

Interactive comment on “Design methodology to determine water quality monitoring strategy of surface water treatment plants” by Petra Ross et al.

Joep van den Broeke (Referee)

joep.van.den.broeke@kwrwater.nl

Received and published: 18 June 2019

The paper “Design methodology to determine water quality monitoring strategy of surface water treatment plants” describes the application of a well-considered generic design strategy to analyse the possibilities and impossibilities of using available water quality sensors at one particular water treatment plant.

The design strategy has been derived from a similar strategy developed for a control-design methodology by the (co)authors van Schagen en Rietveld (van Schagen et al in Water Science and Technology, 10, 121-127, 2010).

[Printer-friendly version](#)

[Discussion paper](#)



After a discussion of the aspects to be considered in each of the steps of assessment proposed, one case study is discussed in which an existing monitoring strategy was analysed and optimised.

Unfortunately, it is in the description of this case study and the subsequent conclusions, that the paper fails to address a number of essential elements that are necessary to judge the effectiveness of the design methodology.

Whereas sensor technology, characteristics of available types of instruments as well as treatment process characteristics and operational handles are described in substantial detail, no attention is given to the following aspects:

1) although making selection a combination of sensors, and suggesting (page 13, lines 37-38) that this new combination was actually installed, the only comment the authors make about the results achieved is that more information is now available (page 13 line 38) and that the balance between disinfection and by-product formation might be improved (page 13 line 49). The former is not an achievement of the presented methodology (any additional sensor installed would have resulted in more information becoming available) and the latter also follows from previous works by the authors, and but this is not confirmed in this study. Therefore, no improvement in process control or effluent water quality (the goals of this study, page 2, lines 31 - 37) are demonstrated.

2) As the authors present a method to determine (and / or optimise) a water quality monitoring strategy, it would be logical that the paper presents the results of the selected strategy and demonstrates that the outcome of the methodology actually produces an optimal monitoring setup. The authors do describe the selection process by describing both sensor options and processes (but not presenting any new information) but do not present any cost benefit analysis of the selected strategy, nor do they give any proof of how it improves the pre-existing monitoring setup at the location analysed. Therefore, the effectiveness of the design methodology cannot be verified based on the results presented.

[Printer-friendly version](#)

[Discussion paper](#)



3) An implementation section, describing the experience with the actual deployment and operation of the sensors, this validation the methodology is missing. For an example, this reviewer would like to point the authors to the van Schagen paper (2010) from which the method described here was described. This paper has an implementation section where the approach is validated

As result of the above the authors' conclusion (also found in the abstract) that the water quality programme of the Weesperkarspel plant was optimised is not sufficiently supported.

The same is true for the conclusion on the use of soft-sensors (also mentioned in the abstract) - the authors state that the use of soft sensors helps to gain online information on parameters for which no online sensors are available. Although this may be correct, no new evidence to support this is presented in this paper. For this to be presented as one of the two major conclusions of the paper, it would be reasonable to expect the authors performed and analysis on the effectiveness of soft sensors in this type of application, and / or would have performed a cost benefit analysis (including CAPEX as well as OPEX costs) of using soft sensors vs. direct measurements.

Specific comments

Page 1 - line 26: The authors appear to extrapolate the situation in the Netherlands to a generic statement (regarding sampling frequency). This is, however, incorrect. The sampling frequency depends on the size of the utility and the population served. It also depends on the parameter analysed. This statement currently is too generic. Either it should be restricted to the situation in the Netherlands (where all water companies / treatment plants are so large daily sampling is required or the water companies themselves take daily samples even if not required), or this statement should be rephrased taking into account daily practice also at smaller utilities in other countries.

Page 2 - line 3: time between sampling and analysis takes at least one day. The authors ignore the intermediate option of rapid tests that can be performed on-site.

[Printer-friendly version](#)

[Discussion paper](#)



Utilities use such rapid tests for on-site measurements, e.g. to calibrate online sensors but also for data collection. Various parameters can even only be measured in this way (e.g. DO, O₃, ...). It might be worth adding this type of analysis to the discussion, especially in view of the comment in the next sentence - such rapid tests can be used to verify performance.

