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Income inequality and mortality in U.S. cities: Weighing the evidence. A response to Ash

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ABSTRACT

Deaton and Lubotsky (2003) found that the robust positive relationship across American cities between mortality and income inequality became small, insignificant, and/or non-robust once they controlled for the fraction of each city's population that is black. Ash and Robinson (Ash, M., & Robinson D. Inequality, race, and mortality in US cities: a political and econometric review. *Social Science and Medicine*, 2009) consider alternative weighting schemes and show that in *one* of our specifications, in *one* data period, and with *one* of their alternative weighting schemes, income inequality is estimated to be a risk factor. All of our other specifications, as well as their own preferred specification, replicate our original result, which is supported by the weight of the evidence. Conditional on fraction black, there is no evidence for an effect of income inequality on mortality.

Main text

Deaton and Lubotsky (2003) investigated the relationship between age-adjusted mortality and income inequality over American cities and states. Mortality rates for metropolitan statistical areas and for states were taken from vital registration data, and were matched to data on income inequality constructed between the 1980 and 1990 censuses. Like many previous writers, we found a robust positive relationship between mortality and income inequality across both states and cities. However, income inequality across places is strongly positively related to the fraction of the population that is black. White incomes are higher and black incomes are lower in places where there is a high fraction of blacks, and this between-race difference induces a strong positive correlation between income inequality and fraction black. Our regressions showed that, once the fraction black was controlled, income inequality as measured by the gini coefficient was no longer a risk factor for mortality. We regard this result as showing that there is no direct effect of *income* inequality on health. Given that we do not know why the racial composition of states and cities is so strongly correlated with mortality, we clearly cannot rule out an effect of *some kind* of inequality on mortality. We believe that this distinction is clear in our paper. Ash and Robinson exaggerate the difference between us when they say that our paper by "posing racial composition as an *alternative* to inequality misses the social and political meaning of race" (Ash and Robinson, 2009). Our paper shows only that, conditional on race, *income* inequality is not important, and we explicitly discuss other inequalities that are associated with race, and that might explain the association with mortality.

Ash and Robinson (2009) consider alternative measures of income and income inequality, using the U.S. Census Summary Tape File (STF) rather than the PUMA data. The former contain more observations—though *not* the whole census—but our sample sizes are more than adequate to estimate city-level gini coefficients. The STF files are simpler to use, and avoid the extensive coding that we undertook, but are otherwise an inferior source of gini's for two reasons: (a) the grouped data cause underestimation of the gini, because grouped data can be regarded as a mean preserving reduction in spread, effectively ignoring within-group inequality, and (b) because inequality of equivalent or even per capita incomes cannot be calculated. On these grounds, we believe that our measures are to be preferred. Even so, the two sources give rather similar results, and this is not the main focus of Ash and Robinson's commentary.

Ash and Robinson's main concern is the weights that we use in our regression, where they claim that we make a coding error. This is not correct. Our regressions were run exactly as we claim, though it is certainly possible to challenge our choice. We are running state- or city-level regressions of an average outcome y_t (here

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a mortality rate) on a vector of average explanatory variables x_i including income inequality and fraction black. The regression coefficients that we compute can be written as:

$$\hat{\beta} = \left(\sum_i w_i x_i x_i^T\right)^{-1} \left(\sum_i w_i x_i y_i\right) \quad (1)$$

where a superscript T denotes the transposition of a column vector into a row vector, and the scalars w_i are the weights that are in dispute. There are two cases that need to be handled separately, but which are often run together in Ash and Robinson's commentary. The first case—homogeneity—is where the model is correct and the effects of the x's on the y's are the same in all places. The second case—heterogeneity—is where the effects are different from one city to another. In the homogeneous case, the expectations of the parameter estimates are independent of the weights, so the choice of weights is entirely a matter of efficiency, of getting maximum precision. In the heterogeneous case, different weights will give different results, because different places are different. However, in this case, the model is mis-specified in some way that merits further investigation. If the results are sensitive to the choice of weights—as is the case here, in at least one specification—the model is wrong so that it is not legitimate to pick any of the results without further investigation.

