PRELIMINARY

The Role of Social Security Benefits in the Increase of Older Women's Employment Rate: Evidence from the Notch Cohorts[1]

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Abstract

To understand trends in elderly women's work decisions, a key question is the extent to which changes in Social Security have played a role. We estimate the effect of Social Security benefits on women's employment rate by examining the Social Security "Notch," which cut women's average Old Age and Survivors Insurance (OASI) benefits substantially in the 1917 birth cohort relative to the 1916 cohort. This led to sharply different benefits for similar women born one day apart. Using Social Security Administration microdata on earnings in the full U.S. population by day of birth, we find evidence for substantial effects of this policy change on elderly women's employment rate. We find that the slowdown in the growth of Social Security benefits in the mid-1980s can account for around one-third of the increase in the growth of older women's employment that occurred during this period.

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1. Introduction

One of the most intriguing phenomena in the U.S. labor market over the past three decades is the striking rise of the employment rate of older women. Current Population Survey data shown in Figure 1 show that the employment-to-population ratio of women 65 and older has more than doubled in less than 30 years, rising from 7.0 percent in 1985 to 14.2 percent in 2013. This large increase is notable in part because it represents a reversal relative to the secular decline in older women's employment rate from 1950 to 1985, from 9.4 percent in 1950 to 7.0 percent in 1985.

To understand the recent trends better, we probe the initial roots of this turnaround in the mid-1980s. Many factors could have contributed to the turnaround in this trend, such as compositional changes across birth cohorts including increases in prior employment across successively later cohorts of women, changes in private pension arrangements like the increase in the 1980s of Defined Contribution pensions relative to Defined Benefit pensions, or improvements in health (Munnell, Cahill and Jivan 2003; Schirle 2008; Blau and Goodstein 2010).

We propose and explore a new partial explanation for the turnaround: Social Security. 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 20 percent of federal government spending (Social Security Administration (SSA) 2015). OASI could be an important determinant of elderly work decisions, as it is a major source of income for the elderly, providing the majority of income for 65 percent of elderly beneficiaries (SSA 2015). Largely due to the 1977 Social Security Act amendments, OASI benefits and replacement rates grew much less quickly beginning in the mid-1980s than prior to this time (Social Security Administration (SSA) 2013a; Clingman, Burkhalter, and Chaplain 2014). These changes should push toward older women's employment rates growing less quickly starting in the mid-1980s, consistent with the evidence in Figure 1.

This serves as a motivation for investigating the microdata to assess the extent to which changes in Social Security played a role; many other factors could have played important roles in explaining the observed turnaround in the time series data. In particular, we investigate the effects of the Security "Notch" created by the 1977 Social Security Act amendments on the employment decisions of older women. Because of this policy change, individuals born on or after Jan. 2, 1917 faced very different OASI benefits than those born earlier. We exploit this change through a Regression Discontinuity Design (RDD). We find that for women born after this date relative to those born earlier, on average mean lifetime discounted real OASI benefits were discontinuously \$2,094 lower.[2] The variation we investigate represents the largest discontinuous change in OASI benefits to our knowledge.

Our main finding is that we estimate large effects of OASI on women's employment rate. Around January 2, 1917, we find a statistically significant discontinuous increase in older women's employment rates. We use this to estimate that an increase in lifetime discounted OASI benefits of \$10,000 causes a decrease in the percent of years with positive earnings from ages 61 to 95 of 1.24 percentage points.

We use these results to calculate how much of the turnaround in the mid-1980s in the growth of older women's employment rate can be accounted for by the change in the growth rate of OASI benefits. Under our RDD estimates, the decrease in the growth rate over time of OASI benefits around 1985 can account for around 33 percent of the contemporaneous increase in the growth rate of the employment rate of those over 65. For the 65-69 year-old population, an even larger turnaround in the employment rate is observed in the mid-1980s (Figure 2). We calculate that the decrease in the growth over time of OASI benefits around 1985 can account for around 40 percent of the contemporaneous increase in the growth of the employment rate of 65-69 year-olds.

Our paper examines only women, whereas the original economics paper that innovated the use of the Notch to study economic outcomes, Krueger and Pischke (1992), examines only men.[3] This paper complements Gelber, Isen, and Song (2016), who investigate the effects of the Notch in the full population (with limited analysis of women in particular). More broadly, our paper is related to other work on the effects of elderly pensions and other retirement income on employment decisions, including the implications of these effects for understanding the time series of employment rates (*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

[2] All dollar amounts are in real \$2012. By "lifetime" we refer to benefits from 1978 to 2012. "Age" in a calendar year refers to the highest age an individual attained during this year.

[3] 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).

1985; Burtless 1986; Stock and Wise 1990; Costa 1995; Samwick 1998; Coile and Gruber 2000; Coile and Gruber 2004; Coile and Gruber 2007; Brown, Coile, and Weisbenner 2006; Mastrobuoni 2009; Costa 2010; Behagel and Blau 2012; Fetter and Lockwood 2016; Manoli and Weber forthcoming).

