

Relative Deprivation, Poor Health Habits and Mortality

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Abstract

While a large body of evidence relates low absolute income to premature mortality, a recent and growing literature argues that relative income influences health as well. Low relative income, or being deprived relative to one's reference group, may cause stress and depression. These conditions are linked to mortality both directly (via heart disease, high blood pressure, and suicide) and indirectly (via increased smoking, poor eating habits, and alcohol abuse). Evidence from biology supports the notion that relative status may influence health outcomes. In this paper, we use restricted-use micro-level data from the National Health Interview Survey (NHIS) Multiple Cause of Death Files (MCOF) from 1988 to 1991 to examine whether relative deprivation increases the probability of dying. We define reference groups using a combination of characteristics including state, race, education, and age, and measure relative deprivation with Yitzhaki's index. Our use of individual-level data allows us to control for characteristics that are specific to reference group. Results indicate that high relative deprivation increases the probability of dying in all age groups and for those death categories with a high behavioral component. Those with high relative deprivation are more likely to self-report poor health, have high blood pressure or disabilities, and have a host of poor health habits including smoking, not wearing safety belts, high body mass index and not exercising. For nearly all health measures, our results suggest that much of the observed statistical relationship between absolute level of income and health found in previous work is actually measuring the impacts of relative deprivation on health.

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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. Furthermore, 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. 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. 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.

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

deprivation¹ 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; Kawachi, Kennedy and Prothrow-Stith, 1996; Wilkinson, 1996). 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

¹ Income inequality might lead to underinvestment in public goods or declines in social capital, which may adversely affect population health.

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, the Behavioral Risk Factor Surveillance System, and the Current Population Survey Tobacco Use Supplements 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 might increase the probability that an individual smokes by as much as 20 percent. Similarly, a one-standard deviation increase in relative deprivation might decrease the probability that an individual wears a seatbelt by 13 percent, or the probability that an individual ever exercises by 10 percent.

These results hint at a relationship between economic conditions, psychological factors, and individual behavior. The fact the relative income affects health may help explain why the gradient between income and health persists even at high income levels (Adler et al., 1994; Deaton 2001). Further, the relative deprivation theory may help explain why the US has worse health outcomes than more egalitarian countries. We do caution, however, that these results do not prove there is a causal relationship between relative deprivation and poor health. Omitted variables such as discount rates could potentially explain the correlations we have uncovered. The results are nonetheless suggestive of the possible negative consequences of relative deprivation.

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 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

²Wilkinson'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.

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. Miller and Paxson 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, an aggregate comparison of 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

³It 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 males⁴ with incomes greater than \$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 has yielded comparable results (Rogot, et al. 1992; Brown 1999; 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

⁴Most 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 results, Kitigawa and Hauser often do not report specific numbers for nonwhites.

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 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 inequality is only injurious to those at the low end of the income distribution. This theory, known as the relative deprivation hypothesis, is often confused with the pure income inequality hypothesis (usually because measures of income inequality do not distinguish between the pure inequality effect and the relative deprivation effect). The major difference between the two conjectures is that the relative deprivation hypothesis contends that the rich—or at least those who are wealthy relative to their particular reference group—should be unharmed by inequality. Wilkinson

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

(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 which affect health both directly (via heart disease, high blood pressure, and suicide) and indirectly (via increased smoking, 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 a Measure 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), we follow others in defining X as income (Yitzhaki 1979, Hey and Lambert 1980, Berrebi and Silber, 1985).

The relative deprivation measure we use is based on Runciman's definition and subsequent theory developed by Yitzhaki (1979), Hey and Lambert (1980), and Podder (1996). For a person i who is part of a reference group with N people, Yitzhaki's measure is defined as

$$(1) \quad RD_i = \frac{1}{N} \sum_j [\ln(y_j) - \ln(y_i)] \quad \forall y_j > y_i$$

Where y is income. This measure posits that relative deprivation for person i is driven by the incomes of

people who earn more than y_i . Following Podder (1996) and Hey and Lambert (1980), we use differences in log incomes rather than income to make the measure invariant to the scale of y .⁶ 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 is 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. The relative deprivation measure can be rewritten as:

$$(2) \quad RD_i = [E(\ln(y)|\ln(y) > \ln(y_i)) - \ln(y)] * pr(\ln(y) > \ln(y_i))$$

If we assume that income is lognormally distributed, we can use the properties of the truncated normal distribution to find a closed form solution to equation (2):

$$(3) \quad RD_i = [\mu_r - \ln(y_i)](1 - F_i) + \phi \left[\frac{\ln(y_i) - \mu_r}{\sigma_r} \right]$$

Where μ_r is mean log income for reference group r , σ_r is the standard deviation of log income for reference group r , F_i is the cumulative distribution of log income evaluated at $\ln(y_i)$, and ϕ is the standard normal probability density function. 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. For this analysis we will use relative deprivation as defined in equation (3) as the key covariate of interest.

