Relative Deprivation, Poor Health Habits and Mortality

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Abstract

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I. **Introduction**

In 1996 the United States' per capita health care expenditures totaled about $4000, first overall and nearly 56 percent more than the second highest spending country in the OECD. In spite of this spending, the US performs poorly in aggregate measures of population health. Among OECD countries, the US ranks 19th and 24th in women's and men's life expectancy respectively, and the US has the 6th highest infant mortality rate in the developed world (Organization for Economic Cooperation and Development, 1998).

These numbers, as well as other evidence, suggest that—at least in developed countries—money and health are not as closely linked as one might guess. One contentious explanation for differences is the "relative deprivation" hypothesis, which argues that individuals are adversely affected when they perceive themselves to be economically deprived relative to their peers. The relative deprivation hypothesis is distinct from more traditional models that argue an individual's health is a function solely of his or her underlying characteristics, such as own income, education, and race. According to the relative deprivation hypothesis, an individual's health is also a function of the incomes of others in her reference group. It's typically assumed that a person's health is negatively related to the income of others, so that as person j becomes richer, person i's health deteriorates. Low relative income may cause stress and depression, conditions that may raise the probability of contracting a disease or increase the tendency to engage in risky behavior.

Much of the evidence for the relative deprivation hypothesis comes from studies that link income inequality to population health. Income inequality can be seen as a proxy for deprivation, in that as inequality increases, the gap between the "haves" and the "have-nots" grows, and the overall deprivation in society increases. However, income inequality could influence health independently of relative deprivation[1] and most of the current literature does not attempt to disentangle the two effects. At the aggregate level, measures of inequality seem to be highly correlated with public health indicators such as mortality rates (Kaplan, et al., 1996; Kennedy, Kawachi, and Prothrow-Stith, 1996; Wilkinson, 1996).

[1] Income inequality might lead to under-investment in public goods or declines in social capital, which may adversely affect population health.

Yet, the current literature suffers from several drawbacks. For example, results are typically based on aggregate data and do not adequately control for individual income. Also, key variables such as education, family size and marital status are often omitted from the analysis. The use of aggregate data makes it difficult to control for conditions that are specific to reference group, such as differences in health habits.

In the first part of this paper, we use restricted-use micro-level data from the National Health Interview Survey Multiple Cause of Death Files (NHIS/MCOD) to investigate relative deprivation's role as a cause of increased mortality risk. We define reference groups using a combination of characteristics including state of residence, age, race, and education. Our results indicate that, even after controlling for reference group effects and individual income, relative deprivation has a positive and statistically significant influence on the probability of death. Relative deprivation also increases the probability of cause-specific mortality, notably for deaths due to tobacco-related cancers and coronary heart disease.

The latter finding is suggestive, mainly because these two causes of death that are highly linked to behavior. The American Heart Association reports that cigarette smoking alone accounts for nearly 20 percent of all deaths in the United States. Cigarette smoking is the direct cause of 87 percent of all lung cancer cases, and the surgeon general calls smoking "the most important of the known modifiable risk factors for coronary heart disease (AHA, 2000)." One theory relating relative deprivation to health outcomes argues that individuals respond to the stress, hostility, and low self-esteem caused by relative deprivation by engaging in health compromising behavior. Wilkinson explains "among the many ways people respond to stress, unhappiness and unmet needs, one is to increase their consumption of various comforting foods . . . including alcohol and of course tobacco" (pp. 185-186). The notion that relative deprivation increases the probability of taking health risks is consistent with evidence that individuals of low socioeconomic status tend to smoke more and exercise less than their peers (Lynch, Kaplan, and Salonen, 1997, Lantz et al., 1998). In the second part of this paper, we use data from the National Health Interview Survey and the Behavioral Risk Factor Surveillance System to explore whether relative deprivation is associated with health- compromising behavior. Using a number of outcomes including smoking, alcohol abuse, seatbelt use, body mass index, self-reported health status, and propensity to exercise, we find that high relative deprivation consistently influences the probability that an individual engages in risky behaviors. A one-standard deviation increase in relative deprivation increases the probability that an individual smokes by as much as 20 percent, and decreases the probability that an individual wears a seatbelt or exercises by 13 and 10 percent, respectively.

II. Income Inequality and Health: A Short Review

Wilkinson (1992) uses income data from the Luxembourg Income Study to show a strong correlation between the percent of income going to the lower 70 percent of the income distribution and life expectancies in 9 OECD countries. He argues that, since this correlation is large compared to the correlation between GNP per capita and life expectancy, relative income may be a more important cause of mortality than absolute income.[2] In a subsequent book (1996), Wilkinson elaborates on the relative income hypothesis, and argues that the inequality/mortality relationship cannot be attributed to omitted country-specific factors such as diet and exercise. As key evidence, Wilkinson cites a study by Marmot and Davey Smith (1989) that compares changes in life expectancies and income distribution in Britain and Japan. While Japan and Britain had similar income distributions and life expectancies in the early 1970s, Japan's income distribution became more equal in subsequent years. The increasing equality in Japan was accompanied by increases in Japanese life expectancy vis-à-vis British life expectancy. Marmot and Davey Smith argue that no other changes in Japanese lifestyle (e.g. diet or exercise patterns) can explain the improvement in Japanese life expectancies.

While Wilkinson reports correlation coefficients, evidence linking mortality and income distribution across countries can be shown in a regression framework as well. Waldmann (1992) finds that, even after controlling for a number of variables, infant mortality rates are positively related to the share of income going to the rich. Kaplan et al. (1996) show that U.S. states with greater income inequality (measured by the percentage of total household income received by the poorest 50 percent) have higher all-cause mortality rates than their more egalitarian counterparts. In this work, the

[2]Wilkinson's work has been criticized on a number of grounds, including his seemingly ad hoc use of the proportion on income going to the bottom 70 percent of the population as a measure of income inequality. See Judge, 1995.

magnitude of the mortality/inequality correlation is highest for the 25 to 64 age group. A similar state-level study by Kennedy, Kawachi and Prothrow-Stith (1996) examines the relationship between the Robin Hood index (the share of total income that would have to be taken from those above the mean and transferred to those below to achieve an equal distribution) and cause-specific mortality rates. Using regression analysis and controlling for poverty and smoking rates, Kennedy et al. find statistically significant associations between the Robin Hood index and all-cause mortality, heart disease mortality, infant mortality, and homicide rates. Miller and Paxson (2000) regress state-level log odds of dying on mean income within groups (defined over state, race, sex, and age) and state mean income. They find that, even after controlling for own-group income and other cofactors, state mean income has a positive, statistically significant coefficient. This result suggests that individuals are adversely affected when others in their state of residence become more prosperous.

Critics of the aforementioned studies raise concerns about the use of aggregate data. As demonstrated by Gravelle (1998) and Rodgers (1979), if the relationship between individual health and individual income is concave, there may be a spurious correlation between income inequality and mortality at the aggregate level. Imagine two communities of equal size, one in which half of the citizens have income I_a and half have lower income I_c and another in which every citizen has income I_b. For simplicity, assume that $(1/2)(I_a+I_c)=I_b$. If income reduces mortality but at a decreasing rate, comparing aggregate outcomes across the two communities will reveal a positive relationship between inequality and mortality. An individual-level study that controls for individual income, in contrast, may not show a relationship between income inequality and mortality at all. Controlling for mean income in the aggregate study will not reconcile this discrepancy since the mean incomes in the communities are the same. The convex relationship between income and mortality leads to what is known as the "ecological fallacy," where inequality erroneously appears to have a causal impact on mortality rates.[3]

A second concern about much of the inequality/mortality literature is that many studies leave potentially important cofactors out of the analysis. Kaplan et al. (1996), for example, adjust only for age

[3]It is still the case that income redistribution could lead to an improvement in the average health of the population. However, it is not inequality that causes poor health outcomes. Rather, it is the increased prevalence of the very poor.

and median income, leaving out race, education, average family size, marital status, and behavioral characteristics. Kennedy et al. (1996) adjust for age, poverty rates, and smoking rates in their analysis. However they still omit a number of known mortality risk factors, notably education. If high school graduation rates, racial composition of the population, or other characteristics are systematically different in regions with high inequality, analyses may be biased by the omission of these variables. This concern is heightened by the fact that states with high inequality appear to be quite dissimilar from states with low inequality. Within the US, income inequality is highest in Southern states. In contrast, low inequality states include Vermont, Utah, and Hawaii, where social norms and behavior may be very different from the rest of the country. In these cross-sectional models, the inequality coefficient may be proxying for state-specific omitted effects, such as healthier lifestyles in Utah.

III. Pathways Linking Inequality, Relative Deprivation, and Health

At this point it is helpful to distinguish between various hypotheses relating income distribution to health and mortality. The first is the absolute income hypothesis, which postulates that people with higher incomes have better health outcomes. Some early evidence connecting income and health comes from an influential study by Kitigawa and Hauser (1973) who linked individual-level mortality data to the 1960 Census Micro Samples. Kitigawa and Hauser found that white males[4] with incomes greater then $10,000 (1959 dollars) were about 77 percent less likely to die than those with incomes less than $2000. Similar but smaller differences are reported for white women. More recent research yields comparable results (Rogot, et al. 1992; Brown 2000; Sorlie et al., 1995; McDonough et al., 1997), however some claim that the protective effects of increases in income level-off after a certain income threshold is reached (Duleep, 1995; Fuchs and Zeckhauser, 1987). In addition to income, other socioeconomic variables, most notably education, seem to have an absolute impact on health as well (Kitigawa and Hauser, 1973). Recent work suggests that at least for men, differences in mortality across education

[4]Most of the specific estimates reported in Kitigawa and Hauser are for whites only. While similar patterns linking socioeconomic status to mortality were discerned for nonwhites, the poor quality of the nonwhite data raises questions about the accuracy of these estimates. As a result, Kitigawa and Hauser often do not report specific numbers for nonwhites.

groups are widening (Pappas and Queen 1993, Preston and Elo 1995). Researchers also note differentials in mortality across race, marital status, and occupation (Sorlie, Backlund and Keller, 1995).