Page 2 - lines 5 - 6: In addition, it should not be underestimated that erroneous control and measurement devices can also cause disturbances (van Schagen et al., 2010).

It is not clear what the authors are trying to say with this sentence. Is the goal of this sentence to indicate that human operators can make mistakes and online sensors will detect this? Then remove the reference to erroneous measurement devices. If the goal is to stress the importance of correct use (installation, maintenance, ...) of online sensors, then this should be discussed separately. Currently, this sentence suggests online sensors are not trustworthy, whereas the next paragraph stresses the usefulness of such devices. These are now contradicting each other.

Page 3 - line 39: please reconsider this statement; fast degradation does not necessarily mean a need for high sensitivity and high accuracy. Sensitivity and accuracy will depend on where the measurements are taken and what the purpose of the measurement is. Examples: Does one want to verify the concentration dosed (at injection point) then the decay is less important and concentration will still be high. Downstream concentrations will be lower. but if initial concentration were high (e.g. with other chemical than ozone) still a not-so-sensitive sensor would still do the job. It seems that a fast measurement is most important for rapidly decaying ozone.

Page 4 - lines 26 - 28 Here it is stated the chapter focuses on mixed influent and mixed effluent for the processes ozonation and BAC filtration. However, in the preceding sentence the authors described the treatment plant consists of two parallel lanes. If this is correct, then the methodology appears to be incorrectly applied as it focused on the non-existent situation of mixed ozonation effluent and mixed BAC influent. Could the

[Printer-friendly version](#)

[Discussion paper](#)



authors clarify this and if this is indeed the case (2 treatment lanes and no combination of the water between ozonation and BAC filtration), please correct the text. This can be solved by rephrasing lines 26-28 and removing the reference to the mixed streams.

Page 5 - lines 21 - 22: The authors state that the adsorption capacity (of the carbon filters) decreases with increasing polarity (of the organic matter). It appears that the equilibrium between adsorption and desorption will shift and the affinity of more polar organic matter for the carbon filter surface will be reduced. However, the adsorption capacity of the carbon (the maximum amount of material it can adsorb) should not be affected, as the ozonation does not change the surface properties of the adsorbent.

Page 5 - lines 22 - 23: As a result NOM is removed through ... adsorption. The current wording states that because NOM is oxidised and becomes more polar it is removed through biodegradation and adsorption. Is this true, i.e. would NOM be removed only through biodegradation or adsorption if no ozonation takes place?

Page 5 - lines 33 - 34: Therefore, ... control action. This appears to be a missed opportunity. The authors indicate the flow through the plant is variable (because determined by the demand for drinking water). This means the flow through the treatment lanes is variable. Keeping flow constant could be a very useful control measure for the performance of individual treatment steps. Changes the division of the water over the different production lines, (although not used currently) could thus be used to manage the performance of the treatment processes, and optimise it. E.g. though keeping one lanes at ideal operational conditions, which might result in better net quality of the mixed effluent. It would have been worth investigating this possibility. Eliminating this option at the beginning of the evaluation was a missed opportunity. Did the authors consider this type of evaluation?

Have the authors considered this possibility? Is there information (other than the fact that the plant operators do not use this control option) that it would not be been relevant?

[Printer-friendly version](#)

[Discussion paper](#)



Page 5 - lines 45-46. Because pH is important for the functioning of the BAC filters, can one be sure that the influent water (the effluent of the pellet softeners) is always at the correct / optimal pH? If this is the case, it is correct to disregard this parameter for control of the BAC. If the pH can be variable (e.g. because there is no real-time control of the CO₂ dosing), then pH control should be seen as a control action for the BAC, even though the actual dosing equipment might be part of the pellet softener. Please add a statement that explains why it is not necessary to adjust pH before BAC, e.g. because it is always the same or because it is actively controlled in the softening.

