Commentary

Income Inequality as a Public Health Concern: Where Do We Stand? Commentary on “Is Exposure to Income Inequality a Public Health Concern?”

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THE INCOME INEQUALITY/HEALTH LINK: A DISAPPEARING CONNECTION?

Research interest on the link between income distribution and population health can be traced back to Richard Wilkinson’s seminal paper published in 1992 in the *British Medical Journal*, showing a correlation between income inequality and life expectancy among nine industrialized countries (Wilkinson 1992). Ten years on, despite dozens of papers published on this topic, the issue continues to be debated. Is income inequality a public health concern? A growing number of studies argue that it is not. A series of papers published in the January 2002 issue of the *British Medical Journal* (Muller 2002; Osler et al. 2002; Shibuya, Hashimoto, and Yano 2002; Sturm and Gresenz 2002) prompted an editorial that declared that the evidence for the income inequality/health link was “slowly dissipating” (Mackenbach 2002). In this issue of the Journal, Mellor and Milyo provide two additional tests of the empirical link between income distribution and health, and find little support for a robust association (Mellor and Milyo 2002). Is it time then for researchers to pack their bags and go home, reassured now that there is no threat to public health from the widening gulf between the haves and have-nots in America, and in the rest of the world?

Such a conclusion, we argue, would be both hasty and premature. To date, the debate on the income inequality/health link has been carried out almost entirely on the merits of empirical data analyzed by different investigators. Like any debate that hinges on the analyses of empirical data,

this one warrants a close look at questions such as how the researchers defined income inequality (e.g., at what level of geographical aggregation?), what variables they controlled for, and how they analyzed the inherently multilevel nature of the research question. The devil, as they say, is always in the details.

The following commentary is in two parts. In the first section, we provide a commentary on the paper by Mellor and Milyo, and demonstrate how different investigators can come to quite different conclusions even when they analyze *the same data*. In the second part of the commentary, we summarize the multilevel studies—both positive and negative—that have addressed the income inequality/health connection, and suggest ways in which future investigations might shed light on this issue.

A CRITIQUE OF MELLOR AND MILYO

How might income inequality be related to health outcomes? Possible mechanisms include inadequate spending on social goods (such as public education and health care) when the social distance widens between the poor and the rest of society (Kawachi and Kennedy 1999). Income inequality has also been hypothesized to lead to the erosion of social cohesion, which in turn creates a political climate that is less supportive of policies that maintain the public health (Kawachi and Berkman 2000). Lastly, there may be possible direct psychosocial and physiological consequences of invidious social comparisons engendered by income disparities (Kawachi, Kennedy, and Wilkinson 1999). As we have previously argued (Blakely et al. 2000), these mechanisms are unlikely to occur instantaneously—there should be a lag time during which income inequality works through these intermediary pathways, eventually affecting health. We have previously investigated the possibility of a time-lagged association of income inequality and self-rated health using Current Population Survey (CPS) data, concluding that “*Although not conclusive*, these data suggest that income inequality up to 15 years previously *may* be more strongly associated with self-rated health than income inequality measured contemporaneously, for individuals aged 45 years and older at least” (emphasis added) (Blakely et al. 2000).

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Mellor and Milyo are to be commended for tackling the issue of lag-times in their article in this issue of the Journal. However, their empirical analyses also underscore the numerous methodological challenges involved in attempting to detect lag-times that we had previously pointed out (Blakely et al. 2000). First, there is the issue of misclassification bias caused over time by the movement of individuals between states if their residential history is not known. Second, a favorable signal-to-noise ratio is needed to distinguish time lags for contextual variables. In the case of income inequality, this means that there must be both substantial change of income inequality over time *and across states*—that is, there must be meaningful variation over time in the ranking of the states by income inequality. If all fifty U.S. states experienced surges in income inequality over 20 years, but the *relative ranking* of states by income inequality remained unaffected, there simply would not be variation in the exposure of interest to discern time lags (the exception to this argument would be if there was a clear threshold effect of income inequality on health as opposed to a dose-response effect). As we cautioned in our earlier lag-time paper: “Data sets or natural experiments must be sought out where there is sufficient variation in the distribution of income inequality by unit of observation over time—the U.S. states as units of observation may be too limited in this regard” (Blakely et al. 2000).