The rest of the paper proceeds in the following. Section 2 describes the policy change we study. Section 3 discusses the data. Section 4 estimates the causal effect of the Notch policy on older women's participation, as well as the effect of benefit levels on women's participation. Section 5 discusses implications for understanding the time series of older women's participation decisions. Section 6 concludes.

2. Policy Environment

Eligible individuals can claim their OASI benefit through their own earnings history beginning at age 62, the Early Entitlement Age (EEA). In the cohorts we study, individuals can claim their full OASI benefit when they reach the Normal Retirement Age (NRA) at 65.

The 1977 amendments changed the way OASI benefits were determined by earnings histories. The Primary Insurance Amount (PIA) forms the basis for the monthly OASI benefit. Prior to 1977, the PIA was a function of the Average Monthly Wage (AMW). The AMW was calculated as an average of a claimant's nominal earnings over their highest-earning years. The 1972 Social Security Act amendments indexed the AMW-to-PIA replacement rate to the CPI. This meant that inflation increased benefits through two routes: AMW was calculated using nominal wages so inflation raised the AMW, and inflation mechanically increased the replacement rate due to the indexation. This was referred to as "double indexation." Since inflation was high in the mid and late 1970s, this led to benefits that increased very quickly, and policy-makers saw this as financially unsustainable (GAO 1988).

Double indexation ended with the 1977 amendments. For those born in 1922 and later, PIA has been a function of Average Indexed Monthly Earnings (AIME). Like AMW, AIME is calculated as a function of past earnings. However, for calculating AIME, earnings prior to age 62 are inflated by the growth in national earnings.

This policy change led to much lower Social Security benefits for those receiving benefits under the AIME formula. To smooth the transition to the AIME formula, policy-makers developing the 1977 amendments created a special formula for those born between 1917 and 1921 (inclusive), called the "transitional guarantee." Claimants born between 1917 and 1921 received the maximum of benefits calculated in one of the following 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:

$AMW = \sum_{t \in T \text{ and } t<62} w_t / N$.[4] The second method was called the "transitional guarantee."

Social Security rules in a given birth cohort apply to individuals born Jan. 2 or later in that cohort. For example, the rules affecting what we call the "1916 cohort" apply to individuals born Jan. 2, 1916 through Jan. 1, 1917 (inclusive). We use the term "cohort boundary" to refer to the boundary between the cohorts defined in this manner.

In the 1916 cohort, 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).[5] As a result, those born on Jan. 2, 1917 or after faced a substantially different OASI benefit structure than those born Jan. 1, 1916 or earlier.

This policy change could create both income and substitution effects on participation. Because earnings after age 61 were not taken into account 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 someone 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 those in the 1917 cohort relative to those in the 1916 cohort. Under the typical presumption that leisure is a normal good, the income effect of this decrease in benefits should have led to an increase in average participation at the cohort boundary.[6] These cuts in benefits were widely publicized, including in a famous "Dear Abby" column on the discrepancies in benefits for similar individuals (GAO 1988).

There was also a change in substitution incentives at the cohort boundary. Because

[4] 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.

[5] A very small percentage was covered by other methods, the 1977 Old Start Method or the Regular Minimum (McKay and Schobel 1981).

[6] 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.

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 cohorts. The returns to extra 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; discounted over the course of the 18 years an average individual collected OASI benefits, however, 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. 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 small in our data. Indeed, we calculate that the net lifetime return to additional pre-tax, pre-transfer earnings in 1979 fell by 12 percent at the cohort boundary for women. The elasticity of participation with respect to the substitution incentive should be positive, so this substitution incentive should have led to lower participation in the 1917 cohort than the 1916 cohort (all else equal).

Thus, the net effect of the Notch on participation at the cohort boundary is ambiguous. *Ceteris paribus* the income effect should cause a rise in participation at the boundary, whereas *ceteris paribus* the substitution effect should cause a fall in participation at the boundary.

The 1977 amendments were signed into law on December 20, 1977. The legislative history shows that the discontinuity between benefits in 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 participation around the boundary in 1976 and earlier years; we treat 1978 and later as years when the policy discontinuity could have had an effect on participation; and we exclude 1977 from most of our analysis as expectations in this year are unclear.[7]

[7] Because the transitional guarantee formula specified that after December 1978 no inflation adjustments are made to benefits until the calendar year in which 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 cohort boundaries

3. Data

We obtained administrative data on the full U.S. female population from the Social Security Master Earnings File and Master Beneficiary Record 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 in each year separately from 1951 to 2012. All of these 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 remuneration exceeds a modest threshold. Thus, we have data on earnings regardless of whether an employee files taxes. 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, the Earnings Test (and its interaction with the actuarial adjustment and Delayed Retirement Credit), *etc.*—we calculated OASI benefits on the basis of earnings and claiming histories, and we validated our measure of calculated benefits against benefits in the final year of benefit receipt in the Master Beneficiary Record.

Our data allow us to calculate pre-tax OASI benefits; this makes a negligible difference to the results 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, we answer the policy-relevant question of how a given cut in benefits paid by SSA would affect participation.

Our measure of earnings excludes self-employment income, as this can often be 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. We include all other individuals (including those who collect benefits as retired workers, auxiliary beneficiaries, or survivors). Starting in the calendar year after an individual dies, until the final year in the dataset (2012), benefits and earnings appear in the data as zeroes.

