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EVIDENCE FROM THE SOCIAL SECURITY NOTCH

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ABSTRACT

There is widespread and longstanding agreement that life expectancy and income are positively correlated. However, it has proven much more difficult to establish a causal relationship since income and health are jointly determined. We use a major change in the Social Security law as exogenous variation in income to examine the impact of income on mortality in an elderly population. The legislation created a "notch" in Social Security benefits based upon date of birth; those born before January 1, 1917 generally receive higher benefits than those born afterwards. We compare mortality rates after age 65 for males born in the second half of 1916 and the first half of 1917. Data from restricted-use versions of the National Mortality Detail File combined with Census data allows us to count all deaths among elderly Americans between 1979 and 1993. We find that the higher income group has a statistically significantly higher mortality rate, contradicting the previous literature. We also find that the younger cohort responded to lower incomes by increasing post-retirement work effort. These results suggest that moderate employment has beneficial health effects for the elderly.

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I Introduction

In 1996, the Advisory Commission to Study the Consumer Price Index, informally known as the Boskin Commission, released a report concluding that the Consumer Price Index (CPI) overstated the growth in prices by about 1.1 percentage points per year. Because payments in many Federal programs are indexed by the CPI, the Commission concluded that this over-indexation has contributed to an excessive growth in these programs. The Commission calculated that over the period 1997 - 2008, the over indexation of Federal programs would add an additional $1.07 trillion to the national debt. In their recommendations, the Commission suggested that if "...the purpose of indexing is accurately and fully to insulate the groups receiving transfer payments..." then "[t]his could be done in the context of subtracting an amount partly or wholly reflecting the overindexing from the current CPI-based indexing."(p9)

The recommendation to adjust the CPI downward was criticized by a number of groups including those representing unions and senior citizens. The incomes of these constituencies are in many cases tied to the CPI and any adjustment downward in how inflation is calculated would reduce future incomes for these groups. Those testifying before congress painted a grim picture of the elderly on fixed incomes forced to choose between purchasing food or prescription drugs, a situation that would obviously be made worse if cost of living adjustments to Social Security payments were more modest after the CPI was adjusted downward. Testimony from a number of witnesses even suggested that adopting the Boskin Commission's recommendations would raise mortality rates among the elderly.

While the identity of the groups objecting to the Boskin Commission's recommendations are predictable, some of their concerns are not without empirical backing. A large body of literature that spans many disciplines has established that those with lower incomes have poorer health outcomes and higher mortality rates (Kitigawa and Hauser, 1973; Duleep, 1986; Wolfson et al 1993; Fuchs 1993; Chapman and Hariharan, 1994; McDonough et al, 1997, Ettner, 1996; Lantz et al, 1998, Deaton and Paxson, 1998 and 1999). A relationship between health and socioeconomic status (SES) has been documented for virtually all measures of health (infant mortality, mortality, disease incidence, health habits, violence) and SES (income, wealth, occupation and education[1]), within many countries (including Canada, the United Kingdom, The Netherlands, Sweden, France, the United States[2]) and over time,[3] and recent research suggests that the statistical correlation between SES status and mortality may have actually increased over the past 40 years (Feldman, et al, 1989; Pappas et al, 1993; Preston and Elo, 1995; Deaton and Paxson, 1998). A large literature also exists about the correlation between socioeconomic status and health in an elderly population, the particular interest of this paper (Mare, 1990; Menchik, 1993; Smith and Kington, 1997).

It has, however, been difficult isolating income as the causal element in this relationship. The inferential problems are described in detail by Smith (1999). There is for example the simple problem that an equally large literature demonstrates that poor health reduces earnings (Haveman et al, 1995; Bound 1989) and therefore, low current income may be caused by poor health and not the other way around. Smith (1999) also demonstrates that the onset of a poor health shock greatly increases a families' out of pocket health expenses, possibly decreasing resources available in the future. Likewise, low income and high mortality may reflect outcomes of the same process, thereby subjecting the income/mortality relationship to an omitted variables bias. For example, Fuchs (1982) suggests that poor health and low income may both be generated by high discount rates. A high discount rate will discourage both investment in human and health capital, thereby lowering income and raising mortality (Farrell and Fuchs, 1982). This hypothesis is bolstered by evidence which suggests those with lower education have higher mortality and much poorer health habits (Kenkel, 1991; Pincus et al, 1987; Adler et al, 1993; Evans and Montgomery, 1994; Evans, Ringel and Stech, 1999).

One could isolate the impact of income on mortality by assigning different groups higher or lower

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[1] See Menchik 1993, Marmot and various co-authors 1984, 1987, and 1991, Townsend et al., 1988.

[2] See Wolfson et al. 1993, Marmot et al. 1991, Townsend et al. 1988, Kunst et al. 1990, and Feinstein 1993.

[3]As one example, Adler et al. (1993) cite evidence that wealthy Rhode Island taxpayers in the 1860s had mortality rates less than half the population average.

income independent of observed characteristics. Absent this ideal research design, we must find field variation in income that mimics random assignment. Unfortunately, finding such variation has proven to be difficult. The heart of this paper is the use of what is frequently termed the "benefits notch" in Social Security as an exogenous source of variation in the income of Social Security beneficiaries. Concerned with rapidly increasing benefit payments, in 1977, the Federal government changed the way benefits were calculated for new beneficiaries, substantially decreasing the size of payments for recipients born after January 1, 1917. As a result of these changes, two people with identical earnings histories but different birth dates would receive substantially different retirement incomes. Those born after the Notch had little time to adjust since the changes happened late in their work lives. Most, for example, did not realize the impact of the law changes on payments until after they retired.

We examine the link between income and health in an elderly population by estimating a reduced-form relationship between the benefits Notch and mortality. Our econometric model is a simple difference-in-difference estimator where we compare five-year mortality rates for those born in the fourth quarter of 1916 with those born in the first quarter of 1917. Because there may be permanent differences in mortality based on quarter of birth, we use as a comparison group mortality of men born in the fourth quarter of 1915 and the first quarter if 1916. Because there may be cohort-specific differences in mortality rate, we use as a second comparison group women born in 1916:4 and 1917:1. As we demonstrate empirically below, going into retirement, there is little to distinguish those born just before and after January 1, 1917. In the 1970 Census for example, there is no difference in the observed characteristics between those born in the fourth quarter of 1916 and the first quarter of 1917. If income does have a causal impact on mortality, we should find those born just before 1917 to have lower mortality after retirement.

The Notch is an excellent opportunity to examine the income/mortality link for four reasons. First, the changes in monthly payments generated by the notch were substantial. Analysis from a variety of sources suggests that those born in the fourth quarter of 1916 had about 7-10 percent higher monthly Social Security payments and about 4 percent higher incomes than those born just one quarter later. Second, mortality rates in the impacted groups are relatively high, making it easier to detect an effect of income on mortality if one exists. Third, the manner in which the Notch came to be minimized any reaction among the affected populations. Fourth, and maybe most importantly, the incomes of the elderly are routinely changed by the Federal government through such factors as cost of living adjustments and Medicare premiums, and therefore, this research answers a question of direct policy relevance.[4]

The quasi-experiment we examine is conceptually similar the one outlined in Case (2001). In that paper, the author uses large unanticipated changes in the South African pension system to examine the impact of income on health. In South Africa, pensions for elderly Blacks and Coloured men and women were increased to be on par with those received by whites. Case found that in households that pooled income, individual health was positively related to the number of pensioners in the household while in households that did not pool resources, health is correlated with only the pension status of the recipient.

Our results are however quite different from the current literature. Examining mortality rates after age 65, we find those born in the last half of 1916 have higher mortality than those born in the first half of 1917, even though the older group receives higher Social Security benefits. Investigation of post-retirement labor supply for these two groups suggests that the younger cohort has more part-time work than those born in 1916. These results are consistent with research that suggests social isolation may increase mortality

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[4]Here are three examples of proposed Federal programs that would have changed the incomes of the elderly in amounts comparable to the income shift produced by the Notch. As we mentioned above, The Boskin Commission's report suggested that the Federal Government might consider indexing Social Security at a rate lower than the CPI. Had the Commission's recommendations been adopted, monthly Social Security payments would have dropped by about 5 percent over a 5-year period. In 1988, Congress passed legislation providing catastrophic health for seniors. This proposal would have provided, among other benefits, unlimited coverage for hospital and nursing home stays. Currently, Medicare only pays for the first 180 days of a hospital stay and does not normally pay for nursing home care. This insurance would have been financed by a premiums that is based on income with the cost being up to $800/year for a single person and $1,600/year for married couples. Finally, in the early 1980s, Congress instituted the QMB (Qualified Medicare Beneficiary) and SLMB (Specified, Low-income Medicare Beneficiary) programs. QMB paid Medicare Part A (hospitalization) co-payments and deductibles for low income seniors while the SLMB program paid Part B premiums for low-income seniors with incomes too high to qualify for the QMB program. The income limit for QMB eligibility is $716 for an individual and $958 for a couple $855 for an individual while the SLMB limits are $855 for an individual $1,145 for a couple (http://www.aarp.org/confacts/money/qmb.html). The current one-day deductible for a Medicare hospital stay is $792 or roughly one-twelfth of the income of the highest-earning QMB-eligible person. Likewise, the Part B premium is $50/month, or about 5.8 percent of the income of the highest-earning eligible SLMB beneficiary (http://www.hhs.gov/news/press/2000pres/20001018.html)

among the elderly. If part-time work keeps the elderly engages in the community, then there may be some mortality benefits from staying employed past retirement.

This paper is structured as follows. In the next section, we present an empirical baseline that provides an estimate of the impact of income on mortality for those just entering retirement. The results in this section are in the spirit of previous work in the literature and provide a useful backdrop from which we can compare the reduced-form models presented later on in the paper. In Section III, we provide a short description of the Social Security notch and some estimates of how much the notch changed Social Security payments and family income. In Section IV, we propose a simple difference estimator to examine this issue and describe how we use data from restricted-use versions of the Mortality Detail data files to implement this model. In Section V, we present our basic results. Contrary to conventional wisdom, we find that those who received higher Social Security benefits actually had higher mortality than those from younger cohorts. In section VI we explore possible explanations for this result and suggest that increased part-time work of the elderly after age 65 is the likely cause. Using data from the March Current Population Survey, we show that the notch cohorts have substantially higher probabilities of work than older, higher compensated cohorts. Higher work can explain the lower mortality among the notch cohorts if work reduces social isolation among an elderly. A number of researchers have demonstrated a strong positive correlation between social isolation and mortality, especially among the elderly. We close with some concluding remarks in section VII.

