Comment on:
Qualitative monitoring of a treated wastewater reuse extensive distribution system: COD, TSS, EC and pH by T Manios, E Gaki, S Banou, D Ntigakis and A Andreadakis (*Water SA* January 2006, Vol 32 (1) 99-104)

In the study of Manios et al. (2006), variations in some physicochemical parameters of recycled water at the exit of wastewater treatment plant (WWTP) of Hersonissos and in different collectors along the irrigation network are presented and discussed. Having published a paper dealing with effluent quality and quantity, reuse schemes, and economic aspects for the same WWTP (Borboudaki et al., 2005), we would like to clarify some points.

Comments on 'Introduction'

The study of Angelakis et al. (1999) does not include any data regarding the agricultural water use in Crete as reported by Manios et al. (2006). Furthermore, the study of Tsanis and Naoum (2003) deals with the irrigation water demand in Crete which was estimated at 362 ± 135 Mm^3 for the year 1991 and not for the agricultural water use as reported by Manios et al. (2006).

The authors state that *'Standards for wastewater reuse in many countries have been influenced by the World Health Organisation (WHO) Health Guidelines (WHO, 1989) and the United States Environmental Protection Agency (USEPA/ USAID) Guidelines (EPA, 1992).'* However, this statement is not correct. It is well known that the second guideline model is the "Californian Guidelines" which specify required treatment processes and levels of total coliforms less than 23/100 mℓ or less than 2.2/100 mℓ, depending on the use of the recycled water (State of California, 2000).

Comments on 'Methodology'

The basic hypothesis tested in the study of Manios et al. (2006) is that the quality of recycled water in the effluent of WWTP deteriorates throughout its flow in the conveyance network. Indeed the quality of effluent can be affected during its storage in reservoirs or by its retention time within the conveyance network. This is mainly a result of bacterial re-growth or algae development which can occur when recycled effluent is stored in open reservoirs (Kitis, 2004; Derry et al., 2006). However, in the reuse scheme of Hersonissos, recycled water is stored in covered concrete tanks and transported to the irrigated area via a closed irrigation network, thus eliminating the potential for water quality degradation due to evaporation (increase in salt concentration), algal development (increase of TSS) or contamination with inorganic constituents. Moreover, the PVC pipes used in this irrigation network reduce the possibility of increased TSS concentrations due to corrosion. When these facts are taken into consideration, minor or no changes would be expected for the parameters (pH, EC, COD and TSS) investigated in the study of Manios et al. (2006). The only reason therefore for conducting such a study would be to investigate the variation of pathogen populations in sampling points, but unfortunately such data is not provided.

Even if we accept that there are some technical or scientific reasons for conducting the study, the methodology adopted by the authors does not allow for valid conclusions to be reached concerning the potential deterioration of water quality during its storage in reservoirs and/or its flow in the conveyance network. This is due to the fact that the quality of effluent from the WWTP of Hersonissos shows great variations within and between days, particularly in the summer period, because of the large variations in the inflow and the discharged septage (Borboudaki et al., 2005; Figs. 6 and 7 and Table 3). The small storage tank at the exit of the WWTP (300 m^3) is not adequate to compensate for these variations. The recycled water is then transported to the reservoirs, where it is mixed with the recycled water already stored there, hence affecting its physicochemical and biological characteristics. Furthermore, there is a lag time for recycled water to flow from the exit of WWTP to the reservoirs and from there to the collectors. The fact that the pipe distance between collectors differs and the latitude of the collectors from the reservoirs also differs, means that this lag time is difficult to calculate accurately.

Thus, the comparisons made by Manios et al. (2006) actually do not refer to the 'same quality of water' in order to provide concrete evidence for a potential degradation of its quality through its flow in the irrigation network. Based on this fact the differences in the physicochemical parameters (if there were any) reported by the authors could simply have resulted from variations in the effluent quality. Did the authors take into account this fact during sampling?

