

The material below is a version of:

Scanlan JP. Measurement lessons learned, then forgotten. *Journal Review* May 7, 1009 (responding to Mackenbach JP, Stirbu I, Roskam AJ, *et al.* Socioeconomic inequalities in health in 22 European countries. *N Engl J Med* 2008;358:2468-2481): <http://journalreview.org/v2/articles/view/18525043.html>

The compression function of journalreview.org eliminated the paragraph breaks. Those breaks have been restored.

Title: Measurement lessons learned, then forgotten

As Mackenbach et al.[1] note, their article comparing the size of health inequalities in 22 European countries bears a relation to an earlier article, which was co-authored by two of the authors (Johan P. Mackenbach and Anton E. Kunst) and which, like the instant article, involved a collaboration of the European Working Group on Socioeconomic Inequalities in Health. The earlier article[2], a 1997 study in *The Lancet*, has become something of a landmark. The study surprised many by finding that among western European countries, the egalitarian countries of Norway and Sweden showed comparatively large relative differences in mortality between higher and lower social classes, and the study has been much discussed for that reason. The instant study similarly finds “no evidence for systematically smaller inequalities in health in countries in Northern Europe,” a finding the authors regard as “surprising” given the long history of egalitarian policies in those countries.

For over 20 years, I have been making a point that would call into question the seemingly surprising nature of the comparatively large relative inequalities in mortality in Norway and Sweden. About the tenth published expression of the point occurred in a 2000 article called “Race and Mortality,”[3] which explained the statistical pattern whereby the rarer an outcome the greater tends to be the relative difference in experiencing it and the smaller tends to be the relative difference in avoiding it. There, as in some earlier works, I explained why there is reason to expect relative differences in adverse outcomes to be large within particularly advantaged subpopulations (though relative differences in the opposite outcomes to be small in such subpopulations) because adverse outcomes tend to be rarer in advantaged subpopulations. Among examples cited in *Race and Mortality* was the large relative difference between black and white infant mortality rates where mothers were college-educated that had been found in an 1992 *New England Journal of Medicine* study [4] and that, like findings in the 1997 *Lancet* study, had prompted much surprise and commentary. And, as I would point out in a presentation in Oslo the next year, the same reasoning suggests that one should expect to find large relative socioeconomic differences in mortality in countries with low mortality.[5]

At the time, I was not aware of the 1997 *Lancet* study, and, indeed, it was at the Oslo conference that I first even learned of the perception, evidently based on that study, that Norway had comparatively large health inequalities. On later learning of the *Lancet*

study, I pointed out in a number of places, that, even allowing that differences between the health of advantaged and disadvantaged groups in Norway and Sweden might in a meaningful sense be smaller than in other countries, relative differences in adverse outcomes like mortality might be expected to be large in those countries simply because mortality is low in those countries.[6-11]

Meanwhile, in late 2007, Houweling et al.[12] (with Mackenbach and Kunst as co-authors) published an article devoted in substantial part to addressing the points I had advanced in *Race and Mortality* concerning the correlations of relative differences in experiencing and avoiding an outcome with the overall prevalence of the outcome. In my view, the treatment of *Race and Mortality* by Houweling *et al.* was deficient in a number of respects. Among other things, while quarreling with *Race and Mortality* for what they regarded as an overstatement of the strength of the patterns of correlation between prevalence and relative differences in experiencing and avoiding an outcome, the authors failed to discuss the extensive treatment in *Race and Mortality* of the reasons why relative differences would often be larger where outcomes were rarer (*i.e.*, that the patterns were inherent in the shapes of underlying distributions of risk) and why one would often observe departures from the described patterns (*i.e.*, among other things, that, apart from prevalence, observed patterns of relative differences are importantly functions of differences between the means of the risk distributions). The failure of Houweling *et al.* to consider these points detracted from their efforts both to explain the patterns they observed and to suggest ways of taking overall prevalence into account while relying on standard measures of difference between rates to appraise the size of health inequalities.

Nevertheless, the authors reached essentially the same conclusions I had reached in *Race and Mortality* and related works. That is, the Houweling article found systematic correlations between the prevalence of an outcome and relative differences in experiencing it and avoiding it that are basically the same as those described in *Race and Mortality*. And, while the authors were evidently unaware of recent treatments applying the reasoning of *Race and Mortality* to absolute differences (and odds ratios), as in references in 6-8 (and a dozen or so other 2005 to 2007 references listed on the Measuring Health Disparities (MHD) page of jpscanlan.com), the Houweling article also found the same reverse U-shaped pattern of correlations between the absolute differences and the prevalence of an outcome that I had described in those treatments (though they also incorrectly concluded that odds ratios avoided the problem). Crucially, while concluding that measures like relative and absolute differences were useful notwithstanding the correlations with overall prevalence, the authors also concluded that such measures were only useful when overall prevalence was in some manner taken into account.