II An Empirical Baseline

In later sections, we examine whether reduced Social Security payments generated by the notch produced higher mortality rates for younger cohorts. This section sets a baseline for discussing the likely magnitude of the effects by estimating a single-equation model for our population of interest. The backdrop for this work is the large social science literature that has examined the income/mortality relationship. The genesis for much of the work in social sciences is the research of Kitagawa and Hauser (1973) who matched survey data from the 1960 Census long form, conducted in April of 1960, to death records from the May - October 1960 period. The stylized facts from their work are that mortality rates decline with income but at a decreasing rate. This relationship is present for all age groups but Kitagawa and Hauser find less variation in mortality across socioeconomic groups for the elderly. The more democratic nature of morality among the elderly has also been recently documented by Hurd, McFadden and Merrill (1999) and Deaton and Paxson (1998). As we illustrate below, these stylized facts are present in data sets 30 years later than the one analyzed by Kitigawa and Hauser.

The data for the analysis in this section is a sample of individuals from the National Health Interview Survey's (NHIS) Multiple Cause of Death (MCOD) file. The NHIS is an annual survey of 100,000 people from 40,000 households designed to track illness and disability among the non-institutionalized population. Each NHIS had two components. The first is a household file that contains basic demographic information, self-reported health status, height and weight, lists of chronic and acute conditions, and counts of doctor visits and hospitalizations as well as a measure of family income for all household members. The second component of the NHIS are special-interest modules that survey samples of core respondents about current health topics. Modules vary in size and scope and in many years there are numerous special topics.

In an important extension of the NHIS data, the MCOD data file was constructed by merging individual-level records from the 1986-1994 data files with the National Death Index. The MCOD/NHIS identifies whether individuals in the NHIS have died by the end of 1995, when they died, and the multiple causes of death.

As we explain below, our test for whether the notch altered mortality is to compare mortality rates over the first five to eight years of retirement for those born just before and after the notch. The relevant population is therefore people roughly 65 years of age. We want our baseline to reflect the long follow-up periods we use later on so we must eliminate the latest years of analysis from NHIS/MCOD data. We also delete the first year of data, 1986, because the NHIS/MCOD was only one-half the size of other years.

Because our population of interest is such a small birth cohort, we would like to pool as many NHIS surveys together as possible to enhance the sample sizes. Unfortunately, one shortcoming of the NHIS is that family income is a categorical variable where categories have not changed for many years.[5] Inflation makes it impossible to group more than a few years worth of data. To provide as large a data set as possible, we pool data from the first three full-size surveys, 1987-1989.

For each year of the NHIS/MCOD, we have detailed demographic information from the core NHIS data file as well as month, year and cause of death for those who died. NHIS respondents are surveyed throughout the year and the quarter and week within the quarter when the interviews are conducted are coded on the data file. From this information, as well as the data on the month and year of death, we can construct an indicator that measures whether a person died within 5 years to the month of their initial NHIS interview.[6]

To illustrate that this data set can reproduce the stylized facts regarding the correlation between income and mortality, we estimate linear probability equations for three populations: males aged 21-44, 45-59, and 60 and up. The dependent variable is whether a person dies within 5 years of the initial survey. The controls include a complete set of single-year age effects, indicators for white and black respondents (with other race being the reference group), an indicator for Hispanics, seven education dummy variables, plus measures of family income.

In the top-portion of Table 1, we report the sample means and sample sizes for each age group. The stark difference in five-year mortality rates across age groups illustrates the need to estimate models for separate age groups. In the next block of results, we report linear probability estimates where the key covariate is income coded as a categorical variable in \$10,000 increments. The reference group in these

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[5]For the years of NHIS/MCOD that we consider, the family income variable has 27 groups: 20 categories in \$1000 increments through \$20,000, 6 groups in \$5,000 increments through \$50,000, and those making in excess of \$50,000 per year.

[6]We should note that the NHIS/MCOD is limited in that it only contains data for the non-institutionalized. This is not a problem for those aged 21-44, but for higher age groups, the fraction of those institutionalized increases and deaths are high for these groups. In contrast, our reduced-form results in section V contain data for those institutionalized as well.

models are those with annual family incomes less than $10,000. The results from these model illustrate that for all age groups, higher income groups have lower mortality but as incomes rise, the coefficients increase (in absolute value) at a decreasing rate. Consider the case of those aged 45-59. Moving from the lowest to the second lowest income group decreases the 5-year mortality probability by 2.5 percentage points. However, moving from $30,000 - $39,000 to incomes in excess of $40,000 only changes this probability by .61 percentage points. In the final column of Table 1, we report results for those aged 65 and 66 which is representative of the population we examine below. In this case, we see the protective effects of income but a much more linear relationship between income and five-year mortality. As we move through the income groups, adding $10,000 in income ($1^{st}$ to 2^{nd} income group, 2^{nd} to 3^{rd}, 3^{rd} to 4^{th}, 4^{th} to 5^{th}) reduces the probability of death by 5.67, 0.48, 2.20 and 4.62 percentage points, respectively In general, moving from the 1^{st} to 2^{nd} group does produce a larger decline in mortality than movements between other income groups, but the estimated impact is not monotonic.

We can capture the nonlinear relationship between income and mortality using log-income as the single co-variate of interest. Unfortunately, income is top-coded at $50,000 in the NHIS and 19 percent of all men aged 21 and higher report this top-coded value. We use the following procedure to compensate for top-coding. First, we assume income is log normally distributed with a mean of and a standard deviation of and use the responses to the 26 income categories to estimate an ordered probit model. This model produces maximum likelihood estimates of u and , and with these values, we calculate the expected value of log income given that it is top coded. Finally, we use this amount for top-coded responses. For all other persons we use the log of their income category's midpoint.

The results from models with log income as the covariate of interest are reported in the bottom of Table 1. Comparing the first three broad age groups, a fixed percentage change in income actually has a larger impact on mortality for older respondents, but the implied income/mortality elasticity is much lower for the oldest respondents. Both of these results are driven by the higher probabilities of death for those aged

60 and up. The marginal impact of a change in income for our population of interest, those 65 and 66, is 20 percent larger that for those aged 60 and up, but the elasticity is roughly similar to the one for those aged 45-50.

Our results, similar to those of other researchers suggest that if single-equation estimates are consistent, for those aged 65, we would expect a 10 percent increase in income to reduce five-year mortality rates by a half a percentage point, from 13.5 percent to 13 percent, a change in the rate of 3.88 percent. This is the standard of comparison for the estimates that follows.

III The Social Security "Benefits Notch"

The codes establishing Social Security were contained in three sections of Public Law 271, enacted in 1935 by the 74th Congress. Initial Old Age and Survivors Insurance (OASI) payments were a function of the beneficiary's "average nominal wage" and the retiree's age at the time of retirement. These payments remained fixed until Congress passed legislation altering either the method for calculating the average wage or the schedule of benefits. Although beneficiaries lost ground to inflation until Congress acted, it did so frequently, amending the benefit formula 16 times between 1935 and 1972. The real value of Social Security benefits increased substantially over this time. Part of this increase was due to the higher real wages of younger cohorts, but much was due to Congress' generosity. By the early 1970s, Social Security was the largest and least controversial government social program (Munnell 1977, Tynes 1996).

Social Security has from the start been a pay-as-you-go system. Taxes on current workers finance current retirees' benefits. Until the 1970's benefits were figured by computing an average wage and reading the appropriate benefit from a table set by statute. During this pre-70's period, nominal wages rose faster than inflation and the pool of workers paying Social Security taxes expanded. This led to a large current surplus in Social Security and a large projected surplus when current benefit levels were compared to inflated future wages. Based on these projected surpluses, between 1972 and 1974 Congress substantially increased benefit levels. At the same time, Congress instituted a system for indexing the benefit table to the Consumer Price Index (CPI). The early 1970s were a period of relatively high inflation; waiting for statutory adjustment was increasingly costly, and consequently unpopular with seniors.

In figuring benefits based upon an unindexed average wage and an indexed benefit table Congress set the stage for Social Security's first financial crisis. Indexation shields current retirees from inflation, but it also leads to higher initial benefits for new retirees with a given (nominal) wage. However, wages do not stay fixed during a period of inflation. A worker with rising nominal but flat real wages still has a rising "average wage," thus even an unindexed benefit schedule will lead to higher (nominal) initial Social Security payments. When the benefit schedule is fully indexed to inflation, initial benefits will rise faster than inflation.

Looking back, the double indexation of benefits seems clearly an error. If, however, wages and prices had behaved in the 1970's as they had in the previous decades, the increase in real benefits would have been consistent with long term trends and the wage base would have been sufficient to support such benefits. However, during the 1970s, wages growth lagged inflation and projections showed a possibility of insolvency as soon as the mid 1980's (Commission on the Social Security "Notch" Issue, 1996). This led Congress to address the problem with uncharacteristic speed. The result was the first widespread reduction in the generosity of the Social Security system and as a side effect, the creation of the "Benefit Notch."

Congress chose to correct the system by replacing the nominal wage with an indexed wage. The average wage in the year a claimant turned 60 would be used as a basis, and the claimant's earnings in year x would be multiplied by the ratio between average earnings in year x and average earnings in the year he or she turned 60. The legislation, enacted in 1977, allowed those who were eligible for retirement before the new amendments became effective to stay in the old system. Those who were not yet eligible would be forced to use the new system. The effective date of the new amendments was January 1, 1979. Therefore, those born on or before January 1, 1917 could stay in the old system. Those born after January 1, 1917 would be in the new system.

To minimize the abruptness of this change, Congress created a special five-year transitional method for people born between 1917 and 1921. Retirees born between 1917 and 1921 are the group commonly known as the "Notch Babies." The transitional method was identical to the old method except earnings after age 61 could not be used in figuring benefits, and after 1978, no inflation adjustments would be made until age 62. Retires in the 1917-21 cohorts could take the higher of the two benefits, the new or the transitional. Since earnings after the age of 61 were generally years of high nominal earnings, these rules were of limited assistance. They typically helped only those who retired in or near 1979, but were of little help to the majority who worked into the 1980's. The transitional method was only designed to lessen the impact of the law change. It did not alter the fact that people born after January 1, 1917 would receive, with few exceptions, lower benefits than those born prior to that year. This of course was the intent of the 1977 law. This was the first and, to date, only time the generally rising trend in Social Security benefits was reversed.

The principal novelty of what has become known as the "benefit notch" is that younger cohorts of Social Security beneficiaries generally receive less in old age benefits than older cohorts with similar work histories. The Social Security Act had been amended many times, but before 1977, changes in the act generally allowed a beneficiary to choose whether to claim under the new provisions or under the prior law. This meant that new provisions could only increase a retiree's benefits. The 1977 amendments offered no such choice; unlike earlier amendments their purpose was to reduce payments.

