EDITORIAL

This is an extraordinarily and inordinately delayed issue. The York members of the Troika are entirely to blame and are delighted that "volunteers" have been found for RSN43.

It is, of course, a permanent astonishment that the Newsletter comes out. Someone will have to privatise it soon! Yet the contents of this issue appear to bear witness to a lively group. It is not unusual for the ever-prolific health group to produce a wadge, but the Nicaragua group is pressing them hard.

And that's not all! The issue is spiced with a hard hitting riposte from Richard Wilkinson to Carr-Hill's note in RSN41, and a juicy reflection on the complication of the poll tax both for the poor and for us poor statisticians.

Finally, it is surely the privilege of at least one of the editors to acknowledge that one of the speakers at the meeting on 16th November on nuclear 'defence' was his brother (no relation). The delay in publication is unrelated!!

Radical Statistics Group

Fourteenth Annual Conference
ADVANCE WARNING

This will be held at the other end of England, viz. Southampton, hosted by Cecilio Mar-Molinero and Ian Diamond. As per tradition, this will be held on the last Saturday of February, i.e. 25th February. If you want to go, have persuaded anyone else to go, need a bed or a creche, pontificate or otherwise remind us of your existence. Please contact either Cecilio or Ian at the University of Southampton, Southampton, SO9 5NH, tel. 0703 559122.

A reply to Roy Carr-Hill on Class and Health

from Richard G. Wilkinson,

Centre for Medical Research, University of Sussex.

Perhaps I could reply briefly to Roy Carr-Hill's comments (RSN 41, 1988) on Class and Health, which I edited.

First, the argument about the contribution of selective social mobility to class differences in health - are people in lower classes because they are unhealthy, or are they unhealthy because they are in lower classes? I reviewed evidence on selection in later life from the Longitudiunal Study, on selection between early childhood and age 26 from the 1946 cohort, and on the effects of selection of mothers at marriage on perinatal mortality rates from the Aberdeen data bank. Since Class and Health was published, new data on social mobility and health from the 1958 cohort has also been published. With the exception of the Aberdeen data, no one has claimed that data from these sources shows anything but a very small selective contribution to socioeconomic differences in health. Illsley's interpretation of the Aberdeen data stands in marked contrast to the conclusions of all the other work in this field (Illsley 1985). But not only is his data ambiguous, but despite its repeated use to illustrate selection, it was the only source from which there were, prior to my attempt, no published estimates of the contribution of selection to the perinatal mortality differences it showed. Instead the reader was left to make the unlikely assumption that perhaps all the differences in perinatal mortality were to be attributed to selection. Given that no one doubts that social mobility is selective with respect to health, the only way of advancing the argument is to quantify its scale. My

estimate of ten percent was based on the fact that the only part of the Aberdeen data which gives an entirely unambiguous indication that selection exists at all, is the data on maternal height. Unlike perinatal mortality, which reflects numerous influences from conception through pregnancy to labour, maternal height is unambiguously determined before marriage. I merely used the scale of social mobility demonstrated in the height data as a central estimate of the likely scale of the selective contribution to perinatal mortality differences. The true figure may of course be more or less, but at the moment it remains the best estimate we have. I suspect that it is a good estimate simply because maternal height is a good indicator of the longer term influences of maternal health on perinatal outcome.

These small selective effects at different stages in life may of course all add up, but even if they do they still cannot explain the recorded widening of class differences in mortality rates which was afterall, one of the purposes of the selection hypothesis. Not only has there been little change in rates of social mobility in most of the post war period, but even if one ignored the fact that much of the data goes back to the 1930s and assumed that social mobility had only become selective in the last few decades or so, its contribution would still be far too small to account for the post war widening of differentials demonstrated by Pamuk and others.

Carr-Hill seems to have forgotten that the problem of the changing proportion of the population in different classes was taken into account both by the Gini coefficients used by Preston et al (1981) and also by the weighted least squares methods used by Pamuk (1985). Indeed that was the primary reason for using these methods. Contrary to what Carr-Hill asserts, these measures suggest that the widening

mortality differentials were not an artifact of the changing size of classes.

