Abstract

Is mortality higher in countries that are more unequal? To answer this question, we use a new source of data on inequality: the share of the richest 10 percent of the population. Within countries, changes in top income shares have been shown to proxy changes in other inequality measures, such as the gini coefficient. Using data on top income shares from nine developed countries, Australia, Canada, France, Ireland, the Netherlands, New Zealand, Switzerland, the UK and the US, over the period 1905-2002, we investigate the relationship between inequality and mortality. We find no evidence that more inequality reduces life expectancy or increases infant mortality.

* Thanks to participants at the Australian National University Research School of Social Sciences work in progress seminar for valuable feedback on an earlier draft.
1. Introduction

Do more unequal countries have worse health outcomes? In recent years, medical researchers and health economists have sought to discern whether any significant relationship exists, and if so, what the causal channels might be. However, research on the relationship between inequality and health has been hampered by a paucity of reliable data on inequality. This has meant that both cross-regional and cross-national studies have tended to compare inequality and health measures at a single point in time. However, if places that are more unequal also differ on other dimensions, the cross-sectional relationship between inequality and health may be spurious.

We seek to shed light on this debate by using a new source of data on inequality – panel data on the share of personal income held by the richest 10 percent of adults in nine developed countries: Australia, Canada, France, Ireland, the Netherlands, New Zealand, Switzerland, the UK and the US. Moreover, the data is available over almost the entire twentieth century.\(^1\) This allows us to control for country-specific fixed effects, holding constant stable unobservable factors that might be correlated with both inequality and population health.

The existing literature on inequality and health is surveyed by Deaton (2003), who concludes that the theoretical stories suggesting a relationship between inequality and health are stronger than the empirical evidence. Four studies that use time series evidence in developed countries to analyze the inequality-health relationship are especially relevant to our analysis. Wilkinson (1989) and Sen (1999) focus on changes in life

\(^1\) Our earliest observation is 1905 (France), and our latest is 2002 (New Zealand and the US).
expectancy in the UK over the twentieth century, and argue that mortality rates fell most rapidly when the income gap between rich and poor narrowed. However, their measures of inequality are relatively inexact, and they do not account for technological innovations which might have affected life expectancy more in some periods than in others. Focusing on the last thirty years of the twentieth century, Wilkinson (1996) argues that the rise in inequality in the US and UK during the 1980s is the key reason why the rate of decline in infant mortality slowed in the period 1975-85. By contrast, Deaton and Paxson (2001) find no systematic time series relationship between health and inequality in the UK or US from the mid-1970s to the mid-1990s.

To presage our findings, we find no significant relationship between income inequality and population health, regardless of whether we measure health by infant mortality or life expectancy, use levels or differences, or include country-specific characteristics. This suggests that the relationship between inequality and health is either non-existent or too fragile to show up in a robustly estimated panel specification.

The remainder of the paper is structured as follows. Section 2 presents a simple model of the relationship between inequality and health. Section 3 outlines our inequality and health data. Section 4 presents our results, and the final section concludes.
2. A simple model of the relationship between inequality and health

There are two basic channels through which inequality might affect an individual’s health. These are commonly termed the “absolute income hypothesis”, and the “relative income hypothesis”. Under the absolute income hypothesis, it might be the case that health depends only on individual income, but the marginal health gains from an extra dollar of income diminish as income rises. Figure 1 shows a stylized version of such a relationship. A mean-preserving transfer from the richer individual (R) to the poorer individual (P) will raise the health of P by more than it will lower the health of R. Thus more equal societies will have better health holding average income constant. Across countries, the relationship between average income and average life expectancy does follow a pattern similar to Figure 1 (Preston 1975; Deaton 2003).

The relative income hypothesis posits that inequality has some “direct” impact on health, holding individual income constant. Several channels have been proposed.

(a) **Crime.** Inequality has been shown to increase violent crime (Fajnzylber, Lederman & Loayza 2002), which in turn lowers life expectancy.

