Absolute income and life expectancy

Wilkinson¹ and Lynch et al² produce widely differing estimates of the relation between absolute income and life expectancy. In an earlier version, Wilkinson focused on "developed" societies and reported a correlation of 0.08 based on 23 OECD countries. Lynch et al³ focus on levels of affluence and after adding 10 "equally wealthy" countries report a correlation of 0.51.

They also point out that GDP and life expectancy estimates vary according to the data source. It follows that part of the discrepancy between the two findings could be due to using different sources. To quantify this, I attempted to reproduce both analyses from a single data source: the World Bank Development Indicators CD-ROM (1999). Lynch et al used the 1997 version, but the GDP data for 1993 seem identical in the later version. Life expectancy data were also drawn from the same source, whereas Lynch et al used the WHO website. Where data were not available for 1993, they were estimated by linear interpolation from the nearest two years before and after. This leads to slightly different estimates of life expectancy, but should not materially affect the results here.

Two further points are worth mentioning. Firstly, Lynch et al include 33 countries with GDP per capita over $10,000, whereas there are 34 such countries in this dataset. Apparently, Hong Kong was omitted from the earlier version. It has been included here, in keeping with the logic of including all equally wealthy countries. Secondly, in 1993 the OECD comprised 24 member states. Turkey, which ranked 57 in terms of GDP per capita at $5550, has been excluded.

Among the 34 states with GDP over $10,000 the correlation with life expectancy is 0.47, slightly lower than that reported by Lynch et al. The correlation among OECD members is 0.16, twice the value reported by Wilkinson. Thus a large discrepancy remains when the estimates are calculated from a single data source, although it is reduced somewhat.

Can the discrepancy be explained? Wilkinson argues that the non-OECD countries are poorer and that there is a different relation between GDP and life expectancy among poorer countries. This can be modelled in a linear regression as an interaction between GDP and OECD membership. Although the result falls just outside conventional significance ($p=0.0610$), the fitted regression lines are reproduced for illustrative purposes in figure 1. The figure also includes infant mortality as an indicator of a country’s level of development. The area of each circle is proportional to the country’s infant mortality rate per 1000 live births. For clarity, only the non-OECD countries are labelled. (See Lynch et al⁴ for identification of the OECD countries.)

It is clear that the GDP ranges do overlap considerably, but the range is greater among OECD countries and proportionately more of the non-OECD countries are at the bottom end. The mean GDP for OECD countries is $18 503 and $15 453 for non-OECD countries ($r=-2.1, p=0.0425$). Malta has the greatest statistical influence on the regression parameters and the slope for non-OECD countries would otherwise be steeper. Relatively high rates of infant mortality are evident for several of the non-OECD countries, particularly the Bahamas and the oil producing countries of the Middle East.

When infant mortality is introduced into the regression, it predicts 66% of the variance in life expectancy from age 1 is modelled as Wilkinson argues. However, life expectancy from birth involves infant mortality in its calculation so the association will be artificially inflated. When life expectancy from age 1 is modelled instead, the percentage of variance explained by infant mortality decreases from 91 to 85, but the additional contribution of GDP only increases from 1% to 1.2%.

If the infant mortality rate is assumed to be an indicator of a country’s level of "development", then the additional effect of GDP on life expectancy would seem relatively modest, as Wilkinson argues.

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⁴ www.oecd.org/about/general/member-countries

Author’s reply

I am grateful for Geoff Der’s efforts to arbitrate on the disagreement between myself and John Lynch et al on the association—or lack of it—between life expectancy and GNP per capita (GNPpc) among the developed countries. I think we all agree that there is a relation among poorer countries that weakens as countries get richer: the curve of life expectancy against GNPpc rises steeply and then levels off at higher levels of income. As I thought my reply to Lynch et al made clear, whether or not you find an association depends on how rich a group of countries you take. The important issue is surely whether there is a group of rich countries above some level of GNPpc among which the relation has largely or wholly disappeared. Since my previous reply in which I showed that among the 20 or so richest OECD countries in 1995, there was not just no significant association, but actually an inverse association ($r=-0.011$), I have analysed WHO life expectancy and World Bank GNPpc data for 1998.² Among the richest 25 countries for which data are available there is again a slight negative relation between the two ($r=-0.107$).

Nothing is served by adding in a few poorer countries and asserting that there is a relation. We can see that the lack of relation among the richest countries is part of a clear trend showing the declining impact of GNPpc. The fact that there is a substantial group of rich countries among which the

Figure 1. Life expectancy at birth by GDP per capita in 1993 US$ (purchasing power parity) with area of circles proportional to infant mortality per 1000 live births.
relation has completely disappeared is of the utmost importance both in terms of the justification for consuming an increasing share of the world’s resources, and for our interpretation of the social gradient within these countries. As Lynch et al were substantially concerned with the interpretation of the relation between income inequality and mortality within the USA, it is surely appropriate to look to see if there is a relation between GNP per capita and life expectancy among the richest countries alone.

