Income and the Use of Prescription Drugs by the Elderly: Evidence from the Notch Cohorts*

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Abstract

We use exogenous variation in Social Security payments created by the Social Security benefits notch to estimate how retirees' use of prescription medications responds to changes in their incomes. In contrast to estimates obtained using ordinary least squares, instrumental variables estimates based on the notch suggest that lower-income retirees exhibit considerable income sensitivity in their use of prescription drugs. Our estimates are potentially useful for thinking about the health implications of changes in transfer payments to the elderly and for evaluating the benefits of the recently enacted Medicare prescription drug benefit.

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"Most experts believe that the best baseline for planning purposes is to assume that the demographic shift associated with the retirement of the baby-boom generation will be permanent – that is, it will not reverse when that cohort passes away. Indeed, so long as longevity continues to increase – and assuming no significant changes in immigration or fertility rates – the proportion of elderly in the population will only rise. If this fundamental change in the age distribution materializes, we will eventually have no choice but to make significant structural adjustments in the major retirement programs."

(Testimony of Chairman Alan Greenspan, *Economic Outlook and Current Fiscal Issues*, Before the Committee on the Budget, U.S. House of Representatives, February 25, 2004. http://www.federalreserve.gov/boarddocs/testimony/2004/20040225/default.htm)

1. Introduction

Social Security represents one of the most important sources of income for elderly Americans, and for many it is their primary source of income. Though popular with both policymakers and voters, a consensus has emerged that the impending retirement of the "baby-boom" cohorts will pose a significant challenge to the actuarial integrity of the Social Security system. As the quote at the top of the page suggests, this recognition has led to many reform proposals, a number of which advocate benefit reductions as a means of repairing the imbalance between revenues and benefit payments that will exist if payroll taxes are maintained at current levels.[1]

A key concern for health policymakers is that reductions in Social Security benefits, or in other sources of retiree income, could exacerbate the difficulties that seniors face in obtaining needed prescription medications, or reduce their adherence to costly drug regimens. These concerns will be addressed, in part, by the recently enacted Medicare Prescription Drug Improvement and Modernization Act of 2003 (scheduled to take effect in 2006), but income will still be an important factor in determining drug utilization for many Americans due to the

[1] To take one example, in 1996 a commission headed by Michael Boskin concluded that cost-of-living adjustments based on the CPI over-compensated Social Security recipients and recommended that such adjustments be reduced (Snyder and Evans, 2002). Snyder and Evans note that if this recommendation had been implemented, Social Security benefits would have fallen by roughly five percent over a five-year period.

incomplete nature of the coverage provided by the Medicare drug benefit. For example, most retirees will face a $250 annual deductible, a 25% coinsurance rate on their first $2250 of drug expenses, and no coverage for prescription expenses between $2250 and $5100 (corresponding to $3600 in out-of-pocket spending). Premiums are expected to total about $420 per year, with additional assistance being available for people with "low incomes and limited assets."[2]

Moreover, there is considerable uncertainty regarding the extent to which the Act's employer subsidies will prevent the new drug benefit from crowding out coverage provided through employer-sponsored retiree health insurance plans. If significant crowd-out were to occur, many retirees may not experience a meaningful increase in the generosity of their drug coverage. And, given that Medicare is on an even weaker fiscal footing than Social Security,[3] it is possible that the drug benefit will eventually be scaled back, or that Medicare premiums will be increased. Thus, understanding the extent to which the drug benefit shields elderly

[2] The income and asset limits for waiving the premium and cost-sharing provisions of the drug benefit are scheduled to be announced in 2005 (see http://www.medicare.gov/Publications/Pubs/pdf/11054.pdf). Some groups (http://www.medicareadvocacy.org/PrescDrugs_LowIncomeProvisionsof2003Act.htm) have projected that the income thresholds for partial and full waivers are likely to be set at 135 percent and 150 percent of the federal poverty line (FPL). In 2004, the federal poverty guidelines for one- and two-person households were $9,310 and $12,490, respectively, yielding 150% FPL figures of $13,965 and $18,735 (http://aspe.hhs.gov/poverty/04poverty.shtml). To get a rough feel for how many households in our low-education (lower-income) sample would likely be subject to full cost-sharing under the new law, we calculated total household income separately for single (55.7 percent) and married households (44.3 percent) in our estimation sample, and inflated the dollar amounts to 2004 dollars using the CPI inflation calculator from the BLS website (http://www.bls.gov/cpi/home.htm). Approximately 52 percent of single households and 67 percent of married households in our low-education sample have incomes that place them above the 150% FPL thresholds, suggesting that approximately 59 percent of the low-education (lower-income) households in our sample would be exposed to the cost-sharing provisions cited above, with a larger percentage facing partial cost sharing. In contrast to the federal poverty guidelines used by the department of Health and Human Services (HHS), the Census Bureau specifies lower FPL thresholds for those aged 65 and over, but has not updated their figures for 2004. If the Centers for Medicaid and Medicare Services (CMS) utilizes the Census Bureau thresholds, the percentage of households exposed to cost sharing will be slightly higher.

[3] See the *2004 Annual Report of the Board of Trustees of the Federal Old-Age and Survivors Insurance and Disability Insurance Trust Funds*, available online at: http://www.ssa.gov/OACT/TR/TR04/index.html. A concise summary of the full report is available at: http://www.ssa.gov/OACT/TRSUM/trsummary.html.

households from income-induced changes in medication use will be important for assessing the desirability of this costly addition to the Medicare program.

Despite its significance for a variety of public policy issues, little is known about how income influences prescription drug use among the elderly. Most research has instead focused on the impact of cost-sharing provisions in supplemental insurance plans (see, for example, Federman et al., 2001). One exception is a study by Stuart and Grana (1998) who surveyed 4000 Medicare beneficiaries in Pennsylvania. They found that Medicare beneficiaries with annual incomes above $18,000 were 18 percent more likely than those with incomes below $6000 to use prescription drugs to treat their health conditions.