If the model is homogenous across cities, and we are using averaged data, the optimal weights are proportional to the factors n_i/σ_i^2 where n_i is the population in city (say) i, and σ_i^2 is the within-city variance. If the within-city variance is the same in all cities, the optimal weight is proportional to city population. As stated in our paper, we used the square root of city size which would be efficient if the within-city variance were proportional to the square root of city size; this makes sense if within-city heterogeneity grows with city size but not as rapidly. Ash and Robinson also consider identical weights for all cities, $w_i = 1$, or even $w_i = n^2$. If the specification is correct, none of this should matter much for the parameter estimates, and only the standard errors will change, and these are not the main issue. Instead, Ash and Robinson show regressions for white males in 1990 in which the estimated effect of the gini on mortality is sensitive to the choice of weights. In the most extreme case, with $w_i = n_i^2$, which they refer to as weighting by absolute population size, they find a large and significant effect of income inequality on mortality. They also show cases, including one with no weighting—which they argue for in the text—where income inequality is actually estimated to be protective.

How do we decide between these opposing results? Clearly, this model is not homogeneous and the heterogeneity has something to do with city size. In this case, there is no correct weighting scheme, because the model is wrong in some way, and it is not legitimate to pick one of the results and claim it is the correct one. It is doubly incorrect to claim that we made a coding error, and that when the coding error was corrected, income inequality is seen to be a health hazard. Our coding is exactly as described in our paper, and when

Table 1
(a): Sensitivity of 1990 MSA results to different weighting schemes (a): White men only and (b): White women only

(a): White men only	Weight given to each observation			
	1	Square root of population	Population	Population squared
Coefficient on Gini calculated over all people	−0.33 (1.95)	−0.09 (0.61)	0.20 (1.62)	0.49 (6.60)
Controls for city population?	No	No	No	No
Year	1990	1990	1990	1990
Unit of observation	MSAs	MSAs	MSAs	MSAs

(b): White women only	Weight given to each observation			
	1	Square root of population	Population	Population squared
Coefficient on Gini calculated over all people	−0.33 (2.00)	−0.22 (1.51)	−0.05 (0.40)	0.25 (3.11)
Controls for city population?	No	No	No	No
Year	1990	1990	1990	1990
Unit of observation	MSAs	MSAs	MSAs	MSAs

Note: each model has 287 observations. Each model also controls for mean log income per adult equivalent and the fraction black in the city. Each column corresponds to specifying a different weight in Stata. The second column, where the weight is the square root of city population, corresponds to our previously published version. Absolute *t*-values are in parentheses.

Table 2
Sensitivity of 1990 MSA results to controls for city size (a): White men only and (b): White women only

(a): White men only	Weight given to each observation			
	1	Square root of population	Population	Population squared
Coefficient on Gini calculated over all people	−0.47 (2.65)	−0.32 (1.91)	−0.11 (0.67)	0.32 (2.48)
Controls for city population?	Yes	Yes	Yes	Yes
Year	1990	1990	1990	1990
Unit of observation	MSAs	MSAs	MSAs	MSAs

(b): White women only	Weight given to each observation			
	1	Square root of population	Population	Population squared
Coefficient on Gini calculated over all people	−0.41 (2.34)	−0.38 (2.29)	−0.33 (2.18)	−0.28 (2.19)
Controls for city population?	Yes	Yes	Yes	Yes
Year	1990	1990	1990	1990
Unit of observation	MSAs	MSAs	MSAs	MSAs

Note: each model has 287 observations. Each model also controls for mean log income per adult equivalent and the fraction black in the city. Absolute *t*-values are in parentheses.

