

# The Effect of Pension Income on Elderly Earnings: Evidence from Social Security and Full Population Data

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## Abstract

We estimate the effect of pension income on earnings by examining the Social Security Notch, which cut lifetime discounted Old Age and Survivors Insurance (OASI) benefits by over \$6,100 on average for individuals born in 1917 relative to those born in 1916. Using Social Security Administration microdata on the U.S. population by day of birth and a regression discontinuity design, we document that the Notch caused a large increase in elderly earnings. The point estimates show that a \$1 increase in OASI benefits causes earnings in the elderly years to decrease by 46 to 61 cents due to an income effect, and the evidence is consistent with the hypothesis that only current (not future) benefits affect earnings. Under further assumptions we rule out more than a small substitution elasticity. Our results suggest that the increase in OASI benefits from 1950 to 1985 can account for at least half of the dramatic decrease in the elderly employment rate over this period.

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# 1 Introduction

Pensions may be key determinants of elderly work decisions. Among the most important pension programs is Social Security. In the U.S., Social Security Old Age and Survivors Insurance (OASI) is the single largest U.S. federal program, with \$706.8 billion in expenditures in 2014, or roughly one-fifth of federal government spending (Social Security Administration (SSA) 2015a). OASI is a significant source of income for the elderly, providing the majority of income for 65 percent of elderly beneficiaries (SSA 2015b). Using evidence from the U.S. and around the world, Gruber and Wise (1999, 2004) conclude that public pensions like OASI often reduce the incentive to work and therefore reduce work substantially.

Investigating a context that is often seen as one of the cleanest settings for studying the effects of pensions in general and OASI in particular, we come to novel conclusions. Using administrative SSA microdata on the U.S. population, with 24,619,604 observations on 724,106 individuals in our main sample, we examine the effects of the Social Security “Notch” created by the 1977 Social Security Act amendments on the earnings and employment decisions of the full population (both men and women).<sup>1</sup> Because of these amendments, individuals born on or after January 2, 1917 faced sharply different OASI benefits than those born before this date, allowing us to employ a Regression Discontinuity Design (RDD). We show that for individuals born on or after this threshold date, on average mean lifetime discounted real OASI benefits were discontinuously \$6,126 lower than for those born before this threshold, and the marginal lifetime returns to additional earnings (*i.e.* the substitution incentive) on average fell by 21 percent at this threshold.<sup>2</sup>

The variation we investigate represents the largest discontinuous change in OASI benefits to our knowledge, providing a particularly promising environment for studying these issues. At the same time, the 1977 Social Security amendments are important to understand in their own right, as they represent one of the major historical changes in OASI policy, with the potential to affect the time series of aggregate work outcomes substantially.

Our primary new finding is that we estimate very large income effects in the context

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<sup>1</sup> “Notch” in our context refers to the policy variation we describe, not a notch in the budget set.

<sup>2</sup> All dollar figures are in real 2012 dollars. “Lifetime” refers to benefits from the year after the legislation — 1978, when individuals in the 1917 cohort turn 61 — to the last year in our dataset, 2012, when these individuals turn 95. We refer to an individual’s “age” in a given calendar year as the highest age they attained during this year.

of a modern elderly pension program. From before to after the threshold date—which we call the “cohort boundary”—we find a very large, visually clear, and statistically significant discontinuous increase in mean discounted earnings and participation. As placebo tests, we show that the discontinuity in earnings does not appear (1) in our sample before the policy change could have been anticipated; (2) at thresholds between other birth cohorts that were not subject to a discontinuous change in Social Security benefits; or (3) at placebo cohort boundaries at other birthdays in the vicinity of the 1916/1917 cohort boundary.

We next examine the extent to which income or substitution effects underlie the observed patterns, to illuminate how much different aspects of policy reform can affect earnings. In a simple lifecycle model, the decrease at the cohort boundary in the substitution incentive should have unambiguously decreased earnings. Thus, we can use the discontinuity in earnings at the boundary, relative to the discontinuity in benefits, to estimate a bound on the income effect. The point estimate shows that a \$1 increase in discounted lifetime OASI benefits causes mean discounted earnings from 1978 to 2012 to decrease by at least 61 cents.

The estimated effects of the Notch on earnings and participation are insignificant in the period after the policy was passed but before individuals actually began experiencing cuts in the benefits they were receiving at the time, consistent with models in which earnings respond only to benefits received contemporaneously. This can be rationalized if individuals are myopic or liquidity-constrained (though one may presume liquidity constraints to be less relevant in an elderly population). In an empirical specification in which only current benefits affect current earnings, a \$1 increase in OASI benefits is estimated to reduce contemporaneous earnings by at least 46 cents due to an income effect.

Under further assumptions, we can estimate a Frisch substitution elasticity using a “difference-in-discontinuities” design comparing the size of the earnings discontinuity in adjacent, comparable years in which individuals were subject to sharply different, anticipated substitution incentives due to the policy change. We estimate an insignificant Frisch elasticity, and in a baseline calculation our 95 percent confidence interval rules out an elasticity of 0.010 or higher. It is striking to find that Frisch elasticities are at most small, even in the presence of very large substitution incentives. Although beneficiaries may not have understood these particular substitution incentives (Blinder, Gordon, and Wise 1980), such

lack of transparency is common to many incentives created by OASI, other pensions, and other aspects of retirement decisions (Lusardi and Mitchell 2007; Liebman and Luttmer 2012, 2015; Brown, Kapteyn, Luttmer, and Mitchell 2013). Although previous literature has found that public pensions have large disincentive effects on work (Gruber and Wise 1999, 2004), our results illustrate that in a context like ours, responses to substitution incentives can be negligible, and income effects can be very large.

If our results generalize to other time periods, they could help account for important aspects of the evolution of the elderly employment-to-population ratio over the latter half of the 20th century.<sup>3</sup> As shown in Figure 1, Current Population Survey (CPS) data show the striking trend that from 1950 to 1985, the elderly employment-to-population ratio decreased to well under half of its initial level, from 26.6 percent to 11.2 percent. Over the same period, mean OASI benefits and replacement rates more than doubled (Social Security Administration 2015, Clingman, Burkhalter, and Chaplain 2014). In a conspicuous turnaround, from 1985 to 2000 the elderly employment-to-population ratio increased to 13.2 percent and has continued to increase secularly to the present—coincident with a slowdown in the 1980s in the growth rate of OASI benefits shown in Figure 1, which was caused by the 1977 Amendments. For the 65-69 year-old population that we find is most strongly affected by the Notch, an even sharper turnaround in the employment rate is observed in the mid-1980s (Appendix Figure 1). To probe the initial roots of this turnaround in the mid-1980s, we calculate in a baseline that under our RDD estimates, the decrease in the growth rate of mean OASI benefits around 1985 can account for 28 percent of the contemporaneous increase in the growth rate of the employment rate of those over 65, and 57 percent of the increase for those 65 to 69 years old. If earnings react to contemporaneous benefits as our results suggest, then our estimates would imply that the 1950 to 1985 increase in OASI benefits can account for 58 percent of the decrease in the elderly employment-to-population ratio over this period, and 77 percent of the decrease among 65 to 69 year-olds.

In an illustrative case we use our crowdout estimates to calculate that a \$1 decrease in OASI benefits would decrease the unified federal government deficit by around \$1.12. This

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<sup>3</sup>The labor force participation rate shows similar trends to the employment-to-population ratio. We discuss the employment-to-population ratio because it seems conceptually more similar to the positive earnings dummy in our data.

is because the \$1 benefit cut would increase individuals’ average earnings, thus increasing federal income and payroll tax revenue by 12 cents. The combined Old Age, Survivors, and Disability Insurance (OASDI) Trust Fund alone would reduce its deficit by \$1.06 due to a \$1 decrease in OASI benefits. As a result, the benefit cuts necessary to eliminate OASDI’s 75-year actuarial deficit would be on the order of \$577 billion smaller in present value than under a calculation that did not take behavioral effects into account.

Our paper is related to an existing body of work on the effects of elderly pensions and other retirement income on employment, earnings, or retirement decisions (*e.g.* Diamond and Hausman 1984; Fields and Mitchell 1984; Hurd and Boskin 1984; Burtless and Moffitt 1985; Gustman and Steinmeier 1985; Hausman and Wise 1985; Burtless 1986; Stock and Wise 1990; Krueger and Pischke 1992; Samwick 1998; Coile and Gruber 2000; Coile and Gruber 2004; Brown, Coile, and Weisbenner 2006; Mastrobuoni 2009; Behagel and Blau 2012; Manoli and Weber forthcoming).<sup>4</sup> Although none of the literature has clearly documented very large income effects in the context of a modern elderly pension program as we do, Costa (1995, 2010) and Fetter and Lockwood (2016) find large income effects of pensions on retirement in the contexts of U.S. civil war veterans and the Old Age Assistance Program, respectively. In a more modern context, some literature has found evidence of an income effect in OASI (*e.g.* Coile and Gruber 2007), but usually of modest size.<sup>5</sup>

Aside from our paper’s broader relevance to understanding pensions and employment patterns more generally, it is also the first to our knowledge in our context to examine the labor supply effects of the Notch in the full population; to use an explicit RDD strategy; and to use administrative data on the full U.S. population. Our paper examines both men and women, whereas the original economics paper that innovated the use of the Notch to study economic outcomes, Krueger and Pischke (1992, hereafter “KP”), examines only men.<sup>6</sup> Using Current Population Survey data and variation in OASI benefits and labor force participation

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<sup>4</sup>Several papers have examined the Social Security Retirement Earnings Test (RET) (Burtless and Moffitt 1985; Friedberg 2000; Song and Manchester 2007; Gelber, Jones, and Sacks 2013). The RET is a substantially different context than ours, for example because the RET creates large, salient substitution incentives but negligible changes in income in the region of the exempt amount that is the focus of this literature.

<sup>5</sup>Brown, Coile, and Weisbenner (2010) find large inheritance wealth effects on retirement.

<sup>6</sup>Other literature has examined the effects of the Notch on other outcomes, including elderly living arrangements (Engelhardt, Gruber, and Perry 2002), mortality (Snyder and Evans 2006), prescription drug use (Moran and Simon 2006), weight (Cawley, Moran and Simon 2010), long-term care services (Goda, Golberstein, and Grabowski 2011), and mental health (Golberstein 2015).

across birth years, KP find no evidence that OASI benefits significantly affect men’s labor force participation. KP’s confidence intervals rule out more than a moderate effect on male participation, and indeed we find a moderate effect on male (and female) participation, consistent with their confidence intervals. The moderate participation response we find corresponds to very large crowdout of earnings. Our results isolate variation across cohorts at the day-of-birth level, as opposed to the year-of-birth level, and we take advantage of the statistical power afforded by full population data and comparisons of otherwise closely comparable individuals born as little as one day apart.

The rest of the paper is structured as follows. Section 2 describes the policy environment. Section 3 describes the data. As an initial empirical step, Section 4 documents the causal effect of the Notch policy on earnings and participation. Section 5 briefly describes a simple lifecycle framework for understanding the responses; describes the relationship of our identification strategy to this model; and explores estimation of income and substitution effects. Section 6 discusses the relationship between our results and KP. Section 7 concludes. The online appendix presents additional results.

## 2 Policy Environment

OASI provides annuity income to the elderly and to survivors of deceased workers. Individuals with sufficient years of eligible earnings can claim OASI benefits through their own earnings history as early as age 62, the Early Entitlement Age (EEA). Individuals in our sample reach the Normal Retirement Age (NRA) at 65, when they can claim their full OASI benefits.

The 1977 Social Security Act amendments changed the formula determining OASI benefits as a function of claimants’ earnings histories. Before 1977, the Primary Insurance Amount (PIA), which forms the basis for the monthly OASI benefit, was an increasing and (typically) progressive function of the Average Monthly Wage (AMW). The AMW was an average of an individual’s nominal earnings over ages  $t$  among the set  $\mathbb{T}$  of their highest-earning years,  $AMW = \sum_{t \in \mathbb{T}} W_t / n$ , where  $n$  is the total number of years in the set  $\mathbb{T}$ . For OASI beneficiaries who became eligible in 1970, for example, the PIA was calculated as: 81.83 percent of the first \$110 of AMW, plus 29.76 percent of the next \$290 of AMW,

plus 27.81 percent of the next \$150 of AMW, plus 32.69 percent of the next \$100 of AMW. The 1972 Social Security Act amendments indexed the replacement rate in each bracket to the CPI. This created “double-indexation” of Social Security benefits, under which inflation raised benefits through two channels: inflation both increased the AMW (because the AMW was calculated using nominal wages), and inflation mechanically increased the replacement rate. In the high-inflation years of the late 1970s, this implied rapidly increasing benefits that were widely seen as financially unsustainable (GAO 1988).

The 1977 amendments introduced a new formula that ended double indexation. For birth cohorts starting in 1922, PIA is based on Average Indexed Monthly Earnings (AIME). AIME is calculated as a function of past earnings, where earnings prior to age 62 (in the highest-earning years) are inflated by the growth in national earnings, and its replacement rate schedule is generally more progressive than the AMW schedule (SSA 2015b).

This policy change implied sharply lower Social Security benefits for those receiving benefits under the new formula. To ease the transition to the new formula, the 1977 Amendments created a “transitional guarantee” for those born between 1917 and 1921 (inclusive). Claimants in these cohorts received the maximum of benefits calculated two ways: (1) Under the new formula based on the AIME; or (2) under the old AMW formula with one change relevant for the 1917 cohort: earnings after age 61 are not used in calculating average earnings, *i.e.*  $AMW = \sum_{t \in \mathbb{T} \text{ and } t < 62} W_t/n$ .<sup>7</sup> The second method was the “transitional guarantee.”

Social Security rules in a given birth cohort apply to individuals born January 2 or later in that birth year. For example, the rules affecting what we call the “1916 cohort” apply to individuals born January 2, 1916 through January 1, 1917 (inclusive).

In the 1916 cohort, nearly everyone was covered by the AMW formula, whereas in the 1917 birth cohort, more were covered by the transitional guarantee than by the AIME formula (McKay and Schobel 1981).<sup>8</sup> As a result, those born on January 2, 1917 or after faced a substantially different OASI benefit structure than those born January 1, 1917 or earlier.

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<sup>7</sup> The 1972 Social Security Act amendments indexed the replacement rate within each bracket to the CPI, but the transitional guarantee formula also specified that after December 1978, no such inflation adjustments are made to benefits until the calendar year in which an individual reaches age 62 and following years. However, since those in the 1917 cohort reached age 62 in 1979, *i.e.* just after December 1978, this provision did not discontinuously affect those in the 1916 and 1917 cohorts. However, this provision did lead to small discontinuities in average benefits at cohort boundaries from 1917/1918 to 1921/1922.

<sup>8</sup> A very small percentage was covered by other methods, the 1977 Old Start Method or the Regular Minimum benefit (McKay and Schobel 1981).

This policy change could create both income and substitution effects on earnings. Because earnings after age 61 were not used in calculating the AMW for those covered under the transitional guarantee, and because the OASI rules guarantee that earnings after age 61 can only cause an increase—but cannot cause a decrease—in an individual’s PIA, the AMW of an individual in the 1916 cohort who earned in their highest-earning years after age 61 would be higher than the AMW of an individual with the same earnings history in the 1917 cohort. This led to a substantial decrease in average benefits for the 1917 cohort relative to the 1916 cohort, which should lead to income effects on earnings. These decreases in benefits were widely publicized, including in a famous “Dear Abby” column on the discrepancies in benefits for similar individuals (GAO 1988).<sup>9</sup>

There was also a change in substitution incentives at the cohort boundary. Because earnings after age 61 were not taken into account in calculating the AMW under the transitional guarantee, the net marginal returns to additional earnings after age 61 fell at the boundary. In other words, additional earnings after age 61 often raised (and never lowered) AMW and therefore OASI benefits in the 1916 cohort, but had no effect on OASI benefits for those receiving the transitional guarantee in the 1917 cohort. The returns to additional earnings in the 1916 cohort were very large, as average marginal replacement rates were very large in part because the 1972 amendments caused them to grow quickly. An increase in earnings in a given year led to a modest change in future OASI benefits received in each year; however, discounted over the course of the 18 years an average individual collected OASI benefits, this typically cumulated to a large net incentive to earn more in any given year. By contrast, in the 1917 cohort, earning an extra dollar had at most a small average effect on lifetime Social Security benefits.<sup>10</sup> Indeed, we calculate that the net lifetime return to additional pre-tax, pre-transfer earnings in 1979 fell by 21 percent at the cohort boundary for otherwise identical individuals.

Although the change in substitution incentives at the boundary was very large, these substitution incentives may have been opaque to many beneficiaries (Blinder, Gordon, and

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<sup>9</sup>When we say that a variable (*e.g.* benefits) “increased” (“decreased”) at the cohort boundary, we mean that the variable increased (decreased) when moving from the end of the 1916 cohort to the beginning of the 1917 cohort.

<sup>10</sup>For individuals subject to the actuarial adjustment or Delayed Retirement Credit (as they interact with the Earnings Test), a change in earnings in a given year could affect lifetime OASI benefits under the transitional guarantee, but on average such an effect is very small in our data.



Wise 1980).<sup>11</sup> Understanding these substitution incentives required a sophisticated understanding of the dependence (or lack thereof) of future OASI benefits on current earnings. This stands in apparent contrast to the wide publicity about the Notch’s cut in OASI benefit *levels* from the 1916 cohort to subsequent cohorts, and to the ease of knowing one’s own income level.

The 1977 amendments were signed into law on December 20, 1977. The legislative history shows that the discontinuity in benefits between the 1916 and 1917 cohorts could not have been anticipated with confidence until 1977 (GAO 1988). Because of this history, we assume that the policy discontinuity from the 1977 amendments would not yet have had a discontinuous effect on earnings around the boundary in 1976 and earlier years; we treat 1978 and later as years when the policy discontinuity could have affected earnings; and we exclude 1977 from most of our analysis as expectations in this year are unclear.

Because the transitional guarantee specified that after December 1978 no inflation adjustments are made to benefits until the calendar year when an individual reaches age 62, the 1977 amendments also created small discontinuities in benefits at the 1917/1918, 1918/1919, 1919/1920, 1920/1921, and 1921/1922 boundaries (GAO 1988). Because these benefit discontinuities are much smaller than the 1916/1917 discontinuity, we expect to have less statistical power in these contexts, and we primarily focus on the 1916/1917 boundary.

### 3 Data

To investigate the effect of these policy changes, we obtained administrative data on the full U.S. population from the Social Security Master Earnings File (MEF) and Master Beneficiary Record (MBR) for birth cohorts 1916 through 1923. The data have information on exact date of birth; OASI benefits paid in the last year an individual received benefits; exact date of death; month and year of initially claiming OASI; gender; race; and annual earnings capped at the OASI maximum taxable earnings level in each year separately from 1951 to 2012. Annual total uncapped earnings are also available beginning in 1978. All of the earnings data come from W-2s, mandatory information returns filed with the Internal Revenue Service (IRS) by employers for each employee for whom the firm withholds taxes and/or to whom

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<sup>11</sup>Burkhauser and Turner (1981) present a contrasting view.

remuneration exceeds a modest threshold. Thus, we have data on earnings regardless of whether an employee files taxes. We use capped earnings so that the measure of earnings is held constant from before 1978 to after; when we use annual uncapped earnings information since 1978, we obtain extremely similar results. Using information on Social Security rules from Social Security Annual Supplements—*e.g.* benefit schedules of PIA as a function of AIME or AMW, cost-of-living adjustments, special minimum benefits, spousal benefit rules, the actuarial adjustment, the Delayed Retirement Credit (DRC), the Earnings Test (and its interaction with the actuarial adjustment and DRC), *etc.*—we calculated OASI benefits on the basis of earnings and claiming histories, and we validated our measure of calculated benefits against MBR information on benefits in the final year of benefit receipt.

The data measure pre-tax earnings. This represents our main outcome of interest, as it is relevant to evaluating the net effects of OASI expenditures on the government budget, as well as to welfare evaluation (Chetty 2009). Our data allow us to calculate pre-tax OASI benefits; this affects the results negligibly relative to measuring after-tax benefits, because OASI benefits only became taxable in 1984, when the vast majority of individuals in the 1916/1917 cohorts had low enough income that their Social Security benefits were not taxable. By examining pre-tax benefits and pre-tax earnings, we answer a policy-relevant question: how a given cut in benefits paid by SSA would affect earnings supply.

Our measure of earnings excludes self-employment income, as this is often subject to manipulation (Chetty, Friedman, and Saez 2013). We remove from the data those who received DI or OASI benefits before our period of interest begins in 1977, or who died before 1977.<sup>12</sup> We include all other individuals, including those who collect benefits as retired workers, auxiliary beneficiaries, or survivors. From the calendar year after an individual dies until 2012, benefits and earnings appear in the data as zeroes.

When one spouse earns less than the other, under the OASI rules the spouse with lower PIA in total receives the maximum of: (a) the benefit to which they are entitled on their own record, or (b) one-half the benefit due to the higher-PIA spouse (either because the lower-PIA spouse collects this amount as a “secondary” beneficiary who is ineligible to collect on

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<sup>12</sup>The effect of Social Security disability benefits on disability beneficiaries’ earnings is another question of interest, but our setting is not well suited to study this issue. At the NRA, DI beneficiaries begin to receive benefits as an OASI beneficiary would. Only a very small fraction of our cohorts claims DI between 1977 and NRA.

their own record, or because they are “dual-entitled” and thus their own benefit plus their spousal benefit equals this amount). Wives typically earn less than their husbands in these cohorts, and 60 percent of women in our sample collected benefits as a secondary or dual-entitled beneficiary. Thus, for wives who are secondary or dual-entitled beneficiaries, their total OASI benefit is constant (all else equal) regardless of which side of the discontinuity their own DOB lies on, because their total benefit received depends only on their husband’s DOB.<sup>13</sup> Thus, our estimated effects for women apply to a population with particularly high lifetime earnings relative to their husbands. For the higher earner (specifically non-dual-entitled primary beneficiaries), the OASI benefit schedule is discontinuous at the cohort boundary in their own DOB. Among the men that we study, nearly everyone collected OASI as a non-dual-entitled primary beneficiary, implying that their own benefits depend only on their own DOB (and earnings history).

Due to the nature of the data, we cannot consistently estimate a husband’s response to a wife’s OASI benefit or vice versa. We only observe husbands linked to their wives when one spouse is collecting as a dual or secondary beneficiary, and whether one is a dual or secondary beneficiary is endogenous to the size of the husband’s and wife’s separate benefits. This is because higher benefits for a husband make it more likely that the wife claims as a secondary or dual beneficiary, which makes an analysis of within-household responses untenable.<sup>14</sup>

In those cases in which we discount, for illustrative purposes benefits or earnings are discounted in the baseline at a three percent real interest rate (the average real Treasury rate over 1978 to 2012, rounded to the nearest percent). We discount to 1977 terms and then express discounted benefits or earnings in real 2012 dollars.

How OASI affects consumption or savings is an important complementary question. The available data on consumption from the Consumer Expenditure Survey, or on saving or assets from the Survey of Consumer Finances or the Health and Retirement Study, have much smaller sample sizes than the full population data—at least two orders of magnitude smaller. Power calculations on datasets with information on consumption, savings, or assets

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<sup>13</sup>This assumes that the OASI benefit based on a wife’s own earnings history does not exceed one-half the benefit of the primary earner, both when the wife is born in 1916 and in 1917.

<sup>14</sup>It also does not make sense to limit the sample in any given year to individuals claiming in that year, since claiming OASI is potentially endogenous to one’s OASI benefit. Similarly, we cannot limit the sample to those collecting only as primary beneficiaries, as this could also be endogenous.

have shown that indeed, it would not be possible to estimate significant effects even with very large effect sizes.

Table 1 shows summary statistics. We use data from 724,106 individuals born within 100 days of the cohort boundary from 1978 to 2012, corresponding to 24,619,604 individual-year observations. After averaging by DOB, we have 200 observations on each of our main outcomes. Mean discounted earnings from 1978 to 2012 is \$85,901.01. 11.03 percent of individual-year observations from 1978 to 2012 have positive earnings. Mean discounted benefits from 1978 to 2012 are \$120,151.60. The mean age of claiming OASI is 63.50. Each day on average has 3,979 observations; this is smaller than counts for the full U.S. population born on each day due to our sample restrictions.

## 4 Documenting the Causal Effect of the Notch Policy

As a first empirical step, we document the causal effects of the Notch policy on OASI benefits, substitution incentives, earnings, and participation at the cohort boundary. We describe our empirical strategy; present graphical and statistical evidence of discontinuities in the outcomes at the boundary; and demonstrate that our findings pass key validity checks.

### 4.1 Basic Empirical Strategy for Documenting Effect of Notch

To estimate the effect of the Notch policy we use an RDD, which exploits the discontinuous relationship between DOB and OASI benefits at the boundary, relative to the assumed smooth relationship between DOB and average earnings that would exist in the absence of the discontinuous change in OASI benefits (see Imbens and Lemieux 2008 and Lee and Lemieux 2010 for surveys of RDD methods). Thus, our evidence effectively documents whether we observe a sharp change in earnings (and other outcome variables) at the boundary.

Specifically, we estimate this regression:

$$E_j = \beta_1 D_j + \beta_2 DOB_j + \beta_3 (D * DOB)_j + \varepsilon_j \quad (1)$$

Here  $j$  indexes DOB;  $E$  represents an outcome of interest (such as mean discounted real earnings by DOB);  $D$  is a dummy for DOBs on or after January 2, 1917;  $DOB$  is a linear

trend in day of birth; and  $(D * DOB)$  is an interaction between  $D$  and  $DOB$ .<sup>15</sup> The subscript  $j$  indicates that we have taken the mean of the variable on DOB  $j$  across all individuals in the sample. The main coefficient of interest here is  $\beta_1$ , representing the change in the mean level of the outcome variable at the cohort boundary. We interpret this as the average treatment effect of the Notch policy, estimated among those at the boundary. We use robust standard errors throughout the paper.

Of course, many other factors could have affected earnings in our sample, such as private pension amounts, health (including the effects of the pandemic flu of 1918), macroeconomic factors, *etc.* The RDD identification assumption is that such factors would have affected earnings smoothly in date of birth, as opposed to the sharp change in benefits experienced by those in the 1917 cohort relative to those in the 1916 cohort. Similarly, the 1978 and 1986 amendments to the Age Discrimination in Employment Act (ADEA) extended the ages at which age discrimination in employment was prohibited, which could have increased elderly work (Burkhauser and Quinn 1983). However, neither of these amendments had a discontinuous effect on elderly work incentives around the 1916/1917 cohort boundary and therefore should not confound our identification strategy. Our fine-grained data by DOB are helpful because more aggregate data could be confounded by other factors that lead to smooth trends in outcomes over the calendar year (Buckles and Hungerman 2013).

