This hobby requires inspiration and breathing of large volumes of air, making the lung alveoli expand more than in other people. This fact could facilitate the penetration of carcinogens in the cells of the lung epithelium, and this could be more harmful to smokers. We have found no other studies that have reported this possible association. It would therefore be necessary to explore this association in greater samples of professionally exposed persons to ascertain whether this finding is consistent or due to chance.

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How important is personal exposure assessment in the epidemiology of air pollutants?

The paper by Harrison and colleagues1 and the accompanying editorial by Cherrie2 in the October 2002 issue of Occupational and Environmental Medicine address the important issue of personal exposure assessment (of air pollutants) in environmental epidemiology. After reading both papers we would like to make some comments with regard to the design, conduct and statistical analysis of the study by Harrison et al and at the same time answer the question raised by Cherrie in his editorial.

Coming from the occupational exposure assessment arena it is interesting to see that our environmental colleagues are still relying to a large extent on static (microenvironmental) sampling and even rely on shadowing to represent personal exposure. The latter brought back memories of old occupational hygiene textbooks with pictures of technicians standing with a sampling probe in the breathing zone of a worker (clearly hindered while carrying out his work task). It is interesting to note that Dr Cherrie’s very relevant earlier work3 on whether wearing sampling pumps affects exposure (it hardly did) was not mentioned in both papers.

The paper by Harrison and colleagues1 clearly states as one of its goals to answer the question “Does modelling through the use of microenvironment measurements and activity diaries produce reliable estimates of personal exposure to air pollutants?”. However, in the only setting where personal exposures were actually measured (phase 1, volunteers; with regard to phase 2 we do not think that shadowing results can be seen as equivalent to personally measured exposure) it is hard to grasp from both fig 1 and table 2 which exposure was actually modelled (1 hour averages, 2–3 day averages) and how (a formula was only provided for measurements within the susceptible groups).

When comparing direct personal measurements for CO and PM10, with the modelled results, the authors exclude all data which are not directly comparable—that is, when the volunteer spent most of their time out of house, and all the data for smokers. It is therefore not surprising that good correlations were found between the two exposure measurement results. Why were smokers excluded? Was their measured CO exposure representing a different kind of CO leading to a different health effect? We know that excluding smokers or people with unventilated gas heaters is common practice in the statistical analyses of environmental exposures, but this would only make sense if we were expecting different risk from the same exposure originating from different sources.

In fig 1 the authors present 120 comparable data points for 11 individuals; given the repeated nature of the sampling these data points cannot be seen as statistically independent. Putting a simple regression line through these points is therefore not correct and application of a mixed effects model would have been more appropriate. Besides that, when estimating environmental exposure, for instance, for a panel study, we are interested in the full range of exposures both in the temporal and spatial sense (not only for the room with the static sampler). However, Harrison et al conclude, “…modelled personal exposure is unable to reflect the variability of measured personal exposures occasioned by the spread of concentrations within given microenvironments”.

Both Cherrie and Harrison et al claim that microenvironmental sampling would be a good alternative for direct personal exposure measurements that supposedly are “costly and time consuming”. However, the costs for sampling microenvironments in a general population study will be far greater if we want to measure all the microenvironments people end up in (for instance, in table 7 seven environments are indicated, and most of them will most likely be different for each study participant). In addition, it will be practically impossible to measure some of these environments as the authors point out. In their study, it was not possible to collect data for all appropriate microenvironments, even for a comparatively small number of subjects.

Recently, a very insightful paper was presented at the X2001 conference in Gothenburg. Seixas and colleagues4 showed that in a study to assess occupational noise exposure, a task based methodology (analogous to microenvironmental sampling in environmental exposure assessment) could only account for 30% of variability in daily exposures. They even considered this estimate somewhat optimistic since their estimated noise exposures were derived from the same data on which the daily average exposures were estimated. In addition they clearly pointed out that using simple task based averages that artificially compress exposure variability resulted in a very substantial negative bias in the estimated daily exposure.

In our opinion, we should aim to collect personal exposure measurements when estimating exposure for epidemiological studies.
We agree that smaller and lighter sampling instruments will need to be developed, as was suggested by Cherrie in his editorial. Recent studies in both the occupational and environmental arenas have shown that study subjects are capable of carrying out personal measurements (and by doing so, cutting out all the costs of the technician). In all these studies except one, far more than 100 personal measurements were generated, which shows that studies of this size are not exceptional as was suggested in the editorial by Cherrie.