When one spouse earns less than the other, under the OASI rules, the lower-earning

(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.

spouse in total receives the maximum of either: (a) the benefit to which they are entitled on their own record, or (b) one-half the benefit due to the higher earner (either because they collect this amount as a "secondary" beneficiary, or because they are "dual-entitled" and 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 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.[8] For the higher earner (specifically non-dual-entitled primary beneficiaries), OASI benefits are discontinuous at the cohort boundary in their own DOB. Thus, our estimated effects for women are local to a population with particularly high lifetime earnings relative to their husbands.

Due to the nature of the data, we cannot consistently estimate a wife's response to a husband's OASI benefit. We only observe wives linked to their husbands 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 as 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.

For illustrative purposes, in those cases in which we discount, in the baseline benefits are discounted 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 in real 2012 dollars.

Table 1 shows summary statistics. We use data from 384,354 individuals born within 100 days of the cohort boundary from 1978 to 2012, corresponding to 13,347,390 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 $53,132. 9.7 percent of the sample has positive earnings in any given year from 1978 to 2012. Mean discounted benefits from 1978 to 2012 are $106,534. Each DOB on average has 1,907 observations; this is smaller than counts for the full U.S. population due to our sample restrictions.

4. Effects of Notch on participation

[8] 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.

As a first empirical step, we document the causal effects of the Notch policy. Next, we use these results to estimate an income effect of OASI on older women's participation.

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 cohort boundary, relative to the assumed smooth relationship between DOB and average participation 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 will effectively document whether we see a sharp change in participation at the cohort boundary.

Specifically, we estimate this regression:

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

Here j indexes DOB; E represents an outcome of interest (primarily the percent of years with positive earnings, which we call "participation"); 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. 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. The main coefficient of interest is β_1, representing the change in the mean level of participation 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 participation 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 participation 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 changes to the ADEA has a discontinuous effect on elderly work incentives around the 1916/1917 cohort boundary and therefore should not confound our identification strategy. It is important to use our fine-grained data by DOB, as more aggregate data could be confounded by other factors that lead to smooth trends in outcomes over the course of 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 (CCT, 2014) to select the bandwidth. For our main outcome, *i.e.* the percent of years from 1978 to 2012 with positive earnings, CCT selects a bandwidth of 62 days. To hold the sample constant across specifications, in our main results we use this bandwidth throughout.

We call (1) is a "linear" specification because we control for a linear function of DOB on both sides of the boundary. This specification without additional controls minimizes the Akaike Information Criterion (AIC) and Bayes Information Criterion (BIC).

We were able to obtain one additional predetermined variable in the SSA data, race. In some specifications we additionally control for the means of a dummy for being non-white by DOB.

We interpret the discontinuity in participation at the cohort boundary as reflecting movements along labor supply curve, not changes in demand by firms, as 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 as reflecting responses net of any adjustment frictions such as lack of awareness. 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.

It will also be useful to compare the discontinuity β_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 $B_{jPDV} \equiv \sum_{i \in I} \sum_{t=t_0}^{T} B_{ijt} / n$, where t_0 = 1978 and $T = 2012$ in our empirical application, the subscript j indicates that we have taken the mean on DOB j across all individuals i, and I reflects the full set of individuals in the sample. We can then run a regression of B_{jPDV} on the covariates:

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

Preliminary results

Our figures show the means of outcome variables averaged by 10-day bins of DOB around the cohort boundary. (The graphical patterns are robust to using bins of other sizes.) We show seven bins on either side of the boundary to display at a minimum the variation within the CCT bandwidth of 62 days of the boundary.

Figure 3 shows that the number of observations appears continuous at the boundary (following McCrary 2008). Table 2 confirms that there is no significant discontinuity. Table 2 and Figure 4 show that the proportion male (in the combined male and female population) and the proportion white are also smooth through the boundary.

Figure 5 verifies that discounted OASI benefits from 1978 to 2012 ("lifetime benefits") decrease discontinuously and quite substantially when crossing the cohort boundary. Table 3 Row A shows that in the baseline specification, lifetime benefits fall discontinuously by $2,094.

Discontinuities in participation rates at the cohort boundary

Our main outcome of interest is the percent of individual-calendar year observations from 1978 to 2012 with positive earnings by DOB. This is relevant to evaluating the implications of our results for women's employment patterns, our primary question of interest in the paper. Figure 6 shows a main result: at the cohort boundary, we observe a sharp increase in the participation rate from 1978 to 2012. Table 3 shows that in the baseline the participation rate increases by 0.26 percentage points at the boundary ($p<0.05$).

To illustrate how the effects vary across ages, in Figure 7 we show the coefficient and confidence interval on β_1 from model (1) when the dependent variable is the percent of years from 1978 to 2012 with positive 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 has an insignificant effect on participation shortly after the policy went into effect, in 1978-1980. The effects of the Notch on participation 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. The effects decline to insignificant in 1993 and after, corresponding to ages 76 and above for the 1917 cohort, when individuals typically have low participation rates (in all cohorts).

