Relative Income, Race, and Mortality

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I. Introduction

The topic of this paper is the relationship between relative income and health. We examine whether people whose incomes are high relative to others who live in the same geographic area have lower or higher mortality. This analysis holds own income fixed, so the question is not only whether own income matters for health, but also whether the incomes of others affects health given own income. It is well-established that wealthier individuals are healthier and live longer (see Sorlie et.al. (1995) and Elo and Preston (1996), and the review in Adler et.al. (1994)). The proposition that *relative* income affects health is more controversial. Although there are reasons why there could be health benefits from having wealthier neighbors, a growing body of literature argues that low relative income is a health hazard. This proposition runs counter to the Pareto principle and, if correct, could have very unorthodox implications for economic policy.

We examine this topic through the lens of racial differences in mortality in the United States. Blacks in the United States have much higher mortality rates than whites. Figures from the National Center for Health Statistics (1998) indicate that in 1990 the life expectancy at birth for black males was 64.5 years, compared to a life expectancy at birth of 72.7 years for white males. Life expectancy for white females was 79.4 years, in contrast to 73.6 years for black females. Figure 1 uses data from the National Center for Health Statistic's Mortality Detail Files, and shows the log odds of mortality by years of age for black and white men and women, in 1980 and 1990. Black mortality is higher at all ages except in the late teen-age years and at the very oldest ages.[1] Black mortality is higher than white mortality for almost all causes of death, accidents and suicides being notable exceptions. There is a large body of research that seeks to explain the U.S. black-white mortality differential, and specifically how much of the differential can be accounted for by racial differences in socioeconomic status. Although some questions remain, much of this

[1] This “mortality cross-over”, where the oldest blacks have lower mortality rates than whites, has been extensively researched, and is very likely an artefact of great age misreporting for blacks in both Census data and death records (Preston, 1997).

evidence concludes that racial differences in socioeconomic status account for a large portion of racial mortality differences.[2]

The research in this paper exploits differences in mortality and income levels of blacks and whites within geographical areas to examine how relative income affects health. Our starting point is a model in which an individual's health is a function of his or her own income, and the incomes of those who live in the same geographical area. We show how this individual-level model can be estimated using semi-aggregated data on the mortality rates of people categorized by age, race, gender and place of residence. The analysis uses mortality data from the 1980 and 1990 Compressed Mortality Files (CMF), which have information on all-cause mortality and mortality by cause of death broken out by county, gender, racial group, and age group, merged with income data are from the 1980 and 1990 5% Public Use samples of the Census.

Our identification strategy is illustrated by the following example. Consider two different areas, County A and County B, and assume that the income levels of members of a specific race and gender group—for example, black men—are very similar in these two counties. Suppose also that the *per capita* income level in County A is higher than in County B. The question we ask is whether the mortality of black men is higher in County A or B. A finding that black men in County A have higher mortality suggests that low relative income is harmful to health. A finding that black men in County A have lower mortality suggests that there are positive spillovers from having relatively wealthy neighbors.

[2]Research surveyed by Smith and Kington (1997) and House and Williams (2000) indicates that racial differences in socioeconomic status, typically measured by income, education and sometimes occupation, account for between 60% to 100% of the black-white mortality difference in the United States. Racial differences in socioeconomic status appear to explain more of the black-white mortality difference at older ages than at younger ages (Sorlie et.al, 1992). House and Williams (2000) discuss often subtle racial differences in access to health care and the quality of medical treatment as possible explanations. (See also Multcher and Burr, 1991, and Escarce and Puffer, 1997, for discussions of racial differences in health care.)

Section II of the paper discusses the mechanisms through which relative income might influence health, and reviews existing literature on this topic. Section III presents an empirical model of mortality and relative income, and describes the data and methods used to estimate the model. One of our key concerns in this section is to show how models of the effects of relative income on the mortality of individuals can be estimated using semi-aggregated data. Section IV presents evidence of the effects of relative income on both all-cause mortality and mortality by cause of death. We find no evidence that there are benefits from having relatively wealthy neighbors, and some evidence that low relative income is associated with higher mortality. This evidence is strongest for younger (aged 25-64) black men. For this group, an increase in the income of others is estimated to have an (adverse) effect on mortality that is half as big as an equivalent decline in own income. We also show that the effects of relative income work through several, although not all, specific causes of death. In Section V, we present an extension of the model that allows for a more flexible relationship between mortality and the income of "others." This model is estimated using semi-parametric methods, and the results are consistent with those presented in Section IV.

II. Relative Income and Mortality

Why might relative income matter for mortality? One theory is that an individual's health is affected by his or her income—or, more broadly, social status—relative to others in the individual's "reference group." Low social status increases psychosocial stress, which may have adverse biological consequences and could also have detrimental effects on health-related behaviors (such as smoking). This idea has been the focus of a growing body of recent research. There is convincing research on humans and other animals that low social status compromises immune function and is a risk factor for disease (see, for example, Cohen et.al. 1991, Cohen et.al. 1996, Sapolsky, 1993, 1994). However, low social status is not synonymous with low relative income and these results, although intriguing, do not provide clear evidence that low relative income has adverse health consequences.

The model of psychosocial stress implies that low relative income is harmful to health. However, several alternative models imply that low relative income could be either beneficial or harmful. One such alternative is a model of pecuniary externalities, in which the availability and prices of locally-produced goods and services that influence health are affected by the income level of a region. Suppose, for example, that certain medical services in a region have upward-sloping supply curves. Increases in per capita income would cause the prices of these services to rise. In this case, holding own income fixed and assuming that medical care affects health, individuals will be harmed by having wealthier neighbors. However, these effects could just as easily work in the opposite direction. If health services have decreasing costs, increases in aggregate income could work to reduce prices or improve availability of health-related goods. Consider an example in which mortality is a decreasing function of a specific type of medical facility—for example, a neonatal intensive care unit—which plausibly has high fixed costs of operation. Provided aggregate demand for the facility's services increases with income, richer areas should be more likely to have a facility, and (barring price regulation) to charge less for the facility’s services. In this case, relatively poor people may benefit from having wealthier neighbors, since their presence increases the availability and reduces the price of a service that improves health.

One problem with this theory of pecuniary externalities is that it is difficult to identify specific goods through which these effects might operate. There is evidence that access to medical services does *not* have large effects on health (see, for example, Adler et.al.1994, and House and Williams, 1995). However, it is possible that pecuniary externalities work through other goods, such as housing or land quality. Similar but perhaps more plausible links between health and relative income may operate through the provision of public goods that affect health. For example, it is possible that higher average incomes result in reduced expenditure on publicly provided goods that improve the health of poorer people. Even in this case, though, an increase in income in a region (holding own income fixed) could be either harmful or beneficial to health. A higher concentration of wealthy people in an area could lead to more expenditure on public goods or a greater demand for environmental regulations that improve the health of everyone. Whether relative income affects mortality, and whether being relatively poor is detrimental or beneficial to health, is an open empirical question.

