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Drinking Water Engineering and Science Discussions

Interactive comment on "Correlations between total cell concentration, total adenosinetri-phosphate concentration and heterotrophic plate counts during microbial monitoring of drinking water" *by* E. Siebel et al.

Anonymous Referee #2

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This paper compares two new and rapid methods (total cell concentration determined using flow cytometry and ATP measurements) to heterotrophic plate counts for measuring bacterial quality of drinking water. Measurements were obtained in several buildings on a scientific campus on several dates. The authors found a strong correlation between the two newer methods, but not between these methods and HPC values.

Although the paper is well written and the work appears to be carefully done, it should not be published in the refereed version of Drinking Water Engineering and Science. This is because it is simply too limited in scope at the present time to make a strong

contribution. The authors are encouraged to continue their work, taking into account, among other things, the suggestions below, and eventually to resubmit a much more robust manuscript for publication. It is my understanding that the present version will remain archived and available online in Drinking Water Engineering and Science Discussions,which will be important for the authors and useful for other researchers.

The main limitations in scope, and therefore the main weaknesses of the paper, are that the analyses were only conducted at one location (i.e. with one type of water) and that the concentration range was rather limited: in line 3 on page 76 the authors present results showing that the total cell concentrations ranged over slightly more than one order of magnitude. In terms of bacterial numbers this is a rather narrow range, and does not allow the authors to draw conclusions regarding the broader validity of the correlations obtained. Similarly, the fact that results for only one water at essentially one location were obtained (in this context the different buildings on one campus can be considered as one location) does not allow the authors to conclude how good this correlation might be in general. (Among other things, different waters may be expected to have somewhat different microbial communities.) Even at the existing location the authors state (lines 8 and 9 on page 78) "Results from other floors had often more variations in daily changes, and amongst the various parameters."

If the authors are able to establish more broadly a good correlation between the two rapid methods they should also address how these results would be used in the management of drinking water quality. Since the results provide only an indication of bacterial numbers, and no information regarding the presence of pathogens (bacterial or other), the authors should clearly and specifically indicate how the rapidity of the analyses (which in practice is likely the main advantage of the newer methods compared to traditional HPC analyses) would allow for improved management and intervention practices.

As a final note, the authors may wish to consider using additional methods for determining heterotrophic plate counts, in addition to the one they used. It may be that other

methods, such as the one developed by Reasoner, provide better correlations with the newer methods.

As stated initially, the authors are encouraged to make their investigations more comprehensive to lead to a much more valuable contribution.

Interactive comment on Drink. Water Eng. Sci. Discuss., 1, 71, 2008.