A fundamental challenge faced by such studies is isolating the underlying causal relationship between income and prescription drug use from a large number of inherently unobservable characteristics that likely influence both an individual's lifetime earnings (and therefore the amount of income available after retirement), and their propensity to use prescription medications. For instance, individuals differ in the extent to which they discount the future, with those placing a higher value on current consumption being less likely to invest in human capital, be it in the form of education, on-the-job training, or other, less formal, means of acquiring skills. Given their higher discount rates, such individuals may also be less willing to forgo current consumption to purchase prescription drugs, whose benefits often accrue in later years.[4] Other unobservable traits, such as ability or risk aversion, may also jointly contribute to lifetime earnings and prescription drug use, further confounding attempts to estimate the causal impact of income on drug utilization.

[4] This hypothesis has been advanced by Grossman (1972) and Fuchs (1982) and has received support from a large literature that documents higher mortality rates and worse health habits among those with low educational attainment (Kenkel, 1991; Evans, Ringel and Stech, 1999).

In this paper, we seek to circumvent these endogeneity problems by relying on a natural experiment that generated large, exogenous differences in Social Security payments for otherwise identical individuals based on their year of birth. The so-called Social Security benefits "notch," which we detail in the next section, came about as a result of an error in the manner by which payments to beneficiaries were indexed for inflation, coupled with a subsequent modification of the benefits formula to correct the initial error. The large differences in benefits occasioned by these changes in the Social Security law have been used by others to estimate the effect of income on retirement behavior (Krueger and Pischke, 1992), mortality (Snyder and Evans, 2002), elderly living arrangements (Engelhardt, Gruber and Perry, 2004) and elderly poverty rates (Engelhardt and Gruber, 2004).

One practical difficulty with using the benefits notch to study prescription drug use is that most of the variation in Social Security income induced by the notch applies to birth cohorts centered around 1916, making it difficult to locate data sources with information on prescription drug use that also contain a large enough sample of "notch babies" to yield reasonably precise estimates. The only data source (of which we are aware) that meets these and other desirable criteria is the Study of Assets and Health Dynamics Among the Oldest Old (AHEAD), a companion survey to the better known Health and Retirement Study.[5] The AHEAD is a longitudinal survey of individuals who were aged 70 or older in 1993 (i.e., those born in 1923 or earlier) and their spouses, making it the only recent dataset that has substantial numbers of retirees from the notch and adjacent cohorts which also has information on prescription drug use and Social Security payments.

[5] The Consumer Expenditure Survey potentially satisfies these conditions, but suffers from other drawbacks which render it substantially less useful than the AHEAD for our purposes. We discuss the advantages of using the AHEAD in Section 3.

Using data from the 1993 wave of the AHEAD, we find small and statistically insignificant effects of income on prescription drug use when Social Security payments are treated as exogenous. However, when the benefits notch is used as an instrument for Social Security income,[6] a larger and statistically significant effect of income on drug utilization emerges for households headed by beneficiaries with less than a high school education (approximately 42 percent of our sample). We were unable to obtain estimates for more-educated retirees because the notch instrument does not yield a sufficiently strong first-stage relationship to permit a valid inference for these households.[7] Nonetheless, it is plausible that the large income effects we find for less-educated households may apply more generally.

Our estimates indicate that a $1000 increase in post-retirement income (in 1993 dollars) for those in our low-education (lower-income) group would increase the number of prescription medications used in a typical month by approximately 0.55 prescriptions per household. Evaluated at the mean levels of drug utilization and Social Security income in the low-education sample, this translates into an elasticity of roughly 1.32, suggesting lower-income retirees exhibit

[6] We use Social Security income in our analysis, rather than total income, for two reasons. First, a substantial number of respondents in the AHEAD were unable to accurately report their total income. This was much less of a problem for Social Security income, which respondents were able to report on a monthly basis (i.e., the amount they receive in their monthly check). Second, the notch may have led to other behavioral changes that affected total income, e.g. a change in post-retirement work (Snyder and Evans, 2002), that would appear to offset the permanent increase in wealth created by the notch if total income were used in our regressions.

[7] Because the 1977 Amendments to the Social Security law also raised the covered earnings maximum, higher-income workers in later birth cohorts saw an increase in the percentage of their earnings that were counted toward their Social Security benefit relative to higher-income workers in earlier cohorts, a phenomenon that partially offset the across-cohort differences in benefit levels created by the notch for these workers. This was less of an issue for lower-income workers because their earnings were generally below the cap in these cohorts. As a result, the increase in Social Security benefits associated with the notch was significantly larger for lower-income workers than for higher-income workers, yielding a better first-stage relationship for the former than for the latter. Following Engelhardt, Gruber and Perry (2004), we split the sample based on educational attainment, rather than income, because income is directly affected by the notch. Using a similar split based on education, they did not find a statistically significant relationship between notch-induced changes in Social Security income and elderly living arrangements for more-educated retirees but, like us, did find a significant effect for retirees with less education.

considerable income sensitivity in their use of prescription drugs.[8] Thus, to the extent that future retirees can be expected to behave like earlier cohorts, one would anticipate that reductions in Social Security benefits (or other policy changes that affect retirement incomes, such as increases in Medicare premiums) would significantly reduce prescription drug use among lower-income beneficiaries, and possibly among those with higher incomes as well.

The paper proceeds as follows. In Section 2, we provide a brief overview of the Social Security benefits notch and how it has been used to identify income effects in other settings. In Section 3, we discuss our data and empirical strategy. In section 4, we present our main results along with a series of specification checks. Concluding remarks are offered in Section 5.

2. The Benefits Notch

Detailed accounts of the legislation that led to the benefits notch, as well as the specifics of how Old Age and Survivor's Insurance (OASI) benefits are calculated, can be found in all of the existing papers that have made use of the notch (Krueger and Pischke, 1992; Snyder and Evans, 2002; Engelhardt, Gruber and Perry, 2004). Here, we provide only a brief overview, referring the reader to the papers listed above for a more in depth discussion.[9]

Prior to 1972, most changes in Social Security benefits enacted by the Congress were made on an ad hoc basis. For example, increases in benefits exceeding the rate of inflation were passed by Congress in 1967, 1969, and 1971. In 1972, benefit payments were, for the first time, explicitly indexed for inflation; however, the benefit formula continued to be based on nominal, as opposed to real, wages for those who had not yet retired. The high rates of inflation that

[8] When (potentially endogenous) controls for health status are included, the coefficient estimates and associated elasticities are somewhat smaller; 0.36 and 0.87, respectively.

