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LETTER TO THE EDITOR

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Comments on:

"Pollution Monitoring Using Marine Sediments: A Case Study On The Istanbul Metropolitan Area" 23, 39-48, 1999

In their recent paper in this journal (23:39-48), Algan et al. (1999) claimed that their analyses on two batches of sediment samples collected from Istanbul Strait (Bosphorus) detected 'no significant' changes in Cu, Pb and Zn contents, increases in Fe and Mn, and decreases in Hg and Ni. After comparing their results with previous studies, they concluded that the sediment contamination in their study area was not significant. Then, despite acknowledging a significant contribution from municipal wastewater discharges, they concluded that their results were due to the dynamic two-layer circulation in the region.

I have several concerns about their study. First, their sampling and analytical methods may have violated several statistical rules. For example, they did not mention the depth at which surface sediment and gravity cores samples were collected in either their study or previous studies. However, the chemical composition (especially in the uppermost parts) of sea water can vary due to seasonal temperature, salinity differences, waves, and current action at different depths or columns of water masses (Pickard 1964, Parsons et al. 1984). In their case, the uppermost water samples may only represent the chemical composition of the Black Sea since it flows on top of Mediterranean Sea water.

The authors based their measurement of metals upon five replicates. However, they committed, 'pseudoreplication' (Hurlbert 1984) because they obtained the means for their two sampling periods (February and December 1996) from subsamples rather than statistically independent samples. Subsamples should not be treated as samples because pooling subsamples confounds the variance among treatment means with variance among the subsamples (Hawkins 1986, Heffner et al. 1996). Independence among replicates and/or samples is crucial for the determination of the probability of Type-1 error (alpha) (rejecting a true null hypothesis) (Manly 1993, Underwood 1997). Moreover, it appears from Figure 1 that sampling locations were not randomly selected (21 in the Marmara Sea (5 of which were core samples) and 8 in Black Sea (2 of which were core samples)).Yet randomization is one of the most important ways to increase the accuracy and the power of statistical analyses. In this study, it appears that more attention was paid to the Marmara coasts than the Black Sea. As a result, the study may have failed to detect pollution in the Black Sea. Algan et al. compared their measured values with the BCR and AQCS reference material SL-1 and SL-7 (Table 2). They found 'generally small' differences 'between measured and reference concentrations except for Fe and Ni in CRM 142 and Zn in SL-1'. Despite the fact that their water samples were taken from a marine environment, CRM 142 was applied to 'light sandy soil', SL-7 to 'soil', and SL-1 to 'lake sediment'. They failed to explain why they compared these different types of habitats.

Algan et al. compared the mean values of their metal concentrations with those of four previous studies. There appear to be two main errors in their methods. First, they did not define whether they used the mean values of replicates or samples. Second, even if mean values were used in all studies, they did not note how many were used in those previous studies. Comparing means based upon different sample sizes could yield erroneous results.

The authors did not explain whether the selection of their five effluent discharge sites was random. How

many discharge points exist in the study area? This introduces another source of bias into their study because there is only one site on the Black Sea coast but four on the Marmara Sea coast. They found high values of pollution in the Marmara Sea, but ignored the effect of pollution on Black Sea coasts, thereby committing a Type-II error (e.g., Wester 1992). Another error in the study concerns sampling periods. The samples were collected during the winter. However, precipitation over the study area is seasonal, with a winter maximum above 40% (Türkeş 1999). This implies that more precipitation would flow into the seas during winter than summer. It would have been preferable to collect data throughout the year; the current results cannot be generalized among seasons.

Finally, based on my comments above, the results of this study should be re-evaluated; otherwise, the study should not be considered a "case study", as the authors have called it. Such environmentally related case studies include a long period of time with well designed hypotheses, methods and analyses. Results gained from studies performed over a short period (like this study) cannot represent the reality. I agree with them that monitoring studies should be performed over a longer term to provide solid evidence for the effects of pollution on the biological systems. However, even granting my agreement on that point, their results (if not re-evaluated) are not supportable, and are questionable, as I have argued.

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