Neurobehavioural testing in workers occupationally exposed to lead

Whether or not low to modest levels of exposure to lead have a detrimental effect on cognition is an important issue given the growing attention, for example, in the United States, that has recently been paid towards potentially revising downward the levels of lead exposure allowed in the workplace. Thus, we read with interest the meta-analysis of studies that have recently been published towards potential downward revision of the levels of lead that have a detrimental effect on cognition, for example, in the United States.

Whether or not low to modest levels of exposure to lead have a detrimental effect on cognition is an important issue given the growing attention, for example, in the United States, that has recently been paid towards potentially revising downward the levels of lead exposure allowed in the workplace. Thus, we read with interest the meta-analysis of studies that have recently been published towards potential downward revision of the levels of lead that have a detrimental effect on cognition, for example, in the United States.

Unfortunately, we found no direct support for this conclusion in the publication. Moreover, numerous flaws in their method limit any specific inferences that can be drawn. In general, we found that the meta-analysis combined evidence from studies of widely varying quality and did not account for significant confounding within and between studies. Given these and other flaws, it is predictable that the authors did not find an association between blood lead levels and neurobehavioural test scores.

Specific concerns that we had with the method include: (1) The authors offer no evaluation of the quality of the evidence from available studies based on study design and analytical method. (2) The authors combine data from poorly done studies with data from well-done studies, clouding any effects that are observable from the better conducted studies. (3) Although age and education adjustment within studies is assessed, six studies were included that did not adjust for age and another three studies did not adjust for education. These are the two most well-established predictors of neurobehavioural test scores and the most important potentially confounding variables. (4) Even among the remaining studies that did adjust for age and education, the authors do not address the confounding in the meta-analysis that is caused by variation in age and education across study populations. (5) The authors’ main effect measure is an exposed versus control comparison. Among the options that could have been pursued, this is the effect measure with the lowest power. It is unable to assess a dose-effect relation, and it is also the one most prone to selection bias. (6) Reliance on a small number of unspecifiable studies for effect estimates. Table 2 of the meta-analysis reports the number of studies that were combined to derive effect estimates, but does not specify which studies were combined. This omission does not allow the reader to determine whether solid evidence was combined with more questionable evidence, or to evaluate whether any of the issues described above were germane to the effect estimates reported.

Two more concerns exist regarding the authors’ treatment of the issue of cumulative versus ongoing lead exposure, as well as the identification of the source of funding for this study. In their introduction, the authors quote the review by Balbus-Kornfeld et al, which noted that “the current scientific evidence is flawed because of inadequate estimation of cumulative exposure to or absorption of lead”... but fail to acknowledge this issue in the interpretation of their own meta-analysis. In fact, as has been widely reported in the literature, methods are now available to non-invasively measure bone lead levels as a reliable and accurate measure of cumulative lead dose. Several studies suggest that cumulative lead dose, as measured by tibia lead levels, is a very important biological marker that may be related to cognitive decrements not predicted by blood lead levels. With regard to funding, the authors note that they are mainly from the Exponent Health Group in Alexandria, Virginia, and Menlo Park, California; however, they fail to describe what motivated the study or sources of funding for the study. We believe this information would be of interest to scientists and policy makers engaged in work on this topic.

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References
Suicide mortality among electricians

Järh olm and Stenberg have evaluated suicide mortality rates among electricians (“exposed to electromagnetic fields (EMFs)”) and glass and wood workers (“unexposed to EMFs”) in the Swedish construction industry. Standard mortality rates were lower for the two job groups compared to the Swedish general population. This is likely to be due to the healthy worker effect. The internal cohort analysis showed that electricians had a lower suicide mortality rate than glass and wood workers.

As the authors rightfully point out, these results should not be seen as evidence against the association between exposure to EMFs and suicide, in particular because no quantitative estimates of exposure were obtained to directly evaluate this association. Järh olm and Stenberg cited a small measurement survey in the Swedish construction industry, which indicated that exposure levels were low and comparable between the two occupational groups. Therefore, one would not expect to see an EMF mediated increase in suicide risk among electricians compared to glass and wood workers, if an association between EMF exposure and suicide truly exists.

Järh olm and Stenberg suggested that the difference in suicide rate between the two job groups was unlikely to be due to differences in socioeconomic factors, but they do not provide an alternative explanation. One possible explanation may be a healthy worker survivor effect related to employment status (for example, at time of death) within this cohort. That is, active workers may be more physically and mentally fit than those who left the industry or are unemployed, and may therefore be at lower risk of committing suicide. A large body of literature suggests that unemploy ment and suicide are positively related, and being out of work was positively associated with suicide in the electric utility industry. Since cessation of work also leads to cessation of work related exposures, employment status may be an important potential confounder (or perhaps effect modifier) for the association between work related exposures and suicide. The lower suicide rate among electricians compared to glass and wood workers may be explained by a larger proportion of glass and wood workers with an inactive employment status at the time of death.

Although it is unlikely that consideration of employment status, if possible, would greatly alter the conclusions reached by Järh olm and Stenberg, it would be informative to see its influence on the rate ratio.