The question raised by Cherrie, “How important is personal exposure assessment in the epidemiology of air pollution?”, can only be answered with a firm “very important”, if we want to capture the full range of personal exposures experienced in the general environment, which shows that studies of this size are not exceptional as was suggested in the editorial by Cherrie.

Microenvironmental monitoring and consequent modelling based on diaries will not provide sufficient resolution and accuracy. The wearing of sampling pumps affect exposure assessment in the epidemiology of air pollutants.

A number of epidemiological, experimental, and clinical studies have suggested that endotoxin is effective against cancer. A recent study by Pfaller et al reported that endotoxin has been reported by others conducting human investigations as suggested by Rapiti and colleagues. Rylander and Lange previously reviewed the epidemiological literature on reduced cancer rates in various occupations that are exposed to endotoxin.

Experimental studies have suggested that benefit of endotoxin exposure is most effective during initiation of lung cancer with a finding of less benefit for established tumours. The relation of endotoxin to occupational setting is not well understood. Benefits of endotoxin were seen in sewage workers, mortality study of cancer among sewage workers.

Mortality studies of machining fluid exposure with risk from endotoxin. Experimental studies have shown that endotoxin in an occupational setting is one of benefit, but rather methodology and bias, including differences in smoking rates. However, this explanation is not supported by experimental and clinical investigations involving endotoxin. The major influence on lung cancer is tobacco use (smoking). Although smoking is identified as one of the reasons for lower than expected rates in some populations, some studies have shown that smoking is not always an explainable factor or bias for reduced lung cancer. For example, Rapiti and colleagues reported that the consumption of cigarettes and prevalence of smoking in a population of municipal waste workers was higher than the general population, but the incidence of cancer deaths (standardised mortality ratio) for lung cancer in this group was 0.55. Epidemiological studies need to include and report not only detrimental outcomes but also potentially beneficial associations.

References
Neurobehavioural testing in workers occupationally exposed to lead

The article of Dr Goodman and coworkers on "Neurobehavioural testing in workers occupationally exposed to lead" covers an interesting approach with a surprising main message: "None of the individual studies is conclusive or adequate in providing information on the subclinical neurobehavioural effects ...". The article also argues that alcohol use or the presence of pre-existing neuropsychiatric conditions could also act as powerful confounders. The studies in our meta-analysis had varying strengths and limitations and further inclusion or exclusion based on quality would be a matter of judgement. However, an additional analysis based on the 13 studies that adjusted for age and education and their meta-analysis results were still opposed to the original findings based on all 22 studies, none of the tests showed a statistically significant difference in all three models. (See OEM website for results table.)

(4) "Reliance on experimental comparisons rather than only including studies that reported beta coefficients for the blood lead versus test score relation, or adjusting for mean blood lead levels in exposed and non-exposed groups." The authors conclude that blood lead levels, that are described as 'moderate' in one location in the manuscript and 'low' in another, are not associated with neurobehavioural test scores. This represents our conclusions listed on page 222 of our paper.

(6) "Reliance on a small number of unselected studies for effect estimates. Table 2 of the study reports the number of studies that were combined to derive effect estimates, but does not specify which studies were combined." The original version of the paper included information on each individual study; however, based on the reviewers' and editor's comments, we had to shorten the manuscript substantially. We will make this information available online as an addendum to the purported omission from our meta-analysis of the May 2001 article by Schwartz and colleagues.

For the Santa Ana preferred hand test, the effect size changed from non-significant negative to non-significant positive. For the Santa Ana non-preferred hand the result changed slightly towards the null and remained statistically non-significant. For the visual symbol test, the results changed from the null and remained statistically significant in the fixed effects model, but changed slightly towards the null and was no longer statistically significant in the random effects models.

(3) "Inclusion of studies that did not control for age and education." Schwartz et al do not provide evidence that age and education are the two most important predictors. They argue that alcohol use or the presence of pre-existing neuropsychiatric conditions could also act as powerful confounders. The studies in our meta-analysis had varying strengths and limitations and further inclusion or exclusion based on quality would be a matter of judgement. However, an additional analysis based on the 13 studies that adjusted for age and education and their meta-analysis results were still opposed to the original findings based on all 22 studies, none of the tests showed a statistically significant difference in all three models. (See OEM website for results table.)