Two key issues must be addressed prior to testing the relative income model. The first concerns the definition of relative income. Any measure of relative income requires a “reference” income to which own income is compared. The different theories discussed above offer vague guidelines as to how this reference group should be defined. The model of pecuniary externalities suggests that the reference group should consist of individuals in the same market for health-related goods: possibly those in the local community around a hospital. The political economy story indicates that the reference group consists of individuals living in the politically-defined unit —a city, county, or state—in which decisions about relevant public goods are made. The psychosocial stress model provides even less direction, since the reference group is defined by the individual. Reference groups could in theory be defined by any level of geography (e.g. a neighborhood, a state, or even an entire country), or could instead by defined by membership in a common group (e.g. a religious or ethnic group) that cuts across regions.

Much of the existing work on relative income models, as well as similar work that examines the relationship between inequality and health, defines reference groups in terms of geography—either states, cities or counties. We follow this practice here. In most of the results we present, reference income is defined at the county-group (“puma”) level. We have also estimated all of our models using the state as the level of aggregation, and show some of these results here.[3]

A second issue concerns data. As is discussed in Wagstaff and Doorslaer (2000), the best kind of data for testing the relative income model are individual-level data on health or mortality that includes geographical identifiers, so that measures of individual health and mortality can be related to both individual income and income in the individual's local area. However, such data are rare. Wagstaff and Doorslaer (2000) present an excellent review of existing evidence on the relative income model (along with evidence on a variety of other models) and cite only a handful of studies based on individual-level data. Mellor and Milyo (2000) use the Current Population Survey's information on self-reported health status and conclude that, controlling for own-income, aggregate state income has no effect on health. Soobader and LeClere (1999) also use self-reported health information from the National Health Interview Study, matched to Census tract income data, and reach a similar conclusion. Although self-reported health status is a useful outcome to study in its own right and is correlated with subsequent mortality, this research does not directly address the relationship between relative income and mortality. Because mortality is a rare event, studying mortality requires much larger sample sizes. Further, large-scale data sets with information on mortality, income, and geographical location are rare.[4]

In the absence of large-scale individual-level mortality data, how can the relative income model be tested? As Wagstaff and Doorslaer point out, aggregated data on mortality and income are of limited use. It is difficult to test whether relative income within counties or states affects mortality if the mortality data are aggregated at the county or state level. In this paper, we deal

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[3]A full set of results that use the state to define the reference group are available upon request.

[4]Several large data sets contain information on both income and mortality. These include the National Longitudinal Mortality Study and the National Health Interview Survey (mortality follow-back.) However, geographic identifiers are suppressed in the public-use versions of these data sets.

with this problem by using semi-aggregated mortality data, that breaks out mortality rates by age, race and gender *within* counties. Specifically, we examine whether the mortality of subgroups of the population within counties is affected by only subgroup income, or whether the incomes of others not in the subgroup matter. Racial identity and age are useful because they serve to distinguish between members and non-members of subgroups.

III. An Empirical Model of Relative Income and Mortality

a. <u>The model</u>

To fix ideas, we start with a simple linear empirical relationship between the health of individual *i* living in region *r* in year *t* (h_{irt}), his or her own (logarithm of) income (y_{irt}), and the income of those in the individual's reference group (z_{irt}):

$$h_{irt} = \alpha_{at} + \beta_1 y_{irt} + \beta_2 z_{irt} + \varepsilon_{irt} \qquad (1)$$

The subscripts on the intercept (α_{at}) indicate that the intercept varies with age and time. On average, we expect that health will deteriorate as individuals grow older, and will improve over time holding age fixed. Assume, for now, that z_{irt} is the average logarithm of income of those within *i*'s reference group. This assumption will be loosened in section V, below. An additional restriction that could be imposed on (1), and which will be tested in what follows, is that $\beta_2 = -\beta_1$, which implies that *only* relative income matters for health. This specification is the starting point in Deaton and Paxson (1999), which examines the implications for estimates of β_1 if z_{irt} is assumed to be unknown.

Equation (1) could in principle be estimated with individual-level data on health, income, and reference group income. However, with data on mortality rates for groups of individuals, equation (1) must be aggregated to the appropriate unit of analysis. We work with region-level mortality data (where *r* denotes the region, which could be a state, county, or city) for *G* groups within each region and year, where groups are defined by gender, age, and race. Equation (1) can be aggregated to the group level by averaging across all individuals within each region, year and group:

$$\overline{h_{grt}} = \alpha_{at} + \beta_1 \overline{y_{grt}} + \beta_2 \overline{z_{grt}} + \varepsilon_{grt} . \tag{2}$$

Estimation of all parameters in (2) requires that the reference groups differ from the groups over which (1) is aggregated. If individuals' reference groups were defined along the lines of age, race, and gender, then y_{grt} and z_{grt} would be identical and only the sum of β_1 and β_2 could be estimated. In this case, if own income and reference group income have equal but opposite effects, it will appear as if income has no effect on health. If instead the reference group consists of all people who live in the region, then $\overline{z_{grt}}$ will simply equal average regional income, and will be identical for the *G* subgroups within each region. In what follows, we assume that reference group income is equal to average regional income, so that $\overline{z_{grt}}$ is replaced by $\overline{y_{rt}}$:

$$\overline{h_{grt}} = \alpha_{at} + \beta_1 \overline{y_{grt}} + \beta_2 \overline{y_{rt}} + \varepsilon_{grt} . \tag{3}$$

Although it is plausible that there is some underlying index of "health" that is linear in income, the linear specification is not appropriate for a model of mortality. A better assumption is that the logarithm of the probability of death is linear in the observables.[5] Specifically, we assume that:

$$p_{irt} = \exp(\alpha_{at} + \beta_1 y_{irt} + \beta_2 \overline{y_{rt}}), \tag{4}$$

where p_{irt} is the probability of death for individual *i*, and we have incorporated the assumption that reference group income is equal to average regional income. Equation (4) implies that, provided β_1 is negative, individual health (measured as $1 - p_{irt}$, the survival probability) is concave in own income. The concavity of health in income is a common assumption and may produce a positive correlation between health and income inequality in *aggregate* data, even if no

[5]We also estimated models in which the log odds of mortality was assumed to be linear in the observables, with little effect on the results.

such relationship exists at the individual level. Miller (2000) uses an estimation methodology similar to the one here to examine the aggregate correlation between income inequality and health.

Without individual-level mortality data, (4) must be aggregated over individuals prior to estimation. We integrate over all individuals in the group to obtain:

$$\overline{p_{grt}} = \int_{i \in g,r,t} \exp\left(\alpha_{at} + \beta_1 y + \beta_2 \overline{y_{rt}}\right) f_{grt}(y)\, dy + \varepsilon_{grt} \tag{5}$$

where $f_{grt}(y)$ is the density function for income of people in group g in county r and year t, and ε_{grt} is a group/county/year error term.[6] Although (5) is slightly more complicated than its linear analog (3), its parameters can be estimated given estimates of $f_{grt}(y)$.

b. Data and Implementation

We use two sources of data to estimate (5). The first is data from the 1980 and 1990 Compressed Mortality Files (CMF). The CMF is a census of all deaths in the US, and is compiled by the National Center for Health Statistics at the Center for Disease Control. The CMF contains information on the number of deaths by cause, within each county, gender group, racial group, and age group. The age of the deceased is recorded in 13 groups. We use information on deaths of people between the ages of 25 and 84, grouped into six 10-year age bands. We do not use data on deaths after age 85, because we are concerned about the well-documented racial differences in age misreporting at the oldest ages. We also restrict our analysis to blacks and whites, because it is difficult to obtain good estimates of $f_{grt}(y)$ for smaller racial and ethnic groups.