In Gelber, Isen, and Song (2016) we run a number of placebo tests that help establish that the discontinuity in participation was due to the causal effect of the Notch. In particular, we show that the discontinuity in participation 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.

If some individuals retire exactly on their birthday, this could cause a discontinuity in our measure of participation if it leads people to receive positive earnings in an extra calendar year. However, our placebo tests in Gelber, Isen, and Song (2016) show no systematic evidence of a discontinuity in participation at other cohort boundaries. Moreover, we have tried limiting the sample to those born Jan. 1, 1917 or up to 62 days prior and test whether those born Jan. 1, 1917 show significantly different participation relative to a smooth linear trend over previous birthdays. Those born on this date faced the incentives of the 1916 birth cohort, but if they retired on their birthday, we should find that they have significantly higher participation. In fact, those born on this date have insignificantly *lower* participation than those born on previous days, suggesting that this factor does not drive the results, and we rule out more than a small positive change in participation on this date. The effect of the Notch on a dummy for earnings above a small positive threshold, such as $1,000, shows similar results to Table 3.

Estimating an income effect

Since participation increases at the boundary, the income effect must dominate the substitution effect in our context. Since *ceteris paribus* the substitution effect should unambiguously push participation 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. These estimates will be a lower bound as long as the substitution effect is (weakly) positive, a core presumption of standard theory. 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 when leisure is a normal good).

Under these assumptions, we can estimate a lower bound on the income effect of OASI benefits on participation through a 2SLS model in which equation (2) is the first stage, and the second stage is:

$$E_j = \alpha_1 B_j + \alpha_2 DOB_j + \alpha_3 (D*DOB)_j + \eta_j \qquad (3)$$

We interpret α_1 as a lower bound on the local average treatment effect of discounted OASI benefits on participation, where this is local to those at the boundary.

Table 4 shows the 2SLS estimates. In the baseline specification in Column 1, we find that a $10,000 increase in lifetime discounted benefits causes a decrease of 1.24 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 the participation rate with respect to lifetime discounted benefits of 1.36.[9]

Different groups could show different-sized effects. Table 7 estimates the effects among those with average earnings prior to 1977, *i.e.* from 1951 to 1976, that are below *vs.* above the median for the full population. The point estimate is larger in the above-median prior earnings group than in the below-median group, and the estimate is insignificant in the low prior earnings group. Note that relative to the above-median group, the below-median group is much more likely to receive one-half of a husband's benefit and therefore has a much weaker first stage regression.

In most parameterizations of this lifecycle model, the effect on the participation rate 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 consumer 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 and participation 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. 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.

In Gelber, Isen, and Song, we find no evidence for a substitution effect of the policy change, by examining closely comparable years with sharply different substitution effects due to the policy change. Moreover, we estimate that the upper bound on the substitution elasticity is at most small. Thus, the lower bound on the income effect we estimate here can be considered tantamount to a point estimate of the income effect.

5. Implications for the time series

[9] At other cohort boundaries from 1917-1921, the first-stage discontinuities in benefits are lower, so we have much less power.

Using these results, we can perform a simple calculation of 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. 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 several years after when the Notch legislation occurred (1977), but the 1917 cohort reached age 65 and thus became included in the elderly group shown Figures 1 and 2 only in 1982.

From 1973 to 1984 the employment-to-population ratio among those 65 and over decreased by 0.059 percentage points per year on average, whereas it rose by 0.22 percentage points per year on average from 1985 to 2010. Meanwhile, from 1973 to 1984 women's mean real annual OASI benefit rose by $191.35 per year on average, but due largely to the 1977 Amendments it rose on average from 1985 to 2010 less quickly, by only $148.02 per year (Social Security Administration 2013a). To apply our estimates of the discontinuity in participation among ages 65 and over examined in Figure 1, we estimate an effect of the Notch on the female participation rate from 1982 to 2012 of 0.25 percentage points, and we find a discontinuity in mean yearly benefits over these years of $117.66. Thus, we find that the slowdown in the growth rate of OASI benefits can account for 33 percent of the actual change in the participation growth rate around 1985 (*i.e.* 0.25*(191.35-148.02)/(117.66*(0.22+0.059)) = 33 percent). For the 65-69 year-old group that was most directly affected immediately by the reform, we use analogous methods to calculate that the slowdown in the growth rate of OASI benefits can account for 40 percent of the actual change in the participation rate growth rate around 1985. Thus, overall we find that the slowdown in growth of OASI benefits can account for quite a substantial fraction of the turnaround in older women's employment rates.

These statistics on employment rates 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 older women's participation. Other empirical choices, such as estimating the change in slope from other sets of years around 1985, yield similar conclusions.[10]

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[10] Our 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.

We ignore substitution elasticities in this calculation since our results in Gelber, Isen, and Song (2016) 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 also grew less quickly after the mid-1980s than before, incorporating the effects of substitution incentives would if anything strengthen our conclusion that changes in OASI benefit growth rates can account for an important part of the changes in the growth of the employment-to-population ratio.

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 clearly other factors have played important independent roles in determining elderly employment rates.