We use data aggregated to the day-of-birth level—rather than at the individual level—to estimate standard errors that are likely to be “conservative” (Angrist and Pischke 2008), given the possibility of positively correlated shocks to individuals at the DOB level. We weight the regression by the number of non-missing observations on each day of birth.

We use the procedure of Calonico, Cattaneo, and Titiunik (2014, henceforth “CCT”) to select the bandwidth. The results are very similar when we use the optimal bandwidth selection procedure of Imbens and Kalyanaraman (2009) or when we use cross-validation, and we show that the results are generally significant under any bandwidth from 20 to 100. For our main outcome, *i.e.* mean discounted real earnings, the CCT procedure selects a bandwidth of 56 days. To hold the sample constant across specifications, we use this

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<sup>15</sup> Allowing for different slopes on either side of the boundary makes little difference to our results, relative to constraining the slope to be equal on both sides.

bandwidth throughout our main results. Appendix Table 1 shows comparable results when we choose the bandwidth separately for each outcome.

We call (1) the “linear” specification because we control for a linear function of DOB on both sides of the boundary. Gelman and Imbens (2014) argue that RDDs should use local linear or quadratic polynomials. In an alternative specification, we add a quadratic trend in DOB to model (1), as well as an interaction of this quadratic trend with a dummy for the 1917 cohort. In some specifications we additionally control for the means of gender and race dummies by DOB. For all of our main outcomes, the specification that minimizes the Akaike Information Criterion (AIC) and Bayes Information Criterion (BIC) is always the linear specification without controls, and we choose this as our baseline.

It will also be useful to compare the discontinuity  $\beta_1$  in an outcome at the cohort boundary to the discontinuity in discounted real OASI benefits. We define mean lifetime discounted OASI benefits  $B_{jPDV}$  as  $B_{jPDV} \equiv \sum_{i \in I} \sum_{t=t_0}^T \frac{1}{(1+\rho)^{t-t_0}} \frac{B_{ijt}}{N}$ , where  $t_0 = 1978$  and  $T = 2012$  in our empirical application,  $\rho$  is the discount rate, and  $I$  reflects the full set of  $N$  individuals in the sample. We can then regress  $B_{jPDV}$  on the covariates:

$$B_{jPDV} = \gamma_1 D_j + \gamma_2 DOB_j + \gamma_3 (D * DOB)_j + \nu_j \quad (2)$$

$E_{jPDV}$  denotes mean discounted real earnings on DOB  $j$ , defined analogously to  $B_{jPDV}$ .

## 4.2 Preliminary Results

We begin by showing preliminary results that demonstrate the suitability of our RDD design. Our figures show the means of outcome variables averaged by 10-day bins of DOB around the cohort boundary. 10-day bins represent the largest bin size that passes the two tests of excess smoothing recommended by Lee and Lemieux (2010) for RDDs, though the graphical patterns are robust to using bins of other sizes. We show seven bins on either side of the boundary to display the variation within the bandwidths selected by CCT, Imbens and Kalyanaraman (2009), or cross-validation.

Figure 2(a) shows that the number of individuals in 1977 appears continuous at the boundary (following McCrary 2008). Figure 2(b) shows the point estimates and 95 percent confidence intervals on  $\beta_1$  in (1) when the outcome is the number of observations, for each

bandwidth from 20 to 100.<sup>16</sup> Figure 2(b) and Table 2 confirm that there is no significant discontinuity. Table 2 and Appendix Figures 2-3 show that the proportion male and the proportion white are also smooth through the boundary.

Figure 3 verifies that discounted lifetime OASI benefits decrease discontinuously and quite substantially when crossing the cohort boundary. Table 3 Row A shows that at the baseline bandwidth, lifetime benefits fall discontinuously by \$6,125.64. This effect is significant at the 1% significance level; in general, the effects we estimate in the main tables are significant at the 1 percent or the 5 percent level, unless otherwise noted.<sup>17</sup> We calculate  $\mu_{ijt}$ , the lifetime discounted increase in OASI benefits caused by \$1 of extra earnings in year  $t$  for individual  $i$  born on DOB  $j$ , by simulating the effect on lifetime discounted benefits of an increase in year  $t$  earnings of \$1.  $\mu_{j1979}$ , the mean by DOB  $j$  of  $\mu_{ijt}$  in 1979, falls discontinuously at the boundary by 22 percentage points (Table 3 Row B). This corresponds to a discontinuous 21 percent decrease at the boundary in  $1 + \mu_{j1979} - \tau_{j1979}$ , the lifetime discounted returns to earnings net of both benefits ( $\mu_{j1979}$ ) and marginal tax rates ( $\tau_{j1979}$ ).<sup>18</sup>

### 4.3 Discontinuities in Earnings and Participation Rates

Having demonstrated the suitability of the RDD design, we proceed to estimate discontinuities in our outcomes at the boundary. Figure 4(a) shows a main result: at the cohort boundary, we observe a sharp increase in  $E_{jPDV}$  of several thousand dollars. Figure 4(b) shows that the estimated discontinuity is always statistically significant, and the point estimate of  $\beta_1$  is always several thousand dollars. Although the point estimate is usually smaller for larger bandwidths, typically the estimates at different bandwidths are statistically indistinguishable, and other bandwidths are less relevant than the CCT bandwidth of 56. Table 3 shows that in the baseline specification discounted earnings from 1978 to 2012 rise by \$3,766.02 at the cohort boundary.

Figure 5 shows the “participation rate”: the percent of individual-year observations from 1978 to 2012 in which an individual has positive earnings (as before, taking the mean of this variable by DOB bin). Figure 5(a) shows a clear increase in participation at the boundary.

<sup>16</sup>For bandwidths under 20, the regressions are usually under-powered.

<sup>17</sup>Significance tests throughout the paper refer to two-sided tests of coefficient difference from zero.

<sup>18</sup>We calculate tax rates using TAXSIM (Feenberg and Coutts 1993) to find the mean marginal tax rate of those 65 and older in the IRS Statistics of Income public use files. Using averages avoids endogeneity of marginal tax rates.

Table 3 Row D shows that in the baseline estimate of (1), the participation rate increases by 0.40 percentage points at the boundary. The implied effects are similar when examining the log odds of participation in Row E.

We interpret the discontinuity in earnings at the cohort boundary as reflecting movements along an earnings supply curve, not changes in demand by firms, since such changes should have been materially similar on either side of the boundary as should any general equilibrium effects of the policy change more broadly. We interpret our measured effects, including the income effects and substitution elasticities we measure later, as reflecting responses net of any adjustment frictions such as lack of awareness of substitution incentives. Even without being explicitly aware of a policy discontinuity at the cohort boundary, we could observe a response because beneficiaries are reacting, for example, to the amount of OASI payments they are receiving, or to their total income, both of which could be more salient.

### 4.3.1 Results by Time Period

To illustrate how the effects vary across ages, in Figure 6 we show the coefficient  $\beta_1$  from model (1) and its confidence interval when the dependent variable is mean earnings by DOB in each three-year time period  $t$ , and we run the regression separately for each  $t$ . The figure shows that the Notch had an insignificant effect on earnings shortly after the policy went into effect, in 1978-1980. The effects are largest—nearly \$400 a year—in the 1980s, when the 1917 cohort was in its mid sixties to early seventies. The estimated effects decline and become insignificant in 1999 and after, corresponding to ages 82 and above for the 1917 cohort, when individuals typically have low mean earnings (in all cohorts). The effects on participation are also largest in the 1980s. Appendix Figure 4 shows a similar effect of the Notch on earnings and participation by time period among those who lived until at least 80 years old, allowing us to compare a constant sample over time until 1997.

### 4.3.2 Other Outcomes

Table 3 shows that the notch causes an increase in the year of “retirement,” *i.e.* the mean of the last calendar year an individual earned a positive amount, by 0.16 years in the baseline (see Appendix Figure 5).<sup>19</sup> Appendix Figure 6 and Appendix Table 2 show the “intensive

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<sup>19</sup>If some individuals retire exactly on their birthday, there is no reason this should lead to a discontinuity in earnings, but it could cause a discontinuity in our measure of participation if it leads people to receive positive earnings in an extra calendar



margin”: mean yearly earnings conditional on having positive earnings in that year, from 1978 to 2012. The point estimate is usually modest, positive, and insignificant, which could suggest that the “extensive margin” (*i.e.* participation) effects can account for much of the observed effect on earnings. However, an important limitation affects this analysis: the sample is selected based on an outcome variable (*i.e.* whether earnings are positive), which can lead to biased and inconsistent estimates. Appendix Table 2 also shows that the Notch has an insignificant effect on earnings in 1977 and 1978, before individuals in the 1917 cohort reached the EEA.<sup>20</sup> Appendix Figure 7 shows the age of claiming OASI; these claim age results are difficult to interpret since all claimants are recorded as claiming on the first of the month, creating a small discontinuity in claim age at the beginning of every birth month, including January 1917.<sup>21</sup>

## 4.4 Placebo Tests

We run a number of placebo tests, which help establish that the discontinuity in earnings documented above was due to the causal effect of the Notch. Figure 7 shows the coefficients and confidence intervals on  $\beta_1$  when we run model (1), except rather than estimating the discontinuity at the cohort boundary (*i.e.* defining the dummy  $D$  as 0 before January 2, 1917 and 1 after), we instead place the discontinuity at 100 “placebo cohort boundaries,” on each DOB from 50 days before January 2, 1917 to 50 days after (*i.e.* for each of these placebo cohort boundaries, we define  $D$  as 0 before that placebo date and 1 after it). The estimated effect is maximized exactly at the actual cohort boundary. Thus, this permutation test shows an effect that is significant at the 1 percent level.

We further conduct four more placebo tests in contexts where there is no reason we should

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year. However, our placebo tests show no systematic evidence of a discontinuity in participation at other cohort boundaries.

<sup>20</sup>Some of the effect on earnings we find could in principle be mediated through an effect on mortality, which could affect the interpretation of the effects. The effects on earnings that we estimate are policy-relevant, in the sense that they reflect the raw effect on earnings (which is pertinent, for example, to estimating the revenue consequences of a policy change). Moreover, Snyder and Evans (2006) find moderate mortality effects of the Notch, suggesting that at most only a small fraction of the earnings effects we estimate could be mediated through this outcome. Appendix Figure 8 does not show clear evidence for an effect of the Notch on mortality, and the point estimates are also small. Further investigating the mortality effect of the Notch using SSA data would be a fruitful topic for future research.

<sup>21</sup>Social Security rules specify that a claimant must be 62 throughout the month in order to receive benefits at the EEA in that month, thus leading to slightly different benefits for those born on January 2 than for those born earlier (Kopczuk and Song 2008). (Social Security follows English common law and specifies that an individual attains an age on the day before his or her birthday, implying that the cutoff is January 2 rather than January 1.) This is also true on the second of every month. However, lifetime benefits are barely affected on average because the actuarial adjustment is nearly actuarially fair. Moreover, if this were a substantial factor in our results, then we should find discontinuities at placebo boundaries, including in other years and months, but we do not.

find an earnings discontinuity. First, Figure 6 shows that prior to the Notch legislation—in 1968-1970, 1971-1973, or 1974-1976—there is no significant discontinuity in earnings. As an example, Table 4 shows that this is true in the regressions for 1974-1976. Second, among the cohorts for which we were able to obtain a 100% sample of the SSA data, *i.e.* from 1916 to 1923, only one cohort boundary—the 1922/1923 cohort boundary—has no discontinuity in policy and can therefore serve as a placebo cohort boundary. Table 4 shows that there is no significant change in earnings or participation at this boundary.

Third, we were able to obtain a 10 percent random sample of individuals in the SSA data for birth cohorts between 1910 and 1930. Among these cohorts, the DRC discontinuously increased at the 1924/1925, 1926/1927, and 1928/1929 boundaries. Thus, to pool data across all of these cohort boundaries for which there is no discontinuity in policy (and outside the 1922/1923 boundary we investigated above), we pool the 10 percent sample from ten boundaries, from 1910/1911 to 1915/1916 as well as 1923/1924, 1925/1926, 1927/1928, and 1929/1930. Using this pooled sample we regress earnings or participation on a dummy for being born after January 2 around any of these boundaries, along with a linear spline in date of birth relative to these boundaries. Table 4 shows no significant change in earnings or participation at the pooled cohort boundaries in this specification.

Fourth, we were able to obtain W-2 wage earnings data from IRS on the full U.S. population from 1999 to 2013 on all cohort boundaries from 1923/1924 to 1936/1937. Among these boundaries, seven—1923/1924, 1925/1926, 1927/1928, 1929/1930, 1931/1932, 1933/1934, and 1935/1936—have no discontinuity in the DRC or another policy. Appendix Table 3 shows summary statistics for these data. Table 5 shows highly significant discontinuities in discounted earnings and participation at the 1916/1917 boundary in the SSA data over the same sets of ages we observe in the IRS data, but we find a significant discontinuity in the IRS data around only one of the seven boundaries (1927/1928).<sup>22</sup> After a Bonferroni correction for multiple comparisons, there is no longer a significant discontinuity at the 1927/1928 boundary, or any other boundary, in the IRS data.<sup>23</sup> Moreover, the discontinuities in the

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<sup>22</sup>Because the IRS data cover 1999 to 2013, these cohorts are observed in the IRS data over a subset of the ages we observe for the 1916/1917 boundary in the SSA data: in the IRS data we observe ages 76 to 90 for the 1923 cohort, ages 75 to 89 for the 1924 cohort, *etc.* To make an apples-to-apples comparison between the IRS data and the SSA data, we investigate the discontinuity in discounted real earnings in the SSA data over the same ages, using the same sample restrictions as the SSA data. For comparability we also cap IRS W-2 earnings at the maximum taxable income level in each year.

<sup>23</sup>When pooling all seven boundaries in the IRS data and defining a dummy for being born after January 1 around any of the

SSA data for the 1916/1917 boundary in these age ranges are jointly significantly different from those in the IRS data at the 1 percent level and always show larger point estimates.<sup>24</sup>

Mandatory schooling policies could imply that first quarter births discontinuously have lower schooling on average than fourth quarter births (Angrist and Krueger 1991), pushing toward a decrease in earnings at the cohort boundary. However, such effects do not appear in our placebo tests around other cohort boundaries or before 1977. Moreover, this factor should push toward lower earnings in the 1917 cohort relative to the 1916 cohort, if anything working against our finding of a large increase in earnings at the cohort boundary.

## 5 Income and Substitution Effects

Having demonstrated that the Notch caused a large increase in earnings at the boundary, we now explore the role of income and substitution effects. We begin with a benchmark lifecycle model, which we use to estimate a bound on the income effect and a Frisch elasticity. Next, motivated by our empirical results, we explore a model in which earnings are determined by contemporaneous unearned income, as would arise from myopia or liquidity constraints.

### 5.1 Initial Lifecycle Framework

As a starting point for grounding the empirical analysis, we review a simple lifecycle model of earnings determination as an illustrative benchmark, adapted from Blundell and MaCurdy (1999). Following Saez (2010) and other papers that have access to administrative data on earnings but not on hours worked, we model individuals as trading off consumption, in which utility is increasing, against pre-tax earnings, in which utility is decreasing (because it requires effort to produce earnings). Assume that each individual  $i$  born on date of birth  $j$  has a quasi-concave utility function that is separable across time from periods  $t$  to  $T$ :

$$U_{ijt} = U_{ij}(U_{ij}^t(C_{ijt}, E_{ijt}, X_{ijt}, Z_{ijt}), U_{ij}^{t+1}(C_{ijt+1}, E_{ijt+1}, X_{ijt+1}, Z_{ijt+1}), \dots, U_{ij}^T(C_{ijT}, E_{ijT}, X_{ijT}, Z_{ijT})) \quad (3)$$

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boundaries, the coefficient on this dummy in the resulting pooled regression is insignificant; see the notes to Table 5 for details. It does not make sense to investigate the 1916/1917 boundary in the IRS data, since in the SSA data the effect on earnings and participation at this boundary turns insignificant by the 1999 to 2013 period covered by the IRS data (Figure 6).

<sup>24</sup>Using SSA data we also investigated those ineligible for Social Security, for example because they do not accumulate sufficient earnings history, around the 1916/1917 boundary. This also shows no significant discontinuity, though these regressions are substantially under-powered because the sample size is reduced by a factor of around 10 (results available upon request).

where  $C_{ijt}$ ,  $E_{ijt}$ ,  $X_{ijt}$ , and  $Z_{ijt}$  are period- $t$  consumption, pre-tax-and-transfer earnings, observable individual attributes that could affect utility, and unobservables that could affect utility, respectively. Utility is maximized subject to the intertemporal budget constraint:

$$A_{ijt+1} = (1 + r_{t+1})[A_{ijt} + B_{ijt} + E_{ijt}(1 + \mu_{ijt} - \tau_{ijt}) - C_{ijt} + R_{ijt}] \quad (4)$$

where  $A_{ijt}$  represents assets in period  $t$ ,  $B_{ijt}$  is OASI benefit income, and  $R_{ijt}$  is other virtual unearned income.<sup>25</sup>  $\tau_{ijt}$  is the total marginal tax rate *absent* the effects of OASI benefits (so including the effects of FICA and personal income taxes, for example), as before  $\mu_{ijt}$  is the marginal increase in lifetime discounted OASI benefits due to a unit increase in earnings in year  $t$ , and  $r_{t+1}$  is the interest rate.<sup>26</sup>

After deriving the first order conditions, earnings supply can be written as:

$$E_{ijt} = L(\lambda_{ijt}, 1 + \mu_{ijt} - \tau_{ijt}, X_{ijt}, Z_{ijt}) \quad (5)$$

where  $\lambda_{ijt}$  represents the year- $t$  marginal utility of wealth. In this framework  $\lambda_{ijt}$  depends on lifetime income, which in turn depends via a function  $f()$  on discounted lifetime OASI benefits  $B_{ijPDV}$  and discounted lifetime wealth from other (*i.e.* non-OASI) sources  $Y_{ijPDV}$  :  $\lambda_{ijt} = f(B_{ijPDV} + Y_{ijPDV}, X_{ijt}, Z_{ijt})$ .

Consistent with the presumption that leisure is a normal good, so that  $\frac{\partial E_{ijt}}{\partial B_{ijPDV}} < 0$  for those with positive earnings, *ceteris paribus* the income effect should push earnings to rise at the boundary (as long as  $X_{ijt}$ ,  $Y_{ijPDV}$ , and  $Z_{ijt}$  on average have an impact on earnings that is continuous at the boundary).<sup>27</sup> Starting in 1979,  $(1 + \mu_{ijt} - \tau_{ijt})$  discontinuously decreases at the boundary and the Slutsky matrix is negative semidefinite, implying  $\frac{\partial E_{ijt}}{\partial (1 + \mu_{ijt} - \tau_{ijt})} \geq 0$ , so *ceteris paribus* the substitution effect should push earnings to (weakly) fall at the boundary.

Under these assumptions, the net effect of the Notch on earnings at the cohort boundary

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<sup>25</sup> Consider a nonlinear tax-and-transfer schedule  $T_{jt}(E_{ijt})$  specifying taxes (net of transfers) as a function of earnings. Following the public finance literature, we can rewrite the budget set in a linearized form:  $C_{ijt} = A_{ijt} - A_{ijt}/(1 + r_{t+1}) + B_{ijt} + Y_{ijt} + E_{ijt} - T_{jt}(E_{ijt}) = A_{ijt} - A_{ijt}/(1 + r_{t+1}) + B_{ijt} + Y_{ijt} + E_{ijt} - E_{ijt}T'_{jt}(E_{ijt}) + E_{ijt}T'_{jt}(E_{ijt}) - T_{jt}(E_{ijt})$ , where  $Y_{ijt}$  is other unearned income. We then define  $R_{ijt} \equiv Y_{ijt} + E_{ijt}T'_{jt}(E_{ijt}) - T_{jt}(E_{ijt})$ , and note that  $T'_{jt}(E_{ijt}) = \tau_{ijt} - \mu_{ijt}$ .

<sup>26</sup>  $\tau_t$  is subtracted from  $\mu_t$ , rather than entering multiplicatively, because OASI benefits are untaxed before 1984 and are rarely subject to taxation in our sample after 1984.

<sup>27</sup> Conditional on other variables there should be a threshold level of benefits  $B_{ijPDV}^*$  above which  $\frac{\partial E_{ijt}}{\partial B_{ijPDV}} = 0$ ; once  $B_{ijPDV}$  reaches a sufficiently high level that someone ceases to have positive earnings, further increases in  $B_{ijPDV}$  should have no effect on earnings. As long as there are some individuals below this threshold, and leisure is a normal good for them so that  $\frac{\partial E_{ijt}}{\partial B_{ijPDV}} < 0$ , *ceteris paribus* average earnings by DOB should increase at the boundary due to the income effect.

is ambiguous, with income and substitution effects that push in opposite directions. Since our empirical results showed that earnings increase discontinuously at the boundary, the income effect must dominate the substitution effect in this context.

Note that this model allows “extensive margin” decisions about whether to earn a positive amount, *i.e.*  $E_{ijt}$  can equal zero if the net marginal returns to earnings are not sufficiently high to justify positive effort. As we show in Appendix A following Eissa, Kleven, and Kreiner (2008), if we allow a fixed cost of participation so that individuals choose between zero earnings and a discrete positive amount,  $E_{ijt}$  will additionally depend on the net “average tax rate”  $1 - AT R_{ijt} = 1 - [T_{jt}(\check{E}_{ijt}) - T_{jt}(0)]/\check{E}_{ijt}$ , where  $\check{E}_{ijt}$  is the individual’s earnings level if s/he chooses to have positive earnings and  $T_{jt}()$  is a general tax-and-transfer schedule defined as a function of earnings in year  $t$ . Thus, with a fixed cost of participation we can write  $E_{ijt} = L(\lambda_{ijt}, 1 + \mu_{ijt} - \tau_{ijt}, 1 - AT R_{ijt}, X_{ijt}, Z_{ijt})$ . As a result, in this model it is still the marginal utility of lifetime wealth and the net incentives (at the intensive and extensive margins) to earn in  $t$  that govern  $E_{ijt}$  (aside from the influences of  $X_{ijt}$  and  $Z_{ijt}$ ). We likewise have  $\frac{\partial E_{ijt}}{\partial (1 - AT R_{ijt})} \geq 0$ , and  $(1 - AT R_{ijt})$  also decreases discontinuously at the boundary starting in 1979, so this extensive margin substitution effect should also push earnings to (weakly) fall at the boundary. As we discuss in the Appendix, all of the results that follow hold in a framework with a fixed cost of participation, except where otherwise noted.

Assuming the discount factor  $\kappa$  is equal to  $1/(1 + r_{t+1})$ , and if there are no shocks to lifetime wealth, then the Euler equation implies we have a constant  $\lambda$  for all  $t$  (Blundell and MaCurdy 1999). When any of the exogenous variables is uncertain, the Euler equation is modified to account for uncertainty:  $\lambda_{ijt} = \kappa E_t[\lambda_{ijt+1}(1 + r_{t+1})]$  (MaCurdy 1985). The only substantive difference between the cases with certainty and uncertainty is that the marginal utility of wealth is a random variable that is realized only at the start of each period. In our context, a common shock to both the 1916 and 1917 cohorts in a given time period  $t$  should be removed by the RDD setup comparing similar individuals in each period.

In most parameterizations of this lifecycle model, the effect on yearly earnings should be larger when an unanticipated cut in benefits occurs closer to retirement rather than earlier in life (Imbens *et al.* 2001; Mastrobuoni 2009). The intuition is that when a change in benefits

is anticipated further in advance, in most parameterizations the individual can react by changing consumption over a longer period rather than changing earnings as much. When an unanticipated change in benefits occurs close to retirement, the individual has less time for consumption to react, and therefore adjusts earnings more. In this light, our results would be most applicable to evaluating the effects of unanticipated cuts in benefits that occur close to retirement age.

Next, we use this simple framework to estimate a lower bound on the income effect. By a “lower bound” on the income effect, we refer to a lower bound on the *absolute value* of the income effect (which is itself negative).

## 5.2 Lower Bound on the Income Effect

Since *ceteris paribus* the substitution effect should unambiguously push earnings to fall at the boundary beginning in 1979, we can estimate a lower bound on the income effect by running a two-stage least squares (2SLS) regression in which we use the notch dummy to instrument for benefits. As a starting point we use the more parsimonious model without a fixed cost of participation. We divide  $Z_{ijt}$  into two components,  $Z_{ijt} = (Z'_{ij}, Z'_t)$ , where  $Z'_{ij}$  varies by individual but not over time, and  $Z'_t$  varies over time but not by individual. We assume that  $Z'_{ij}$  is initially drawn randomly, so that realized earnings unconditional on  $Z'_{ij}$  are stochastic.<sup>28</sup> To make progress on estimation, we assume that expected earnings unconditional on  $Z_{ijt}$ ,  $\mathbb{E}(E_{ijt}|B_{ijPDV}, Y_{ijPDV}, 1 + \mu_{ijt} - \tau_{ijt}, X_{ijt})$ , can be expressed in this additive form that is linear in  $B_{ijPDV} + Y_{ijPDV}$ :

$$\mathbb{E}(E_{ijt}) = \alpha(B_{ijPDV} + Y_{ijPDV}) + g(1 + \mu_{ijt} - \tau_{ijt}) + h(X_{ijt}) \quad (6)$$

where  $g()$  relates earnings to substitution incentives (and  $g'() \geq 0$ ), and  $h()$  relates earnings to observable characteristics  $X_{ijt}$ .<sup>29</sup> For ease of notation we suppress the conditioning variables,

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<sup>28</sup>  $Z'_{ij}$  is stochastic but is drawn initially, so that given the realization of  $Z'_{ij}$ , individuals make choices under certainty. Technically, we partition  $Z_{ijt}$  into  $(Z'_{ij}, Z'_t)$  because the initial framework is specified under certainty, so we let  $Z'_t$  evolve deterministically over time rather than choosing it stochastically in each period. As noted, it is straightforward to incorporate uncertainty in all variables into the framework (results available upon request), including letting  $Z_{ijt}$  vary stochastically over time for each individual in an general way.