Two theories link income inequality to health outcomes. The first is the pure income inequality hypothesis. This theory posits that holding income constant, income inequality itself causes bad health outcomes, regardless of an individual's particular income level. There are several explanations for why income inequality might have adverse health consequences for individuals at all income levels.[5] Kaplan, Pamuk, Lynch, Cohen, and Balfour (1996), for example, find a positive correlation between the percent of income received by the least well-off 50 percent of households and the percent of total state spending allocated to education. To the extent that educational spending affects health (Grossman, 1972), we might expect income inequality to harm the wealthy as well as the disadvantaged. Another possibility is that highly unequal communities lack "social capital"—defined as trust, friendliness, civic involvement, etc. (Putnam, 2000; Kawachi, Kennedy, Lochner, and Prothrow-Stith, 1997). Communities with low levels of social capital may have elevated stress levels and high violent crime rates, including higher homicide rates. These community-wide problems might have adverse impacts for rich and poor alike.

A second hypothesis argues that individuals who have low income relative to their reference group are at a greater risk for mortality. This theory, known as the relative deprivation hypothesis, is often confused with the pure income inequality hypothesis.[6] Income inequality and relative deprivation are related, because if within-group income inequality rises, those at the bottom end of the reference group income distribution become relatively more deprived. But studies linking income inequality and health do not usually distinguish between the pure inequality effect and the relative deprivation effect. Wilkinson (1997) suggests that relative deprivation influences health primarily through psychosocial stress that affects those with low relative incomes. Individuals who feel they are economically disadvantaged compared to their peers may be depressed and disgruntled, conditions that affect health both directly (via heart disease, high blood pressure, and suicide) and indirectly (via increased smoking,

[5] For a summary of the pathways linking income inequality to health, see Kawachi and Kennedy, 1999.

[6] For instance, a website for the International Health Program at the University of Washington argues that income inequality "reflects the amount of relative deprivation in a society." (See http://www.washington.edu/eqhlth/).

poor eating habits, and alcohol abuse). The relative deprivation hypothesis is distinct from the absolute income hypothesis in that individuals with high absolute income can be relatively deprived, as long as their peers are more well-off than they are. Thus, a lawyer may be wealthy in an absolute sense, but deprived in a relative sense.

There is biological evidence to support the notion that relative status plays a role in both psychological and physical health. Studies indicate that socially subordinate monkeys have lower levels of serotonin, higher basal cortisol concentrations, and greater susceptibility to viral infections than dominant animals (Shively, et al., 1997; Sapolsky, et al.; 1997, Cohen, et al. 1997; McGuire and Raleigh, 1985). Low serotonin levels and high basal cortisol concentrations are associated with numerous adverse health outcomes including affective disorders, anorexia nervosa, sleep disorders, and Alzheimer's Disease. The relationship between social status and health persists even when the social hierarchy of the monkey troop is manipulated scientifically. While human social hierarchies are more complicated and more difficult to study than those of monkeys, social scientists draw parallels between research on primates and the potential relationship between relative income and health in humans (Frank 1985; Wilkinson, 1996; Cohen et al., 1997).

Further evidence of the harmful effects of relative deprivation is found in the famous Whitehall study that tracked the mortality outcomes of members of the British Civil Service. Evaluation of 10-year age-adjusted mortality rates reveal that the lowest-ranking civil servants were 3 times more likely to die than the highest-ranking civil servants (Marmot et al., 1984; Marmot, 1986). Moreover, the greatest discrepancies in mortality rates occurred for coronary heart disease and lung cancer, two types of death that are greatly influenced by behavioral factors. Though these results are not adjusted for income or education levels, even the lowest-ranking civil servants were employed and had access to nationalized health care. One conclusion that is often made from the Whitehall Study is that at least part of the mortality difference between the highest and lowest civil service grades was driven by relative deprivation.

IV. Constructing Measures of Relative Deprivation

In this paper, we want to examine whether mortality and other health outcomes are correlated with whether a person is deprived financially compared to others in his reference group. This requires that we define both the reference group and the measure of deprivation. The seminal definition of relative deprivation is accredited to Runciman (1966), who argues that an individual is relatively deprived if:

> (i) He does not have X, (ii) he sees some other person or persons, which may include himself at some previous or expected time, as having X (whether or not this is in fact the case), (iii) he wants X, and (iv) he sees it as feasible that he should have X.

Thus, we feel relatively deprived if others in our reference group possess something that we do not. While the object of deprivation (X) could be measured using any number of attributes (physical strength, attractiveness, intelligence, personal possessions), we follow others in defining X as income (Yitzhaki 1979, Hey and Lambert 1980, Berrebi and Silber, 1985).

Our starting point for measuring relative deprivation (RD) is based on Runciman's definition and subsequent theory developed by Yitzhaki (1979). For a person i with income y_i who is part of a reference group with N people, Yitzhaki's measure is defined as:

$$(1) \quad YRD_i = \frac{1}{N}\sum_j (y_j - y_i) \qquad \forall\, y_j > y_i$$

This measure posits that relative deprivation for person i is driven by the incomes of people who earn more than i does. The summation in equation (1) is divided by the size of the reference group for two reasons. First, this makes the measure invariant to the size of the reference group – without dividing by N, if the size of the population doubles (holding income distribution constant), relative deprivation doubles as well. Second, dividing by N can be interpreted as adjusting for the probability of making a comparison. If person i and person j are alone on a desert island, N is low and the probability of making a comparison in high. In contrast, if person i and person j are co-inhabitants of New York City, N is high and the probability of making a comparison is low. If income for person i is thought of as a draw from a distribution, the relative deprivation measure in equation (1) can be rewritten as:

(2) $YRD_i = [E(y \mid y > y_i) - y_i] * prob(y > y_i)$

Intuitively, Yitzhaki's RD measure is equal to the expected difference between i's income and the expected income of those with incomes greater than y_i, times the probability that income is greater than i's income.

One concern with the measure discussed above is that it does not take into account differences in the scale of the income distribution across reference groups. In other words, if everyone's income doubles, relative deprivation will double as well. This would certainly be a problem if we were looking at relative deprivation over time and incomes were unadjusted for inflation. But since we deal with cross-sectional data (discussed below), it is not clear whether we should be concerned about the scale of the reference group income distribution. If people view within-reference group income differences in proportional terms, then YRD will overstate the relative deprivation of individuals in high-income reference groups. But if absolute differences within reference group matter, then YRD is appropriate. The latter scenario would make sense if everyone uses a common yardstick to measure relative deprivation (say average US income), but comparisons are only made within reference group.

Since it's plausible that people measure relative deprivation in proportional terms, we construct two additional measures of relative deprivation that do not vary with the scale of the reference group income distribution. First, we construct a measure identical to YRD except using log income, which is equivalent to substituting ln(y) for y in equations (1) and (2). If we assume that income is log-normally distributed, we can use the properties of the truncated normal distribution to find a closed form solution to equation (2):

(3) $RDL_i = [\mu_r - \ln(y_i)] * (1 - \Phi_i) + \phi_i$

Where μ_r and σ_r are the means and standard deviations of log income, respectively for reference group r, , and Φ_i and ϕ_i are evaluations of the standard normal CDF and PDF respectively at $[\ln(y_i) - \mu_r]/\sigma_r$ The specification in equation (3) is convenient because we can now solve for i's relative deprivation if we know i's income and the mean and standard deviation of the logs of the reference group income distribution.

A third relative deprivation measure that is insensitive to the scale of reference group income is YRD divided by individual i's income:

$$(4) \quad RDI_i = \frac{YRD_i}{y_i}$$

Deaton (2001) suggests a measure similar to RDI where Yitzhaki's RD measure is divided by mean reference group income (m_r) instead of y_i. We prefer to divide by i's own income because YRD/m_r is sensitive to changes in the income distribution for individuals with incomes less that y_i. In particular, if average income below y_i increases then YRD/m_r will fall.

The three relative deprivation measures suggested above presume that the distance between y_i and y_j matters, either in proportional or absolute terms. Yet the animal studies discussed in section III emphasize rank over distance. To examine this possibility, we use the individual's centile rank within the reference group income distribution (where income is sorted in ascending order) as our fourth and final measure of relative deprivation. Unlike YRD, RDL, and RDI, centile rank is unaffected by changes in the shape of the income distribution. Thus, unlike the other three measures, centile rank does not reflect differences in income inequality across groups.[7]

Wilkinson and others argue that deprivation is the primary mechanism through which relative income affects mortality outcomes. Yet, evidence from biology does not make a clear distinction between the negative effects of being deprived relative to one's peers and the beneficial effects of being prosperous relative to one's peers. The latter effect is sometimes referred to as relative satisfaction. In this analysis, we test whether i's health is negatively correlated with the incomes of referents that have income greater than y_i. While this framework seems to ignore the relative satisfaction effect, it can be shown that the relative deprivation measures we use are directly correlated with reasonable measures of

[7]Although relative deprivation is an individual as opposed to an aggregate measure, YRD is closely related to income inequality. It can be shown that the average relative deprivation in a society is equal to μG, where G is the Gini coefficient of income (Yitzhaki, 1979). Using relative deprivation as opposed to income inequality has three advantages. First, it allows us to move from aggregate to individual-level data, allowing us to avoid the ecological fallacy. Second, we can empirically evaluate one of the specific pathways implicated in the relationship between income inequality and mortality. Third, since relative deprivation is constructed at the individual level, we can control for reference-group specific fixed effects. The lack of these type of controls have may caused omitted variables bias in previous work.

relative satisfaction.[8] Thus, in this work the effect of an increase in relative deprivation is identical to a decrease in relative satisfaction.

V. Reference Groups

In order to address the relative deprivation hypothesis, one must consider how individuals define reference groups. The social psychology literature suggests that members of one's reference group are typically selected on the basis of either similarity or geographic proximity (Singer, 1981). While geographic proximity is relatively easy to determine, "similarity" is a more nebulous concept. Various studies report that individuals define reference groups along demographic lines such lines as sex, education, and race (Merton and Kitt, 1950; Singer, 1981; Bylsma and Major, 1994). However, it is well acknowledged that there is no perfect formula for determining reference groups. Critics assert that the "Achilles heel" of social evaluation theory is the "failure to explain adequately how the relevant comparisons are selected in the first place" (Pettigrew, 1978; p. 36).