In the next section, we document the differences in monthly benefits between those born before and after January 1, 1916. The Notch became as large as it did for two reasons. First, those born before 1917 could continue to benefit from the effects of over indexation no matter when they retired. Second, the years when the pre-1917 cohorts were likely to remain in the labor force were years of high inflation. In the years 1979 through 1982, the annual increases in the CPI were 9.9, 14.3, 11.2 and 7.4 percent, respectively. Had inflation remained at 5 percent between 1979 and 1982, the difference in monthly payments between those born before and after January 1, 1917 would have been much smaller.

It is customary to refer to the cohorts born between 1917 and 1921 as the "Notch babies." These workers were subject to the transition rules and they received less money than those born slightly earlier. Their benefits however, were in line with long term trends. It is probably more helpful to think of those born just before the Notch cohorts as the "treatment" group. They received more income than the long run trend of OASI payments and more income than Congress intended to give. This is an important distinction in understanding the continuity of retirement behavior across the cohorts. In effect, the Notch was a windfall for the older cohorts, not a calamity for younger cohorts.

There is scattered evidence about the impact of the Notch on Social Security payments. The bipartisan Commission on the Notch used estimates calculated by the Social Security Administration's (SSA) Office of the Actuary using the same computer program that Social Security field offices use to calculate actual benefits. A necessary input into the program were estimates of the work history of the Notch cohorts. Because the impact of the Notch on payments is a function of retirement age, simulations were conducted for two groups: those that retire at 62 and 65. The numbers from these simulations suggest that the Notch generates a loss of $7 for those born after January 1, 1917 and retiring at 62. The difference however rises to $110 if a worker retires at age 65. Krueger and Pischke (1992) take a similar approach, but they calculate benefits for each cohort at many different retirement ages. In both cases, the authors use as wage histories the average covered earnings for the years these cohorts were working.

We use a similar methodology as the two previous efforts, but we use a different time series of earnings histories for the Notch cohorts. Since the wage profile varies with age, and since there are significant cohort effects on wages, using average earnings for all workers potentially introduces errors into the calculation of benefits. We use instead cohort-specific earnings profiles constructed from Census data and various March Current Population Surveys (CPS). In particular, we calculate time series for three groups of male workers: those that report 8, 12 or 16 or more years of education. Data for the years 1964 through

1982 are taken from the March CPS. We calculate the earnings for only those who are from the 1916 and 1917 birth cohorts (e.g., approximated by those who report ages of 59 and 60 in the March 1976 CPS). Data from the 1950, 1960, and 1970 Census PUMS generates estimates for 1949, 1959 and 1969 respectively. Data for the period 1950 through 1963 are interpolated using the Census estimates. Revisions to the Social Security law eliminate the need to consider earnings before 1950 in calculating benefits; benefits are figured only on post-1950 wages for those with income after 1950. The time series of earnings for the three education groups is displayed in Figure 1.

Our calculation of OASI benefits are done by first breaking the Notch cohorts into a series of cells based on age at retirement and years of education. We then calculate a benefit for each side of the Notch in each cell, take the differences, and figure an average difference by weighting each cell according to its share of the cohort. To obtain estimates of the fraction of the population that retires at particular ages, we use data from the Social Security Administration's New Beneficiary Survey (NBS). The NBS is a one-percent sample of all those making an initial claim for benefits between June 1980 and May 1981. We use the entire population of male retirees to estimate the age distribution of new claimants. Since the NBS contains month of birth and month of initial claim we can figure age at claiming to the month. Figure 2 shows that the distribution of claims is extremely lumpy. Virtually half of all claims by men are filed the month of the pensioner's 62 birthday (22 percent) or 65th birthday (25 percent).[7] After the first month, claims are steady at one percent per month for ages 62 and 63, rising to 2 percent per month at age 64. Over 95 percent of males filing initial claims were between the ages of 62 and 65. In the early 1980's there was only a very small enhancement to benefits for working beyond age 65. From the data in Figure 1 we create five (year/month) cells, 62.0, 62.6, 63.6, 64.6 and 65.0. We have placed those working past 65 into the 65.0 cell.

The number of sample points in each year's data are quite small, but this makes little difference in the final estimates for two reasons. Social security is figured from a simple average of the highest N years

[7]A change in the law governing minimum age for claiming actually occurred during the year from which the NBS drew its data. We use 62.0, even though the minimum age changed to 62.1 in January 1981 to simplify exposition.)

of covered earnings (N=21 or 22 for these two cohorts), so the averaging across years reduces the impact of any year-to-year estimate. Second, the "covered" earnings are limited to a certain maximum, so variations in wages above the maximum will have no effect on benefits. Figure1 shows the three profiles and the maximum covered wage for each year. For most of the period under consideration the Social Security system was quite egalitarian, average wages of high school graduates (and certainly college graduates) exceeded the maximum. A large fraction of the working population earned identical Social Security "credits," even when their wages differed substantially.

In figuring the weights for each education/retirement age cell, we used the proportions from the 1980 Census data for each of the 3 levels of educational attainment. We did not make any effort to assign other educational levels to one group or another. Given the mild gradient in the Notch effect across the educational levels, inclusion of other education levels could have only minor effects on the results.

Tables 2 presents our findings from these simulations. They are in line with previous estimates, showing that the Notch is very small for those retiring at 62, and much larger for those who work an additional 3 years. The resulting 7 percent difference in benefits is thus an amalgam of a large group with a reduction around 10 percent, and a smaller group with a reduction of 1 or 2 percent. Also, the 10 percent reduction is conservative, since there is a small group working past 65, who will continue to add additional years of high earnings to their benefits. In the end, we estimate that the 1916 cohort was receiving on average $41 more per month than those born in 1917. For these groups, average family income was about $12,000 per year when they reached age 65, so this represents about a 4 percent increase in income.

We would like to examine the impact of the Notch on Social Security incomes in a regression context with micro data but most data sets do not identify month and year of birth. Had the 1990 census asked for quarter of birth we could have used that data. The 1980 Census PUMS does identify quarter of birth but the earnings data from the Census related to 1979 when the 1916:4 and 1917:1 cohorts turned 63 and 62 respectively. We can however obtain an estimate of the Notch effect on Social Security payments using data from the Annual Demographic File of the March CPS. The March CPS contains information about income received in the previous year and one category is Social Security payments. The main drawback to the CPS is the poor identification of birth year. The questionnaire asks for age at the time of the survey (mid March) and for earnings and employment in the previous calendar year. Year of birth can therefore only be defined as (survey year) - age -1 . This will place about 80 percent of respondents with the appropriate calendar year of birth, but the other 20 percent (those born January through mid March) will be grouped with those born in the previous year. Since it is calendar year that changes the applicable benefit formula, this will bias any estimate of a Notch effect on Social Security payments downward.

To conduct the test, we first extract a sample of males who report aged 68 in 1985, 69 in 1986, 70 in 1987. These males are roughly from the 1916 birth cohort and in these surveys, this cohort reports Social Security payments received when they were 67-69 years of age. We then draw a companion sample of those aged 67 in 1985 through 69 in 1987. Most of these men were born in 1917 and these respondents report Social Security earnings ages 66-68.[8] Next, we regress real annual Social Security earnings in 1987 dollars on a complete set of fixed effects for race, education, marital status, and reporting year, plus a dummy for the older (1916) birth cohort. The results from this exercise are reported in Table 3. This sample has a large number of observations (3059) and real mean annual Social Security earnings over these four years is slightly under \$5,900. During this period, the 1916 birth cohort received \$496 more per year than their nearest younger cohort, about 8.4 percent of than the sample mean. Since only 80 percent of the people we put into the 1917 birth cohort are actually from that group, with 20 percent coming from the 1916 group, we would expect this number is low by about 20 percent. Inflating the parameter estimate by 20 percent and dividing by 12, we find that the Notch increased payments to the 1916 birth cohort by about \$50 a month, similar to

[8]It is important to only include earnings starting in 1984 for the 1917 cohort because including data for 1983 would add Social Security payment data for this group in a year when these men turn 65 and as a results, many will not have a full years' worth of earnings data.

the estimate we produced through our simulation exercise in Table 2.[9]

To establish that this difference is not found in the other populations we will compare to the "Notch" cohort we report comparable results for two other groups, women born in 1916/1917 and men born 1915/1916. During this time period, most women who receive OASI payments are qualifying for payments based on their husband's earnings (Reno and Ycas, 1982; Iams and Ycas, 1988). Subsequently for women, there should be no Notch effect when we compare the 1916 and 1917 cohorts. In the next column of the table, we redo the same exercise with women from the same years and age ranges, we see that women from the 1916 birth cohort earn only \$9 more per year than their nearest younger cohort. These results are presented in column (2) of Table 3. If we take men who were 67 in 1984, 68 in 1985, and 69 in 1986, we generate a sample of Social Security earnings from the 1916 cohort when these men were 67 through 69. If we match this group to men who were 68 in 1984, 69 in 1985, and 70 in 1986, we produce a sample of earnings for men aged 67-69 from the 1915 cohort. Defining the "Notch" dummy variable for the "older" cohort (those born in 1915), we see in the final column of the table that men from the 1915 cohort only earn \$8 more per year from Social Security than their next younger cohort. These last two results are important because later on, we will use mortality differences for women from the 1917 and 1916 cohorts and men from the 1916 and 1915 cohorts as comparison groups for our primary comparison – the mortality difference between the 1917 and 1916 cohorts. Therefore, after retirement, there are large differences in Social Security earnings in our primary groups of interest (1916 versus 1917) but no such differences in our comparison groups.

Although the benefits Notch was a windfall for some cohorts, there is little evidence that the differences in OASI payments for cohorts born in the 1911-1921 time period altered retirement behavior.

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[9]Respondents in the CPS are in the sample for the same four months over a two year period (for example, January through April in both1985 and 1986). As a result, our sample will sometimes include two observations from the same household but in different years. Restricting our attention to people who were in the sample for the first year, we cut the sample in half, but the coefficient on the "Notch" dummy variable is essentially unchanged dropping to \$487.51 with a standard error of \$155.76.

For those born prior to 1917, we might expect the increase in retirement income to induce an earlier exit from the labor force. However, because Congress continued to allow over-indexation for older cohorts and these workers reached age 62 at a time when inflation was high, the longer an individual stayed in the labor force the greater the impact of over-indexation. In effect, Congress created a substitution effect to offset any wealth effect. The cost of retiring young was high, which would tend to delay retirement. Krueger and Pischke (1992) find no difference in the retirement profiles for those born before and after January 1, 1917.

