

Commentary: Persistent Social Inequalities in Health—Insensitive Outcomes, Inadequate Policies, or Both?

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IN THEIR THOUGHTFUL LETTER RESPONDING TO OUR ARTICLE IN *The Milbank Quarterly's* December 2011 issue (Frank and Haw 2011), McCartney and colleagues (2013) raise six issues on which we would like to comment.

First, they challenge our main criticism of the routinely collected population health outcomes that recent annual Scottish reports have used to monitor health inequalities by socioeconomic status (SES)—namely, that many of these eleven outcomes are inherently unresponsive (or only very slowly responsive) to policy or program interventions that can feasibly be delivered by public-sector authorities. Specifically, McCartney and colleagues disagree with our contention that the epidemiological insensitivity of many routinely collected health indicators to prompt change is one of the reasons that Scotland and other jurisdictions (Mackenbach 2012; Marmot et al. 2012) are currently finding health inequalities by social class very hard to reduce, at least in terms of the “absolute difference” between the most and the least privileged socioeconomic groups’ measures of health status. McCartney and colleagues imply that we are too readily letting the governments of these jurisdictions “off the hook,” in that these national policy documents have tended to commit repeatedly to reducing such inequalities. They go on to suggest that these governments have not adequately addressed the structural and economic policy-related determinants of social stratification per se. They then offer examples of recent situations around the world demonstrating that deliberate, redistributive government policies, combined with global

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and national economic trends, have rapidly shifted both socioeconomic inequality itself and health inequalities by social class.

In response, our article did not attempt to assess the relative success of recent UK and Scottish economic—for example, tax and welfare-benefit—policies in reducing SES inequalities as such. That work properly lies in the domain of economists and social policy analysts. Rather, our article focused entirely on the epidemiological and other (e.g., communications-related) attributes of the particular health outcomes, as well as the associated statistical analyses, routinely used to monitor health inequalities by SES in Scotland. We argued that the first step in monitoring health inequalities by socioeconomic position in any jurisdiction—and especially Scotland, which has implemented many policies and programs with the specific aim of reducing such inequalities—is to ensure that the measurements and analyses used in monitoring inequalities meet basic epidemiological standards for population health status measurement. Our article put forward such standards, in the form of “critical appraisal criteria,” for assessing *any* set of analyses used for monitoring population health inequalities by SES, over time. We then showed that although the analyses of indicators presented in recent Scottish reports meet many of those criteria (and also represent the methodologically best reports we have been able to identify internationally), they do not meet all. In particular they do not meet the criterion “responsiveness to change.” In other words, our criticisms were not specifically aimed at the Scottish reports but rather at the entire global field of endeavor, in an attempt to improve methodology in the future.

We do agree with McCartney and colleagues, and also with a thoughtful recent article by Capewell and O’Flaherty (2011), that there are credible examples of relatively rapid changes in the health status of entire populations, particularly in regard to cardiovascular morbidity and mortality. Thus we agree that even such late-life disease outcomes (see our second comment below) can be promptly altered in five years or less by powerful exogenous phenomena. However, we should point out that these phenomena usually are major economic, political, or natural events such as recession, war, or infectious epidemics rather than public-sector policies. The exception to this is public health legislation resulting in rapid population-level shifts in exposure to major cardiovascular risk factors: for example, Mauritius’s systematic change in its supply of cooking oil (Chitsono, Collins, and Dowse 1995) and Scotland’s ban on smoking

in public places (Mackay 2010; Pell et al. 2008). Though of great importance, these rapid, policy-related shifts in cardiovascular or pulmonary mortality and morbidity represent the exception rather than the rule in public health efforts to reduce health inequalities. That is because these measures were successful in quickly improving *overall* population health by virtue of acting “upstream” in a way that impacted entire populations, obviating individual-level compliance with, for example, healthier “lifestyle”-related behaviors. Indeed, as Macintyre has pointed out, this is precisely the sort of public health intervention that could be expected to have the largest impact on reducing inequalities (Macintyre 2007).

Second, McCartney and colleagues also contend that we wrongly attributed this insensitivity, of most routinely collected indicators of health inequalities, to their capture of largely *later-life* health events and morbid conditions, such as all-cause, cardiovascular, and cancer mortality; life and health expectancy; incident cancers; and most hospital admissions for chronic diseases. Conversely, we have argued that these outcomes are subject to a kind of “epidemiological inertia” due to their determination by individuals’ entire previous life course of exposures which cannot be changed in retrospect. McCartney and colleagues contend instead that “inequalities in mortality in Scotland are very high among young adults” and that such mortality inequalities “show marked temporal and geographical variation, suggesting that socioeconomic determinants of health have a profound impact on these outcomes.” In sum, they conclude that “the relative stability in recent years of inequality measures is as likely to be due to policy failure as to a lack of amenability [to change].”