[9] Our summary borrows heavily from these articles.

occurred shortly thereafter resulted in large increases in benefits for individuals born in 1910 or later, creating a potential threat to the solvency of the system. In 1977, in response to a rapid escalation in benefit costs brought on by this error, Congress amended the Social Security Act to correct the mistake and, in the process, created a sizeable reduction in benefits for those born after 1916 relative to earlier cohorts. As others have noted, the differences in benefits engendered by these changes in the Social Security law were both large and unanticipated, making them well suited for the analysis of income/wealth effects among the elderly.[10] Other changes to the law, including increases in the covered earnings maximum, led to a resumption of the upward trend in real benefits for those born after 1921, creating both a "notch" in benefits (for those born between 1917 and 1921), and a "hump" (which varied based on income and retirement age) for those born in various sub-periods between 1910 and 1921, relative to adjacent birth cohorts.

As discussed earlier, the upward trend in benefits was substantially more pronounced for those with higher incomes because an increase in the covered earnings maximum resulted in a differential increase in the fraction of earnings subject to Social Security taxes for high-income workers relative to low-income workers. Thus, when looking at real benefit levels across birth cohorts, the size of the benefits "hump" created by double indexation is much larger for lower-income retirees than for higher-income retirees due to the offsetting increase in benefits for the high-income group arising from the higher earnings cap. Because our instrumental variables approach compares retirees from the birth cohorts who received unusually high benefits (due to

[10] Given their ad hoc character, the legislated changes in benefits prior to 1972 were largely unpredictable, as must have been the error which led to double indexation. One might argue that the ensuing reduction in benefits created by the 1977 Amendments could have been anticipated, but this does not seem to have been the case. As Snyder and Evans (2002) note, it was actually the interaction between over-indexation and the accelerating inflation of the 1970s that made the initial mistake so costly, and which led Congress to abruptly modify the law in 1977 - a change which most accounts suggest went unnoticed until after the affected cohorts began to retire (Snyder and Evans, pp. 19-20).

double indexation) to retirees with more typical benefits from adjacent cohorts, any flattening of the associated benefits hump weakens our first-stage regressions. This attenuation in the benefits differential across our "treatment" and "control" groups precludes us from obtaining reliable estimates for better-educated (higher-income) households, but yields a strong first-stage relationship, and precise parameter estimates, when the roughly 42 percent of households with less than a high-school education are considered.

It is worth noting that many studies that rely on changes in the Social Security law to identify income effects actually utilize the "hump," rather than the "notch," although the latter phrase has become so standard that modifying the terminology would likely create confusion.[11] For example, Engelhardt, Gruber and Perry (2004) compare the living arrangements of those who experienced relatively high Social Security incomes (i.e., those born during the "hump" period of 1910 and 1921) with surrounding birth cohorts from 1900 to 1933 whose real Social Security benefits were lower.[12] They find that the propensity of elderly individuals to live in shared living arrangements is inversely related to income, and that this effect is concentrated among lower-income retirees, which they proxy for using differences in education.

In the first paper to use the benefits notch as a source of exogenous variation in retirement incomes, Krueger and Pischke (1992) exploit the fact that the 1977 Amendments created a pronounced break in an otherwise upward trend in the real value of benefits to test whether the trend toward earlier retirement was causally linked with rising Social Security benefits. The absence of a reversal in this trend for the notch cohorts, who experienced a large,

[11] A somewhat related point was made by Snyder and Evans (2002), who observed that in thinking about the variation created by the notch, it is more natural to view those who received abnormally high benefits due to double indexation as being the "treatment" group, with those whose benefits were reduced back to a level in line with longer-run trends as being the "control" group.

[12] Engelhardt and Gruber (2004) utilize similar treatment and control groups in studying the impact of Social Security on elderly poverty rates.

permanent reduction in their Social Security wealth relative to other retirees born between 1901 and 1930, suggests that rising Social Security benefits most likely did not play a causal role in hastening retirement. By contrast, Snyder and Evans (2002) focus more narrowly on the "notch" years by comparing mortality rates among individuals who differed only by a quarter of birth; specifically, those born in the fourth quarter of 1916 (the approximate peak of the notch) relative to those born in the first quarter of 1917. They find that, in contrast to the conventional wisdom on the subject, an increase in income during retirement leads to higher mortality rates, possibly due to greater activity and reduced social isolation on the part of those with reduced incomes.

Our approach is closest to that of Engelhardt, Gruber and Perry (2004) in that we will be comparing birth cohorts whose Social Security payments were elevated by double indexation to adjacent cohorts whose benefits were lower in real terms. In the next section, we describe our data and identification strategy in more detail.

3. Data and Empirical Strategy

3.1. *General Issues*

As mentioned in the Introduction, there are few datasets with information on prescription drug utilization that also capture a significant number of individuals from the notch and adjacent birth cohorts. Aside from the AHEAD data, the only other dataset that we were able to identify that meets these criteria is the Consumer Expenditure Survey (CEX). There were, however, two considerations which argued in favor of using the AHEAD. First, unlike the AHEAD, the CEX is based on a random sample of the entire population, so one would have to rely heavily on data from the late 1970s and early 1980s to capture a significant number of individuals born during the notch and surrounding years. Second, the CEX provides data on drug *expenditures* (not quantities), which are often filtered through supplemental health insurance policies of varying generosity in the case of the elderly. Thus, if two households were to experience the same exogenous increase in Social Security income, one might opt to purchase additional prescriptions directly, while the other might do so indirectly by purchasing a more generous Medigap policy, or by upgrading to a policy that contains drug coverage from one that does not. Though both households may be equally responsive to changes in their income, one would appear to have a more elastic response if out-of-pocket spending were used as the utilization measure. In contrast, the AHEAD asks respondents about the number of prescription medications taken during a typical month, thereby allowing us to estimate the total effect of an increase in Social Security income on prescription drug utilization, taking both of the aforementioned channels into account.[13]

3.2. *Data*

The AHEAD is a longitudinal survey of individuals aged 70 or older in 1993 and their spouses (regardless of age). The first wave of the AHEAD, fielded in 1993, provides extensive information on 8,222 individuals in 6,047 households on items such as their demographic characteristics, income, wealth, employment status, health status, insurance holdings, and utilization of medical care. Some questions were asked of only one household member, but were designed to apply to the household as a whole, while others were asked of each household member separately. African Americans, Hispanics, and residents of the state of Florida were over-sampled, with sample weights available to adjust for this and other non-representative elements of the sample design. A response rate of just over 80 percent was obtained in the first

[13] Because we only have a single source of exogenous variation in Social Security income to work with, we are unable to examine the relative contributions of supplemental health insurance and directly-purchased prescriptions to overall consumption.

wave of the survey. One follow-up survey was conducted in 1995 and the surviving AHEAD respondents were integrated into the HRS in 1998.