Another explanation for the apparent non-impact of 1977 amendments was that the changes in the benefits generated by the new law were not widely understood. The true impact of the benefit changes was produced through the interaction of legislation and the inflation experienced after the legislation passed. The perceived unfairness of the Notch only became a political issue after the 1917-1921 cohorts began to retire. The Commission's account of public awareness of the Notch is instructive:

> "*After* comparing their benefit checks against the larger checks of their pre-"Notch" colleagues, neighbors, and friends with similar employment records, they began expressing their dissatisfaction to public officials- and the "Notch" issue was born." (Commission on the Social Security "Notch" Issue 1996) (Our italics)

Some may find it puzzling that there was not more foresight among the general public regarding the change in benefits. In 2001 new retirees are accustomed to thinking of a given real benefit level as an entitlement. Until the late 1970's, however, Social Security benefit levels had always been subject to arbitrary adjustments. No one could predict their benefit, even if they could predict their wages. Survey data suggests that today still, workers have poor understanding of their future OASI benefits (Gustman and Steinmeier, 2001).

IV Econometric Model

As mentioned above, the 1977 amendments to Social Security changed the way initial OASI benefits are calculated, which in turn produced sharply lower payments for recipients born after January 1, 1917. We examine the income/mortality hypothesis in an elderly population by examining whether the higher payments received by older cohorts lead to lower mortality rates. The econometric model we propose is a "difference-in-difference" estimator where the structure of the test is driven by the available data and the need for large samples.

Within a group of pregnant women, it is essentially a random process that generates who gives birth today and who gives birth tomorrow. Subsequently, we would expect to find very little difference in the social, economic and behavioral characteristics of people born just before and after January 1, 1917. However, these two groups face different streams of OASI payments once they retire. Therefore, any difference in post-retirement mortality between these groups can reasonably be attributed to the difference in OASI payments.

This simple analysis is complicated by two facts. First, the difference in income generated by the Notch is not huge and as we demonstrate below, we will need a large data set to have any hope of detecting a meaningful difference in mortality across the two groups. This makes it impossible to compare cohorts born in the days right before and after January 1, 1917. Expanding the size of the cohorts does come at a price. There are a number of secular trends in the economic, health and social characteristics of cohorts. On average, younger cohorts have more education and live longer than older groups. If we compare outcomes for full year cohorts born in 1916 and 1917, these groups contains people that vary in age anywhere from 1 day to almost 2 years. If mortality itself or the determinants of mortality (such as education or income) are changing rapidly, comparing two groups that span so many months may introduce a difference in mortality that is produced by secular trends rather than OASI payments. Consequently we cannot rely on a simple difference estimator as the basis of our analysis.

The difference-in-difference estimator is generated by finding a control group that can accurately measure the difference in mortality after age 65 between the 1917:1 and 1916:4 groups that would have existed in the absence of the treatment (the Notch). In this case, we have two potential control groups. Each helps control for a different type of secular variation in mortality. The first includes men born one year earlier.

The second control group is women born during the same months as our male cohort. Each provides a control for a different alternative cause of differences in mortality.

There can also variation across cohorts in mortality rates based on a number of factors that may confound a simple difference analysis. For example, we know that age-adjusted mortality rates are lower for younger cohorts. Although the 1917:1 cohort is only born one quarter later than the 1916:4 cohort, one might be suspicious that a higher mortality rate after age 65 for the 1916:4 group is a secular difference rather than a shock produced by the impact of the Notch. Therefore, we will use mortality differences for males between the 1916:1 and 1915:4 cohorts as a second control group.

There is growing evidence that conditions present before and right after birth have a lasting impact on health. Barker (1998) for example argues that when the fetus is faced with a poor environment, changes in the supply of nutrients received by vital organs can hard wire these organs for later susceptibility to disease. Using a variety of models, Almond (2001) finds that the 1918 birth cohort, which was born during a major influenza epidemic, has substantially higher age-adjusted mortality during the 1980s than adjacent cohorts. If there is something that adversely impacted either the 1917:1 or 1916:4 birth cohorts, we might find differences in mortality across these groups that is unrelated to the Notch. To control for this possibility, we use women born in the 1917:1 and 1916:4 periods as a control. Women born in 1916/1917 face the same perinatal infant environment as males. As we note above, most women born in this generation receive Social Security benefits based on their husband's earnings, and, since most women marry men older than them, there should be little impact of the Notch on Social Security income across these narrow birth cohorts. This is in fact verified in Table 3. Women at age 65 do however have substantially lower mortality rates than men the same age so in this model, we use difference in log mortality rates as the key outcome.

A. A Note on Sample Sizes

We showed using two different procedures that the Social Security Notch generated about 4 percent higher incomes among those born in the 1916 compared to those born in the next year. In Table 1, we showed that for people aged 65 and 66, the coefficient on log income in a linear probability model where the outcome is 5-year mortality is about -0.05. Therefore, if this relationship represents the "true" impact of income on total mortality, we would expect those born before 1917 to have 2 tenths of one percentage point lower five-year mortality rate than those born in 1917 (0.04*(-0.05) = -0.002). In this section, we use a simple calculation first suggested by Evans and Ringel (1999) to estimate the sample sizes necessary to produce a statistically significant estimate of -0.002. This calculation is necessary because we have to expand our samples around January 1, 1917 and we want to expand them enough to have a fair test of detecting a statistically precise relationship.

Suppose we have two equally size cohorts -- one born after January 1, 1917 and one born just before. The reduced-form equation of interest can be written as a simple bivariate regression

$$(1) \qquad y_i = \alpha + \beta_{RF}\, z_i + \epsilon_i$$

where y_i is an indicator that equals 1 if the respondent died within 5 years of their 65th birthday and z_i is an indicator for whether the respondent is pre Notch baby, i.e., someone born in 1916. Let $p_1 = (y|z_i{=}1)$ and $p_0 = (y|z_i{=}0)$. Because both z and y are discrete, the estimate for $_{RF}$ in equation (1) can be shown to equal.

$$(2) \qquad \hat{\beta}_{RF} = (\bar{y}|z_i{=}1) - (\bar{y}|z_i{=}0) = \hat{p}_1 - \hat{p}_0$$

This reduced-form estimate will only be statistically significant if:

$$(3) \qquad \left|\frac{\hat{\beta}_{RF}}{\hat{\sigma}_{RF}}\right| \geq 1.96$$

where $\hat{}_{RF}$ is the standard error of $_{RF}$. Under the assumptions we have made, we can solve this expression for the minimum number of observations necessary to generate a statistically significant coefficient of $_{RF}$ = -0.002. Because y is discrete and both samples are assumed to have the same number of observations, $^2_{RF}$ approximately equals $[p_1(1-p_1) + p_0(1-p_0)]/n$. For equation (3) to be true, it must be the case that n [1.96/ $_{rf}$ $]^2[p_1(1-p_1) + p_0(1-p_0)]$. Using the means from Table 1, we can set p_0 =0.13 and notice that $p_1 = p_0 + {}_{RF}$. It is then easy to show that n, the size of the treatment and control groups, must be approximately 214,000 observations. This is roughly the size of the number of people born in the 1st quarter of 1917 who are alive at the time of the 1980 Census. At the start, we then want to compare the post-retirement mortality for people born in the 4th quarter of 1916 and the 1st quarter of 1917.

B. Pre-retirement Characteristics of the 1916:4 and 1917:1 Male Cohorts

Going into retirement, there is little difference along any demographic characteristic between these two cohorts. This is easily demonstrated with data using the 1970 Census Public Use Micro Samples (PUMS). The PUMS samples are drawn from the one-sixth of the households who received the Census long form. In 1970, Public use data is available from six different samples. The samples differ in geographic information available and in the questionnaires they received, but the core demographic information we need is common to all samples. By aggregating data from all of these samples, we obtain a 6-percent nationally representative sample. For our purposes, one key piece of information in the PUMS is the respondents quarter of birth. Using the fact that the Census day is April 1, 1980, the first day of the second quarter, we can then back out the quarter and year of birth. Respondents from the 1916:4 and 1917:1 cohorts would report 53 years of age in the 1970 Census. In Table 4, we compare the means across the 1916:4 and 1917:1 cohorts along a number of different demographic characteristics. In the table, we report the sample average for each variable when these two cohorts are pooled together, the difference in means across these cohorts, and the t-statistic on this difference. We see there is no statistically significant difference in earnings, years education, the fraction with a high school degree, weeks worked, hours worked per week, the fraction that worked full time, the fraction self employed, marital status, or the fraction disabled. The small differences which exist in observed characteristics between the two cohorts is not unique. We report the same type of results for different 4th quarter/1st quarter contrasts and the only persistent difference is that those born in the 1st quarter have lower education levels. In this case, the result is statistically significant in 3 of 5 cases. This result has been documented, notably by Angrist and Krueger (1991), who use data from the 1970 and 1980 Census PUMS data sets to show that in cohorts born between 1920 and 1949, men born in the 1st quarter tend to have about one tenth of a year fewer years of education than do those born at other times of the year. Angrist and Krueger interpret this as being generated by interactions between laws governing minimum school start age and compulsory education laws -- those born in the 1st quarter start school at an older age and they are more likely to age out of compulsory education laws, making them more likely to drop out. The differences in education in our data are very close to those reported in Angrist and Krueger.

C. Restricted-Use Mortality Detail Data

Although there are some retrospective data sets that identify mortality for a cross-section of people (such as the National Longitudinal Mortality Survey and the NHIS/MCOD file introduced above), the sample size calculations above show these data sets are inappropriate for the task at hand. The only data sets of the appropriate size are the Mortality Detail data sets. Detailed micro data on births and deaths are available starting in 1968 from the NCHS Natality and Mortality Detail data files. Mortality Detail files contain a census of births in all years except in a few when a 50 percent sample is available. The Mortality Detail data provides information on age, sex, race, ethnicity, plus detailed geographic data about the place of residence and death. Some states also provide education and the industry and occupation of the deceased. The mortality data also identifies the month and cause of death. Public use tapes do not however provide year or month of birth. This data is however available on research files available to NCHS staff. We reached a special agreement with the NCHS and they provided us with restricted-use Mortality Detail files that contain all public-use information plus the month and year of birth. We received data for deaths over the 1979-1990 period.

With this data, we can count the total deaths from a birth cohort after a particular date. We must however, control for the possible differences in cohort size that exist. For example, the cohort born in the 1st quarter of 1917 is slightly larger than the cohort born in the previous quarter, so we would expect to find some difference in the number deaths across these two groups. We therefore need to construct death rates that condition on the number of people alive in a cohort at the start of the period. To calculate the denominator in the death rate, we use population counts of cohort size from the 1980 Census Public Use Micro Samples (PUMS). The 1980 PUMS data is available from three samples: the 5-Percent, a 1-Percent Urban/Rural sample and a 1-Percent Metro Sample. All three are nationally representative samples of the population and merged together represent a 7-percent random sample of the population. Dividing the number of people in a particular birth quarter by .07 in these combined samples, we then have an estimate of the number of people from the cohort who were alive on April 1, 1980. Counting deaths in previous or subsequent quarters for cohorts and subtracting or adding these numbers from population total from the Census, we have an estimate of the number of people alive at the start of any particular quarter. For example, those born in 1916:4 turn 65 in the 4th quarter of 1981. Subtracting deaths for this cohort in the 1980:2 - 1981:3 from the population numbers from the Census gives us the number of people alive as of the start of the quarter this group turns 65.