<sup>29</sup> We have linearized the budget constraint as in Gruber and Saez (2002) and other literature, abstracting from possible nonlinearities elsewhere in the budget constraint. Note also that in our model without fixed costs, participation can be written as a function of the same variables,  $D_{ijt} = L(\lambda_{ijt}, 1 + \mu_{ijt} - \tau_{ijt}, X_{ijt}, Z_{ijt})$ , where  $D_{ijt}$  is a dummy for positive earnings, leading to a linear probability model as a function of the same variables,  $\mathbb{E}(D_{ijt}) = \varsigma(B_{ijPDV} + Y_{ijPDV}) + \zeta(1 + \mu_{ijt} - \tau_{ijt}) + \varrho(X_{ijt})$ . As we show, a model involving the log odds of the participation rate yields similar results.

so that  $\mathbb{E}(E_{ijt})$  refers to  $\mathbb{E}(E_{ijt}|B_{ijPDV}, Y_{ijPDV}, 1+\mu_{ijt}-\tau_{ijt}, X_{ijt})$ . To estimate a lower bound on  $\beta$ , we set  $g() = 0$  for illustrative purposes. We then take means of (6) at the DOB level  $j$  to estimate conservative standard errors as above.<sup>30</sup> We assume that  $X$  affects earnings and OASI benefits through  $h()$  in a way that is on average smooth across DOBs, that  $Y_{jPDV}$  is smooth across DOBs, and that these influences can be captured through a linear spline, as in (1) and (2) above. Under these assumptions we can estimate the income effect of OASI benefits on earnings through a 2SLS model in which equation (2) is the first stage, and the second stage is:

$$E_{jPDV} = \alpha_1 B_{jPDV} + \alpha_2 DOB_j + \alpha_3 (D * DOB)_j + \eta_j \quad (7)$$

We interpret  $\alpha_1$  as a lower bound on the local average treatment effect of discounted OASI benefits on earnings (*i.e.* local to those at the boundary). Earnings respond to the shock to lifetime income as soon as it is reflected in the marginal utility of wealth, when the reform is anticipated. For an agent who understands the reform, it should have an effect beginning in 1978 (or in 1977 if it is anticipated then), which justifies examining discounted earnings from 1978 to 2012.

Table 6 shows the 2SLS estimates. In the baseline specification in Column 1, an increase in discounted lifetime OASI benefits of \$1 is associated with a decrease in mean discounted lifetime earnings of 61 cents. This is the central finding of the paper: since the lower bound on the income effect on earnings is very large, we conclude that the income effect must be very large. We find that a \$10,000 increase in lifetime discounted benefits causes a decrease of 0.65 percentage points in the mean yearly participation probability from 1978 to 2012. Evaluating elasticities at the means of the relevant variables, these estimates imply an elasticity of lifetime discounted earnings with respect to lifetime discounted benefits of 0.86, and an elasticity of the participation rate with respect to lifetime discounted benefits of 0.70. The estimates are very similar with controls.<sup>31</sup>

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<sup>30</sup>Taking means at the DOB level also will result in regressions that are weighted differently than those at the individual level. We argue below that the DOB mean level is more useful for policy purposes, and we extensively describe the individual-level estimates in Appendix D.

<sup>31</sup>At other cohort boundaries from 1917-1921, the first-stage discontinuities in benefits are lower, so we have much less power. When we pool data from the 1917/1918 to 1921/1922 cohort boundaries, the crowdout point estimates continue to be large, though less precisely estimated (given the small first stage at each of these boundaries): the coefficient on OASI benefits indicates that a \$1 increase in benefits leads to an 78-cent decrease in earnings, but this is insignificant at conventional levels (standard error 56 cents).

Different groups could show different-sized effects. Table 7 shows that the lower bounds for the income effects on earnings and participation are somewhat larger among women, though insignificantly so. Among those whose average earnings prior to 1977 were below the median, the point estimate is larger relative to the above-median group, but these estimates are insignificantly different from one another (and the estimate is marginally significant in the low prior earnings group).

The earnings crowdout estimate is substantially larger than the estimates of the marginal propensity to earn among lottery players in Imbens, Rubin, and Sacerdote (2001) or Cesarini, Lindqvist, Notowidigdo, and Östling (2015), including estimates among the elderly or nearly-elderly in particular. At the same time, our results are not inconsistent with these studies, as there are important differences between our sample and those examined in these studies—*e.g.* those studies are of individuals who play the lottery in particular countries and time periods, whereas we examine those subject to the Notch policy change.

### 5.2.1 Robustness Checks

As we discuss in Appendix B, our results are similar under other choices, including varying the discount rate (Appendix Table 4), investigating the results in 1979 to 2012 aggregated (Appendix Table 6), and excluding birthdays on and near January 1-2, 1917 (Appendix Table 6). In Appendix C, we describe an alternative empirical strategy that relies on “simulated” benefits as the endogenous variable rather than actual benefits (as in Gruber and Saez 2002), and we discuss how and why this yields estimates in Appendix Table 5 that are nearly identical to our baseline.

In principle, it would be possible to use an alternative empirical strategy, in which we effectively compared an individual’s incentives to the individual’s earnings decision. This would produce estimates that were weighted differently than those at the DOB-mean level: as in a standard differences-in-differences framework, if the income effect  $\beta_1$  is heterogeneous across individuals and the size of the cut in benefits due to the Notch is correlated across individuals with the income effect, this model would estimate a weighted average of the income effects in the population, with greater weight on individuals with larger cuts in benefits. By contrast, our DOB-mean-level estimates are policy-relevant and form our baseline



because they effectively use weights that reflect the causal effect of the Notch on earnings for the population at the boundary, which is relevant to analyzing the actual effects of the policy on aggregate earnings in this population. In Appendix D and Appendix Table 7, we present the results of an individual-level strategy, which show comparable magnitudes to our DOB-mean-level results.

### 5.3 Frisch Earnings Elasticity

Having estimated a lower bound on the income effect, we turn to estimating an intertemporal substitution elasticity, which holds the marginal utility of wealth constant. Following Chetty, Guren, Manoli, and Weber (2013), we call this a “Frisch” elasticity, but we recognize that extensive margin Frisch elasticities are technically not defined because non-participants do not locate at an interior optimum. We interpret our Frisch elasticity as reflecting a combination of intensive and extensive margin responses, holding the marginal utility of wealth constant. In a Frisch framework, it is necessary to assume additive separability of utility across periods in (3) (Blundell and MaCurdy 1999). To make progress on estimating this Frisch elasticity, we linearize the earnings supply function (6) further by specifying earnings as a linear function of the substitution incentives and personal characteristics (*i.e.* setting  $g(1 + \mu_{ijt} - \tau_{ijt}) = \delta(1 + \mu_{ijt} - \tau_{ijt})$  and  $h(X_{ijt}) = \theta X_{ijt}$ ):

$$\mathbb{E}(E_{ijt}) = \beta \lambda_{ijt} + \delta(1 + \mu_{ijt} - \tau_{ijt}) + \theta X_{ijt} \quad (8)$$

Here  $\beta$  reflects an income effect, and  $\delta$  reflects a substitution effect. Given (8), we have:

$$\mathbb{E}(E_{ij1978}) = \beta \lambda_{ij1978} + \delta(1 + \mu_{ij1978} - \tau_{ij1978}) + \theta X_{ij1978} \quad (9)$$

$$\mathbb{E}(E_{ij1979}) = \beta \lambda_{ij1979} + \delta(1 + \mu_{ij1979} - \tau_{ij1979}) + \theta X_{ij1979} \quad (10)$$

To make further progress, we can assume that in 1978 and 1979, on average at the DOB-mean level  $j$  marginal tax rates and other factors are approximately constant, *i.e.*  $\tau_{j1978} \approx \tau_{j1979}$  and  $X_{j1978} \approx X_{j1979}$ . Since a change in the marginal utility of wealth  $\lambda$  from the expected benefit cuts of the 1977 amendments should be realized immediately when the policy change was anticipated, and given that the discount factor  $\kappa = 1/(1 + r_{t+1})$ , to make progress we can also assume that  $\lambda_{j1978} \approx \lambda_{j1979}$ . Meanwhile, the substitution incentive

should only affect earnings beginning in 1979, the calendar year when the 1917 cohort turns 62. Thus, taking means at the DOB level  $j$  and subtracting (9) from (10), we have:

$$\mathbb{E}(E_{j1979} - E_{j1978}) = \delta(\mu_{j1979} - \mu_{j1978}) \quad (11)$$

Due to a substitution effect, mean earnings in 1979 minus mean earnings in 1978 for each date of birth  $j$ ,  $\Delta E_j \equiv \mathbb{E}(E_{j1979} - E_{j1978})$ , should decrease discontinuously at the boundary due to the discontinuous decrease in  $\Delta\mu_j \equiv \mu_{j1979} - \mu_{j1978}$ . With even a moderate substitution elasticity, the fall in  $\Delta E_j$  at the boundary should be very large. For illustrative purposes, assume a substitution elasticity of 0.25. Thus, the fall in average earnings at the boundary should be 5.3 percent ( $= 0.25$  multiplied by the 21 percent decrease in the substitution incentive) of baseline earnings, or a \$905 decrease in yearly earnings—far larger than the discontinuity in mean yearly earnings over 1978-2012, which is clearly observable in the data (Appendix Table 2).<sup>32</sup>

We can run a regression to determine the magnitude of any discontinuity in  $\Delta E_j$ :

$$\Delta E_j = \kappa D_j + \kappa_1 DOB_j + \kappa_2 (D * DOB)_j + \pi_j \quad (12)$$

This is a “difference-in-discontinuities” empirical framework (Card and Shore-Sheppard 2004): our identification strategy compares the magnitude of the earnings discontinuity in 1978, when the substitution incentive was continuous at the boundary, to the magnitude of the earnings discontinuity in 1979, a closely comparable year when the substitution incentive was discontinuous at the boundary. Figure 8 shows that no discontinuity in  $\Delta E_j$  appears in the data. Table 8 confirms that the change is insignificant, and the confidence interval rules out more than a small decrease in earnings.<sup>33</sup> To address the fact that mean earnings are

<sup>32</sup> “Baseline” earnings are mean earnings in 1979 of those born in 1916 cohort within 100 days of the boundary. As we might expect, the discontinuity at the boundary in  $1 + \mu_{j1979} - \tau_{j1979}$ , 21 percent, is the same as the discontinuity in  $\Delta\mu_{j1979}$  as a percentage of the mean of  $1 + \mu_{j1979} - \tau_{j1979}$  in the baseline 1916 cohort. In a more recent context and relying on different variation, Liebman, Luttmer, and Seif (2009) find that beneficiaries respond to substitution incentives created by variation in their benefits.

<sup>33</sup> Although the level of  $\Delta E_j$  appears continuous at the boundary, in Figure 8 its slope appears to increase at the boundary. Since individuals on average greatly reduce their earnings at the time of the year when they reach the OASI EEA at age 62, we would expect exactly this pattern. Those born in 1916 turn age 62 in 1978 and turn 63 in 1979, whereas those born in 1917 turn age 61 in 1978 and 62 in 1979. Thus, those born in 1917 should greatly reduce their earnings in the month they turn 62, and those born later in 1917 are age 62 for a smaller fraction of 1979 than those born earlier in 1979—leading to a steep upward slope in  $\Delta E_j$  among those born in 1917, but not among those born in 1916. This notable change in the slope of  $\Delta E_j$ , where  $\Delta E_j$  is calculated from the difference between earnings at ages 62 and 61 for the cohort to the right of the boundary, is visible around *every* cohort boundary; these results are available upon request. Although this mechanism causes a change in *slope* at the boundary, it should not cause a change in the *level* of  $\Delta E_j$  at the boundary and thus does not threaten our

different in 1978 and 1979, Appendix D and Appendix Figure 9 show there is no evidence for a decrease in the (approximate) percentage change in earnings from 1978 to 1979.

We can use these estimates to calculate a Frisch elasticity. Specifically, we additionally estimate the magnitude of the discontinuity in substitution incentives  $\Delta\mu_j$  at the boundary:

$$\Delta\mu_j = \phi D_j + \phi_1 DOB_j + \phi_2 (D * DOB)_j + \zeta_j \quad (13)$$

If we estimate  $\hat{\varkappa}$  from (12) and  $\hat{\phi}$  from (13), then we can estimate the substitution effect as  $\hat{\delta} = \hat{\varkappa}/\hat{\phi}$ . Evaluated at the mean level of earnings and the substitution incentive in our 1916 cohort data in 1978,  $\bar{E}$  and  $1 + \bar{\mu} - \bar{\tau}$ , respectively, the estimated Frisch elasticity  $\hat{\varepsilon}^F$  is given by:<sup>34</sup>

$$\hat{\varepsilon}^F = \hat{\delta} \frac{1 + \bar{\mu} - \bar{\tau}}{\bar{E}} = \frac{\hat{\varkappa}}{\hat{\phi}} \frac{1 + \bar{\mu} - \bar{\tau}}{\bar{E}} \quad (14)$$

We calculate standard errors using the delta method. Table 8 shows that the point estimate of  $\hat{\varepsilon}^F$  is small, negative, and insignificantly different from zero. The upper bound of the 95 percent confidence interval on  $\hat{\varepsilon}^F$  rules out an elasticity greater than only 0.010.<sup>35</sup> In Appendix D and Appendix Table 8 we find similar results when using (approximate) percentage changes in earnings from 1978 to 1979 rather than the levels, or when using simulated substitution incentives rather than actual incentives.

Substitution incentives could also affect the choice of whether to earn a positive amount. Figure 8 shows no visible or statistically significant discontinuity in the change in the participation rate or the log odds of participation. Table 8 shows that the point estimate of the implied participation elasticity with respect to the substitution incentive ( $1 + \mu_{ijt} - \tau_{ijt}$ ) is negligible, and the confidence interval rules out an elasticity greater than 0.048. We also estimate an insignificant substitution elasticity when we add the relevant terms to the individual-level model discussed in Appendix D.<sup>36</sup>

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identification strategy. Appendix Figure 9 shows that the change in slope is much less pronounced when specifying the outcome in (approximate) percentage changes from 1978 to 1979, rather than in levels.

<sup>34</sup>The estimated elasticities are similar when evaluating them at other similarly-defined earnings levels.

<sup>35</sup>Although it illustrates certain key forces determining earnings, the benchmark lifecycle model does not consider the option value of work (Stock and Wise 1990), which could affect the returns to additional earnings. The finding that there is no discontinuity in  $\Delta E_j$  at the cohort boundary is robust, and we can consider the resulting numerical elasticity estimates to be illustrative. The value of the option to continue working also falls at cohort boundary, so if anything our method could underestimate the discontinuity in the substitution incentive and thus over-estimate the substitution elasticity; this reinforces our conclusion that the substitution elasticity is small. It would be necessary to have information about future earnings probabilities to estimate the Stock and Wise model formally.

<sup>36</sup>In each of the groups in which we investigated heterogeneity (men *vs.* women and below- *vs.* above-median pre-1977

If individuals face a fixed cost of earning a positive amount, we would not be able to use this strategy to estimate an elasticity of earnings, because we have only one moment (the change in earnings from 1978 to 1979) but would like to estimate two parameters, the elasticity of earnings with respect to both the marginal and average net-of-tax rates. However, in this case we could still estimate an elasticity of participation with respect to the average net-of-tax rate, because the participation probability should depend on the average net-of-tax rate but should not depend at the margin on the marginal net-of-tax rate (see the discussion in the Appendix). Appendix Table 8 shows similar results when we estimate the participation elasticity with respect to  $(1 - ATR_{ijt})$ .

Our results do not imply that the Frisch elasticity is near zero in all contexts, or in all retirement contexts. For example, our estimated elasticity may not reflect the elasticity that would obtain if individuals were fully aware of the substitution incentives (Chetty, Looney, and Kroft 2009). In this sense, we interpret our estimated substitution as “observed” rather than “structural” elasticities, in the terminology of Chetty (2012).

Theory implies that the Frisch elasticity must be zero or greater; our point estimate of the earnings substitution elasticity is close to zero, insignificant, and negative, so our preferred estimate of the substitution elasticity is zero. Thus, our preferred estimate of the income effect on earnings is the same as the lower bound estimate in Table 6. To estimate an upper bound on the income effect we could instead calculate income effects assuming that the Frisch elasticity is 0.010, the upper bound on the baseline 95 percent confidence interval. If this were hypothetically the elasticity at all times, then in the baseline the implied income effect of \$1 of OASI benefits on earnings would be -\$0.64, and the implied income effect of \$10,000 in benefits on the yearly participation rate would be -0.69 percentage points. These upper bounds are only slightly greater in absolute value than the lower bounds.<sup>37</sup>

We compare the discontinuity in 1979 and 1978 because these are temporally the closest years that faced sharply different incentives. As discussed in Appendix D, investigating

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earnings), we also find precise zero substitution elasticities. The point estimates and confidence intervals are also similar when we use other discount rates.

<sup>37</sup>If the actual substitution incentive in year  $t$  is  $(1 + \mu_{ijt} - \tau_{ijt})$ , then earnings  $E'_{ijt}$  that the individual would have had given a hypothetical  $\mu'_{ijt} = 0$  (and holding  $\lambda_{ijt}$  constant) is given by  $E'_{ijt} = E_{ijt}[1 - \varepsilon^F \mu_{ijt}/(1 + \mu_{ijt} - \tau_{ijt})]$ . We calculate the presented discounted value of  $E'_{ijt}$ , which we define as  $E'_{ijPDV}$ , and its mean by DOB,  $E'_{jPDV}$ . If we then use  $E'_{jPDV}$  as the dependent variable in equation (7) in our 2SLS model, the coefficient  $\alpha_1$  will reflect the income effect under the “upper bound” assumption that the Frisch elasticity is  $\varepsilon^F = 0.010$ .

adjacent years allows us to hold other exogenous factors constant in the most credible way. Two assumptions underlying this strategy appear to be consistent with the evidence. First, looking for a response in 1979 presumes that individuals were able to react to the incentives within the year. With such a large change in substitution incentives (21 percent) that should cause a visible reaction in the data even with a small substitution elasticity, one might expect that some measurable response would appear in the first year even with a small elasticity. Furthermore, Gelber, Jones, and Sacks (2013) find that earnings react almost fully to large policy changes in the OASI Earnings Test within the year that the policy change occurs. Moreover, Figure 6 shows that the magnitudes of the earnings and participation discontinuities grow through the early-to-mid 1980s, which is the opposite of what we might predict if there were a reaction to the substitution incentives that grew over this period (which would instead push toward declining discontinuities). That said, our estimates apply most directly to an immediate reaction to such substitution incentives. Second, as described in Appendix E, unanticipated inflation from 1978 to 1979 could have affected earnings decisions. However, Appendix E explains that the evidence appears at odds with the hypothesis that inflation mattered greatly for the crowdout estimates. Moreover, neither inflation expectations nor these substitution incentives should influence behavior if earnings decisions are made in a static framework where individuals are myopic, as we investigate next.

## 5.4 Framework with Myopia or Liquidity Constraints

The lifecycle framework makes a number of assumptions, including that individuals are forward-looking and can freely transfer capital across periods. This framework can be modified to assume that individuals effectively face a static decision in each period, due to myopia or constraints on transferring capital across periods like liquidity constraints. In our context, in which individuals are near retirement age, typically have substantial assets, and typically should not want to borrow since their incomes are usually falling thereafter, it would arguably be surprising to find large liquidity effects, but myopia has been proposed as an important phenomenon in retirement (*e.g.* Diamond and Köszegi 2003, Kaplow 2015).

Our empirical results support the hypothesis that individuals act as if the earnings deci-

sion is static. In particular, in 1978 to 1980, a period after policy enactment when individuals faced only a small average discontinuity in benefits (Figure 9), the earnings discontinuity is insignificant in Figure 6. Appendix Table 2 shows that the earnings discontinuity is also insignificant in 1977 and 1978, before those in the 1917 cohort reached the EEA.<sup>38</sup> The absolute value of the discontinuities in both benefits and earnings rise to a maximum in 1984-1986, before both falling at older ages when a smaller fraction of the population remains alive. The myopia hypothesis is also consistent with the lack of a measured substitution effect, as the substitution incentive operated through an effect of current earnings on future benefits. (To be clear, the negligible substitution elasticity could also be due to incentives that are not salient—though lack of salience could itself occur because the substitution incentive operates through effects on future rather than current benefits—or simply due to low responsiveness even with fully rational individuals.)

In a linearized version of the static specification in Blundell and MaCurdy (1999), expected earnings  $\mathbb{E}(E_{ijt})$  at time  $t$  can be written as a function of the net returns to work  $(1 + \mu_{ijt} - \tau_{ijt})$  in that period, unearned income  $B_{ijt} + Y_{ijt}$ , and other factors  $X_{ijt}$ :

$$\mathbb{E}(E_{ijt}) = \alpha(B_{ijt} + Y_{ijt}) + \beta(1 + \mu_{ijt} - \tau_{ijt}) + \gamma X_{ijt} \quad (15)$$

Assuming that  $\tau_{jt}$ ,  $X_{ijt}$ , and  $Y_{jt}$  are continuous through the boundary, we can estimate a corresponding empirical model in which average earnings in each year  $t$  are related to average benefits in  $t$  as in (15), by estimating this 2SLS regression separately in each period  $t$ , again using means by DOB  $j$ :

$$B_{jt} = \xi_1 D_j + \xi_2 DOB_j + \xi_3 (D * DOB)_j + \rho_{jt} \quad (16)$$

$$E_{jt} = \vartheta_1 D_j + \vartheta_2 DOB_j + \vartheta_3 (D * DOB)_j + \varsigma_{jt} \quad (17)$$

Alternatively, we can pool data across years and estimate the following 2SLS specification:

$$B_{jt} = \varpi_1 D_j + (DOB * I)_{jt} \varpi_2 + (D * DOB * I)_{jt} \varpi_3 + I_t \varpi + \nu_{jt} \quad (18)$$

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<sup>38</sup>Those born in December of 1916 (as well as those born January 1, 1917) were eligible to collect benefits in December of 1978. Thus, there is a very small discontinuity at the boundary in benefits received in 1978 of only \$38. Even given crowding out in the range of 50 cents per dollar of OASI benefits, this should cause a discontinuity in earnings of only around \$19, *i.e.* a negligible effect that our regressions would be underpowered to detect.

$$E_{jt} = \chi_1 D_j + (DOB * I)_{jt} \chi_2 + (D * DOB * I)_{jt} \chi_3 + I_t \chi + v_{jt} \quad (19)$$

where  $I_t$  represents a vector of calendar year dummies,  $(DOB * I)_{jt}$  reflects a vector of separate DOB trends in each year, and  $(D * DOB * I)_{jt}$  reflects a vector of separate DOB trends above the boundary in each year. We interpret the coefficient  $\chi_1$  as a lower bound on the effect of yearly benefits on contemporaneous yearly earnings, in this static framework.

Table 9 shows the results with the specification (18)-(19). In the baseline linear specification without controls where the actual OASI benefit is the endogenous variable and earnings is the dependent variable, the coefficient on yearly OASI benefits is -0.46, indicating that a \$1 increase in yearly OASI benefits causes a decrease in yearly earnings of 46 cents. The coefficient on OASI benefits is around one-third smaller than the coefficient of -0.61 in the lifecycle model without liquidity effects, which is not surprising since earnings are much more front-loaded than benefits within the elderly years. Using model (16)-(17) for each time period separately, Figure 10 shows that the point estimates of earnings and participation crowdout generally fall over time.<sup>39</sup> An individual-level regression model shows comparable results.

Overall, our evidence is consistent with this static model, but we do not have dispositive evidence for such a model. The point estimate of the effect in 1978, or in 1978-1980, is positive and not far from significant. The pattern of earnings discontinuities over time mirrors the pattern of benefits discontinuities, but in principle in the lifecycle model there could be heterogeneity in crowdout across ages or time periods that happens to follow the same time pattern as the benefits discontinuity. Although on balance the static model captures the data best, we also extensively explore the lifecycle model as it is still a useful benchmark. Ultimately, crowdout is large in both models, and thus our conclusion is that crowdout is large.

## 5.5 Spousal Benefits and Interpretation of Crowdout Estimates

Because wives' OASI benefits are effectively determined by the husband's earnings record when wives are secondary or dual-entitled beneficiaries, this has implications for how to

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<sup>39</sup>Benefits received in 1979 show only a negligible upward discontinuity of \$38 at the boundary. Since benefits received in 1978 show a negligible downward discontinuity of \$38, the discontinuity in the change in benefits received from 1978 to 1979 is only \$76. Thus, if individuals respond to contemporaneous benefits as in (15), our strategy for identifying the Frisch elasticity would be materially unaffected since the change in benefits received from 1978 to 1979 is essentially continuous at the boundary.

interpret our crowdout estimates. The measure of OASI benefits used above is calculated *only* for the recipient, but for those husbands whose wives are secondary or dual-entitled beneficiaries, the discontinuity in the couple’s total benefits is 150 percent as large as the discontinuity in the husband’s benefit alone (since the wife’s total benefit in these couples is 50 percent as large as the husband’s benefit). Thus, the first stage for husbands above corresponds to an extreme case in which husbands effectively act separately from their wives and husbands’ earnings are not influenced by their wives’ OASI benefits. An alternative assumption is a “unitary” model of the family (Becker 1976), in which the family acts as if it maximizes a single utility function and therefore pools the unearned income of both spouses. In this case, the first-stage discontinuity in benefits for husbands would be 30 percent larger (due to the influence of around 60 percent of wives claiming as a dual-entitled or secondary beneficiaries), and the first stage discontinuity for the full population would be 15 percent larger; this would deflate the corresponding 2SLS estimates by 30 and 15 percent, respectively.<sup>40</sup>

## 6 Relationship to Krueger and Pischke (1992)

We return to compare our work with KP (1992), to investigate the seeming divergence of our results among their study population consisting only of older men. KP find insignificant effects and rule out that the Notch has more than a moderate impact on male labor force participation, estimating an increase in the log odds of participation of no more than 0.059 in a representative specification, and they do not estimate impacts on earnings. We indeed estimate only a moderate impact of the Notch on male participation. Examining ages 60 to 68 as KP do, our RDD results for the 1916/1917 boundary show an elasticity of men’s log odds of participation with respect to lifetime discounted benefits of -0.66 (standard error 0.21), which is insignificantly different from their estimated elasticity of 0.105 (standard error 0.265,  $p > 0.10$ ). In this sense, our findings are compatible with KP’s.

Moreover, we have discovered no mistake in KP’s analysis, and indeed we are able to replicate it when we use their specifications. As we discuss in detail in Appendix F, in Appendix Table 9 we show that with specifications parallel to theirs, *i.e.* using our SSA data

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<sup>40</sup>If households are not unitary, for example in a “collective” model of household bargaining (*e.g.* Chiappori 1992), then payments to a beneficiary’s spouse could have a smaller effect on the beneficiary than payments to the beneficiary.



on men aggregated to the birth cohort-calendar year level, we obtain empirical results similar to theirs. This suggests that a primary reason for the difference in results is the difference in the level of variation examined (*i.e.* daily *vs.* yearly).<sup>41</sup> In the cohorts and years considered in KP, the variation across days of birth in the participation rate is substantial relative to the variation induced by the benefit discontinuity (Handwerker 2011). The cross-cohort empirical design is informative in ruling out more than a moderate impact on participation. At the same time, our results take advantage of the statistical power of our full population data by day of birth.