Finally, the authors state in **Materials and Methods** that '*All the effluent is reused in olive tree irrigation*.' This is false because a significant portion is used for landscape irrigation in hotels and roadsides (for details see Borboudaki et al., 2005).

Comments on 'Results and Discussion'

With regard to the results presented in the study several mistakes can be found and in many points the authors' interpretation is not valid.

Mistakes

The following mistakes were found:

1. In Figure 4, the *Y* axes must be wrong. The EC of the effluent of the WWTP is shown at approximately 8 mS/cm, which is too high.
2. In Figure 6, the *Y* axes must also be wrong. The pH of the effluent of the WWTP is shown at approximately 2.5, which is extremely low. Evidently, Fig. 6 refers to EC and Fig. 4 to pH.
3. In Figs 3, 4, 5, 6, 7, and 8 commas (,) are used instead of full stops (.).
4. The reference Tchobanoglous and Barton (1996) is misdated. The authors probably mean Tchobanoglous and Barton (1991).

pH and COD

In terms of pH the authors state that '*The increase of the mean values in most outlets, compared to those of the effluent, is not substantial and can only be explained…..*' Why do the authors provide explanations for pH change when

no significant effect was observed for that parameter (Fig. 4; Manios et al., 2006)?

The authors state that '*Corrosion and external contamination contribute to and increase (Fig. 7) the average value of COD in comparison with the effluent in almost all 18 collectors*.' However, as results from Fig. 7 show, there is no obvious difference between COD concentration at the exit of WWTP and it the collectors. The average value in the effluent is approximately 60 mg/ℓ, while the standard deviation ranges from approximately 10 to 110 mg/ℓ. How did the authors come to that conclusion? Were they to apply a statistical test to compare means, they would probably withdraw this conclusion. Moreover, the irrigation network of Hersonissos consists of PVC pipes and is closed (the reservoirs are also covered) so how can corrosion or external contamination be justified?

Some lines further down in the paper the authors state that '*Nevertheless, the fact that those excessive values (for COD) occurred during a period of lower inflow (May and end of August, beginning of September), indicates that those measurements may have recorded a random and isolated incident*.' However, Fig. 2 shows that during the end of August and the beginning of September the highest rates of inflow occur and not the lowest as the authors state.

In another part of the discussion it is stated that '*In three out of the ten samplings, COD in the effluent exceeded the limit set by the operational and the EPA reuse guidelines* (Figs. 7 and 8).' However EPA has not set limits for COD in its reuse guidelines, but only for BOD_5 (US. EPA, 1992). The limits of EPA for BOD_5 were never exceeded (Borboudaki et al., 2005).

TSS

With regard to the TSS values, a substantial increase is reported by the authors in collectors compared to the exit of WWTP but again, as for the other parameters, such an effect is not clearly supported by Fig. 9. Even if we accept that a statistically significant increase was observed, the justification provided by the authors, '*corrosion and external contamination should be considered responsible for these increased values of TSS*', is not satisfactory. The extent that corrosion affects collectors depends on the physicochemical properties of the water and hence it would have a similar effect in all the collectors. Moreover, if the rate of corrosion was that high the collectors should have been completely destructed but it is not true. Similarly, if there was an external source of contamination it would have affected all the collectors. In our opinion inappropriate sampling conditions were responsible for the increased concentrations of TSS in the recycled water of collectors. The location of some collectors in the irrigation network of Hersonissos favours the accumulation of TSS which settle, especially in collectors that are not frequently used. In these cases if adequate time is not provided for water to flow from sampling point, settled solids may be released and the obtained samples may not be representative of the recycled water quality. It is therefore likely that some of the extremely high values of TSS (up to 316 mg/ℓ) which have been reported by the authors (collector 4) are in fact due to the release of TSS which had settled in the pipes.

Correlations

The authors refer to close correlations between the values of the parameters (EC, pH, COD) at the exit of WWTP and the collectors without providing any details about them. It appears from the text that the existence of these correlations has been concluded rather than estimated from the existing data. If the authors need to support their point some correlation coefficients should be provided with their statistical significance. In our opinion such correlations cannot be established since all the parameters presented in this study do not seem to differ significantly between sampling points as can be inferred by the high standard deviation during the period of this study (Figs. 3, 5, 7, 9).