(4) "No adjustment for age or education, or lead dose differences across studies." This criticism appears to be somewhat theoretical, as the data did not allow such adjustment.

(5) "Reliance on experimental comparisons rather than only including studies that reported beta coefficients for the blood lead versus test score relation, or adjusting for mean blood lead levels in exposed and non-exposed groups." The authors conclude that blood lead levels, that are described as 'moderate' in one location in the manuscript and 'low' in another, are not associated with neurobehavioural test scores. This represents our conclusions listed on page 222 of our paper.
We have found inconsistent mental health results in our three recent studies examining the impact of aircraft noise on child health around Heathrow airport.2 3 In the West London Schools Study,4 aircraft noise was weakly associated with hyperactivity and psychological morbidity as measured by the Strengths and Difficulties Questionnaire (SDQ) completed by parents. The SDQ is one of the most widely used psychometrically valid instruments to detect psychological morbidity in children in both the UK and internationally. However, in our other two studies using both the parent completed SDQ, the teacher completed Student Behaviour Checklist, and child self reported Depression (Child Depression Inventory, CDI) and Anxiety (Revised Child Manifest Anxiety Scale) we did not find any association between mental ill health and aircraft noise exposure.5

The Austrian results should be placed within the context of existing studies with respect to two points: (1) the construct being measured in the Austrian study; and (2) the small effect size and inconsistency with previous research.6

In the Heathrow studies we used internationally recognised child mental health screening tools, that have equivalent psycho-metric properties to those used only in German speaking countries. It is worth noting that the KINDL is normally defined as a “valid and reliable index of quality of life,” rather than a sensitive screening tool to detect specific mental health problems. It is possible that the mental health results reported by Lercher and colleagues are tapping into impaired quality of life and wellbeing, rather than a precise mental health outcome such as “depression.” The definition of “mental health” used by the authors needs to be clarified. The fact that the Austrian results do not replicate our Heathrow results raises the question: Does the KINDL measure wellbeing and quality of life rather than mental health? Furthermore, teacher reports of classroom adjustment would not normally be classified as a “mental health” problem. It might be more accurate to conclude from the Austrian research that: “ambient levels of noise in the community are associated with decreased quality of life and poorer classroom behaviour (rather than “mental health”) in elementary school children.”

In summary, we feel that new research is necessary to provide further evidence about the effects of noise on child mental health. Even though Lercher and colleagues have taken the field of research forward with their two stage study design strategy, there is still more work to be done to clarify the terminology and measurement of mental health in the field of non-auditory effects of noise. Specifically, a clear definitional and operational distinction needs to be made between stress/wellbeing/quality of life and mental health.

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References


No change in sex ratio in Ramsar (north of Iran) with high background of radiation

A few areas of the world show high levels of natural radiation, and one of these areas is located in Iran. Ramsar is a northern coastal town situated in the Caspian littoral (in Mazandaran province, Iran) on the slopes of the Alborz mountain range, and in a coastal area. It is situated at 49° 40’ eastern longitude and 36° 53’ northern latitude. The area is rich with mineral springs. Investigations into the amount of radium-226 in water started more than 30 years ago.7 It has been reported that inhabitants of Ramsar receive an annual radiation absorbed dose from background radiation that is up to 260 mSv, much higher than the 20 mSv that is permitted for radiation workers.8

Annual births subdivided by gender, were obtained from Statistical Center of Mazandaran province.9 Because of the relatively small number of annual births in the urban area of Ramsar (currently about 670 per annum), analysis was carried out on the 11 year total for male and female live births, for the period 20 March 1989 to 19 March 2001, equal to Iranian calendar 1368 to 1379 Hejirae (HS). The data was not available for the 1378 HS (equal to 20 March 1999 to 19 March 2000).

To test the null hypothesis that the probability of a male live birth in Ramsar is equal to that in the control populations, a χ² test was conducted. A value of p<0.05 was considered significant. The sex ratio is expressed as the proportion of total live births that were males.