## 7 Conclusion

How pension income affects elderly work is a key question for policy and for understanding work patterns in this population. Our most important new finding is that we estimate very large income effects of OASI. In a lifecycle specification, the lower bound point estimate of the income effect shows that a \$1 increase in discounted lifetime OASI benefits causes an increase in lifetime discounted earnings of 61 cents. The earnings patterns are consistent with the hypothesis that individuals respond to current (not future) benefits, and with this specification the lower bound point estimate shows that a \$1 increase in OASI benefits causes a contemporaneous decrease in earnings of 46 cents. Under further assumptions we rule out a substitution elasticity greater than only 0.010.

The pairing of very large income effects with negligible substitution effects in our context stands in contrast to previous literature finding that substitution effects are important but that income effects generally are not very large. Our findings illustrate that in contexts in which substitution incentives may not be fully transparent and/or operate through effects on future income—which are common in pensions—the reaction may be small. By contrast, both in the Notch context and in other contexts, income effects could naturally arise because the elderly plausibly have a good sense of their income. Finding large income effects in the late 20th century suggests that in our setting, the large income effects estimated by Costa (1995, 2010) or Fetter and Lockwood (2016) persisted substantially later in U.S. history.

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<sup>41</sup> As a secondary analysis, Snyder and Evans (2006) also use the CPS to investigate the effect of the Notch on the probability of working, but they do not examine earnings crowdout, which is our primary contribution. We discuss the relationship between our work and theirs in Appendix F.

The distinction between income and substitution effects is important in part because only substitution effects are associated with distortions in a standard public finance setting (in the absence of a pre-existing distortion), as income effects simply reflect transfers of resources rather than changes in relative prices. If applicable more broadly, our finding of large income effects could imply that an important part of the effect of pensions is not associated with large distortions. Of course, OASI is financed through taxation, which can cause deadweight loss through this separate channel. A full analysis of the implications of our results for a welfare analysis is beyond the scope of this empirical paper. Such a welfare analysis could require specifying other assumptions, such as imperfections (or lack thereof) in the private annuities market, and it would be a fruitful topic for future research.

Like other empirical work that estimates local effects, our results apply locally to individuals born in 1916 and 1917 in the period after the Notch legislation. Our estimates are most pertinent to contexts with an unanticipated change in OASI benefits experienced close to retirement age, relevant to policy-makers interested in the effects of such changes along the transition path to a new steady-state OASI system. If individuals respond to contemporaneous benefits as our findings suggest, then our results could also apply whenever individuals receive benefit cuts (whether these are anticipated or not).

If applicable more broadly, our estimates may be useful to policy-makers interested in simulating the earnings effects of policy reforms, for example as the projected 2034 exhaustion of the OASDI Trust Fund nears. The cuts in OASI benefits sustained by the Notch cohorts are the same order of magnitude as many OASI policy changes contemplated today. For example, a one-year unanticipated increase in the Normal Retirement Age would cut benefits by 6.67% for all recipients (absent a behavioral response); for a recipient with discounted OASI wealth of \$85,901 (the mean in our sample), our lifecycle income effect estimates imply that this policy would cause an increase in lifetime discounted earnings of \$3,458. For an average retired worker beneficiary receiving \$1,363.30 in monthly benefits (SSA 2015b), our static model estimates imply that a one-year increase in the Normal Retirement Age would cause a decrease in average annual earnings of \$502 beginning at age 62.

Our estimates also have implications for the fiscal consequences of change in OASI benefits. Assuming for illustration that earnings are taxed at a 25 percent marginal rate on

average in this population (including both federal payroll and income taxes), and assuming that OASI benefits are untaxed (as is typical), a \$1 decrease in OASI benefits would decrease the unified federal government deficit by around \$1.12 according to our static model (and would reduce the present value of its debt by around \$1.15 in the lifecycle estimates), net of the resulting increase in federal income and payroll tax revenue. From a \$1 decrease in OASI benefits, the OASDI Trust Fund alone would reduce its deficit by \$1.06 in our static model. Since the present value of the OASDI Trust Fund’s 75-year unfunded obligation is \$10.7 trillion (SSA 2015a), these results imply that the benefit cuts necessary to eliminate OASDI’s 75-year actuarial deficit are around \$577 billion smaller in present value than under a calculation that did not take behavioral effects into account.

Likewise, our results may have implications for understanding the evolution of elderly employment rates in the latter half of the 20th century. As noted in the Introduction, the elderly employment rate declined greatly from 1950 to the mid-1980s, but in a striking turnaround, it has risen since the mid-1980s (Figure 1). We probe the initial roots of this turnaround in the mid-1980s, recognizing that Social Security was just one of many factors that could have affected the elderly employment rate (Munnell, Cahill and Jivan 2003; Schirle 2008; Blau and Goodstein 2010). Specifically, we calculate the fraction of the turnaround that the decrease in the growth rate of OASI benefits in the 1980s can account for. The timing of the turnaround in the mid-1980s matches well with the years when we find the biggest effects on participation, *i.e.* 1981 to 1989. The mid-1980s occur a few years after when the Notch legislation occurred (1977), but the 1917 cohort reached age 65 and thus became included in the elderly group shown in the figure only in 1982.<sup>42</sup>

As we describe in Appendix G, in an illustrative calculation we find that the slowdown in the growth rate of OASI benefits can account for 28.0 percent of the actual change in the participation rate growth rate around 1985 in the static model, and 37.2 percent in the lifecycle model. Focusing only on the 65-69 year-old population that our estimates suggest was most strongly affected by the policy change, and using our RDD estimates for this group in Appendix Table 2, we find that the change in the growth rate of benefits can account for

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<sup>42</sup>As mean OASI benefits grew in absolute terms after the mid-1980s, it must be the case that other, unrelated factors led to the increase in the absolute level of employment in this period. The change in benefit growth can provide a partial explanation for the change in slope, though other factors have played important independent roles in determining elderly employment rates.

56.5 percent of the change in the growth rate of the employment rate for this group in the static model, and 74.9 percent in the lifecycle model. If earnings are determined by benefits contemporaneously received, consistent with our findings, then our results could be relevant to understanding the effects of changes in OASI benefits that were anticipated longer before retirement age, including other OASI policy changes through the second half of the 20th century. Thus, using analogous methods we find that the increase in OASI benefit levels can account for 57.6 percent of the decrease in elderly participation from 1950 to 1985, helping to explain much of the striking time series association between elderly employment and Social Security benefits over this period.<sup>43</sup> In the 65-69 year-old group, the increase in OASI benefit levels can account for 76.7 percent of the decrease in elderly participation from 1962 to 1985. As one might anticipate in moving from micro-economic estimates to implications for macro-economic time series, these calculations are subject to several important caveats discussed in the Appendix, including the potential for general equilibrium effects and spousal interactions; however, we view our calculations as illustrative of the order of magnitude of the implications of rising OASI benefits over time, which appears to be large.

Men and women show different trends in employment over the second half of the 20th century, and our results show different effects on men's and women's decisions. In future work, it would be illuminating to explore men's and women's separate responses further.

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<sup>43</sup>See also Boskin (1977), Moffitt (1987), KP (1992), Feldstein and Liebman (2002), Kreuger and Meyer (2002), or Blau and Goodstein (2010).

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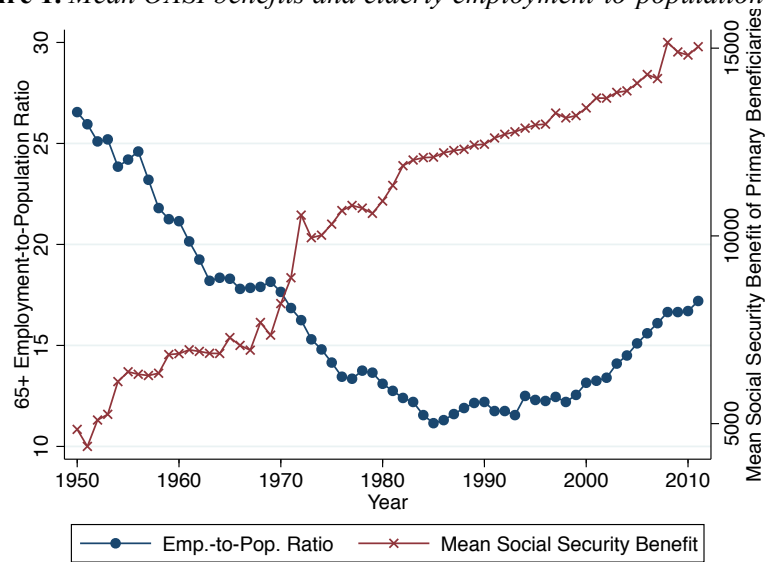
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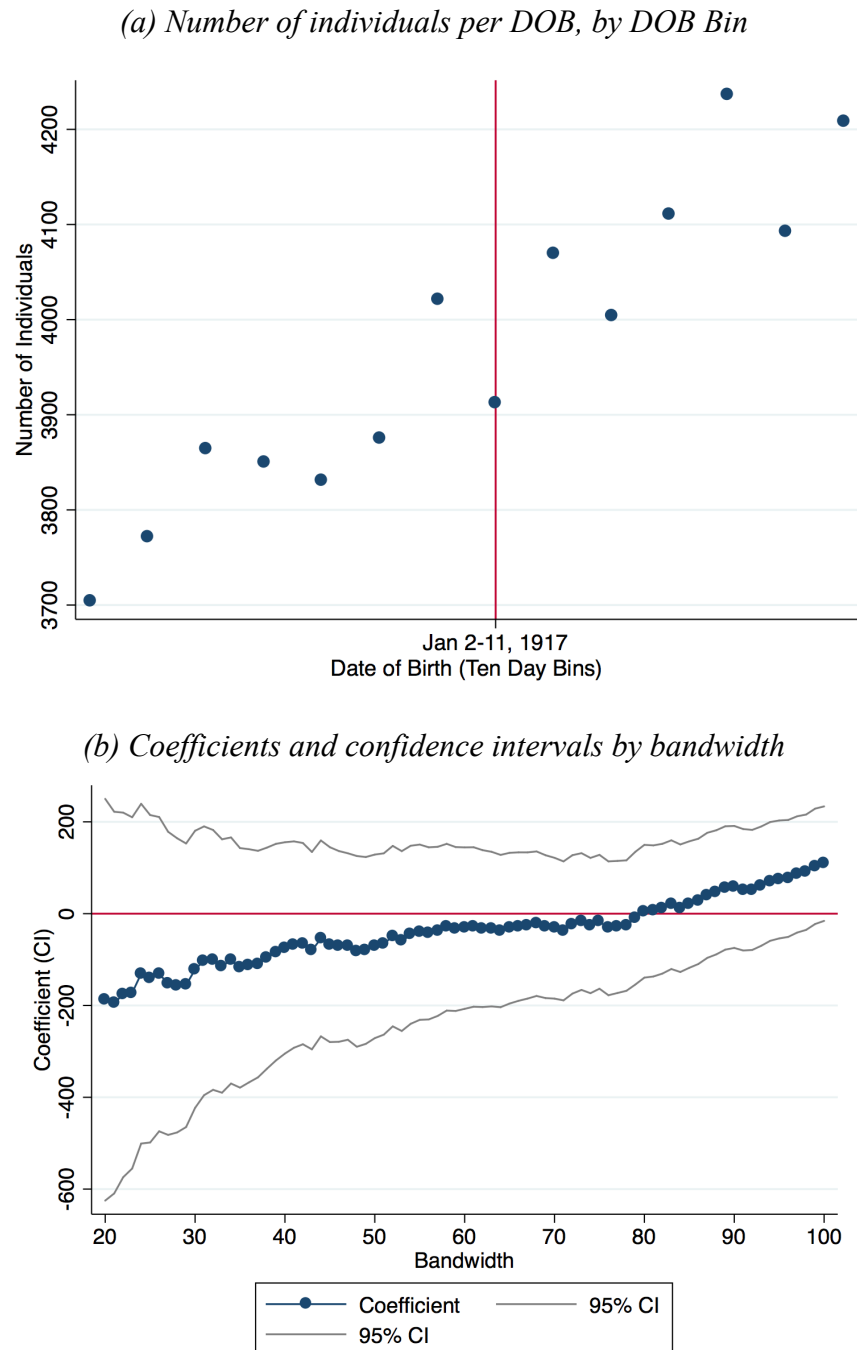


**Figure 1. Mean OASI benefits and elderly employment-to-population ratio**



Notes: The figure shows the employment-to-population ratio for those 65 and over, as well as the mean OASI benefit, by year from 1950 to 2012. The data on the employment-to-population ratio among those 65 and older come from the Bureau of Labor Statistics. The data on mean OASI benefit of primary beneficiaries come from Social Security Administration (2015b). The mean yearly OASI benefit reported by SSA is different than the mean benefit in our SSA Master Beneficiary Record data because the mean OASI benefit reported by SSA is influenced by the benefits of other birth cohorts that are not in our Master Beneficiary Record extract from the 1916 to 1923 cohorts.

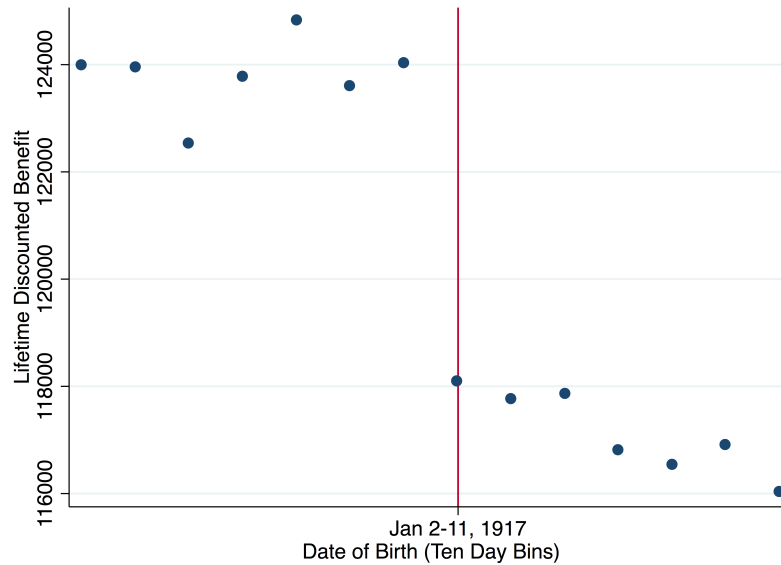
**Figure 2.**



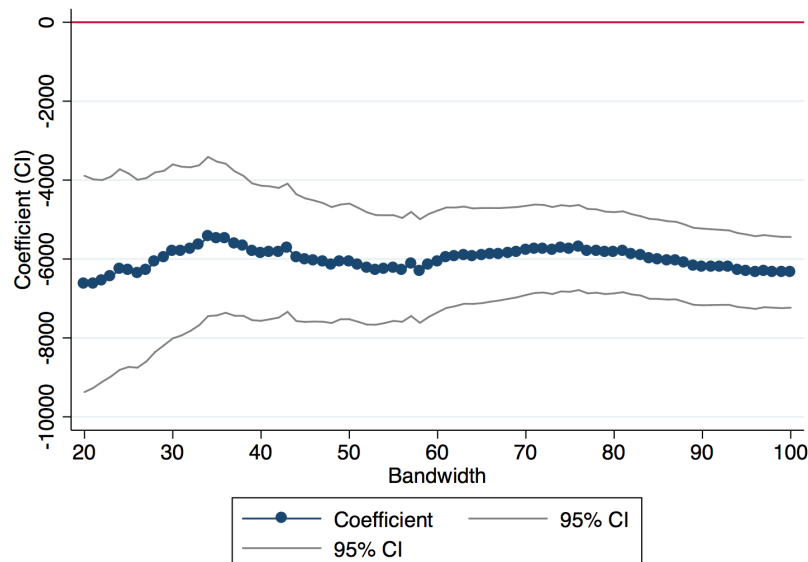
Notes: Panel (a) shows the mean number of individuals observed in 1977 per DOB in 10-day bins around the boundary separating the 1916 birth cohort from the 1917 birth cohort (*i.e.* January 2, 1917). Panel (b) shows the coefficients and 95 percent confidence intervals for the coefficient  $\beta_1$  on a dummy for being born on or after January 2, 1917 from model (1), as a function of the bandwidth (in days) chosen around this cutoff, when the dependent variable is number of observations. The data are a 100% sample from the Social Security Administration Master Earnings File and Master Beneficiary Record, with the sample restrictions described in the text.

**Figure 3.**

(a) *Mean discounted real OASI benefits, 1978 to 2012 (ages 61 to 95)*



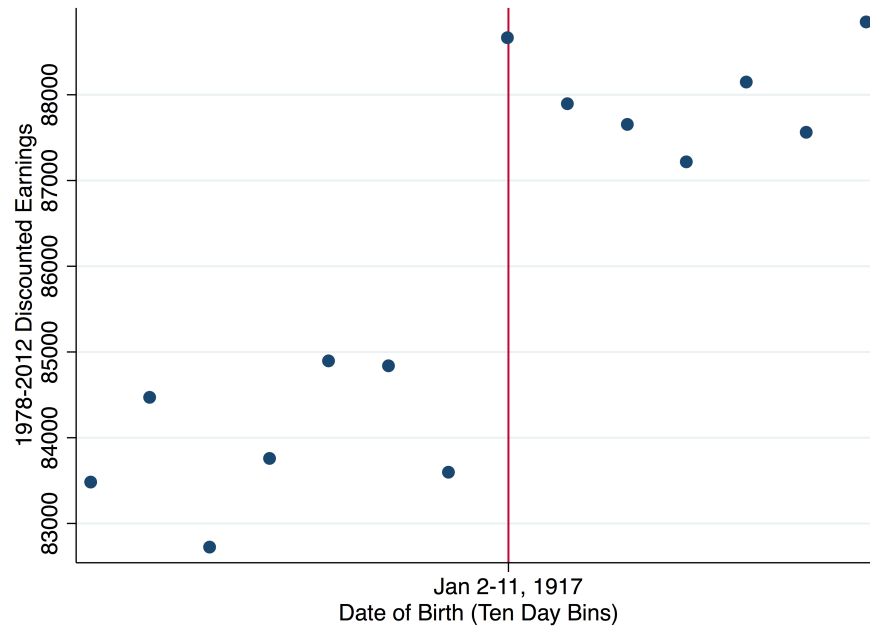
(b) *Coefficients and confidence intervals by bandwidth*



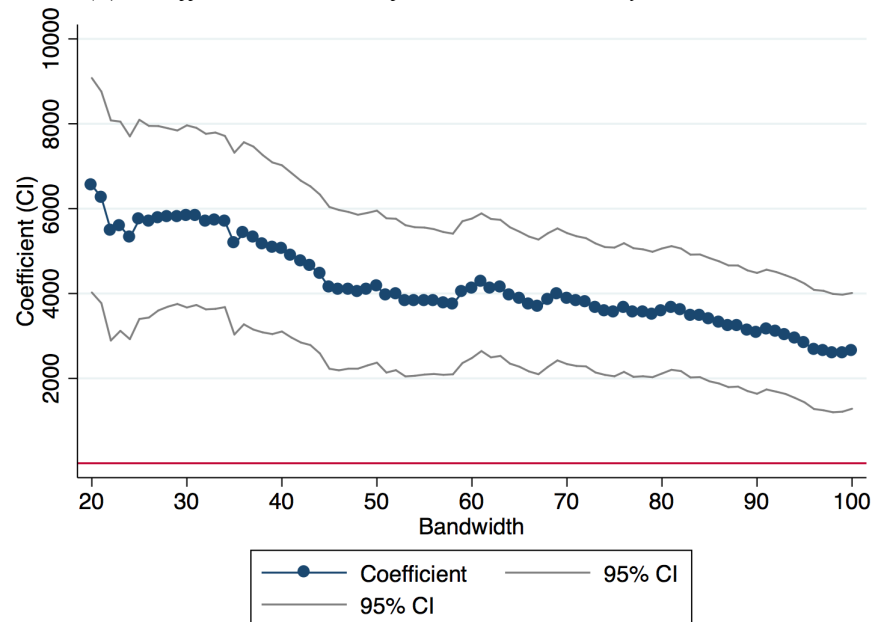
Notes: Panel (a) shows individuals' mean discounted OASI benefits from 1978 to 2012, in 10-day bins around the discontinuity separating the 1916 birth cohort from the 1917 birth cohort. We discount to 1977 terms and then express all dollar amounts in real 2012 dollars. For illustrative purposes we use a 3 percent real discount rate. Panel (b) shows the coefficients  $\gamma_l$  and associated 95 percent confidence intervals from model (2), when the dependent variable is mean discounted OASI benefits from 1978 to 2012. The 1917 birth cohort reaches ages 61 to 95 during the calendar years 1978 to 2012, respectively. See other notes to Figure 2.

**Figure 4.**

*(a) Mean presented discounted value of real earnings, 1978 to 2012 (ages 61 to 95)*



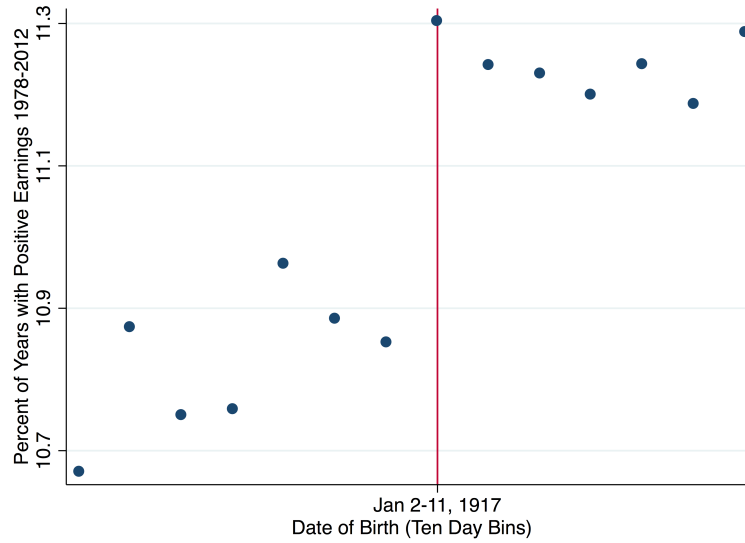
*(b) Coefficients and confidence intervals by bandwidth*



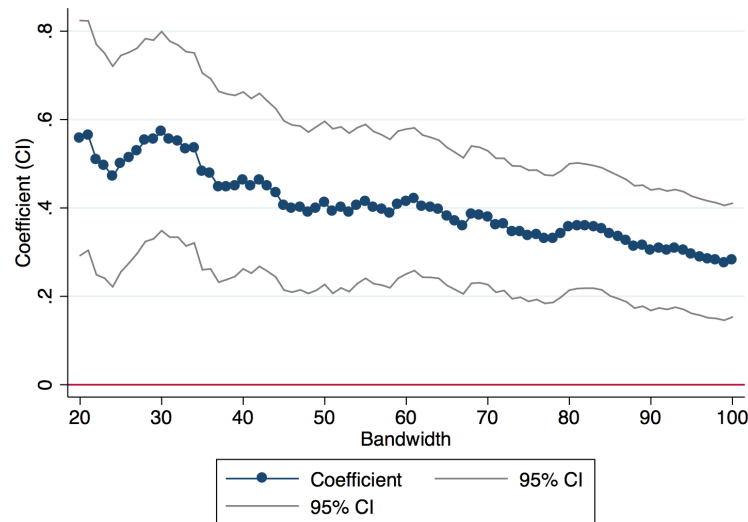
Notes: The figure shows results when the outcome of interest is the mean presented discounted value of real earnings from 1978 to 2012, when the 1917 birth cohort reaches ages 61 to 95. See other notes to Figure 3.

**Figure 5.**

*(a) Extensive margin: percent of years with positive earnings, 1978 to 2012 (ages 61 to 95)*

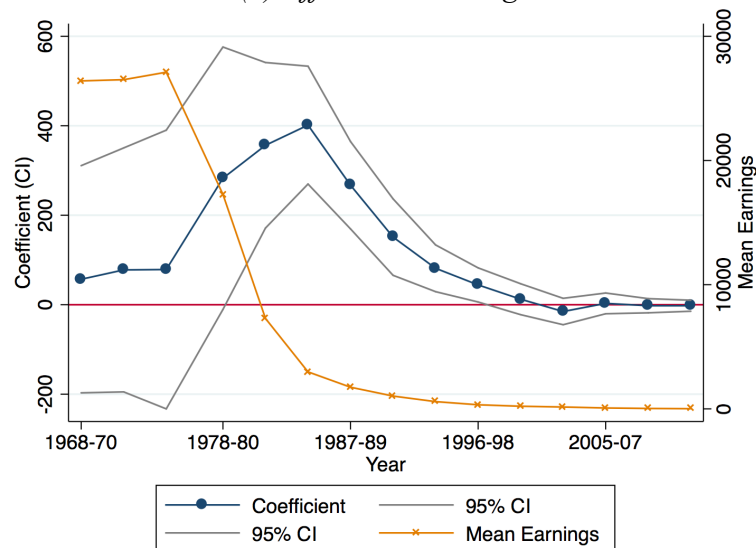


*(b) Coefficients and confidence intervals by bandwidth*

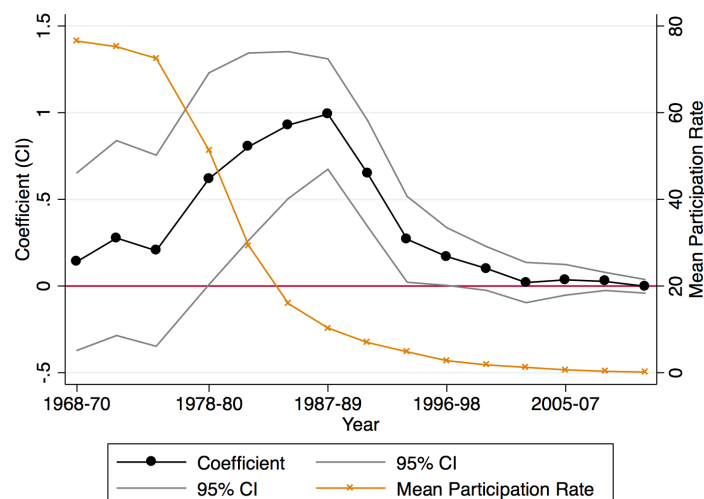


Notes: The figure shows results when the outcome is the percent of years from 1978 to 2012 in which individuals have positive yearly earnings. See other notes to Figure 3.