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[6]In this paper we model the error term as a puma/age/year random effect. One alternate strategy is to model the aggregate error term as the sum of a collection of i.i.d. individual errors. This approach lends itself to a minimum chi-squared estimation, and is discussed in general in Powell and Stoker (1985).

Information on mortality in 1980 and 1990 is matched to information from the 1980 and 1990 5% samples of the U.S. Census. These are the largest samples available for public use. Matching of the CMF and Census data is done at the "county group" (for 1980) and "puma" (for 1990) levels. Although there are slight differences between county groups and pumas, they are similar in most ways (and in what follows we refer to both as "pumas"). Pumas usually consist of counties or contiguous groups of counties within states, and are constructed to have populations of at least 100,000.[7] In some cases, densely populated counties are broken into more than one puma. We matched the CMF and Census data by aggregating the mortality data in the CMF up to the puma level, and (in some cases) aggregating puma information up to the county level. After matching, there are 849 pumas in 1980 and 903 in 1990.

The Census data are used for two purposes. The first is to construct puma-level income information that is included in the models. The variable $\overline{y_{rt}}$ is measured as the average of y_{irt}, the logarithm of household income per adult equivalent, in each puma and year. Adult equivalents are defined to be the number of household members aged 18 or more, plus half the number of members aged 17 or less. We also constructed averages of y_{irt} by racial group within pumas, and measures of the fraction of the population in each puma and year that is white.

Second, the Census data are used to construct estimates of the density of income, $f_{grt}(y)$, for each of the *G* groups within each puma and year. We did this simply by estimating a 200-bin histogram for the logarithm of income per adult equivalent for each group in each puma and year. Income is referred to by the midpoint of each of the 200 bands, y^m, where m=1,2,...,200. The probability that income for an individual in group *g*, puma *r* and year *t* falls into bin *m* is denoted as λ^m_{grt}. Equation (5) is then replaced with:

[7] Detailed information on the specific counties contained in county groups and pumas can be found at the IPUMS web site at the University of Minnesota: http://www.ipums.umn.edu/usa/

$$\overline{p_{grt}} - \sum_{m=1}^{200} \lambda_{grt}^{m} \exp(\alpha_{at} + \beta_1 y^{m} + \beta_2 \overline{y_{rt}}) = \varepsilon_{grt} \;, \qquad (6)$$

where $\overline{p_{grt}}$ is equal to the mortality rate reported in the CMF for the relevant group.

The parameters of (6) are chosen to minimize the sum of squared errors. Because the relationship between health and income may differ across whites and blacks and men and women, we estimate (6) separately for each of these groups. There is also evidence that the relationship between income and health becomes less pronounced at older ages (Backland et.al., 1996) and so we estimate separate models for younger (aged 25-64) and older (aged 65-84) age groups. This means that, for any one model we estimate, the data consist of mortality and income observations for either "older" or "younger" people of a given race, gender. Each observation is for a single puma/year/age group (where age group is defined by 10-year age bands) cell. There are four 10-year age groups represented in the younger samples (25-34, 35-44, 45-54, and 55-64) and two for the older samples (65-74 and 75-84), so the intercept term in (6) consists of 8 age group/year dummies for the younger group, and 4 dummies for the older group.

For some groups, most notably black men, there are pumas in which very few people (or even no people) in the group died in a given year. These are typically sparsely populated pumas for which there are few Census observations. To cite an extreme example, the 1990 CMF indicates that there were 39 black men aged 55-64 living in all of Vermont, none of whom died. (Only one black male in this age group from Vermont is represented in the 5% Census sample.) Measures of mortality rates and estimates of the income densities in cases such as these are likely to be very noisy. We deal this "small cell" problem in two ways. First, we weight all errors by the square root of the population of the relevant racial group in the puma. This means that, for example, when we estimate models for blacks, those pumas with small numbers of blacks relative to other pumas receive less weight. We also exclude observations if the population in the cell is less than 500.[8] This results in the elimination of a large number of observations for blacks. For example, the sample sizes for each group shown in Table 1 indicate that there are 2968 observations for younger black men, as compared to 7008 for younger white men, and 760 observations for older black men compared to 3493 observations for older white men. To make sure that differences in the results between blacks and whites are not driven by differences in the type of pumas being selected, we have also estimated a set of models for whites that use the same samples of pumas used for blacks. In most of our results, we also include a control for the fraction of the population in the puma that is white. Recent work by Deaton and Lubotsky (2001) indicates that racial composition is correlated with health outcomes for both blacks and whites, and we do not want to confound the effects of racial composition with the effects of relative income. In some specifications we also include a set of state dummies, to ensure that results are not driven by non-time-varying state specific characteristics that may be correlated with the income terms.

Figure 2 illustrates how our data are organized, and highlights the source of identification for our models. The Figure shows income data for one sub-group of people—black men aged 55-64—who live in pumas in which their population is at least 500. Each point represent a pair of averages of the logarithm of household income per adult equivalent for a single puma. The y-axis shows averages over all people in the puma, and the x-axis shows averages for black men aged 55-64. The figure indicates that although the incomes of black men in this age group are positively correlated with average incomes in their pumas, men in this demographic group have, on average, lower incomes than others. There is also a great deal of variation in the average income of black men in this group relative to the puma means, and it is this variation that allows us to identify the effects of relative income on mortality.

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[8]We experimented with excluding groups with populations less than 1500, and obtained similar results to those shown here.

IV. Results

We start by examining whether there is a relationship between mortality and own-group income. Table 1 presents variants of (6) that do not include $\overline{y_{rt}}$, puma-level average income, for each of the eight subgroups used for the analysis. Table 2 shows results of identical models, using state-level rather than puma-level data. Column (1) includes only own-group income, column (2) adds the fraction of the population in the puma that is white, and column (3) includes a set of state fixed effects. Each of these models includes a set of age-group/year dummies. Although these coefficients are not shown, they indicate that (holding income fixed) mortality declined between 1980 and 1990 for nearly all demographic groups. Exceptions include black and white men aged 25-34 and 35-44, as well as black women 25-34. Mortality increased between 1980 and 1990 for these groups. HIV/AIDS is the likely explanation.

For both states and pumas, the results in column (1) indicate that, for nearly all groups, high own-income is related to lower mortality. The association between income and mortality is larger for younger groups than for older groups, and larger for males than females. These patterns are consistent with other evidence that income gradients in mortality decline with age and are larger for males (Deaton and Paxson, 2000). The coefficients on own income for the younger group range from –0.113 for white females to –.290 for white males. These coefficients are large. For example, the estimate for black males of –.221 implies that a doubling of income reduces the probability of death by 22%. These gradients shrink to values of less than –.10 for older groups, and for older white women own income is not protective.