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. We also ignore the possibility that changes in OASI policy affected realized benefits through the channel of effects on earnings (though any effect on earnings would only occur for a few years before the mid-1980s, so such effects on benefits are likely to be small). 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 the slowdown in the growth rate of OASI benefits, which appears to be quite substantial.

6. Conclusion

We propose that change in OASI benefits may have played a role in the increase in older women's employment rates that began in the mid-1980s. To shed light on this using microdata, we study the effects of the Social Security Notch. The point estimate shows that a $10,000 increase in discounted lifetime OASI benefits causes a decrease in the yearly participation rate of 1.24 percentage points from ages 61 to 95. If these results apply more broadly, we calculate that changes in the growth rate of Social Security benefits can account for fully one-third of the turnaround in the trend in older women's employment rates in the mid-1980s. Thus, Social Security may be an important factor, among others, in explaining this turnaround.

OASI also experienced other changes in substitution incentives around this period, including through a slowdown in the growth rate of the replacement rate. For example, the OASI Earnings Test gradually became less stringent over this period, leading to stronger employment incentives that could have also played a role in increasing the employment rate. Indeed, Gelber, Jones, Sacks, and Song (2016) find preliminary evidence for this hypothesis. It would be valuable to complement this work by investigating further the potential role of substitution effects of OASI in explaining recent trends in elderly employment rates.

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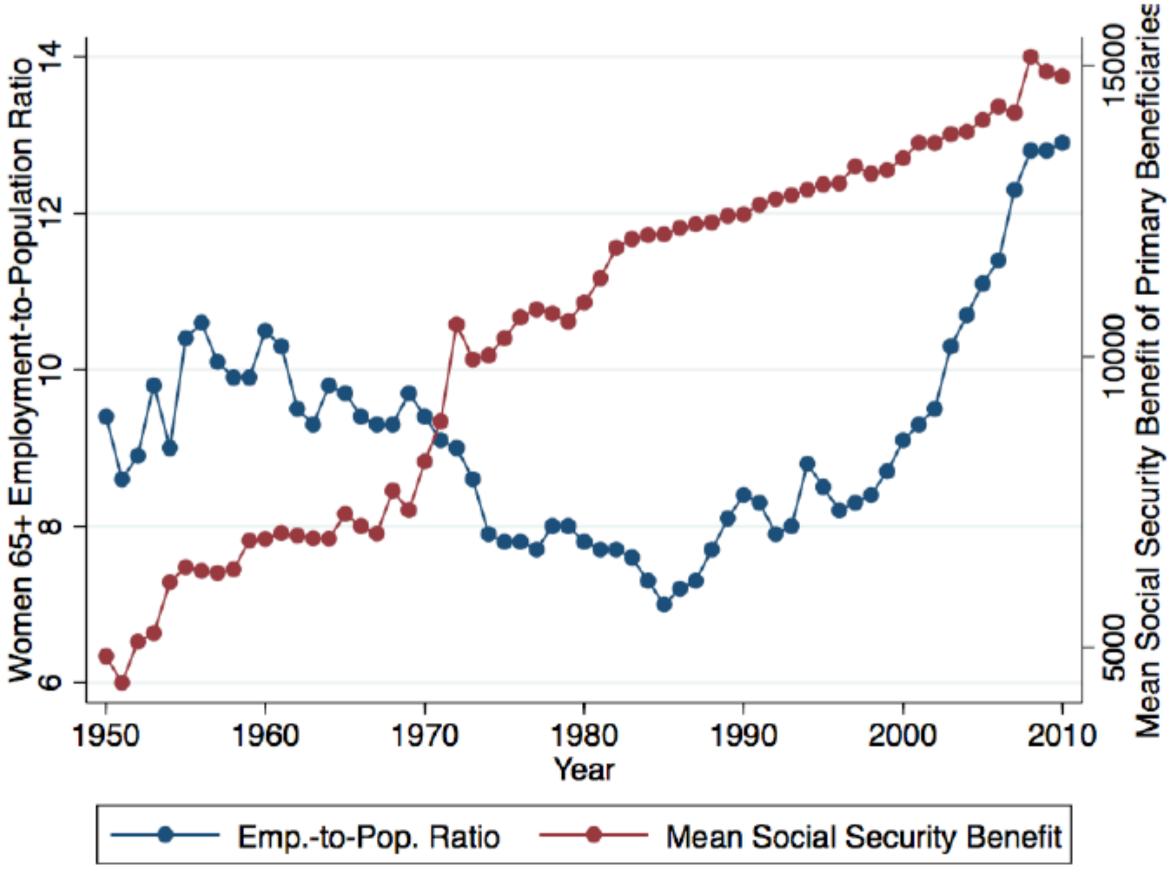
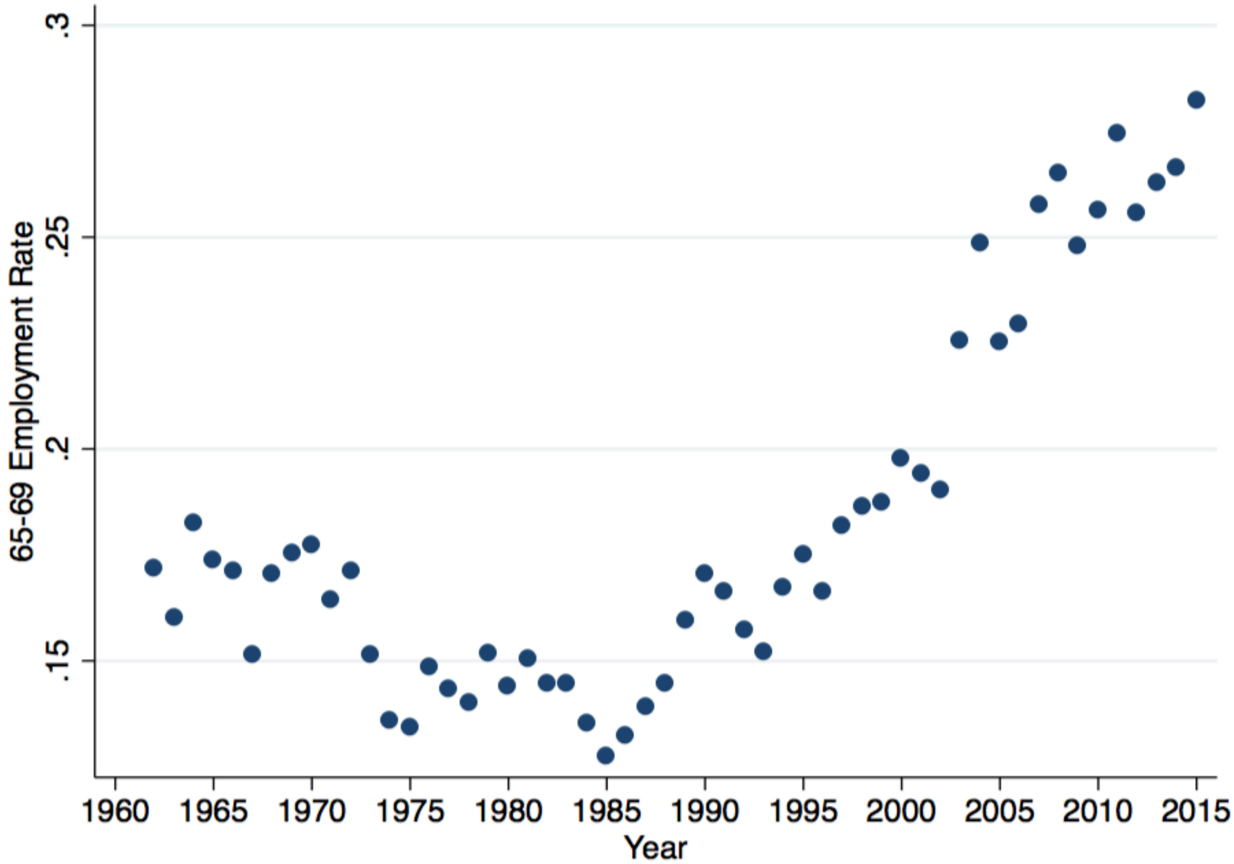
Figure 1. *Mean OASI benefits and elderly employment-to-population ratio*