For each cohort, we want to construct death rates over a fixed period of time. The beginning periods for the death rates will be defined by birth dates rather than calendar time. Although cohorts born before and after January 1, 1917 are roughly the same age, calculating death rates for say the January 1, 1982 - December 31, 1986 period will, by construction, produce differences in death rates simply because the 1917 cohort is younger than the 1916 cohort. We therefore must define death rates over the same age. Because the vast majority of people on Social Security have retired by their 65^{th} birthday and because there is little Notch effect for those who retire, we construct five and eight-year mortality rates that of people who died within 5, 7 and 9-year years of their 65^{th} birthday.

Death rates is constructed as

(4) $DR(J)_i = D(J)_i/n_i$

where D(J) is the number of deaths over the next J years after the quarter cohort i turns 65, and n_i is the number of people from a cohort who lived until the quarter they turned 65. Standard errors on rates such as $DR(J)_i$ are typically defined by sampling variance. In this case, there is no sampling variance in the numerator – we have a complete census of deaths for the cohort for the J years after their 65^{th} birthday. The sampling variance is introduced into the variable because the denominator is based on a 7 percent sample. Using a 1^{st}-order Taylor's Series expansion, the variance on $DR(J)_i$ is defined as

(5) $Var[DR(J)_i] = D(J)_i^2 \, Var(n_i)/n_i^2$

and $Var(n_i)$ is calculated as follows. Suppose there are N people surveyed in the PUMS, within the population there is a fraction p_i people from cohort i and the sampled fraction is p_i. Because the PUMS is a 7 percent random sample of the population, the number of people we estimate in the cohort is therefore $n_i = N\, p_i/0.07$. Since whether a sampled person is from cohort i or not is the results of a Bernoulli process, $Var[n_i] = Np(1\text{-}p)/(0.07)^2$. A consistent estimate of $Var[n_i]$ is obtained by using p_i instead of p.

V Results

In this section, we present basic difference-in-difference estimates, comparing the five-year mortality rates after age 65 for those born in the 1916:4 and 1917:1 quarters. To control for any difference in mortality for men generated by a quarter of birth effect, we use our first control group men born one year preceding the year of the Notch as comparison groups. In the first test, we examine the difference in cumulative mortality rates after age 65 for males from the 1916:4 and 1917:1 cohorts. There may be persistent differences in mortality between these two cohorts generated by the their quarter of birth. For example, we know that among the elderly, there are more deaths in the fourth quarter than in 1st quarter. We also know that death rates increase with age. Among the 1916:4 and 1917:1 cohorts, using mortality from age 65:0, the older group is turning older at a worse time of year. As the cohort ages, we may see above-average mortality for this group, solely due to an interaction between the quarter of birth and the age profile of mortality. To eliminate this type of bias, we can use the contrast in mortality between younger cohorts with 4th and 1st quarter births as a difference control group for this analysis. Specifically, we start by using the difference in the five-year mortality rate between the 1915:4 and 1916:1 cohorts as a measure of the expected difference in mortality due to these seasonal factors. Because the five-year mortality rate is on roughly the same scale for the 1915:4/1916:1 and 1916:4/1917:1 contrasts, we focus on differences in mortality.

A graphical presentation of this estimate is presented in Figure 3. In this figure, we graph the difference in cumulative mortality for the 1916:4 and 1917:1 cohorts as well as the 1915:4 and 1916:1 groups. Notice that this latter difference tends to hover around zero for the 20 quarters after age 65. In contrast, the difference in cumulative mortality for the Notch cohorts (1916:4 - 1917:1) grows steadily over time.

The numeric results that correspond to the graphical presentation are reported in the top half of Table 5. There is almost no difference in five-year mortality rates for the two pre-Notch cohorts and the difference-in-difference estimate again suggests that if anything, the 1916:4 cohort has higher mortality than the 1917:1 group. In this case, if the results in Table 1 represent a causal relationship, the expected change in mortality should be -0.002 percentage points – a 4 percent increase in income should produce about a -0.002 percentage point drop in mortality. The 95 percent confidence interval for this estimate is (-0.0012, -0.0028). Again, the standard error on this estimate means that a 95 percent confidence interval will overlap the confidence interval from the expected results. In the top half of Table 6, we expand the sample so that we compare 1916:3 and :4 to 1917:1 and :2 using the difference in mortality between 1915:3 and :4 and 1916:1 and :2 as the control group. In this case, the results are nearly identical to those in Table 5, but the standard errors are now small enough such that we can reject the hypothesis that the single-equation and reduced-form models are providing the same statistical coefficients.

As a check on our estimates, we can repeat the analysis from Figure 3 and the top halves of Tables 5 and 6 using female rather than male mortality rates. As we noted previously, most women from these cohorts quality for Social Security based on their husband's earnings. Subsequently, there should be little Notch-effect on Social Security income for women in the 1916:4 and 1917:1 cohorts. This fact is verified in Table 3. If our difference in difference model is controlling for the secular differences in mortality that should be impacting the Notch cohorts, then we should see a small notch effect on mortality when we re-run the basic models using data for women.

In Figure 4, we present a graphical presentation of the basic difference in difference results using women from the 1917:1, 1916:4, 1916:1 and 1915:4 cohorts. In this graph, although women have lower mortality rates than men, we have kept the scale of the graph the same to allow a comparison with the results from Figure 3. The solid line in the figure is the difference in quarterly cumulative mortality rates between the 1916:4 and 1917:1 cohorts. In contrast to the results for males, for these cohorts, there is no systematic difference in mortality rates for these Notch cohorts – the difference in mortality rates hovers around zero for the 20 quarter period.

In the bottom half of Table 5, we report the basic difference in difference estimates for 20-quarter mortality for females that corresponds to the graph in Figure 4. Notice that the difference in the 20-quarter mortality rate between the 1916:4 and 1917:1 cohorts is only four-tenths of a percent of the mortality rate for the 1916:4 group. The difference in difference estimate generated by using the pre-Notch difference of 1915:4 minus 1916:1 produces a small, negative and statistically insignificant Notch effect. In Table 6, when we expand the cohorts to include half-year samples, we find a statistically insignificant difference in difference estimate for women that is only 40 percent of the size of the result for males.

As we mentioned above in Section IV, comparing the difference in mortality in the 1916:4 and

1917:1 cohorts with the difference in the 1915:4 and 1916:1 groups controls differences in mortality that are associated with quarter of birth but are persistent across birth cohorts. There could however be differences in mortality between the 1916:4 and 1917:1 birth cohorts that are cohort specific. To control for permanent differences in mortality associated with these quarters, we can use women from the 1916:4 and 1917:1 as a second comparison group. In this case, because the mortality rates are larger for males than females, we compare differences in log mortality rates.

In Figure 5, we graphically illustrate this contrast. The dashed line is the difference in cumulative log mortality rates, 1916:4 minus 1917:4, for females. After the first quarter, there is a very small difference in cumulative log mortality rates for these women, with the difference bouncing around zero for the first 20 quarters past age 65. The solid line represents the difference in cumulative log mortality for males over the same period and for the same cohorts. We see a pronounced positive difference in log mortality with the 1916:4 cohort having higher mortality throughout the entire period, suggesting once again that in contrast to the conventional wisdom, the higher income, the 1916:4 group, has higher mortality rates.

A numeric version of this analysis is contained in Table 7. The top row of the table reports the 20 month (five-year) mortality rates starting from the quarter when the cohort turns 65. If the higher incomes received by the 1916:4 cohorts lead to lower mortality, we should see a lower mortality rate for this group. This is not the case. The difference in log mortality rates is actually a positive 0.0164. The second row of the table reports results for women over the same time period. If our results were driven by some time-specific variation in mortality rates we would expect the data for women to show a similar pattern. It does not. There is a slightly lower mortality rate in the fourth quarter and a difference in difference estimate suggests that the 1916:4 male cohort has a 2 percent *higher* mortality rate after age 65 than the 1917:1 cohort. However, recall that the elasticity of five-year with respect to income in a single-equation model is about -0.388 and we estimate that the Notch elevated incomes of the 1916:1 cohort by about 4 percent over the 1917:1 group, so if the single-equation estimates are true, we would expect a coefficient of about -0.01552 and the 95 percent confidence interval on this estimate would be (-0.0093,-0.0217). Given the standard error of 0.0164 on the difference in difference estimate, we cannot reject the hypothesis that 0.0142 equals -0.01552.

To increase the power of the test, we expand the cohorts by one quarter and compare mortality rates for 1916:3 and :4 with 1917:1 and 1917:2. This doubles the sample size and will decrease the standard errors by square root of 2. The results for these expanded samples are reported in Table 8. Again, with the expanded samples, there is little difference in female mortality across the two groups. There is however a large 2.39 percent difference in mortality between the 1916:3 and :4 and the 1917:1 and :2 cohorts. In this case, the difference has a t-statistic of 1.96. The 95 percent confidence interval for this estimate does not overlap with the confidence intervals for the estimates predicted from the single-equation models. In this case, the change in mortality generated by the Notch produces a result that is statistically distinguishable from the results implied by the single-equation model outlined in Table 1.

VI Why Are Mortality Rates Higher for Pre-Notch Group?

The results in the previous section are rather striking – although the 1916 birth cohort receives larger Social Security payments than slightly younger groups, five-year mortality for this group is noticeably larger, given the change in incomes. This results is in stark contrast ton the conventional wisdom. In particular, our results differ from those in Case (2001) who finds improved health from higher pension in South Africa, although that experiment provided a much larger income change to a much poorer population that the one we consider here.

Our results are however not alone in the literature. Eibner and Evans (2001) find that much of the impact of high income on mortality is actually driven by a person's relative position in their reference group's income distribution. Ruhm (2000) finds that state-specific total mortality and eight of the ten cause-specific mortality rates are pro-cyclic. The lone death category that has a statistically significant counter-cyclic relationship are suicides. In this work, the level of economic activity is measured as the unemployment rate and even though a small fraction of elderly work, Ruhm also claims a pro-cyclic relationship for those aged 65 and over. Ruhm finds some evidence that poor health habits such as smoking rise but health investments such as routine exams decline when the economy improves.