The second column in each panel includes a control for the fraction of the puma population that is white. The fraction white has large *protective* effects for both racial groups, especially for males and younger adults. The coefficient of –0.587 for younger black men implies that an increase in the fraction white from 80% to 95% (which is a move from the 25th to the 75th percentile in fraction white across pumas) reduces the probability of death by 8.8%. The inclusion of state fixed effects in column 3 does not affect the protective effects of both own-income and fraction white, so these results are not the product of unobserved non-time-varying state characteristics.

The association between local racial composition and mortality is the subject of Deaton and Lubotsky (2001), who obtain similar results using CMF and Census data aggregated to the city level. In addition, they find that the inclusion of the racial composition measure accounts for the positive relationship between income inequality and mortality that is observed at both the state and city level. The source of the association between mortality and racial composition is a puzzle that they (and we) do not resolve. The association appears within all regions of the US. It is not accounted for by differences in education across areas with different racial compositions. Alesina, Baqir and Easterly (1999) argue that greater racial diversity may reduces expenditure on public goods, and while there is no evidence that this is the case it remains a possible mechanism. Another untested alternative is that selective migration results in higher concentrations of healthier people in areas with more whites. Whatever the reason for the negative association between "fraction white" and mortality, the inclusion of "fraction white" does not have large effects on the income gradient in mortality.

The central results of our analysis are in Table 3 (for men) and Table 4 (for women). The results shown in the first column in each panel are for models that include own income, average puma income, as well as the age/year interactions and the fraction white. As before, both "fraction white" and own income are positively associated with lower mortality. The coefficient on mean puma income provides evidence that having relatively wealthy neighbors is not beneficial to health, and in some cases is harmful. For younger black men, younger women of both races, and older white men and women, the coefficient is estimated to be positive and significant, indicating that higher levels of mean state income are associated with higher mortality. For older black men and women and younger white men the coefficient is not significantly different from zero. The magnitude of the coefficient for younger black men indicates that doubling the mean income in a state (holding own income fixed) is associated with a 16.3 percent increase in the mortality rate. For each group, the magnitude of the coefficient on state mean income is smaller than the magnitude of own income. This implies that if everyone in the state experiences the same growth in income, mortality is predicted to decline.

In column (2), we impose the restriction that the coefficients on own and mean puma income are equal and opposite in sign. This would be the case if *only* relative income matters for mortality, so that overall income growth has no effect on mortality. A likelihood ratio test indicates that this hypothesis is rejected for all demographic groups except older black and white women, and older black men. The test for older black men is nearly rejected, with a P-value of .053. These results indicate that, for the younger groups for whom income has the strongest association with mortality, the strict version of the relative income model is not valid.

So far we have focused on income relative to mean income for everyone in the puma. It is plausible that the incomes of those who are members and non-members of the individual's racial group have different affects on health. For example, if the "political economy" mechanism discussed above is valid and if people are more altruistic towards members of their own racial group, then having relatively wealthy neighbors of the same race could be beneficial, whereas having relatively wealthy neighbors of a different race could be harmful. It is also possible that, in a psychosocial stress model, reference groups are defined along racial lines. In column 3 of Tables 3 and 4, we control for both the mean income of puma residents in the same racial group and the mean income of puma residents from the other racial group. Although the results are somewhat mixed across the different demographic groups, several patterns emerge. First, for five of the eight demographic groups (young and old black men, young black women, and young and old white women) there is a significant positive association between mortality and mean income of other races. In other words, having an income that is low relative to that of members of a different racial group is harmful. Second, the coefficients on mean income of those in the same racial group indicate are typically not significant (an exception is for older white men.) Furthermore, in several instances the coefficient on own income becomes insignificant when mean own-race income is included. This may reflect measurement error in own income, so that the mean income level for the entire racial group is picking up some of the effects of own income.

The results in column 4 include state fixed effects. In this specification, the models are identified from variation in income and mortality between different pumas and over time within a state. Including state fixed effects weakens some of our earlier results. The results for younger black men still indicate a strong association between low relative income and mortality. However, for four other demographic groups (older white men and women, and younger black and white women), the previous finding that puma income is negatively associated with mortality is not robust to inclusion of state dummies. Several of these groups have coefficients on puma mean income which are estimated to be of moderately large and positive, but none are statistically significant at the 5% level.

To summarize the findings from Tables 3 and 4, there is some evidence that having a lower relative income is associated with higher mortality. This evidence is most robust for younger black men. One possible explanation for this finding is that low relative income is harmful. However, another possible explanation is biases caused by unobserved heterogeneity in price levels across pumas. Specifically, suppose that mortality is negatively related to own real income but uncorrelated with puma-level income, so that β_1 is negative and β_2 is zero. Suppose also that there is unobserved heterogeneity in the price levels across pumas that is not accounted for by our deflation using the national CPI. In this case, mean puma income may simply be serving as a proxy for a puma-price deflator. If puma-level income is positively correlated with puma price levels, then it should also be positively related to mortality. In the absence of puma level price indexes, we cannot examine this hypothesis directly. However, if this argument were correct, we would expect to see a positive relationship between puma income and mortality for all groups, or at least for those groups for whom own income is associated with mortality. The fact that there is variation in the results across the demographic groups is evidence against this argument.

In order to shed more light on the relative income effects, we separate mortality into by-cause categories, and estimate our models separately for each category. In this section we focus our analysis on the younger (25-64) age groups. We examine six categories of mortality: infectious diseases, cancers, diabetes, cardiovascular, accidents, and homicide. We estimate our relative income model at the puma level with terms for own income, mean puma income, and fraction white in the puma.

Results are presented in Table 5. Infectious disease mortality exhibits strong relative income effects. For black men and for women of both races mean puma income is associated with much higher infectious disease mortality. The results for white men are equally strong as the other groups, but with opposite signs. These results may reflect AIDS mortality in wealthy urban centers in 1990. Cancer mortality is also strongly related to relative income. This is true of all demographic groups examined except for black women.

We include diabetes mortality because it may be related to stress, one of the proposed mechanisms for the relative income hypothesis. Diabetes mortality in our data is not correlated with relative income for any of the groups. Similarly, cardiovascular mortality (while strongly related to own income and fraction white) is not related to relative income for any of the demographic groups. This, coupled with the lack of correlation with diabetes mortality, is cause for skepticism about psychosocial stress mechanisms for relative income effects. It is also interesting to consider the cancer results together with the cardiovascular results. If the correlation between cancer mortality and relative income was due to greater levels of behavioral risks such as smoking, we would also expect cardiovascular mortality to be correlated with relative income. As this is not the case, a plausible explanation for the cancer results may be that they are due to heightened environmental risks associated with lower relative income.

We also include two external causes of death in our analysis, accidents (which for the younger age groups is largely comprised of motor vehicle accidents) and homicide. The results for accidents are very different from our other relative income results. For all groups, puma mean income (as well as own income) is measured to be protective with respect to accident mortality. This is consistent with better provision of accident-reducing public goods, such as better roads or more traffic enforcement resources. It is also consistent with the notion that richer people get in fewer accidents which, as many motor vehicle accidents involve more than one vehicle, means that those around richer people also get into fewer accidents. Finally, for homicide mortality we find no relative income effects for men (where homicide is a larger share of mortality), and mixed results for women. For black women, own income is measured to be protective and mean puma income as positively correlated with mortality. For white women just the opposite is true. We are unsure why this is so.