Mellor and Milyo interpret their analyses as not supporting a time-lagged (or any) association of state-level income inequality with health in the United States. They present both “multilevel” analyses for self-rated health and ecological times series analyses for mortality data. The multilevel analyses use the CPS data for the years 1995 to 1999—essentially the same set of data that we previously analyzed (Blakely et al. 2000). However, Mellor and Milyo have added two steps to the analyses of CPS data that cause us concern. First, they control for health insurance and education status at the individual level, which we have previously argued is a potential overadjustment of the individual model, since both variables plausibly lie on the pathway between income inequality and health outcomes (Blakely and Kawachi 2002; Blakely, Lochner, and Kawachi 2002).

Second, Mellor and Milyo introduce fixed-effects (or dummy variables) for the nine census divisions, to address the issue of possible regional-level confounding of the state-level association between income inequality and self-rated health. However, including such a fixed-effect for groupings of states is problematic since it treats each division as a separate and independent observation. This would have been appropriate provided the authors had an explicit interest in making inferences about specific divisions. This does not

seem to have been the case since the authors did not report the fixed-effect sizes of the different divisions. An alternative, and arguably superior, strategy to implement the theoretical concern related to the presumed clustering of health outcomes within divisions (and between states) is to specify a *random parameter at the divisional level*, that estimates the between-division variations after taking into account the “between-states within-division” and “within-state between-individual” clustering. Stated differently, a more appropriate analysis—given the authors’ concerns—would have been to test a three-level model with variance being partitioned simultaneously at the individual, state, and divisional levels.

Since, we happened to have the CPS data at hand, we were able to test the three-level model. The characteristics of this sample are described in detail elsewhere (Blakely et al. 2000). In brief, we modeled 90,006 CPS respondents (aged 45 years and older) for years 1995 and 1997. The individual-level covariates were sex, race (black, white, other), four 10-year age groups, equivalized household income (9-level categorical variable), and state-level median household income. The state-level Gini coefficient of household income is the exposure of interest. We would have preferred to use state-level *mean* income (as opposed to median income) to avoid overcontrol of the Gini (Blakely and Kawachi 2001), however the data were not readily available to us for our reanalyses. Previously, we used the SAS macro GLIMMIX to allow for random effects at the state level (Blakely et al. 2000). For the reanalyses, we fitted models using the *MLwiN*—a software specifically designed to fit complex random effects statistical models (Rasbash et al. 2000). Since the response variable has two possible outcomes (one if fair/poor health, zero otherwise) binary logistic multilevel models were used with a logit-link function (Goldstein 1995). We fitted the models using the improved Predictive Quasi Likelihood (PQL) second-order approximation procedures (Goldstein and Rasbash 1996).

We present the effect of Gini measured at 1979–1981; 1983–1985; 1987–1989; 1991–1993; and 1995–1997 on the poor self-rated health assessed in 1995 and 1997. The effects of state level income inequality (the Gini) are estimated after allowing for the fixed individual effects of age, sex, race, and income, as well as the fixed state effects of median income. We estimate the effects of Gini using four distinct modeling strategies and the results are presented in Table 1.

Model 1 results is based on a modeling strategy whereby the clustering of individuals within states (and states within divisions) is completely ignored. Model 2 results presents the effects of Gini after allowing for the clustering at

Table 1: The Association of State-Level Income Inequality (Measured at Five Different Points in Time Using CPS Data) with Fair/Poor Self-rated Health for Alternative Model Specifications. Shown in the Table are: (1) The Odds Ratios (95% Confidence Intervals) of Fair/Poor Health among 90,006 CPS Respondents in 1995 and 1997, Aged 45 Years and Older, for a 0.05 Increase in the State-Level Gini; (2) Variance Components (Standard Errors in Parentheses) for Alternative Model Specifications.