We did not assess in our article, nor do we feel capable of doing so here, whether recent Scottish Government (SG) policies to tackle social and economic inequalities per se—and thereby to mitigate their health consequences—have been “adequate.” Our article merely points out that most of the eleven health outcomes analyzed in the annual SG reports on health inequalities, since 2008, have substantial human biological or epidemiological reasons for being rather slow to change, especially in an equitable way that reduces inequalities, by implementing the feasible policy and program interventions currently available to any government. We note in particular that the key factor influencing this relative “non-responsiveness to change” is the heavy weighting, in whole-population mortality/life- and health-expectancy and hospitalization measures, of

late-life health outcomes resulting largely from chronic disease processes typically some decades long.

We completely agree with McCartney and colleagues that the *relative* magnitudes of Scottish inequalities in youth and young-adult mortality by SES—as indicated by their Relative Index of Inequality (RII) values (Leyland et al. 2007)—are large. We further agree that these deaths are mostly due to behaviors—typically related to alcohol and drug misuse, suicide and violence/trauma—which are all strongly affected by local “culture” (as opposed to some sort of long-latency biological process, such as carcinogenesis), and therefore they are potentially amenable to change. Finally, as Leyland and colleagues (2007) have themselves shown, these youth and early-adulthood mortality inequalities have indeed been growing more rapidly, in recent decades in Scotland, than relative health inequalities in later-life outcomes. However, the pattern for *absolute* inequalities in Scottish mortality, across the entire life course, is rather different. Because the hospitalization and fatal events that comprise most of the outcomes analyzed in the Scottish inequalities reports are so much more common in later life than similar events in young adult life, even the rather high RIIs for younger adults’ rates of these events, by SES, do not contribute much to overall population inequalities (i.e., those computed across nearly all age groups, usually under age seventy-five, in the recent Scottish reports). The reports document very stable RIIs of 1.5 to 1.8 for all-cause mortality in 15- to 44-year-olds, annually since 1998, equivalent to a five-fold ratio between the relatively unchanging rates of the top and bottom SES deciles. Thus, while later-life inequalities in hospitalization and mortality in Scotland (driven by the usual chronic diseases) are relatively stable and mostly smaller, in relative magnitude, than the equivalent inequalities in young adults (largely driven by alcohol, illegal drugs, and violence), the former still largely determine the overall Scottish population’s health inequalities, and will do so for some time to come.

Third, McCartney and colleagues imply that our article prematurely “dismissed” the Warwick-Edinburgh Mental Well-Being Scale (WEMWBS) on the grounds that so far there has been no statistically (or clinically) significant change in the measure, for either the Scottish population as a whole, or the difference between the top 10 percent and bottom 10 percent of the population stratified by SES. They then argue that many countries have found that measures related to “happiness” and life satisfaction are relatively stable in the modern era (which

surely reinforces our point about this measure's likely insensitivity to change).

In response, we freely acknowledge that the WEMWBS measure, used for only four Scottish Health Surveys to date, from 2008 to 2011, may eventually turn out to be capable of detecting changes in either overall population-level or SES-strata-related health status. We have merely noted in our article that it is not a very promising measure because there already are clues that it may be insensitive to *both* SES and change over time. We arrived at this conclusion because the WEMWBS measure shows only a 10 percent difference in mean scores between the *extreme* (top and bottom) deciles of the Scottish population, ranked by the postal code Scottish Index of Multiple Deprivation (Scottish Government 2012), and no meaningful change in scores (either overall or for the rich-poor gap) between 2008 and 2011. This holds true despite the advent of a major recession between the initial and second WEMWBS data-collection sweeps by the Scottish Health Survey in 2008 and 2009, respectively—a rather severe recession in Scotland, still in full swing (Scottish Government 2012). We agree that data for WEMWBS should continue to be collected. But we also think that researchers should actively develop an alternative population measure of mental health and well-being that might be more sensitive to both SES and policy-related change, in the future.

Fourth, McCartney and colleagues acknowledge our point that low birth weight is no longer a useful measure of early life population health in developed countries, because it conflates widespread but opposite trends in its two component elements: prematurity and intrauterine growth retardation (IUGR).

We are grateful for this acknowledgment and concur that the internationally recognized solution to this problem—the routine collection and analysis of accurately (ultrasound) assessed gestational age in populations, via its incorporation in “pure prematurity” indices and “pure IUGR” indices—should be implemented in Scotland as soon as is practicable.