Perhaps because of the difficulty of determining reference groups according to "similarity", most studies dealing with health and inequality define relative deprivation within the context of geographical location. Studies of the U.S. typically use state of residence as the implicit reference group. Restricting inequality measures to geographic boundaries makes sense if we expect that inequality affects health through its impact on public investment in human and social capital. However, if Wilkinson's psychosocial pathways are the more probable culprits, then it's not clear that reference groups should be limited to geographical confines. Individuals may compare themselves to others of similar demographic backgrounds, regardless of geographical location. Frank makes this point in his 1985 book *Choosing the Right Pond* (pp. 33-34):

> To be sure, people in similar circumstances, even though located far away, can be even more important than people nearby whose circumstances are markedly different. For example, a 35-year old vice president in a bank branch in San Francisco may take a much greater interest in the

[8] Yitzhaki proposes an analogous relative satisfaction metric that is equal to μ-YRD. Since we are using reference-group fixed effects, this measure is a linear combination of the fixed effect and the relative deprivation measure. A second potential measure of relative satisfaction, $\{\ln(y_i)-E(\ln(y)|\ln(y)<\ln(y_i))\}*prob(\ln(y)<\ln(y_i))$, is a linear combination of YRD, $\ln(y_i)$, and μ.

salary of her counterpart at the Los Angeles branch than in the salary of the 50-year old dentist in her own neighborhood.

Deaton (1999) addresses the issue of "similar circumstances" by using birth cohorts to define reference groups. Using mortality data from the Berkeley Mortality Database and income data from the March Current Population Survey, Deaton regresses aggregate mortality rates by cohort on income inequality and mean income. While Deaton finds no relationship between income inequality and mortality at the cohort level, he demonstrates that the gradient between income and mortality is steeper when income inequality is higher.

Deaton acknowledges that birth cohorts provide an imperfect measure of an individual's true reference group. However, he claims that birth cohorts should contain a high ratio of "relevant to irrelevant reference people" as compared to the general population. Thus birth cohorts can act as a rough proxy for true reference groups. In this study, we construct reference groups based on observable demographic characteristics such as state of residence, race, education, and age. Groups defined using such characteristics do not necessarily constitute the unobservable true reference groups. Yet, members of such groups have a high degree of similarity and are likely to contain a high proportion of relevant reference people. Following Deaton, we assume that reference groups with a high "relevant/irrelevant" ratio are reasonable proxies for the unobservable true reference groups.

VI. Relative Deprivation and Mortality

a. Data

One reason the literature on income distribution and mortality has been slow to move from aggregate to individual data is that data containing both mortality and geographical information are difficult to find. Mellor and Milyo (1999) try to avoid fallacies of aggregation by using individual-level income data from the Current Population Survey. However, since the CPS does not include information on mortality, they use self-reported health status as their outcome of interest. Fiscella and Franks (1997) use individual-level mortality data from the epidemiological follow-up to the National Health and

Nutrition Examination Survey I (NHEFS). Yet the survey's small size (14,407 observations) is a concern especially since death is such a rare event in this sample. Moreover, Fiscella and Franks calculate income inequality across primary sampling units (PSUs) using data from the NHEFS. On average, the NHEFS contains only 131 observations for each PSU, and income is recorded as a 12-level categorical variable. Thus, income inequality measures derived from this source are likely to be noisy.

For this paper, our primary sample is a restricted-use version of the National Health Interview Survey Multiple Cause of Death Files (NHIS/MCOD). The NHIS is an annual survey of the United States civilian non-institutionalized population conducted by the National Center For Health Statistics. Individual information from the NHIS person file contains about 120,000 observations each year. The person file includes a wide variety of demographic data (age, sex, race, family income, etc.), as well as health-related information such as height, weight, and self-reported health status. Respondents from the 1986 through 1994 NHIS were tracked in the National Death Index and the data indicates whether a person died by the end of 1997. As a result, for each year of the NHIS a corresponding Multiple Cause of Death File (MCOD) is available that contains year of death, month of death, and cause of death for deceased NHIS respondents. Data from the MCOD can be merged with the NHIS persons file to form a linked file that we will refer to as the NHIS/MCOD.

The public use version of the National Health Interview Survey Multiple Cause of Death Files (NHIS/MCOD) withholds geographical location due to confidentiality concerns. Geographical information for this survey is available for restricted use, however, and for the purposes of this paper we were able to gain access to state-of-residence information from the 1988-1991 NHIS/MCOD restricted-use file. This access gives us an advantage in that we can use individual-level income data, we can use mortality as our outcome of interest,[9] and we can include geography as part of our reference group definition. To construct reference groups, we use income data from the 1990 Public Use Micro Data Sample (PUMS). Since the data from the NHIS is centered around 1990, we can exploit the extremely

[9]Mortality is a useful outcome because it is an objective measure of health that is precisely measured. However, there are other outcomes–such as episodes of illness–that might be relevant as well. We will address this issue in subsequent sections of this paper by using activity limitations status and work limitation status as dependent variables.

large sample size from the PUMS to construct accurate estimates of reference group income characteristics.

The NHIS/MCOD contains several pieces of information that are crucially needed to address the relative deprivation hypothesis. First, it contains information on year and month of death, allowing us to construct fixed time windows over which we can track mortality.[10] Second, since we were granted access to the restricted-use NHIS data, we have information about an individual's state of residence. This information allows us to define reference groups across geographic regions. Additionally, this information allows us to control for state-specific characteristics that may be correlated with both inequality and mortality. Finally, since the NHIS contains a family income variable, we can examine the effect of relative deprivation on health independent of the effect of absolute income on health.

In order to construct the relative deprivation measure outlined above, we need data on individual income and information about the income distribution for the reference group. While sample sizes in the NHIS are relatively large (about 120,000 observations each year), these sample sizes diminish greatly when reference groups are stratified along demographic dimensions.[11] Additionally, since the family income variable in the NHIS is a 27-level categorical variable,[12] estimates constructed from this measure will be noisy. Rather than rely on data from the NHIS to construct measures of relative deprivation, we instead match income variables from the NHIS to household income[13] data from the 1990 Public Use Micro-Data Sample. The 1990 PUMS is the best available source of income data because it has extremely large sample sizes and the income variable is continuous. While household income is topcoded

[10]We impute month of interview using the quarter and week of interview.

[11]If state of residence from the NHIS is used to construct reference groups sample sizes for each reference group fall to as low as 179. Sample sizes drop even further as more dimensions are added to reference groups, such as race and education.

[12] Income categories are: <$1000, $1000-1999, $2000-2999, $3000-3999, $4000-4999, $5000-5999, $6000-6999, $7000-7999, $8000-8999, $9000-9999, $10000-10999, $11000-11999, $12000-12999, $13000-13999, $14000-14999, $15000-15999, $16000-16999, $17000-17999, $18000-18999, $19000-19999, $20000-24999, $25000-29999, $30000-34999, $35000-39999, $40000-44999, $45000-49999, $50000 and over.

[13]Although family income is recorded in the NHIS, the survey does not give guidelines as to what constitutes a family. In the PUMS, families are defined as two or more related individuals living together. Single people without children in the PUMS are assigned family income equal to zero because–technically–they are not part of a family. Respondents in the NHIS clearly interpret family income differently because single people in the NHIS report positive family income. Since family income is the only income variable available in the NHIS and family income in the PUMS is not applicable for single people, we construct relative deprivation using the household income variable in the PUMS and the family income variable in the NHIS.

at a level that varies by state, topcoded individuals are assigned household income equal to the median household income in their state given that income is greater than the topcode value.[14] We restrict our sample from the PUMS to male householders and male spouses over the age of 20.[15] This gives us a sample of 3,316,833. We then use this information to construct relative deprivation measures for each individual in the NHIS data set. Reference groups are defined using in four different ways: 1) state only, 2) state and age-group, 3) state, age-group, and race, 4) state, age-group, race, and education.[16] These moments are then merged into NHIS/MCOD files for the years 1988-1991.[17] Our final data set contains NHIS/MCOD observations for the years 1988-1991 linked to the mean and standard deviation of reference-group log income from the 1990 PUMS. We restrict our sample to include men only[18] over the age of 20 who have non-missing data for family income, age, education, and race in the NHIS. The total number of observations in our linked data set is 122,504. For the final regression analysis, we only use NHIS respondents for whom the PUMS reference group contained at least 50 observations. This causes sample size to diminish slightly in the more stratified reference groups.

Table 1 reports descriptive statistics for our sample. Because of vast differences in mortality across age groups, we will estimate models that explain mortality for two groups: those aged 21-64 and those 65 and above. Accordingly, we report descriptive statistics for these two subsamples. Among those under 65, about 2.5 percent die in the five years following the initial NHIS interview while the number is roughly 10 times this value for those 65 and above. Annual incomes are about $10,000 higher in the younger group. Relative deprivation (YRD, RDL, RDI, and centile rank[19]) behaves as we would expect—average relative deprivation is highest when reference groups are defined over states only. This

[14]Information on topcode imputations for each state can be found on the IPUMS website, http://www.ipums.org/usa/volii/topcode_odd.html.

[15]The sample is restricted to householders and spouses to avoid counting two observations from the same household.

[16]Age groups are recorded in 5-year increments, 21-25, 26-30, etc. The final age group, 86 and over, is open-ended. Race is defined as white non-Hispanic, black non-Hispanic, other non-Hispanic, or Hispanic. Education is high school dropout, high school graduate, some college, or college grad.

[17]In the interest of confidentiality, a randomized state indicator replaces state identification codes after the merge.

[18]Women are excluded from the analysis because, since women are less likely to work than men, relative income deprivation may be a less accurate measure of status for women than it is for men.

[19] Centile rank is inversely related to the other measures of relative deprivation

result is predictable; as reference groups become more similar, income differences become less pronounced. The same pattern holds for the standard deviation of relative deprivation—it diminishes as reference groups become more narrowly defined.

B. The Model

For our baseline results, we estimate the following equation using a weighted linear probability model:

$$(5) \quad \textit{Died in } 5_{ir} = \beta_o + \beta_1 RD_{ir} + \sum_{k=1}^{26} income_{kir}\Theta_k + \delta_r + X_{ir}\Gamma + \varepsilon_{ir}$$

Where Died in 5_{ir} is a binary variable indicating whether or not the individual died within 5 years of the NHIS interview, and i and r are subscripts for individual and reference group, respectively. RD is one of the four relative deprivation measures discussed above. The income distribution to construct relative deprivation is taken from the 1990 PUMS, and the individual income used to construct the relative deprivation measure is set at the midpoint of the income interval from the NHIS (e.g., for the $0-1000 category, income is set at $500). For the topcoded category in the NHIS (income≥$50,000), individual income is set at the reference group conditional mean income given that income is greater than or equal to $50,000. This conditional mean is taken from the PUMS. We use imputed income values for the relative deprivation measure only—to control for income independently of the relative income effect, we add a complete set of dummy variables for the 27 income categories. The independent income effect is captured in the term $income_{kir}$—this term equals 1 if the individual's income is in group k, zero otherwise.