Comments on 'Conclusions'

Organic matter (represented in the study by COD) found in the recycled effluent provides food for micro-organisms and its removal improves the efficiency of disinfection. Similarly, TSS are an important parameter in reuse guidelines/regulations because they are related to the effects of particulate mater on shielding the pathogenic organisms from the disinfection process. To ensure a reliable removal of pathogens, particulate matter must be reduced to low levels. Turbidity levels of less than 2 NTU (nephelometric turbidity units) are suggested prior to disinfection in some guidelines/regulations. Therefore, we find the following statement in the conclusions arbitrary '*COD, TSS, EC and pH should be considered of secondary importance as wastewater reuse standards compared to pathogen indicators*.'

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Response to comments made by Dr NV Paranychianakis and Dr KP Tsagarakis on:

Qualitative monitoring of a treated wastewater reuse extensive distribution system: COD, TSS, EC and pH by T Manios, E Gaki, S Banou, D Ntigakis and A Andreadakis (published in *Water SA* January 2006, Vol 32 (1) 99-104)

Response to comments on 'Introduction'

Estimating water consumption for agriculture in Crete is a rather difficult and complicated task. The absence of data has resulted in a number of estimations and calculations. The value presented in our paper originates from information presented in the work of Tsanis and Naoum (2003), the Region of Crete through their own publications and data presented in a study funded by INTEREG III, as well as work un-published at the time of the original submission by Manios and Tsanis (2006). The latter was removed from the manuscript at the request of the editor, since it was still under review at that moment. Angelakis et al. (1999) was confused with another paper of Dr A. Angelakis, Angelakis and Diamantopoulos (1995) which was consulted in order to make the water consumption evaluation.

The comment regarding the influence of EPA and WHO guidelines in many countries' standards is not clear to us, or at least not clearly stated. As long as these organisations have published guidelines, and they have, and as long as these organisations are known world-wide and respected for their work, and they are, and as long as legislators are using them as indicators, and they have, it is safe to state that they have influenced the guidelines of other countries. We never stated that those two organisations are the only ones which have influenced legislative bodies and we never stated that the EPA guidelines did not originate or weren't influenced by California State legislation. On the contrary the comment made by Dr suggests that all relevant regulations all over the world are influenced only by the State of California legislation. It would be helpful to us if the two correspondents could clarify their original comment, since their meaning might be somewhat different and it might make a valuable contribution to the paper.

Response to comments on 'Methodology'

The authors state in an extensive paragraph and for a number of reasons, that monitoring such an extended distribution system for physiochemical parameters does not have an important scientific and technical interest. We conducted this field work trying to evaluate exactly this statement, which the reviewers make so positively. We felt that **we could not be so certain** (science never is) and that even though a number of parameters guarantee the quality of the distributed wastewater, this should be checked. This is the value of such full-scale application monitoring, which was clearly stated in the paper's introduction. The acceptance of the paper for publication by *Water SA* indicates that the journal's reviewers agreed with us. There is a large number of publications mentioning that flow of water and wastewater in long distribution networks effects their quality, for example Lindenauer and Darby (1994); Higgins et al. (2002); Gehr et al. (2003).