⁶Equation (1) is essentially the RD measure specified by Yitzhaki, though Yitzhaki uses pure income instead of log income. Commenting on Yitzhaki's work, Hey and Lambert (1980) suggest using utility functions instead of pure income to characterize RD. Podder (1996) explicitly suggests using logs instead of pure income.

Although relative deprivation is an individual as opposed to an aggregate measure, RD_i 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 log 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.

Wilkinson and others suggest 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 who have income greater than y_i . While this framework seems to ignore the relative satisfaction effect, it can be shown that the relative deprivation measure we use is directly correlated with two reasonable measures of relative satisfaction.⁷ Thus, in this work the effect of an increase in relative deprivation is identical to a decrease in relative satisfaction.

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

⁷ Yitzhaki proposes an analogous relative satisfaction metric that is equal to μ -RD. 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 RS measure, $\{\ln(y_i) - E(\ln(y) | \ln(y) < \ln(y_i))\} * \text{prob}(\ln(y) < \ln(y_i))$, is a linear combination of RD, $\ln(y_i)$, and μ .

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 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; Gartrell, 1987).

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 culprit, 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*:

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 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.

V. 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) may cause their estimates to be imprecise, 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.

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,⁶ 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 large sample size from the PUMS to construct accurate estimates of reference group income characteristics.

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. Starting in 1986, individuals who responded to the NHIS questionnaire were tracked through the National Death Index. 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 MCODE can be merged with the NHIS persons file to form a linked file that we will refer to as the NHIS/MCOD.

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

⁶Mortality is a useful outcome because it is an objective measure of health that is precisely measured. However, there 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.

construct fixed time windows over which we can track mortality.⁷ 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 in equation (3), we need the log of 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.⁸ Additionally, since the family income variable in the NHIS is a 27-level categorical variable,⁹ moment estimates constructed from this measure will be noisy. Rather than rely on data from the NHIS to find the mean and standard deviation for the relative deprivation measure, we instead use household income¹⁰ 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 at a level that varies by state, topcoded individuals are assigned household income equal to the

⁷Month of interview must be imputed using quarter and week of interview.

⁸If 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.

⁹Income 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.

¹⁰Although 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.

median household income in their state given that income is greater than the topcode value.¹¹ We restrict our sample from the PUMS to male householders and male spouses over the age of 20.¹² This gives us a sample of 3,316,833. We then construct mean and standard deviation estimates for 4 different reference groups: 1) state only, 2) state and age-group, 3) state, age-group, and race, 4) state, age-group, race, and education.¹³ These moments are then merged into NHIS/MCOD files for the years 1988-1991.¹⁴ 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 only¹⁵ 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 highly-stratified reference groups.

Table 1 reports descriptive statistics for our sample. As we mention below, 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 (RD) behaves as we would expect—average relative deprivation is highest when reference groups are loosely defined over state

¹¹Information on topcode imputations for each state can be found on the IPUMS website, at http://www.ipums.org/usa/volii/topcode_odd.html.

¹²The sample is restricted to householders and spouses to avoid counting two observations from the same household.

¹³Age-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.

¹⁴In the interest of confidentiality, state identification codes are replaced by a randomized state indicator after the merge.

¹⁵Women 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.

only. This result is predictable, because 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. Nevertheless, it is useful to note that a one-standard-deviation movement in RD is roughly equal to 0.5 regardless of how reference groups are defined for the younger sample, but the standard deviation of RD falls by 20 percent for the older sample as one moves to more finely-defined reference groups.

B. The Model

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

$$(4) \quad Died_{ir} = \beta_0 + \beta_1(RD_{ir}) + \sum_{k=1}^{25} (income_{kir} \Theta_k) + \delta_r + X_{ir}\Gamma + \varepsilon_{ir}$$

Where $Died_{ir}$ 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 the relative deprivation measure, defined in equation (3). The moments needed to construct RD are taken from the 1990 PUMS, and the individual income used to construct the RD 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 ($income \geq \$50,000$), 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 RD measure only—to control for income independently of the relative income effect, we add a complete set of dummy variables for the 26 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 specification of both δ_r and X_{ir} changes slightly 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 Paxon, 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 (4) 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. The relative deprivation effect varies depending on 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 state of residence is probably a very noisy proxy for the true reference group (because states are large and contain many people who are irrelevant to each other in the social comparison process). When reference groups are narrowed by stratifying the data using additional demographic data, the relative deprivation effect becomes more pronounced. Overall, the relative deprivation effect is quite substantial. A coefficient on relative deprivation that is equal to 0.05 implies that a one standard deviation increase in

relative deprivation would raise the probability of death by nearly one-hundred percent. This result seems large, but it is 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. In a linear probability model where RD_i is not included as a covariate moving from \$40,000 to \$10,000 doubles the probability of death.