Although the AHEAD is structured longitudinally, we were unable to take advantage of this feature in our work because respondents were only asked about the number of prescription drugs they use in the first wave of the survey. The drug questions that were repeated in later waves either pertain to out-of-pocket spending, which is difficult to interpret for the reasons discussed earlier, or are discrete outcomes that didn't exhibit sufficient variation to generate estimates with any reasonable degree of precision; e.g., whether the respondent took any prescription medicines versus none. The restriction to a single cross-section limits, to some extent, our ability to control for age and cohort effects. After presenting our initial estimates, we will also present results from a series of specification checks which examine the sensitivity of our baseline estimates to these potential confounders.

3.3. *Estimation Sample*

The unit of observation for our analysis is the household because it seems likely that married or cohabitating individuals pool resources and make joint decisions when it comes to purchasing items like prescription drugs. Approximately 46 percent of our sample is made up of two-person households (in the vast majority of cases, a husband and a wife), with 54 percent of households having only one member.

Our first task is to designate a primary Social Security beneficiary for each household. We do so for three reasons: (1) to select the set of birth cohorts included in the sample; (2) to determine which households were likely to have benefited the most from double indexation (an indicator of which serves as our instrument); and (3) to split the sample based on the educational attainment of the primary beneficiary, which turns out to be important for our estimation strategy.

In so doing, we follow previous work in designating the male member of two-person households as the primary Social Security beneficiary, omitting a small number of cases in which the female is the only household member drawing benefits.[14] Thus, for all households containing a male member, we use the male's year of birth to assign the household to either the treatment or control group. Households with no male members can be divided into two categories: never-married females and widowed/divorced females. In the case of never-married females, we designate the female as the primary beneficiary and use her year of birth to determine treatment-control status for the household. In the case of widowed/divorced females, we designate the deceased or former husband as the primary beneficiary and follow Engelhardt, Gruber and Perry (2004) in subtracting three years from the female's year of birth to generate a birth year for the deceased/former husband, which is then used to assign the household to either the treatment or control group.[15]

In creating our estimation sample, we restrict attention to the birth cohorts studied by Krueger and Pischke (1992); namely, households whose assigned Social Security beneficiary was born between 1901 and 1930. We also drop a small number of households that report no Social Security income, or that report Social Security income of less than $100 per month.[16]

[14] The vast majority of married women in these birth cohorts qualified for benefits based on their husband's earnings history. See Snyder and Evans (2002, p.18) for a discussion and additional references.

[15] Three years is the median difference in spousal ages for widowed/divorced elderly as calculated by Engelhardt, Gruber and Perry (2004) using the 1982 New Beneficiary Survey published by the Social Security Administration. As we discuss later, our estimates are largely unchanged if these households are excluded from the analysis.

[16] Results are very similar if households receiving less than $200 per month in Social Security benefits are dropped, or if no households are dropped.

These restrictions, coupled with observations lost from missing or incomplete data, yield a final sample of 4,228 households.

3.4. *Empirical Specification*

To estimate the impact of Social Security income on prescription drug use, we estimate equations of the form shown below.

$$D_h = \alpha + \beta SSIncome_h + \delta \mathbf{X}_h + \varepsilon_h \tag{1}$$

where h indexes households. D_h is the typical number of prescription medications that each household uses in a month, $SSIncome_h$ is annual household Social Security income (measured, for ease of interpretation, in thousands of 1993 dollars), $\mathbf{X}_h$ is a vector of control variables and ε_h is the error term. The coefficient on household Social Security income, β, measures how a one thousand dollar increase in annual Social Security income affects the number of prescription drugs each household uses in a typical month. In our baseline specification $\mathbf{X}_h$ includes a standard set of demographic variables for each household: indicator variables for the type of household (male head - married or cohabitating; male head - single; female head - never-married; female head - widowed; and female head - divorced), age of the head, race of the head (white, African American, or other), Hispanic ethnicity of the head, whether the household is located in an MSA, and location (indicators for each of the nine Census regions).

It bears mentioning that all of the demographic variables described above are based on the characteristics of the head of the household, and not the designated Social Security Beneficiary, because the latter is unlikely to be contributing to the household's use of prescription drugs in cases where the household is headed by a divorced or widowed female. As a rough specification check, we will also explore the sensitivity of our results to the inclusion of some additional control variables, such as health status and insurance coverage, that are likely jointly determined with drug utilization.

Equation (1) will be estimated twice for each model specification; first by ordinary least squares and second using an instrumental variables estimator that accounts for the likely endogeneity of income. The first-stage regression is displayed in equation (2) below.

$$SSIncome_h = \gamma + \theta Notch_h + \phi \mathbf{X}_h + u_h \quad (2)$$

Our instrument, labeled $Notch_h$, is an indicator variable that takes the value "one" for households whose primary Social Security beneficiary was born during the notch years[17] of 1915-1917, and "zero" for households whose primary beneficiary was born in any other year between 1901 and 1930. The years 1915-1917 were selected because they represent the peak of the benefits notch (or hump) and provide the strongest first-stage relationship between Social Security income and notch status for the cohorts in our sample.

Table 1 provides descriptive information on the main variables used in our analysis, separated into two sub-samples based on the educational attainment of the primary Social Security beneficiary: 1,755 households whose primary beneficiary had less than a high school education and 2,473 households whose primary beneficiary had a high school diploma or better.[18]

As mentioned previously, respondents were only asked about the number of prescription drugs they use in the first wave of the survey. This is potentially problematic because the

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[17] To maintain consistency with previous work, we will use the term "notch" to refer to those households that benefited from double indexation, although the variation that we utilize is more consistent with the benefits "hump" described in Section 2. In particular, our IV approach compares households at the top of the hump (those who benefited the most from double indexation) to those in surrounding cohorts whose real benefits were lower, ceteris paribus.