We are in agreement with the comments about the quality of the influent, its quantitative variation with time, and irregular flow pattern, the hydraulic retention time in the pipe's network, the storage tank volume (actually there are three storage tanks in the system mentioned in our paper, a fact that might have been missed by the two correspondents) made by Dr Paranychianakis and Dr Tsagarakis. We never stated in the manuscript that all these parameters were controlled or did not affect the results or their evaluation. On the contrary we did regard them as parameters affecting the results. As to their affect on the sampling methodology the following should be noted:

- We tried to overcome all these problems by repeated sampling and analyses on ten different dates over a five to six months period. We hoped that in this way we would be able to present a more realistic picture of what was taking place.
- Our methodology was presented thoroughly to the reviewers of the article, and was relatively simple; samples were taken from 21 points on 10 sampling dates and the samples were analysed as soon as physically possible for six parameters (using standard methodology) four of which are presented in the paper. We support the idea that this methodology represented the reality of the extended wastewater distribution system as well as possible. The results are no doubt open to different interpretations depending on the background and interests of the reader and we have tried to make our own interpretation in a similar way.
- We did monitor indicator populations together with the physiochemical parameters but these data are presented in a separate manuscript submitted to another journal. Mrs Borboudaki, who is a co-author of the relevant-to-our-work paper written by Dr Paranychianakis and Dr Tsagarakis (Borboudaki et al., 2005), must have informed them since she is also a co-author in this second manuscript of ours (Gaki et al., 2006).

We would also like to thank the authors for providing the opportunity to inform not just them but also the readers of *Water SA* that the General Secretariat for Research and Technology (GSRT) of the Greek Ministry of Development funded with €70.000 a research proposal of the Technical University of Crete, the Technological Education Institute of Crete, the Municipal Enterprise for Water Supply and Wastewater Services (MEWSWS)of Hersonissos and the Griffith University of Australia entitled **'Developing an Evaluation Model for Extensive Wastewater Reuse Distribution Systems Effect, on Soil, Plant and Effluent Quality'**. The original research reviewers of GSRT thought that this was a valid issue since in this call from 260 proposals only 60 were funded. This specific proposal received a marking of more than 90%.

Response to comments on 'Results and Discussion'

We would like to apologise for the errors in Figs. 4 and 6 and thank Dr Paranychianakis and Dr Tsagarakis for bringing it to our attention. As they rightly state, the values presented in Fig. 4 as EC are the pH values which should have been presented in Fig. 6 and vice versa. Figures 3 and 5 from which these results originate will hopefully help the readers to see the error.

The reference Tchobanoglous and Barton (1996) is misdated, and the correct date is 1991. Again our thanks to the two correspondents.

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Response to comments on pH and COD

pH

If we understand clearly the two correspondents' objection is that we should not have made any comment on the pH values because there is no substantial differentiation between the sampling points and the influent. We would consider it most unusual to present a value in two figures and then ignore it and not try to make any suggestions or provide any explanations about the values no matter how reasonable, expected or simple those values are. If the correspondents have a different opinion regarding the interpretation we would be pleased to receive their input.

COD

We went through the COD comments again to try and understand the difference between our statements and the interpretation of Dr Paranychianakis and Dr Tsagarakis. Our main observations subsequent to their comments are:

- The data are **available** in the paper and any one can interpret them as their background and ability allows, which is what we as authors have tried to do.
- We never mentioned that the increased COD in the different collectors was **significant** as this would require further statistical analyses to support such a strong statement. If the two correspondents a have conducted such a statistical analyses we would very much appreciate a copy of their analysis or a reference to a suitable publication. We only compared values and mean values, which showed that there is some increased COD in the collectors compared to the effluent.
- Within the limitations of the data we think that the **effluent value determines the COD values** along the distribution network. From reading the comments of the two correspondents we assumed that they also support this statement. In our text and exactly in the next sentence to that presented in the comments we state: *'Nonetheless, this addition of acidically oxidised substances is small in relation to those provided by the effluent, resulting in the second conclusion. If there is no large and easily identifiable external contamination along a reuse distribution pipeline such as this, then the COD variations should be considered predictable and short-range.'*
- The correspondents seem convinced that no external contamination can have occurred. In the practical scenario we were dealing with we were monitoring a system consisting of in excess of 15 km of pipes, three tanks and a significant large number of collectors and we still cannot support **positively the occurrence or not of external contamination**. Even though the pipes were made from PVC, the connections and collectors were not. The photographs below illustrate the types of connections found in the systems. Photo 1a shows a characteristic collector and Photo 1b shows a 'T' connection of the wastewater distribution pipe placed just before Collector 11 in a control/monitoring manhole.
- Regarding our comments on the excessive COD values in the influent and their correlation with the flow rate the comments of the two correspondents are possibly a result of a misunderstanding of the system or perhaps they missed the relevant information in our paper. Our comment referred to septage receiving and not the raw influent from the wastewater collection system. According to information provided by the MEWSWS, August is the peak period for septage receiving with almost 100% increase in comparison to May and September.