In the lower half of Table 2, we report estimates from equation (4) for men 65 and over. Again, RD has a positive coefficient. 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 low sample size for the older individuals. While there are 104,320 men ages 21-64 in our NHIS/MCOD sample, there are only 18,184 men over the age of sixty-four. The magnitude of the relative deprivation effect is substantial for the older group, though not as large as it was for the younger group. This result is consistent with Kaplan et al., who find that inequality is more strongly correlated with mortality for younger-aged men. For men ages 65 and over, a relative deprivation coefficient of 0.04 (which is roughly consistent with the coefficients estimated in the state/age-group and state/age-group/race models) implies that a one-standard-deviation increase in RD raises the probability of death by 8.6 percent.

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 noninstitutionalized 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 RD 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. 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 is 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), ischemic

heart disease (IHD),¹⁶ tobacco-related cancers,¹⁷ and accidents/external events.¹⁸ All of these causes of death are linked to behavior, particularly through the use of tobacco and alcohol. 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 oropharyngeal 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 esophagus, larynx, 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).

The remaining rows of Table 2 show results for the cause-specific mortality models. For CHD, IHD, and mortality due to 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 RD increases the probability of death by 100 to 163 percent. Likewise for the older age-group, RD generally has a positive coefficient in the CHD models. However for men 65, and over the standard errors on RD are large, and the results approach statistical significance only when reference groups are defined over state and age-group. As with the results for all-cause mortality, the magnitude of the relative deprivation effect is proportionately smaller for the older age-group. If we assume the

¹⁶ ICD-9 codes 410-414. These are the same codes as used by Kennedy, Kawachi, and Prothrow-Stith (1996) to define coronary heart disease.

¹⁷ 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).

¹⁸ This category includes motor vehicle accidents, other accidents, suicide, homicide, legal intervention, and other external causes (ICD-9 codes E800-E899).

coefficient on RD is equal to the upper bound of 0.0644, a one-standard-deviation movement in deprivation would increase the probability of death due to CHD by about 38 percent.

In the third row of table 2 we look at IHD, a narrow definition of heart disease that is primarily heart attacks. Again we find the relative deprivation has a strong and statistically precise impact on the probability of death for the younger age-group. A one-standard deviation increase in relative deprivation might increase the probability of death for a man aged 21-64 by as much as 170 percent. The magnitude of the coefficients is smaller and the standard errors are larger for men over aged 65. Yet the effect of relative deprivation is still positive, and—as an upper bound—a one-standard deviation increase in RD might increase the probability of death for the older age-group by 63 percent.

Results for tobacco-related cancers 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 RD effect imply that a one-standard-deviation increase in RD could increase the probability of death due to tobacco-related cancer by as much as 160 percent for the younger age-group, and by as much as 78 percent for the older age-group.

For mortality due to accidents and adverse effects, relative deprivation does not seem to play as much of a role as it did for the other causes. For the younger age-group, the coefficients on RD are imprecisely measured, and they are sometimes the wrong sign. While the sign on RD is positive for the older age-group, standard errors are again large. Perhaps relative deprivation has a less predictable impact on accidental death since accidents are less related to behavior than mortality due to lung cancer and ischemic 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.

E. Results by Race

Miller and Paxson (2000) and Deaton (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 finds 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. These results are reported in Table 3.

For those aged 21-64, the change in mortality generated by an increase in relative deprivation is nearly the same for blacks and whites. Because death rates for Blacks is about 50 percent higher than the rate for whites, relative deprivation appears to have a larger relative impact on Whites. In contrast, we find a large impact of relative deprivation on Blacks aged 65 and over but sketchy 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.

F. 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 can look at the effect a discrete change in income has on the probability of death. Suppose income increases from y_1 to y_2 . In the linear probability framework, the change in the probability of death that comes about from this increase is:

$$(5) \quad \Delta \text{Prob}(D_{ir} = 1) = \beta(RD_{ir1} - RD_{ir2}) + (\Theta_2 - \Theta_1)$$

Where β is the coefficient on relative deprivation from equation (4), RD_{ir1} is the individual’s initial level of relative deprivation, and Θ_1 is the coefficient on the dummy variable representing the individual’s initial income category. Likewise, RD_{ir2} and Θ_2 are the individual’s new relative deprivation level and income coefficient, respectively. Table 3 shows the change in the probability of death for a white male aged 40 with a high school education as he moves from \$10,000 to \$15,000, from \$20,000 to \$25,000, and from \$30,000 to \$35,000. Since we cannot identify specific states in the NHIS, we average the relative deprivation effect across all states. Table 3 confirms the notion that, while increases in income are protective against mortality, the effect of an increase in income diminishes as absolute income

increase. Moving from \$10,000 to \$15,000 decreases the probability of death by about 1 percentage point, while moving from \$30,000 to \$35,000 decrease the probability of death by about 0.1 percentage points.