Table 3
1990 MSA Results by City Size (a): White men only and (b): White women only

(a): White men only	Quartile of city size			
	Smallest	2	3	Largest
Coefficient on Gini calculated over all people	−0.37 (0.88)	−0.78 (1.89)	−0.57 (1.71)	0.02 (0.08)
Controls for city population?	Yes	Yes	Yes	Yes
Weight?	None	None	None	None
Year	1990	1990	1990	1990
Unit of observation	MSAs	MSAs	MSAs	MSAs

(b): White women only	Quartile of city size			
	Smallest	2	3	Largest
Coefficient on Gini calculated over all people	−0.67 (1.51)	−0.64 (1.62)	−0.22 (0.69)	−0.44 (1.35)
Controls for city population?	Yes	Yes	Yes	Yes
Weight?	None	None	None	None
Year	1990	1990	1990	1990
Unit of observation	MSAs	MSAs	MSAs	MSAs

Note: each model has 71 or 72 observations. Each model also controls for mean log income per adult equivalent and the fraction black in the city. Observations are unweighted. Absolute *t*-values are in parentheses.

Table 4
Sensitivity of All Results to Choice of Weights (a): White men only and (b): White women only

(a): White men only

	Model											
	1	2	3	4	5	6	7	8	9	10	11	12
Coefficient on Gini calculated	−0.49	−0.24	−0.04	0.01	−0.22	−0.38	0.20	−0.09	−1.04	−1.15	−0.70	−0.90
over all people	(2.16)	(0.79)	(0.19)	(0.04)	(1.76)	(2.47)	(1.62)	(0.61)	(6.78)	(6.30)	(4.80)	(5.29)
Sample	All men	All men	White men	White men	All men	All men	White men	White men	All men	All men	White men	White men
Controls for city population?	No	No	No	No	No	No	No	No	No	No	No	No
Weight?	Population	Sq. root of population	Population	Sq. root of population	Population	Sq. root of population	Population	Sq. root of population	Population	Sq. root of population	Population	Sq. root of population
Year	1980 + 1990	1980 + 1990	1980 + 1990	1980 + 1990	1990	1990	1990	1990	1980	1980	1980	1980
Unit of observation	States	States	States	States	MSAs	MSAs	MSAs	MSAs	MSAs	MSAs	MSAs	MSAs
Table from original paper	New	Table 1	New	Table 1	New	Table 2	New	Table 2	New	Table 3	New	Table 3

(b): White women only

	Model											
	1	2	3	4	5	6	7	8	9	10	11	12
Coefficient on Gini calculated	−0.41	−0.36	−0.26	−0.30	−0.33	−0.42	−0.05	−0.22	−0.63	−0.91	−0.42	−0.75
over all people	(1.49)	(1.12)	(0.93)	(0.95)	(2.70)	(2.80)	(0.40)	(1.51)	(3.32)	(4.26)	(2.24)	(3.68)
Sample	All women	All women	White women	White women	All women	All women	White women	White women	All women	All women	White women	White women
Controls for city population?	No	No	No	No	No	No	No	No	No	No	No	No
Weight?	Population	Sq. root of population	Population	Sq. root of population	Population	Sq. root of population	Population	Sq. root of population	Population	Sq. root of population	Population	Sq. root of population
Year	1980 + 1990	1980 + 1990	1980 + 1990	1980 + 1990	1990	1990	1990	1990	1980	1980	1980	1980
Unit of observation	States	States	States	States	MSAs	MSAs	MSAs	MSAs	MSAs	MSAs	MSAs	MSAs
Table from original paper	New	Table 1	New	Table 1	New	Table 2	New	Table 2	New	Table 3	New	Table 3

Note: State-level regressions have 102 observations; MSA-level regressions have 287 observations. State-level regressions also include a indicator for 1990 data and controls for mean log income per adult equivalent and the fraction black. Absolute *t*-values are in parentheses.

the weights matter to this extent, there is no alternative but to go back to square one and find out what is going on.