<i>Fixed Effects</i> <i>State-Level Variance</i> <i>Divisional-Level Variance</i>	<i>Model 1</i>		<i>Model 2</i>		<i>Model 3</i>		<i>Model 4</i>	
	<i>Baseline^a</i> <i>No</i>	<i>No</i>	<i>Baseline</i> <i>Yes</i>	<i>No</i>	<i>Baseline, Divisional Dummies</i> <i>Yes</i>	<i>No</i>	<i>Baseline</i> <i>Yes</i>	<i>Yes</i>
<i>Year Gini Measured</i>	<i>OR (95% CI) per 0.05 increase Gini</i>							
1979–1981	1.19	(1.13–1.26)	1.35	(1.14–1.59)	1.10	(0.94–1.27)	1.18	(1.01–1.37)
1983–1985	1.23	(1.17–1.28)	1.32	(1.13–1.53)	1.07	(0.93–1.23)	1.15	(0.99–1.33)
1987–1989	1.20	(1.15–1.25)	1.26	(1.10–1.44)	1.02	(0.90–1.16)	1.09	(0.96–1.24)
1991–1993	1.18	(1.13–1.23)	1.29	(1.13–1.48)	1.08	(0.96–1.21)	1.14	(1.01–1.29)
1995–1997	1.09	(1.06–1.13)	1.18	(1.04–1.34)	1.04	(0.93–1.15)	1.06	(0.95–1.18)
<i>Year Gini Measured</i>	<i>Variance components</i>							
	<i>State</i>	<i>Division</i>	<i>State</i>	<i>Division</i>	<i>State</i>	<i>Division</i>	<i>State</i>	<i>Division</i>
1979–1981		0.036		0.015		0.020		0.020
		(0.008)		(0.004)		(0.005)		(0.012)
1983–1985		0.033		0.014		0.019		0.019
		(0.008)		(0.004)		(0.005)		(0.011)
1987–1989		0.033		0.014		0.019		0.018
		(0.008)		(0.004)		(0.005)		(0.011)
1991–1993		0.032		0.014		0.019		0.016
		(0.007)		(0.004)		(0.005)		(0.010)
1995–1997		0.037		0.014		0.019		0.024
		(0.008)		(0.004)		(0.005)		(0.014)

^aThe “baseline” model includes dummy variables for sex, race, four 10-year age groups, equivalized household income (nine categories), and continuous variables for state-level median income and Gini coefficient.

the state level, but ignoring the clustering of states by divisions (i.e., Model 2 is a *two-level* variance component model with individuals at level 1 nested within states at level 2). Technically, this two-level model is superior to Model 1 for two reasons. First, it allows for the within-state clustering of individuals and accordingly adjusts the standard errors of the individual fixed point estimates. Second, since the model is specified at the micro and macro level (in this case, individual and state, respectively), it is only through this multilevel model that a higher level covariate (e.g., Gini) can be correctly specified, and as such as this variable *cannot* vary between individuals within a state. This appropriately widens the confidence interval around the Gini odds ratio (given that we have an *N* of 50, and *not* 90,006, for the Gini), thereby yielding a robust estimate of the effect size. However, the effect sizes of the higher-level covariate (e.g., state-level Gini) may also be misestimated in models that do not explicitly model the distinct sources of variation (Jones and Duncan 1998). The increase in the odds ratios for state-level inequality between Models 1 and 2 suggests that our previous analyses (and those of Mellor and Milyo) underestimated the Gini effect.

Model 3 results seeks to partially replicate the analytical approach adopted by Mellor and Milyo, that is, by accounting for the individual clustering within states and introducing divisional dummies (one each for every census division) in the *fixed* part of our model. We say “partial” replication, since it was not clear whether Mellor and Milyo implemented a “variance component model” or a “marginal model” whereby the standard errors of the fixed-part estimates are simply adjusted for the clustering of individuals within states. Specifying a variance-components model, as we have done here, allows us to correctly specify the higher-level covariates, and to partition the amount of variability that can be attributed to the state level (Subramanian, Jones, and Duncan in press). As Mellor and Milyo found, adding the divisional dummies results in a large reduction of the state income inequality (Gini) effect (Model 3, Table 1). As an aside, we note that only four out of the eight differentials for the divisional dummies were significant, suggesting overspecification of the statistical model.