Fifth, McCartney and colleagues expressed considerable reservations about the use of spline-based tests of nonlinearity in socioeconomic gradients in health outcomes before summary indices like SII and RII are used. In response, we sympathize with these reservations. We still, however, think there is something “fishy” about the anomalously non-linear (even by eye!) gradients in the two alcohol-related outcomes

(hospitalization and mortality) reported annually in Scotland because of a clear excess in the lowest SES decile of the population and a corresponding dearth of events in the middle deciles. These are the only outcomes, out of the eleven analyzed in the annual Scottish reports, to show this nonlinear pattern so clearly. We posit “reverse causality” as a possible explanation, but agree that additional research that monitors changes in address postal codes, over time in subjects with relatively early alcoholism-related morbidity markers, is critical to confirming or refuting our hypothesis. We freely acknowledge that downward social mobility, after illness sets in, has rarely been shown to drive *overall* population-level health inequalities by SES. But we also note that downward social mobility resulting from illness is widely accepted to be the norm for chronic major *mental* health problems in adulthood (such as schizophrenia); so we wonder why alcohol-related health outcomes would behave any differently.

Sixth, McCartney and colleagues agree with our concern about the biological heterogeneity of the diverse cancer outcomes lumped together for two of the eleven analyses in the Scottish inequality reports, but they express concern about the power of more detailed site-specific analyses. (Each of the mortality and incidence rates of “all cancers” combines all cases or deaths from all [nonskin] anatomic sites into one outcome.)

We appreciate their agreement with our concern and accept McCartney and colleagues’ realistic caveat about the available statistical power for site-specific analyses of cancer outcomes in the Scottish population, which numbers only about 5 million. However, we would prefer to see commonly used smoothing techniques, such as rolling multiple-year averages for rate trends, applied to data from Scotland and other relatively small populations, rather than further reinforce the biological fallacy that all cancers are similar in their causes and prevention. Adding together cancers that are associated with high SES (such as melanoma) and those associated with low SES (such as lung or cervical cancer) surely cannot be good epidemiological practice in any analysis of overall disease and death patterns by SES.

In sum, we appreciate McCartney and colleagues’ many excellent points. But their comments have not changed our minds about either the usefulness of the generic “critical appraisal criteria” that our article offers for use in assessing such health inequalities analyses internationally, or the specific ways in which the Scottish reports—which we still firmly believe are the best in the world—could still be improved.

Finally, we note that the publication in October 2012 of the latest annual report on Scottish health indicators (Scottish Government 2012) adds another year of data to most of the eleven health outcomes analyzed annually for the last dozen or so years. But, notably, it does not materially change the longer-term time trends on which we commented in our December 2011 article, which was based on the four previous annual reports. Indeed, nearly all the absolute rich-poor gaps depicted in the new report's graphs show either no biologically significant decrement over the dozen or so years analyzed, or else a recent increase in that gap as seen, for example, in "arrived alive" hospitalizations for "heart attack" in those under age seventy-five. (Ironically, as our December 2011 article points out, this could, perversely, be due to *improved* survival from acute myocardial ischemia syndrome before arrival at hospital, in lower-SES Scottish patients—hardly bad news.) A major exception to the recent continuation of previously unpromising trends in Scottish health inequalities by SES appears to be low birth weight, for which prevalence rates at birth do appear to have come down significantly since about 2006. Other evidence, compiled in a recent article (Mackay et al. 2012), co-written by one of us (Sally Haw), strongly suggests that this may well be due to an unanticipated but very welcome effect of Scottish smoke-free legislation in March 2006 in materially reducing the rate of premature delivery. We can only hope that other such population-wide mandated and "upstream" interventions are implemented in the future to further reduce the relatively steep health inequalities by SES (Popham and Boyle 2010) from which Scotland continues to suffer.

So, in answer to the question posed in our title (above), we are unable to say—on the basis of the health status evidence we have examined—whether the relatively static picture of health inequalities by SES in Scotland is largely due to inadequate policies aimed at reducing underlying social and economic inequalities; we welcome economic and policy experts' analyses of that question. What we can say is that the particular suite of outcomes analyzed in the annual SG reports on health inequalities could be improved upon, as judged by the "best practice" criteria put forward in our article. Surely we should use the most sensitive and robust outcomes possible in such monitoring reports, if we are to draw from them correct inferences about current changes in the distribution, across social and economic groups, of adverse health outcomes. Indeed, at a time when most health inequalities—as traditionally measured—appear to be "persistent" in many developing countries, surely public

health should critically examine precisely how it is measuring them, and improve those practices to the greatest extent possible.

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