[18] Although the AHEAD does not provide information on the birth year of deceased or former husbands, it does provide data on their level of education. We use this variable to form the sample split depicted in Table 1.

benefits notch is based on year of birth, which is perfectly correlated with age in a single cross section of individuals. Previous studies have addressed this problem either by relying on extremely large datasets to compare individuals that differ only slightly in age (Snyder and Evans, 2002), or by exploiting age by year-of-birth variation (Krueger and Pischke, 1992; Engelhardt, Gruber and Perry, 2004; Engelhardt and Gruber, 2004).

Because we have a relatively small number of observations, and do not have age by year-of-birth variation in the data, we attempt to address this issue by performing two checks. First, for every model considered, we show that our results are not sensitive to the inclusion of a progressively more flexible specification of the age variable; in particular, we enter age as a polynomial function whose order ranges from one (linear) to three (cubic).[19] Second, we show that our coefficient estimates are not greatly affected by including detailed measures of health status, subject to the caveat that such measures are likely to be jointly determined with prescription use. Finally, it is worth noting that the average age among those receiving higher Social Security benefits due to the notch (our treatment group) is lower than the average age of those in adjacent cohorts (our control group). Thus, if younger households are healthier on average, any residual effects of age not captured by our age variables, or our health status controls, should impart a downward bias to the estimated relationship between Social Security income and prescription drug use.

A related concern is that there may be pure "cohort effects" in drug utilization that potentially confound the treatment-control comparison groups we have formed. Two such possibilities are the long-term repercussions of *in utero* exposure to the influenza virus in late

[19] Results were also generally unchanged when age was entered as a quartic. Unfortunately, allowing this level of flexibility in the age variable induced a near singularity in the **X** matrix due to the high degree of collinearity among the age terms. This led the coefficient estimates to become unstable in some cases.

1918 and early 1919 (Almond, 2003), as well as the more general worry that using cohorts that are sufficiently far apart in time could render the estimates susceptible to biases from any unobserved differences between cohorts. In the next section, we provide some suggestive evidence that these types of cohort effects are not driving our results. First, we show that removing the "flu babies" from our sample, the most obvious source of a systematic difference in drug utilization across birth cohorts, has virtually no impact on our estimates. And second, we show that restricting attention to a narrower, and presumably more comparable, set of birth cohorts (those born between 1910 and 1920) also does not greatly affect our results.

3.5. *Effect of the Notch by Education Group*

As was discussed in detail earlier, the impact of the benefits notch varies substantially based on income/education. This difference is evident in Table 2, where separate estimates of equation (2) are presented based on the educational attainment of the primary Social Security beneficiary. For the low-education (lower-income) group, having been born at the peak of the notch years (1915-1917) increases household Social Security income by between $1,000 and $1,400 per year in 1993 dollars. Relative to a mean household Social Security income of $9,625 for this group, this translates into an increase of between 10 and 14.5 percent. These regressions provide a strong first-stage for our IV estimation strategy, with partial F-statistics for the notch indicator that are uniformly above the threshold cited by Staiger and Stock (1997) for establishing bias due to weak instruments.

By contrast, the relationship between notch status and Social Security income is much weaker for households whose primary beneficiary has a high school diploma or better. For these households, having been born during the notch years only increases household Social Security income by between $150 and $340 per year, corresponding to a percentage increase of between

1.4 and 3.2 percent when evaluated at the mean household Social Security income for this group ($10,782). Moreover, the partial F-statistics from these regressions are all below 1.00, suggesting that there is not a strong enough first-stage relationship present to support an IV estimation strategy for this group.[20]

As a result, we focus on the 1,755 households from the low-education sub-sample (approximately 42 percent of the full sample) to obtain estimates of the effect of income on prescription drug utilization. It should be emphasized that what we refer to as the "low-education" group, those with less than a high school diploma, is far more representative of the population as a whole than would be the case today. Indeed, Goldin (1999), who analyzed trends in education in the early part of the 20th century, found that the rate of high school completion (relative to the population of 17 year olds in that year) was just 13 percent in 1915 and was only 41 percent as late as 1935.

4. Results

4.1. *Baseline Estimates*

Our main estimates of the effect of income on prescription drug utilization are presented in Table 3. Three model specifications are estimated to examine the sensitivity of our findings to alternative methods of controlling for the effect of age on prescription use. All specifications include the full set of demographic variables described in the previous section and all are weighted using the AHEAD household weights. Because our instrument varies only by year of birth, we adjust our (robust) standard errors for clustering within each birth year (1901-1930).

[20] Not surprisingly, the standard errors on the second-stage estimates for the high-education group are too large to permit any reasonable inference concerning the magnitude of the income effect for this group.

Looking across the three age specifications, we find small and statistically insignificant effects of income on the number of prescriptions used by each household when these effects are estimated by ordinary least squares. By contrast, when the benefits notch is used as an instrument for household Social Security income, we find large and statistically significant effects of income on drug use across all three model specifications, with the magnitude of the income effect rising a bit as tighter controls for age are incorporated.[21] Focusing on the quadratic age specification, our coefficient estimate indicates that a $1000 increase in Social Security income (in 1993 dollars) would increase the number of prescription medications used in a typical month by approximately 0.55 per household. Evaluated at the mean levels of drug utilization and Social Security income in the sample (shown in Table 1),[22] this translates into an elasticity of approximately 1.32, implying a high degree of income sensitivity on the part of lower-income retirees.

4.2. *Additional Control Variables*

In Table 4, we examine how the addition of control variables for health status affects our estimates. Because the prevalence of most medical conditions increases with age, this provides an additional check for age confounding. However, given that many of these variables are likely to be jointly determined with prescription drug use, we recognize that the resulting estimates must be interpreted with caution. We offer them here only as a rough sensitivity check on our baseline estimates.

