CORRESPONDENCE

Surveillance systems and the role of a preventive medical team in chemical incidents

Editor—In their review of the role of a medical team in the emergency management of chemical incidents, Baxter et al have highlighted the health issues and current deficiencies in disaster planning that present health professionals from effective participation. Plans, the needs for them, and for epidemiological surveillance are well described. There are also statutory requirements for district health authorities to have in place plans for reacting to such incidents. They must include the designation of an officer responsible for collecting emergency data and long term follow up as appropriate.

Although active surveillance systems are in place in Scotland and Wales, in England at the local level there is as yet no allocated funding for active surveillance. The Department of Health has asked (personal communication), when toxicological problems occur in the general environment:

How often, if at all, is a real risk of harm at issue?

How often, if at all, are local services unable to cope?

How frequently has there been difficulty in obtaining necessary information and advice?

What criteria should be used to research these questions and who should do the work?

Hospital accident and emergency department records are one useful source of routinely collected data to help answer these questions and identify future preventive measures. As an example, and although the hitherto unpublished data were collected 16 years ago, I hope the following audit of an acute chemical incident will help to support the recommendation of Baxter et al for active surveillance systems that allow rapid collation of information on health effects in the exposed population.

On June 11 1980, a lorry carrying 35-gallon drums of sodium hydroxide, shed its load while navigating an exit of the M5 motorway near Bristol. Several of the drums burst, spreading the powder. Shortly afterwards it began to rain. The resultant chemical reaction released a cloud of sulphur dioxide that fortunately dispersed upwards. Sulphur dioxide is an irritant gas due to the formation of sulphurous and sulphuric acids on contact with moist mucosa. Although the public were not affected, under a newspaper heading, "Families alerted in poison peril" the Bristol Evening Post correctly reported that "police and emergency services were poised to evacuate hundreds of families from Avonmouth today following a chemical lorry disaster" and that some "emergency workers were overcome by a gas cloud". Accident and emergency department staff were used to explore the health effects (table 1).

Of the 19 firemen seen at hospital, 17 were taken there by ambulance and two referred themselves. In contrast, 13 of the 14 police officers referred themselves. However, only six of 19 (32%) of the firemen had any respiratory symptoms recorded on arrival whereas 11/14 (79%) police officers did. (χ² 5.360, p<0.05). The two ambulance staff with respiratory symptoms were not taken to hospital by their colleagues. All of those with symptoms were given oxygen by facemask, and bronchodilators or steroids by both hospital, and two police officers were admitted for inpatient treatment of tracheobronchitis or pneumonitis. All were discharged within four days.

The presentation patterns suggest that emergency workers taken to hospital by ambulance from this incident were less likely to have respiratory symptoms than personnel who referred themselves. Accident and emergency department staff were puzzled. Subsequent inquiry showed that local firemen exposed to chemical incidents and for which personal breathing apparatus was worn, were entitled by their trades union to medical examination afterwards. At the time, there were no policies for other emergency personnel involved in such incidents. The findings were used by emergency planning officers to ensure that after any such future exposures fire brigade staff would be first seen, wherever possible, at or near the incident by medical staff experienced in toxicological problems instead of being led to medical examination afterwards. The aim being to keep any unprotected emergency personnel from being exposed to high concentrations of chemicals.

The emergency plans for dealing with chemical incidents were revised to incorporate these points. Elsewhere too, attention has been drawn to the need in such situations for protocols to manage many casualties exposed to chemicals, particularly when they arrive at hospital without warning. 1

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Factors affecting recognition of cancer risks of nuclear workers

Editor—We would like to comment on a recent paper by Kneale and Stewart on factors affecting recognition of cancer risks of nuclear workers. 1

The statistical techniques used in this paper differ substantially from those used in other analyses of various groups of radiation workers. 2 In their analysis, Kneale and Stewart fit several models by maximum likelihood techniques to various cohorts of United States Department of Energy radiation workers, using those at Hanford. They claim to find evidence of a significantly excess of cancer risks, with a very low doubling dose (8.2 mSv) and a highly curvilinear (downward bending) dose-response. As the authors point out, these findings are somewhat at variance with the analyses of cancer risk in the Japanese atomic bomb survivors and in most other occupationally exposed groups of workers, although as confidene intervals are not quoted for their fitted model parameters it is difficult to assess the extent of the statistical incompatibility. However, there are at least two serious methodological problems in their analysis. First is that they do not fit model parameters in their model take integer values and therefore the statistics used to estimate significance probably do not have the desired asymptotic distribution. Therefore the tests for significance may be incorrect. The second and more serious problem is that the authors' calculations is that when they allow the parameters to vary, and in particular the minimum-age-at-exposure parameter, the number of degrees of freedom in the likelihood, as determined by the numbers of records with non-zero effective doses, reduces considerably. By doing this, the analysis only takes account of a small subset of the data. The authors have no account of this in their analyses and it will much reduce the nominal significance of the fits. For example, the favoured fourth model fitted to fatal cancers incorporates an age-at-exposure parameter which effectively discards all doses received before the age of 58.

In summary, this paper suffers from several quite serious methodological weaknesses, which are sufficient to invalidate the results of the authors' analysis. A more reliable analysis of at least the Hanford workforce data has been published, which finds convincing indications of a trend with dose
only for multiple myeloma. Moreover, as the authors acknowledge, analyses of various cohorts of radiation workers (which overlap in part with the cohort studied by Kneale and Stewart) in the United States and elsewhere\(^4\) find no evidence of statistical incompatibility between the cancer risks associated with such occupational exposure and those found in the survivors of the Japanese atomic bombings. Consequently, we think that little weight should be attached to the results of the paper of Kneale and Stewart.