**Figure 6.** *Effects on earnings and participation, as well as sample means, by time period*  
 (a) *Effects on earnings*

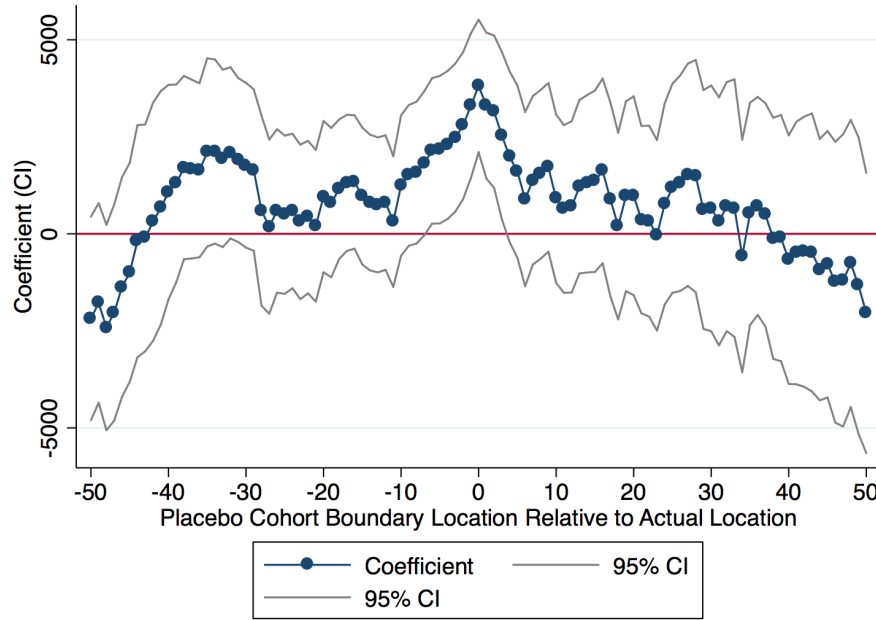


(b) *Effects on participation*



Notes: The figure shows the discontinuity at the boundary in mean yearly earnings, by 3-year period. It illustrates that the effects of the Notch on earnings are largest in the 1980s and early 1990s when individuals are 64 to 75 years old, and decline to insignificant in the later elderly years. Specifically, the y-axis shows the point estimate of  $\beta_l$  and its associated confidence interval from model (1) when we run it separately in each three-year time period  $t$  and the dependent variable is mean real earnings in those years (Panel (a), left axis) or the mean percent of years with positive earnings (Panel (b), left axis). For context, the mean of the dependent variable (yearly earnings and percent participation in Panels (a) and (b), respectively) among the 1916 cohort is also shown (right axis). The x-axis shows the time period in question.

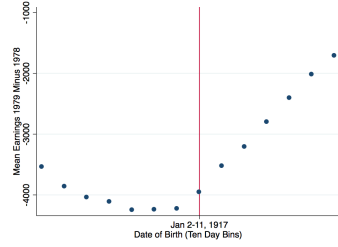
**Figure 7.** *Estimates of earnings effects for actual and placebo boundary locations*



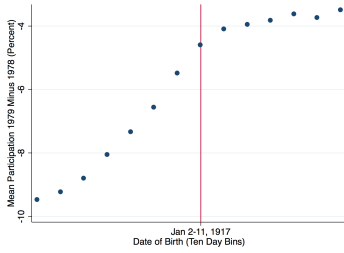
Notes: The figure shows the coefficients  $\beta_1$  and associated confidence intervals when we run model (1), except that instead of estimating the discontinuity only at the cohort boundary (*i.e.* defining the dummy  $D$  as 0 before January 2, 1917 and 1 after), we also separately place the discontinuity at 99 “placebo cohort boundaries,” on each DOB from 50 days before January 2, 1917 to 50 days after (*i.e.* for each of these placebo cohort boundaries, we define  $D$  as 0 before that placebo date and 1 after it). The figure shows the coefficient and confidence interval ( $y$ -axis) as a function of the location of the placebo cohort boundary relative to the actual cohort boundary, which is normalized to zero on the  $x$ -axis. The figure shows that the estimated effect is maximized exactly at the *actual* cohort boundary, and we estimate significant effects only when the “placebo cohort boundary” is placed near the actual cohort boundary.

**Figure 8. Illustrating the effects of substitution incentives**

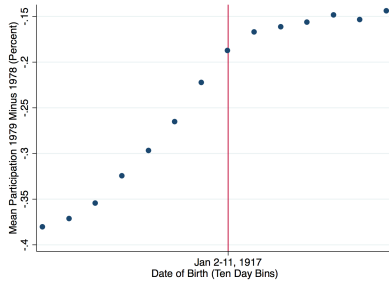
(a) Difference in mean yearly earnings, 1979 minus 1978



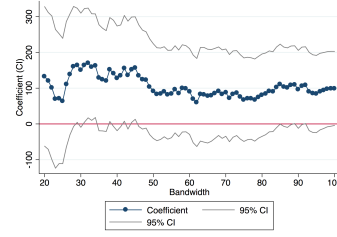
(b) Difference in participation rate, 1979 minus 1978



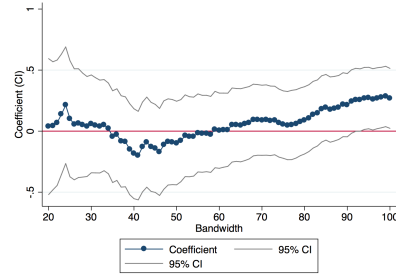
(c) Difference in log odds of participation, 1978 minus 1978



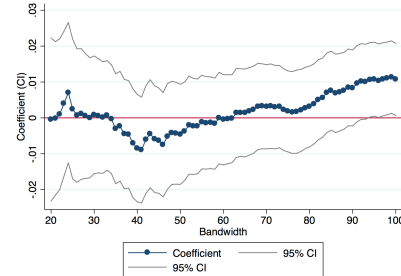
(d) Results by bandwidth for difference in mean yearly earnings, 1979 minus 1978



(e) Results by bandwidth for 1979 minus 1978 participation rate



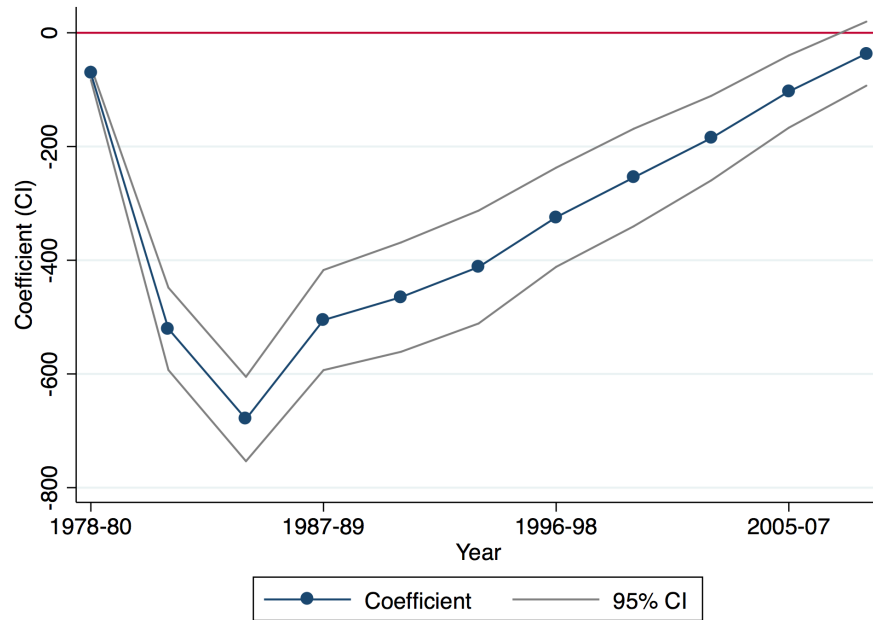
(f) Results by bandwidth for difference in log odds of participation, 1979 minus 1978



Notes: Panels (a), (b), and (c) show the mean of the dependent variable indicated, in 10-day bins around the discontinuity separating the 1916 birth cohort from the 1917 birth cohort. Panels (d), (e), and (f) show the coefficients  $\beta_1$  and associated 95 percent confidence intervals on a dummy for being born on or after January 2, 1917 from model (1), as a function of the bandwidth (in days), chosen around this cutoff, with the indicated dependent variable. The figure shows that there is no evidence that the change in earnings or participation from 1978 to 1979 decreases discontinuously at the cohort boundary, consistent with essentially no reaction to the large change in substitution incentives. Appendix Figure 9 shows that the earnings results are similar when we specify them in terms of (approximate) percent changes, rather than absolute levels. See other notes to Figure 3. Although the level of the dependent variable appears continuous at the boundary, its slope appears to increase at the boundary. Since individuals on average greatly reduce their earnings at the time of the year when they reach the OASI EEA at age 62, we would expect exactly this pattern. Those born in 1916 turn age 62 in 1978 and turn 63 in 1979, whereas those born in 1917 turn age 61 in 1978 and 62 in 1979. Thus, those born in 1917 should greatly reduce their earnings in the month they turn 62, and those born later in 1917 are age 62 for a smaller fraction of 1979 than those born earlier in 1979—leading to a steep upward slope in  $\Delta E_i$  among those born in 1917, but not among those born in 1916. This change in the slope of  $\Delta E_i$  appears around every cohort boundary in our data. Although this mechanism causes a change in slope at the boundary, it should not cause a change in the level of  $\Delta E_i$  at the boundary and thus does not threaten our identification strategy. Appendix Figure 9 shows that the change in slope is much less pronounced when specifying the outcome in (approximate) percentage changes from 1978 to 1979, rather than in levels.

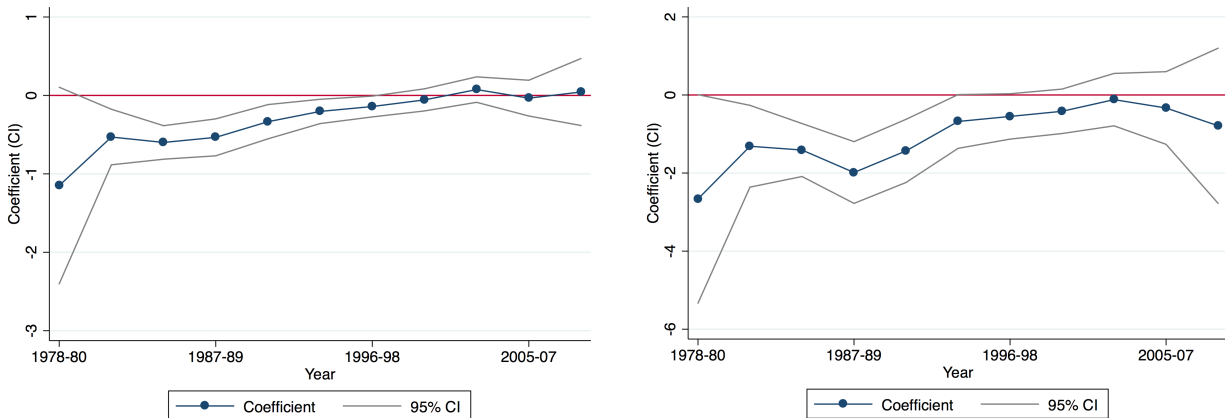


**Figure 9.** *Discontinuity in benefits received, by time period*



Notes: The figure shows the discontinuity at the boundary in mean yearly OASI benefits by 3-year period. It illustrates that the discontinuity at the boundary in OASI benefits is largest in the mid-to-late 1980s and early 1990s when individuals are 64 to 75 years old, and declines to insignificant levels in the later elderly years. Specifically, the y-axis shows the point estimate of  $\gamma_t$  and its associated 95 percent confidence interval from model (2) when we run it separately in each three-year time period  $t$  and the dependent variable is mean real OASI benefits in those years. The x-axis shows the time period in question.

**Figure 10.** *Effects of contemporaneous benefits received, by time period*  
 (a) *Earnings effects* (b) *Participation effects*



Notes: The figure shows the effect of yearly OASI benefits on contemporaneous earnings and participation by 3-year period. Specifically, the y-axis shows the point estimate of  $\vartheta_t$  and its associated confidence interval from the 2SLS model (16)-(17), when we run it separately in each three-year time period shown on the x-axis.

**Table 1: Summary statistics: mean (standard deviation) of main variables**

Variable	Mean (SD)
Discounted Earnings, 1978 to 2012	\$ 85,901.01 (3,607.13)
Percent of years with positive earnings, 1978 to 2012	11.03 (0.35)
Discounted OASI benefits, 1978 to 2012	\$120,151.60 (3,645.21)
Last calendar year earned positive amount	1979.78 (0.20)
Age of claiming	63.50 (0.04)
Number of individuals per day of birth	3,979.40 (258.30)

Notes: The source is SSA administrative data from the Master Earnings File and Master Beneficiary Record on the universe of U.S. data, with the sample restrictions described in the text. The table shows means and standard deviations of the main variables in our sample. We report the means and standard deviations of the means of variables by DOB, rather than reporting the mean and standard deviation in the individual-level SSA data, since we use the DOB-mean-level variables in our primary regression analysis. The sample consists of those born within 100 days of January 2, 1917. The means and standard deviations shown above are based on 200 observations in each case. Starting in the calendar year after an individual dies, their earnings and benefits are set to zero prior to averaging by DOB. All earnings amounts are expressed in real 2012 dollars. The number of individuals per day refers to the number of individuals per day of birth who are alive in 1978. This corresponds to 724,106 individuals within 100 days of the cohort boundary, or 24,619,604 individual-year observations from 1978 to 2012 (inclusive).

**Table 2. Testing smoothness of predetermined variables**

Specification	(1) Percent male	(2) Percent white	(3) Number of individuals
A) Coefficient (SE) on Jan. 2, 1917 dummy (linear)	-0.25 (0.29)	(0.53) (0.68)	-96.30 (159.44)
B) Coefficient (SE) on Jan. 2, 1917 dummy (quadratic)	(0.21) (0.40)	0.87 (1.02)	-255.73 (292.55)

Notes: The table demonstrates the smoothness of predetermined variables around the 1916/1917 cohort boundary. The table shows the results of OLS regressions corresponding to model (1) in the text, where the dependent variable is shown in the column heading. Row A shows a specification in which the control for the running variable (*i.e.* DOB) is a linear function (allowing for a change in slope at Jan. 2, 1917), and Row B shows a specification in which the control for the running variable is a quadratic function (allowing for a different coefficient on either side of the boundary, for both the linear and quadratic terms, where Jan. 2, 1917 has been normalized to zero). We use robust standard errors in Table 2 and throughout the other tables. We show the results for the bandwidth of 56, chosen using the Calonico *et al.* (2014) procedure when the outcome is our primary outcome (discounted real earnings from 1978 to 2012), to hold the sample constant across regressions; in Appendix Table 1 we show the results when the bandwidth is chosen separately for each outcome. Thus, all regressions have 112 observations. None of the estimated coefficients is significant at a standard significance level. See other notes to Table 1.

**Table 3. Effect of Notch on benefits, earnings, and participation**

Outcome	(1) Linear	(2) Linear	(3) Quadratic	(4) Quadratic
A) Discounted benefits, 1978 to 2012	-6,125.64 (673.10)***	-6,109.97 (664.65)***	-5,958.18 (1,180.62)***	-6,393.76 (1234.24)***
B) Substitution incentive, $\mu_{j1979}$	-0.22 (0.0030)***	-0.22 (0.0035)***	-0.22 (0.0079)***	-0.22 (0.0069)***
C) Discounted earnings, 1978 to 2012	3,766.02 (858.30)***	3,865.18 (865.10)***	5,996.94 (1,144.14)***	5,702.02 (1,139.05)***
D) Percent years with positive earnings, 1978 to 2012	0.40 (0.09)***	0.41 (0.09)***	0.52 (0.12)***	0.51 (0.12)***
E) Log odds of fraction of years with positive earnings, 1978 to 2012	0.040 (0.0088)***	0.042 (0.0093)***	0.053 (0.012)***	0.051 (0.013)***
F) Last year earned positive amount	0.16 (0.06)***	0.18 (0.065)***	0.24 (0.094)**	0.22 (0.092)**
Controls?	N	Y	N	Y

Notes: The table shows the results of OLS regressions corresponding to the RDD model (2) (Row A) or RDD model (1) (Rows B to F) described in the text estimating the effect of the Notch on outcomes, in which each outcome is regressed on a dummy for being covered by the Notch policy (*i.e.* being born on or after Jan. 2, 1917), as well as a smooth trend in DOB. The “controls” columns show the regressions with additional controls for percent white and percent male by DOB. In all cases, the specification that minimizes the Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC) is the linear specification without controls. Throughout the tables, \*\*\* refers to significance at the 1% level; \*\* at the 5% level, and \* at the 10% level. See other notes to Table 2.

**Table 4. Placebo tests: OLS regressions of earnings outcomes on a dummy for being affected by the Notch**

	(1) No controls	(2) With controls
A) Mean earnings 1974 to 1976	78.63 (158.92)	73.62 (127.36)
B) Percent years with positive earnings 1974 to 1976	0.20 (0.28)	0.27 (0.27)
C) Discounted earnings age 61 to 2012 ( <i>i.e.</i> 1984 to 2012), 1922/23 boundary	204.83 (761.97)	89.51 (759.20)
D) Percent of years with positive age 61 to 2012 ( <i>i.e.</i> 1984 to 2012), 1922/23 boundary	-0.0022 (0.11)	-0.0012 (0.10)
E) Discounted earnings age 61 to 2012, 1910/11-1915/16, 1923/24, 1925/26, 1927/28, 1929/30 boundaries, 10% sample	-1,702.59 (2,252.07)	-2,266.33 (2,038.24)
F) % of yrs with earnings > 0 age 61 to 2012, 1910/11-1915/16, 1923/24, 1925/26, 1927/28, 1929/30 boundaries, 10% sample	-0.11 (0.40)	0.15 (0.37)

Notes: The table shows the results of OLS regressions corresponding to model (1) in the text. We show that in three placebo samples not subject to discontinuities in OASI benefit policy, there is no discontinuous change in earnings at the placebo cohort boundaries. In Rows A and B, the dependent variable is mean earnings and participation, respectively, in 1974-1976, prior to the time when the discontinuity in benefits at the 1916/1917 cohort boundary could have been anticipated. In Rows C and D, we examine earnings around the 1922/1923 boundary, which did not experience a discontinuous change in policy and for which our data contain a 100% sample of the population. For Rows E and F, we were able to obtain a 10% sample of the population around other placebo cohort boundaries without discontinuous changes in OASI incentives: 1910/1911 to 1915/1916, 1923/1924, 1925/1926, 1927/1928, and 1929/1930. We use the baseline linear specification from Table 3. Participation is expressed so that coefficients reflect percentage point changes. See other notes to Table 3.

**Table 5. Discontinuity in earnings and participation at placebo boundaries and the 1916/1917 boundary**

Age range (cohort boundary)	(1) SSA data, discounted earnings, 1916/17 boundary	(2) IRS data, discounted earnings, 1999-2013	(3) SSA data, % of years with earnings > 0, 1916/17 boundary	(4) IRS data, % of years with earnings > 0, 1999-2013
A) 75 to 89 (1923/1924)	313.51 (129.57)**	89.02 (118.99)	0.16 (0.068)**	0.039 (.054)
B) 73 to 87 (1925/1926)	542.76 (176.27)***	38.37 (158.03)	0.25 (0.081)***	0.12 (0.075)
C) 71 to 85 (1927/1928)	918.11 (230.25)***	529.13 (210.18)**	0.37 (0.094)***	0.26 (0.10)**
D) 69 to 83 (1929/1930)	1,448.24 (285.26)***	256.88 (306.23)	0.52 (0.11)***	0.14 (0.15)
E) 67 to 81 (1931/1932)	2,094.22 (360.41)***	-277.78 (448.42)	0.62 (0.12)***	-0.09 (0.22)
F) 65 to 79 (1933/1934)	2,900.87 (461.57)***	1,092.02 (662.71)	0.74 (0.14)***	0.32 (0.22)
G) 63 to 77 (1935/1936)	3,099.92 (616.61)***	965.82 (1069.22)	0.77 (0.16)***	0.15 (0.32)

Notes: The table shows using a 100% population sample from SSA and IRS data that a strong discontinuity in earnings only regularly shows up around the 1916/1917 boundary, not around placebo boundaries that do not have OASI policy discontinuities. In particular, we were able to obtain a 100% sample of IRS W-2 wage earnings data from 1999 to 2013 on all fourteen cohort boundaries from 1923/1924 to 1936/1937. Among these boundaries, seven—1923/1924, 1925/1926, 1927/1928, 1929/1930, 1931/1932, 1933/1934, and 1935/1936—have no associated discontinuity in the Delayed Retirement Credit or another OASI policy, so we investigate these boundaries as placebos. These cohorts are observed in the IRS data over a subset of the ages that we observe the 1916/1917 cohorts when using in the SSA data: in the IRS data we observe ages 76 to 90 for the 1923 cohort, ages 75 to 89 for the 1924 cohort, *etc.* To make an apples-to-apples comparison between the IRS data and the SSA data, we investigate the discontinuity in discounted real earnings in the SSA data over the same ages. Table 5 shows that over each of these sets of ages, we find highly significant discontinuities in discounted earnings and participation at the 1916/1917 boundary in the SSA data, but we find a significant discontinuity in the IRS data around only one of the seven boundaries. For a given cohort boundary, the age range reported refers to the highest age attained in a given calendar year of data for the *younger* cohort around the boundary; for example, “ages 75 to 89” refers to the fact that around the 1923/1924 boundary, those born in 1924 attained ages 75 to 89 in 1999 to 2013, respectively. It makes sense that the standard errors are larger on the estimates for cohorts the IRS data than those in the SSA data for 1916/1917 over the comparable set of ages; as noted in Appendix Table 3, the means and standard deviations of earnings are larger in the IRS data due to the secular trend of increasing elderly participation and earnings across cohorts from 1917 to 1937. When pooling all seven boundaries in the IRS data and defining a dummy for being born after Jan. 1 around any of the boundaries, the coefficient on this dummy in regression (1) implemented on this pooled data is insignificant ( $p = 0.20$  for discounted earnings and  $p = 0.33$  for participation). In these regressions we cluster the standard error by DOB relative to the cohort boundary, though the results are also insignificant if we do not cluster. See other notes to Table 3.

**Table 6.** *Lower bound income effect of discounted lifetime benefits on discounted lifetime earnings or participation*

	(1) Discounted earnings	(2) Discounted earnings	(3) Percent of years with earnings > 0, 1978 to 2012	(4) Percent of years with earnings > 0, 1978 to 2012
	-0.61 (0.17)***	-0.63 (0.17)***	-0.65 (0.17)***	-0.67 (0.18)***
Controls?	N	Y	N	Y

Notes: The table shows the results of two-stage least squares regressions corresponding to regressions (2) and (7) in the text, estimating the effect of discounted lifetime OASI benefits on discounted lifetime earnings. The excluded instrument is the dummy for being in the 1917 cohort. The dependent variable is discounted earnings from 1978 to 2012 (Columns 1 and 2), or the percent of years with positive earnings from 1978 to 2012 (Columns 3 and 4). For ease of interpretation, for the participation specification, the coefficient and standard error have been multiplied by 1,000,000, so that the quoted coefficients reflect the percentage point effect on participation of a \$10,000 increase in discounted lifetime OASI benefits (which, for reference, is 63 percent larger than the actual baseline discontinuity in discounted OASI benefits). We use the baseline linear specification of the running variable. As discussed in the main text, we interpret the results as estimates of lower bounds on the income effect in the context of an illustrative lifecycle model. See other notes to Table 3.

**Table 7.** *Heterogeneity analysis*

Dependent variable	(1) Men	(2) Women	(3) Low pre-1977 earnings	(4) High pre-1977 earnings
A) Income effect on earnings	-0.61 (0.17)***	-0.89 (0.43)**	-0.87 (0.46)*	-0.35 (0.097)***
B) Income effect on participation	-0.55 (0.16)***	-1.24 (0.61)**	-1.01 (0.72)	-0.38 (0.090)***

Notes: The table shows the results of two-stage least squares regressions corresponding to regressions (2) and (7) in the text, estimating the effect of discounted lifetime OASI benefits on discounted lifetime earnings. The dependent variable is mean discounted earnings from 1978 to 2012 in the group shown in the column heading. Columns (3) and (4) show the results for those with mean real earnings in years prior to 1977 that are below and above the median, respectively. We use the baseline linear specification of the running variable. The results are similar when calculating separate optimal bandwidths for each group. See other notes to Table 6.

**Table 8. Responses to substitution incentives**

Outcome	(1)	(2)
A) Earnings 1979 minus 1978 (levels)	85.64 (61.75)	88.30 (62.30)
B) Earnings substitution elasticity: levels, actual substitution incentive	-0.017 (0.014)	-0.017 (0.013)
C) Participation 1979 minus 1978 (levels)	-0.019 (0.16)	-0.024 (0.16)
D) Participation 1979 minus 1978 (log odds)	-0.0014 (0.0065)	-0.0016 (0.0066)
E) Participation substitution elasticity: actual substitution incentive	0.0033 (0.023)	0.0026 (0.022)
Controls?	N	Y

Notes: In Row A, the dependent variable is the DOB-level mean of earnings in 1979 minus the DOB-level mean of earnings in 1978. As described in the text, Row B calculates the implied substitution elasticities using equation (14) and the actual substitution incentive individuals face. The 95 percent confidence interval rules out more than a small positive elasticity in each case. Rows C and D estimate the discontinuity in participation rates (expressed in percentage points) and the log odds of participation, respectively. Row E shows the implied participation elasticity with respect to the actual substitution incentive,  $(1+\mu_{ijt}-\tau_{ijt})$ . See other notes to Table 3.

**Table 9. Effect of yearly OASI benefits on contemporaneous earnings**

Outcome	(1) Actual benefits	(2) Actual benefits	(3) Simulated benefits	(4) Simulated benefits
A) Yearly earnings, 1978 to 2012	-0.46 (0.12)***	-0.46 (0.12)***	-0.51 (0.13)***	-0.52 (0.13)***
B) % of years with positive earnings, 1978 to 2012 (x100)	-1.31 (0.33)***	-1.35 (0.34)***	-1.46 (0.37)***	-1.50 (0.38)***
Controls?	N	Y	N	Y

Notes: The table shows the results of the 2SLS model (18)-(19) described in the text, in which earnings in a given year are related to OASI benefits in that year. The coefficients on OASI benefits are around 34 percent smaller than those in the lifecycle model estimates in Table 6, which is not surprising since the presented discounted value of benefits decreases faster in the discount rate than discounted earnings do (since benefits are much larger than earnings at older ages but are more similar at younger ages). We begin the regressions in 1978 since that is when those born in December of 1916 reached the EEA. For ease of interpretation, for the participation specification, the coefficient and standard error have been multiplied by 100,000, so that the quoted coefficients reflect the percentage point effect on participation of a \$1,000 increase in average yearly OASI benefits. We interpret the results as estimates of lower bounds on the income effect within a static model. See other notes to Table 6.