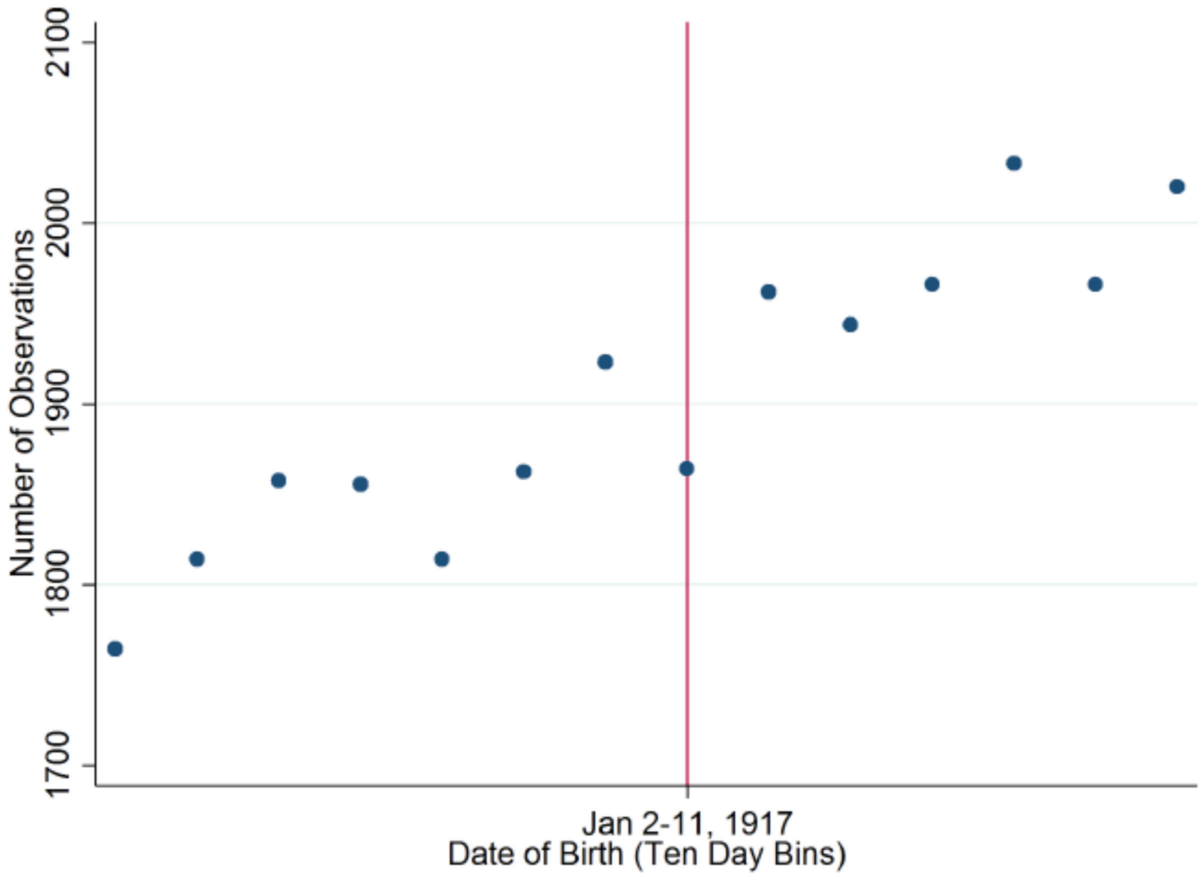
Notes: The figure shows the employment-to-population ratio for women 65 and over, as well as the mean OASI benefit, by year from 1950 to 2012. The data on the employment-to-population ratio among those 65 and older come from the Bureau of Labor Statistics. The data on mean OASI benefit of primary beneficiaries come from Social Security Administration (2013a). 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. *Age 65-69 employment-to-population ratio by calendar year*