The health status controls that we employ are a set of 13 indicator variables that take the value "one" if either member of the household reports being in fair or poor health (55 percent of

[21] This pattern is also evident in many of the specification checks presented later in the paper.

[22] Note that the Social Security income figure reported in Table 1 must be converted into thousands of dollars before performing the elasticity calculation.

households), as opposed to rating their health as good, very good, or excellent; being in frequent pain (50 percent of households); or having had any of the following medical conditions: high blood pressure (69 percent of households); diabetes, currently (22 percent of households); cancer, excluding minor skin cancers (18 percent of households); lung disease, excluding asthma (19 percent of households); a heart condition (46 percent of households); stroke (13 percent of households); psychiatric problems (19 percent of households); arthritis, during the past 12 months (39 percent of households); hip fracture (6 percent of households); incontinence, during the past 12 months (29 percent of households); cataract surgery (34 percent of households); or some "other" health condition (42 percent of households).

The IV estimates reported in Table 4 are somewhat smaller than the corresponding estimates from Table 3. Focusing again on the quadratic age specification, the associated income elasticity would be 0.87, which is considerably smaller than the elasticity obtained in the absence of health controls, but still quite large in economic terms. All of the IV coefficient estimates are statistically significant at the 10 percent level or better. The OLS estimates are a bit larger than those from Table 3, but remain relatively small (in comparison to the IV estimates) and do not rise to the level of statistical significance.

Turning next to Table 5, we perform a similar robustness check using three indicator variables for the presence of health insurance: specifically, indicators that take the value "one" if either member of the household reports having coverage from Medicaid (14 percent of households); from another government insurance program - Railroad retirement, CHAMPUS, CHAMPVA, or other military programs (3 percent of households); or from private health insurance (74 percent of households). The last category is composed of those with supplementary private insurance plans (Medigap or other supplements).[23]

Given the likely endogeneity of these variables, a similar caveat as was given in the case of the health status controls applies here. That said, the IV estimates in the presence of the health insurance controls (Table 5) are quite similar to those obtained in the absence of these variables (Table 3), and all are statistically significant at approximately the 1 percent level or better. The OLS estimates are again small and statistically insignificant.

4.3. *Exclusion of Widows and Divorced Females*

In assigning a birth year to households headed by divorced or widowed females, it was necessary to rely on the assumption that the former husbands of these women were born three years earlier than their wives; three years being the median difference in spousal ages for divorced and widowed elderly in 1982 (Engelhardt, Gruber and Perry, 2004). Although this assumption would appear to be innocuous, in Table 6 we provide a quick check by re-estimating equation (1) with these households omitted from the sample. Because the results are quite similar to those from Table 3, we retain these households to enhance the precision of our estimates when conducting specification checks that require the use of a smaller sample.

4.4. *Cohort Effects*

To examine the sensitivity of our results to possible cohort effects, we focus on two that seem especially plausible. The first involves individuals born during 1918 and 1919, who may have had *in utero* exposure to the flu virus that was present in epidemic proportions during those years. Almond (2003) presents evidence that such individuals may suffer from poorer health throughout their lives, suggesting that our instrument may be correlated with unmeasured

[23] The AHEAD does not contain information on whether these plans provide drug coverage, nor does it identify the plan letter for those with Medigap policies.

differences in prescription drug utilization. We investigate this possibility by dropping those households whose primary Social Security beneficiary was born during the flu years of 1918 and 1919. Results from this sensitivity check, which are displayed in Table 7, indicate little change relative to our baseline estimates.

A more general issue relates to the overall comparability of our treatment and control groups, given that our control group is made up of households whose primary beneficiaries were born over a period spanning 30 years. As one compares the notch households to households that are further removed in time, the likelihood of there being systematic differences in prescription drug use, for reasons unrelated to (notch-induced) differences in Social Security income, increases.

We examine this possibility by re-estimating equation (1) using only those households whose primary Social Security beneficiary was born between 1910 and 1920, reducing by almost two decades the range of cohorts being compared. As can be seen from Table 8, restricting attention to a narrower range of birth cohorts slightly increases the estimated effect of Social Security income on prescription use in our IV regressions (and this effect remains statistically significant at conventional levels), while the OLS estimates are once again small and statistically insignificant.

The findings presented in this section obviously cannot completely exclude the possibility of confounding due to unobserved differences across birth cohorts. However, for such effects to compromise our results they would have to vary systematically across our treatment and control groups. Two plausible sources of systematic differences in prescription use across the notch and non-notch cohorts were examined and were not found to dramatically affect our estimates. Nonetheless, the potential existence of biases related to such cohort-based effects should be kept in mind when considering our results.

5. Conclusions

In this paper, we made use of a unique natural experiment that led otherwise identical retirees to receive substantially different payments from the Social Security system. By comparing those households that enjoyed artificially high benefits (the so-called notch cohorts) to adjacent cohorts whose benefit levels were lower, ceteris paribus, we were able to construct instrumental variable estimates of the income sensitivity of prescription drug use among lower-income retirees. We found that a one percent increase in Social Security income leads to an increase of between a 0.87 and a 1.32 percent in the number of prescription medications used by lower-income elderly households in a typical month. These estimates contrast sharply with the small and statistically insignificant effects one obtains when Social Security income is treated as exogenous. Although we were not able to obtain estimates for retirees with higher incomes, the large elasticities we found for the 42 percent of elderly households belonging to our low-education (lower-income) sample is suggestive of some degree of income sensitivity among retirees further up the income distribution.

Given that Social Security comprises a larger share of total income for lower-income households, and given that these households are generally considered to be the most vulnerable to economic and health shocks, the large income effects we found for this group are potentially important when considering policy changes that affect transfer payments to lower-income elderly, such as changes in Social Security benefits or Medicare premiums, as well as for assessing the extent to which the new Medicare drug benefit will shield lower-income retirees from income-induced changes in their use of prescription medicines.[24] Moreover, because approximately 59 percent of the households in our low-education (lower-income) sample would still face significant cost sharing for prescription drugs under the new Medicare law, our results bear directly on how future changes in Social Security are likely to affect their drug utilization, and possibly, the drug utilization of somewhat higher-income beneficiaries for whom our estimates may also be applicable.