However, the approach adopted by Mellor and Milyo is only one—and possibly the most problematic—way of addressing the potential confounding by regional factors of the association between state income inequality and health. First, modeling census divisions as a fixed-effect unrealistically assumes that each division is a separate and independent entity. Second, even if our interest is in making separate and independent inferences about the different divisions (which does not seem to have been the motivation

behind Mellor and Milyo's analysis), such an approach may not be reliable if some divisions have a small number of states and individuals within them.

An alternative, and arguably superior, analytic strategy is to treat the divisions as coming from a distribution that can be summarized with a mean and variance. Such a strategy is both parsimonious and appropriate if our interest is primarily in accounting for the clustering of the states, as opposed to estimating division effects *per se*. (Having said this, it is still possible to estimate division-specific predictions, based on "borrowing strength" of all the divisions, and as such these estimates are "precision-weighted" [Jones and Bullen 1994]). In the final column of Table 1 (Model 4), we present the results of a *three*-level model, in which we have individuals at level 1, nested within states at level 2, and nested within divisions at level 3, with a variance component being estimated at the state and division level. In this model, there is a modest association between state Gini and poor self-rated health, with some of the confidence intervals excluding 1.0, and a pattern suggesting time lags of up to 15 years.

Importantly, our demonstration highlights the different results that can be obtained by adopting different modeling strategies *on the same dataset*. It is therefore imperative that investigators in this area should provide a clear rationale for the choice of "multilevel" models to test the links between income inequality and health, a point that is missed in most existing studies on income inequality and health.

If census divisions (and divisional characteristics) are indeed a significant confounder of the association between state-level income inequality and health, then the three-level modeling strategy that we have presented is most appropriate and parsimonious. Such a strategy still suggests an association between state income inequality and poor self-rated health, even after controlling for (a) the individual effects of age, sex, race, and income; (b) the state effects of median income; and (c) the within-division clustering of states. Moreover, the association appears to be the strongest for a time lag of 15 years, weakening thereafter.

The analyses we present here are largely illustrative and are not comprehensive. For example, we have not explicitly modeled the potential state-level confounds of the association of income inequality with health. This is empirically challenging because of collinearity between state-level variables, and the difficulty in identifying pure confounders as opposed to variables that are likely to be both confounds *and* pathway (or a proxy for pathway) variables between income inequality and health outcomes.

One alternative form of empirical analysis is the mixed ecological study design (Morgenstern 1998) where one looks for the association of changing income inequality with changing health status over time at the state level. The second part of Mellor and Milyo's study attempts this analysis. The advantage of this study design is that by looking at changes over time within states, it efficiently cancels out the potential confounding of unobserved and fixed attributes of the fifty U.S. states. However, there are several reasons to be concerned about the validity of Mellor and Milyo's ecological study.

First, the mortality rates used in the analysis are crude rates—and not age-standardized rates as is standard practice in epidemiology. The authors note that it is technically incorrect in ecological *inference* to have standardized variables on one side of the equation, but not on the other (Rosenbaum and Rubin 1984). However, as we have previously pointed out to the authors (Kawachi and Blakely 2002), their argument applies to situations in which the researcher tries to draw inferences about *individual*-level associations from grouped data (for example, using national income and the national injury rate to infer the individual-level association of personal income to personal injury risk). In analyses of income inequality, however, the variable on the right hand side of the equation is an inherently *contextual* or ecological variable that has meaning in its own right. Given the decline over time in mortality rates, and the aging of the population, we have argued previously that age-standardized mortality rates are preferable to crude death rates (Kawachi and Blakely 2001).