One surprising finding is that, while an increase in income leads to an increase in the probability of death, most of the income effect comes through relative deprivation and not through a change in absolute income. Figures 1 and 2 graph the coefficients on income from linear probability models (fitted with a cubic spline) both with and without the relative deprivation term.¹⁹ When relative deprivation is not included (Figure 2), the coefficients on income are higher when income is lower. This indicates that low income is associated with a greater probability of death. In contrast, when relative deprivation is included in the model (Figure 1), the coefficients on income are not monotonically decreasing as income increases. In fact, the income coefficients sometimes show that—after controlling for relative deprivation—an increase in income may be deleterious to health.

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's 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

¹⁹Reference groups are defined over state, age-group, and race.

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 his blood pressure was high. We restrict the BRFSS sample to include men only who are over the age of 20, and we use data from 1988, 1990, and 1991.²⁰ 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 doctors 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.²¹

In Table 4, we report results from weighted linear probability models on the three non-mortality outcomes discussed above. The models are identical to equation (4), except the dependent variable is either fair/poor self-reported health, limited activity status, or high blood pressure. Two interesting patterns emerge from these regressions. First, for all three outcomes, the sign of the coefficient is

²⁰A more-detailed description of the BRFSS data is available in the next section.

²¹Author's own calculations. The Third National Health and Nutrition Examination Survey contains data from 1988-1994.

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 than either the nation or the locality" because states are too large to permit individual comparison between residents.²² This notion is supported by the fact that, in all of the mortality regressions described in above and in many of the behavior regressions discussed below, the relative deprivation effect is weakest when reference groups are characterized by state only.

A second finding from Table 4 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.

²²Nevertheless, Deaton uses state of residence to construct reference groups in his work.

VI. Relative Deprivation and Health Compromising Behavior

McGinnis and Foege estimate that, in 1990, fifty percent of all mortality in the United States could be attributed to behavioral factors. Twenty percent of this mortality was caused by smoking, 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 three sources—the Behavioral Risk Factor Surveillance System (BRFSS), the Current Population Survey Tobacco Use Supplements (CPS-TUS), and the National Health Interview Survey (NHIS)—to determine whether increases in relative deprivation increase the probability that an individual takes health risks.

A. Data

Much of 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.²³ Together the BRFSS surveys from 1989, 1990, and 1991 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,

²³States that did not participate were Alaska, Nevada, Wyoming, Kansas, Arkansas, and New Jersey.

current and former smoker, and excessive drinking.

As a second data source, we use information from the National Health Interview Survey (NHIS), which was introduced in the previous section. In order to access geographical information from the NHIS, we must use the restricted access files, available only for on-site use at the National Center for Health Statistics. The variables of interest from the NHIS are height and weight, which can be used to calculate body mass index.

Finally, we use data from the Current Population Survey Tobacco Use Supplements (CPS-TUS) taken in September 1992, January 1993, and May 1993. The CPS-TUS is a monthly survey administered to about 57,000 households across the country. Individuals are interviewed eight times, each for a four month interval over two years to allow for month-to-month and year-to-year comparisons across households. The three tobacco use surveys were conducted five months apart to ensure that respondents were not interviewed twice. Each tobacco supplement contains smoking behavior for household members ages 15 and over, and there are about 112,000 observation in each month. We restrict the sample to men over the age of 21 which yields 140,901 observations. The variable of interest from the CPS supplements is whether or not the individual currently smokes. Additionally, the supplements report basic demographic data including family income. Since the tobacco use supplements were conducted in the early 1990s, they can be linked to detailed data on income distribution from the 1990 PUMS.

B. Estimation Issues

The empirical model for this section is identical to the one we used for the mortality regressions in the previous sections. Two of the behaviors we examine, body mass index and minutes of exercise per month, are continuous. 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 the Yitzhaki's measure, 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, the CPS-TUS, and the NHIS report income as a categorical variable. In the BRFSS, household income is reported as a 7-level variable with a topcoding value of \$50,000. In the NHIS, family income is a 27-level variable topcoded at \$50,000, and in the CPS-TUS family income is a 13-level variable topcoded at \$75,000. 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.²⁴

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 cancer. Smoking is also associated with other forms of morbidity and mortality, including heart disease and emphysema. In chapters one and two of this dissertation, we argue that an individual’s propensity to smoke may be influenced by relative deprivation. In this chapter, we test this hypothesis using data from the BRFSS (1989-1991), and the CPS/TUS (1992-1993). Because the disparity in outcomes for these health habits is not as stark across age groups as mortality, we do not report separate models for those 21-64 and 65 and over. These results are available from the authors on request.