# Online Appendices

## A Model with Fixed Cost of Participation

### A.1 Initial Model

To add a fixed cost of participation to the model, we follow Eissa, Kleven, and Kreiner (2008). Assume that the utility function is as in (3), but that the individual faces a fixed cost  $q_i$  of participation in each period:

$$U_{ijt} = U_{ij}(U_{ij}^t(C_{ijt}, E_{ijt}, X_{ijt}, Z_{ijt}) - q_i \mathbf{1}(E_{ijt} > 0), \dots, U_{ij}^T(C_{ijT}, E_{ijT}, X_{ijT}, Z_{ijT}) - q_i \mathbf{1}(E_{ijT} > 0)) \quad (\text{A.1})$$

where  $\mathbf{1}$  is the indicator function. Each individual randomly draws the fixed cost from distribution  $\Psi_i(q_i)$  with density  $\varphi_i(q_i)$ , implying that each individual has an *ex ante* probability of participation in each period. The fixed cost is drawn initially, so that the individual conditions their decisions in each period on this initial draw of  $q_i$ . Utility is maximized subject to the intertemporal budget constraint (4), where again we allow a nonlinear tax function  $T_{jt}(E_{ijt})$  as described in the text and linearize the budget set. To illustrate the ideas as transparently as possible, we specify a fixed cost that is equal across periods and separable from  $U_{ij}^t()$ , though this can be generalized (results available upon request).

Given positive earnings  $E_{ijt} > 0$ , the first order condition (FOC) specifies  $(1 + \mu_{ijt} - \tau_{ijt}) \frac{\partial U_{ij}^t}{\partial C_{ijt}} = -\frac{\partial U_{ij}^t}{\partial E_{ijt}}$ , so that earnings if the individual participates,  $\check{E}_{ijt}$ , are related to the marginal net-of-tax rate  $(1 + \mu_{ijt} - \tau_{ijt})$ , namely  $\check{E}_{ijt} = L(\lambda_{ijt}, 1 + \mu_{ijt} - \tau_{ijt}, X_{ijt}, Z_{ijt})$ . The individual then chooses whether to have earnings  $\check{E}_{ijt}$  or to have zero earnings. We can define a cutoff level of the fixed cost of participation  $\bar{q}_i$  above which the individual does not participate, and below which s/he participates, and we can define an average tax rate  $ATR_{ijt} = [T_{jt}(\check{E}_{ijt}) - T_{jt}(0)] / \check{E}_{ijt}$ . The participation decision will then depend on the average net-of-tax rate,  $1 - ATR_{ijt}$ . Thus, we can write:

$$E_{ijt} = L(\lambda_{ijt}, 1 + \mu_{ijt} - \tau_{ijt}, 1 - ATR_{ijt}, X_{ijt}, Z_{ijt}, q_i) \quad (\text{A.2})$$

As above, expected earnings unconditional on  $Z_{ijt}$  and  $q_i$ ,  $\mathbb{E}(E_{ijt} | B_{ijPDV}, Y_{ijPDV}, X_{ijt})$ , can be expressed in this additive form that is linear in  $B_{ijPDV} + Y_{ijPDV}$ :

$$\mathbb{E}(E_{ijt}) = \alpha(B_{ijPDV} + Y_{ijPDV}) + f(1 + \mu_{ijt} - \tau_{ijt}) + g(1 - ATR_{ijt}) + h(X_{ijt}) \quad (\text{A.3})$$

where  $f()$  and  $g()$  are weakly increasing functions with  $f'() \geq 0, g'() \geq 0$ , relating earnings to substitution incentives, and  $h()$  relates earnings to observable characteristics  $X_{ijt}$ . For ease of notation here and subsequently we suppress the conditioning variables, so that  $\mathbb{E}(E_{ijt})$  refers to  $\mathbb{E}(E_{ijt} | B_{ijPDV}, Y_{ijPDV}, X_{ijt})$ . To estimate a lower bound on  $\beta$ , we set  $f() = 0$  and  $g() = 0$  for illustrative purposes. We then take means of (A.3) at the DOB level  $j$  to estimate conservative standard errors as above, defining  $E_{jt}$  as the mean of  $E_{ijt}$  by DOB. We assume that  $X$  affects earnings and OASI benefits through  $h()$  in a way that is on average smooth across DOBs, that  $Y_{jPDV}$  is smooth across DOBs, and that these influences can be captured

through a linear spline, as in (1) and (2) above. Under these assumptions we can again estimate the income effect of OASI benefits on earnings through a 2SLS model exploiting the discontinuity in benefits, in which equation (2) is the first stage, and the second stage is:

$$E_{jPDV} = \alpha_1 B_{jPDV} + \alpha_2 DOB_j + \alpha_3 (D * DOB)_j + \eta_j \quad (\text{A.4})$$

We can also write a parallel linear probability regression model with the probability of participation as the dependent variable.

## A.2 Model Relevant for Estimating Frisch Elasticity

To make progress on estimating a Frisch elasticity, we linearize the earnings supply function further by specifying earnings as a linear function of the substitution incentives and personal characteristics (*i.e.* setting  $f(1 + \mu_{ijt} - \tau_{ijt}) = \delta(1 + \mu_{ijt} - \tau_{ijt})$ ,  $g(1 - ATR_{ijt}) = \gamma(1 - ATR_{ijt})$ , and  $h(X_{ijt}) = \theta X_{ijt}$ ):

$$\mathbb{E}(E_{ijt}) = \beta \lambda_{ijt} + \delta(1 + \mu_{ijt} - \tau_{ijt}) + \gamma(1 - ATR_{ijt}) + \theta X_{ijt} \quad (\text{A.5})$$

Here  $\beta$  reflects an income effect, and  $\delta$  and  $\gamma$  reflect substitution effects. Given (A.5), we have:

$$\mathbb{E}(E_{ij1978}) = \beta \lambda_{ij1978} + \delta(1 + \mu_{ij1978} - \tau_{ij1978}) + \gamma(1 - ATR_{ij1978}) + \theta X_{ij1978} \quad (\text{A.6})$$

$$\mathbb{E}(E_{ij1979}) = \beta \lambda_{ij1979} + \delta(1 + \mu_{ij1979} - \tau_{ij1979}) + \gamma(1 - ATR_{ij1979}) + \theta X_{ij1979} \quad (\text{A.7})$$

To make further progress, as in the main text we can assume that in 1978 and 1979, on average at the DOB-mean level  $j$  other factors affecting labor supply, and marginal tax rates, are approximately constant, *i.e.*  $X_{j1978} \approx X_{j1979}$  and  $\tau_{j1978} \approx \tau_{j1979}$ . A change in the marginal utility of wealth  $\lambda$  from the expected benefit cuts of the 1977 amendments should be realized immediately when the policy change was anticipated, so certainly by 1978. Thus, to make progress, and given that the discount factor  $\kappa = 1/(1 + r_{t+1})$ , we can also assume that  $\lambda_{j1978} \approx \lambda_{j1979}$ . Meanwhile, the substitution incentive should only affect earnings beginning in 1979, the calendar year when the 1917 cohort turns 62. Thus, taking means at the DOB level  $j$  and subtracting (A.6) from (A.7), we have:

$$\mathbb{E}(E_{j1979} - E_{j1978}) = \delta(\mu_{j1979} - \mu_{j1978}) + \gamma(ATR_{j1978} - ATR_{j1979}) \quad (\text{A.8})$$

In this case, the discontinuity in earnings should be related to both  $\delta$  and  $\gamma$ , so we cannot separately identify the two parameters.

We can write a parallel linear probability model with the probability of participation as the dependent variable. As noted above, changes in the marginal tax rate  $(1 + \mu_{ijt} - \tau_{ijt})$  will affect  $\check{E}_{ijt}$ , but such changes in  $\check{E}_{ijt}$  have no impact on participation due to the envelope theorem. Thus, similar to (A.8), we can relate the average change in participation  $p_{j1979} - p_{j1978}$  to the change in incentives, but only the change in the average tax rate (not the change in the marginal tax rate) will be relevant:

$$\mathbb{E}(p_{j1979} - p_{j1978}) = \gamma(ATR_{j1978} - ATR_{j1979}) \quad (\text{A.9})$$



We can then transform an estimate of  $\hat{\gamma}$  into a participation elasticity following a parallel method to (14).

### A.3 Model with Liquidity Constraints or Myopia

In a linearized version of the static specification in Blundell and MaCurdy (1999), with fixed costs as in Eissa, Kleven, and Kreiner (2008), expected earnings  $\mathbb{E}(E_{ijt})$  at time  $t$  could be written as a function of the net returns to work  $(1 + \mu_{ijt} - \tau_{ijt})$  in that period, the average net-of-tax rate  $1 - ATR_{ijt}$ , unearned income  $B_{ijt} + Y_{ijt}$ , and other factors  $X_{ijt}$ :

$$\mathbb{E}(E_{ijt}) = \alpha(B_{ijt} + Y_{ijt}) + \beta(1 + \mu_{ijt} - \tau_{ijt}) + \gamma(1 - ATR_{ijt}) + \delta X_{ijt} \quad (\text{A.10})$$

We can again estimate a lower bound on the income effect as in models (16)-(19).

## B Robustness Checks for Income Effect Lower Bound Estimates

Our results are robust to other choices. Appendix Table 4 shows that the estimates are in the same range when we vary the assumed discount rate from 1 percent to 5 percent, ranging from crowdout of 54 cents with a 1 percent discount rate to 69 cents with a 5 percent rate.

Kopczuk and Song (2008) note that more individuals are reported as born on the first of the month than other days (particularly including January 1), and that OASI gives individuals differential incentives to report being born on the second of each month (including January 2). If these issues were important in driving our results, we would expect to find discontinuities in placebo samples, but we do not. To address this issue further, in Appendix Table 6 we find comparable results when we exclude birthdays from December 30, 1916 to January 4, 1917 (which encompasses January 1 and 2, plus surrounding days that could be secondarily affected). The results are also robust to excluding only January 1 and 2, or to excluding other similar ranges of birth dates.

We also show that the results are extremely similar in the years 1979 to 2012 aggregated (Appendix Table 6), *i.e.* we remove 1978 because the Notch policy did not affect substitution incentives in 1978.

## C Effect of Simulated OASI Benefits on Earnings

Table 6 estimates the effect of actual lifetime OASI benefits on earnings. This is an object of policy interest, as policy-makers are interested in the effects of a hypothetical change in actual benefits. Note that the observed discontinuity in benefits at the cohort boundary is influenced by both the mechanical component due to the policy change, and it is also influenced slightly by the endogenous response to this change in benefits. Policy-makers may also be interested in the effect on earnings of the cuts in benefits that they schedule, holding behavior fixed. Note, however, that in this context there is very little scope for average benefits in the 1917 cohort to change materially as a result of endogenous changes in earnings, since earnings starting in 1979 did not affect OASI benefits under the transitional guarantee.

A different specification, which estimates a different empirical object of interest, estimates the discontinuity in the *simulated* lifetime benefit, as in a “simulated instruments” strategy (*e.g.* Gruber and Saez 2002). This is relevant to a two-stage least squares specification that estimates the effect of a *simulated* change in lifetime OASI benefits on lifetime earnings (taking inflation as given). Thus, in the specification in Appendix Table 5, we estimate a 2SLS regression in which the cohort boundary dummy serves as an instrument for “simulated” OASI benefits that an individual could have if his or her behavior were unaffected by the 1977 amendments. Specifically, we simulate earnings in calendar years  $t = 1977, 1978 \dots 2012$ , by using the evolution of earnings in the 1916 cohort (keeping data from the 1916 cohort within 100 days of the cohort boundary, to make the sample comparable to our sample from the 1917 cohort). Specifically, using these 1916 cohort data, we separately regress earnings in each year  $t$  on earnings in 1975 and its square, as well as earnings in 1976 and its square:

$$E_{ijt} = \psi_0 + \psi_1 E_{ij1975} + \psi_2 E_{ij1975}^2 + \psi_3 E_{ij1976} + \psi_4 E_{ij1976}^2 + \varepsilon_{ijt} \quad (\text{C.11})$$

In *both* the 1916 and 1917 cohorts, we then simulate earnings for each individual  $i$  in each calendar year  $t$  by obtaining the fitted values  $\hat{E}_{ijt}$ , given that individual’s actual earnings in 1975 and 1976. The rationale behind this procedure is that we wish to simulate the earnings that an individual *would* have had if they were in the 1916 cohort, absent the Notch policy change that affected the 1917 cohort (and using the same procedure for calculating simulated earnings in both the 1916 and 1917 cohorts).

We add a random term  $u_{ijt}$  drawn from a normal distribution with mean zero and variance equal to the within-person variance of earnings from 1978 to 2012 in the 1916 cohort. Thus, we obtain “simulated” earnings  $\tilde{E}_{ijt} = \hat{E}_{ijt} + u_{ijt}$ . This random term is conceptually necessary because higher moments (beyond the mean) of the earnings distribution help to determine the path of OASI benefits, and therefore we must model the year-to-year variance of earnings to correctly capture the path of OASI benefits. Specifically, we need to capture the fact that for some individuals in the 1916 cohort, earnings are sufficiently high in the post-retirement years to raise their AMW and therefore raise their OASI benefit—whereas this is not true for other individuals. Without the random term, the model predicts only mean earnings, with no variance, and therefore counterfactually under-predicts OASI benefits in the 1916 cohort (on which the simulated earnings measure is based), as mean earnings tend to fall from year to year in the elderly and near-elderly years.

We then calculate “simulated” OASI benefits  $B_{ijt}^{sim,T}$  for each individual  $i$  in the 1916 and 1917 cohorts in each calendar year  $t$  by assuming that each individual in each of these cohorts had simulated earnings  $\tilde{E}_{ijt}$  in each year, and by assuming that everyone (in both the 1916 and 1917 cohorts) claimed at the mean age of claiming in the 1916 cohort; this forms our measure of “simulated” OASI benefits. To calculate simulated benefits, we also assume that each individual claimed at the average claim age observed in the last 100 days of the 1916 birth cohort, and that each individual died at the mean age of death observed in the last 100 days of the 1916 birth cohort (with year of death imputed as 2013 for the one percent of this cohort that is still alive). The results are not sensitive to these assumptions,

as we would expect because benefits depend very little on (endogenous) earnings in the 1917 cohort.

The first-stage coefficient on the Notch dummy, when the dependent variable is simulated OASI benefits from 1978 to 2012, is -6,459.52 (standard error 636.89) without controls. Thus, the first stage is only slightly different for simulated benefits than for actual benefits (around 5 percent larger for simulated benefits than for actual benefits).

It is not surprising that the discontinuity in simulated benefits is similar to the discontinuity in actual benefits. In the 1916 cohort, simulated and actual benefits should be very similar if we have modeled earnings appropriately, since simulated benefits are calculated using the fitted values from a regression involving actual earnings in 1916. In the 1917 cohort, earnings in 1979 and after should not affect actual benefits through the channel of the transitional AMW, because the transitional AMW is unaffected by earnings in 1979 and after. Thus, the only ways in which earnings after the Notch legislation could affect the benefits of the 1917 cohort are: (a) earnings in 1977 and 1978 can affect benefits (this factor should *ceteris paribus* decrease the discontinuity in simulated benefits relative to the discontinuity in actual benefits); (b) claim age could have responded, though given that the actuarial adjustment is roughly actuarially fair this should have had a small effect on lifetime discounted benefits; (c) earnings can affect benefits through the Earnings Test, which reduces current benefits in proportion to earnings above an exempt amount; and (d) earnings can affect benefits through the interaction of the Earnings Test with the DRC and actuarial adjustment, which increase future benefits when current benefits are reduced due to the Earnings Test. Factors (c) and (d) should roughly cancel out given that the increase in later benefits described in (d) offsets the effect of the Earnings Test on immediate benefits described in (c). In principle, the change in the benefit schedule could also have affected mortality and thus affected realized benefits, though the magnitude of any such affect is moderate enough that it would at most only affect benefits modestly (see Appendix Figure 8 and Snyder and Evans 2006). In all, these affect benefits in relatively minor ways, and they offset each other to some extent.

Appendix Table 5 shows that the 2SLS estimates are only slightly smaller when the endogenous variable is simulated OASI benefits (rather than actual OASI benefits). Since the first stage is so similar when using actual and simulated benefits, this is the expected result.

We also calculate a simulated version of  $\mu_{ijt}$ , by calculating the marginal increase in OASI benefits if an individual increased earnings by \$1 relative to their simulated earnings  $\tilde{E}_{ijt}$ . Finally, we calculate a simulated version of the average tax rate  $ATR_{ijt} = [T_{jt}(\tilde{E}_{ijt}) - T_{jt}(0)]/\tilde{E}_{ijt}$ , calculated using simulated earnings  $\tilde{E}_{ijt}$ . We show the substitution elasticities calculated using simulated substitution incentives in Appendix Table 8.

## D Alternative Empirical Strategies

### D.1 Individual-Level Strategy

In principle, it would be possible to use an alternative empirical strategy, in which we effectively compared an *individual's* incentives to the *individual's* earnings decision. This would produce estimates that were weighted differently than those at the DOB-mean level: as in a standard differences-in-differences framework, if the income effect is heterogeneous across individuals and the size of the cut in benefits due to the Notch is correlated across individuals with the income effect, this model would estimate a weighted average of the income effects in the population with greater weight on individuals with larger cuts in benefits. By contrast, our DOB-mean-level estimates effectively use weights that reflect the causal effect of the Notch on earnings for the population at the boundary, which is relevant to analyzing the actual effects of the policy on aggregate earnings for this population. Thus, the DOB-mean-level estimates are policy-relevant and form our baseline.

To show the robustness of our results to the individual-level strategy, we exploit variation in the benefit formula at the boundary to drive the regression estimates, relating the size of an individual's policy-related change in OASI benefits to the individual's earnings. As in Appendix C, we follow a “simulated instruments” strategy (*e.g.* Gruber and Saez 2002). As in Appendix C, in a first step, we run regressions to simulate earnings using the experience of the 1916 cohort. Specifically, we run a separate regression of earnings  $E_{ijt}$  on earnings before the policy change—in 1975 and 1976—for each year of earnings  $t$ :

$$E_{ijt} = \psi_0 + \psi_1 E_{ij1975} + \psi_2 E_{ij1975}^2 + \psi_3 E_{ij1976} + \psi_4 E_{ij1976}^2 + \varepsilon_{ijt} \quad (\text{D.12})$$

We then obtain the fitted values  $\hat{E}_{ijt}$  given each individual's actual earnings in 1975 and 1976. As in Appendix C, we add a random noise term to  $\hat{E}_{ijt}$ , with mean zero and variance calculated from the variance of earnings in the 1916 cohort to capture heterogeneity in earnings, to obtain “simulated” earnings  $\tilde{E}_{ijt} = \hat{E}_{ijt} + u_{ijt}$ .

In the second step, as in Appendix C we calculate simulated benefits in each year  $t$ ,  $B_{ijt}^{sim,T}$ , by applying cohort  $T$  benefit rules to the set of  $\tilde{E}_{ijt}$  across all years  $t$ ,  $\{\tilde{E}_{ijt}\}$ .

In the third step, for each individual in the 1917 cohort in each calendar year  $t$ , we calculate the change  $\Delta B_{ijt}^{sim}$  in the yearly benefit amount due to policy variation:

$$\Delta B_{ijt}^{sim} = B_{ijt}^{sim,1917} - B_{ijt}^{sim,1916} \quad (\text{D.13})$$

In other words,  $\Delta B_{ijt}^{sim}$  represents the change in an individual's simulated benefit due to being born in 1917 rather than 1916. For the 1916 cohort, we set  $\Delta B_{ijt}^{sim} = 0$ , to reflect the fact that they did not experience any policy change.

In the final step, for the 1917 cohort we define  $\Delta B_{ijt}^{act}$  as the difference in an individual's benefit due to being born in 1917 rather than 1916 under the individual's *actual* (rather

than simulated) earnings history:

$$\Delta B_{ijt}^{act} = B_{ijt}^{act,1917} - B_{ijt}^{act,1916} \quad (D.14)$$

For the 1916 cohort, we set  $\Delta B_{ijt}^{act} = 0$ , again to reflect the fact that they did not experience any policy change. We then calculate the discounted value of the change in simulated benefits,  $\Delta B_{ijPDV}^{sim}$ , and the discounted value of the change in actual benefits,  $\Delta B_{ijPDV}^{act}$ , and we run a 2SLS regression in which we use  $\Delta B_{ijPDV}^{sim}$  to instrument for  $\Delta B_{ijPDV}^{act}$ , clustering by DOB:

$$\Delta B_{ijPDV}^{act} = \alpha_0 + \alpha_1 \Delta B_{ijPDV}^{sim} + \alpha_2 DOB_j + \alpha_3 (D * DOB)_j + \Gamma_t \alpha_4 + \varepsilon_{ij} \quad (D.15)$$

$$E_{ijPDV} = \beta_0 + \beta_1 \Delta B_{ijPDV}^{act} + \beta_2 DOB_j + \beta_3 (D * DOB)_j + \Gamma_t \beta_4 + \eta_{ij} \quad (D.16)$$

In (D.16), the dependent variable is  $E_{ijPDV}$ , the present discounted value of earnings from 1978 to 2012 (or another period), for each individual  $i$  born on DOB  $j$ .

The rationale behind this strategy is that we calculate the actual and simulated benefit cuts an individual experiences from the Notch policy, and we let the simulated benefit cut instrument for the actual. We then relate discounted earnings to the benefit cut at the individual level, using the discontinuous policy variation at the cohort boundary to drive the estimates by controlling for a linear spline in DOB.

In Appendix Table 7, we present the results of this individual-level strategy from the 2SLS regression (D.15)-(D.16). These results prove to be similar to our DOB-mean-level results. Column 1 shows a point estimate of the lower bound on the income effect of -0.63, similar to our baseline estimate of -0.61 from the DOB-mean-level estimates. Column 2 shows that under this strategy a \$10,000 increase in lifetime discounted OASI benefits causes a 2.67 percentage point decrease in the yearly employment rate, which is substantially larger than the baseline estimate in Table 6. Adding a term for substitution incentives to this regression shows insignificant coefficients on the substitution incentives, similar to the baseline DOB-mean-level results in Table 8.

## D.2 Comparing 1978 and 1979 for Substitution Elasticity Estimates

We perform our estimate of the substitution elasticity by comparing 1978 and 1979 because these years are adjacent and therefore most closely comparable, aside from the sharp change in substitution incentives across the two years. It would alternatively be possible to perform our estimates across a wider range of years, but other pairs of years are likely to be less comparable. In other words, it seems less likely the assumptions of  $X_{j1978} \approx X_{jt}$ ,  $\tau_{j1978} \approx \tau_{jt}$ , or  $\lambda_{j1978} \approx \lambda_{jt}$  would hold if we chose years  $t$  other than the adjacent year of  $t = 1979$ .

Investigating a wider range of years could also lead to additional issues. For example, the net returns to extra work  $\mu_{jt}$  are highest in the 1916 cohort at younger ages, when the lifetime consequences for OASI benefits of a given change in earnings tend to be largest because they are discounted over many future years of benefit receipt. At the same time,

those in the late elderly years tend to have very low earnings and may be less responsive to changes in incentives than those at younger ages (as our empirical results in Figure 6 suggest). Thus, there could be a negative correlation across ages between the discontinuity at the cohort boundary in the incentive for extra work and the discontinuity in average earnings—as younger ages show a larger decrease at the boundary in the incentive for extra work *and* a larger increase in earnings at the boundary—despite the fact that no individual in the population has a negative elasticity in a standard model (as this would violate the restrictions imposed by standard theory, specifically that the substitution effect is weakly positive).

Because of such issues, we instead estimate the substitution effect by comparing behavior at adjacent ages that appear as closely comparable as possible.

### D.3 Examining Approximate Percentage Changes in Earnings from 1978 to 1979

Much as we investigated the log odds of participation, the mean level of earnings is different in 1978 and 1979, which could lead us to wish to investigate percentage changes in earnings from 1978 to 1979 as an alternative specification. Thus, earnings could in principle be related to the substitution incentive through a log-log specification in which log earnings are a function of the log substitution incentive. However, the log of zero is undefined, which is an important issue in our context because many older individuals have zero earnings. Thus, to address this issue we approximate the log of earnings using the inverse hyperbolic sine (IHS) of earnings  $E$ , which is defined as  $IHS(E) = \ln(E + \sqrt{1 + E^2})$ . The IHS transformation is well known in the statistics literature; it approximates log earnings for large values of the argument but is defined at zero and negative values (*e.g.* Burbidge *et al.* 1988, Pence 2006, or Gelber 2011). If  $IHS(E_{ijt}) = \beta\lambda_{ij} + \delta IHS(1 + \mu_{ijt} - \tau_{ijt}) + \theta X_{ijt}$ , then parallel to (11), taking means by DOB we have:

$$\Delta IHS(E)_j \equiv IHS(E)_{j1979} - IHS(E)_{j1978} = \delta \Delta IHS(1 + \mu - \tau)_{j1979} \quad (D.17)$$

where  $\Delta IHS(1 + \mu - \tau)_{j1979} \equiv IHS(1 + \mu - \tau)_{j1979} - IHS(1 + \mu - \tau)_{j1978}$ . We use a subscript after the parentheses rather than before to indicate that we are taking the mean by DOB of the IHS of the indicated quantity, as opposed to taking the IHS of the mean of the variable (the latter of which would not be consistent with theory).

We implement this empirically through the following two-stage least squares specification, using the Notch dummy to instrument for the change in the substitution incentive, where (D.18) is the first stage and (D.19) is the second stage:

$$\Delta IHS(1 + \mu - \tau)_{j1979} = \rho_1 D_j + \rho_2 DOB_j + \rho_3 (D * DOB)_j + \zeta_j \quad (D.18)$$

$$\Delta IHS(E)_j = \omega_1 \Delta IHS(1 + \mu_{j1979} - \tau_{j1979}) + \omega_2 DOB_j + \omega_3 (D * DOB)_j + \epsilon_j \quad (D.19)$$

Appendix Figure 9 and Appendix Table 8 show that  $\Delta IHS(E)_j$  shows no significant change at the boundary. Rows G and H of Appendix Table 8 implement the 2SLS specification (D.18)-(D.19); they show comparable results to those in the levels specification. All

of these results are similar when we use  $\Delta \log(1 + E)_j$  as the dependent variable rather than  $\Delta IHS(E)_j$ .

## E Interpreting Estimates in Light of Inflation

It is relevant to discuss the interpretation of our estimates in light of the fact that inflation affected the size of the income effect. Greater inflation translates into higher nominal earnings in the 1916 cohort, which led to larger AMWs in this cohort, which in turn led to greater OASI benefits in this cohort. In the 1917 cohort, meanwhile, the transitional AMW was unaffected by inflation subsequent to 1978. (We refer AMW calculated under the transitional guarantee as the “transitional AMW.”) Thus, as inflation increased, the size of the discontinuity at the cohort boundary in benefits paid increased. Similarly, the size of the discontinuity in substitution incentives also increased as inflation increased.