Our first thought was that changes in some health habit such as smoking rates could explain our counter-intuitive result. Smoking rates are negatively related to incomes (Evans, Ringel and Stech, 1999), but in this population, as we demonstrate below, smoking quit rates are negatively related to income. Smoking cessation, even at advanced ages, has tremendous health benefits. According to the 1990 Surgeon General's Report on smoking, male smokers aged 65-69 have mortality rates that are three times higher than those who have never smoked. In this age group, former smokers who have been off cigarettes six to nine years have a mortality rate that is 40 percent lower than current smokers (Table 7, page 95). In the end however, we found that the impact of higher incomes on smoking rates was not large enough to explain the difference in mortality across the birth cohorts. To demonstrate this point, we examined data on complete smoking histories that is available as part of the Tobacco Use Supplements (CPS/TUS) to the regular September 1992, January 1993 and May 1993 monthly CPS surveys. These three samples are designed to be pooled together to form one large data set on smoking histories. Limiting the sample to 948 males aged 65 who smoked five-year prior to the CPS/TUS, we find that 25 percent quit over the next five years. In a linear probability model, we regressed this outcome on controls for marital status, race, ethnicity, education, the month of the survey and log income. The coefficient on log income was -0.055 with a t-statistic of -2.5, suggesting that an eight percent increase in income will reduce five-year smoking quit rates by .44 percentage points. Given this modest change in the smoking rate, our back of the envelope calculation suggests that a lower smoking quit rate induced by higher Social Security payment can explain about 5 percent of the higher mortality rate among the 1916 birth cohort.

One area that we thought could possibly explain the results noted above is changing social networks.

In recent years a growing literature has demonstrated a link between social networks and mortality with those less connected to their community, friends, relatives or coworkers experiencing a higher mortality rate. This literature is reviewed in Putnam (2000). The first empirical evidence for this relationship were generated from mortality follow-up surveys from small geographic areas like Alameda County, California and Tecumseh, Michigan. These surveys tracked a random sample of people over time, collecting important demographic data at baseline, plus measures of the social network such as church and group membership and contact with friends and relatives (Berkman and Syme, 1979; Blazer, 1982; House et al, 1982; Berkman 1995 and 2000; Cohen et. al. 1997; Colantonio et. al. 1993; Zuckerman et. al. 1984). Berkman and Syme (1979) for example found that the age-adjusted death rates for those most isolated were 2.3 to 2.8 times that of others. This relationship was found to be independent of self-reported health status, socioeconomic status and health practices. The mortality impacts for the elderly are particularly important. Seeman et al (1987) found that social ties remain a significant predictor of mortality risk even for those aged 70 and older. Their results suggest that for the elderly, contacts with friends and /or relatives are the most important ties. The study by Blazer (1982) of 30-month mortality in a sample of persons aged 65 years or older at baseline found that both a general lack of social ties with children and siblings as well as low perceived support from one's social network were each independently associated with increased mortality risk.

Given this literature, we looked for ways in which more income may reduce social networks. For example, many seniors move from their lifelong homes after the age of 65. If this is positively correlated with income, the 1916 cohorts may be more likely to move than younger groups. Migration may have many benefits, but the movement away from a known social network may negatively impact health. We could however find no evidence that the Notch impacted mobility. In the 1990 Census, because the census is conducted on the first day of the second quarter, three-quarters of the people 72 were born in 1917 and three-quarters of those aged 73 were born in 1916. In these age group, nearly all who will receive Social Security have begun to claim benefits, so we can safely assume that the roughly 10 percent of the population without

Social Security income in the Census are people not eligible. Looking at the difference in 5-year migration rates between 73 and 72 year-olds with Social Security and comparing this to the difference for those outside of the system, we find no "Notch" impact on migration rates.

In a relate paper, Englehardt, Gruber and Perry (2002) use the Notch as an instrument for OASI payments in models that related whether seniors live independently. The authors find that living independently is a normal good and the elasticity of living independently given a change in OASI payments is -0.40. In this case, if living independently detracts from health, then the higher rates of living independently we would expect to find among the older yet higher-income cohorts could explain part of our results.

Another area where we might find some explanation for our results is in the post-retirement careers of the Notch generation. Research by others suggests that the Notch did not alter the pre-retirement age behavior of affected populations, but a change in retirement income might be expected to alter post-retirement behavior. In particular, because of lower Social Security benefits, we might expect the Notch cohorts (those born after 1917) to work more than older groups. As Lumsdaine and Mitchell (1999) suggest in their survey of the literature on retirement, the behavior of older Americans is increasingly complex. Older workers may leave the full-time labor force, but return as part-time workers, often working in different industries, and generally at a reduced wage from their primary career employment. This less intense form of labor force participation is aptly characterized as post-career employment. This work could have positive health benefits if the work keeps the seniors connected to the community and reduces social isolation.

We would expect to only find an impact of the Notch on part-time work. During the period for which we present mortality data individuals at or above age 65 could claim Social Security benefits and earn up to a specified amount without incurring any reduction in the OASI benefits. Once the earnings threshold was crossed, however, the reduction in benefits was severe. Benefits were reduced by \$1 for every \$2 earned. These penalties were in effect until age 70 (72 before 1982, the penalty was reduced to 1\$ for every \$3 earned in 1990). The penalty threshold increased in both nominal and real terms during the relevant time period, from \$5,000 in 1980 to \$9,720 in 1991, but even this higher level is less than half average full time earnings. The Social Security system discouraged full-time employment for OASI recipients, but not part-time employment, if the compensation was low enough. One possible response to the Notch is to increase part-time employment while receiving retirement benefits.

To investigate the possibility of a post-retirement labor force response to the Notch, we began by constructing a balanced data set of labor force information for those born between 1909-1920. Our source was the March CPS for years when the year-to-year cohorts were between the ages of 56 and 70. Only the CPS offers a large sample of the appropriate ages with earning and labor force status reported in a consistent manner. As discussed above, the main drawback to use of the CPS is the poor identification of birth year. Since it is calendar year that changes the applicable benefit formula this is a serious problem. We therefore define the Notch to equal 1 for people born in 1917 after they reach age 65. Since retirement prior to age 65 has little impact on Social Security payments, there should be little labor supply effects of the Notch. To control for the noisy identification of birth cohorts, we let the value equal 0.2 for the 1916 birth cohort.

With this data, we can construct an indicator that equals 1 is a person worked in a particular age/year cell. However, hours worked last week is only available starting in 1976 so to preserve the balance in the panel, we are forced to restrict our attention to people from the 1913-1920 birth cohorts when they were 62-70 years of age. We then construct two outcomes: an indicator for "worked" and hours worked per year. We regress this on year of birth and age effects, controls for education, race, and marital status, plus an indicator for cohorts impacted by the Notch. This model is similar in spirit and specification to that of Krueger and Pischke, with the primary difference being our use of balanced cohort samples and the definition of the treatment effect.

The results of this exercise are reported in Table 9. In the first column, workers have a 3 percentage point higher labor force participation rate than those from earlier birth after age 65. Breaking the treatment effect up into different age groups, we find in the next column that all of the difference is produced by a large increase in work after age 67. In the final two columns of the table, we continue the analysis using hours worked as the dependent variable. The same pattern is apparent. The Notch has increased work after age 65 for those born 1917 and later and all of this increase is concentrated in he post 67 age range.

Can this higher work in the 68-70 period explain the relative fall in mortality for the 1917 cohort relative to 1916? The timing is consistent with Figure 3. Notice that the cumulative difference in mortality between the 1916 and 1917 cohorts does not start to appear until after age 68. There is also some evidence that the magnitudes are aligned as well. The correlation between post-retirement work and mortality in an elderly sample is illustrated in Table 10 where we estimate linear probability models that explain five-year mortality rates for those aged 65-70 and 68-70 in the NHIS/MCOD file. In these models, we control for age, race, marital status and education and include an indicator that equals 1 if the respondent reported that they worked in the previous 2 weeks. In this model, work decreases the chance of death by 5.9 percentage points for those aged 65-70 and 4.5 percentage points for those aged 68-70. In the larger sample, this difference falls to 4 percentage points when we control for income and 2 percentage points when we control for self-reported health status and the number of bed-days experienced in the past 2 weeks. The coefficient on "Worked" is much smaller in the older sub-sample in all cases. We should note however that if work does impact health, then the last four sets of variables can show impact from work. Looking at the results in the final column and first row, and matching these up with the results from Table 10, if post-retirement work does decrease mortality by 4.5 percentage points, and the Notch increased work by 5.2 percentage points, we would expect 5-year mortality rates to fall. Also, we would expect the Notch to decrease 5-year mortality rates by 0.0023, which is nearly identical to the estimates in tables 8 and 9.

VII Conclusion

Heading into retirement, men born in the last part of 1916 looked similar in many respects to those born in the first half of 1917. They had similar incomes, labor force participation rates and intensity of work. Once they reached retirement age however, one group was rewarded with substantially higher Social Security payments than the other. We use this variation to examine the potential impact of income on mortality in an elderly population. To be clear, our research design answers a very narrow question: will mortality be impacted by transferring more income to an elderly population? Our results are somewhat counterintuitive: the 1916 birth cohort which received more income in Social Security benefits ends up to have higher mortality than the lower earning 1917 birth cohort. We suggest that these results could be driven by changes in labor supply by the older cohort. The lower incomes received by the Notch babies encouraged them to have more post-retirement part-time work. In fact, our results suggest that this cohort is 5 percentage points more likely to work during ages 68-70 than older cohorts. A number of researchers have suggested that among the elderly, social isolation is an important cofactor in mortality. If part-time work keeps the elderly engaged and helps prevent social isolation, then the increased part-time work may have reduced mortality.

Do higher incomes reduce mortality? These results suggest that in this particular case, the source of the income is probably as important as the amount of income. Here, the lower incomes for the 1917 cohort seems to have encouraged an activity that is in the end healthy. We might then expect very different results from an "income effect" generated by a wage change for example. This type of income change is particular to a specific population so the results cannot be readily applied to other groups. But as we note in the introduction, this is not an uninteresting group or policy consideration. The Federal government routinely proposes changes to social insurance programs that resemble the Notch experiment in magnitude of dollars.