Page 6 - lines 14 - 15: Monitoring of TOC/DOC/SUVA will only be necessary if the composition of the NOM changes. The authors describe the water is seepage water, which is groundwater. Please explain why this variable is relevant for the operation of this treatment plant: is there a variable composition in NOM that would necessitate monitoring in this example.

Page 6 - line 28: Bromate is possibly or probable carcinogenic. Suggest to refer to only one reference that reflects the latest insights into this topic. Referring to both possibly and probable is confusing.

Page 6 - line 38: ATP or flowcytometry. Please explain this statement. There was no reference before to these methods and as to why they would be required. Therefore, the statement on the absence of a need for these methods is confusing and seems irrelevant.

Page 8 - bromide: bromide sensors (ion selective electrodes) are available from various manufacturers. Please correct.

Page 8, Ozone, soft sensor available. Yes, developed based on UV measurement The measurement of ozone using UV spectroscopy is a direct measurement; O₃ has a distinct spectrum which is directly measured. In the case of a soft sensor a number of measurements are taken together to estimate a parameter which can not directly be measured. For O₃ by UV spectroscopy this is explicitly not the case, as the O₃

[Printer-friendly version](#)

[Discussion paper](#)



spectrum itself is directly measured.

Page 12 - lines 27-28: When evaluating... it is not required to monitor ... due to robustness of the BAC filtration step. As the fact that it is not necessary to monitor the AOC was already known from previous work by the authors, why did they ignore this knowledge in the preceding discussion. This type of prior knowledge should flow into the design methodology as early as possible, as it prevents a waste of time (e.g. in this case the discussion on AOC could have been skipped).
Technical corrections
Page 3 line 43: in "A wide range of measurement" Methods or Methodologies or Techniques would be a better term. A measurement is the action of measuring.

Page 3 line 49: measurement range sensitive enough
Incorrect English: the measurement range is wide enough or the method is sensitive enough.

Page 5 - line 1: IpH should read pH

Page 7 - line 4: the word back-wash is missing between "treatment train, next"

Page 7 - line 6: in figure 1 and text describing this figure, it is stated that pressure drop is measured. In this sentence (page 7, line 6) the authors suggested it is not yet measured, but should be measured. Check for consistency and correctness.

Page 8: UV254 Hach 2018 it is strange to refer to the website of one manufacturer for a method which has been on the market for 20+ years and which is sold by a range of manufacturers. Please find a better reference for this. Example: <https://iwaponline.com/ebooks/book/435/Compendium-of-Sensors-and-Monitors-and-Their-Use>

this report contains an exhaustive overview of parameters for which online sensors were available at the time of publication.

Page 8: Surrogate parameters, UV254: Incorrect statement: measurement at all wavelengths is not a surrogate for UV254. If an instrument can measure across this range, it can also provide UV254, but the full spectrum is not a surrogate for UV254.

Printer-friendly version

Discussion paper



Page 8, bromate, Thermofisher 2018 Please explain this reference: referring to one manufacturer as proof that no online method exists is unconvincing. Please only indicate no (as above) which shows the authors have not found a method, or refer to an impartial review.

Page 8: ozone, Hach as comment for UV254.

Page 9: Phosphate, Hach as comment for UV254 and Ozone

Page 9: nitrogen please specific more precisely what parameter is meant here. Total nitrogen, Kjeldahl N, NO₃?

Page 10 lines 10 -11: incorrect cause-effect relationship: the fact that the WHO published parameters does not mean sensors are available (as is suggested here). There are many parameters for which this is not the case. A more correct statement would simply be that for a number of WHO parameters sensors are available and then give some examples.

Page 12: i::scan Why mention specific product where it is only the parameter that is relevant. This product was not discussed before, nor are specific products described for turbidity and pH. Suggest to remove reference to specific product.

Page 13 line 6 and line 7: TM should be in superscript

Page 13 line 7: s::scan should be s::can or scan Messtechnik GmbH

Interactive comment on Drink. Water Eng. Sci. Discuss., <https://doi.org/10.5194/dwes-2019-8>, 2019.

Printer-friendly version

Discussion paper