In the first rows of Table 6, we present parameter estimates for the relative deprivation variable from linear probability models where the outcome of interest is current smoking status from the

²⁴Because 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 (CPS-TUS) by regressing incomes over \$50,000(\$75,000) 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 (CPS-TUS).

CPS/TUS and BRFSS samples. Twenty-seven percent of the BRFSS sample report that they currently smoke, while twenty-four percent of the CPS/TUS sample are current smokers. In the BRFSS regression, relative deprivation is significantly and positively associated with the probability that an individual smokes regardless of how reference groups are defined. The increase in the probability of smoking predicted by a one-standard deviation increase in relative deprivation ranges from 11.5 percent (when reference groups are defined using state) to 26.0 percent (when reference groups are defined using state and age-group).²⁵ As in the mortality models described in chapter two, the relative deprivation effect in the BRFSS data is strongest when reference groups are defined using state and age-group, and weakest when reference groups are defined using state only. In the CPS/TUS regressions, relative deprivation again seems to increase the probability that an individual smokes. As with the morbidity outcomes discussed earlier, when reference groups are defined using state only, the coefficient on RD is negative and statistically significant. This counterintuitive results may stem from the fact that state alone is an inappropriate reference group. The rest of the findings for smoking are consistent with the hypothesis that relative deprivation increases the probability that an individual smokes. As an upper bound, these results imply that a one standard deviation increase in RD might increase the probability that an individual smokes by up to 22 percent.

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. Data from the CPS/TUS indicate that 95 percent of people who ever smoke begin smoking before they reach the age of 21, 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.

²⁵These results are relative to the baseline mean of smoking, which is 26.7 percent. A one standard deviation increase in relative deprivation is approximately equal to 0.5.

Results are reported in line 3 of Table 6.²⁶ In the BRFSS data, relative deprivation appears to be unrelated to the probability that an individual ever smoked. The coefficients on RD in line 3 are statistically imprecise, and the sign vacillates depending on how reference groups are constructed. This result is intuitive because current relative deprivation should have no impact on an individual's past smoking habits.

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:

$$(6) \quad BMI = \frac{(Weight \text{ in kgs})}{(Height \text{ in meters})^2} = (703) * \frac{(Weight \text{ in lbs.})}{(Height \text{ in inches})^2}$$

Using the height and weight variables recorded in the BRFSS and the NHIS, we can construct a continuous measure of BMI to evaluate in the framework outlined in equation (4). We limit the sample to exclude men with BMI values less than 16 or greater than 59.²⁷ The average BMI for men 21 and over is 25.74 in the BRFSS and 25.83 in the NHIS. 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-

²⁶Ever smokers are defined as individuals who report that they smoked at least 100 cigarettes in their lifetime. We do not report "ever smoked" results from the CPS/TUS data because a large fraction of the survey is completed by proxy respondents. This is a particular problem for the ever smoked question because proxy respondents are often unaware of the previous smoking histories of their family members. Only 50 percent of the CPS sample reports ever having smoked, versus 58 percent in the BRFSS.

²⁷BMI's 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.

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 and the NHIS.²⁸ Rows (4) and (5) show the results found after regressing BMI on relative deprivation and the covariates from the BRFSS and NHIS samples, respectively. The estimates from the BRFSS suggest that a one standard deviation increase in RD will increase BMI by somewhere between 1 and 1.8 percent. In the NHIS data, the coefficient on RD is positive in three of the four reference group constructions. However, the coefficient is only statistically significant when reference groups are defined using state, age-group, race, and education (column 4). The results from the NHIS suggest that a one standard deviation increase in RD might increase BMI by slightly less than 1 percent.

A key contributor to obesity is lack of exercise. Sedentary behavior is harmful not only because of its 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, Mosbacher, and Ismail, 1983, Crews and Landers, 1987). Using data from the BRFSS, we can measure whether or not an individual exercised in the past month. We can also calculate the (self-reported) minutes of exercise spent per month on an individual's primary and secondary activity.

Row (6) of Table 6 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. Three of the four columns show that those who have low income relative to their reference group are less likely to exercise. When reference groups are constructed using state and age-group, state, age-group,

²⁸Authors own calculations.

and race, or state, age-group, race and education, the coefficients on relative deprivation are negative and statistically significant. A one standard deviation increase in relative deprivation might decrease the probability that an individual ever exercises by as much as 12 percent.

In row (7), we report the results when the outcome is defined as minutes of exercise per month. In these models, the coefficient on relative deprivation is statistically significant and quantitatively important only when reference groups are defined using state alone.