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**Author’s reply—**Most of the difficulties which Little and Sharp have with our paper would seem to stem from a failure to understand that our statistical methods were simply practical applications of the general theory of nested likelihood ratio tests. This theory is a very general method of identifying statistical tests that have a wide range of optimum properties provided one has a sufficiently general statistical model of a parametric kind that describes how the data might have been generated, and provided this model satisfies certain easily checkable mathematical restrictions. Why is this so can be discovered in any general text book of mathematical statistics, either the one referenced in our paper—namely, Kendall and Stuart—or the one preferred by Little and Sharp—namely, Cox and Hinkley.\(^4\)

For studying the relation between radiation and carcinogenesis relevant parametric models can be found in Breslow and Day.\(^1\) For example, how intense a disease about a carcinogenic effect can be justified by making use of a parametric model is shown in Vol I page 248, and two formulae for parametric models for cohort studies are given in Vol II page 261. The choice of which formula is used depends upon whether the cohort study is progressing in continuous time (first formula) or in discrete units of time (second formula). We naturally used the second formula as the recorded doses were only available as annual totals.

The mathematical techniques and limited computer facilities available in 1980 did not allow direct (case-control) methods to be used in cohort studies. Therefore, Vol II of Breslow and Day deals mainly with studies where various approximations (such as Poisson regression when person-years at risk can be calculated) can be used. The choice of which formula is used depends upon whether the cohort study is progressing in continuous time (first formula) or in discrete units of time (second formula). We naturally used the second formula as the recorded doses were only available as annual totals.

But although we made use of the formulae on page 186, our numerical methods (for completing the necessary maximum likelihood calculations) differed from the ones recommended by Breslow and Day over 15 years ago.

If new variations of standard methods were not allowed there would be little progress in science. Nevertheless, as we agree with Cardis and Sharp, new methods should have its results checked against standard methods, we included in our paper two tables expressably for this purpose. Thus table 7, which includes the basic nested likelihood ratio tests and parametric estimates by maximum likelihood, shows that the most important of the three extra variables influencing relations between radiation and carcinogenesis was the age.

This is important because virtually all of our models which included an optimised minimum value for critical exposure age were significant at the 5% or 1% level, whereas the methods with it, it is optimised, x was to this variable had much lower levels of significance. Then comes table 9, which shows the results of testing the null hypothesis of no radiation effect by a method exactly equivalent to the others only available recommended by Little and Sharp, but uses the windowed doses prescribed by optimised values of minimum critical exposure age and minimum critical latency. Consequently the significance levels (which vary between 1% and 0.1%) and which were calculated by the standard formulae, had greater strength than the ones in table 7, as they did not account of the window restrictions.

It should also be noted that although in two papers which used the standard methods approved by Little and Sharp,\(^1\) lip service was paid to the idea that latency was important, it was always in terms of upper confidence limits—this was a crude method of estimating optimal latency compared with the one we used.

Having explained the theory behind our methodology we turn now to the more detailed criticisms of the Little and Sharp. It is true that in our analysis the critical minimum for exposure age and latency were confined to values of integral years as, with dose estimates available for whole years, we were forced to use the discrete time formula of Breslow and Day. However, with maximum likelihood as the method of risk estimation, the integral year estimate would normally have a slightly lower maximum than any fractional year estimates for continuous doses. Therefore, as our calculated level of significance was based on integral years, it provided a less rigid test of the null hypothesis than one based on daily or weekly doses, and thus left us “playing safe.”

Little and Sharp’s criticism of the degrees of freedom estimate for a window which allows a zero estimate of effective dose, shows some confusion in their minds about the standard tabular \(\chi^2\) (which has many degrees of freedom for testing independence in tabulated data) and the \(\chi^2\) corresponding to generalised deviance in the nested likelihood ratio theory. The degrees of freedom for this \(\chi^2\) depend not on the number of cells in some mythical table containing all the relevant data, but on the number of variation parameters being estimated. The situation is exactly analogous to the formulae used by Gilbert et al\(^3\) and Cardis et al\(^4\) for calculating the significance of their results based on logged dose estimate. When some workers with recorded doses were left with an effective dose of zero. But this no more altered the approximate normal distribution of their test statistic, than it would have altered the approximate \(\chi^2\) distribution arising from the nested likelihood.

Finally, for anyone interested in this arena of statistical theory, it may be of interest to know that the theory of nested likelihood ratio tests was discovered by combining the standard (tabular) \(\chi^2\) with the standard analysis of variance ratio of \(\chi^2\), and that was how \(\chi^2\) came to be relevant. All this can be investigated either in Kendall and Stuart or in Cox and Hinkley.

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**Parental exposure to radiation and childhood cancer**

This study\(^1\) was referred to by Roman et al (p 78) as a “study linking dosimetry information contained within the National Registry of Radiation Workers (NRWW) with records of childhood cancer held in the National Registry of Childihood Tumours (NRCT)”. This statement needs amplification. From the outset the Office of Survey of Childhood Cancers (OSSC) has been a partner in this project. Identifying particulars for some 40000 parents have been abstracted from the interview records of the ONS; these data are now used to fill in the files of the National Registry of Childhood Tumours.

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**Offspring sex ratio as an indicator of reproductive hazards**

**Editor—**In a recent letter the sex ratio was discussed as an indicator of occupational exposures.\(^2\)

The sex ratio at birth is a prevalence measure reflecting the sex programming at the time of conception and the survival until birth. The sex ratio has been suggested to reflect the hormone concentration at the time of conception.\(^3\) According to this hypothesis high levels of oestrogens changes the ratio towards more girls. External exposures may have this effect as