(b) **Public spending on healthcare.** Alesina, Baqir and Easterly (1999) show that the average value of public goods to members of a community decreases as heterogeneity increases. Income heterogeneity (inequality) might therefore have a direct impact on both public health measures and public provision of individual medical care. Szreter (1988) shows that public sanitation reforms in the UK only occurred when political power was extended to the poor.
(c) **Social capital.** Inequality might also affect public health expenditures via trust. Several studies have found that people in more unequal places tend to be less trusting (Knack and Keefer 1997; Alesina & LaFerrara 2002; Leigh 2003), which may in turn affect the provision of public health care. Kawachi et al (1997) find a cross-sectional relationship between inequality and social capital in American states, and in turn between states’ social capital and mortality.

(d) **Relative deprivation.** If individuals measure their well-being by reference to others, then inequality might engender “[l]ow control, insecurity, and loss of self esteem” (Wilkinson 1997).

With only aggregate data on inequality and health, it is not possible to distinguish fully between the absolute income hypothesis and the relative income hypothesis. A useful way to see this is to combine both hypotheses algebraically. Here, we adapt the model presented in Gravelle, Wildman and Sutton (2002), who begin by hypothesizing that absolute individual income, $y$, is the only factor affecting an individual’s mortality risk, $m(y)$.

The expression $m(y)$ can be equivalently expressed in terms of individual income $y$ and mean income $\bar{y}$, through the following second order approximation:

$$m(y) \approx m(\bar{y}) + m'(\bar{y})(y - \bar{y}) + \frac{1}{2} m''(\bar{y})(y - \bar{y})^2$$  \hspace{1cm} (1)
We now introduce the relative income hypothesis. Suppose that individual mortality also depends on how an individual’s income compares with the mean income in some other reference population (the relevant population might be those in the same city, state, nation or workplace). For simplicity, let that reference group be the entire national population, and let the effect of inequality on individual mortality be a linear function of the variance of incomes in the population, \( V(y) \).

\[
m(y) \approx \left\{ m(\bar{y}) + m'(\bar{y})(y - \bar{y}) + \frac{1}{2} m''(\bar{y})(y - \bar{y})^2 \right\} + \alpha V(y)
\]

(2)

Taking expectations of each side:

\[
Em(y) \approx m(\bar{y}) + m'(\bar{y})E(y - \bar{y}) + \frac{1}{2} m''(\bar{y})E(y - \bar{y})^2 + \alpha EV(y)
\]

(3)

Which simplifies to:

\[
Em(y) \approx m(\bar{y}) + \frac{1}{2} m''(\bar{y})V(y) + \alpha EV(y)
\]

(4)

Note from equation (4) that:

- If the second derivative of mortality with respect to income is positive, then there will be a positive relationship between inequality and mortality.
Using aggregate data, we will be unable to estimate the second derivative of mortality with respect to income, and therefore unable to distinguish between the absolute and relative income hypotheses. However, if we do not find a relationship between inequality and health in aggregate data, it is unlikely that either hypothesis is true.

One way of attempting to circumvent the aggregation problem is to estimate an equation which includes both $\overline{y}$ and $\overline{y}^2$. But if the square of average income does not equal the average squared income, ie. $\overline{y}^2 \neq \frac{1}{N} \sum y^2$, then this will not solve the aggregation problem. In what follows, we experiment with specifications that include only $\overline{y}$, and those that include both $\overline{y}$ and $\overline{y}^2$.

Gravelle, Wildman and Sutton (2002) point out two further problems with most estimates of the relationship between inequality and health that use aggregate data. First, other country-specific factors may be correlated with both inequality and health; and second, the chosen inequality measure may not properly capture the true effect of inequality on health. By comparison with most of the previous cross-country literature in this field, our dataset allows us to do a better job of controlling other country characteristics, since all our specifications include a country fixed effect term, capturing any characteristics of the country that do not vary over time. However, with regard to the correct measure of inequality, we may not do as well as the previous literature. We do not know the best measure of inequality to capture the inequality-health relationship, but most theories suggest that a measure of inequality across the full distribution (such as the widely-used
Inequality and Health: Long-Run Evidence from a Panel of Countries

The Gini coefficient, is likely to do better than the measure we use: the share of the top 10 percent. Yet so long as the share of the top 10 percent is positively correlated with the ‘best’ measure of inequality, the relationship between aggregate health and the top 10 percent share is likely to reflect the relationship between aggregate health and other inequality measures.