The term δ_r is a reference group fixed-effect, meant to capture persistent differences across reference groups. Finally, X_{ir} is a vector of dummy variables that control for individual-specific characteristics such as age, education, and marital status. The set of variables that enter both δ_r and X_{ir} change depending on how reference groups are defined. In the first model, where reference groups are defined by state alone, δ_r is a state fixed effect, and X_{ir} contains all of the other demographic dummy variables including age group, race, and education. As reference groups become more narrowly defined, the relevant variables are moved out of X_{ir} and entered into δ_r as interaction terms. For instance, when reference groups are defined using state and age group, δ_r becomes a state*age-group interaction term, and age effects are no longer included in X_{ir}.

C. Basic Results

In keeping with earlier literature (Kitigawa and Hauser 1973; McDonough Duncan et al. 1997; Miller and Paxson, 2000), we report separate results for men who are less than 65 and men who are 65 and over. In the top-half of Table 2, we report the baseline results estimated from equation (5) for men ages 21 to 64. Even after controlling for individual income and a number of covariates, relative deprivation appears to have a large and statistically significant impact on the probability of dying in all but the centile rank models. The relative deprivation effect varies depending on the measure and how reference groups are defined, and the weakest effect is found where reference groups are broadly defined using state of residence. This result is intuitive, since when reference groups are more finely defined, they contain, in the words of Deaton, a larger fraction of relevant to irrelevant members. Overall, the relative deprivation effect is quite substantial. The coefficients are largest in the state/age-group models, where the effect of a one standard deviation increase in YRD appears to increase mortality anywhere from 38.9 percent (in the RDI model) to 120.8 percent (in the RDL model). These results seem sizeable, but they are not inconsistent with other literature on socioeconomic status and mortality. For instance, Marmot (1986), found that British civil servants from the lowest socioeconomic class were 3 times more likely to die than their high-status counterparts. As another example, in a linear probability model where relative deprivation is not included as a covariate, moving from \$40,000 to \$10,000 doubles the probability of death.

In contrast, the results using centile ranks as the measure of relative deprivation suggest that absolute position in the income distribution is positively related to mortality – holding income constant, those in a higher percentile of the income distribution have higher 5-year mortality rates. This result is statistically significant in all but the model where reference groups are defined by state. These results are hard to interpret from the standpoint of “relative deprivation.” If incomes above person i’s increase, their centile rank does not change and therefore there is no impact on mortality.

In the lower half of Table 2, we report estimates from equation (5) for men 65 and over. Here the coefficients on relative deprivation have the expected sign in the YRD and RDL models. However, the results for men 65 and over are not as precise as the results for the younger men, and we cannot rule out the null hypothesis that relative deprivation has no effect. The large standard errors may be caused, in part, by the relatively small sample size for the older cohorts, which has about one-sixth the number of observations as our under 65 sample. Ignoring statistical significance, the effect of a one-standard deviation increase in income would not be as substantial as it is for men aged 21-64. The largest impact is found for RDL in the state/age-group/race models, where a one-standard deviation increase in relative deprivation would increase the probability of death by 7.8 percent. This result is consistent with Kaplan et al., who find that inequality is more strongly correlated with mortality for younger-aged men.

It bears mentioning that our results are potentially biased down due to a “harvesting effect” if relative deprivation in an earlier portion of a person’s life increased mortality and therefore prevented people from entering our sample. Another potential problem is the fact that the NHIS only surveys the non-institutionalized population. This omission will bias our results downwards if relative deprivation leads to sickness, which in turn leads to institutionalization and then death. However, if deprived individuals are less likely to enter nursing homes than individuals who are not deprived, our results will be biased upwards. Finally, our results might be biased downwards if there are health-related externalities, such as lower susceptibility to contagious diseases that accumulate as others become wealthier.

One concern about the above results is that they may be sensitive to the way we construct relative deprivation measures for topcoded individuals. The income variable in the NHIS is topcoded at $50,000, and imputing the topcoded values using the conditional mean from the PUMS may provide a very rough estimate of actual income. Thus, we tried several tests to ensure that our results were unaffected by the way we imputed topcoded values. For example, in one case, we interacted the relative deprivation term with a dummy variable for whether or not the individual’s income was topcoded. In other models, we used fixed incomes for all topcoded values. None of these adjustments changed our basic conclusions.

A second concern is that the use of linear probability models may be inappropriate, especially for the younger age group where the probability of death is quite low (see Greene (1997) for an overview). Eibner (2001) shows that the basic results are unchanged when we use a logistic model for this limited dependent variable.

D. The Effect of an Increase in Income

If we incorporate relative deprivation into a theory about how income affects health, an increase in individual income should have two effects. First, an increase in income decreases relative deprivation which in turn, benefits health. Income may also have a direct impact on health. Much of the existing literature on absolute income and mortality suggests that an increase income should improve health outcomes, although the pathways by which income improves health are not known. However, several recent papers draw this notion into question. Ruhm (2000) finds that mortality is procyclical, i.e. mortality rates rise when the economy is doing well. Ruhm argues that certain risky behaviors, such as drinking and driving, are more prevalent when times are good. In his conclusion, Ruhm suggests that permanent income may be protective of health, but transitory income is deleterious. Similarly, Deaton and Paxson find that increases in income may actually be detrimental to the health of young men aged 25-39. They argue that certain diseases of affluence, such as AIDS and alcoholism, may be positively related to additional income. Evans and Snyder (2001), find that higher incomes generated by the Social Security "notch" actually increased mortality among the elderly.

To examine the effect of a change of income, we would like to compute the derivative of the probability of death with respect to a change in income. However, since we control for income in our models by including a full set of income dummy variables, it is impossible to take the derivative directly. Instead we calculate the probability of death for a representative individual at each of the 27 possible income levels in the NHIS. The probability of death given that income equals k (ignoring X_{ir} and the state fixed effect) is simply[20]:

[20] We ignore the intercept, $X_{ir,}$ and the state fixed effect in equation (6) because they are held constant in this

(6) $P(D_{ir} = 1 \mid Income = k) = \beta RD_{ir|k} + \Theta_k$

Where β is the coefficient on relative deprivation from equation (5), $RD_{ir|k}$ is the individual's relative deprivation at income k, and Θ_k is the coefficient on the dummy variable representing income category k. Figure 1 shows the probability of death for a white male aged 31-40 with a high school education as he moves across the 27 income categories in the NHIS. Since we cannot identify specific states in the NHIS, we average the relative deprivation effect across all states. Figure 1 confirms the notion that income protects health. Moreover, the effect of an increase in income is remarkably similar regardless of how relative deprivation is specified. The curves for YRD, RDL, and RDI almost completely overlap.[21] The centile rank curve follows the same pattern, but at a slightly higher level than the other curves. This may reflect a difference in the intercept for the centile rank regression. If one interprets the link between relative deprivation and mortality as causal, all of these results suggest that income is protective of health, and that the mechanism by which income is beneficial may be relative deprivation.

E. Cause-Specific Mortality

One reason that relative deprivation may be linked to mortality is that individuals who feel deprived may be particularly likely to engage in health-compromising behaviors, such as smoking. If the link between behavior and relative deprivation were correct, we would expect relative deprivation to have an especially pronounced effect on mortality that is strongly linked to behavior. To test this conjecture, we re-estimate equation (1) using cause-specific mortality as opposed to all-cause mortality as our outcome of interest. The four causes of death that we investigate are coronary heart disease (CHD), tobacco-related cancers,[22] all other cancers (non-tobacco related), and accidents/external events.[23]

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analysis. Thus P can be thought of as the probability of death attributable to income.

[21] We drop the probability of death for the lowest income group in the RDI model because this value was an extreme outlier with a very large standard error.

[22] This category includes malignant neoplasms of the lip, oral cavity, and pharynx (ICD-9 codes 140-149), and malignant neoplasms of respiratory and intrathoracic organs (ICD-9 codes 160-165).

[23] This category includes motor vehicle accidents, other accidents, suicide, homicide, legal intervention, and other external causes (ICD-9 codes E800-E899).

Cigarette smoking is the direct cause of 87 percent of all lung cancer cases, and the surgeon general calls smoking "the most important of the known modifiable risk factors for coronary heart disease" (American Heart Association, 2000). Cigarette smoking is also linked to cancers of the oral cavity, as is smokeless tobacco. The American Cancer Society (ACS) estimates that about 90 percent of people with cancers of the oral cavity and oropharngeal cancers are tobacco users (ACS, 2000). While moderate alcohol consumption is protective against heart disease, alcohol is associated with various cancers including cancers of the esophogus, larnyx, and oral cavity. Moreover, alcohol consumption is associated with motor vehicle accidents. The National Highway Traffic Safety Administration (NHTSA) reports that drunk driving is responsible for about 39 percent of all traffic fatalities nationwide (NHTSA, 2000).

Tables 3.A and 3.B show results for the cause-specific mortality models. For CHD and tobacco-related cancers, the relative deprivation effect is statistically significant and proportionately stronger in magnitude than it was in the all-cause mortality models. Looking at all heart disease mortality for the younger age group, a one-standard-deviation movement in YRD increases the probability of death by 60 to 107 percent in the YRD models and 100 to 163 percent in the RDL models. Coefficients are statistically significant and of the expected sign in two of the three RDI models, though not in the centile rank models. Likewise for the older age group (top panel, table 3.B), the coefficients on relative deprivation generally have the expected signs in the YRD, RDL, and RDI models. However for men 65, and over the standard errors on the relative deprivation coefficients are large. As with the results for all-cause mortality, the magnitude of the relative deprivation effect is proportionately smaller for the older age group. Using the largest coefficient on RDL in the CHD models (0.0694 when reference groups are defined by state and age), a one-standard-deviation movement in deprivation would increase the probability of death due to CHD by about 33 percent.

Results for tobacco-related cancers (the second panel of tables 3.A and 3.B) are similar to the results for heart disease. Relative deprivation has a larger effect (in percentage terms) than it did in the all-cause mortality models, and the effect is greater for the younger age group. Again, the results for the 65 and older group are less precise than the results for the younger men. Upper-bound estimates for the magnitude of the relative deprivation effect imply that a one-standard-deviation increase in RDL could increase the probability of death due to tobacco-related cancer by as much as 168 percent for the younger age-group, and by as much as 67 percent for the older age-group.

