Letters to the Editor

The place of population change in explaining geographical inequalities in health in New Zealand
From JAMIE PEARCE1* and DANNY DORLING2

We appreciate Harper’s comments on the role of population change in explaining rising geographical inequalities in health in New Zealand.1 As we note in our original paper, we agree that selective migration patterns may well be an important explanation as to why regional health status in New Zealand has become more geographically polarized during the 1980s and 1990s.2 Selective patterns of migration between the regions of New Zealand and the high levels of immigration into the country, and emigration from it, are likely to lead to a high level of population sorting between areas and to have strengthened the widening life expectancy gap during the 1980s and 1990s. Given that New Zealand experiences almost the highest rate of combined immigration and emigration (population turnover) in the world,3 if selective migration were to have an explanatory role anywhere it would be in helping to understand changing health patterns within this country.

Although we agree that selective migration is likely to be an important factor in explaining our results, there are five areas of interpretation where we disagree with Harper. First, Harper contends that growing geographical inequalities in health observed using the Slope Index of Inequality (SII) may be due to differential levels of population growth between regions of the country (i.e. if areas with the highest or lowest life expectancies grow at a faster rate then the gap will increase even if the life expectancy in those areas remains the same). Harper gives an example of where he thinks the problem lies:

“Given the near-linear relationship between socioeconomic rank and life expectancy among health districts, even if regional life expectancies had stayed exactly the same, but over this time there was more rapid population growth among areas of higher and lower life expectancy, the SII would register an increase.” (Harper, 2006, p. 604)

It is not hard to see how Harper came to assume this—it is an easy error to make and perhaps we could have explained the SII more fully—but it is important that researchers recognize that the results are not an artefact of using the SII as Harper suggests. Rather, an increase in the index means that inequality amongst people measured between areas has risen. Suppose you were measuring educational attainment and over time a greater number of people failed to gain any qualifications and more gained degrees—inequality would have increased between people in terms of educational attainment. It is not that the population sizes of these groups had simply altered. Places are not entities of themselves—they are collections of people.4

The SII is the slope of the regression line from the hypothetically poorest individual to the hypothetically richest individual derived from the relative poverty ranks of life expectancy for each geographic area, weighted for population size. The SII takes into account all measures for all areas and not, say, simply the worst-off and best-off 10th or 5th of areas. The index is most effective as a summary measure when the two measures are linearly related, as is the case with the data we analysed. The index has a further advantage that it is, by definition, unaffected by general increases or decreases in life expectancy over time (in this case the constant—that is the intercept—changes but not the slope). The SII is the slope coefficient in a simple regression analysis of life expectancy in years against a ranking of each area, where ranking is expressed as the cumulative proportions of the total population. An example of use of the SII in comparisons of life expectancy over time between continents shows that in 1950–55 the hypothetical worst-off person in Africa had a life expectancy 41.9 years shorter than the best-off person in North America, an SII of 41.9. The SII fell to 23.7 years by 1990–95 but rose to 25.4 years 10 years later. Had the coefficient not been calculated allowing for the increasing population of Africa then the fall in SII would have appeared to have been less, and the recent rise would appear to be great but would have been exaggerated.4 However, although Harper’s suggestion that allowing for population size somehow artificially inflences the meaning of SII is erroneous, his observation that changing population matters is apposite in other ways as we outline below.

Second, Harper suggests that our analysis is limited by the non-availability of a measure of deprivation at the District Health Board (DHB) scale for the years prior to 2001. Harper contends that if the relationship between deprivation and life expectancy was weaker at the start of the study period then the increase in the SII during the 1980s and 1990s could have been overestimated. However, while there may have been absolute changes in the degree of deprivation experienced by people in regions in New Zealand during the 1980s and 1990s, it is highly unlikely that the relative ranking of DHBs will have changed significantly during the study period.5

Third, Harper proposes that he can discern a narrowing in the difference in life expectancy between the North and...
South Islands of New Zealand from inspecting Figure 3 of our original paper. Before North and South Islanders contact us to ask which island is doing better, we should point out that the narrowing he observed is between the life expectancy of men and women in New Zealand. He misread the key to Figure 3. There is no difference between mean life expectancy of the population of the two main islands of New Zealand—a broad geographical equality few other territories can emulate when their populations are divided so simply.

Fourth, Harper is cautious about our interpretation that growing economic inequality during the late 1980s and early 1990s in New Zealand is a driver of the corresponding widening geographical differential in health. He states that:

"...the longer term trends show that economic inequality declined sharply in New Zealand from the end of The Second War to the mid-1980s, which would be inconsistent with the rise in health inequality Pearce and Dorling show from 1981 to 1986. (Harper, 2006, p. 605)

However, our results show that geographical inequalities in health increased only very slightly between 1981 and 1986 (~0.3 years for males and females), and, hence, we are making no great claims for this time period. Rather, the rapid increase in health inequalities took place in the 10 year period after 1986, a period which coincided with a rapid rise in income inequality in New Zealand.

Finally, Harper is probably mistaken in attempting to reduce the observed rise in inequality in health into a single explanation. It is unlikely that the sharp rise in social and spatial inequalities in health in New Zealand is the product of just one explanation or process. Rather, as noted elsewhere, rising geographical inequalities in health are likely to be a manifestation of many complex and socially patterned factors, which operate at a range of scales across the lifecourse. Therefore, in New Zealand rising geographical inequalities in health probably reflect a range of multifaceted explanations of which growing income inequality and selective migration are two key intertwined drivers. We would argue that the processes of selective migration and economic inequality are inextricably linked—as economic inequality increases so the patterns of migration become more selective and over time there are some places to which, increasingly, only the most wealthy have the resources to be able to move to.

In conclusion, while we agree with Harper’s assertion that migration is likely to play an important role in explaining rising geographical inequalities in health in New Zealand, it is unlikely that population change by itself fully explains New Zealand’s widening spatial health divide. Further, we would argue that increasing levels of income inequality in New Zealand are also an important component of the explanation for rising health inequalities. While Harper is right to note that other New Zealand studies have found that income inequality and health are not related, it is perhaps not surprising that given New Zealand’s small population the evidence tends to be inconsistent. Because of the high proportion of New Zealand residents who were born overseas, the high levels of age-specific emigration and the high levels of internal migration within the country, New Zealand is an important setting to study the effects of selective migration upon health. However, studying cohort by cohort ‘multiple age- and cause-specific pathways’ is problematic but could well be worthwhile given the extent of inequalities within the two main islands.

References


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Obesity prevention: life course approach vs continuing environmental ‘detoxification’
From MIRANDA J PALLAN, KK CHENG and PEYMANÉ ADAB

Editor—In their editorial, Lawlor and Chaturvedi devote much attention to the potential for obesity prevention by brief intervention at critical periods of development, particularly the perinatal period, infancy, and puberty. This approach to obesity prevention appears interesting, but there are issues that need to be considered. First, although no definition of ‘brief intervention’ is given, the implication is that a short-lived, relatively simple intervention delivered at a critical