Given the prominence of Mackenbach and Kunst in the field of measuring health inequalities, this conclusion was of potentially immense importance. For, as with my work in this area, in addition to calling into question the 1997 *Lancet* study, the conclusion called into question virtually all research into the comparative size on health inequalities – whether with regard to different geographical settings, different conditions, or different points in time – since no such research to date had even recognized the

effects of overall prevalence of an outcome on measures of differences between rates much less attempted to take such effects into account.

Yet the instant study by Mackenbach *et al.* is precisely that same sort of research. And not only do the authors fail to consider the implication of overall prevalence, as the Houweling article found was necessary, they fail even to reference the Houweling article that two of them co-authored, and they fail as well to indicate any of the understanding reflected in the article as to the effects of overall prevalence on various measures of differences between rates.

As explained at length in *Race and Mortality*, interpreting the size of differences between rates is invariably complicated. Reference 13, which employs a method for measuring differences between outcome rates that should be unaffected by the overall prevalence of an outcome, suggests that the comparatively large relative differences in mortality observed in Norway and Sweden may not have been solely or mainly functions of the low mortality in those countries. See also Table 8 of reference 14. And see generally the material on the Solutions and Solutions Database sub-page of the MHD.

These materials suggest how very difficult it may be to take overall prevalence into account. Despite such difficulties, however, it makes absolutely no sense to attempt to compare the size of inequalities across countries, or to opine about the causes underlying observed patterns, without considering the implications of overall prevalence.

The same holds with regard to various other points in the instant Mackenbach article that involve interpretations of the size of differences between rates. The authors note, for example, that relative socioeconomic inequalities in rates of poorer self-assessed health are generally smaller than inequalities in death from any cause. Armed with the understanding of the role of overall prevalence in the size of relative differences that one could derive from *Race and Mortality* or a hundred or so other references on MHD – or the article by Houweling *et al.* – an observer's first thought here would be that relative inequalities may well be smaller for poorer self-assessed health than for death from any cause because poorer self-assessed health is more common than death from any cause. In order to know whether that first thought is validated in reality, however, one needs to see the actual rates. This is why setting out the actual underlying rates is crucial in any study that endeavors to compare size of inequalities. But the article fails to provide that information.

The authors also note that obesity is unlikely to be a major contributor to international variations in inequalities in health, because inequalities in obesity are large where inequalities in mortality are small. This reasoning is critically flawed for failing to consider that inequalities in rates of experiencing a mortality-related condition like obesity will tend to be large where the condition is rare and that, where a condition is rare, it may have little overall effect on inequalities in mortality. It is in circumstances where obesity is more prevalent (and hence where relative differences in obesity tend to be smaller) that obesity is likely to have a large role health inequalities.

For example, consider the case where country A is a high obesity country and country B is a low obesity country. Let us assume that in fact in the two countries the obesity risk distributions differ by half a standard deviation. Thus, based on Table A of reference 8, with slight rounding we can devise a hypothetical where in country A the obesity rates for low and high SES are 50% and 30% (a risk ratio of 1.67); in country B, the obesity rates for low and high SES are 13% and 5% (a risk ratio of 2.6). As noted, the difference between the obesity distributions of high and low SES is in fact the same in each country. But the perceived difference in obesity according to SES, based on the relative risk of obesity, is greater in country B where obesity is less prevalent.

Of course, the relative difference in avoidance of obesity is greater in country A, where avoidance of obesity is rarer, than in country B. That is, in country A the risk ratio for avoidance of obesity is 1.4 (70/50) (with the high SES rate as the numerator); in country B the risk ratio is only 1.09 (95/87). That the relative difference in obesity is larger in country B, while the relative difference in avoidance of obesity is larger in country A, is a reason why it makes no sense to regard either relative difference as indicating which country has the larger SES-related obesity difference in a meaningful sense.)

Let us also posit that in both countries among both high and low SES, obesity doubles the risk of mortality and that the risk ratio of low SES to high SES is 1.5 for both obese and non-obese. More specifically, in both countries, for low SES the mortality rate is 1.5% among the non-obese and 3.0% among the obese; for high SES the mortality rate is 1.0% among the non-obese and 2.0% among the obese.

The assumption that the low-to-high SES mortality risk ratio is the same for both obese and non-obese is not realistic. For reasons explained in Race and Mortality and various other places, the SES risk ratio for obesity would tend to be lower among the obese (where mortality is higher) than among the non-obese (where mortality is lower). Further, it would be irrational to expect the same SES mortality risk ratio among the obese as among the non-obese, for the simple reason that it is impossible for obesity to have the same proportionate effect in increasing the mortality of two groups that is has in reducing the survival of the two groups (as discussed on the Subgroup Effects/Interaction sub-page of the Scanlan's Rule page of jpscanlan.com.) Thus, a more precise hypothetical would employ rates that reflect similar effects of obesity on the underlying distributions, information that could also be derived from information in Table 1 of reference 8. But the departures from a more realistic hypothetical are unlikely to make much difference and the hypothetical numbers set out above are satisfactory for illustrating the point that it is where there is much obesity (which tends to correspond with lower relative differences in obesity) that obesity differences by SES can have a large impact on SES differences in mortality. From another perspective, it is a large absolute difference in obesity rates that can have a large effect on mortality. And large absolute differences in obesity tend to correspond with low relative differences.