Secondly, while adding a time dimension to an ecological study design is a good idea in theory, it is also very demanding of the data and (in all likelihood) underpowered. Given the secular trend of decreasing mortality rates over time, there must be substantial change of income inequality over time that varies by states in order to observe an association in a mixed ecological study design. Attempting to identify the correct time lag is even more demanding of the data.

Finally, including a state-level dummy (as a means of controlling for state-level confounding) in an ecological state-level analysis is severely problematic from a power perspective. Moreover, if there is indeed clustering of states (as seems to be motivation in Mellor and Milyo's analyses with fixed effects of census divisions in the first part of their paper), then specifying fifty separate state dummies is conceptually contradictory and inconsistent with their CPS analysis with fixed-divisional dummies. Unfortunately again, Mellor and Milyo do not report or discuss the fixed state-effects.

MULTILEVEL EVIDENCE ON INCOME INEQUALITY AND HEALTH: SUMMARY AND REDIRECTION

In the second part of this commentary, we turn our attention to the published studies addressing the link between income inequality and health. Tables 2a and 2b present, respectively, the studies that either support such a link, or found no evidence of an association. In order to address the potential confounding of income inequality by individual income, studies must collect information on both income at the individual level, and income distribution at the aggregate level, that is, they must be multilevel (Wagstaff and van Doorslaer 2000). The studies summarized on Tables 2a and 2b meet this criterion. As is evident from the tables, there are somewhat more negative studies than there are positive studies. What sense, then, can we make of the accumulated data?

We draw the reader's attention to three emerging trends in the published data. First, studies supporting a link between income inequality and health outcomes have (so far) been exclusively carried out within the United States (Table 2a). In contrast, more than half of the null studies have been carried out in societies that are *more egalitarian* than the United States, and moreover have welfare state protections that are more far-reaching than in this country—for example, Japan, Sweden, Denmark, New Zealand, and even the U.K. (Table 2b). Second, it is noteworthy that the studies with positive findings (Table 2a) generally tended to have larger sample sizes, especially comparing the positive and negative studies carried out on data within the United States (Table 2b). Third, if one scans down the column labeled “unit of aggregation,” the reader will immediately spot that the studies with positive findings for income inequality have all conceptualized income inequality as a U.S. state-level covariate (with the sole exception of Soobader and LeClere 1999). By contrast, the majority of studies with null results have been carried out at units of aggregation that are smaller than the U.S. states—for example, municipalities in Sweden (Gerdtham and Johannesson 2001), parishes within a single city (Osler et al. 2002), regions within New Zealand (Blakely, O'Dea, and Atkinson 2002), constituency and regions in the U.K. (Jones, Duncan, and Twigg 2001), or U.S. counties (Fiscella and Franks 1997) and metropolitan areas (Blakely, Lochner, and Kawachi 2002; Sturm and Gresenz 2002).

What lessons can be drawn from the studies in Tables 2a and 2b? First, with respect to sample size, studies need to be sufficiently powered to find an effect of income inequality on individual health outcomes. In other words, there must be a sufficient number of individuals within a sufficient number of

Table 2a: Published Multilevel Studies That Support an Association between Income Inequality and Health Outcomes

<i>Authors</i>	<i>Setting</i>	<i>Sample Size</i>	<i>Unit of Aggregation</i>	<i>Health Outcome</i>
Kennedy et al. 1998	Behavioral Risk Factor Surveillance System (1993, 1994)	205,245 adults	U.S. states	Self-rated health
Blakely et al. 2000; Blakely, Kennedy, and Kawachi 2001	Current Population Survey (1995, 1997)	279,066 adults	U.S. states	Self-rated health
Wolfson et al. 1999	National Longitudinal Mortality Study (1990)	7.6 million person-years	U.S. states	Mortality
Soobader and LeClere 1999	National Health Interview Survey (1989–1991)	9,637 white males	U.S. counties	Self-rated health
Diez-Roux, Link, and Northridge 2000	Behavioral Risk Factor Surveillance System (1990)	81,557 adults	U.S. states	Hypertension, smoking, sedentarism, body mass index
Kahn et al. 2000	National Maternal Infant Health Survey (1991)	8,285 women	U.S. states	Depressive symptoms, self-rated health
Lochner et al. 2001	National Health Interview Survey–National Death Index linked study (1987–1995)	546,888 adults	U.S. states	Mortality
Subramanian et al. 2002	2000 National Socioeconomic Characterization Survey, Chile	101,374 adults	Chilean communities	Self-rated health