One final concern is worth noting. Finding a correlation between inequality and health does not necessarily imply that the causal relationship runs from inequality to health. Instead, since sicker individuals are less likely to work, countries with lower health standards may have more earnings inequality. With long time series, one could use Granger causality tests to see whether lagged inequality affected current health, or whether lagged health affected current inequality. Since we find no statistically significant relationship between inequality and health, we do not pursue this approach.

3. Data on inequality and health

One major problem in earlier studies of the relationship between income inequality and health has been data quality. As Judge, Mulligan and Benzeval note in their review of the literature:

“Many of the studies use multiple sources of income distribution data and/or data from a wide range of years, which makes comparability between countries difficult.”

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2 An exception is Waldmann (1992), who finds a strong positive relationship between the income share of the richest 5 percent of the population and the infant mortality rate. His measures are based on a cross-section of 57 countries, with inequality and infant mortality measured at around 1970.
questionable. Only five of the studies use data based on a measure of equivalent
disposable income. In fact, we believe it is the generally poor quality of the
income data that poses the most serious weakness in most of the studies we have
reviewed.” (1998, 569)

While most studies use measures of inequality from the Deininger and Squire dataset or
the World Income Inequality Database, Judge, Mulligan and Benzeval opt instead to use
data from the Luxembourg Income Study (LIS), which uses a consistent measure of
income, size-equivalized disposable income, for all countries and years. Atkinson and
Brandolini (2001) have shown that using high-quality inequality data can make a
difference to the results. Making use of the LIS, Judge, Mulligan and Benzeval find no
significant relationships between inequality and life expectancy, or between inequality
and infant mortality.

However, using inequality measures from the LIS leads to a dramatic reduction in sample
size. Judge, Mulligan and Benzeval have only 16 countries in their sample, and only 10
countries with more than one observation. By contrast, three of the studies cited in their
literature review have a sample size of 50 observations or more. If there is indeed an
underlying relationship between health and inequality, it may be difficult to discern in
small samples, particularly if we wish to hold a number of other factors constant.

Here, we employ an alternative approach, measuring inequality by using the share of
income held by the richest 10 percent of the population. Derived from income reported to
the taxation authorities, our inequality measure is particularly sensitive to changes at the
top of the distribution. To a large extent, the share of the top 10 percent is affected by the
incomes of the top 1 percent. Regressing the top 10 percent share on the top 1 percent
share, in a specification including country and year fixed effects, the coefficient on the
top 1 percent share is 1.39 (T=32.2). Leigh (2004) shows the results from regressing the
top 10 percent share on various measures of inequality, taken from the Luxembourg
Income Survey (note that the measure of income in the LIS is equivalized household
disposable income). The relationship between the top 10 percent share and measures of
inequality that use after-tax household income is positive but weak in a specification that
does not include country or year fixed effects. However, in specifications with country
and year fixed effects, the relationship is positive and statistically significant for most
inequality measures. Separately regressing the top 10 percent share on different measures
of inequality, the coefficient on the gini is 0.746 (T=3.5), the coefficient on the 90:50
ratio is 0.890 (T=3.2), and the coefficient on the 50:10 ratio is -0.137 (T=-0.6). One
plausible explanation for this is that household structure and redistribution differ
systematically between countries; so while the levels of pre-tax top income shares are
only weakly related to levels of after-tax household inequality, changes in pre-tax top
income shares are a good proxy for changes in after-tax household inequality.

We use two measures of population health. The first is life expectancy at birth, which is
effectively a composite measure of the probabilities of dying at all ages, in a given year.
The second is the log of the infant mortality rate, which is the proportion of children born
alive who do not survive to their first birthday. Partly because infant mortality has a
strong impact on life expectancy, these measures are highly negatively correlated with one another (regressing one on the other, in a specification with country and year fixed effects, the T-statistic is -9.1; without country and year fixed effects, the R² is 0.84). In all specifications, we also control for real per capita GDP. Some specifications also control for the average educational attainment of the adult population, real per capita public health spending, and real per-capita private health spending. These three measures are only available from 1960. Appendix Table 1 presents summary statistics. Further details on variable construction may be found in the Data Appendix. Figures 2, 3 and 4 plot the inequality and health variables for each country.