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.²⁹ In

²⁹The 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.

the BRFSS data, 54.5 percent of men report that they always wear a seatbelt. Row (8) of Table 6 summarizes the results found by regressing seatbelt use on relative deprivation and other covariates. In the first column of this row, where reference groups are defined using state only, we get the counterintuitive result that RD increases the probability that an individual always wears a seatbelt. However, as with the smoking models discussed above, the sign on the relative deprivation coefficient reverses when reference groups are defined more narrowly. In the columns 2, 3, and 4, the coefficient on relative deprivation is 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 nearly ten percent.

Alcohol Consumption: In 1997, thirty-nine percent of all traffic fatalities in the U.S. were alcohol-related (CDC, 2001). Alcohol is linked to other types of accidents, such as drowning, and there are also long-term health consequences of alcohol use. Prolonged alcohol use can lead to liver disease, heart disease, cancer (primarily of the mouth, throat, esophagus, and voice box), and pancreatitis (National Clearinghouse for Alcohol and Drug Information, 2001). While all of these conditions can be fatal, alcoholics are particularly at risk for death due to chronic liver disease and cirrhosis, which taken together are one of the top ten causes of death for men 25-64 in the United States (Statistical Abstract of the United States, 2000).

The BRFSS asks a number of questions about an individual's drinking habits, including whether or not the individual drinks at all, how many drinks he or she had in the past month, and how many times he or she consumed more than 4 drinks on one occasion in the last month. Unfortunately, the information provided in these questions is not sufficient to accurately characterize individuals as alcoholics according to the criteria outlined in the Diagnostic and Statistical Manual of Mental Disorders,

Edition 4 (DSM-IV).³⁰ There are, however, quantitative guidelines for safe alcohol consumption. Both the National Institute on Alcohol Abuse and Alcoholism (NIAAA) and the Substance Abuse and Mental Health Services Administration (SAMHSA) report that, as a rule of thumb, most men can safely consume up to two drinks per day without any adverse health effects. Following these guidelines, we use BRFSS data to construct a variable indicating whether or not an individual consumed more than 60 drinks in the past month—or an average of more than 2 drinks per day. The final row of Table 6 reports the coefficients for relative deprivation when an alcohol consumption indicator variable is used as the outcome of interest.

In contrast to the smoking and seatbelt use models, which suggest that relative deprivation may lead to health compromising behaviors, there seems to be no association between relative deprivation and alcohol consumption. The coefficient on relative deprivation is statistically imprecise no matter how reference groups are constructed, and the sign of the coefficient is negative in two of the four models. These results can be explained in a number of ways. First, since alcoholism is not diagnosed based on the number of drinks an individual habitually consumes, the construction of the BRFSS data makes it difficult to draw the line between moderate alcohol consumption and problematic drinking. Second, it seems likely that alcohol consumption is under reported in the BRFSS data. Only about 6 percent of the men in our sample report that they consumed more than 60 drinks in the past month, whereas the NIAAA reports that, in 1992, 6.81 percent of males over the age of 18 could be diagnosed with either alcohol dependence or alcohol abuse.³¹ We suspect that our definition of heavy drinking is broader than DSM-IV's definition of alcoholism or alcohol abuse. Thus we would expect the percent of people who consume more than 60 drinks a month to be greater than the percent of people who are alcohol

³⁰The DSM-4 defines alcoholism as a combination of alcohol craving, the inability to limit one's drinking, a physical dependence on alcohol leading to withdraw symptoms, and a need for increasingly large quantities of alcohol to feel its effects.

³¹The data for the NIAAA report came from the 1992 National Longitudinal Alcohol Epidemiologic Survey, and Alcohol dependence and Alcohol Abuse were diagnosed using criteria from the DSM-IV.

dependent.

VII. 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 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.

We should stress however that these results are only suggestive of a causal link between relative deprivation and poor health. It is quite 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 are 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.

In spite of these caveats, 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 and the CPS/TUS. 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 BMI and negatively associated with the probability of exercise. To our knowledge, this is the first work to look specifically at behavior and relative deprivation.

Two other results are of special note. First, relative deprivation appears to account for the well-established positive relationship between income and health. After controlling for relative deprivation, we fail to observe the standard result linking absolute income to mortality. Second, most of the previous work using aggregate data has defined reference groups at the state level. Across all measures of health, the weakest evidence of the deleterious impacts of relative deprivation are contained in models that define reference groups by state of residence.