References


Authors’ reply

We appreciate Dr Wijngaarden’s interest in our report and his suggestion for understanding the differences in risk. Dr Wijngaarden suggests that difference in unemployment between electricians and glass and wood workers could be an explanation.

We have no data on employment status at time of death and can therefore not test this hypothesis. However, if employment status is an important predictor, this could explain some of the difference, as the wood workers had a different employment structure to the other groups. Electricians and glass workers had permanent positions for a long time, while wood workers were employed for a certain project, for example, building a house, before the 1990s. When the project was finished they had to find another employer.

Today, most construction workers have permanent positions for a long time, and some are employed full-time for years. Furthermore, the older hairdressers had more customers and observed an increased prevalence of respiratory problems in older hairdressers. However, the authors focused on age as a risk factor for occupational asthma among hairdressers (bleaching, dye, and permanent wave) and observed a 3.6 times higher risk of occupational asthma among hairdressers with high duration, which probably is a result of the healthy worker effect. Hollund and colleagues reported a significant trend ($\chi^2$ = 4.9, $p = 0.027$). However, we did not observe any excess by work duration, which probably is a result of the healthy worker effect. Hollund et al stated that the older hairdressers had more customers than the younger ones, which may be

References


Importance of work intensity on respiratory problems in hairdressers

We read the report by Hollund et al with great interest. We agree that there is limited information about the prevalence of respiratory problems caused by highly reactive chemicals in hairdressing salons. In this well designed study, authors focused on age as a risk factor and observed an increased prevalence of respiratory symptoms among the oldest and youngest hairdressers and observed more symptoms among hairdressers over 40 years of age.

Work intensity, work duration, working conditions, and job titles (master, and fellow hairdresser) should also be considered as risk factors for occupational asthma and respiratory symptoms. With the exception of work intensity, these features have been reported as risk factors in previous studies.** Work intensity is an objective parameter for evaluating occupational exposures. In our study, we calculated work intensity from the average number of chemical applications per week (bleaching, dye, and permanent wave) and observed a 3.6 times higher risk of occupational asthma among hairdressers with high work intensity ($\chi^2$ = 4.9, $p = 0.027$). However, we did not observe any excess by work duration, which probably is a result of the healthy worker effect. Hollund et al stated that the older hairdressers had more customers than the younger ones, which may be
In the United States and United Kingdom, the term “hairdressers” is inclusive, denoting women’s hairdressers and barbers for men. In Turkey, however, the term addresses women’s hair salons only. Most of the studies on hairdressers have been published from Nordic and industrialised countries. Studies from developing countries will help to describe the extent of occupational health problems among hairdressers and to identify aetiologi cal factors.

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References
6 Occupational exposures of hairdressers and barbers and personal use of hair colorants; some hair dyes, cosmetic colorants, industrial dyestuffs and aromatic amines. IARC monographs on evaluation of carcinogenic risks to humans 1993; 57:43–66.

NOTICES
27th International Congress on Occupational Safety and Health at Work, Iguassu Falls, Brazil, 23–28 February 2003

The Congress will have about nine keynote conferences, addressing different aspects of the Central Theme; those themes will then be discussed in depth by Panels (60), where different opinions will be debated. There will be about 60 mini-symposia organised by ICOH Scientific Committees and Work Groups; facilities for the presentation of 1000 posters; and about 500 free papers. Interest groups may schedule meetings in Congress areas.

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First World Congress on Work-Related and Environmental Allergy (1st WOREAL), and Fourth International Symposium on Irritant Contact Dermatitis (ICD), Helsinki, Finland, 9–12 July 2003

Congress on Work-Related and Environmental Allergy
• Work related and environmental aspects of respiratory and skin allergy
• Specific issues related to pathophysiology and skin allergy

• Management and prevention of allergy

Irritant Contact Dermatitis Symposium
• Occupational irritant dermatitis
• Prevention of irritant dermatitis
• Alternative methods for the assessment of irritants
• Irritant dermatitis from cosmetics

Satellite events
• Satellite Symposia, 9 July 2003
• Allergy School, 9–10 July 2003
• 7th International NIV A Course on Work-Related Respiratory Hypersensitivity, 11–15 July 2003

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CORRECTIONS
We apologise for the following errors in table 4 of the paper by Wong et al (Associations between daily mortalities from respiratory and cardiovascular diseases and air pollution in Hong Kong, China. Occup Environ Med 2002; 59:30–5).
• Mortality from pneumonia and influenza: 4 Pollutant model, under NO: “1.004 (1.017 to 1.025)” should read: “1.004 (0.984 to 1.025)”.
• Mortality from ischemic heart diseases: 2 Pollutant model, also under NO: “1.022 (1.011 to 1.033)” should read: “1.022 (1.011 to 1.033)”.

We apologise for the following error in the paper by Yassin et al (Knowledge, attitude, practice, and toxicity symptoms associated with pesticide use among farm workers in the Gaza Strip. Occup Environ Med 2002; 59:387–393). The page reference at the start of the paper should be 387–393, and not 387–394.