Carr-Hill dismisses my work on income and mortality on the grounds that the results are based on too few cases but, despite his pleas for statistical rigour, he apparently pays no attention to tests of statistical significance. What other way is there of telling how much reliance we can place on measures of association based on any stated number of cases? In fact of course if results are significant on small numbers it means that the relationships must be very much stronger and more important than results reaching a similar level of significance on larger numbers. However, I used data from only eleven OECD countries to relate life expectancy to income distribution simply because Carr-Hill had previously advised me that these were the only reliable data. The correlation was significant at the level p<0.001. Similar observations have of course been made on larger data sets by Rodgers (1979) and more recently by Le Grand (1987). Both show that the relationship is independent of the level of GNP per capita. My attempt to test for a relationship between the real value of state old age pensions and the death rates of pensioners was confined just to the 16 years prior to 1982 in order to tie up results with information from the Family Expenditure Surveys. First-order partial correlation coefficients controlling for GNP p.c. and results using another form of control were however all significant. If it had been possible to use a longer series there would presumably have been questions about whether the results were applicable to the more recent years alone.

On my analysis of relationship between changes in the relative positions of occupations in the earnings and death rates leagues between 1951 and 1971, the four occupational classifications used in my sources allowed me to match up only 22 occupations covering some

2.5 million people. The correlations reached significance in two of the four age-groups on the unweighted data. When the occupations were weighted by the proportion of the population in each, the coefficients all reached the level required for significance. I did not mark the weighted results as significant because of doubts as to the interpretation of significance tests on weighted data. I am however repeating the analysis for the period 1971-81 when it is possible to match more like 80 occupations.

What is important about these results, and indeed why I chose to look at income-mortality relationships using these methods in preference to using data from the large cohort studies, is because of their special implications for policy. In terms of policy we need to know two things: the first is whether health is affected by exogenous changes in income, and the second, (which I discussed in chapter 6 of Class and Health), is whether, if there is a causal relationship between income and health, it has a curvilinear shape which would suggest that the health of the population as a whole would benefit from income redistribution. In order to inform policy on levels of benefits, pensions or whatever it is not enough to know simply that richer people are healthier or even that people who get richer also get healthier. Changes in income which people bring about themselves, that is to say changes which are not exogenously determined, can always be seen merely as a reflection of other personal characteristics which differ between the rich and the poor. However carefully controlled, evidence from cohort studies on the causal relationship between income and health is always vulnerable. Given the difficulty of mounting radomised control trials in this area, my work on pensions and on occupational mortality differentials was simply an attempt to look at the impact on health of exogneous income changes - i.e. changes for

which the income recipients were not individually responsible.

My own and other people's more substantial work on the international cross-sectional relationship between measures of health and measures of income distribution, along with Jay Winter's (1986) work on the timing of increases in life-expectancy in Britain this century, is important as a confirmation that the relationship between income and health is indeed curvilinear. It suggests that the health of the population as a whole would benefit very substantially from a more egalitarian distribution of income.

Illsley R. (1986) Occupational Class, Selection and the Production of Inequalities in Health. Quarterly Journal of Social Affairs, 2, 2, 151-165.

Le Grand J. (1987) An International Comparison of Inequalities in Health. Welfare State Programme Discussion Paper 16.

Pamuk E.R. (1985) Social Class Inequality in Mortality from 1921-72 in England and Wales. Population Studies, 3°, 17-31.

Preston S.H., Haines M.R. and Pamuk E.R. (1981) Effects of Industrialization and Urbanization on Mortality in Developed Countries. In Solicited Papers, Vol.2, IUSSP, 19th International Population Conference, Manila 1981, Liege.

Rodgers G.B., (1979) Income and Inequality as Determinants of Mortality: an International Cross-section Analysis. Population Studies, 33, 343-51.

Winter J.M., (1986) Public Health and the Extension of Life Expectancy in England and Wales 1901-60. In The Political Economy of Health and Welfare edited by M. Keynes, Eugenics Society Symposium, London: Macmillan.

32