For mortality due to all other cancers (the second panel of tables 3.A and 3.B) accidents and adverse effects (panel 4, tables 3.A and 3.B), relative deprivation does not seem to play as much of a role as it did for the other causes. For both young and old the coefficients in these panels are statistically imprecise, and the sign of the coefficients vacillates from model to model. The statistical insignificance of the results is not surprising in the non-smoking related cancers model, since these cancers are less related to behavior than mortality due to lung cancer and coronary heart disease. While a large percentage of traffic fatalities are linked to alcohol consumption, our accidental death variable includes other causes such as fires, poisonings, prescription drug errors, and homicide.

F. Results by Race

Miller and Paxson (2000) and Deaton and Lubotsky (2001) suggest that the interplay between Black and White incomes in a state (or reference group) may be an important component of the relative deprivation hypothesis. Miller and Paxson find that large income differentials between Blacks and Whites may increase mortality among Blacks. Similarly, Deaton and Lubotsky find that aggregate White mortality is positively related to the fraction of Blacks within a state. These results suggest that relative deprivation may have a differential impact on Blacks and Whites, and that the impact of relative deprivation might vary depending on whether race is a component of reference group determination. To examine whether or not relative deprivation impacts Blacks and Whites differentially, we estimate the linear probability from Table 2 separately for each race. Coefficients are reported in Table 4 for the RDL models only. In the previous tables we find the most sizeable relative deprivation effects in the RDL models, so the coefficients in Table 4 can be taken as upper bound estimates.

For those aged 21-64, the change in mortality generated by an increase in relative deprivation is nearly the same for blacks and whites. The coefficients for Blacks are larger than the coefficients for Whites in absolute magnitude, but Blacks have a higher probability of death than Whites. In the state/age-group/race models, a 0.5 increase in RDL increases the probability of death for Whites by 123 percent and the probability of death for blacks by 96 percent. The impact is higher for whites, but not dramatically so. In contrast, we find a large impact of relative deprivation on Blacks aged 65 and over but unconvincing evidence of an impact of deprivation for older Whites. Among older Whites, the relative deprivation coefficient is positive and statistically significant in the first three reference groups but the coefficient is negative and statistically imprecise when reference groups are defined by state/age/race/education.

G. Other Health Outcomes

Mortality is a convenient measure of health because it is easily observable and precisely measured. However, there are some drawbacks to using mortality as our primary outcome. Death is a rare event for younger people. Further, it is possible that relative deprivation might have an adverse impact on morbidity without directly affecting mortality. To explore this issue we look at three additional outcomes: self-reported health status, limited activity status, and high blood pressure.

Both self-reported health status and limited activity status are measures that can be taken from the NHIS. Self-reported health status is a categorical variable with five possible outcomes: excellent, very good, good, fair, and poor. Following Mellor and Milyo (1998), we construct a binary variable that equals one if the individual reports fair or poor health. Studies have shown that self-reported health status is highly correlated with mortality (Idler and Benyamini, 1997). Limited activity status measures whether or not the individual is physically restricted or unable to perform activities, which might include work, school, or other pastimes. The question has four possible responses: (1) unable to perform major activity, (2) limited in kind/amount of major activity, (3) limited in other activities, (4) not limited. We create a binary variable that is equal to one if respondents report any limitation. In total, 16.6 percent of our sample reports being limited in some capacity.

Blood pressure is not measured in the NHIS, so we use blood pressure data from the Behavioral Risk Factor Surveillance System (BRFSS), an annual survey conducted by the Centers for Disease Control. The blood pressure question in the BRFSS reports whether an individual was ever told by a health care professional that is his blood pressure was high. We restrict the BRFSS sample to include men over the age of 20, and we use data from 1988, 1990, and 1991.[24] This yields a total of 94,644 records, but after limiting our sample to men who respond to the blood pressure question and who have non-missing data for relevant control variables, we are left with 85,841 observations. In this sample, 21.1 percent report that they have ever been told by a health care professional that they have high blood pressure. The blood pressure question in the BRFSS raises concerns about sample selection, because the very sick and the very diligent are more likely to have doctor check-ups. However, clinical data from the Third National Health and Nutrition Examination Survey shows that a similar fraction of men over the age of twenty–24.8 percent–had high blood pressure.[25]

In Table 5, we report results from weighted linear probability models on the three non-mortality outcomes discussed above. The models are identical to equation (5), except the dependent variable is fair/poor self-reported health, limited activity status, or high blood pressure. The covariate of interest is RDL. Two interesting patterns emerge from these regressions. First, for all three outcomes, the sign of the coefficient is negative when reference groups are defined using state only. For both self-reported health status and limited activity status, the coefficient is statistically significant. This result is reiterated in the next section where we find that health-compromising behavior is often negatively related to relative deprivation when reference groups are defined using state only. While it is not clear why this counterintuitive result manifests itself for a number of different outcomes, it may be related to the fact that state alone is a poor proxy for the individual's true reference group. Deaton (2001) argues that the nation as a whole might be a reasonable reference group, because people are exposed to diverse living standards through the national media. Alternatively, he suggests that small localities or neighborhoods might be plausible reference groups, because individuals compare themselves to others in their immediate vicinity. However, he argues that "state as a reference group is less plausible then either the nation or the locality" because states are too large to permit individual comparison between residents. This notion is supported by the fact that, in all of the mortality regressions described above and in many of the behavior regressions discussed below, the relative deprivation effect is weakest when reference groups are

[24] A more-detailed description of the BRFSS data is available in the next section.

[25] Authors' calculations from the Third National Health and Nutrition Examination Survey. This survey contains data from 1988-1994.

characterized by state only.

A second finding from Table 5 is that, when reference groups are defined more narrowly (columns 2-4), relative deprivation is positively related to infirmity, and the coefficients are precisely estimated. The first panel of the table suggests that a one-standard deviation increase in relative deprivation might increase the probability that an individual reports having poor or fair health by 9.7 to 43.0 percent. Similarly, a one-standard deviation increase in relative deprivation might increase the probability of being limited in one's activities by as much as 100 percent, or the probability of having high blood pressure by as much as 20 percent.

VII. Relative Deprivation and Health Compromising Behavior

McGinnis and Foege (1993) estimate that, in 1990, fifty percent of all mortality in the United States could be attributed to behavioral factors. Smoking caused twenty percent of this mortality, however other risky behaviors such as poor diet, alcohol abuse, and dangerous driving played a role as well. In the previous section, we show that relative deprivation is associated with an increased risk of mortality, especially causes of death linked to behavior. If relative deprivation impacts mortality by increasing the probability that an individual takes health risks, then we would expect to see a direct link between relative deprivation and health-compromising behavior. In this section, we use data from the Behavioral Risk Factor Surveillance System (BRFSS) to determine whether increases in relative deprivation increase the probability that an individual takes health risks.

A. Data

The data for this section come from the Behavioral Risk Factor Surveillance System (BRFSS) for the years 1989-1991. We choose these years so that the survey can be matched to information on the distribution of income from the 1990 Public Use Micro Data. The BRFSS is an on-going telephone survey conducted by the states and supported by the Centers for Disease Control (CDC). Households are telephoned at random, and a series of questions are asked to a randomly selected adult member of the household. BRFSS data is available for the years 1987 to the present. While initially only 15 states conducted BRFSS surveys, by 1990, forty-four states and the District of Columbia participated.[26] Together the 1989-1991 BRFSS surveys contain 236,270 observations, but after limiting our sample to men over the age of 20, we have 94,644 records. In addition to basic demographic data, the BRFSS contains measures of seatbelt use, exercise habits, body mass index, current and former smoker, and excessive drinking.

B. Estimation Issues

The empirical model for this section is identical to the one we used for the mortality regressions in the previous sections. One of the behaviors we examine, body mass index, is measured as a continuous variable. For the remaining behaviors, such as whether or not an individual smokes, outcomes are discrete and we will use a linear probability model. Relative deprivation is constructed using RDL[27] and centile rank, and the moments of the reference group income distribution are taken from the 1990 PUMS. Reference groups are constructed using the four definitions from above. Like many data sets that contain information about health, the BRFSS reports income as a categorical variable. In the BRFSS, household income is reported as a 7-level variable with a topcoding value of $50,000. As in the previous section, we include a full set of dummy variables for all income categories. We handle topcoded incomes in a manner similar to that discussed in the previous section.[28]

C. Results

Smoking: The Surgeon General calls tobacco consumption the “number one preventable cause of disease and death in the United States” (U.S. Department of Health and Human Services, 2000). Current smokers have over twice the mortality risk of non-smokers (McGinnis and Foege, 1993, U.S. Department of Health and Human Services, 1990), and there is a well-established link between smoking and lung

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[26] States that did not participate were Alaska, Nevada, Wyoming, Kansas, Arkansas, and New Jersey.

[27] Results from the other measures (YRD and RDI) were similar to the RDL results.

[28] Because we had more flexibility to merge and add variables in the non-restricted use data, we were able to impute topcoded incomes in the BRFSS by regressing incomes over $50,000 in the PUMS on age, race, education, marital status, and state of residence. We then used the regression coefficients to impute income for topcoded individuals in BRFSS.

cancer. Smoking is also associated with other forms of morbidity and mortality, including heart disease and emphysema. In the first rows of Tables 6.A and 6.B, we present parameter estimates for the relative deprivation variable from linear probability models where the outcome of interest is current smoking status from the BRFSS. Twenty-nine percent of men 21-64 report that they currently smoke (table 6.A), while 15 percent of the older men are current smokers (table 6.B). For younger men, relative deprivation is significantly and positively associated with the probability that an individual smokes in all of the RDL models. These models predict that a one-standard deviation increase in RDL will increase the probability of smoking by 13 to 17 percent. Unlike the results for mortality, there is some evidence that increases in centile rank are associated with decreases in the probability of smoking. The coefficients on centile rank are negative and statistically significant in the state/age-group and state/age-group/race models, and of similar magnitude to the RDL results. A one standard deviation decrease in centile rank (which represents an increase in relative deprivation) increases the probability of smoking by about 11 percent. As with the mortality results, the coefficients for the older group (reported in table 6.B) are almost always statistically insignificant.