If two series with persistent trends are regressed on one another, it can give rise to what time series econometricians have termed the “spurious regression problem.” Before estimating the relationship between inequality and health, we therefore carry out a standard diagnostic test for a unit root. In the case of the share series, this presents a special problem. Since the top 10 percent share is bounded between 0.1 and 1, it cannot technically have a unit root. Nonetheless, since the shares do not in practice ever reach either the upper or lower bounds, the series might exhibit nonstationary behavior, and standard OLS regressions may suffer from the spurious regression problem. In order to carry out unit root tests, we therefore follow Atkinson and Leigh (2004), and transform our bounded share variable S using the transformation \( \log S/(1-S) \).

Panel A of Table 1 presents Augmented Dickey-Fuller tests (Dickey and Fuller, 1981) against the null of a unit root, for the inequality and health variables. In both cases, we
cannot reject the hypothesis of a unit root for any of the five countries. Panel B presents the results of a Johansen (1995) test for cointegration between the top 10% share and each of the two measures of population health. With two exceptions – life expectancy in the Netherlands and Switzerland – the trace statistic is below the 5% threshold at which we would typically judge the series to be cointegrated. In the regressions that follow, we therefore estimate the relationship using OLS, but show specifications with both levels and differences.

4. Empirical Strategy and Results

To test the relationship between inequality and population health, we estimate the equation:

\[ m_{jt} = \alpha + \beta (\text{Share10})_{jt} + \gamma Z_{jt} + \delta_j + \rho_t + \varepsilon_{jt} \]  \hspace{1cm} (5)

where \( m \) is a measure of mortality (life expectancy or infant mortality) in country \( j \) in year \( t \), \( \text{Share10} \) is the income share of the richest 10 percent of the population, \( Z \) is a vector of time-varying country characteristics (including GDP, GDP squared, education, health expenditure), \( \delta \) is a country fixed effect, \( \rho \) is a year fixed effect, and \( \varepsilon \) is an error term. By including a country-fixed effect term, we capture a large set of unobservable country characteristics which might be correlated with both inequality and health. The year fixed effect term is intended to capture non-linear time trends which are common to
all countries, such as technological innovations. Standard errors are clustered at the country level.

We also estimate the first-differenced analog of equation (5):

\[
\Delta m_{jt} = \alpha + \beta (\Delta \text{Share10})_{jt} + \gamma \Delta Z_{jt} + \delta_j + \rho_t + \epsilon_{jt} \tag{6}
\]

To account for a variety of possible time periods over which the effect might operate, we estimate equation (6) with one-year differences, 5-year differences, and 10-year differences. These three specifications are designed to capture both short-term and long-term effects. Most of the theories outlined above would be likely to have an effect only over a 5-year or 10-year horizon. If the inequality-health channel is affected by public spending on healthcare, social capital, or a sense of relative deprivation, the effect of a rise in inequality will probably take some years to show up in health statistics. However, one possible channel that might have a short-term impact is crime: if increases in inequality lower life expectancy via crime, this effect might be felt within a year or two. Another factor that might operate rapidly is the impact of absolute income on infant mortality.

Tables 2, 3, 4 and 5 present the results of these regressions. Moving from left to right within each table, additional time-varying country characteristics are included in the \( Z \)

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3 This is of course something of an oversimplification, since technological innovations do not reach all countries at the same point in time. For example, Deaton and Paxson (2001) argue that technological innovations tend to reach the UK about four years after they arise in the US. However, since our variation is only at the country*year level, we cannot include a country*year fixed effect term.
vector. Columns 1 and 2 control only for GDP, columns 3 and 4 also control for GDP squared, columns 5 and 6 estimate the same equation, but only for 1960 onwards, and columns 7 and 8 control for average educational attainment, public health spending, and private health spending (available only from 1960 onwards).

