Tuberculosis among homeless people at a temporary shelter in London

SIR—Each year the charity Crisis sets up temporary Christmas shelters, not for "the homeless" but for homeless people. Kumar et al use the former term on 23 occasions in their report of a chest x-ray screening programme.1

Issues of concern were the high prevalence of tuberculosis and suboptimal management of known and presumed cases due to patient "obligation". These problems have been previously documented in a very similar group of homeless people in London during the mid 1980s.2 In the earlier survey, there was a notable association between tuberculosis and alcohol (ab)use,3 which may have an impact on treatment compliance. Data from the Crisis Open Christmas4 could perhaps be used to ascertain whether there was an association between "loss to follow up" and accommodation status of "no fixed abode" or self reported "regular alcohol consumption". The documented health care needs of single homeless, or homeless and roofless people are multiple.1 The prevalence of mental illness noted by Kumar et al,5 low by comparison with other survey data,6 may have been significantly underestimated by selection or reporting bias.

A comparably wide range of accessible and acceptable health care provision therefore seems appropriate and is, indeed, a feature of some dedicated services for homeless people.7 Recent recommendations to improve current provision, based on specialist outreach respiratory/tuberculosis services, will have limited capacity to address important and potentially confounding morbidity. The most effective means of delivering comprehensive health care to homeless people, including case finding and management of those with tuberculosis and their contacts, needs formal evaluation. However, the views of professionals in primary and community health services and in the voluntary sector, who best understand the motivating factors and influential social networks of homeless people, should not be overlooked. Indeed, the suggestion of patient held records was recently raised in The Big Issue.8

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10 The Big Issue 1996;170:25.

Figure 1. Age specific mortality rates in relation to selected cohorts in Taiwan. The 1941 curve is close to the 1951 curve. It is excluded to avoid visual confusion. The legend indicates the mid-points of the five year cohorts.

Reply

Dr Cheung points out correctly that the age–cohort model we used to estimate cohort life expectancy in 'Taiwan' has an implicit assumption of constant age pattern across cohorts. Dr Cheung also explains why such an assumption is unlikely to be met, especially in a population under epidemiologic transition. I fully agree with his views.

In the context of age-period-cohort analysis, the problem of non-parallelism in age patterns has been explicitly addressed.9 Unfortunately, the proposed methodologies are a bit too complex for the present purpose. Instead of resorting to models with interaction terms, I feel that a practical solution to this problem may be to perform a "separate analysis". For example, we can break down the data by age into, say, "childhood", "adolescence", and "elderly" group, each modelled separately, and then the results combined. By this method, different segments of the population assume different cohort trends. Alternatively, we can break down the all-cause data into their component causes, modelled separately and then combined—an approach suggested in our paper.1 Clearly, such an approach pays due respect to the different roles of communicable and the non-communicable diseases during the epidemiologic transition. However, the benefits we gain from these more "sophisticated" approaches must be weighed against what we can infer from "crude" but "simple" age-cohort modelling as shown in our paper.1

Finally, Dr Cheung has stated that the assumption of parallelism is not met in our data after the 1961 cohort. I wish to point out that we also refute the results after 1961, though for a different reason.

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