While the theory predicts that relative deprivation should cause individuals to be current smokers, there is no reason to expect that relative deprivation should affect previous smoking history. Evans, Farrelly and Montgomery (1999) show that 90 percent of people who ever smoke begin smoking by age 20. Therefore, in a sample of adults, we should find no correlation between "ever smoked" and current measures of relative deprivation. As a specification check, we regress an indicator for whether or not an individual ever smoked on our relative deprivation measure and other controls. Results are reported in panel 2 of tables 6.A and 6.B.[29] For younger aged men, the results are negative and statistically significant in two of the RDL models, implying that relative deprivation decreases the probability of ever smoking. The centile rank results are also negative and statistically significant in two of the models, and since centile rank is inversely related to relative deprivation, these results imply that relative deprivation increases the probability of smoking. The remaining four coefficients reported in panel (2) of table 6.B

[29]Ever smokers are defined as individuals who report that they smoked at least 100 cigarettes in their lifetime.

are statistically imprecise. Overall, we take this as evidence that relative deprivation is not systematically related to the probability of ever having smoked. For older men (panel 2 of table 6.B), the coefficients on relative deprivation are statistically imprecise in the majority of the regressions.

Body Mass Index and Exercise Habits: Obesity is another health risk factor that is related to behavior through lack of exercise and consumption of fattening foods. Individuals who are obese or overweight are at a significantly higher risk for mortality due to heart disease, stroke, type 2 diabetes, hypertension, and various other causes. To measure obesity, we use body mass index (BMI) which is a height to weight ratio defined as follows:

$$(7) \quad BMI = \frac{(Weight\ in\ kgs)}{(Height\ in\ meters)^2} = (703) * \frac{(Weight\ in\ lbs)}{(Height\ in\ inches)^2}$$

Using the height and weight variables recorded in the BRFSS, we can construct a continuous measure of BMI to evaluate in the framework outlined in equation (5). We limit the sample to exclude men with BMI values less than 16 or greater than 59.[30] The average BMI for both older and younger men in our sample is slightly over 25. According to the Centers for Disease Control (CDC, 2001), a BMI value between 18.5 and 24.9 is healthy, a BMI value between 25 and 29.9 is overweight, and a BMI value greater than 30 is obese. Height and weight in the BRFSS and the NHIS are self-reported, so one might reasonably suspect BMI to be biased downward. However, the average BMI for men 21 and over in the Third National Health and Nutrition Examination Survey (1988-1994), where all body measurements were recorded by trained professionals, is 26.65–only slightly higher than the means in the BRFSS.[31] Panel (3) of tables 6.A and 6.B show the results found after regressing BMI on relative deprivation and the covariates from the BRFSS. Results are statistically significant for the younger aged men in all of the RDL models. These coefficients suggest that a one standard deviation increase in RDL might increase

[30] BMIs less than 16 or greater than 60 are unusual and, in many cases, due to coding errors (e.g. one observation in the BRFSS had a recorded height of 6 inches). A 6-foot man would need to weigh 442 pounds to have BMI of 60, or 118 pounds to have a BMI of 16.

[31] Authors' calculations.

BMI for men aged 21-64 by about 1.4 percent.

A key contributor to obesity is lack of exercise. Sedentary behavior is harmful not only because of it's impact on weight, but physical inactivity alone is shown to be an independent risk factor for cardiovascular mortality and coronary artery disease (Blair, et al. 1996, Morris et al., 1990). Individuals who exercise are also at a lower risk for high blood pressure and certain types of cancer (Lee, 1994, Kampert et al., 1996). Moreover, exercise is shown to decrease depression in men, and to increase feelings of self-confidence and self-esteem (Lobstein, Moscbacher, and Ismail, 1983, Crews and Landers, 1987). Using data from the BRFSS, we can examine how relative deprivation relates to exercise habits.

Panel (4) of tables 6.A and 6.B highlights the results found by using a binary indicator for whether or not an individual exercised in the past month as the dependent variable in the framework discussed above. For both younger and older men, the results are statistically significant and of the expected sign for three of the four RDL models. A one standard deviation increase in RDL might decrease the probability that an individual ever exercises by 7-14 for either age group. However for the older group, the coefficients on centile rank are also statistically significant and negative, implying a counterintuitive effect. For the younger men, results are statistically significant and counterintuitive in sign for the state only model.

Seatbelt Use: The U.S. Department of Transportation reports that wearing a lap or shoulder belt can reduce the risk of being fatally injured in a car crash by as much as 45 percent (NHTSA, 2001). Due largely in part to state laws requiring seatbelt use that were enacted during the 1980s, the protective effects of seatbelt use are well known. Rates of seatbelt use in this country climbed from 10-15 in the early 1980s to 69 percent in 1997, largely because public knowledge of the importance of seat belt use increased during this time period. If we believe that motorists are aware that seatbelt usage can prevent fatalities, then the act of not wearing a seatbelt can be equated with an increased willingness to accept risk (Hersch and Viscusi, 1990).

We can examine whether relative deprivation is associated with a lower probability of seatbelt use using data from the BRFSS survey. The BRFSS contains a seatbelt use question that asks whether an individual always, nearly always, sometimes, seldom, or never wears a seatbelt. Following Dee (1998), we create a binary indicator variable for seatbelt use that is equal to 1 if the individual always wears a seatbelt, and zero otherwise. Dee explains that this construction generates a rate of seatbelt use that closely matches the rate observed at randomly selected intersections throughout the United States.[32] In the BRFSS data, 54 percent of men aged 21-64 and 58 percent of men aged 65 and over report that they always wear a seatbelt. Panel (5) of table 6.A and 6.B summarizes the results found by regressing seatbelt use on relative deprivation and other covariates. Results are statistically significant for younger men when relative deprivation is measured using RDL. As in the ever exercise models, when reference group is defined using state only we get the counterintuitive result that relative deprivation increases the probability that an individual always wears a seatbelt. However, the sign on the relative deprivation coefficient reverses when reference groups are defined more narrowly. In the state/age, state/age/race, and state/age/race/education models, the coefficients on relative deprivation are negative and statistically significant, indicating that an increase in relative deprivation decreases the probability that an individual always wears a seatbelt. As an upper bound, these estimates imply that a one standard deviation increase in relative deprivation might decrease the probability of wearing a seatbelt by ten percent.

VIII. Conclusion

Researchers in the social sciences are increasingly concerned about the interplay between income inequality, relative deprivation, and health. Yet studies of these relationships are difficult to conduct, mainly because of a lack of appropriate individual-level data linking health outcomes to income and reference group information. In this paper, we use unique data from the NHIS/MCOD restricted-access files that allow us to observe income, mortality, and state of residence at the individual level. With these data we can examine the relationship between relative deprivation and mortality while simultaneously controlling for individual income and reference group fixed effects. We find that relative deprivation has

[32] The observed data comes from the National Highway Transportation Safety Administration. Seatbelt use was observed in 19 cities: Atlanta, Baltimore, Birmingham, Boston, Chicago, Dallas, Fargo/Moorhead, Houston, Los Angeles, Miami, Minneapolis/St. Paul, New Orleans, New York, Phoenix, Pittsburgh, Providence, San Diego, San Francisco, and Seattle.

a strong, positive, and statistically significant impact on the probability that an individual dies within 5-years of the NHIS survey. Our results are particularly pronounced for all men aged 21-64 and for Black men aged 65 and over.

Our results paint a consistent picture of the impact of relative deprivation on health. From a theoretical standpoint, relative deprivation is thought to impact health via risky behavior. We find that for heart disease mortality and tobacco-related cancers, the relative deprivation effect is proportionately stronger than it was in the all-cause mortality models. Likewise, we examine relative deprivation's impact on various health habits using data from the BRFSS. We find that relative deprivation increases the probability that an individual smokes and decreases the probability that an individual wears a seatbelt. Further, we find that relative deprivation is positively associated with body mass index and negatively associated with the probability of exercise. Too our knowledge, this is the first work to look specifically at behavior and relative deprivation.

We should stress however that these results are only suggestive of a causal link between relative deprivation and poor health. It is possible that our results simply reflect a statistical correlation. For example, Fuchs (1982) has long argued that the persistent differences in health socioeconomic status can be generated by differences in the discount rate. People with high discount rates are less likely to invest in projects where returns are not realized until long in the future. Fuchs argues that a high discount might be reflected in two separate decisions: low investment in both human capital and health capital. Subsequently, the positive relationship between income and health may not be causal. Rather, the same types of people who do not invest in human capital are also those who do not invest in health. Fuchs's hypothesis may explain the results we find above. Our results indicate that people who are performing poorly financially within their reference group are more likely to die early and more likely to have poor health habits. Within a reference group, it may also be the case that those who are lagging financially are those who have not invested relative to their peers. For example, although most lawyers would report the same years of education, some spend more hours working earlier in their careers than others. This type of investment may increase earnings later in life. If, because of some unobserved factor such as the discount rate, the people who make relatively large investments in their earnings capabilities were also the people who make large investments in their health, then we would detect a relationship between relative deprivation and mortality. However, this impact would not be causal.

We do find some evidence that is contrary to the Fuchs hypothesis. If the Fuchs hypothesis is correct, we would expect individuals with high-discount rates to be more likely to have smoked at some point in their life. Holding reference group constant, we would also expect high-discount rate people to be performing worse financially relative to their peers. Since the decision to "ever smoke" is made early on in one's life, a contemporaneous correlation between relative deprivation (say at age 50) and whether a person ever smoked (a decision made before age 21) would certainly suggest the models are subject to an omitted variables bias. The lack of a consistent correlation between relative deprivation and whether a person ever smoked gives us some comfort that our models are not subject to this criticism.

Three other results are of special note. First, although relative deprivation has a qualitatively large impact on mortality, all models predict that higher incomes reduce mortality. Indeed, nearly all models regardless of how or whether deprivation is measured predict the same change in mortality from a fixed change in income. Second, most of the previous work using aggregate data defines reference groups at the state level. Across all measures of health, the weakest evidence of the deleterious impacts of relative deprivation is contained in models that define reference groups by state of residence. Finally, relative deprivation seems to have the strongest impact on health when it reflects income differences between individuals as opposed to income rank. When we measure relative deprivation using centile rank, which ignores the magnitude of the income difference between individuals, our results are often statistically imprecise and in many cases, counterintuitive in sign. Yet, when we use relative deprivation measures that quantify income differences between individuals, our results suggest that relative deprivation is linked to mortality, morbidity, and an array of deleterious health habits.

These results hint at a relationship between economic conditions, psychological factors, and individual behavior. The relationship we investigate may provide insights to other observed correlations in the data. For example, that relative income affects health may help explain why the gradient between income and health persists even at high levels of income (Adler et al., 1994; Deaton 2001) or why mortality appears to be pro-cyclic (Ruhm, 2000). Likewise, there may be a link between rising income inequality over the past 25 years and rising inequality in health outcomes (Preston and Elo, 1995) and in health habits (Evans, Ringel and Stech, 1999).