Of the 32 specifications, the coefficient on the inequality variable is statistically significant in only two regressions – column 3 of Table 2, and column 4 of Table 5. Both are significant at only the 10 percent level, and the effects run in different directions. Overall, there is no evidence of any robust relationship between inequality and health in these data. Indeed, even excluding the country fixed effects, the relationship between inequality and health remains statistically insignificant (results not shown).

The coefficients of the controls, in contrast, mostly accord with expectations. Real per capita GDP is positively associated with better health outcomes, while the coefficient on the square of GDP is negative, suggesting that the protective effect of average income on health declines as GDP rises. Education is negatively correlated with infant mortality. Public health spending is associated with better health outcomes in the levels specification (though in the case of infant mortality, the sign of the effect is reversed in the 1-year and 5-year differenced specification). In general, we do not find any relationship between private health care expenditure and health outcomes, except in the 10-year differenced specification, where increases in private health spending are weakly

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4 We also experimented to see whether there was a relationship between inequality and homicide or suicide rates. Using death statistics from the World Health Organization’s Mortality Database (8 December 2004 version), which has coverage from 1950 onwards, we found no significant relationship between the top 10 percent share and the homicide rate, nor between the top 10 percent share and the suicide rate (results not shown).
associated with worse health outcomes, perhaps because health spending responds to negative health shocks.

5. Conclusion

By comparison with the strong positive relationship between GDP and health, past research has found that the inequality-health relationship was fragile. Here, we have used a new measure of income inequality – the income share of the richest 10 percent of the population – to test the relationship between inequality and health. By holding constant country fixed effects and allowing for a non-linear time trend, we hoped to circumvent some of the problems that have plagued past cross-country studies of health and inequality.

Our results showed that more GDP is associated with better health outcomes, and that this effect declines as GDP rises. But the relationship between inequality and health was both economically and statistically insignificant. Take for example the coefficients from the levels specification, controlling for GDP and GDP$^2$ (Table 2, Columns 3 and 4). The 95 percent confidence interval around the estimated impact of a one percentage point rise in the top 10 percent share shows that the effect is between a 0.0006 year decrease in life expectancy and a 0.007 year increase in life expectancy. Likewise, the 95 percent confidence interval around a one percentage point increase in the top 10 percent share on infant mortality suggests an effect between a 2 percent decrease and a 0.6 percent increase in infant mortality.
There are at least two possible explanations for our findings. One possibility is that our measure of inequality may not be capturing the type of inequality that affects health. While the top 10 percent share is highly correlated with the gini coefficient, it is uncorrelated with the 50:10 ratio. If inequality at the bottom of the distribution is what matters, we are not measuring the independent variable of interest.

Alternatively, our results may just indicate that the true relationship between inequality and health is non-existent, or too fragile as to show up in a specification such as this one. This would be consistent with a number of other careful cross-country papers, such as Judge, Mulligan and Benzeval (1998), and Deaton and Paxson (2001).
Data Appendix

Sources of life expectancy data

Most life expectancy at birth is taken from the Human Mortality Database (HMD), found at www.mortality.org. There are three exceptions:

- United States data are from the National Vital Statistics Reports, Vol.52, No.14, February 18, 2004, Table 12 (found at www.cdc.gov/nchs/about/major/dvs/mortdata.htm). For 1900-28, the figures are from death-registration states only. From 1929 onwards, they cover the entire US.
- Australian data are from Australian Bureau of Statistics, Australian Historical Population Statistics, ABS Catalog Number 3105.0.65.001, Table 48.

The following should also be noted:

- In the case of New Zealand, life expectancy from the HMD is available for 1937 onwards for Maori, non-Maori, and the total population, and from 1876 onwards for non-Maori only. We use the ratio of Maori to non-Maori life expectancy in 1937 and 1938 to form a consistent life expectancy series for the entire population from 1876-1936. This method assumes that the ratio of Maori to non-Maori life expectancy was the same in the pre-1937 period as in 1937-38.
- Figures for Canada 1997-2002 are from Statistics Canada (www.statcan.ca).
- Life expectancy is linearly interpolated for missing years.