Although Lynch et al refer to me as suggesting a $10 000 cut off point (in 1990 dollars) for GNPpc, they took this from a graph that was simply where my graphics package happened to start to label the x axis for a graph of OECD countries. Given the nature of the gentle flattening curve of rising life expectancy against GNPpc, it would be silly to define a hard and fast cut off point.

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4 World Bank. 2000 World Development Indicators CD-ROM.

Authors’ reply

We thank Der for his independent replication of our findings—that in contrast with Wilkinson’s results—there is an association between GDP/capita and life expectancy among wealthier countries. However, we are somewhat puzzled why Der suggests that some “discrepancy” exists that requires explanation. We clearly stated that any answer regarding the strength of association between GDP and life expectancy crucially depends on which countries are included. One of our primary goals in that paper and others has been to raise the issue that investigation of a grand hypothesis concerning the mechanisms linking income inequality to health should begin with a full rendering of the empirical evidence.

Der concludes that if infant mortality is a reasonable indicator of a country’s “development” then indeed Wilkinson’s conclusions are basically correct—that GDP does not tell us much about international differences in life expectancy. This is certainly informative, but in our view, beside the main point. Wilkinson focused on a selection of OECD countries—Turkey, a lower GDP/lower life expectancy country was excluded. Der argues that Wilkinson chose these 23 countries based on a criterion related to their state of “development”, while we simply included all 33 countries above Wilkinson’s $10 000 GDP/capita cut off. Thus, in Der’s terms we used a criterion of “affluence”. Again, we have already clearly stated, “What constitutes an appropriate set of countries for comparisons of this sort is certainly open to debate” (page 405).

Wilkinson’s inclusion criteria are even more convoluted than suggested by Der. In justifying his selection of countries Wilkinson stated that, “Using data from the OECD countries reduces the influence of extraneous cultural differences by restricting the comparisons to developed, democratic countries with market economies” (page 592). Thus, in addition to the undefined notion of “development”, there are apparently considerations of democratic government and market-based economy that serve to reduce confounding by “extraneous cultural differences”.

Evidently such cultural differences are not a barrier when comparing OECD countries like Portugal, Australia and Finland. In fact, international comparisons have suggested quite the opposite—that the complex intersections of historical, socioeconomic and cultural factors may be important in understanding international differences in cause specific health inequalities. If criteria of development, democracy and market economy were used and not simply OECD membership, should we then include richer, democratic, market-based countries such as the Bahamas, Malta, Israel, and Korea?

Some readers may judge that Wilkinson was correct in choosing the criterion of OECD membership as being the most salient, but that is not the point. The point is, that in the end, whatever selection criteria are used—they are arbitrary and should be justified. Is it appropriate to examine only those 15 countries with lowest infant mortality or those 20 with highest life expectancy? In our view, data selections based on arbitrary criteria should not form one of the central aspects of the empirical evidence-base used to support the currently influential psychosocial hypothesis of health inequalities. As our original and now Der’s analyses have shown, under different data selection criteria, conclusions about links between GDP/capita and life expectancy are the opposite of those reached by Wilkinson. A grand hypothesis that is so heavily dependent on choice of data is clearly not strongly supported, with different inclusion criteria giving different results. Basing conclusions on only one set of potential inclusion criteria is also contrary to one of the basic tenets of evidence epidemiological analysis—that we should only take notice of results that are stable under a variety of reasonable sampling and analytic assumptions.

OECD membership is based on historical, political and economic factors. Interestingly, Armada and Muntaner (Armada F, Muntaner C, VIII Latinoamerican Congress of Social Medicine, Alaines: Colombia, Bogota, 2000) have recently found a negative correlation ($r = -0.59$) among OECD nations, between years of social democratic government (1946–1980) and the extent of income inequality (1990). Using OECD as an indicator of development also does not capture the international political influence that OECD countries yield over peripheral nations through military interventions and control of financial institutions, which may in turn affect health within those countries.

International differences in health among the richest countries of the world are unlikely to be explained by any version of a one size fits all grand hypothesis—be it material or psychosocial. Such understanding will more probably come from historical, political, economic and culturally contextualised accounts of how and why socioeconomic relations within and between countries have been and continue to be linked to the ways in which certain groups avoid negative health exposures and acquire health protective resources.

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