To summarize our by-cause results, we find different patterns for the differing causes of death. Generally speaking, being of low relative income is associated with higher infectious disease and cancer mortality, but not higher diabetes or cardiovascular mortality. For accident mortality, being in a richer puma is protective. And for homicide, we find no relative income effects for men. Overall, these results continue to offer mixed evidence on the relative income hypothesis. They offer no support for the psychosocial stress mechanism of relative income, but raise the possibility that increased environmental risk may be associated with low relative income.

V. Semi-Parametric Extensions

a. <u>The model</u>

Up to this point, we have assumed that "reference income" is equal to the average of the logarithm of income within a puma and year. This choice of average income is arbitrary. It is possible that the effects of relative income on health work not through mean income but through some other summary measure of the income level on the puma. Possibilities include the median of income, some other percentile of the income distribution, or one of many other statistics related to the "level" of income in a given area. In this section we perform a semiparametric estimation that relaxes our assumption on the form of the relative income term. As will be seen in what follows, the methods we develop would be difficult to implement with puma-level data, and we focus instead on estimates that use the state as the unit of analysis.

We relax our assumption on the form of the relative income term by modeling mortality as dependent on own income and an unknown "level of income" term, $G(y_{1s}, y_{2s}, \ldots, y_{Ns})$, where y_{js} is the income of person *j* in state *s*. (Time subscripts have been suppressed). We make only two assumptions about $G(\cdot)$. First, we assume that $G(\cdot)$ is weakly increasing in each of its arguments. Since $G(\cdot)$ represents the "level" of income, it makes sense to assume that if any individual's income increases, it cannot be the case that the level of income declines. Second, we assume that $G(\cdot)$ is invariant to the total size of the state population. In other words, scaling the size of the population while leaving the distribution of income unchanged is assumed to have no effect on $G(\cdot)$.

If we consider two arbitrary income distributions Y_1 and Y_2, and a function $G(\cdot)$ that satisfies the two assumptions above, it is the case that if Y_1 first-order stochastically dominates Y_2, then $G(Y_1) \geq G(Y_2)$. To test whether the income level in an area affects mortality, we compare pairs of states for which the income distribution of one stochastically dominates that of the other. If mortality (conditional on own income) is systematically different in the states with dominant income distributions, we interpret this as evidence of an effect of relative income on mortality.

To implement this idea, we rewrite our micro-level relationship as:

$$p_{ist} = \exp(\alpha + \beta_1 y_{ist} + \beta_2 G(Y_{st})) \qquad (7)$$

Aggregating to the state level and taking logarithms yields:

$$\ln(\overline{p_{gst}}) = \ln[\int \exp(\beta_1 y) f_{gst}(y)\, dy] + \alpha + \beta_2 G(Y_{st}) + \varepsilon_{gst} \quad (8)$$

where we have included an additive aggregate error term to the equation. Since $G(Y_{st})$ is unknown, (8) cannot be estimated as written. Instead, we estimate a model based on differences in (8) between pairs of states. The difference in the function $G(Y_{st})$ between any two states s and s', which is also unknown, is replaced with an indicator function that equals one if the income distribution for state s first-order-stochastically dominates that of state s':

$$\ln(\overline{p_{gst}} / \overline{p_{gs't}}) = \ln[\int \exp(\beta_1 y) f_{gst}(y)\, dy / \int \exp(\beta_1 y) f_{gs't}(y)\, dy] + \beta_2^* I(Y_{st} \succ Y_{s't}) + v_{gss't} \quad (9)$$

As in our previous work, we estimate separate models for those in different race, gender and (broad) age groups.

The micro specification in (7) differs from our previous specification in two ways. First, it includes the unknown function $G(\cdot)$ in place of mean log income. This change is the main focus of this section. A second change is that the error term is modeled as linear in log mortality, rather than linear in the mortality rate. This is done so as to be able to implement our differencing strategy. When we re-estimate our primary models using this specification, we obtain results qualitatively similar to those shown above.

b. Estimation issues

Several complications arise from the differencing operation used to get from (8) to (9). The first issue concerns the true number of independent observations and the error correlation structure produced by differencing. Although each “observation” in the data used to estimate (9) consists of a pair of states, there are only 51 actual state observations in each year. The estimated covariance matrix must be adjusted for this differencing.

To compute the variance of our estimator, we begin by rewriting equation (8) in matrix form $M = H(\beta) + \varepsilon$, where M is the vector of values for $\overline{\ln(p_{gst})}$ and $H(\beta)$ is a vector of values of the right-hand-side of (8), written as a function of the parameters. (The independent variables are suppressed for notational convenience.) The differenced regression can be obtained by pre-multiplying $M = H(\beta)$ with a "differencing" matrix F, so that:

$$FM = FH(\beta) + F\varepsilon\,, \tag{10}$$

where F is a matrix with as many rows as there observations in the differenced regression. F has 51 columns (if all 51 states are in the regression), and each row of F has one 1 and one -1, in the appropriate columns corresponding to which states are being differenced. If we knew the function $G(Y_{gst})$, then (10) would correspond to (9) exactly. In practice, however, the differences in this function are replaced by the indicators for stochastic dominance, as discussed above.

Given weights assigned to each differenced pair (these weights are discussed below), our NLLS estimator minimizes the weighted sum of squared errors:

$$\hat{\beta} = argmin\,(M'F' - H'F')\,W\,(FM - FH) = \varepsilon'F'WF\varepsilon\,. \tag{11}$$

Denote the derivative of H with respect to β as $D = \partial H(\beta)/\partial\beta$. The variance of our estimator is[9]:

$$V(\hat{\beta} - \beta) = (DF'WFD')^{-1}DF'WFV(\varepsilon)F'WFD'(DF'WFD')^{-1} \tag{12}$$

Ideally, we would estimate this variance in way in that would account for within-state clustering of the errors. However, we observe only differenced residuals, rather than state-level errors. As such, we are forced in this section to assume that the errors within each state are independent, and

[9]To get from (11) to (12), note that the first-order conditions for (11) imply that $eF'WFD' = 0$, where e is the (predifferenced) residual from the regression. Expand this around the true β, and using the fact that $(\partial^2 H/\partial\beta^2)\,Fe$ goes to zero due to the independence of the errors, to obtain: $(\hat{\beta} - \beta) \approx (DF'WFD')^{-1}DF'WFe$.

our estimated variances are likely to be overly precise. Finally, we are concerned about the sensitivity of the estimates to weighting. We present unweighted estimates, as well as estimates in which each observation is weighted by $(1/N_s + 1/N_{s'})^{-1/2}$. This puts greater weight on pairs of states with greater populations.

c. Results

Our first task is to estimate the stochastic dominance indicators between each pair of states. To do this, we construct an empirical cumulative density function (CDF) of income for each state in each year, computing the fraction of individuals with incomes less than each of 200 equally spaced points ranging from (a log income of) 0 to 13. For each pair of states within a year, we compare the CDFs. If state A's CDF is never less than state B's CDF, and is higher for at least one income value, we mark state B's income distribution as stochastically dominating that of state A. In order to avoid spurious conclusions drawn from comparisons in the tails of the distributions, we only make comparisons for those income points which are greater than both A and B's 1st percentile of income, and less than their 99th percentile of income. Once we have constructed the income dominance relationships, we estimate model (9) as discussed above.