In closing, a few potential limitations of our empirical approach should be noted. First, because we did not have age by year of birth variation in our data, we were not able to control for the independent effects of age in a fully nonparametric way (i.e., by using a full set of age dummies). Instead, we modeled the effects of age using progressively higher-order polynomials and found that our results were not particularly sensitive to the choice of a functional form for the age variable. Second, because the Social Security notch varies by birth cohort, there is no way to definitively control for pure cohort effects in the use of prescription drugs or other variables. However, to bias our results, any unobserved cohort-level effects would have to vary systematically across our treatment and control groups and two specification checks based on plausible sources of cohort-based effects did not produce large departures from our initial estimates. These limitations notwithstanding, the changes in Social Security income created by the benefits notch provide a rare opportunity to utilize plausibly exogenous variation in retirement incomes for the purpose of estimating the income sensitivity of prescription drug use among a population of significant policy interest.

[24] Assuming the drug benefit does not lead to significant crowd out of currently-available retiree health benefits or is not scaled back as Medicare's fiscal problems become more urgent.

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Table 1. Summary Statistics

	Low Education Group: Less than High School (N = 1755)			High Education Group: High School Diploma or More (N = 2473)		
	Mean (Std. Dev.)	Min.	Max.	Mean (Std. Dev.)	Min.	Max.
Household Prescriptions (Usual number of medications taken per month)	4.01 (3.00)	0.00	22.00	3.82 (2.89)	0.00	26.00
Household Social Security Income (1993 dollars)	9625 (4305)	1620	30,756	10,782 (5195)	1392	48,000
Social Security Beneficiary Born Between 1915-1917	0.18 (0.39)	0.00	1.00	0.18 (0.38)	0.00	1.00
Head is a Single Male	0.13 (0.34)	0.00	1.00	0.10 (0.30)	0.00	1.00
Head is a Never-Married Female	0.02 (0.13)	0.00	1.00	0.02 (0.15)	0.00	1.00
Head is a Female Widow	0.36 (0.48)	0.00	1.00	0.37 (0.48)	0.00	1.00
Head is a Divorced Female	0.03 (0.16)	0.00	1.00	0.04 (0.20)	0.00	1.00
Age of Head	77.43 (5.40)	63.00	93.00	76.58 (5.14)	64.00	92.00
Head's Race is African American	0.11 (0.31)	0.00	1.00	0.04 (0.20)	0.00	1.00
Head's Race is Other	0.03 (0.16)	0.00	1.00	0.01 (0.10)	0.00	1.00
Head's Ethnicity is Hispanic	0.08 (0.27)	0.00	1.00	0.02 (0.15)	0.00	1.00
Household is located in a MSA	0.64 (0.48)	0.00	1.00	0.78 (0.42)	0.00	1.00

Notes: Summary statistics are weighted using the AHEAD household weights. The omitted category for the head-of-household variable is married male. All regression models also contain indicators for the nine Census regions.

Table 2. Effect of the Benefits Notch on Social Security Income by Educational Attainment

	Low Education Group: Less than High School			High Education Group: High School Diploma or More		
	Linear Age Specification	Quadratic Age Specification	Cubic Age Specification	Linear Age Specification	Quadratic Age Specification	Cubic Age Specification
Notch Indicator - Effect on household Social Security income (thousands of 1993dollars)	1.402 (0.243) [0.000]	1.064 (0.236) [0.000]	1.044 (0.255) [0.000]	0.343 (0.421) [0.421]	0.147 (0.428) [0.734]	0.172 (0.436) [0.696]
R-Squared	0.434	0.443	0.443	0.346	0.347	0.347
N	1755	1755	1755	2473	2473	2473
F-Statistic on Notch Indicator	33.18	20.40	16.80	0.67	0.12	0.16

Notes: The dependent variable is annual household Social Security income measured in thousands of 1993 dollars. The notch indicator equals "one" if the head of the household was born in 1915, 1916, or 1917, and "zero" otherwise. The age variable used in each specification is the age of the head. All models also include controls for the type of household (male head – married or cohabitating; male head – single; female head - never-married; female head – widowed; and female head - divorced), race of the head (white, African American, or other), Hispanic ethnicity of the head, whether the household is located in a MSA, and region (indicators for each of the nine Census regions). All regressions are weighted using the AHEAD household weights. Robust standard errors, adjusted for clustering within birth year, are displayed in parentheses; p-values are displayed in brackets.

Table 3. Effect of Social Security Income on Number of Prescriptions: Baseline Estimates

	Linear Age Specification		Quadratic Age Specification		Cubic Age Specification	
	OLS	IV	OLS	IV	OLS	IV
Household Social Security Income (thousands of 1993 dollars)	0.013 (0.026) [0.626]	0.489 (0.129) [0.001]	0.003 (0.024) [0.896]	0.552 (0.174) [0.004]	0.003 (0.024) [0.903]	0.583 (0.203) [0.008]
R-Squared	0.138	--	0.142	--	0.142	--
N	1755	1755	1755	1755	1755	1755
First-Stage Regression						
Notch Indicator - Effect of benefits notch on household Social Security income (thousands of 1993dollars)	--	1.402 (0.243) [0.000]	--	1.064 (0.236) [0.000]	--	1.044 (0.255) [0.000]
F-Statistic on Instrument	--	33.18	--	20.40	--	16.80

Notes: The dependent variable is the usual number of prescription medications taken per month by the respondent and their spouse. The age variable used in each specification is the age of the head. All models also include controls for the type of household (male head – married or cohabitating; male head – single; female head - never-married; female head – widowed; and female head - divorced), race of the head (white, African American, or other), Hispanic ethnicity of the head, whether the household is located in a MSA, and region (indicators for each of the nine Census regions). All regressions are weighted using the AHEAD household weights. Robust standard errors, adjusted for clustering within birth year, are displayed in parentheses; p-values are displayed in brackets.