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**Figure 1: Wage Profiles,
Men Born 1916-1917, by Education**

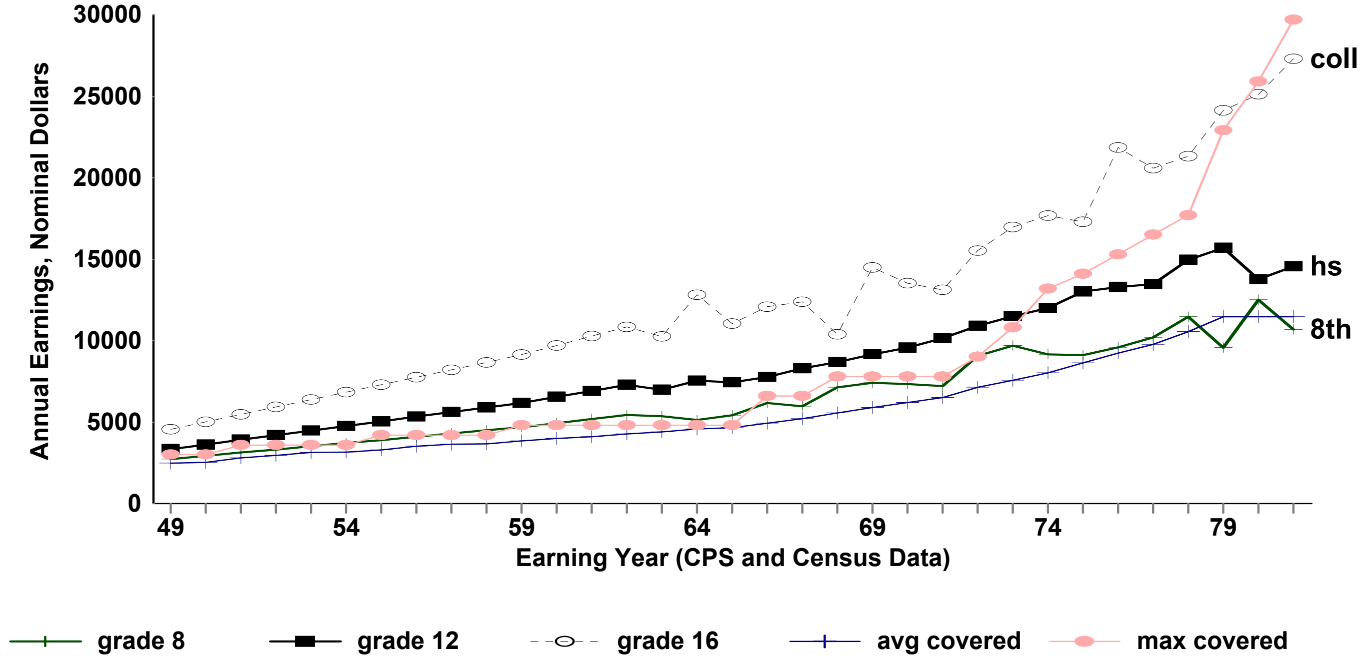

Figure 2: Distribution of Age
at Initial Claim NBS Men Age 62 - 65

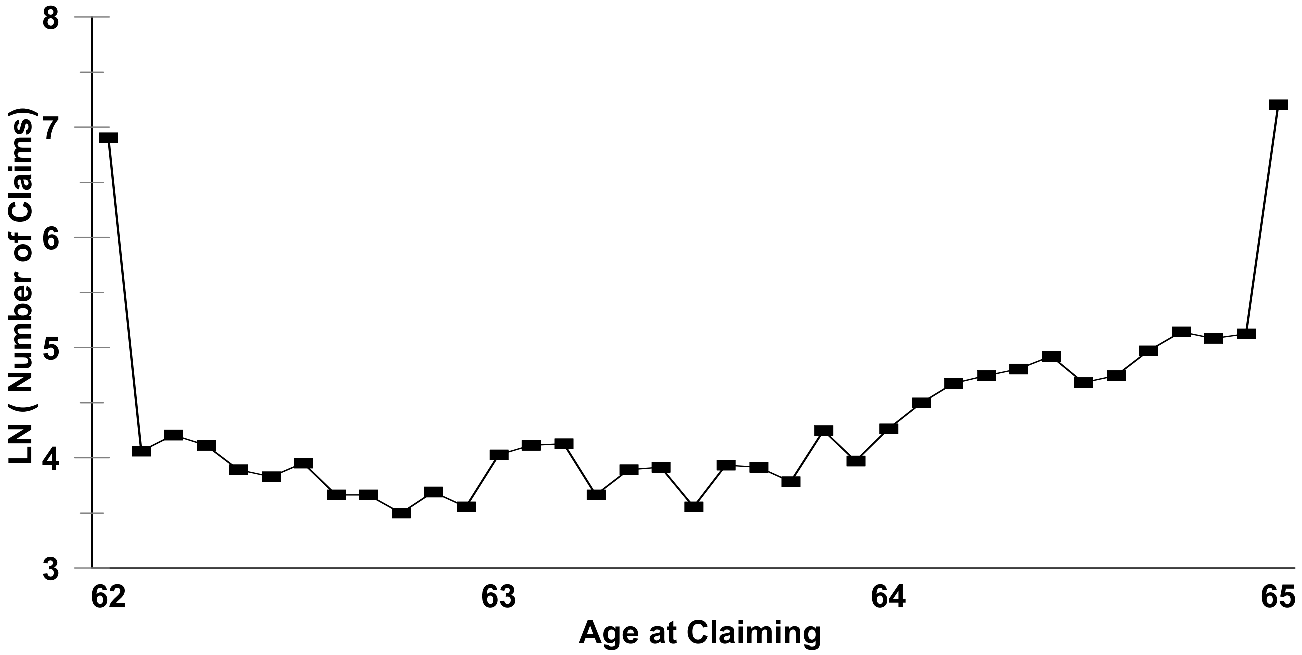

Figure 3: Difference in Difference Estimates: Males

Figure 4: Difference in Difference Estimates: Females

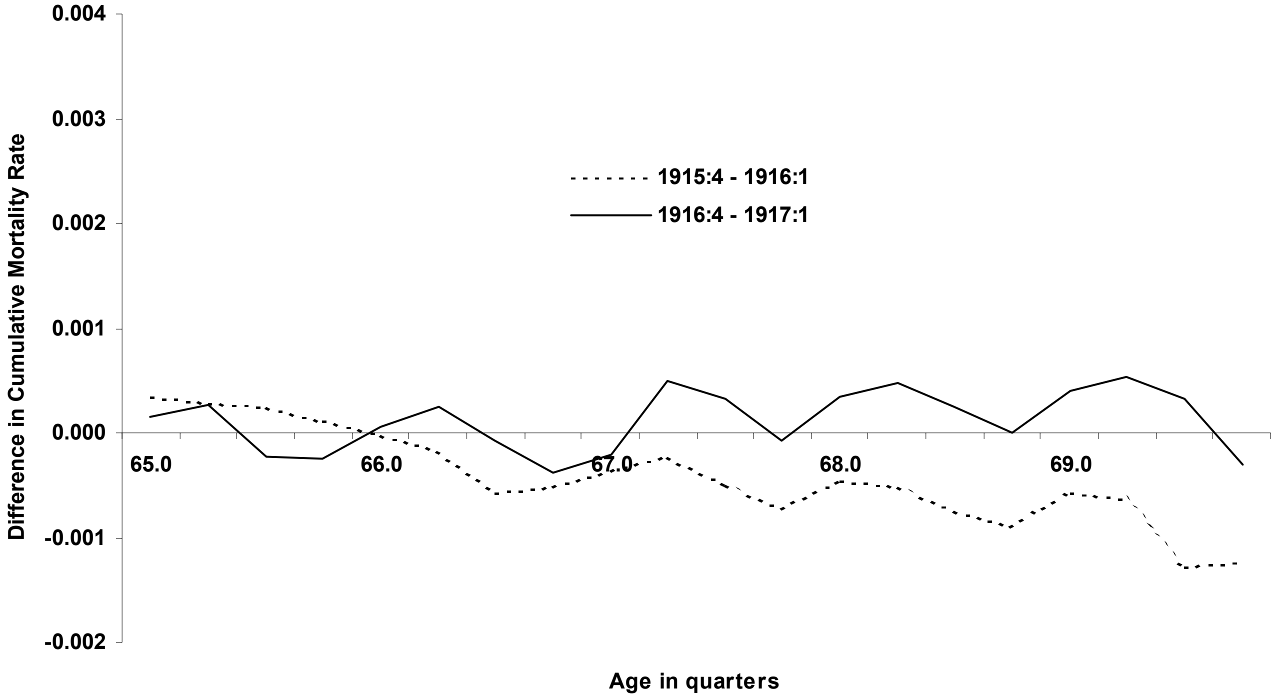

Figure 5: Difference in Log Mortality Rates, 1916:4 minus 1917:1

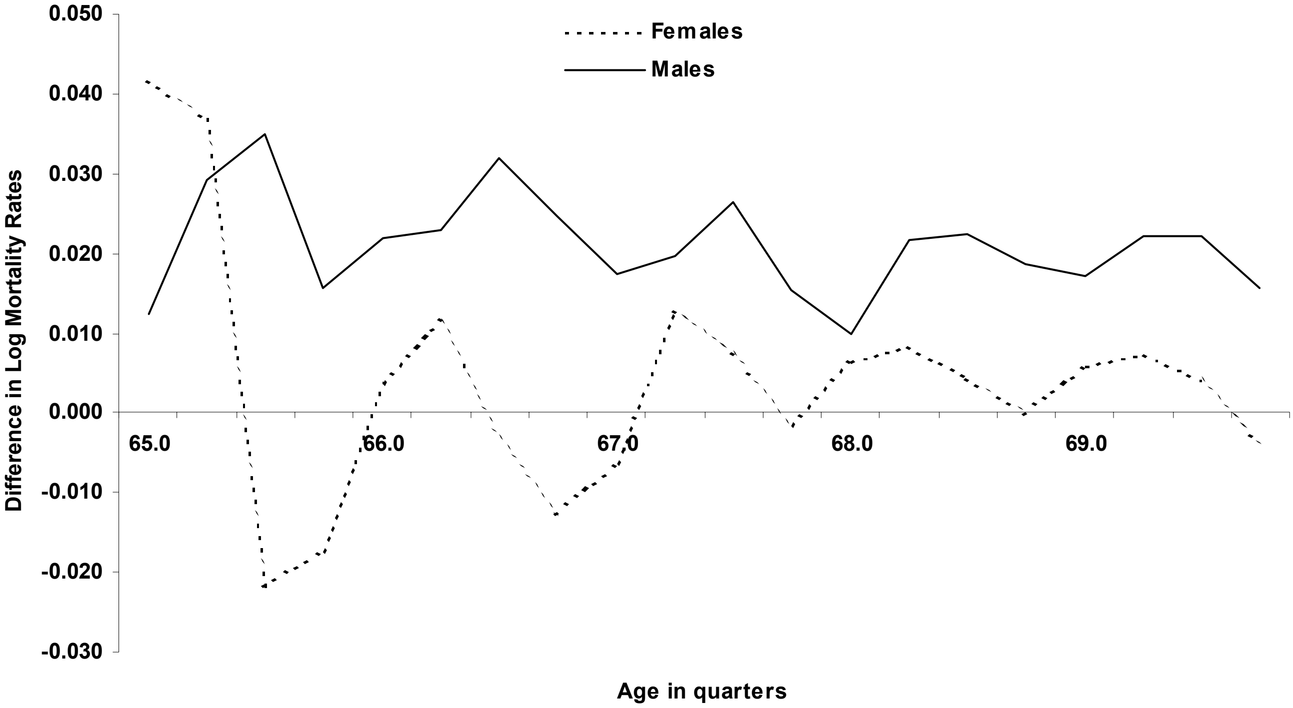

Table 1
Linear Probability Estimates, 5-Year Mortality Equations
1987-1989 NHIS/MCOD Data

	Age Group			
Variable	21-44	45-59	60+	65-66
Observations	53,606	20,748	19,967	2,253
Sample mean of Dep. Variable	0.010	0.042	0.197	0.130
	Model 1			
Income \$10,000 - \$19,999	-0.0049 (0.0018)	-0.0250 (0.0065)	-0.0395 (0.0084)	-0.0567 (0.0235)
Income \$20,000 - \$29,999	-0.0073 (0.0018)	-0.0456 (0.0065)	-0.0743 (0.0096)	-0.0615 (0.0259)
Income \$30,000 - \$39,000	-0.0092 (0.0018)	-0.0538 (0.0066)	-0.0842 (0.0114)	-0.0835 (0.0304)
Income \$40,000 +	-0.0116 (0.0018)	-0.0599 (0.0064)	-0.1016 (0.0108)	-0.1297 (0.0290)
	Model 2			
ln(Income)	-0.0037 (0.0005)	-0.0178 (0.0019)	-0.0402 (0.0039)	-0.0504 (0.0103)
Elasticity at sample mean	-0.370	-0.424	-0.204	-0.388

Other covariates include a complete set of single-year age effects, indicators for white and black respondents, an indicator for Hispanics, and seven education dummy variables.