The sex ratios at birth in the urban area of Tonkabon, the nearest city to Ramsar (about 20 km distance) and the urban areas of Mazandaran province (excluding Ramsar) were used as controls. The overall sex ratios in Ramsar, Tonkabon, and the urban areas of Mazandaran province were 0.511 (total live births = 7591), 0.517 (total live births = 14266), and 0.509 (total live births = 253981), respectively. There was no significant difference between Ramsar and either Tonkabon (χ² = 0.95, df = 1, p = 0.33) or urban areas of Mazandaran province (χ² = 0.13, df = 1, p = 0.71).

It has been reported that the sex ratio in the offspring of male radiologists is significantly lower than that in control populations.10 However, this is not consistent with the present result. This discrepancy could be attributed to the exposure of both parents to ionising radiation. Alternatively, because the inhabitants of Ramsar have lived for many generations in an area of high background radiation,
The traditional method of administering amyl nitrite to victims of cyanide poisoning is to break an ampoule in a handkerchief and then intermittently hold this under the victim's nose. 1, 2

I would like to suggest two alternative methods for administering amyl nitrite. The first method is to use a nebuliser. The second method is to use an inhaler similar to the Pentrox device, normally used to administer methoxylfluorane for emergency analgesia.

With appropriate training, either method could be used by first aid staff. This could be of particular value to remote mines sites where the absence of medical staff may preclude intravenous administration of cyanide antidotes such as dicyclobutidate, sodium thiosulphate, sodium nitrite, or hydroxocobalamin.

Both methods offer the following advantages over the traditional method:
- Oxygen can be administered during treatment
- Rapid delivery of the drug
- Accurate dose delivery
- Less risk of inhalation by first aid or medical staff
- Less risk of injury due to glass fragments

The inhaler device would also be particularly well suited to the treatment of large numbers of victims following industrial disaster or terrorist attack—the risk of which has been recently alluded to. 3

One concern about introducing these methods is the potential for amyl nitrite toxicity. Experimental research is recommended to determine safe dosages and frequencies for each method.

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References
that the cases were circulated to all the contributors to this volume and the overall response summarised in the text. The lack of complete agreement in many instances is comforting at one level—“textbook” cases are the exceptions in practice—and this approach gives a far better feel for the real life situation.

Another attractive feature of this book is the chapters dedicated to descriptions of certain industries and the problems that arise from those workplaces, including mining, farming, the automotive industry, and health carers among the seven chapters. This does lead to repetition of some information between chapters but, as the authors rightly point out, will tend to dip into one particular part of the book, and repetition under these circumstances is helpful rather than an irritation. The chapters on specific disciplines used in the investigation and management of occupational lung disease (for example, imaging and occupational hygiene) are good and sufficient for most needs in this context. The chapters on legislation divided geographically into North America, Western Europe, and the Pacific, Far East, and Australasia is an excellent attempt to widen the relevance of the book.

My criticisms are few and minor. While there are good generic sections on how to take and analyse an occupational history and on surveillance, it might have been a useful addition to include a chapter on epidemiological aspects unrelated to surveillance and more to the research field. This would allow greater expansion on the healthy worker effect and perhaps also the opportunity to compare the now burgeoning literature on the health effects of the broader environment and how these findings might apply to the occupational scene. Boxes have been used for specific sections within chapters. Sometimes this works, but sometimes it does not. There are one or two boxes which run to four or five pages and I feel that these would quite happily sit as sections within the chapter rather than boxes. Boxes need to be short and punchy.

This book is an excellent addition to the literature in this area, complimenting nicely the classical standard textbooks, and at a penny under £100 is good value for money. It is targeted at all physicians, hygienists, health and safety officers, and administrators, and successfully hits that target for all these groups. For exam purposes (for example, AFOM in the UK) this should be regarded as the standard text.

J O Ayres

Bone’s Atlas of Pulmonary and Critical Care Medicine, 2nd edition

Edited by G Douglas Campbell Jr and D Keith Payne (pp 315 plus index and colour plates; £95) 2001. Hagerstown, MD: Lippincott Williams & Wilkins. ISBN 0 7817 3436 3

This book covers to cover an enormous subject, and the editors have done very well to contain it to a little over 300 pages. Its 26 chapters are grouped into six sections, in the latter’s order being occupied by respiratory topics, with critical care being limited to the relatively short final section. The atlas format is certainly stylish and on the whole achieves the editors’ aim—that is, of helping busy clinicians and students of chest disease absorb a large amount of information in a relatively short amount of time.