Finding exogenous variation that can establish or falsify a causal link between relative deprivation and mortality may be difficult. In practice, one would need to identify a group of people whose incomes remained constant while incomes in their reference group changed. For example, we are investigating the possibility of using the fact that during the 1980s, wages of high-skilled public sector workers fell relative to their counterparts in the private sector in such a manner. In any event, we believe the future of the research in this area will follow the trend towards individual-level micro data established by Fiscella and Franks (1997) and Mellor and Milyo (1998) and continued in this paper. Since mortality rates are relatively low for those in younger age cohorts, research in the future will probably gravitate towards other measures of health and health habits. Given the results of animal studies cited above, an interesting advance would be to correlate serotonin and basal cortisol concentrations in blood to relative deprivation.

Table 1
Descriptive Statistics, Men 21 and Over, NHIS/MCOD,
Means and Standard Deviations

Variable	State	State and age	State, age and race	State, age, race and educ.
	Reference group defined by			
Males aged 21-64 (104,320 observations)				
YRD/10,000	1.679	1.649	1.579	1.454
	(1.108)	(1.227)	(1.181)	(1.180)
RDL	0.4672	0.4604	0.4470	0.4133
	(0.5289)	(0.5227)	(0.5065)	(0.4776)
RDI	1.48	1.37	1.29	1.13
	(5.90)	(5.58)	(5.31)	(4.94)
Centile Rank	50.34	48.44	49.00	49.05
	(29.58)	(30.60)	(30.30)	(29.72)
% Died in 5 years	2.44	2.44	2.44	2.45
Income	38,237	38,237	38,272	38,432
	(21,659)	(21,659)	(21,657)	(21,645)
Age	39.1	39.1	39.1	39.2
	(11.8)	(11.8)	(11.8)	(11.8)
% White	78.8	78.8	79.1	80.8
Observations	104,320	104,320	103,834	101,577
Males aged 65+ (18,184 observations)				
YRD/10,000	2.33	1.411	1.359	1.256
	(1.10)	(0.794)	(0.781)	(0.907)
RDL	0.7250	0.4924	0.4808	0.4426
	(0.5172)	(0.4262)	(0.4104)	(0.3823)
RDI	1.99	1.23	1.16	0.98
	(3.88)	(2.74)	(2.48)	(2.40)
Centile Rank	32.15	45.71	46.34	45.59
	(26.24)	(27.71)	(27.44)	(27.07)
% Died in 5 years	23.26	23.26	23.23	23.17
Income	25,577	25,583	25,655	25,624
	(18,443)	(18,446)	(18,453)	(18,407)
Age	72.6	72.5	72.5	72.4
	(5.8)	(5.8)	(5.8)	(5.7)
% White	87.2	87.2	88.4	89.9
Observations	18,184	18,177	17,921	17,395

Table 2
Weighted Linear Probability Models, 5-Year Mortality Equations
NHIS/MCOD

Relative Deprivation Measure	Coefficients and standard errors on Relative Deprivation: Reference group defined by State	State and age	State, age and race	State, age, race and educ.
Males aged 21-64 (104,247 observations, 2.44 % Died in 5 Years)				
YRD/10,000	0.0041	0.0120	0.0106	0.0069
	(0.0023)	(0.0014)	(0.0014)	(0.0011)
RDL	0.0235	0.0564	0.0508	0.0359
	(0.0109)	(0.0062)	(0.0057)	(0.0052)
RDI	0.0014	0.0017	0.0013	0.0004
	(0.0005)	(0.0003)	(0.0003)	(0.0003)
Centile Rank	0.0001	0.0005	0.0005	0.0002
	(0.0001)	(0.00009)	(0.00009)	(0.00007)
Males aged 65+ (18,180 observations, 23.36 % Died in 5 Years)				
YRD/10,000	0.0112	0.0149	0.0142	0.0017
	(0.0187)	(0.0170)	(0.0162)	(0.0107)
RDL	0.0121	0.0323	0.0499	0.0085
	(0.0752)	(0.0538)	(0.0483)	(0.038)
RDI	-0.0048	-0.0048	0.0007	-0.0014
	(0.0046)	(0.0043)	(0.0038)	(0.0023)
Centile Rank	-0.0014	-0.0004	0.0002	0.00002
	(0.0011)	(0.0007)	(0.0007)	(0.0005)

The means and sample sizes reported in the table are for the models using states as a reference group. Sample sizes for other reference groups are slightly smaller. Standard errors in parentheses. Unreported covariates include income, age-group, race, education, marital status, family size and year of interview.

Table 3.A
Weighted Linear Probability Models, Cause-Specific 5-Year Mortality Equations
Men 21-64

	Coefficients and standard errors on Relative Deprivation: Reference group defined by			
Cause (% Died in 5 Years)	State Only	State and Agegroup	State, Agegroup, and Race	State, Age, Race, and Educ
CHD (0.66)				
YRD/10,000	0.0035	0.0050	0.0049	0.0027
	(0.0012)	(0.0008)	(0.0007)	(0.0006)
RDL	0.0207	0.0210	0.0214	0.0124
	(0.0058)	(0.0033)	(0.0031)	(0.0028)
RDI	0.00017	0.00028	0.00030	0.00003
	(0.00027)	(0.00015)	(0.00015)	(0.00014)
Centile Rank	-0.0000	0.0001	0.00013	0.00004
	(0.0001)	(0.00005)	(0.00004)	(0.00004)
Smoking Related Cancer (0.29):				
YRD/10,000	0.0000	0.0018	0.0023	0.0014
	(0.0008)	(0.0005)	(0.0005)	(0.0004)
RDL	0.0010	0.0080	0.0096	0.0076
	(0.0038)	(0.0022)	(0.0020)	(0.0018)
RDI	-0.000034	0.00019	0.00013	-0.000033
	(0.00018)	(0.000099)	(0.000098)	(0.000092)
Centile Rank	0.0000	0.0001	0.00007	0.00005
	(0.0001)	(0.00003)	(0.00003)	(0.00003)
All Other Cancers (0.42):				
YRD/10,000	-0.0006	0.0008	0.0005	0.0009
	(0.0010)	(0.0006)	(0.0006)	(0.0005)
RDL	-0.0040	0.0043	0.0033	0.0038
	(0.0046)	(0.0026)	(0.0024)	(0.0022)
RDI	-0.00005	0.0000015	0.000011	-0.000032
	(0.00022)	(0.00012)	(0.00012)	(0.00011)
Centile Rank	0.0000	0.0001	0.00009	0.0000
	(0.0001)	(0.00004)	(0.00003)	(0.00003)
Accidents/Adverse (0.38)				
YRD/10,000	-0.0013	-0.0004	-0.0003	0.0003
	(0.0009)	(0.0006)	(0.0006)	(0.0004)
RDL	-0.0074	-0.0012	0.0010	0.0032
	(0.0044)	(0.0025)	(0.0023)	(0.0021)
RDI	-0.0003	-0.00008	-0.000014	0.00008
	(0.0002)	(0.00011)	(0.00011)	(0.00011)
Centile Rank	-0.0000	-0.00001	0.00002	0.00003
	(0.0001)	(0.00003)	(0.00003)	(0.00003)

The means and sample sizes reported in the table are for the models using states as a reference group. Sample sizes for other reference groups are slightly smaller. Standard errors in parentheses. Unreported covariates include income, age-group, race, education, marital status, family size and year of interview.

Table 3.B
Weighted Linear Probability Models, Cause-Specific 5-Year Mortality Equations
Men 65 and Over

	Coefficients and standard errors on Relative Deprivation: Reference group defined by			
Cause (% Died in 5 Years)	State Only	State and Agegroup	State, Agegroup, and Race	State, Age, Race, and Educ
CHD (8.91)				
YRD/10,000	0.0060	0.0182	0.0129	0.0103
	(0.0129)	(0.0117)	(0.0111)	(0.0073)
RDL	0.0184	0.0694	0.0490	0.0398
	(0.0516)	(0.0369)	(0.0331)	(0.0261)
RDI	0.0009	-0.00002	0.0036	0.0009
	(0.0032)	(0.0029)	(0.0026)	(0.0018)
Centile Rank	0.0007	0.0011	0.0012	0.0004
	(0.0008)	(0.0005)	(0.0005)	(0.0004)
Smoking-Related Cancers (2.31)				
YRD/10,000	0.0059	0.0110	0.0091	0.0005
	(0.0069)	(0.0063)	(0.0060)	(0.0040)
RDL	0.0044	0.0363	0.0220	0.0030
	(0.0277)	(0.0199)	(0.0178)	(0.0142)
RDI	0.0000	0.0010	0.0007	0.00006
	(0.0017)	(0.0016)	(0.0014)	(0.00096)
Centile Rank	-0.0009	-0.0004	-0.0003	-0.0001
	(0.0004)	(0.0003)	(0.0003)	(0.0002)
All Other Cancers (3.91)				
YRD/10,000	-0.0050	-0.0123	-0.0078	-0.0104
	(0.0089)	(0.0081)	(0.0077)	(0.0051)
RDL	-0.0221	-0.0393	-0.0130	-0.0381
	(0.0357)	(0.0256)	(0.0230)	(0.0182)
RDI	-0.0013	-0.0023	-0.00099	-0.0012
	(0.0022)	(0.0020)	(0.0018)	(0.0012)
Centile Rank	-0.0007	-0.0003	-0.0002	-0.0000
	(0.0005)	(0.0003)	(0.0003)	(0.0002)
Accidents/Adverse Events (0.61)				
YRD/10,000	0.0011	0.0010	-0.0004	0.0014
	(0.0036)	(0.0032)	(0.0031)	(0.0020)
RDL	0.0130	0.0057	-0.0003	0.0066
	(0.0143)	(0.0101)	(0.0092)	(0.0072)
RDI	-0.00005	-0.000017	0.00014	0.0003
	(0.0009)	(0.0008)	(0.0007)	(0.0005)
Centile Rank	0.0002	0.0001	0.00021	0.0002
	(0.0002)	(0.0001)	(0.00013)	(0.0001)

The means and sample sizes reported in the table are for the models using states as a reference group. Sample sizes for other reference groups are slightly smaller. Standard errors in parentheses. Unreported covariates include income, age-group, race, education, marital status, family size and year of interview.

