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Interactive comment on "Quantity and Quality Benefits of in-Service Invasive Cleaning of Trunk Mains" *by* Iftekhar Sunny et al.

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Quantity and quality benefits of in-service invasive cleaning of trunk mains

Very nice work, definitely worth publishing.

There are some unclarities in the field work that I think you should address:

* Section 3,2 is water quality assessment, and the trials that were done are some sort of a risk assessment to see how much of a turbidity response is caused by a certain increase in shear stress (due to an increase in flow). This is a controlled flow increase test, if you like. It resembles the standardized RPM (resuspension potential method, Vreeburg and Schaap 2004), except that it was not controlled to the same increase in flow each time the test was done. I believe the term "conditioning test" should be

avoided. It is a risk assessment, not a cleaning action. Also, the PODDS explained shear stress conditioning, to avoid high future turbidity responses, is something very different. Hence, I would avoid using "conditioning" in this paper.

* It is not clear to me why the 6 trials should best be compared (in fig 5) by dividing the turbidity by the product of shear stress and pipe wall area. I would like to see fig 5 also for the clean turbidity*Q data, and a better explanation of why this division of tau is valid or could be valid.

* Why does table 1 not contain the results of trial 3, 4 and 5? Is it possible to find some sort of correlation between ks and turbidity response (corrected for shear if you like)? Could pressure and flow data indicate over time the diameter reduction and thus indicate the growth of the loose material (plus biofilm)? It would be worthwhile to check this briefly and discuss, without being able to prove this based on only one trunk main.

* If for Table 1 I add diameter and two times the roughness, I approximately get the assumed diameter of 228 mm. In the calibration test, is the sum of D and ks limited to this? If yes, please mention this. If not, would it be a good idea to do so?

* Fig 5 suggest turbidity response after 12 months was similar to pre cleaning, whereas the ks was not yet increased to the same amount.

* Fig 4b shows that PODDS was able to simulate the measured data quite well. Since the text says that the max of 1 NTU was not expected, I assume the PODDS result could not have been generated before the trial results were available. It would be helpful to clarify this in the text. I am wondering, what would PODDS have predicted based on the data of fig 4a? This could indicate what the actual results of the cleaning were. Could you use PODDS to predict for each trial what the turbidity response would be for a set controlled flow increase? Thus mimicking the test under the same conditions, and then compare the results. In which case the division by shear stress would not be needed.

* Fig 3: shear stress during ice pigging must be much higher, but is not easy to calculate. Instead, I would leave this part out to avoid confusion. What happened around 27 September (downstream pipe break?)? Caption should say 2015.

* I do not understand how asset deterioration (for other than cast iron pipes) would lead to water quality issues. I can see that if no cleaning is done, time will cause more particulate accumulation, but this does not relate to the age or the condition of the pipe.

* This is one of several studies that "suggests" a temperature dependence. The reference to Sharpe's thesis is very limited. The biofilm explanation is not substantiated with this particular AC trunk main study.

There are quite some grammar mistakes and typos that need to be looked at. For a conference paper the limited number of references was ok, but I would like to see an introduction with some more references added, in order to place this work more in perspective.

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