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References


Personal exposure assessment in the epidemiology of air pollutants

In commenting on our paper published recently in Occupational and Environmental Medicine, Kromhout and van Tongeren admonish us for paying insufficient attention to the earlier literature on occupational pollutant exposures. While no doubt an element of their criticism is justified, we feel that the exposure situation for the general public is sufficiently different that it should not be assumed that findings in the occupational environment can necessarily be extrapolated to environmental exposures of the general population. A large component of the exposure arises from diffuse sources and may therefore be very spatially homogeneous at locations such as people’s homes which are often relatively remote from outdoor pollution sources.

There has been some controversy in the literature regarding the extent to which measurements at fixed central urban background locations can reflect the exposure of large urban populations who spend much of their time indoors at locations relatively remote from the monitoring station. It has been typical to find that for an individual, daily personal exposures correlate with concentrations at the monitoring station, while if data are pooled from many individuals, the exposures appear to be uncorrelated with ambient air data. This finding suggests that the diffuse background as represented by the central urban monitor does account for a substantial proportion of variance in the exposure of an individual, and this conclusion is supportive of causality in the time series epidemiological studies, which would appear implausible if the monitoring data were unrelated to human exposures. The finding of our paper that microenvironment measurements do, in general, well represent individual personal exposures in that microenvironment (except for the personal cloud of PM10) is far from self-evident from much of the earlier literature and is a useful addition to knowledge. The fact that cigarette smokers were outliers in the regression analysis shows not unexpectedly that they generate strong local concentration gradients and would therefore need to be treated differently in any modelling of personal exposures. In the absence of such local sources of pollution, our study supports the concept that were sufficient microenvironment measurements data available, it would be perfectly feasible to model personal exposures with some degree of reliability.

Kromhout and van Tongeren advocate the use of personal exposure measurements in environmental epidemiological studies. In doing so, they fail to acknowledge the magnitude of such studies. For example, in the large North American cohort studies, 8,111 subjects were recruited in the Harvard Six Cities Study and over one million in the American Cancer Society Study. Were it possible to reconstruct the exposure environments of those individuals, even in a rather general way from time activity diaries, a considerable refinement would have been achieved. Even in panel studies, which typically recruit a far smaller number of individuals, the subjects are doing so, they fail to acknowledge the magnitude of such studies. For example, in the large North American cohort studies, 8,111 subjects were recruited in the Harvard Six Cities Study and over one million in the American Cancer Society Study. Were it possible to reconstruct the exposure environments of those individuals, even in a rather general way from time activity diaries, a considerable refinement would have been achieved. Even in panel studies, which typically recruit a far smaller number of individuals, the subjects are doing so, they fail to acknowledge the magnitude of such studies. For example, in the large North American cohort studies, 8,111 subjects were recruited in the Harvard Six Cities Study and over one million in the American Cancer Society Study. Were it possible to reconstruct the exposure environments of those individuals, even in a rather general way from time activity diaries, a considerable refinement would have been achieved. Even in panel studies, which typically recruit a far smaller number of individuals, the subjects are doing so, they fail to acknowledge the magnitude of such studies. For example, in the large North American cohort studies, 8,111 subjects were recruited in the Harvard Six Cities Study and over one million in the American Cancer Society Study. Were it possible to reconstruct the exposure environments of those individuals, even in a rather general way from time activity diaries, a considerable refinement would have been achieved. Even in panel studies, which typically recruit a far smaller number of individuals, the subjects are doing so, they fail to acknowledge the magnitude of such studies. For example, in the large North American cohort studies, 8,111 subjects were recruited in the Harvard Six Cities Study and over one million in the American Cancer Society Study. Were it possible to reconstruct the exposure environments of those individuals, even in a rather general way from time activity diaries, a considerable refinement would have been achieved. Even in panel studies, which typically recruit a far smaller number of individuals, the subjects are...
Mortality results for polyurethane manufacture understated

Sorahan and Nichols, writing in this journal, incorrectly understated the strength of evidence for work related increased mortality among their cohort of production workers in the UK flexible polyurethane foam industry. Their study actually found “some” evidence for a work related increase in all-cause mortality, respiratory disease mortality, and lung cancer mortality in this exposure circumstance, especially taking into account the healthy worker effect.’ We are concerned to correct this error, because the United Automobile, Aerospace and Agricultural Implement Workers of America (UAW) represents substantial numbers of workers exposed to this process, and the UK data provide the first evidence of a mortality hazard in this industry, in contrast to two previous, perhaps weaker studies. 1,2

The authors observed an all-cause standardised mortality ratio (SMR) among men of 107 (101 to 113), and a respiratory disease SMR of 120 (101 to 141). Increased mortality of similar magnitude from these causes was observed among the smaller number of women, and the SMRs for both genders combined were significantly increased. Raised SMRs for all-cause and respiratory disease mortality are hardly ever seen in occupational cohorts except for foundry and asbestos workers. Typically, the SMR for all-cause mortality is about 80 and the SMR for most cancer causes about 90 in the absence of exposure to a carcinogen at the site. 3 We have observed SMRs for all-cause mortality as low as 60 in UAW vehicle assembly and stamping cohorts. 4 We are not aware that these authors noted a deficit for all-causes in the abstract of their previous study of this cohort, but make no mention of the excess in the present paper. 5 For lung cancer, the authors noted a significant SMR of 181 (126 to 251) for lung cancer among women. They discount this partly because the SMR of 107 (90 to 227) among men was only slightly increased compared to the general population, without also noting that the combined SMR was 117 (101 to 136) and statistically significantly increased. The authors also fail to mention that the SMRs for pancreatic cancer were increased to a similar degree in both genders, and the combined SMR was 147 (102 to 212) and significantly increased. We believe that consistency in direction of effect is more important than statistical significance, especially in view of the healthy worker effect bias against seeing an effect if it were there.