Estimates of (9) are in Table 6. We present both weighted and unweighted estimates of β_2^*, for each of the demographic groups, both with and without state fixed effects. The results indicate that, for many of the demographic groups, β_2^* is not significantly different from zero, indicating that the income level of the state of residence does not matter. However, there are a number of groups for which β_2^* is estimated to be significantly greater than zero. The strongest results are for working-aged black men. Consistent with our earlier models, we find significantly higher mortality associated with being in a richer state. The magnitude of this effect is moderate. The point estimate for the WLS regression indicates that being in a richer state is associated with an equivalent amount of increased mortality as (using the coefficient on own income) having 25% less individual income. Significantly positive coefficients are also estimated for working aged white men (in the models with state fixed effects) and black women (for the cross-sectional models), and retirement aged white women (for the cross-sectional models). In each of these cases, the magnitude of being in a richer state is comparable to having about 28% less income.

VI. Conclusion

The results in this paper provide some evidence that being relatively poor is harmful to health. This evidence is most robust for younger black men. For this group, a 10 percentage point increase in average puma income (holding own income fixed) is estimated to have the same effect on mortality as a 5 percentage point *decrease* in own income. For other race, age and gender groups the evidence on relative income models is less clear-cut. However, in no case do we find support for the idea that there are general health benefits from having wealthier neighbors.

Interesting patterns emerge when we examine mortality due to different causes. The relative income effect is found for mortality due to infectious diseases and cancer, but not for cardiovascular mortality. Accident mortality shows patterns opposite to the trend - we do find evidence that having wealthier neighbors results in lowered accident mortality.

The evidence in this paper does not speak strongly to the mechanisms that underlie the association between relative income and mortality, although it takes some first steps in that direction. We do not know if younger black men who are relatively poor are subject to greater psychosocial stress, or whether low relative incomes result in less healthy living conditions or poorer quality health services. Some of the mortality causes presumed to be associated with psychosocial stress (such as diabetes and cardiovascular mortality) show no relative income effect. The lack of a relation for cardiovascular mortality is especially intriguing, as it is commonly thought to be related to stress. Further, it reflects both behavioral risk factors and medical treatment. Given that (for younger black men) we find a relative income effect for overall mortality but not for cardiovascular mortality, these paths as intervening mechanisms are called into question. The fact that the association appears for some but not all causes of death makes it unlikely that a single simple story can be identified. A possible direction for future research would be to examine whether low relative incomes are associated with health-related behaviors, such as smoking, that affect the risk of mortality.

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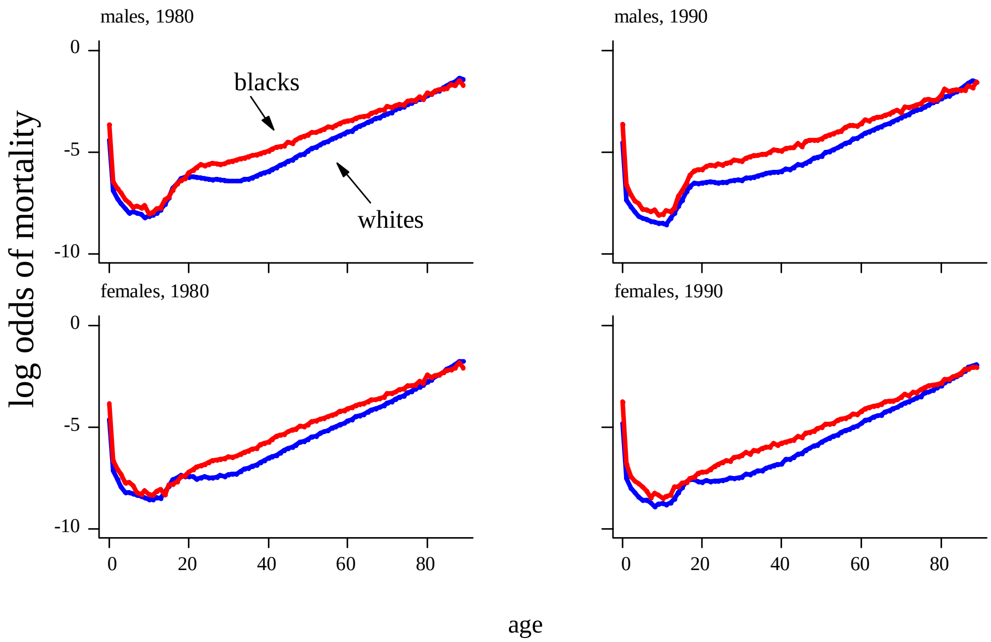

