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Drinking Water Engineering and Science Discussions

Interactive comment on "Development of a iron pipe corrosion simulation model for a water supply network" *by* M. Bernats et al.

Anonymous Referee #2

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I would like to thank the authors for their time and effort. This research aims at proposing a model to estimate the internal corrosion rate in an iron pipe. The research, in general, sounds novel and very interesting with practical applications. However, I regret to recommend the paper not to be published in the DWES journal. A major revision is required if the authors still wish to submit their manuscript for the publication in the DWES. The rejection decision is not because the research has no novelty; it is rather associated with the lack of clarity of the manuscript. In fact, indeed, the manuscript suffers significantly from many issues including editorial, structural, and technical. All these make the manuscript be very confusing, poorly-written and ultimately very hard to be understood. Very often, the English language is extremely poor and the manuscript requires an ample editing. The literature review section is very short (only 9 articles reviewed). There are other articles with significant contributions in the literature that

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the authors should have discussed in the manuscript. Examples may include (but not limited to) Yamini and Lence (2010), Sadiq et al. (2004), and Rajani et al. (2000). I am familiar with research topic/area and therefore I can imagine the contribution(s) of the manuscript; however, the manuscript must be clear and self-explaining. The authors did not truly communicate contribution(s) very well. The manuscript also stands on few simplifying assumptions for which no evidence or proof is given. As an example, the authors assumed (line3 13-15, page 93) the long term corrosion is a function of mean velocity with changes temperature while other chemicals remain unchanged. The reality, however, is totally different. Clearly, the oxidant level (in this manuscript chlorine) as well as other influential chemicals significantly changes in both short- and long-term. Moreover, I failed to understand why the authors assume the first year corrosion as constant, while their results indicate (Fig. 4) a higher corrosion rate for the first year. As well, the authors should have clearly discussed the bases for the equations (4) and (5). How ere these equations derived? Why did the authors study, in these equations, only Ca, Cl-, SO42-, alkalinity, pH, and temperature, while dissolved oxygen and conductivity have also significant impacts on pipe corrosion? Additionally, the authors chose (lines 18-20 on page 95) a power law for the 2nd region on Fig. 5 simply because they have one single data point. This is not a well-supported argument. In fact, vast verity of fitting functions can be used that match very well with that single data-point. Additionally, there is no consistency between the data on Fig. 4 and those in Table 2? I got different results when I tried to calculate the data on Fig. 4 by using those in Table 2. I am also wondering how the “valid errors” in Table 4 were determined. They do not match with the rest of the data on the Table. Table 2 also indicates a decrease in the-so-called “corrosion process dynamics” in short- and long-term? First, what does “corrosion process dynamics” refers to? Does it mean “”corrosion rate”? Second, how come the “corrosion process dynamics” decrease (Table 2) over time, while mass loosed increases (Fig. 2)?

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