These findings apply to an exposure circumstance with several suspect agents. The isocyanates are a family of strongly associated with non-malignant respiratory disease. Therefore, the increase in mortality from this cause is of distinct interest. In addition, pancreatic cancer has been noted in gavage studies of toluene diisocyanate. 4 In our experience, the most substantial exposure with carcinogenic risk in foam moulding is methylene chloride, although brominated and chlorinated alcohol flame retardants, and formaldehyde are usually present in foam moulding operations. Catalyst amines may also be absorbed through the skin in physiologically significant amounts. 6 These multiple exposures, often in different parts of the process, including off-gassing from stored foam, undermind the ability to see an effect of isocyanates alone.

We now turn to the exposure response portion of the study. Health related termination of exposure has previously been noted as an obstacle to finding an exposure response effect based on duration. 7 Those with highest exposure to isocyanates would be expected to be sensitised and migrate into lower exposure jobs; in any event, there were only 19 of 1652 deceased workers with more than five years in higher exposed jobs, no lung cancer victims, and only two respiratory disease victims. In our view, the absence of an exposure response relation in a cohort with such a small higher exposed group detracts little from our concern for occupational cancer of an observed excess.

More damaging to the evidence of occupational causation is the absence of a monotonic increasing trend with latency from first exposure: The general trend of increased risk in exposure strata greater than 10 years latency, clearly significant for all-cause mortality, is not seen in those with greater than 30 years latency. However, we note that the all-cause and respiratory disease SMRs are at unity or slightly above for this long latency strata, itself a highly unusual observation. The confidence intervals overlap strata, so while there is not a significant increase, there is no inverse latency response relation. In addition, much the largest portion of this long latency group must have come from two of the 11 plants (factories 3 and 4 in table 1) where exposures may have been different to those in the other nine facilities.

In summary, this study has found a highly unusual and statistically significant increase in all cause mortality and respiratory disease mortality in the cohort as a whole, especially in both men and women. For women alone, and men and women combined, there was significantly increased mortality for lung cancer, and for both genders combined, pancreatic cancer. This is certainly “some evidence” for work related mortality, respiratory disease mortality, and cancer mortality in the exposure circumstance of polyurethane foam production. The nature of the cohort gave little prospect for observing an exposure response relation, if it were there.

We also note that, unlike all the other papers in this edition of the journal, these authors have neglected to acknowledge their funding source.

References

8 National Toxicology Program. TR-251: toxicology and carcinogenesis studies of commercial grade-2,4 (80%)- and 2,6 (20%)-toluene diisocyanate (CAS No. 26471-62-5) in F344/N rats and B6C3F1 mice (gavage studies) 1986.
Authors’ reply

Constructive informed criticism of occupational epidemiological studies from Trades Union representatives is to be welcomed, even though on this occasion the criticisms are levelled at ourselves. However, we are not convinced that it is fair to say that we made no mention of the excess SMR for all causes mortality when the second sentence of the results section stated: “In males, there were significantly increased SMRs for all causes (Ohs 1298, SMR 107, p < 0.05) ...”. It is fair to say that we did not attach very much importance to this slightly increased SMR. We remain convinced that it is very unlikely that occupational exposures in any industry could have a discernible influence on all-cause mortality without obvious major effects on cause specific mortality being apparent. The finding of an all-cause SMR of 105 in the subcohort of male workers with any period of toluene diisocyanate (TDI) exposed employment is consistent with such a conviction. It should also be remembered that, in the UK at least, socioeconomic status has a major influence on all-cause mortality for reasons other than occupational exposure.

We also remain convinced that the excess SMR for female lung cancer is not due to isocyanate exposure because none of the female lung cancer cases had any period of isocyanate exposed employment. Dr Mirer suggests that occupational exposures other than TDI should be considered, and the usefulness and completeness of the study could be improved if it were possible to carry out a retrospective quantitative exposure assessment for all exposures of interest and for each of the 4612 unique factory/department/job entries in the study job dictionary. While we mentioned in our original discussion section that all the available human studies are “low” exposure studies, the particular limitation that only a little more than 1% of all deaths in our study occurred in subjects with five years or more of “higher” isocyanate exposure could also have usefully been mentioned.

We have never wished to suggest that the current UK update is the last word on the topic of possible long term health risks associated with the manufacture of flexible polyurethane foam. It is likely that an update of the epidemiological study of Swedish flexible polyurethane foam industry workers will be published in 2003, and it will be important to compare the Swedish findings with the recent UK findings. Finally, we apologise to our sponsors (International Isocyanates Institute, Inc.) for not mentioning them; we had not noticed that the provision of such information had become commonplace in Occupational and Environmental Medicine.

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Other sections deal with decontamination and protection: suits and respirators are described. Managing an incident is described and the authors sensibly build their recommendations on how chemical incidents that do not involve chemical warfare agents should be managed. Detection systems are described. Detector paper, as well as complex instruments, is described and this is helpful: detector paper may have an important role to play in rapid checking of chemicals spilled on the ground. A nice device for detecting nerve agents is described: the Detection Ticket 90 system. The book concludes with a short glossary and a carefully chosen bibliography.

In conclusion, this is the best short and well illustrated account of chemical weapons I have seen. The authors have sensibly stayed away from too much detail and controversy, but anybody following their advice will handle an incident involving chemical weapons in a safe and competent way. This is an excellent book that should be in every toxicologist’s library.

R L Maynard

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