Despite the numerous contributors, the book’s layout is uniform and very accessible; text is limited and punchy and extensive use has been made of diagrams, flow charts, and tables to supplement the generally good quality photographic images. The grouping of the colour plates to the final pages of the book, to contain printing costs, is a little distracting but a justifiable compromise.

All of the material is up to date and well referenced, though tends to some extent to be dominated by North American sources and opinion. I found the chapters dealing with lung cancer, bronchiolitis obliters and other bronchiolar airway disorders, and sarcoidosis to be particularly useful and excellent sources of a large and diverse amount of information. In contrast the chapter dealing with interstitial lung disease was to me a little disappointing. The chapters covering sleep disorders, HIV and fungal infections, lower respiratory tract infections, and nutrition are new to this edition and are welcome additions. The use of graded evidence based recommendations for diagnostic and therapeutic interventions is variable between chapters and its more consistent application would add further to this book’s already considerable value.

I am sure this atlas will have broad appeal to both undergraduate and postgraduate students of chest medicine as well as busy practitioners. It would be a valuable aid to those preparing for postgraduate exams as well as to specialist registrars in respiratory medicine, who I’m sure would find it a very useful source throughout their trainee years. Intensivists and trainees in critical care will, I expect, find the balance towards respiratory medicine less appealing. It has few competitors in terms of its breadth and clarity and it represents good value for money; in short it deserves a place in all good medical libraries.

W S Tunnilliffe

The Health Effects of Chrysotile Asbestos


The famous mortality study led by Corbett McDonald has followed 11 000 Canadian chrysotile miners and millers until 80% were dead; only 33 mesotheliomas were reported and excess lung cancers occurred only at very high exposure levels. Yet that same chrysotile used in textile manufacture in South Carolina was associated with a 50 times greater lung cancer mortality.

This volume, published in 2001 by The Canadian Mineralogist, reports the papers presented and the ensuing discussion and commentary at a symposium in 1997 called by the Canadian Government to discuss the health issues surrounding the continued production and use of chrysotile asbestos. Can the mineral be used safely? To most uninformed observers, the answer must be a clear no. The true answer is of course not so cut and dried. Much of the evidence suggests that chrysotile itself is much less hazardous than the asphaliboles and that the serious risks associated with chrysotile are a consequence of its contamination by tremolite, an amphibole that is found in geological intrusions into the amphibole or body. These are the issues discussed by the distinguished geologists, mineralogists, epidemiologists, risk analysts, and pathologists who contributed to the symposium. Among them are the last published contributions of two who made great contributions over many decades to investigating the hazards of asbestos and to protecting workers, the late Chris Wagner and Bob Murray.

The resolution of this conundrum may seem unimportant to those who live in countries where past exposures have been to mixtures of amphiboles and chrysotile and where use of asbestos has effectively ceased. However, industry continues to need durable fibres and the poor world sees substantial advantages in using cheap asbestos cement for water pipes and roofing material. And the issue is of course important to the Canadian and Russian chrysotile industries and their employees. Anyone who has been involved in the asbestos debate, who gives advice to industry or lawyers on asbestos issues, or who is interested in the complexities of the interface between science and regulation will find much of fascination in this volume.

A Seaton
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CORRECTIONS

The authors of “Association between job strain and prevalence of hypertension: a cross sectional analysis in a Japanese working population with a wide range of occupations: the Jichi Medical School Cohort Study” (Tsutsumi A, Kayaba K, Tsutsumi K, Igarashi M, Occup Environ Med 2001;58:367–7) have asked for the following errors to be pointed out.

• There are errors in the abstract (line 16) and text (page 368, left hand column, line 5). A part of the baseline data was collected in 1995 in two of the 12 study sites so that the correct period was 1992–95 (not 1992–94).

• On page 368, left hand column, line 24, the number of older participants (over 69) should be 696 and not 699.

These facts do not, however, affect the study findings.

We apologise for the following error in the book review, “Late lessons from early warnings: the Precautionary Principle 1896–2000” by R L Maynard. A copy of this book is available to download free of charge from EEA Online. The URL, however, was published incorrectly. The correct link is: http://reports.eea.eu.int/environmental_issue_report_2001_22/en.