Table 4
Weighted Linear Probability Models, 5-Year Mortality Equations
NHIS/MCOD

			Coefficients and standard errors on RDL: Reference group defined by			
Racial group	% Died in 5 years	Obs.	State	State and age	State, age and race	State, age, race and educ.
			Males aged 21-64			
Whites	2.33	81,161	0.0275	0.0619	0.0573	0.0396
			(0.0126)	(0.0072)	(0.0068)	(0.0058)
Blacks	3.79	11,341	0.0635	0.0674	0.0729	0.0360
			(0.0424)	(0.0229)	(0.0247)	(0.0220)
			Males aged 65+			
Whites	23.09	15,381	0.0447	0.0339	0.0213	-0.0246
			(0.0817)	(0.0605)	(0.0591)	(0.0416)
Blacks	27.55	1,882	0.4343	0.5369	0.4253	0.2785
			(0.3221)	(0.2018)	(0.1659)	(0.1893)

The means and sample sizes reported in the table are for the models using states as a reference group. Sample sizes for other reference groups are slightly smaller. Standard errors in parentheses. Unreported covariates include income, age group, education, marital status, family size and year of interview.

Table 5
Weighted Linear Probability Models, Health Status Equations
NHIS/MCOD

			Coefficients and standard errors on RDL: Reference group defined by			
Health outcome	% answering yes	Obs.	State	State and age	State, age and race	State, age, race and educ.
Fair or poor health?[a]	16.63	123,969	-0.1759	0.1431	0.1284	0.0322
			(0.0187)	(0.0103)	(0.0097)	(0.0090)
Limited in activity?b[b]	11.15	123,665	-0.1101	0.2320	0.2358	0.1077
			(0.0224)	(0.0123)	(0.0117)	(0.0108)
High blood pressure?[c]	21.16	85,841	-0.0190	0.0788	0.0849	0.0336
			(0.0314)	(0.0174)	(0.0167)	(0.0154)

The means and sample sizes reported in the table are for the models using states as a reference group. Sample sizes for other reference groups are slightly smaller. Standard errors in parentheses. Unreported covariates include income, age group, race, education, state of residence, marital status, family size, and year of interview.

a. The dependent variable is equal to one if individuals report being in poor or fair health. The dependent variable is equal to zero if the individual reports being in good, very good, or excellent health.
b. The dependent variable is equal to zero if the individual reports being unable to perform his major activity, limited in ability to perform major activity, or limited in other activities. The dependent variable is zero if the individual reports no limitation.
c. The dependent variable is equal to one if the individual reports that he was ever told that his blood pressure was high by a doctor, nurse, or other health professional. The dependent variable is equal to zero otherwise.

Table 6.A
Linear Probability and OLS Models for Various Health Habits,
Men 21-64, BRFSS

				Coefficients and standard errors on Relative Deprivation: Reference group defined by:			
Outcome	Relative Depriv. Measure	Mean of Dep. Variable	Sample Size	State	State and Age	State, Age, Race	State, Age, Race, Educ.
Current Smoker?	RDL	0.29	72825	0.0780	0.0744	0.0744	0.1059
				(0.0388)	(0.0221)	(0.0207)	(0.0186)
	Centile Rank			-0.0009	-0.0010	-.0011	0.0001
				(0.0005)	(0.0003)	(0.0003)	(0.0003)
Ever smoker?	RDL	0.56	72949	0.0215	-0.1213	-0.1008	0.0339
				(0.0418)	(0.0238)	(0.0223)	(0.0200)
	Centile Rank			0.00006	-0.0016	-0.0015	0.0004
				(0.0006)	(0.0003)	(0.0003)	(0.0003)
BMI	RDL	25.8	72512	0.7343	0.8181	0.7507	0.6605
				(0.3303)	(0.1880)	(0.1759)	(0.1586)
	Centile Rank			-0.0023	0.0021	0.0032	0.0012
				(0.0044)	(0.0024)	(0.0024)	(0.0022)
Ever exercise?	RDL	0.73	73011	0.0636	-0.1798	-0.1400	-0.1060
				(0.0374)	(0.0212)	(0.0199)	0.0179
	Centile Rank			0.0009	-0.0001	0.0002	0.0005
				(0.0005)	(0.0003)	(0.0003)	(0.0002)
Always wear seat belt?	RDL	0.54	72873	0.0549	-0.0517	-0.1036	-.01216
				(0.0425)	(0.0242)	(0.0227)	(0.0204)
	Centile Rank			0.0009	0.0010	0.0002	-.00006
				(0.0006)	(0.0003)	(0.0003)	(0.0003)

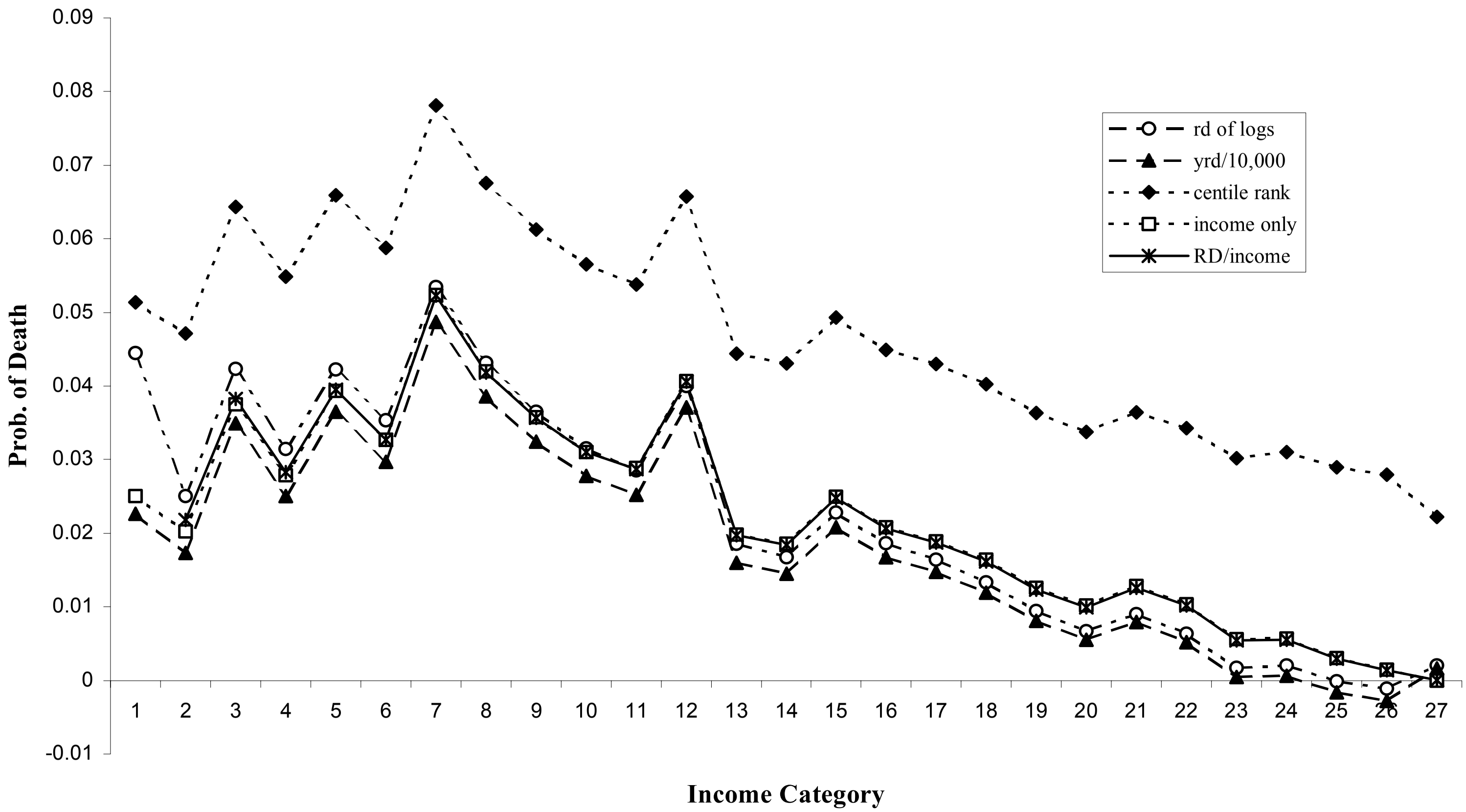
The means and sample sizes reported in the table are for the models using states as a reference group. Sample sizes for other reference groups are slightly smaller. Standard errors in parentheses. Unreported covariates include income, age-group, race, education, marital status, and year of interview.

Table 6.B
Linear Probability and OLS Models for Various Health Habits,
Men 65 and Over, BRFSS

				Coefficients and standard errors on Relative Deprivation: Reference group defined by:			
Outcome	Relative Depriv. Measure	Mean of Dep. Variable	Sample Size	State	State and Age	State, Age, Race	State, Age, Race, Educ.
Current Smoker?	RDL	0.15	12809	-0.0244	0.1219	0.0899	-0.0133
				(0.0773)	(0.0554)	(0.0507)	(0.0376)
	Centile Rank			-0.0001	0.0003	-0.0006	-0.0000
				(0.0011)	(0.0007)	(0.0007)	(0.0005)
Ever smoker?	RDL	0.66	12865	0.2816	0.0838	0.0370	0.0082
				(0.1021)	(0.0730)	(0.0669)	(0.0494)
	Centile Rank			0.0041	-0.0005	-0.0009	0.0020
				(0.0014)	(0.0009)	(0.0009)	(0.0007)
BMI	RDL	25.5	12686	1.759	0.0801	-0.0916	-0.0942
				(0.7755)	(0.5550)	(0.5109)	0.3806
	Centile Rank			0.0165	0.0184	0.0235	-0.0048
				(0.0108)	(0.0070)	(0.0069)	(0.0050)
Ever exercise?	RDL	0.64	12894	-0.0744	-0.1465	-0.2225	-0.1409
				(0.1004)	(0.0717)	(0.0655)	(0.0486)
	Centile Rank			-0.0017	-0.0022	-0.0023	-0.0017
				(0.0014)	(0.0009)	(0.0009)	(0.0006)
Always wear seat belt?	RDL	0.58	12769	0.1283	0.0276	-0.0142	0.0054
				(0.1044)	(0.0748)	(0.0685)	(0.0511)
	Centile Rank			0.0010	0.0003	-0.0008	-0.0010
				(0.0015)	(0.0009)	(0.0009)	(0.0007)

The means and sample sizes reported in the table are for the models using states as a reference group. Sample sizes for other reference groups are slightly smaller. Standard errors in parentheses. Unreported covariates include income, age-group, race, education, marital status, and year of interview.

Figure 1: Prob. of Death Attributable to Income, White Men 31-40 with High School Educ.



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