Figure 1: Log odds of mortality, by race and sex, 1980 and 1990

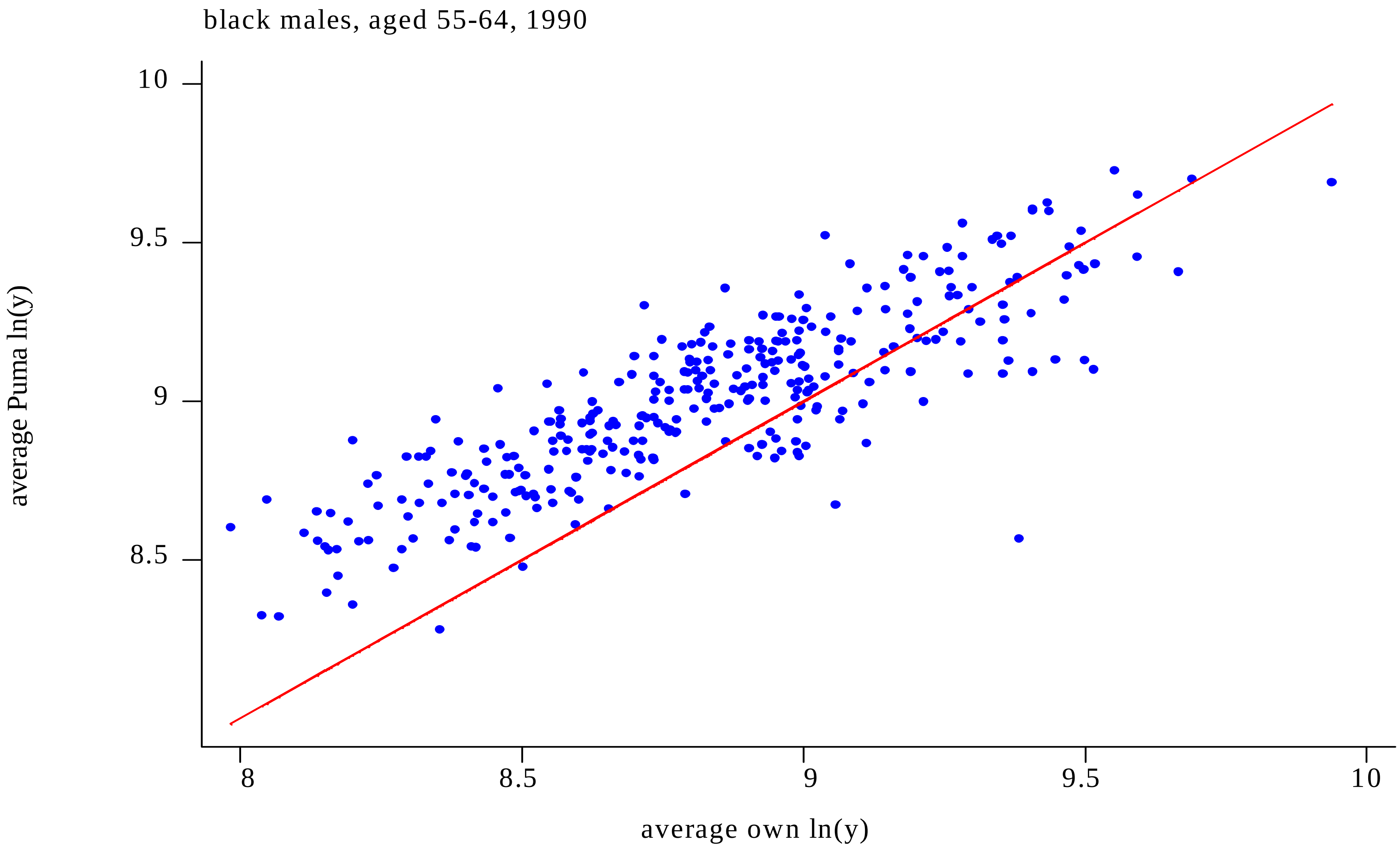

Figure 2: Own income and puma income for one age, race and gender group

Table 1: Effects of own income on the probability of mortality. Puma–level data. Maximum likelihood estimates. T–statistics in parenthesis.

	1	2	3	1	2	3
state fixed effects?	no	no	yes	no	no	yes
	Black men, ages 25–64 (obs=2968)			Black men, ages 65–84 (obs= 760)		
ln(y), own age/race/sex group	–.221	–.170	–.213	–.064	–.057	–.022
	(9.78)	(5.92)	(5.65)	(2.61)	(2.15)	(0.43)
fraction white in puma		–.425	–.492		–.095	–.063
		(6.60)	(6.81)		(1.81)	(1.05)
	White men, ages 25–64 (obs= 7008)			White men, ages 65–84 (obs=3493)		
ln(y), own age/race/sex group	–.290	–.325	–.316	–.080	–.105	–.708
	(15.69)	(22.96)	(19.31)	(5.70)	(7.68)	(5.12)
fraction white in puma		–.587	–.606		–.203	–.207
		(16.97)	(15.50)		(8.34)	(8.08)
	Black women, ages 25–64 (obs= 3157)			Black women, ages 65–84 (obs=1032)		
ln(y), own age/race/sex group	–.154	–.139	–.160	–.051	–.042	.065
	(6.68)	(5.42)	(4.76)	(1.75)	(1.52)	(1.52)
fraction white in puma		–.134	–.170		–.112	–.094
		(2.51)	(2.75)		(2.53)	(1.82)
	White women, ages 25–64 (obs= 7008)			White women, ages 65–84 (obs=3503)		
ln(y), own age/race/sex group	–.113	–.140	–.192	.026	.006	–.009
	(5.93)	(8.24)	(10.61)	(1.62)	(0.34)	(0.56)
fraction white in puma		–.362	–.465		–.151	–.152
		(10.33)	(13.98)		(5.95)	(5.60)

Note: All regressions include age group dummies that differ across years and a year dummy, and are weighted by the group's population in the puma.

Table 2: Effects of own income on the probability of mortality. State-level data. Maximum likelihood estimates. T-statistics in parenthesis.

	1	2	3	1	2	3
state fixed effects?	no	no	yes	no	no	yes
	Black men, ages 25-64 (obs= 337)			Black men, ages 65-84 (obs=140)		
ln(y), own age/race/sex group	–.204	–.158	–.312	–.063	–.062	–.190
	(5.62)	(4.36)	(4.30)	(1.83)	(1.74)	(2.61)
fraction white in state		–.313	–.664		–.010	1.962
		(3.79)	(0.64)		(0.15)	(2.03)
	White men, ages 25–64 (obs=408)			White men, ages 65–84 (obs=204)		
ln(y), own age/race/sex group	–.343	–.345	–.218	–.146	–.148	–.061
	(9.71)	(10.03)	(2.10)	(2.97)	(3.11)	(0.36)
fraction white in state		–.520	–1.39		–.169	.164
		(6.64)	(1.89)		(2.54)	(3.81)
	Black women, ages 25–64 (obs=331)			Black women, ages 65–84 (obs=148)		
ln(y), own age/race/sex group	–.156	–.146	–.336	–.077	–.073	–.010
	(3.84)	(3.70)	(5.60)	(1.89)	(1.80)	(0.06)
fraction white in state		–.069	–.234		–.025	1.39
		(0.69)	(0.26)		(0.50)	(1.22)
	White women, ages 25–64 (obs=408)			White women, ages 65–84 (obs=204)		
ln(y), own age/race/sex group	–.041	–.047	–.120	.019	.008	–.051
	(0.84)	(0.99)	(1.49)	(0.39)	(0.18)	(0.25)
fraction white in state		–.187	.778		–.155	.861
		(2.38)	(1.45)		(2.33)	(1.30)

Note: All regressions include age group dummies that differ across years and a year dummy, and are weighted by the group's population in the state.

Table 3: Effects of own income and aggregate income on the probability of mortality. Puma–level data. Men. Maximum likelihood estimates.

state fixed effects?	no	no	no	yes	no	no	no	yes
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Black men aged 25-64 (obs=2968)				Black men aged 65-84 (obs=760)			
fraction white in puma	–.513	–.587	–.369	–.605	–.119	–.119	–.052	.037
	(6.41)	(9.78)	(5.63)	(6.82)	(1.94)	(1.94)	(0.92)	(0.47)
ln(y), own age/race/sex group	–.250	–.267	–.051	–.274	–.077	–.067	.028	–.004
	(7.89)	(8.44)	(0.81)	(6.54)	(2.16)	(1.90)	(0.78)	(0.08)
mean ln(y), puma	.163	.267		.154	.036	.067		–.028
	(2.64)	(8.44)		(2.02)	(0.81)	(1.90)		(0.56)
mean ln(y), own race group			–.235				–.137	
			(2.77)				(3.31)	
mean ln(y), other racial groups			.197				.113	
			(3.48)				(2.72)	
	White men aged 25-64 (obs=7008)				White men aged 65-84 (obs=3493)			
fraction white in puma	–.599	–.745	–.567	–.602	–.265	–.258	–.203	–.235
	(14.26)	(15.94)	(13.42)	(13.00)	(9.13)	(8.38)	(6.79)	(7.46)
ln(y), own age/race/sex group	–.340	–.406	–.335	–.311	–.189	–.140	–.203	–.112
	(17.65)	(25.6)	(17.39)	(11.99)	(8.86)	(6.58)	(9.91)	(4.90)
mean ln(y), puma	.022	.406		–.007	.101	.140		.039
	(0.75)	(25.6)		(0.19)	(4.91)	(6.58)		(1.78)
mean ln(y), own race group			.037				.119	
			(1.00)				(4.86)	
mean ln(y), other racial groups			–.022				.004	
			(1.17)				(0.26)	

Note: See note to Table 1. In (2), the coefficients on own–group mean ln(y) and state mean ln(y) are restricted to be equal and opposite in sign. A test of the null that these coefficients are equal and opposite indicates that this restriction is rejected for black men 25-64 (P=.000), white men 25-64 (P=.000), and white men 65-84 (P=.000), and nearly rejected for black men 65-84 (P=.053).

