Expression of concern

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Earlier this year, International Archives of Occupational and Environmental Health (IAOEH) published a paper on genotoxic effects of electro-magnetic fields on human fibroblasts in vitro (Schwarz et al. 2008). The paper appeared on Springer's Online First service in February and then in the May issue of the journal. A few days after the article was published online, an accusation of scientific fraud in this piece of research was made against the authors.

In general, the correctness of results in a submitted manuscript cannot be discussed in a scientific journal unless serious methodical errors, for instance in the statistics, have come to light (for such errors and alternative interpretations of results, some journals have a Letters to the Editor column). In view of the seriousness of the matter, Alexander Lerchl, who made the allegation, was invited to submit his criticisms to the journal as a Short Communication (Lerchl 2008). The authors of the original paper were given the opportunity to reply to Lerchl (Rüdiger 2008). Both papers went through a critical review process with three reports each. The Short Communication and Reply are published in this issue of IAOEH.

In the first part of this Letter of Concern, we address the question of whether accepting the Schwarz et al. manuscript for publication was an avoidable wrong decision by the editors of the journal. The second part discusses the credibility of the results reported by Schwarz et al.

Was accepting the Schwarz et al. manuscript for publication an avoidable mistake?

The peer review process has repeatedly been scrutinized (Creutzfeld 1997; Smith 1999). According to the majority view of the international scientific community, there is currently no better alternative. Nevertheless, an editor must be familiar with the weaknesses of the process in order to minimize the risk of making a wrong decision (Creutzfeld 1997).

As a matter of principle, the editors of IAOEH ask at least three expert referees for a critical review of each paper submitted for publication (Drexler und Schaller 2008). The experts should have the required professional competence but should not come from the authors' own environment.

Scientists familiar with the methodology reviewed the paper submitted by Schwarz et al. After the paper was published online and Lerchl questioned its reliability, an experienced statistician was asked for a further review. Had the faults in the statistics claimed by Lerchl been serious and substantiated, then we as editors would have withdrawn the paper immediately. This could have been done without the approval of the authors or a statement by the Medical University of Vienna, where the research was carried out. However, the post-publication review could not confirm that there had indisputably been data fraud.

Lerchl's criticism focuses on (1) a low coefficient of variation reported in the Schwarz paper, (2) the sum of the figures in a table, (3) the choice of statistical test procedures and (4) confusion between standard error and standard deviation (Lerchl 2008). The last of these is justified. However, the mistake appears in the description of the methodical procedure and does not influence the statistical analysis itself or affect the interpretation of the results.

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The other criticisms of the statistics do not stand up to careful scrutiny.

1. Although the coefficients of variation in the Schwarz et al. paper are without doubt conspicuously low, no statistician but only a scientist who works with these methods can answer the question of whether they are correct. The low coefficients of variation themselves cannot be regarded as clear evidence of fraud which a reviewer should have noticed.
2. The criticism that when 500 cells are counted but the sum of the cells divided up into different groups does not result in 500 is understandable if one is unfamiliar with the method. However, if more than the target of 500 cells were inadvertently counted, it would be incorrect simply to leave out the last cells since this could distort the results. Instead the slightly larger sample should be allowed.
3. Lerchl claims that the authors should have used the classic t-test instead of a non-parametric test. However the t-test is only applicable if a normal distribution and variance homogeneity can be assumed. If these cannot be assumed then non-parametric techniques such as the Mann–Whitney–Wilcoxon test should be used. Non-parametric tests are, however, connected with a loss in statistical power to detect significant differences between groups, which in practice is reflected in higher *p* values. Schwarz et al. correctly chose a statistical test which is more dependable and does not easily produce false positive results.

As editors we conclude that the criticism of the statistics does not justify the serious charge of scientific fraud.

Are the results published by Schwarz et al. reliable?

In August 2008, a paper was published (Wolf 2008) that raised considerable doubt about the correct performance of the experiments reported by Schwarz et al. A key part of the authors' argument was the double-blind analysis of the cells. As well as the usual laboratory-internal blind, a second blind was imposed by using a Xc1950 exposure device to expose the cells to electromagnetic rays or not without this choice being detectable. The exposed/unexposed decoding was always done by an external service after the analysis was finished and the results documented. However, Wolf proved that this sophisticated system could easily be bypassed, simply by pressing a button.

We conclude that an essential part of the Methods section (an externally imposed blind) of the Schwarz et al. paper is unreliable because of the undisclosed opportunity for fraud. Therefore, all subsequent parts of the paper (results, discussion) cannot safely be relied on.

The editors of IAOEH wish to express their doubts about the results reported in the paper by Schwarz et al. (2008) in this EXPRESSION OF CONCERN and to apologize to the readers of IAOEH for publishing this paper. It was unfortunate that they did not learn of the contents of Wolf's manuscript (published online on 31st July 2008) until 12th August 2008.

At this point we want to emphasize that laboratory-internal irregularities cannot be revealed in any review process and that the reviewers, editors and the publisher of a scientific journal always have to rely on the honesty of all persons involved in an experiment.

In the absence of new evidence or further action on the part of either the authors of the Schwarz et al. paper or the authors' institution, the journal will not be publishing further statements or communications on this matter.

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