In that spirit, we report two sets of calculations. We first investigated the role of city size. We repeated the 1990 city regressions with city size excluded from the models (Table 1), and included as a control variable (Table 2). We also varied the weights. With weights set to 1, to the square root of population, or to the level of population, the gini is estimated to be protective, but never significantly so. With weights set to the square of population, we get something like Ash and Robinson's result, and the gini is a significant risk factor. Clearly, the inclusion of city size does not remove the heterogeneity in the regression, and once again we have no grounds for preferring one or other of these estimates. In Table 3 we divide up the cities into four groups by population size and rerun the regression separately for each group. For all but the largest group, we again find insignificant protective effects of income inequality. In the largest group, the coefficient is 0.02 with a *t*-value of 0.08. There is little heterogeneity here, but these findings do not support the obvious interpretation of Ash and Robinson's result, that income inequality is more of a hazard in larger cities.

Our second set of calculations goes back to *all* of the regressions that we originally reported, all but one of which were either not investigated or not reported by Ash and Robinson. We ran regressions for whites or for everyone, for 1980, for 1990, both at the city level, or for 1980 and 1990 combined at the state level. Doing this separately by sex gives 24 regressions, which we examined for sensitivity to weighting by running each with weighting by the square root of population—as in our original paper—and with weighting by the level of population, as Ash and Robinson suggest. These results are presented in Table 4. In only one of the 12 paired regressions—the one that they report—do we get Ash and Robinson's result, that an insignificant protective effect of the gini with square root weighting turns into an almost significant ($t = 1.62$) hazardous effect under population weighting. There really is very little heterogeneity in these regressions; in all but one of the 24 specifications, the gini is either insignificant or perverse or both. All but one of our results is unaffected by reweighting and, as we have argued above, the lack of robustness cannot be taken as evidence for an effect of income inequality on mortality.

We conclude with a few general observations on the relationship between inequality and mortality. Since our paper was published, there have been two major surveys of the extensive literature on income inequality and health (Deaton, 2003; Lynch, Smith, Harper, & Hillemeiera, 2004; Lynch, Smith, Harper, Hillemeier, Ross et al., 2004). Lynch, Smith, Harper, Hillemeier, Ross et al. (2004), several of whom had previously espoused the income inequality and health hypothesis, review 98 studies and conclude that "there seems to be little support for the idea that income inequality is a major, generalizable determinant of population health differences within or between rich countries." Deaton (2003) comes to similar conclusions, but is entirely supportive of the idea that the effect of racial composition on mortality reflects an inequality of some kind, though not an income inequality. In our original paper, we discussed several of these—provision of public goods, segregation, quality of health services, inequality of political representation—but we failed to find convincing evidence for any of them. We do not find it helpful—nor do we even know what it means—to conclude that "racial composition *is* inequality." If inequality refers to income inequality, then there is indeed a correlation, but it is far from unity. We are more sympathetic to the idea that racial politics are important, again see Deaton (2003). In recent years, work by Bach, Pham, Schrag, Tate, and Hargraves (2004), as well as by the Dartmouth group (Skinner, Chandra, Staiger, Lee, & McClellan, 2005) has shown that America is running something of an *apartheid* healthcare system, in which most blacks are treated in hospitals or by primary care physicians that treat few or no white patients, and where most whites are treated in hospitals or by primary care physicians that treat few or no black patients. Bach et al. show that the physicians that primarily serve blacks—who may or may not be black themselves—have fewer resources and are less well-qualified. Skinner et al. show that mortality rates after an MCI are higher for all patients in hospitals that treat mostly blacks. In line with this work, our leading hypothesis is that blacks receive worse healthcare than whites, and that this spills over into mortality among whites who live in cities with a large black population and who share, at least in part, their inferior healthcare. This is indeed an important inequality, but it is not an income inequality.

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