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

5-year mortality	Reference group defined by			
	State	State and age	State, age and race	State, age, race and educ.
Males aged 21-64 (104,320 observations)				
RD	0.4672 (0.5289)	0.4604 (0.5227)	0.4470 (0.5065)	0.4133 (0.4776)
% Died in 5 years	2.44	2.44	2.44	2.45
Income	38,237 (21,659)	38,237 (21,659)	38,272 (21,657)	38,432 (21,645)
Age	39.1 (11.8)	39.1 (11.8)	39.1 (11.8)	39.2 (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)				
RD	0.7250 (0.5172)	0.4924 (0.4262)	0.4808 (0.4104)	0.4426 (0.3823)
% Died in 5 years	23.26	23.26	23.23	23.17
Income	25,577 (18,443)	25,583 (18,446)	25,655 (18,453)	25,624 (18,407)
Age	72.6 (5.8)	72.5 (5.8)	72.5 (5.8)	72.4 (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

5-year mortality	% Died	Coefficients and standard errors on RD(j): Reference group defined by			
		State	State and age	State, age and race	State, age, race and educ.
Males aged 21-64 (104,320 observations)					
All cause	2.44	0.0232 (0.0109)	0.0564 (0.0062)	0.0507 (0.0057)	0.0359 (0.0052)
CHD	0.66	0.0209 (0.0058)	0.0212 (0.0033)	0.0215 (0.0030)	0.0122 (0.028)
Ischemic Heart Disease	0.28	0.0047 (0.0038)	0.0078 (0.0022)	0.0098 (0.0020)	0.0085 (0.0018)
Smoking-related cancers	0.29	0.0010 (0.0038)	0.0079 (0.0022)	0.0095 (0.0020)	0.0076 (0.0018)
Accidents/ Adverse events	0.37	-0.0074 (0.0044)	0.0012 (0.0025)	0.0010 (0.0023)	0.0032 (0.0021)
Males aged 65+ (18,184 observations)					
All cause	23.36	0.0122 (0.0752)	0.0321 (0.0538)	0.0498 (0.0483)	0.0097 (0.0383)
CHD	8.44	0.0109 (0.0515)	0.0644 (0.0368)	0.0475 (0.0330)	0.0392 (0.0260)
Ischemic Heart Disease	3.31	0.0058 (0.0328)	0.0271 (0.0235)	0.0143 (0.0211)	-0.0012 (0.0167)
Smoking-related cancers	2.31	0.0046 (0.0277)	0.0363 (0.0199)	0.0220 (0.0178)	0.0031 (0.0142)
Accidents/ Adverse events	0.61	0.0130 (0.0143)	0.0057 (0.0101)	-0.0003 (0.0092)	0.0066 (0.0072)

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.

Table 3
 Weighted Linear Probability Models, 5-Year Mortality Equations
 NHIS/MCOD

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

Table 4
 Total Impact of a Change in Income on Mortality
 White Male, Aged 40, With High School Education
 Average Impact Across States

Change in income:	Reference Groups Defined Using:			
	State Only	State & Age-group	State, Age-group, & Race	State, Age-group, Race, & Educ
\$10,000 to \$15,000	-0.0084	-0.0103	-0.0122	-0.0106
\$20,000 to \$25,000	-0.0035	-0.0051	-0.0055	-0.0039
\$30,000 to \$35,000	-0.0027	-0.0012	-0.0015	-0.0003

Estimated using coefficients from linear probability models in table 2.

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

5-year mortality	% answering yes	Obs.	Coefficients and standard errors on RD(j): Reference group defined by			
			State	State and age	State, age and race	State, age, race and educ.
Fair or poor health? ^a	16.63	123,969	-0.1759 (0.0187)	0.1431 (0.0103)	0.1284 (0.0097)	0.0322 (0.0090)
Limited in activity? ^b	11.15	123,665	-0.1101 (0.0224)	0.2320 (0.0123)	0.2358 (0.0117)	0.1077 (0.0108)
High blood pressure? ^c	21.16	85,841	-0.0190 (0.0314)	0.0788 (0.0174)	0.0849 (0.0167)	0.0336 (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
Linear Probability and OLS Models for Various Health Habits,
Men 21 and Over, BRFSS

Row	Outcome	Source	Mean of Dep. variable	Sample size	Coefficients and standard errors on RD(j): Reference group defined by:			
					State	State and age	State, age and race	State, age, race and educ.
(1)	Current Smoker?	BRFSS	26.7%	85,634	0.0614 (0.0342)	0.1387 (0.0190)	0.1324 (0.0182)	0.0937 (0.0167)
(2)	Current smoker?	CPS	24.1%	128,479	-0.1858 (0.0265)	0.0754 (0.0140)	0.1052 (0.0133)	0.0491 (0.0126)
(3)	Ever smoker?	BRFSS	57.8%	85,814	0.0503 (0.0377)	-0.0380 (0.0209)	-0.0456 (0.0201)	0.0285 (0.0184)
(4)	BMI	BRFSS	25.7	85,198	0.9383 (0.2958)	0.6090 (0.1645)	0.5940 (0.1577)	0.5391 (0.1454)
(5)	BMI	NHIS	25.8	122,971	0.2653 (0.2510)	-0.0993 (0.1388)	0.1664 (0.1315)	0.4601 (0.1215)
(6)	Ever exercise?	BRFSS	71.9%	85,905	0.0451 (0.0341)	-0.1737 (0.0189)	-0.1497 (0.0181)	-0.1007 (0.0167)
(7)	Minutes of exer./month	BRFSS	855	82,787	371.47 (95.63)	22.97 (53.09)	24.72 (50.91)	0.9450 (46.99)
(8)	Always wear seat belt?	BRFSS	54.5%	85,642	0.0670 (0.0383)	-0.0653 (0.0213)	-0.1064 (0.0205)	-0.1005 (0.0188)
(9)	≥2 drinks/day	BRFSS	6.0%	84,950	-0.0172 (0.0189)	-0.0005 (0.0105)	0.0202 (0.0101)	0.0096 (0.0094)

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.