In any case, based on the figures provided above, an adjustment that attributed the high SES obesity profile to the low SES group in each country, would reduce the relative difference in mortality more in country A than in country B. The unadjusted low to high

SES mortality risk ratios are 1.7 in country A and 1.61 in country B; the adjusted risk ratios are 1.5 in both countries. Thus, the adjustment reduces the relative difference by 32% in country A and 19% in country B. In other words, obesity has a greater effect on mortality in country A than in country B.

The treatment of Race and Mortality by Houweling et al., and the subsequent ignoring of Houweling et al. by its authors, invites comparison with the treatment of Race and Mortality by the statisticians of the National Center for Health Statistics (NCHS). In a number of works, the statisticians principally responsible for the NCHS approach to measuring health disparities (as health inequalities tend usually to be termed in the United States) have referenced Race and Mortality and/or an earlier expression of the key point [15] for showing instances where whether a disparity is increasing or decreasing may turn on whether one examines relative differences in an adverse outcome or relative differences in the (opposite) favorable outcome.[16,17,18]. They have dealt with the matter simply by deciding that henceforth all disparities will be measured in terms of relative differences in adverse outcomes.

At one conference session in which we both participated,[19,20] Keppel expressed his disagreement with the reasoning of Race and Mortality and related works (such disagreement involving a claim that the reasoning, while applicable to cross-sectional data, is not applicable to longitudinal data). But none of the published works by Keppel and his colleagues acknowledges that the point of Race and Mortality is not that in particular instances whether a disparity was deemed to have increased or decreased turned on whether one examined the relative difference in the favorable outcome or the relative difference in the adverse outcome, but that the two relative differences tend to change systematically in opposite direction as the overall prevalence of an outcome changes. This failure to acknowledge the point has continued, as in reference 18, even after I have repeatedly challenged the failure to straightforwardly set out the claim or Race and Morality and attempt to address it.[6,8,21]

Thus, on the one hand, NCHS statisticians refuse to address the point I raised in Race and Mortality and elsewhere. On the other hand, Mackenbach and Kunst address the point, largely agree with it and its implications, but then ignore it their own work. In both cases, the public and the research community eventually are led to think not simply that health disparities/inequalities research is sound but that no one has even suggested that it is unsound.

Researchers in the United Kingdom, including one member of the European Working Group on Socioeconomic Inequalities in Health, have recently addressed the implications of the pattern whereby the rarer an outcome the greater tends to be the relative difference in experiencing it and the smaller tends to be the relative difference in avoiding it as the pattern bears on the setting of health inequalities reduction goals. In that context, the authors observed that “[i]f governments fail to take account of ‘Scanlan’s rule’ they run the risk of guaranteeing failure, largely for conceptual and methodological reasons rather than social welfare reasons.” [22] The harms arising from the potential misinterpretation of progress toward meeting inequalities reduction goals because of the failure to address

the measurement issues described in Race and Mortality and elsewhere may in fact be relatively minor compared with the harms associated the vast waste of resources devoted to health disparities/inequalities research without consideration of the extent to which the tools employed in that research are fundamentally unsound.

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Thus, we see from

Mackenbach NEJM 2008 obesity effects					
County	SES	Obese	ObRate	AdjObRate	Mort
A	H	Y	30.00%	30.00%	2.00%
A	H	N	70.00%	70.00%	1.00%
A	L	Y	50.00%	30.00%	3.00%
A	L	N	50.00%	70.00%	1.50%
B	H	Y	5.00%	5.00%	2.00%
B	H	N	95.00%	95.00%	1.00%
B	L	Y	13.00%	5.00%	3.00%
B	L	N	87.00%	95.00%	1.50%

9931 o a 6 show reductions			
County	UR	AR	ObAdjRed
A	1.73076923076923	1.5	0.315789473684211
B	1.61428571428571	1.5	0.186046511627907

Finally, the Times Higher/Greater Issues sub-page of the Vignettes page of jpscanlan.com addresses the pattern whereby in almost every major scientific journal, it is far more common in circumstances where, for example, one rate is 9% and the other 3%. for authors to (incorrectly) describe the former figure as three time higher than the latter rather than (correctly) as three times as high. Three times higher is the same as four times as high. Mackenbach employ the former usage. This would not warrant mention – and, again, the authors have employed the far more common usage – save that the single journal that seems to understand this issue and that makes a concerted to employ the correct usage is the *New England Journal of Medicine*, as illustrated in Tables 2 through

4. The puzzling question is why, given that is editors understand the issue and apparently have chosen to employ the current, they fail to require in every case.