Notes: The figure shows the employment-to-population ratio for women 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.

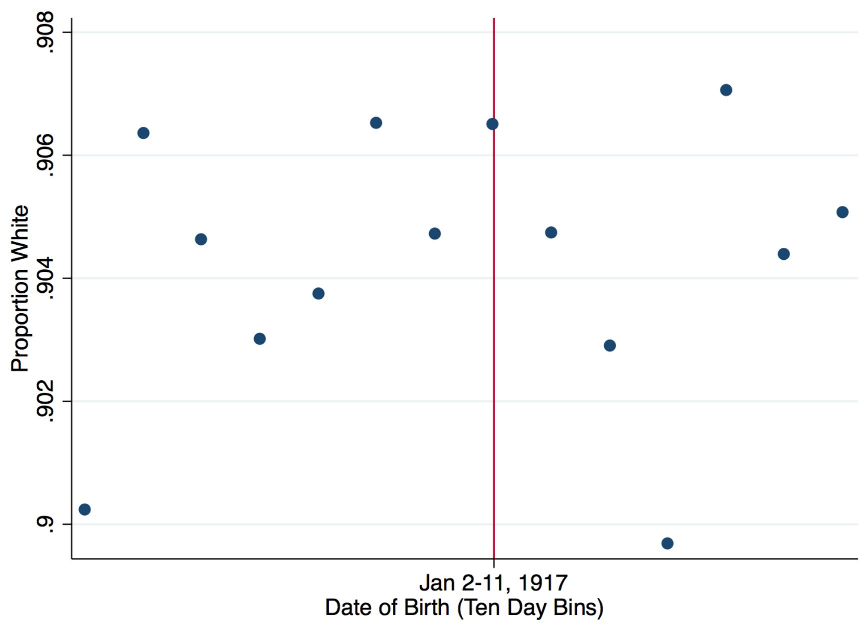
Figure 3. *Number of observations, by DOB Bin*



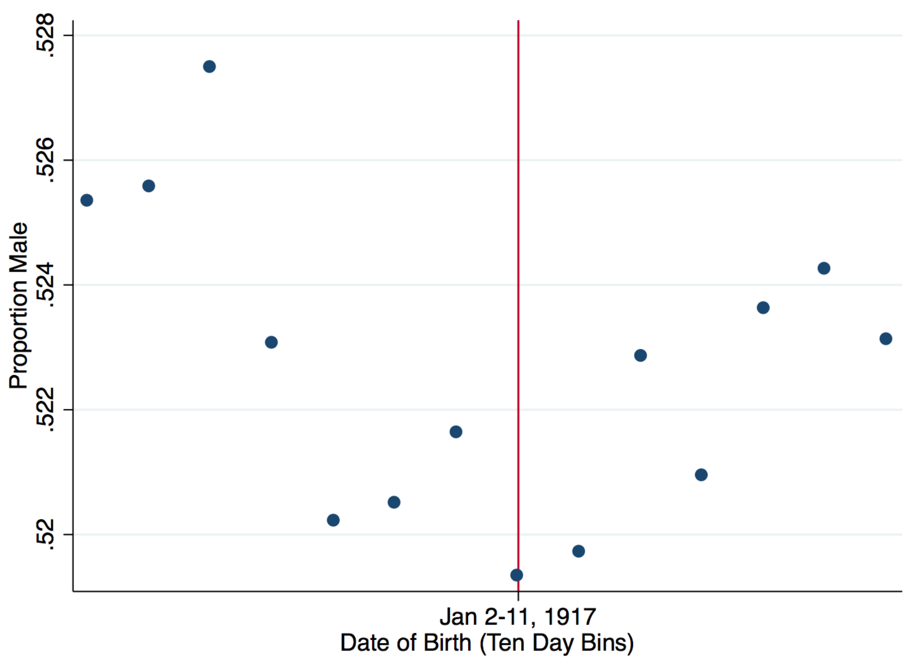
Notes: The figure shows the mean number of observations per DOB in 10-day bins around the boundary separating the 1916 birth cohort from the 1917 birth cohort (*i.e.* January 2, 1917). The data are a 100% sample of women from the Social Security Administration Master Earnings File and Master Beneficiary Record, with the sample restrictions described in the text.