It is possible that the unanticipated inflation of the late 1970s and early 1980s led to a larger realized discontinuity in benefits than was anticipated in 1977—and in this case, we might expect the measured discontinuity in earnings in these early years to *understate* the discontinuity that would have occurred if the full magnitude of the discontinuity in benefits were known throughout. In this sense, our estimates of income effects could be considered lower bounds on the effect that would have occurred if the realized discontinuity in benefits were known with certainty in 1977—thus reinforcing our later conclusion that the observed effects on earnings are very large, and that our income effect estimates reflect lower bounds.

In the lifecycle framework we present, only unanticipated inflation would have caused the discontinuity in expected lifetime OASI benefits at the boundary to be different in 1979 than in 1978. In the lifecycle framework, earnings decisions depend on the marginal utility of wealth and therefore on lifetime OASI benefits, which in turn depend on inflation over a long period (*e.g.* 1977 to 2012). As a first pass one could assume that expectations are rational so that expectations of inflation over a long period approximately match realized inflation. To the extent that we can address how realized inflation diverged from this benchmark, Federal Reserve data show that inflation expectations were below realized inflation in 1978 and 1979, but expectations generally closely matched realized inflation in years since (Mehra and Reilly 2008).<sup>44</sup> In principle, changing inflation expectations could have influenced earnings in 1979 relative to 1978. However, Mehra and Reilly (2008) show an increase in expected inflation from 1978 to 1979 around two percentage points, which plausibly could have caused only a modest change in expected lifetime benefits—and in turn a modest change in earnings that would have been small relative, for example, to the \$905 discontinuity in earnings we would have expected with a substitution elasticity of 0.25.

In fact, the evidence more broadly appears at odds with the hypothesis that inflation mattered greatly for the crowdout estimates. If lifetime inflation expectations were highest around 1980 when inflation was highest, then we might expect the most crowdout during this period and less by the mid-1980s. In fact we observe no significant earnings crowdout during the period from 1978 to 1980 with the highest inflation, whereas we observe the largest degree of earnings crowdout in the mid-to-late 1980s when inflation was low. Thus,

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<sup>44</sup>However, data are not available to assess how well expectations matched reality in 1977.

to the extent the data can speak to the effects of inflation on the estimates, the results do not support the hypothesis that the large earnings crowdout was related to overly high inflation expectations, or that unanticipated inflation in 1978 or 1979 caused a notable reaction.

Finally, if unanticipated inflation did have a substantial effect on earnings decisions, the size of the discontinuity at the 1917/1918 boundary in benefits, and therefore earnings, should increase from 1978 to 1979. Unlike the 1916/1917 boundary, the substitution incentive was continuous at the 1917/1918 cohort boundary in both 1978 and 1979. Appendix Figure 10 shows that there is no visually or statistically apparent discontinuity in the first-difference in earnings between 1978 and 1979 at this boundary. However, we have less statistical power at this boundary.

Inflation expectations should not influence behavior if earnings decisions are made in a static framework where individuals are myopic or cannot transfer capital across periods (*e.g.* due to liquidity constraints), consistent with our evidence in Section 5.4. This could be an explanation for our finding that the evidence appears at odds with the hypothesis that inflation mattered greatly for the crowdout estimates.

## F Comparison to Krueger and Pischke (1992) and Snyder and Evans (2006)

The work of Krueger and Pischke (1992) started the literature on the Notch, and it is worth considering the relationship between their results and ours. Of course, our results encompass both men’s and women’s responses, and therefore we investigate a new group. Aside from this difference in the sample, there are three primary differences between their empirical strategy and ours:

1. We use SSA administrative data on the full population, whereas KP use the data that were available to them at the time, CPS March Supplement survey data on less than 0.1 percent of the population;
2. Our variation is based on daily variation in Social Security benefits across birth cohorts and an RDD design, whereas KP’s identification is primarily based on variation in Social Security benefits across yearly birth cohorts—again the best data available to them at the time;
3. KP’s primary outcome of interest is the log odds ratio of labor force participation of men in these cohorts and this age group, whereas we investigate the probability or log odds of positive earnings (as we do not have data on those looking for work that are measured in the CPS).<sup>45</sup> One benefit of the CPS data is the ability to examine the probability of labor force participation, and hours worked, both of which KP analyze.

Turning to the results, it is first worth noting that KP rule out that the Notch has more than a moderate impact on male labor force participation—ruling out an increase in the log

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<sup>45</sup>In a different specification KP estimate the effect of OASI benefits on log hours worked; we cannot estimate the effect on this outcome in our data because we do not observe hours worked.



odds of participation of more than 0.059 in a representative specification—and we indeed estimate only a moderate impact of the Notch on male participation. Nonetheless, at the same time our estimates indicate large earnings crowdout among men, as well as in the full population. Thus, our findings are consistent with KP’s, while also showing a moderate participation effect corresponded to large earnings crowdout.

Moreover, the effect on participation that we estimate for men is insignificantly different from the KP estimate. Our RDD regression results for the 1916 and 1917 cohorts applied to the KP ages of 60 to 68 show an elasticity of men’s log odds of participation in these years with respect to lifetime discounted benefits of -0.66 (standard error 0.21), which is insignificantly different from their estimated elasticity of 0.105 (standard error 0.265,  $p > 0.10$ ).

To illuminate further the reasons that when focusing on men alone, KP estimate insignificant effects on participation and we estimate moderate but significant effects, we can assess whether we obtain empirical results similar to those of KP when we run specifications parallel to theirs using our SSA data on men. Specifically, using our SSA data on men aggregated to the birth cohort-calendar year level (as KP do), we keep the same sample of ages and years as Krueger and Pischke (1992) and implement the same specifications:

$$E_{ij} = \alpha B_{ij} + \Gamma_{ij} + \Psi_j + \varepsilon_{ij} \quad (\text{F.20})$$

where  $E_{ij}$  represents the log odds participation rate in cohort  $i$  in year  $j$ ;  $B_{ij}$  is log mean lifetime discounted OASI benefits in this cohort and year;  $\Gamma_{ij}$  represent age fixed effects;  $\Psi_j$  represent year fixed effects; and  $\varepsilon_{ij}$  is an error. In our SSA data participation for an individual in a given year is defined as having positive earnings in that year. We alternatively omit or include year dummies, paralleling their specifications. We choose the same sample as Krueger and Pischke, which they call their “Notch” sample: men born 1916 to 1922 (inclusive) observed between ages 60 and 68.

Appendix Table 9 shows the estimated effects of log OASI benefits on the log odds ratio of the participation rate. We never find negative and significant coefficients, mirroring KP’s findings. In a specification with both age and year dummies, the confidence interval rules out that the change in OASI benefits due to the Notch caused more than a moderate change in the log odds of participation. Moreover, when we use a 0.1 percent random sample of the SSA data (which has a sample size closer to that of KP), we also never estimate a significant coefficient on log lifetime OASI benefits.

We conclude that when identifying using cross-cohort variation, we confirm KP’s finding of no negative effect of OASI benefits on participation. This suggests that a primary reason for the difference in results is the difference in the level of variation examined (*i.e.* daily *vs.* yearly). The variation at the DOB level is fine-grained enough to pick up moderate participation effects that do not appear at the cohort level, which is potentially subject to other shocks (Handwerker 2011). Moreover, our findings are very similar when using CPS data to implement KP’s strategy but instead defining participation as a dummy for positive earnings, parallel to our definition in the SSA data. In sum, our view is that KP’s paper

examining the Notch was both an innovation in focusing attention on the unique Notch variation, and was executed as well as allowed by the data available at the time.

Although the primary analysis of Snyder and Evans (2006) concerns the effect of the Notch on mortality of men, these authors also briefly examine the effect of the Notch on male participation rates in the CPS as a secondary analysis. Snyder and Evans do not estimate the effect on earnings, which is our primary contribution, and they do not examine income or substitution effects (as such analysis is not directly relevant in their context). Since Snyder and Evans use data from the CPS, their data have all the same limitations and benefits as discussed above in the KP context (including having smaller samples than ours, but also having the ability to measure hours worked and labor force participation). Snyder and Evans estimated that the effect of the Notch on the probability of working is four times as large as our estimated effect, again demonstrating that our more granular data can show substantially different results.

## G Calculation of Fraction of Variation in Employment Accounted for by OASI Benefit Changes

This Appendix describes how we calculate the fraction of the change in the slope of the employment rate in the mid-1980s that can be accounted for with changes in the growth rate of OASI benefit levels. From 1979 to 1984 the employment-to-population ratio among those 65 and over decreased by 0.66 percentage points per year on average, whereas it rose by 0.17 percentage points per year on average from 1985 to 1990. Meanwhile, from 1979 to 1984 the mean real annual OASI benefit rose by \$320.45 per year on average, but due largely to the 1977 Amendments it rose by only 67.85 per year on average from 1985 to 1990 (Social Security Administration 2015b). To apply our estimates of the discontinuity in participation, among ages 65 and over examined in Figure 1, Appendix Table 2 estimates an effect of the Notch on the participation rate from 1982 to 2012 of 0.38 percentage points, and we find a discontinuity in mean yearly benefits over these years of \$310.65. Thus, we find that the slowdown in the growth rate of OASI benefits can account for 37.2 percent of the actual change in the participation rate growth rate around 1985 (*i.e.*  $0.38 \times (320.45 - 67.85) / (310.65 \times (0.17 + 0.66)) = 37.2$  percent). Our estimates for the 65-69 year-old population quoted in the main text—namely that the change in the growth rate of benefits can account for 74.9 percent of the change in the growth rate of the employment rate for this group—rely on analogous calculations for this group, as do our calculations of the fraction of the decline in the level of the employment rate from 1950 to 1985 that can be accounted for through the increase in OASI benefits over this period, as do our calculations relying on the static model.

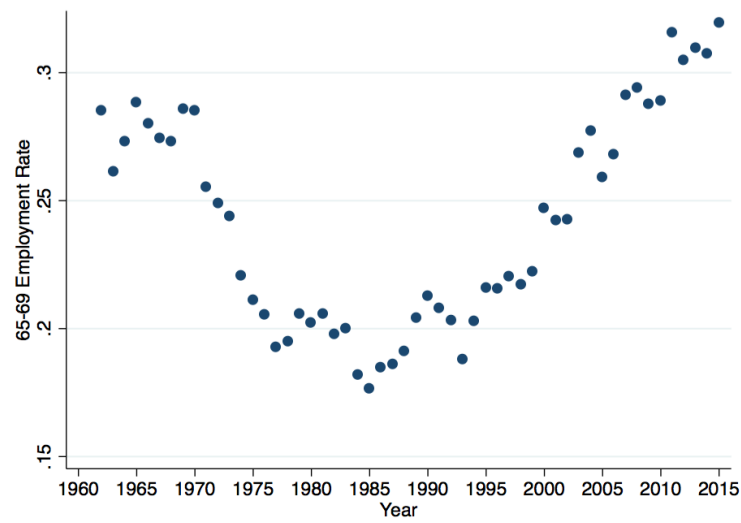
These statistics are from the Current Population Survey, not our SSA data. Nonetheless, our calculation illustrates that changes in the OASI benefit growth rate can account for a substantial fraction of the increase in the growth rate of elderly participation. Our SSA data are only for the cohorts near the Notch cohorts, so we are unable to calculate the fraction with positive earnings in earlier years in our data. Other empirical choices, such as estimating the change in slope from other sets of years around 1985, yield similar conclusions.

This calculation implicitly assumes that the effects are transmitted through contempo-

aneous benefits or through lifetime benefits, consistent with our results. We ignore substitution elasticities in this calculation since our results suggest they were not important. In other contexts—for example with more salient substitution incentives—substitution elasticities could be larger. Since the OASI replacement rate secularly rose over the second half of the 20th century, incorporating the effects of substitution incentives would if anything strengthen our conclusion that OASI benefit changes can account for an important part of the changes in the employment-to-population ratio.

A number of other issues could arise in determining the implications of our estimates for the time series of the employment rate. For example, if spousal leisure is complementary (substitutable), this would suggest that the change in the OASI benefit growth rate could account for a larger (smaller) fraction of the change in the growth rate of the employment rate. Generally, our estimates do not capture general equilibrium impacts of the OASI benefit changes, which could matter as real OASI benefits increased by a factor of around 2.5 from 1950 to 1985. We also ignore the possibility that increases in OASI benefits over time affected earlier earnings histories materially (which could then affect OASI benefits themselves). Overall, we view our calculations of the implied effect of OASI on the elderly participation rate as merely illustrative of the order of magnitude of the implications of rising OASI benefits over time. That said, this implied effect appears to be quite large.

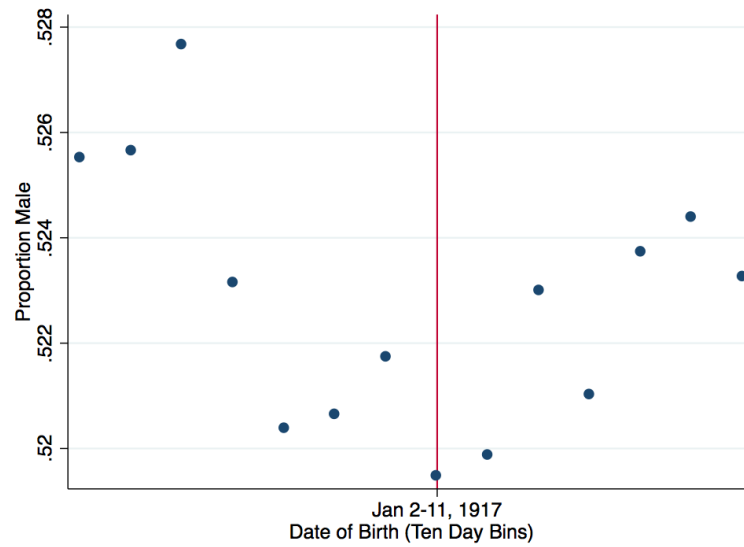
**Appendix Figure 1.** *Age 65-69 employment-to-population ratio by calendar year*



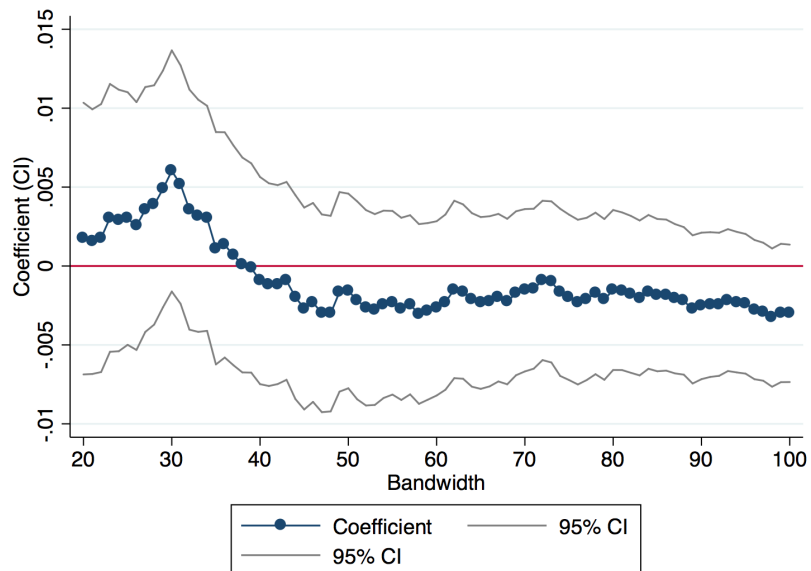
Notes: The figure shows the employment-to-population ratio for those 65 to 69 years old, by year from 1962 to 2015. The data on the employment-to-population ratio come from the Current Population Survey.

Appendix Figure 2.

(a) Proportion male, by 10-day DOB bin



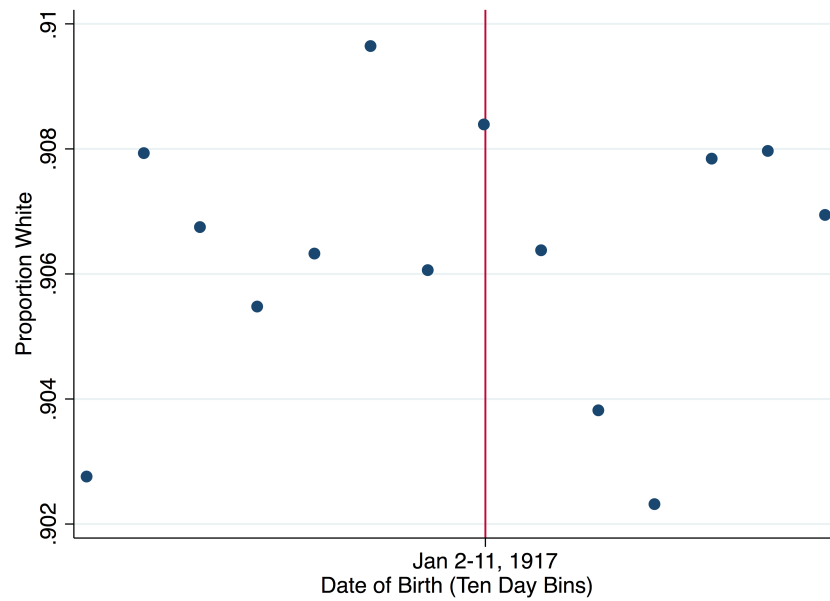
(b) Coefficients and confidence intervals by bandwidth



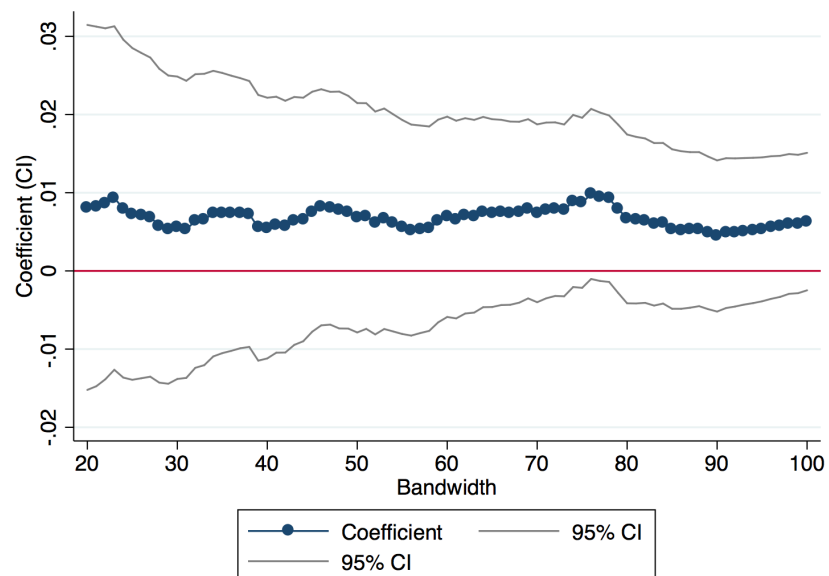
See notes to Figure 2.

**Appendix Figure 3.**

*(a) Proportion white, by 10-day DOB bin*



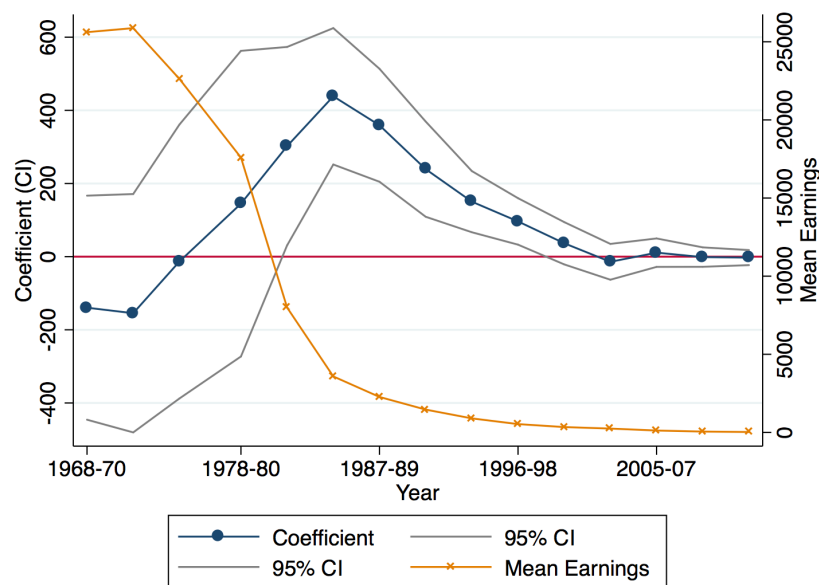
*(b) Coefficients and confidence intervals by bandwidth*



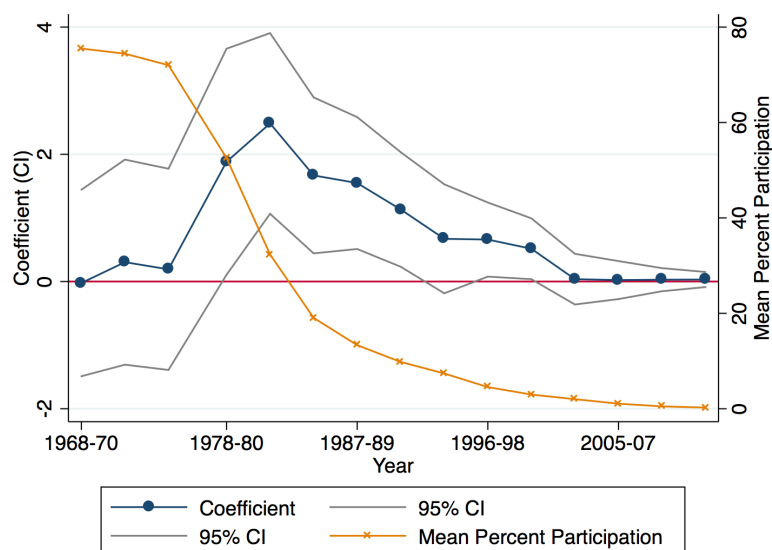
See notes to Figure 2.

**Appendix Figure 4.** *Effect of Notch on earnings and participation by time period, relative to sample means, among those who lived until at least 80 years old*

*(a) Effect on earnings*



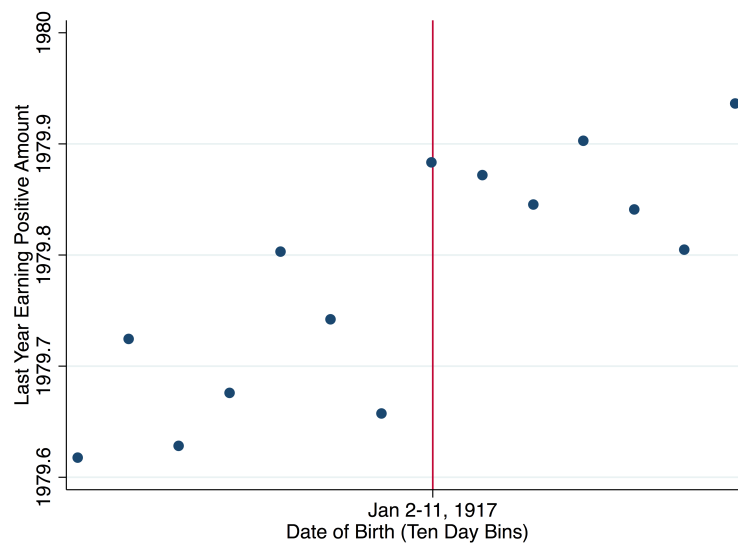
*(b) Effect on participation*



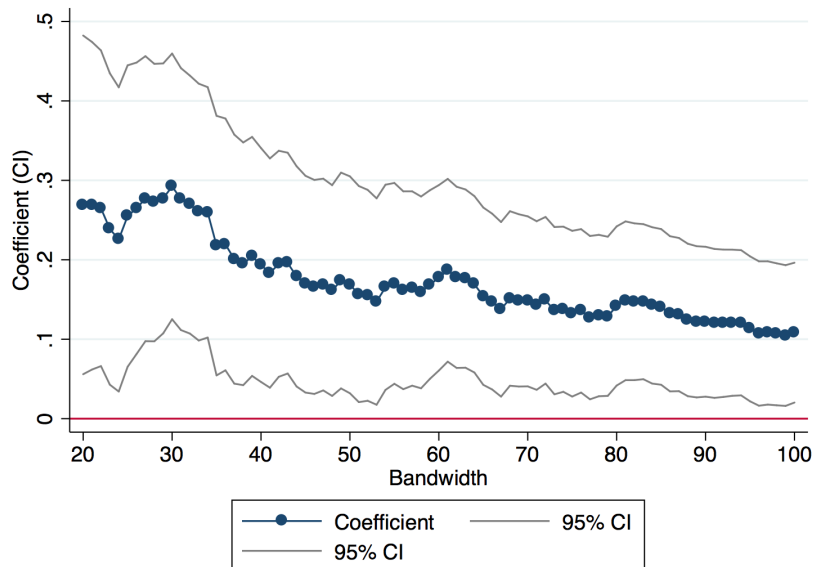
Notes: The sample consists of those who lived until at least 80 years old; the 1917 cohort turns 80 in 1997. This allows us to compare a constant sample over time until 1997. The graphs look similar to those for the full population (Figure 6). The graphs are also similar when we choose a sample of people who lived until other ages, such as 75 or 85. See other notes to Figure 6.

## Appendix Figure 5.

(a) Last calendar year an individual earned a positive amount, by 10-day DOB bin



(b) Coefficient and confidence interval by bandwidth

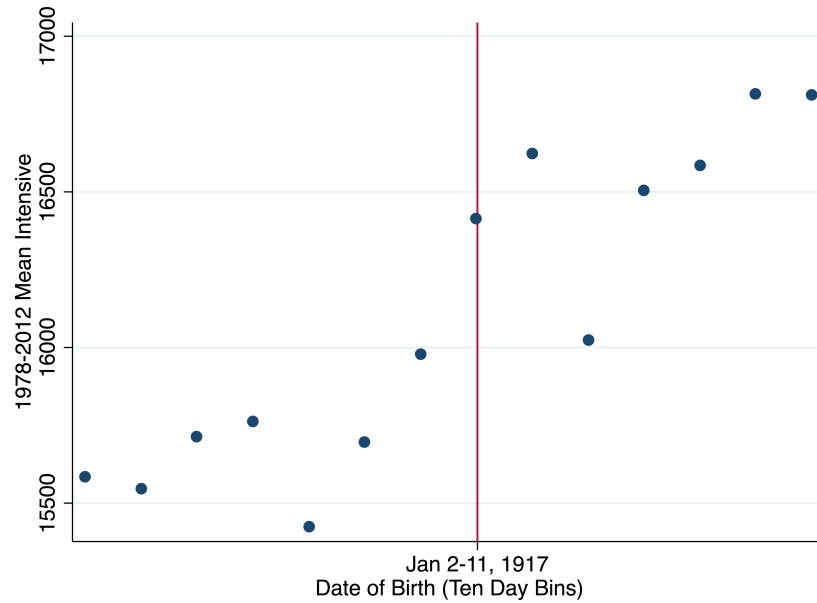


See notes to Figure 4. The 1917 cohort turned 62 in 1979 and turned 63 in 1980.

