considers only parallel basins and not basins in series. Thus, a sentence is added to make it clear for the reader.

- Page 93 line 18: $\frac{V_{i,available}}{Q_{i,t}}$ is added to clarify how to calculate the t_i.
- Page 93: $V_{i,available}$ is modified to $V_{i,req}$
- Page 94: Explained two lines earlier.
- Page 95 lines 5-6: "water volume in the basins and observed inflows to the basins" is added as the parameters that should be updated at the start of each control horizon.
- Page 97 line 6: I calculated the total peak flow mitigation exactly as suggested.
- Page 98: Discussed in paragraph 5 page 10 of the present document.
- Page 103 line 5: Your suggestion is explained in detail in page 104 of thesis.
- Page 103 line 21: The overflows are calculated easily using the mass balance equation and considering the observed inflow rates and the generated outflow rates under dynamic controls. Each time that the V is more than the capacity of the basin, it means that there is an overflow.
- Page 104: The dewatering shown in figure 4-9 is explained in lines 4-5 of page 104 of the thesis report.
- Page 112 line 7: Yes it is the closest rain gage to the case study.
- Page 119 last line: MAE and nRMSE are shown to be appropriate performance criteria to evaluate the accuracy of ANN. That's why we consider them and not the Nash-Sutcliffe criteria. However, Nash-Sutcliffe could be used in some analysis.
- Page 123 last line: Yes, exactly we wanted to produce the simulation behavior while having less computation time.
- Page 124: By SWMM running time, we mean the time that it takes to run the model using PCSWMM for a specific time which is shown in the executing window box of the PCSWMM.
- Page 126 line 18: Using the defined criteria we have achieved a mean value of 47% reduction in velocity of the discharged water however other performance criteria like the one suggested could have been employed too.

- Page 126 line 30: We showed that to some extent this can be true; however we shouldn't forget the impacts of prediction errors.
- Page 130: This sentence is added to the lines 6-9 of this page "A real implementation of the proposed approach could also be considered as the next step of this study in order to measure its benefits using water quantity and quality sensors installed at the outlet of the studied basin and over the receiving watercourse."

Answers to Dr. Vaneekhaute:

- *Chapter 1 : References needed:* Required references were added to this chapter.
- *Page 6: "can be" is repeated twice:* Revised.
- *Figure 1.3: reference?* This figure is designed by me so no reference is needed.
- *Page 16, Line 3: "with a the published":* Corrected thank you!
- *Page 28: Sections should refer to the thesis sections,… :* As discussed during the thesis defense, as this paper was published in 2018 the references are not up to date. However, I tried to include more recent works in the Introduction, and other sections of the Literature review chapter.
- *Literature review? Why 1986-2017? It would be good to update this to 2020. Were all 334 documents integrated or was a selection made thereafter? If a selection was made, how?:* The paper was written in 2017 so the year 2017 is considered as the upper bound. And the year 1986 was considered as the lower bound because before this year there was not any significant study in the related literature. A selection was made based on the recognition of the paper (the number of citations and journal impact factor).
- *Table 2-1: Is the control objective always cost here?:* No, we have two other objective functions in terms of Water Quality and Water Quantity.
- *In general, it may be interesting to work with percentage indicators? #% of literature focuses on combined sewers, etc.:* Regarding that the aim is to compare the dynamic approach results with those of the static control approach, the

percentage can represent a proper comparison between these two methods for different performance criteria like peak flow attenuation.

- *Water quality: Main parameter is TSS? Why is this parameter not directly included in your model? I think some of your conclusions should be adjusted, since this parameter is only indirectly included I think.:* As a further study, we can consider the calculation of TSS reduction via simulation or field measurement. However, in this study we only relied on the strong relation between the detention time and TSS removal. This is now discussed in the Conclusion and some sentences were modified to take this comment into account.
- *Figure 2-3: I would expect to see this figure in the methodology of the review.:* It was at the methodology section in the first place. However the reviewers of the URBAN WATER JOURNAL suggested to move this figure in the conclusion.
- *In Section 2.6, you say that SWMM is one of the most widely used commercial software packages, but below you only describe four other software packages. Some more info on SWMM would be appropriate here. Then, why was SWMM chosen in this study? Did you couple it to another software?:* The SWMM is run in PCSWMM in our research and it is coupled with the optimization and rule-based approach to update the system parameters. PCSWMM and EPA-SWMM are the simulation models that are used in our research group in INRS and showed to be efficient tools for rainfall-runoff modelling. Also, this is the hydrologic/hydraulic simulation models that is by far the most commonly used in North America for urban applications.
- *Chapter 3: Your optimization algorithm, was it then programmed in SWMM? :* No, the simulation model was run in PCSWMM and not the optimization.
- *Figure 3-3: reference? The quality of the figure is poor, we can not read all indications:* It is designed by me. I improved the quality of the figure thank you for noticing.
- *Chapter 3: A maximum allowable outflow, is this always applicable? What about heavy rainfall and overflows?:* Yes, this is a parameter set by the municipality and

it is considered as a constraint in our optimization model to be met. Even in extreme rainfall events, the outflow-rates are generated in such a way that this criteria is met.

- *Equations 9-12: How is the 20h and 40h selected? How do you know this is a generalizable estimation?:* I have justified this choice in the text page 61 line 15-21.
- *Figure 3-9: Only volume of controlled situations is shown, why?:* Because we wanted to analyze the performance of dynamic control approach in producing the required detention time to realize sedimentation.
- *"The safety of stormwater basin can be preserved with the proposed algorithm even in case of a low volume capacity basin with the aid of alternative control measures that equilibrate quality and quantity objectives." This is rather vague. Can you elaborate a bit on that?:* In the text we have showed that even in the small basin scenario, no overflow resulted under the dynamic control approach. Hence, we can conclude that the safety of the basin is preserved.
- *Figure 4-1 and sections below: Water quality sensor, what does that imply in your case?:* As mentioned before I didn't measure the TSS reduction however in real-case it is preferable to provide a TSS removal metrics, given by sensors or by simulation. This figure shows a general smart stormwater management system which may include water quality sensors.
- *Chapter 4: Can we have multiple basins in series?*: As discussed before, this model considers only parallel basins and not basins in series. Thus, a sentence is added to make it clear for the reader.
- *Figure 4-4: Suggest to mention which parameters are updated:* "water volume in the basins and observed inflows to the basins" is added as the parameters that should be updated at the start of control horizon.
- *You say that the OVNN model is beneficial in terms of running times and accuracy; this is as compared to what? SWMM?:* Yes, exactly we wanted to produce the simulation behavior while having less computation time so we

compared the OVNN performance with SWMM and found that it can produce similar inflow rates in less time.

- *Concept of an interdisciplinary interconnected city: This is rather vaguely stated. Maybe add some further information on what this would look like since it seems an interesting concept.:* Thank you for your interest. Yes, it is a very interesting subject to study an interdisciplinary control approach that considers different section of smart city, however this is a very broad and general idea that I have and I have never worked on it. So I tried to only point out to this broad idea at the end of my thesis to mention how far we can go.