Sources of infant mortality data

The infant mortality rate is measured as probability that a baby born live does not survive until its first birthday. This figure is typically expressed as a rate per 1000 births, and we follow this convention.

Most infant mortality data is taken from the Human Mortality Database (HMD), found at www.mortality.org. We use the tables Life Tables by Year of Death (1x1), and calculate infant mortality as q(x)*1000 for x=0, where q(x) is the probability of death between exact ages x and x+1. There are three exceptions:

to 1960, this excludes Alaska and Hawaii. Beginning 1970, it excludes births to, and
deaths of, nonresidents of the United States.
• Australian data are from Australian Bureau of Statistics, *Australian Historical
  Population Statistics*, ABS Catalog Number 3105.0.65.001, Table 46.
• Irish data is from Vital Statistics, 2001 Annual, p.137

Additionally:
• New Zealand data prior to 1937 are adjusted in the same manner as for life
  expectancy.
• UK infant mortality data only covers England and Wales.
• Canadian infant mortality for 1997-2002 is updated with figures from the Statistics
  Canada website.
• Infant mortality for missing years is interpolated log-linearly.

**GDP**
GDP is real GDP per capita (measured in 1990 International Geary-Khamis dollars),
from Angus Maddison, *The World Economy: Historical Statistics*. Data downloaded
from www.eco.rug.nl/~Maddison/

**Educational attainment**
Educational attainment is the average number of years of schooling for the population
Educational Attainment” Journal of Monetary Economics 32: 363-394; Barro, R.J. and
“International Data on Educational Attainment: Updates and Implications” Center for
International Development Working Paper 42. Cambridge, MA: CID.

Barro and Lee provide figures every 5 years from 1960-2000, and we linearly interpolate
for intervening years (and linearly extrapolate after 2000). Data can be downloaded from
www.cid.harvard.edu/ciddata/ciddata.html.

**Health expenditure**
Health expenditure is from the OECD Health Database 2003, worksheets 60-1 and 60-6,
downloaded from www.oecd.org. We use two variables, real public health expenditure
per capita, and real private health expenditure per capita (created as real total health
expenditure per capita minus real public health expenditure per capita).
References


Atkinson, A B and Leigh, A. 2004. “Understanding the Distribution of Top Incomes in Anglo-Saxon Countries over the Twentieth Century”, *mimeo*


Leigh, A. 2004. “Using Panel Data on Top Income Shares to Analyze the Causes and Effects of Inequality”, *mimeo*


Figure 1: A Non-Linear Relationship Between Income and Health

![Graph showing a non-linear relationship between income and health.]

A mean-preserving transfer from R to P will lower inequality and raise average health.

Figure 2: Income share of richest 10%

![Graph showing the income share of the richest 10% over time for various countries.]
Figure 3: Life expectancy at birth

![Life expectancy at birth graph](image)

- Australia
- Canada
- France
- Ireland
- Netherlands
- New Zealand
- Switzerland
- UK
- US

Figure 4: Log infant mortality rate (per 1000 births)

![Log infant mortality rate graph](image)

- Australia
- Canada
- France
- Ireland
- Netherlands
- New Zealand
- Switzerland
- UK
- US
Table 1: Stationarity Tests

Panel A: Dickey Fuller Tests

\[ \Delta Y_t = \gamma Y_{t-1} + \sum_{i=0}^{10} \beta_i \Delta Y_{t-i+1} + \varepsilon_t \]

<table>
<thead>
<tr>
<th>Country</th>
<th>Transformed top 10% share</th>
<th>Log infant mortality</th>
<th>Life expectancy</th>
<th>N</th>
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</thead>
<tbody>
<tr>
<td>Australia</td>
<td>-0.896</td>
<td>-2.711***</td>
<td>2.181</td>
<td>48</td>
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<tr>
<td>Canada</td>
<td>-0.987</td>
<td>-1.975***</td>
<td>1.513</td>
<td>48</td>
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<td>France</td>
<td>0.431</td>
<td>-2.847***</td>
<td>2.924</td>
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<td>Ireland</td>
<td>0.102</td>
<td>-1.893*</td>
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<tr>
<td>Netherlands</td>
<td>1.089</td>
<td>-3.091***</td>
<td>2.099</td>
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</tr>
<tr>
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<td>1.796</td>
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<td>Switzerland</td>
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<tr>
<td>UK</td>
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<td>-2.365**</td>
<td>3.561</td>
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<td>-0.358</td>
<td>-2.331***</td>
<td>2.902</td>
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</table>