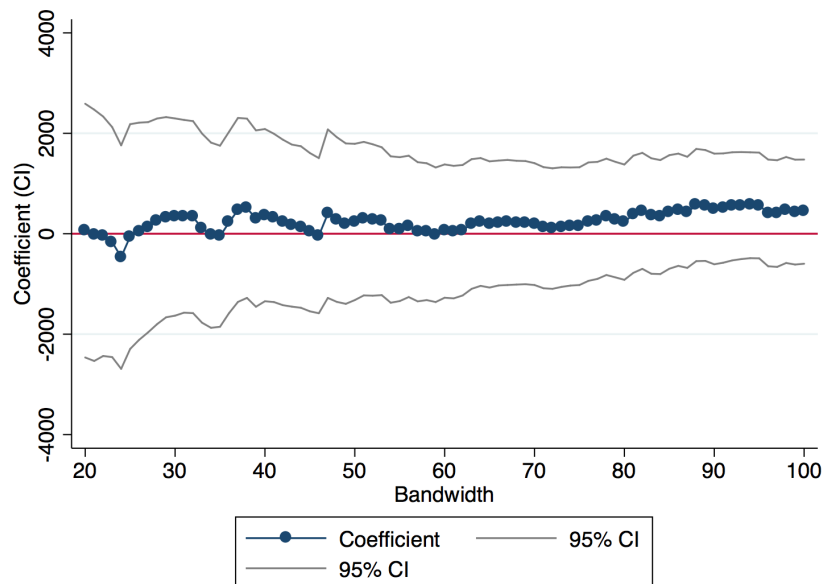


## Appendix Figure 6.

(a) *Intensive margin: mean yearly earnings (conditional on positive earnings in that year), 1978 to 2012 (ages 61 to 95), by 10-day DOB bin*



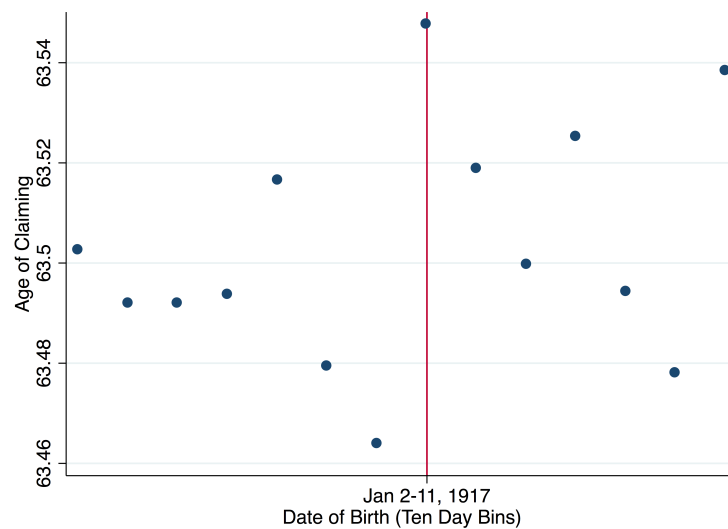
(b) *Coefficients and confidence intervals by bandwidth*



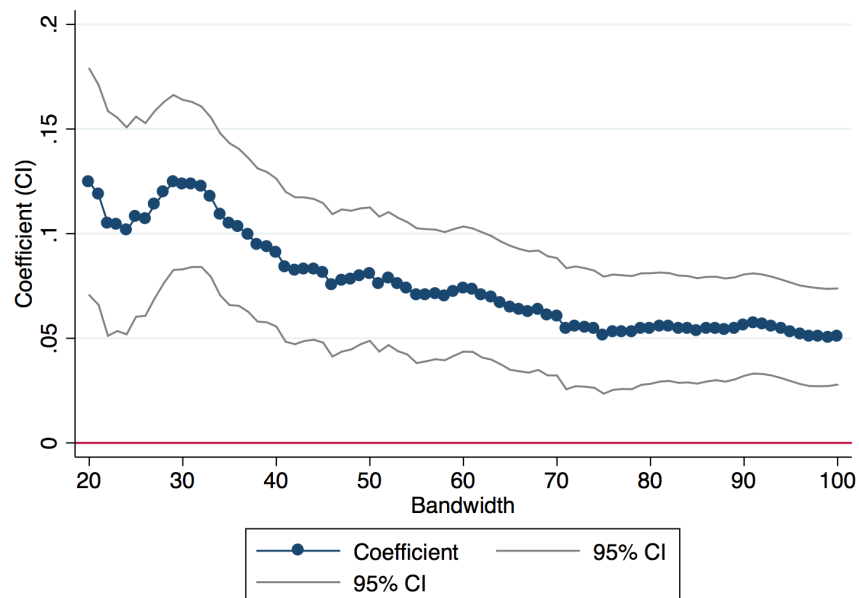
Notes: The figure is parallel to Figure 4, except that in this appendix figure, the outcome of interest is mean yearly earnings, conditional on having positive earnings in that year, from 1978 to 2012. See other notes to Figure 4.

## Appendix Figure 7.

(a) Mean age of initial OASI claim by 10-day DOB bin

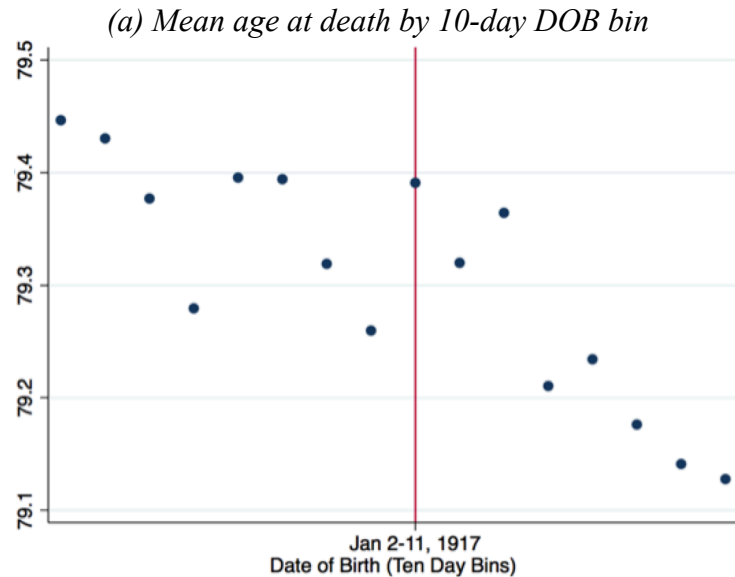


(b) Coefficients and confidence intervals by bandwidth

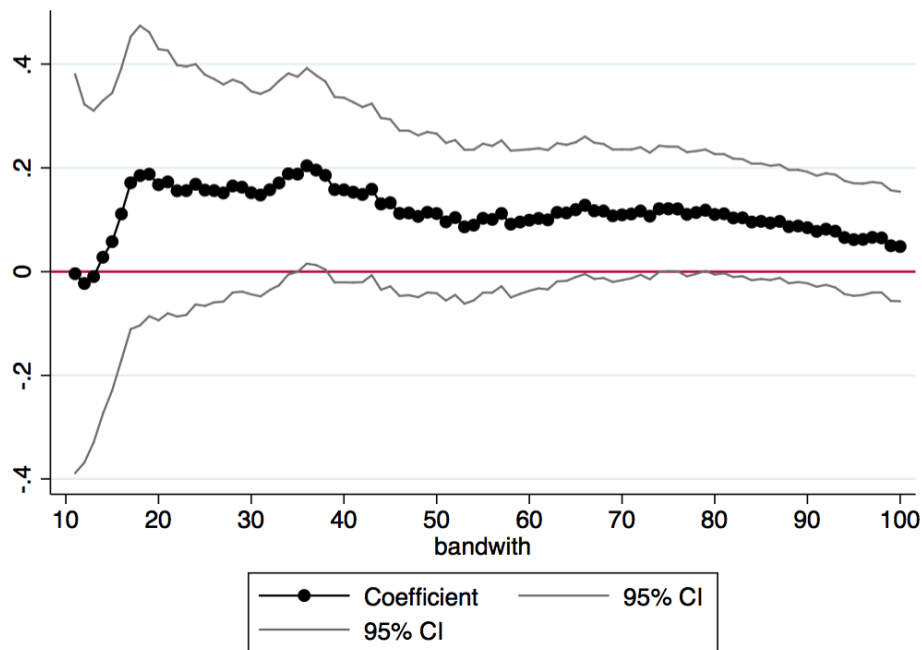


Notes: The figure is parallel to Figure 3, except that this appendix figure shows results for the mean age of initially claiming OASI. These claim age results are difficult to interpret since all claimants claim on the first of the month, creating a discontinuity in claim age on the first of every month, including January 1917. See other notes to Figure 3.

**Appendix Figure 8. *The Notch and age at death***



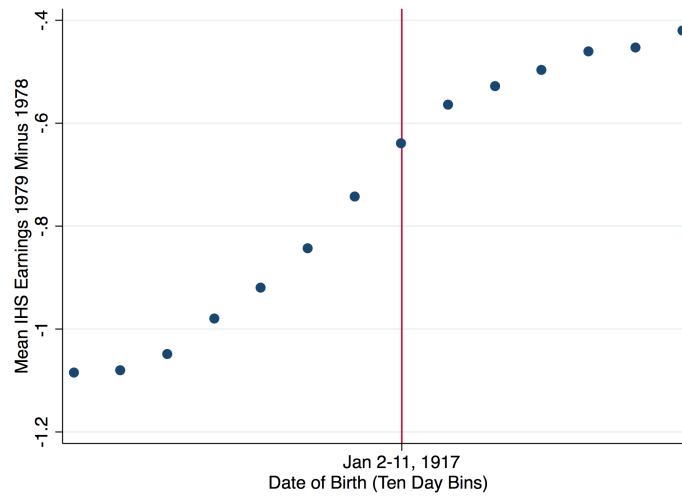
(b) Coefficients and confidence intervals by bandwidth



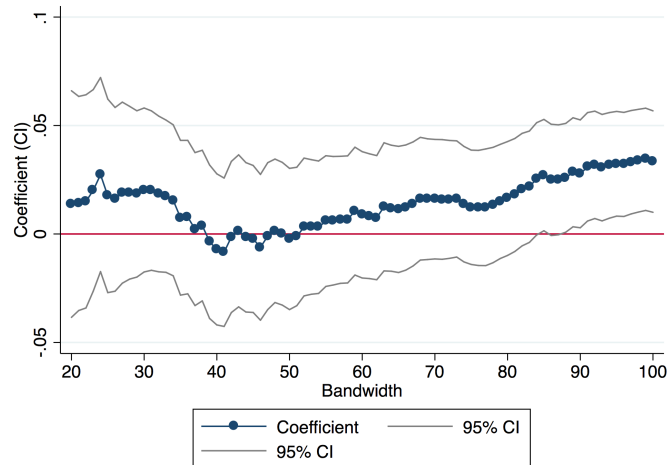
Notes: The figure is parallel to Figure 3, except that this appendix figure shows results for mean age at death. The figure shows that there is no robust evidence for an effect on age at death, and that the point estimates are not large. Those alive in 2012 are coded as having an age of death of 97. When examining the outcome of the probability of being alive by 75, 80, 85, or other thresholds, we find similar (usually insignificantly positive) results. See notes to Figure 3.

### Appendix Figure 9.

(a) Difference in mean IHS yearly earnings, 1979 minus 1978



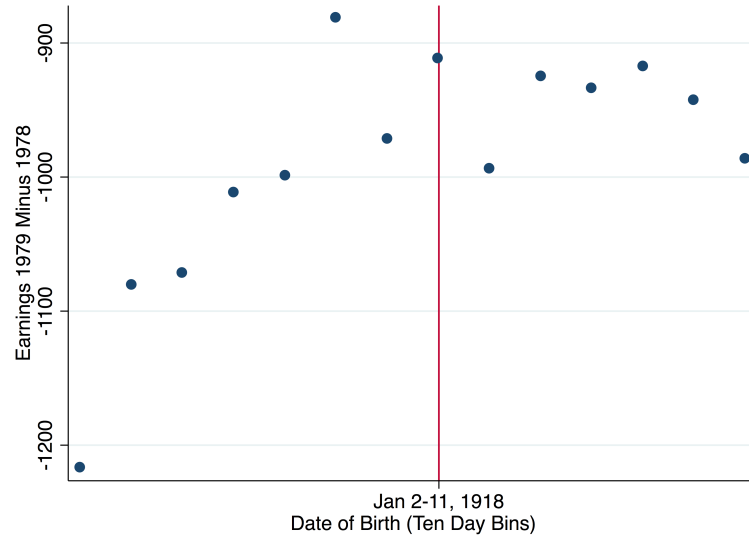
(b) Results by bandwidth for difference in mean IHS yearly earnings, 1979 minus 1978



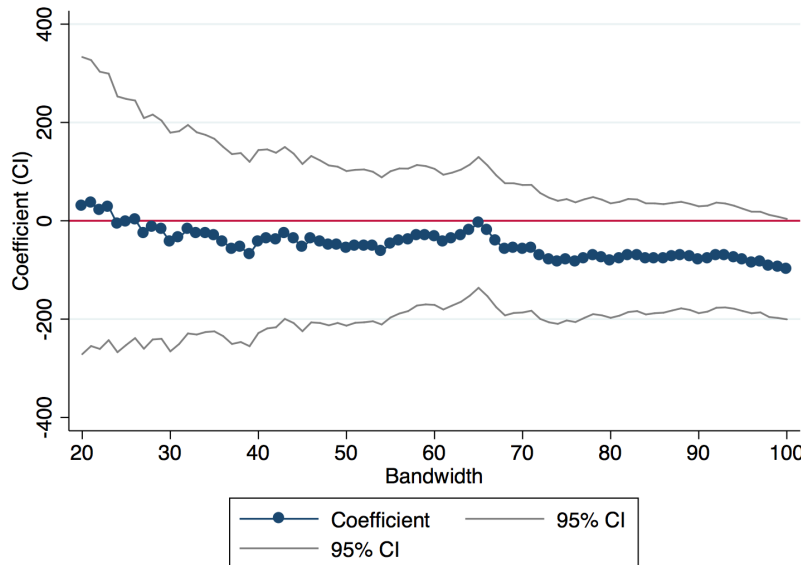
Notes: The figure is parallel to Figure 8 Panels (a) and (d), except that this appendix figure shows results for the inverse hyperbolic sine (IHS) of earnings in 1979 minus the IHS of earnings in 1978. As discussed in the Appendix, the IHS approximates the log function for large values of its arguments, but is defined when the argument is zero. Thus, changes in the IHS of earnings from 1978 to 1979 approximately reflect percentage changes in earnings between these years. The figure shows that there is no robust discontinuity at the cohort boundary in the difference in the IHS of earnings from 1978 to 1979. Relative to the results shown in Figure 8, this addresses the fact that the mean level of earnings changes over these years. See other notes to Figure 8.

### Appendix Figure 10.

(a) Mean yearly earnings, 1979 minus 1978, 1917/1918 cohort boundary, by 10-day DOB bin



(b) Coefficients and confidence intervals by bandwidth



Notes: The figure is parallel to Figure 8 Panels (a) and (d), except that this appendix figure shows results for mean yearly earnings in 1979 minus mean yearly earnings in 1978, *around the 1917/1918 cohort boundary*. If unanticipated inflation had a substantial effect on earnings decisions, the size of the discontinuity at the 1917/1918 boundary in benefits, and therefore earnings, should increase from 1978 to 1979. Unlike the 1916/1917 boundary, the substitution incentive was continuous at the 1917/1918 cohort boundary in both 1978 and 1979. Appendix Figure 10 shows that there is no visually or statistically apparent discontinuity in the first-difference in earnings between 1978 and 1979 at this boundary. See other notes to Figure 8.

**Appendix Table 1. Effect of Notch on earnings and employment: alternative bandwidths**

Outcome	Chosen bandwidth	Coefficient (SE)
<b>Panel A: Placebo Outcomes</b>		
A) Earnings, 1974 to 1976	85	-60.06 (128.12)
B) Participation, 1974 to 1976	91	0.054 (0.21)
<b>Panel B: Main Outcomes</b>		
C) Positive earnings probability, 1978 to 2012	70	0.36 (0.077)***
D) Log odds of mean participation dummy	70	0.037 (0.0079)***
E) Last year earned positive amount	79	0.14 (0.05)***
F) IHS Earnings 1979 minus IHS Earnings 1978	62	0.013 (0.015)

Notes: The table shows the results of OLS regressions corresponding to model (1) in the text. These results differ from those shown in the tables in the main text only because in this appendix table we show the results for each outcome using the CCT bandwidth chosen for that outcome separately. See other notes to Table 2. The estimates are extremely similar with controls.

**Appendix Table 2. Effects of Notch on additional outcomes**

Outcome	Coefficient (SE)
A) Mean yearly earnings, 1978-2012, conditional on positive earnings in that year	39.12 (707.11)
B) Earnings in 1977	161.95 (151.62)
C) Earnings in 1978	263.60 (158.71)
D) Mean yearly earnings, 1978-2012	135.47 (29.91)***
E) Percent of years with positive earnings, 1982-2012 (ages 65 to 95)	0.38 (0.075)***
F) Mean yearly OASI benefit, 1978-2012	-310.65 (46.23)***
G) Percent of years with positive earnings, 1982-1986 (ages 65 to 69)	0.99 (0.23)***
H) Mean yearly OASI benefit, 1982-1986	-615.83 (34.80)***

Notes: The table shows the results of OLS regressions corresponding to model (1) in the text, with the dependent variable shown in the “outcome” column. The estimates are extremely similar with controls. See other notes to Table 3.

**Appendix Table 3. Summary statistics in IRS data**

Cohort	(1) Earnings Mean (SD)	(2) Participation Mean (SD)
A) 1923/1924	\$3,034.33 (307.07)	2.65 (0.14)
B) 1925/1926	\$4,700.94 (431.64)	3.65 (0.18)
C) 1927/1928	\$7,381.78 (612.26)	5.03 (0.25)
D) 1929/1930	\$11,306.38 (718.11)	6.91 (0.33)
E) 1931/1932	17,638.28 (991.00)	9.24 (0.39)
F) 1933/1934	\$28,021.25 (1,556.13)	12.22 (0.46)
G) 1935/1936	\$45,927.34 (2,120.36)	15.88 (0.60)

Notes: The earnings summary statistics in Column 1 refer to discounted real earnings from 1999 to 2013, for 100 days around each of the cohort boundaries shown. The participation summary statistics in Column 2 show the percent of years with positive earnings from 1999 to 2013 for those around each cohort boundary. These means and standard deviations in the IRS data for these cohorts are moderately larger than those in the SSA data for 1916/1917 over the comparable set of ages; this is due to the secular trend of increasing elderly participation and earnings across cohorts from 1917 to 1937. See other notes to Tables 1 and 5.

**Appendix Table 4. Robustness to discount rate**

	(1) 1 Percent	(2) 2 Percent	(3) 4 Percent	(4) 5 Percent
A) Discounted earnings, 1978 to 2012	-0.54 (0.15)***	-0.58 (0.16)***	-0.65 (0.18)***	-0.69 (0.19)***
B) % of years with positive earnings, 1978 to 2012	-0.49 (0.13)***	-0.56 (0.15)***	-0.74 (0.20)***	-0.84 (0.22)***
Controls?	N	Y	N	Y

Notes: Like Table 6, this table shows the results of the 2SLS regressions (2) and (7) from the main text, with the dependent variable shown in the first column. Columns 1 through 4 show the results when we vary the discount rate over 1, 2, 4, and 5 percent respectively, rather than the 3 percent discount rate assumed in the baseline in Table 6. The crowdout estimates are in the same range throughout these specifications. The crowdout estimates are larger with a larger discount rate, because in the elderly years earnings are more front-loaded than benefits are. See other notes to Table 6.

**Appendix Table 5. Effects of simulated OASI benefits on earnings and participation**

	(1) Discounted earnings	(2) Discounted earnings	(3) % of years with positive earnings, 1978 to 2012	(4) % of years with positive earnings, 1978 to 2012
	-0.58 (0.15)***	-0.60 (0.16)***	-0.61 (0.16)***	0.64 (0.17)***
Controls?	N	Y	N	Y

Notes: This table is parallel to Table 6, except that that in this appendix table, the endogenous variable is *simulated* discounted benefits, calculated using simulated earnings as described in the Appendix, whereas Table 6 reports results in which the endogenous variable is actual discounted benefits. The first-stage coefficient on the Notch dummy, when the dependent variable is simulated OASI benefits from 1978 to 2012, is -6,459.52 (standard error 636.89) without controls. See other notes to Table 6.

**Appendix Table 6. Effects of OASI benefits: additional specifications**

Outcome	Coefficient (SE)
A) Discounted earnings, 1979-2012	-0.57 (0.15)***
B) Positive earnings probability, 1979 to 2012	-0.64 (0.17)***
C) Discounted earnings, 1978-2012, no Dec. 30-Jan. 4	-0.50 (0.18)***
D) Positive earnings probability, 1978 to 2012, no Dec. 30-Jan. 4	-0.54 (0.19)***

Notes: This table is parallel to Table 6, except that that in this appendix table, the specification is as described in the first column. In Rows A and B, we investigate the outcomes from 1979 to 2012 (rather than 1978 to 2012 in Table 6), since the substitution incentive operated over 1979 to 2012 (but not in 1978). In Rows C and D, we demonstrate that the results are similar when excluding DOBs near January 1, 1917—namely DOBs from December 30, 1916 to January 4, 1917. These results are also extremely similar when we exclude similar sets of dates such as only January 1 and 2, or excluding a wider range of dates such as Dec. 25-Jan. 9. All of the estimates are also extremely similar with controls. See other notes to Table 6.



**Appendix Table 7. Individual-level regressions: effect of OASI benefits on earnings and participation**

	(1) Discounted earnings	(2) Percent of years with earnings >0, 1978 to 2012
Coefficient	-0.63 (0.015)***	-2.67 (0.046)***

Notes: The table runs individual-level 2SLS regressions (D.15)-(D.16) of lifetime discounted earnings from 1978-2012 on the cut in lifetime discounted benefits from 1978 to 2012 experienced from being in the 1917 birth cohort rather than the 1916 birth cohort, as described in the Appendix. The instrument for the cut in actual lifetime discounted benefits is the cut in lifetime simulated benefits, again as described in the Appendix. All regressions have 443,241 observations, and they are clustered at the date of birth level, with 112 clusters corresponding to the CCT bandwidth of 56. For the participation specification, the coefficient and standard error have been multiplied by 1,000,000, so that the quoted coefficients reflect the percentage point effect on participation of a \$10,000 increase in lifetime discounted OASI benefits.

**Appendix Table 8. Responses to substitution incentives: alternative specifications**

Outcome	(1)	(2)
A) Earnings 1979 minus 1978 (levels)	85.64 (61.75)	88.30 (62.30)
B) Earnings substitution elasticity: levels, simulated substitution incentive	-0.0056 (0.0044)	-0.0057 (0.0045)
C) Participation 1979 minus 1978 (levels)	-0.019 (0.16)	-0.024 (0.16)
D) Participation substitution elasticity w.r.t. $(1+\mu_{ijt}-\tau_{ijt})$ : simulated substitution incentive	0.0011 (0.0075)	0.00088 (0.0075)
E) Participation substitution elasticity w.r.t. $(1-ATR_{ijt})$ : simulated substitution incentive	0.0018 (0.015)	0.0023 (0.015)
F) Earnings 1979 minus 1978 (IHS)	0.0065 (0.015)	0.0063 (0.015)
G) Earnings substitution elasticity: IHS, actual substitution incentive	-0.035 (0.079)	-0.034 (0.081)
H) Earnings substitution elasticity: IHS, simulated substitution incentive	-0.030 (0.068)	-0.029 (0.069)
Controls?	N	Y

Notes: In Row A, the dependent variable is the mean of earnings in 1979 minus the mean of earnings in 1978. As described in the Appendix, Row B calculates the implied substitution elasticities using equation (14) and *simulated* substitution incentives  $(1+\mu_{ijt}-\tau_{ijt})$ . Rows C estimates the discontinuity in participation rates (expressed in percentage points), and Row D shows the implied elasticity using the simulated substitution incentive  $(1+\mu_{ijt}-\tau_{ijt})$ . Row E shows the elasticity using the simulated substitution incentive at the extensive margin  $(1-ATR_{ijt})$ , under the fixed cost model in the Appendix. Because the calculation of the extensive margin substitution incentive under this fixed cost model relies on calculating simulated earnings, it does not make sense to calculate this elasticity using the *actual* substitution incentive. We calculate an extensive margin substitution incentive in 1978 or 1979 by calculating the change in lifetime discounted OASI benefits from having zero earnings in either of these years (again holding earnings in other years constant), rather than having the individual's simulated earnings at age 62 (where earnings is simulated using the earnings experience of the 1916 cohort, as described in the Appendix). Calculating the incentive to participate by simulating the effect on benefits of retiring, *i.e.* not participating in a given year and all subsequent years, yields even smaller participation elasticities, thus strengthening our conclusion that these are small. In Row F, the dependent variable is the mean of the inverse hyperbolic sine (IHS) of earnings by DOB in 1979 minus the mean of the IHS of earnings by DOB in 1978. As described in the Appendix, the IHS function approximates the log for large arguments, but unlike the log function, the IHS function is defined at zero. Rows G and H implement model (D.18)-(D.19) from the Appendix to estimate the substitution elasticity directly from a regression, using actual and simulated substitution incentives, respectively. The 95 percent confidence interval rules out more than a small positive elasticity in each case. Note that for reference Rows A and C display information also shown in Table 8. See other notes to Table 2.

**Appendix Table 9.** *Specifications parallel to Krueger and Pischke (1992)*

	Log Odds of Participation Rate	
Log mean OASI benefits	1.79 (0.34)***	-0.063 (0.11)
Age dummies	Yes	Yes
Year dummies	No	Yes
Observations	53	53

Notes: The table reports estimates of the coefficient  $\alpha$  from model (F.20), reflecting the estimated effect of log mean OASI benefits on the log odds of participation. The data are yearly data on participation rates of men born 1916 to 1922 (inclusive), observed between ages 60 and 68, constructed using the SSA Master Earnings File. Participation in a given year is defined as having positive earnings in that year. This is the same set of birth cohorts and ages used in Krueger and Pischke (1992), who use the CPS. Age is defined in our data as the highest age an individual turns during a calendar year; if instead we define age as the age an individual initially has during the calendar year, we obtain similar results. In the first column we control only for age dummies, and in the second column we additionally control for calendar year dummies (which can be considered a preferred specification since these dummies are jointly significant,  $p < 0.01$ ). These results confirm the findings of Krueger and Pischke (1992) using the CPS, namely that when using variation at the yearly level, there is no evidence that OASI benefits reduce men's participation rates, and the confidence intervals rule out more than a moderate effect on men's participation.