(a)

(b)

Photo 1
A collector (a) and a 'T' connection placed just before Collector 11 (b)

- We must, however, mention that Dr Paranychianakis and Dr Tsagarakis were absolutely right about US EPA guidelines, which only include BOD_5 values of 30 mg/ℓ. According to Tchobanoglous and Burton (1991) the relation between BOD_5 and COD values varies from 0.4 to 0.8. The value of 80 mg/ℓ of COD presented in our work as a quality threshold should be considered more than adequate. Laboratory limitations and the number of samples necessitated our choice of COD rather than BOD_5 in our experimental work.

TSS

Regarding the state of the collectors as well as the other metal parts of the distribution network we refer the two correspondents as well as the readers to Photo 1. Collectors have been changed throughout the network over the six years it has been operating. Water was allowed to flow for at least a few minutes before taking samples to ensure they were representative of the pipe contents and not an artefact resulting from local effects due to the connectors. All collectors selected had at least 3 to 4 smaller distribution pipes connected to them, an indication of some consumption (and flow) of the treated wastewater for irrigation. We were unable to determine how often and with which quantities each farmer irrigated his fields.

Characteristically for Collector 4, the extremely high value which the correspondents mention as an indication of incorrect methodology or sampling, we state in our manuscript: *For example, Collector 4 records the highest value of 316 mg/ℓ in*

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September, whereas the collectors before and after (according to Fig. 1) recorded values of 0.0 and 5.0 mg/ℓ respectively. If this value were related to problems with the WWTP, similar values should have been recorded in both those collectors. That was not the case. This strongly indicates that TSS is a parameter easily affected by conditions at the sampling point.

Correlations

The comment of Dr Paranychianakis and Dr Tsagarakis regarding the correlations is accurate, but probably without significance, since it is obvious to any reader that our comments regarding the correlation are conclusive. The words used in our paper were carefully selected and there is no suggestion of any estimations, rather than a careful evaluation of the results We fully agree with the two correspondents regarding the inability to make any meaningful statistically based correlations from the results, however, we failed to find in our text any comment suggesting the opposite. We would like to thank the two authors for reinforcing our own conclusions.

Response to comments on 'Conclusions'

We found an inconsistency between the comments of the two correspondents here and their previous statements. At the bottom of Page 1 and the top of Page 2 they claim: '*The only reason therefore for conducting such a study would be to investigate the variation of pathogen populations in sampling points, but unfortunately such data is not provided*.' If that is their belief why they are so negative about our comment for secondary importance of the physiochemical parameters studied to the reuse standard. We believe that our work shows that in the type of system we studied that, COD, TSS, pH and EC values of wastewater flowing along the pipelines are rather stable and determined by the effluent quality.

General comments

We would like to thank once more Dr Paranychianakis and Dr Tsagarakis for their time and effort in improving our work. As a final statement we would like to add at this point a small section from the introduction of the original paper which highlights why we believe that this survey was important, an opinion probably shared also by the reviewers: '*In order to help determine the most appropriate and reliable sampling point it is important to consider all data, whether produced in research laboratories and controlled environments or in real life applications. The aim of this paper is to monitor such an actual application, in an extensive, operational tertiary treated wastewater distribution system, by correlating COD and TSS concentrations, as well as EC and pH values, with the effluent quality and downstream distance from the WWTP exit. It is hoped it will allow for the determination of how realistic it is to consider the effluent discharge point of a plant, as the point where quality reuse standards should be imposed.'*

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