Panel B: Johansen Cointegration Tests on Top 10% Share and Health Measures (5 lags)

<table>
<thead>
<tr>
<th>Country</th>
<th>Trace statistic for log infant mortality</th>
<th>Trace statistic for life expectancy</th>
<th>5% critical value</th>
<th>1% critical value</th>
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</thead>
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<td>9.463</td>
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<td>Canada</td>
<td>4.758</td>
<td>4.406</td>
<td>15.41</td>
<td>20.04</td>
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<td>France</td>
<td>11.377</td>
<td>13.002</td>
<td>15.41</td>
<td>20.04</td>
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<td>20.04</td>
</tr>
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<td>15.41</td>
<td>20.04</td>
</tr>
<tr>
<td>US</td>
<td>5.711</td>
<td>3.251</td>
<td>15.41</td>
<td>20.04</td>
</tr>
</tbody>
</table>

Notes:
1. In Panel A, Y is the variable to be tested. In the case of the top 10% share, Y=log S/(1-S), where S is the share of the top 10% group.
2. *** and ** denote rejection of the null hypothesis of a unit root at the 1%, 5% and 10% levels respectively (in all cases, the 10% critical value is -1.610).
3. All specifications include 11 lags of the differenced variable, chosen according to the Schwert criterion.
4. Since the New Zealand and UK series both have breaks, the tests are for New Zealand data after 1953, and UK data before 1990.
### Table 2: Top 10% Share and Health: Levels

<table>
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</tr>
</thead>
<tbody>
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<td>Income share of richest 10%</td>
<td>0.001</td>
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<td>0.003*</td>
<td>-0.008</td>
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**Note:**
1. Robust standard errors, clustered at the country level, in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels respectively.
2. Dependent variables: LE is average life expectancy at birth, IM is the log of the infant mortality rate (per 1000 live births).
3. Columns 5 and 6 are restricted to 1960 onwards.
4. All specifications include a dummy variable for the change from joint to individual filing in New Zealand in 1953, and in the UK in 1990 (see Leigh 2004 for details).
### Table 3: Top 10% Share and Health: 1-year differences

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Note:
1. Robust standard errors, clustered at the country level, in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels respectively.
2. Dependent variables: LE is average life expectancy at birth, IM is the log of the infant mortality rate (per 1000 live births).
3. Columns 5 and 6 are restricted to 1960 onwards.
4. All specifications include a dummy variable for the change from joint to individual filing in New Zealand in 1953, and in the UK in 1990 (see Leigh 2004 for details).
Table 4: Top 10% Share and Health: 5-year differences

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<td>0.022**</td>
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Note:
1. Robust standard errors, clustered at the country level, in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels respectively.
2. Dependent variables: LE is average life expectancy at birth, IM is the log of the infant mortality rate (per 1000 live births).
3. Columns 5 and 6 are restricted to 1960 onwards.
4. All specifications include a dummy variable for the change from joint to individual filing in New Zealand in 1953, and in the UK in 1990 (see Leigh 2004 for details).
Table 5: Top 10% Share and Health: 10-year differences

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<td>0.052*</td>
<td>-0.004</td>
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Note:
1. Robust standard errors, clustered at the country level, in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels respectively.
2. Dependent variables: LE is average life expectancy at birth, IM is the log of the infant mortality rate (per 1000 live births).
3. Columns 5 and 6 are restricted to 1960 onwards.
4. All specifications include a dummy variable for the change from joint to individual filing in New Zealand in 1953, and in the UK in 1990 (see Leigh 2004 for details).
### Appendix Table 1: Summary Statistics

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