LETTERS

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Job-loss and family morbidity

Sir,
Beale and Nethercott (November *Journal*, pp. 510-514) should be congratulated for taking the opportunity of using a natural experiment to investigate the effects of job-loss on health. From the results of their study they conclude that job-loss has a negative effect on health, based on an increase in consultation rates, episodes of illness and hospital referrals in the families of recently redundant factory workers. However, we remain unconvinced that these conclusions can be drawn from their study for the following reasons:

1. Their method of choosing the study group may have biased the survey in favour of their conclusion by excluding fitter and younger employees and their families. Of the original work force of 800, only 129 were selected for investigation, although all became redundant during the study period. Unfortunately, the authors do not give information regarding the 84% of workers who were excluded from the study, but it seems likely that their selection criteria (by excluding migrants) produced an unrepresentative sample of redundant workers for investigation.

2. The method of selecting the control group is not made clear. Were the control and subject groups comparable? From information given of births and deaths within the study period, it appears that the study group may be older than the control group. Furthermore, there was a significant difference in the number of episodes of illness of female Harris employees and their control counterparts at the beginning of the study.

3. In order for a meaningful comparison to be made between the study group and control group, it is important to know the age and sex distribution of the two groups. Unfortunately, the authors have not provided this information. Data from the General Household Survey has demonstrated the association between consulting rates and age and sex. For example, in the age group 16–44 years, the consulting rate of women is more than double that of men but in the 45–64 years age group the consulting rate of men has increased twofold to negate the sex differential.[1] These age/sex trends may be sufficient to explain the changes demonstrated in the subject and control groups of Beale and Nethercott.

4. A difficulty in longitudinal studies based on medical records is that the observer may be influenced by his awareness of the subject being in the control or experimental group, resulting in observer bias. In the paper by Beale and Nethercott there is insufficient information to reassure the reader that this possible bias has been minimized. The significant decrease in episodes of reported illness by the control employees in the latter stages of the study period may be due to observer bias causing differential recording rates between the groups.

Although the authors have shown an increase in consulting rates and episodes of illness over time in a group of families in which one member was made redundant, the conclusions they draw are not valid as they have not controlled for the changes in consulting patterns that occur with age and sex, and have not demonstrated that the study and control groups are comparable. This study highlights the difficulties of demonstrating a causal relationship when so many confounding variables are present and illustrates why the adverse effects of unemployment on health remain uncertain.

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Reference

1. Wells NEJ, Chew RBS. *Compendium of health statistics.* 5th edition. London: Office of Health Economics, 1984.

Sir,
We are sorry that Drs Littlejohns and Williams remain unconvinced that our conclusions were valid.

1. Their first point is answered by a more careful reading of the text, in particular the Introduction to the paper. In fact 302 workers were made redundant at the time of final closure. About a quarter lived in other neighbouring towns and a fifth of the remainder were not registered with our practice. Of the residue, 133 workers fulfilled the criteria given in the Method — criteria adopted to select a population for its stability. The latter characteristic is essential for a longitudinal study linking health record and employment status over many years. In any event we do not feel that our study population (mean length of service at the factory 14.7 years) is unrepresentative of the thousands of workers currently losing their jobs every week, many of whom will have spent many years in stable employment in previously buoyant industries.

2. The procedure for selecting the controls is given in the Method section.

3. Restrictions on space prevented the publication of more population characteristics of the study and control cohorts. Their mean ages were in fact (as at 1 July 1982): Harris employees 42.6 years; control employees 41.5; Harris male employees 44.0; Harris female employees 40.7; control male employees 40.2; and control female employees 46.0. The significant difference in reported episodes of illness between the Harris female employees and their control counterparts was in fact the only significant difference in all the possible comparisons in years 1–4. This difference may indeed represent the influence of age since the control women are somewhat older. However, the