Table 2b: Published Multilevel Studies That Show No Association between Income Inequality and Health Outcomes

<i>Authors</i>	<i>Setting</i>	<i>Sample size</i>	<i>Unit of Aggregation</i>	<i>Health Outcomes</i>
Fiscella and Franks 1997	National Health and Nutrition Examination Study (1971–1975)	14,407 adults	U.S. counties	Mortality
Daly et al. 1998	Panel Study of Income Dynamics (1980, 1990 cohorts)	Not stated (about 6,500 adults)	U.S. states	Mortality
Gerdtham and Johannesson 2001	Swedish Survey of Living Conditions	40,000+ adults	Municipalities in Sweden	Mortality
Blakely, Lochner, and Kawachi 2002	Current Population Survey (1995, 1997)	185,47 children and adults	U.S. metropolitan areas	Self-rated health
Osler et al. 2002	Two cohort studies in Copenhagen, Denmark (1964–1992, 1976–1994)	25,728 adults	Parishes within Copenhagen city	Mortality
Shibuya, Hashimoto, and Yano 2002	Japanese Survey of Living Conditions of People on Health and Welfare (1995)	80,899 adults	Japanese prefectures	Self-rated health
S Sturm and Gresenz, 2002	Healthcare for Communities telephone survey (1997–1998)	8,235	U.S. metropolitan areas	Self-reports of 17 common conditions (e.g., arthritis, depression)
Jones, Duncan, and Twigg 2001	UK Health and Lifestyle Survey (1997)	8,720 individuals	U.K. constituency and regions	Mortality
Blakely et al. In press.	New Zealand Census-Mortality Study	1,391,118 adults, 3 years follow-up	Regions within New Zealand (3 alternatives, $n = 14$, $n = 35$, $n = 73$)	All-cause and cause-specific mortality

areas to make the multilevel analysis meaningful. Second, the studies show a more consistent effect of income inequality at larger units of aggregation (states) than at smaller units (such as wards and parishes), although this pattern is largely driven by the U.S. state-level analyses. Nevertheless, this pattern provides us with a clue that the mechanisms underlying the observed association between income inequality and health likely involve political decisions at the state level regarding patterns of social spending that affect health (Kawachi and Kennedy 1999). Lastly, the observation that all of the positive studies so far have been carried out in the United States may suggest some threshold effect of income inequality on health outcomes. Furthermore, most studies on income inequality and health have also been less attentive to the cross-level interactions whereby state income inequality may affect the health of different population groups in different ways (Subramanian, Kawachi, and Kennedy 2001).

We are rapidly approaching the point in the study of state-level income inequality and health in the United States where we must acknowledge the limits of empirical analysis on what, essentially, is just one natural experiment of sample size 50. It may be that the same source of confounding at the state-level in the United States is giving rise to spurious associations of state-level income inequality and health in studies on this one natural experiment (Blakely and Woodward 2000). Therefore, an important alternative strategy is to search for new evidence on the association of income inequality with health in other “natural experiments” elsewhere in the world—particularly in parts of the world that are even more unequal than the United States. A recent analysis of a nationally representative multilevel data from Chile—a country in a region of the world with much higher levels of income inequality than in North America—suggests a strong effect of community income inequality on self-rated health (Subramanian et al. 2002).

In summary, the Gini is well out of the bottle as far as the income inequality hypothesis is concerned. A decade after Wilkinson’s seminal paper, the topic continues to be relevant, and the debate, lively. If the paper by Mellor and Milyo in this issue is any indication, we see no reason why researchers shouldn’t continue to search for different natural experiments, appropriate methodological strategies, and better data.

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