Table 2
OASI Monthly Payments By Age of Retirement and Education Level

Payment for Men Born 1st Quarter 1917 and
(Difference between 1916:4 and 1917:1)

Age at retirement (year/months)	% that retires at age	Grammar school graduate (28%)	High school graduate (62%)	College graduate (11%)	All
62.0	19%	$471.60	$525.50	$543.50	$517.64
		($5.20)	($7.10)	($5.70)	($6.48)
62.6	11%	$491.30	$546.40	$566.10	$539.22
		($7.70)	($12.60)	($14.10)	($11.52)
63.6	12%	$530.60	$591.20	$611.40	$582.37
		($27.50)	($28.60)	($44.70)	($30.35)
64.6	28%	$569.90	$635.00	$656.70	$625.51
		($53.00)	($47.20)	($79.20)	($52.82)
65.0	30%	$589.50	$656.80	$679.30	$647.00
		($70.20)	($62.30)	($102.20)	($69.52)
Total					$596.79
					($41.49)

Table 3
Impact of Notch on Annual OASI Payments, in Real 1987 Dollars,
March CPS

	1916 cohort: earnings during ages 67-69 1917 cohort: earnings during ages 66-68		1915 cohort: earnings during ages 67-69 1916 cohort: earnings during ages 66-68
	Males	Females	Males
Mean of dependent variable	$5,879	$4,171	$5,950
"Notch" effect -- dummy variable that equals 1 for the oldest cohort (standard error)	495.74 (109.74)	8.47 (69.54)	8.00 (109.78)
Number of Obs.	3,059	3,786	3,189

All models include foxed effects for education, marital status, race and year of the survey. The Notch effect is for the "oldest" cohort

Table 4
Comparison of Means, 1st and 4th Quarter Births,
1970 PUMS 6% Sample

Mean for First Quarter [Difference in Mean for Fourth Quarter] (t Value of Difference)

Outcome of Interest	1914:4 vs 1915:1	1915:4 vs. 1916:1	1916:4 vs 1917:1	1917:4 vs. 1918:1	1918:4 vs 1919:1
Annual Labor Earnings	7386 [-21] (-.25)	7640 [-15] (-.18)	7700 [46] (.56)	7923 [185] (2.23)	7986 [57] (.69)
Years of Education	10.41 [.037] (.86)	10.55 [.088] (2.09)	10.65 [.068] (1.63)	10.76 [.115] (2.81)	10.77 [.147] (3.54)
< High school diploma	53.7% [-.15%] (-.25)	51.8% [-.91%] (-1.49)	50.8% [-.43%] (-.71)	49.2% [-1.69%] (-2.85)	48.9% [-1.11%] (-1.88)
Weeks worked, 1969	45.4 [.11] (.58)	45.8 [.04] (.21)	46.2 [-.11] (-.63)	46.1 [.53] (3.21)	46.3 [.41] (2.59)
Hours worked/week, 1969	35.23 [.04] (.16)	35.61 [.36] (1.68)	36.22 [-.03] (-.13)	36.27 [.40] (1.94)	36.28 [.428] (2.09)
Worked full time, 1969	73.3% [-.0012] (-.21)	74.6% [.0046] (.87)	75.5% [-.0022] (-.42)	75.7% [.0023] (.44)	75.6% [.0084] (1.65)
Self-employed, 1969	16.4% [.4%] (.83)	16.0% [.5%] (1.16)	15.6% [.4%] (.89)	15.4% [-.2%] (-.38)	15.3% [.1%] (.33)
Disabled (1=yes)	9.7% [.08%] (.21)	9.6% [-.16%] (-.44)	8.7% [.49%] (1.42)	8.3% [-.21%] (-.65)	8.5% [-.20%] (-.62)
Married (1=yes)	86.0% [.69%] (1.65)	86.8% [-.24] (-.58)	86.8% [.13%] (-.33)	87.0% [.04%] (.09)	86.6% [-.08%] (-.20)
Number of observations, both quarters	27,024	27,215	27,737	28,618	28,306

Table 5

Difference in Difference Estimates,
Impact of Notch on Five-Year Mortality Rates for Males

A: Males

	4th Quarter (1)	1st Quarter (2)	Difference (1) - (2)
(a) Notch (1916:4 and 1917:1)	0.1519	0.1494	0.0025
	(0.0013)	(0.0013)	(0.0018)
(b) Pre-Notch (1915:4 and 1916:1)	0.1524	0.1527	-0.0003
	(0.0013)	(0.0013)	(0.0018)
		Difference (a) - (b)	0.0028 (0.0026)

B: Females

	4th Quarter (1)	1st Quarter (2)	Difference (1) - (2)
(c) Notch (1916.4 and 1917.1)	.0820	.0823	-.0003
	(.0007)	(.0006)	(.0009)
(d)Pre-Notch (1915:4 and 1916:1)	.0827	.0839	-0.0012
	(.0007)	(.0007)	(.0009)
		Difference (c) - (d)	-0.0015 (0.0013)

Standard errors in parenthesis.

Table 6
Difference in Difference Estimates,
Impact of Notch on Five-Year Mortality Rates for Males

A: Males

	3rd and 4th Quarter (1)	1st and 2nd Quarter (2)	Difference (1) - (2)
(a) Notch (1916:3,4 and 1917:1,2)	0.1517 (0.0009)	0.1482 (0.0009)	0.0036 (0.0013)
(b) Pre-Notch (1915:3,4 and 1916:1,2)	0.1520 (0.0009)	0.1521 (0.0009)	-0.0001 (0.0013)
		Difference (a) - (b)	0.0037 (0.0017)

B: Females

	3rd and 4th Quarter (1)	1st and 2nd Quarter (2)	Difference (1) - (2)
(c) Notch (1916.3,4 and 1917.1,2)	0.0820 (0.0005)	0.0819 (0.0005)	0.0001 (.0007)
(d) Pre-Notch (1915:3,4 and 1916:1,2)	0.0820 (0.0004)	0.0835 (0.0005)	-0.0014 (0.0007)
		Difference (c) - (d)	0.0015 (0.0010)

Standard errors in parenthesis.

Table 7
Difference in Difference Estimates,
Impact of Notch on Mortality Rates
5 Year Mortality from Age 65, Women as Control Group

	Before Notch	After Notch	
	Born in 4th Q. (1)	Born in 1st Q. (2)	Log Difference log(1) - log(2)
(a) Men born 1916.4 or 1917.1	0.1519 (0.0013)	0.1494 (0.0013)	0.0164 (0.0122)
(b)Women born 1916.4 or 1917.1	0.0820 (0.0006)	0.0823 (0.0006)	-0.0036 (0.0110)
		Difference (a) - (b)	0.0200 (0.0164)

Standard errors in parenthesis.

Mortality Rates are computed from the quarter each quarter-of-birth cohort turns 65 and proceeding forward for 20 quarters. The cohort born in 1916.4 turn 65 in 1981.4. Their mortality rate is based upon the population as of January 1, 1982 and the count of deaths between January 1, 1982 and December 31, 1986. The 1917.1 cohort's mortality rate is based upon the population on April 1, 1982 and mortality between April 1, 1982 and March 31, 1987.

Table 8
Difference in Difference Estimates,
Impact of Notch on Mortality Rates
5 Year Mortality from Age 65, Women as Control Group

	Before Notch	After Notch	
	Born in Q. 3,4 (1)	Born in Q. 1,2 (2)	Log Difference log(1) - log(2)
(a) Men born 1916.3,4 or 1917.1,2	0.1517 (0.0009)	0.1482 (0.0009)	0.0239 (0.0086)
(b) Women born 1916.3,4 or 1917.1,2	0.0820 (0.0005)	0.0819 (0.0005)	0.0012 (0.0078)
		Difference (a) - (b)	0.0227 (0.0116)

Standard errors in parenthesis. Rates calculated as in Table 6, referenced from older quarter-of-birth (16.3 and 17.1) for each column

Table 9
OLS Estimates, the Impact of the Notch on Post-65 Labor Supply
Men Born 1913-1920, Aged 62-70,
March CPS

Independent Variables	Worked last year	Worked last year	Hours last year	Hours last year
Notch effects				
1917-20 Cohorts, aged 65+	0.0286 (0.0099)		69.50 (19.66)	
1917-20 Cohorts, aged 65-67		0.0072 (0.0109)		35.42 (21.62)
1917-20 Cohorts, aged 68-70		0.0518 (0.0113)		109.13 (22.53)
R^2	0.120	0.120	0.163	0.163
Mean of dependent variable	0.460	0.460	738	738
Mean of dependent variable, age 68-70				

The models control for education, race, marital status, year of birth and age.

Table 10
Linear Probability Estimates, 5-Year Mortality Equations,
65-70 Year Olds
1987-1989 NHIS/MCOD Data

	Coefficient (standard error) on Currently working	
Model	Age 65-70 (5,393 observations, 14.8 % died)	Age 68-70 (2,899 observations, 17.2% died)
1. Control for age, education, marital status, ethnicity and year of survey	-0.059 (0.011)	-0.045 (0.017)
2. Add income effects	-0.049 (0.011)	-0.036 (0.011)
3. Add self reported health status	-0.023 (0.011)	-0.011 (0.017)
4. Add bed days	-0.020 (0.010)	-0.007 (0.017)