Table 4: Effects of own income and aggregate income on the probability of mortality. Puma–level data. Women. Maximum likelihood estimates.

state fixed effects?	no (1)	no (2)	no (3)	yes (4)	no (1)	no (2)	no (3)	yes (4)
	Black women aged 25–64 (obs=3157)				Black women aged 65–84 (obs= 1032)			
fraction white in puma	–.203	–.277	–.088	–.213	–.156	–.168	–.097	–.096
	(3.20)	(5.88)	(1.72)	(2.98)	(2.70)	(2.95)	(1.88)	(1.58)
ln(y), own age/race/sex group	–.199	–.216	.012	–.191	–.083	–.074	–.037	.064
	(5.76)	(6.37)	(0.16)	(5.09)	(1.83)	(1.70)	(0.66)	(1.18)
mean ln(y), puma	.117	.216		.062	.061	.074		.002
	(2.10)	(6.37)		(1.15)	(1.31)	(1.70)		(0.04)
mean ln(y), own race group			–.254				–.034	
			(2.85)				(0.64)	
mean ln(y), other racial groups			.169				.071	
			(3.69)				(1.42)	
	White women aged 25-64 (obs=7008)				White women aged 65-84 (obs=3503)			
fraction white in puma	–.433	–.481	–.405	–.471	–.213	–.215	–.216	–.176
	(10.28)	(10.13)	(9.75)	(11.41)	(7.69)	(7.96)	(7.00)	(5.62)
ln(y), own age/race/sex group	–.255	–.271	–.194	–.200	–.092	–.099	–.104	–.046
	(7.63)	(6.65)	(5.38)	(5.66)	(3.54)	(4.69)	(4.06)	(1.76)
mean ln(y), puma	.134	.271		.013	.100	.099		.035
	(3.81)	(6.65)		(0.28)	(4.70)	(4.69)		(1.77)
mean ln(y), own race group			.005				.032	
			(0.12)				(1.33)	
mean ln(y), other racial groups			.058				.078	
			(3.18)				(5.32)	

Note: See note to Table 1. In (2), the coefficients on own–group mean ln(y) and state mean ln(y) are restricted to be equal and opposite in sign. A test of the null that these coefficients are equal and opposite indicates that this restriction is rejected for black women 25-64 (P=.000) and white women 25-64 (P=.000), and not rejected for black women 65-84 (P=.293) and white women 65-84 (P=.396).

Table 5: Own income, aggregate income and cause–specific mortality. Puma-level data. Maximum likelihood estimates.

	infectious disease	cancers	diabetes	cardioB vascular	accidents	homicides
	Black men in age groups 25B64 (obs=2968)					
ln(y), own age/race/sex group	B.540 (14.88)	B.257 (2.87)	B.137 (1.13)	B.279 (6.66)	B.399 (9.43)	B.361 (2.84)
mean ln(y), puma	1.59 (5.13)	.287 (2.39)	B.092 (0.46)	.109 (1.40)	B.577 (4.26)	.373 (1.70)
fraction white in puma	B1.85 (6.07)	B.387 (4.76)	B.419 (1.67)	B.383 (4.22)	.211 (1.36)	B.902 (3.08)
	White men in age groups 25B64 (obs=7008)					
ln(y), own age/race/sex group	2.65 (32.11)	B.271 (9.71)	B.141 (1.40)	B.287 (10.22)	B.363 (5.43)	B.619 (17.72)
mean ln(y), puma	B.707 (2.99)	.134 (3.14)	B.351 (1.53)	B.061 (1.03)	B.570 (6.23)	B.216 (1.31)
fraction white in puma	B1.24 (2.65)	B.429 (6.20)	B.149 (0.61)	B.469 (6.09)	.028 (0.20)	B2.20 (6.58)
	Black women in age groups 25B64 (obs=3157)					
ln(y), own age/race/sex group	B.473 (9.15)	.012 (0.18)	B.248 (3.56)	B.291 (7.70)	B.346 (4.72)	B.477 (11.06)
mean ln(y), puma	1.39 (5.23)	.090 (1.04)	B.187 (1.40)	.028 (0.45)	B.357 (1.78)	.592 (3.47)
fraction white in puma	B1.49 (4.10)	B.137 (2.10)	.531 (2.61)	B.103 (1.17)	.379 (1.86)	B.468 (1.61)
	White women in age groups 25B64 (obs=7008)					
ln(y), own age/race/sex group	B.571 (5.72)	B.146 (2.35)	B.255 (1.20)	B.318 (9.33)	.029 (0.23)	1.28 (5.16)
mean ln(y), puma	.465 (3.37)	.286 (4.89)	B.497 (1.64)	B.024 (0.40)	B.646 (4.05)	B1.65 (9.62)
fraction white in puma	B1.90 (2.32)	B.344 (5.19)	.342 (1.45)	B.471 (5.49)	.178 (0.88)	B.231 (0.81)

Note: See note to Table 1.

Table 6: Pair–wise state income dominance and mortality

	β_{NLLS}	β_{WLS}	β_{NLLS}	β_{WLS}
	no state fixed effects		state fixed effects	
black men 25–64	.098	.103	.037	.037
	(3.05)	(4.12)	(2.47)	(3.49)
black men 65–84	.010	.000	–.011	–.006
	(0.44)	(0.00)	(1.08)	(0.78)
white men 25–64	.026	.007	.018	.019
	(1.73)	(0.55)	(3.18)	(3.89)
white men 65–84	.004	.011	.000	–.001
	(0.33)	(1.19)	(0.02)	(0.32)
black women 25–64	.069	.061	.014	.014
	(2.89)	(3.08)	(1.10)	(1.48)
black women 65–84	–.018	–.004	.024	.016
	(0.72)	(0.22)	(1.74)	(1.74)
white women 25–64	.014	.018	–.006	–.002
	(0.97)	(1.49)	(1.09)	(.0.49)
white women 65–84	.029	.023	–.003	–.003
	(2.48)	(2.26)	(0.75)	(1.23)

Note: Results are coefficients on the stochastic dominance term. Coefficients indicate percentage increase in mortality associated with being in a richer state. T–statistics in parentheses.