Figure 1
 Coefficients on Income, Weighted Linear Probability Model
 Reference Groups Defined Over State, Age-Group, and Race
 With Relative Deprivation Term

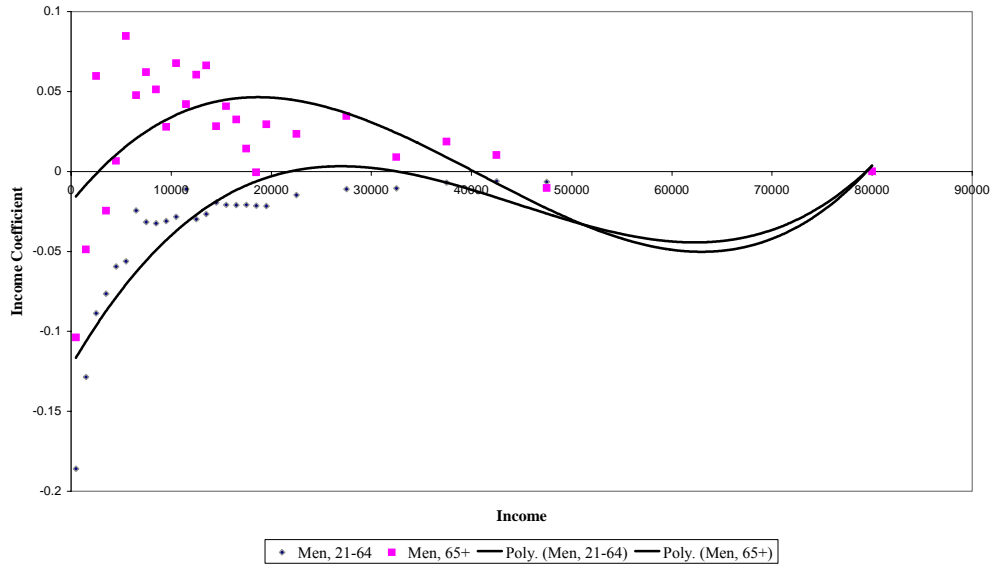
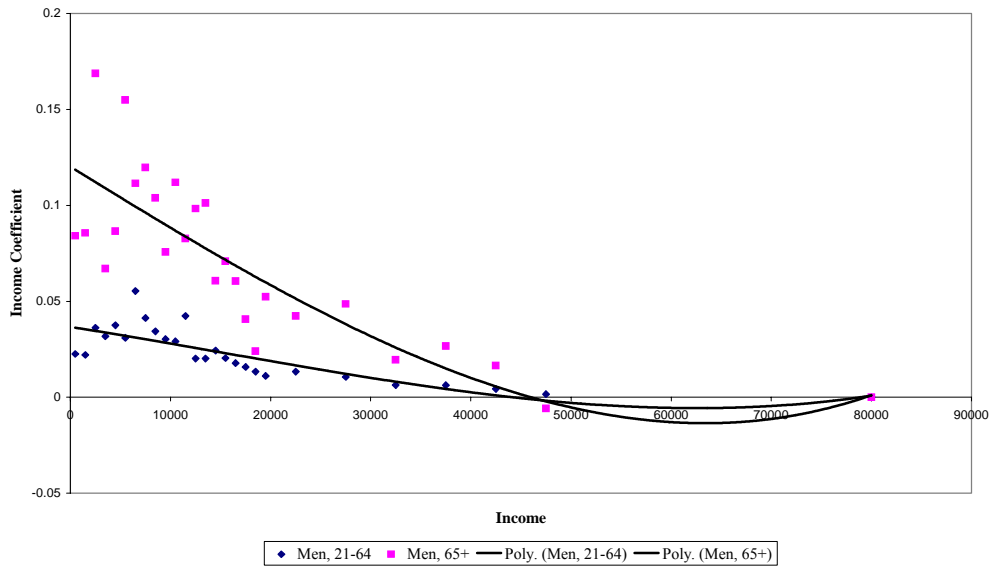


Figure 2
 Coefficients on Income, Weighted Linear Probability Model
 Reference Groups Defined Over State, Age-Group, and Race
 Without Relative Deprivation Term



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