Table 4. Adding Health Status Controls

	Linear Age Specification		Quadratic Age Specification		Cubic Age Specification	
	OLS	IV	OLS	IV	OLS	IV
Household Social Security Income (thousands of 1993 dollars)	0.034 (0.025) [0.177]	0.308 (0.145) [0.042]	0.029 (0.023) [0.221]	0.363 (0.184) [0.059]	0.029 (0.023) [0.216]	0.398 (0.206) [0.063]
R-Squared	0.384	--	0.385	--	0.385	--
N	1747	1747	1747	1747	1747	1747
First-Stage Regression						
Notch Indicator - Effect of benefits notch on household Social Security income (thousands of 1993dollars)	--	1.423 (0.240) [0.000]	--	1.079 (0.229) [0.000]	--	1.049 (0.247) [0.000]
F-Statistic on Instrument	--	35.03	--	22.19	--	18.03

Notes: The dependent variable is the usual number of prescription medications taken per month by the respondent and their spouse. In addition to the controls listed at the bottom of Table 3, all models include indicators that equal "1" if either member of the household reports being in fair or poor health, being in pain frequently, or having had any of the following medical conditions: high blood pressure; diabetes (currently); cancer (excluding minor skin cancers); lung disease (excluding asthma); a heart condition; stroke; psychiatric problems; arthritis (past 12 months); hip fracture; incontinence (past 12 months); cataract surgery; or "other" health condition. All regressions are weighted using the AHEAD household weights. Robust standard errors, adjusted for clustering within birth year, are displayed in parentheses; p-values are displayed in brackets.

Table 5. Adding Health Insurance Controls

	Linear Age Specification		Quadratic Age Specification		Cubic Age Specification	
	OLS	IV	OLS	IV	OLS	IV
Household Social Security Income (thousands of 1993 dollars)	0.030 (0.025) [0.228]	0.448 (0.121) [0.001]	0.020 (0.023) [0.378]	0.460 (0.154) [0.006]	0.020 (0.022) [0.377]	0.473 (0.175) [0.011]
R-Squared	0.151	--	0.155	--	0.155	--
N	1754	1754	1754	1754	1754	1754
First-Stage Regression						
Notch Indicator - Effect of benefits notch on household Social Security income (thousands of 1993dollars)	--	1.446 (0.214) [0.000]	--	1.146 (0.204) [0.000]	--	1.144 (0.226) [0.000]
F-Statistic on Instrument	--	45.65	--	31.69	--	25.50

Notes: The dependent variable is the usual number of prescription medications taken per month by the respondent and their spouse. In addition to the controls listed at the bottom of Table 3, all models include indicators that equal "1" if either member of the household reports having coverage from Medicaid, from another government insurance program (Railroad retirement, CHAMPUS, CHAMPVA, other military programs), or from other private health insurance. All regressions are weighted using the AHEAD household weights. Robust standard errors, adjusted for clustering within birth year, are displayed in parentheses; p-values are displayed in brackets.

Table 6. Excluding Households Headed By Widows and Divorced Females

	Linear Age Specification		Quadratic Age Specification		Cubic Age Specification	
	OLS	IV	OLS	IV	OLS	IV
Household Social Security Income (thousands of 1993 dollars)	0.029 (0.030) [0.342]	0.436 (0.132) [0.002]	0.0162 (0.028) [0.571]	0.437 (0.181) [0.022]	0.016 (0.028) [0.571]	0.443 (0.187) [0.025]
R-Squared	0.092	--	0.099	--	0.099	--
N	1069	1069	1069	1069	1069	1069
First-Stage Regression						
Notch Indicator - Effect of benefits notch on household Social Security income (thousands of 1993dollars)	--	2.214 (0.465) [0.000]	--	1.821 (0.481) [0.001]	--	1.808 (0.486) [0.001]
F-Statistic on Instrument	--	22.67	--	14.35	--	13.82

Notes: The dependent variable is the usual number of prescription medications taken per month by the respondent and their spouse. All regressions include the controls listed at the bottom of Table 3 and are weighted using the AHEAD household weights. Robust standard errors, adjusted for clustering within birth year, are displayed in parentheses; p-values are displayed in brackets.

Table 7. Excluding Cohorts Born During the 1918 Flu Epidemic: Birth Cohorts 1918 and 1919

	Linear Age Specification		Quadratic Age Specification		Cubic Age Specification	
	OLS	IV	OLS	IV	OLS	IV
Household Social Security Income (thousands of 1993 dollars)	0.020 (0.026) [0.458]	0.455 (0.136) [0.002]	0.008 (0.025) [0.750]	0.497 (0.181) [0.011]	0.007 (0.024) [0.767]	0.518 (0.232) [0.034]
R-Squared	0.138	--	0.143	--	0.143	--
N	1534	1534	1534	1534	1534	1534
First-Stage Regression						
Notch Indicator - Effect of benefits notch on household Social Security income (thousands of 1993dollars)	--	1.457 (0.273) [0.000]	--	1.084 (0.253) [0.000]	--	1.065 (0.298) [0.001]
F-Statistic on Instrument	--	28.49	--	18.33	--	12.74

Notes: The dependent variable is the usual number of prescription medications taken per month by the respondent and their spouse. All regressions include the controls listed at the bottom of Table 3 and are weighted using the AHEAD household weights. Robust standard errors, adjusted for clustering within birth year, are displayed in parentheses; p-values are displayed in brackets.

Table 8. Utilizing A Narrower Range of Birth Cohorts: 1910 – 1920

	Linear Age Specification		Quadratic Age Specification		Cubic Age Specification	
	OLS	IV	OLS	IV	OLS	IV
Household Social Security Income (thousands of 1993 dollars)	0.007 (0.034) [0.848]	0.507 (0.143) [0.005]	0.003 (0.030) [0.931]	0.606 (0.225) [0.022]	0.002 (0.031) [0.961]	0.586 (0.222) [0.025]
R-Squared	0.191	--	0.192	--	0.192	--
N	1062	1062	1062	1062	1062	1062
First-Stage Regression						
Notch Indicator - Effect of benefits notch on household Social Security income (thousands of 1993dollars)	--	1.202 (0.236) [0.000]	--	1.000 (0.246) [0.002]	--	1.072 (0.261) [0.002]
F-Statistic on Instrument	--	26.00	--	16.49	--	16.89

Notes: The dependent variable is the usual number of prescription medications taken per month by the respondent and their spouse. All regressions include the controls listed at the bottom of Table 3 and are weighted using the AHEAD household weights. Robust standard errors, adjusted for clustering within birth year, are displayed in parentheses; p-values are displayed in brackets.