Figure 4.

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

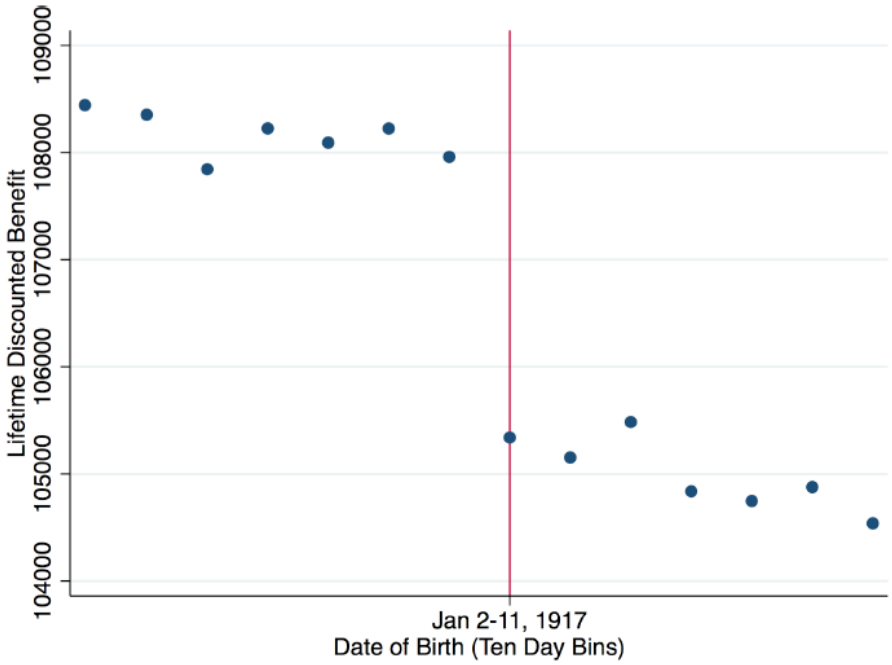


(b) Proportion male, by 10-day DOB bin



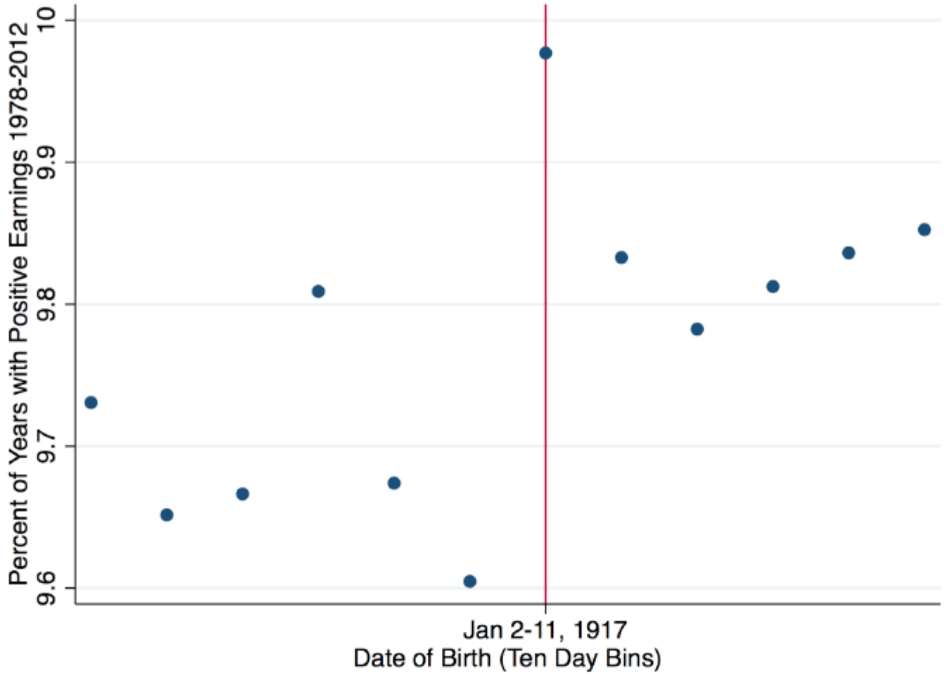
See notes to Figure 3. Note that in Figure (b) the dependent variable is the fraction male in the full population of both men and women.

Figure 5. *Mean discounted real OASI benefits, 1978 to 2012 (ages 61 to 95)*



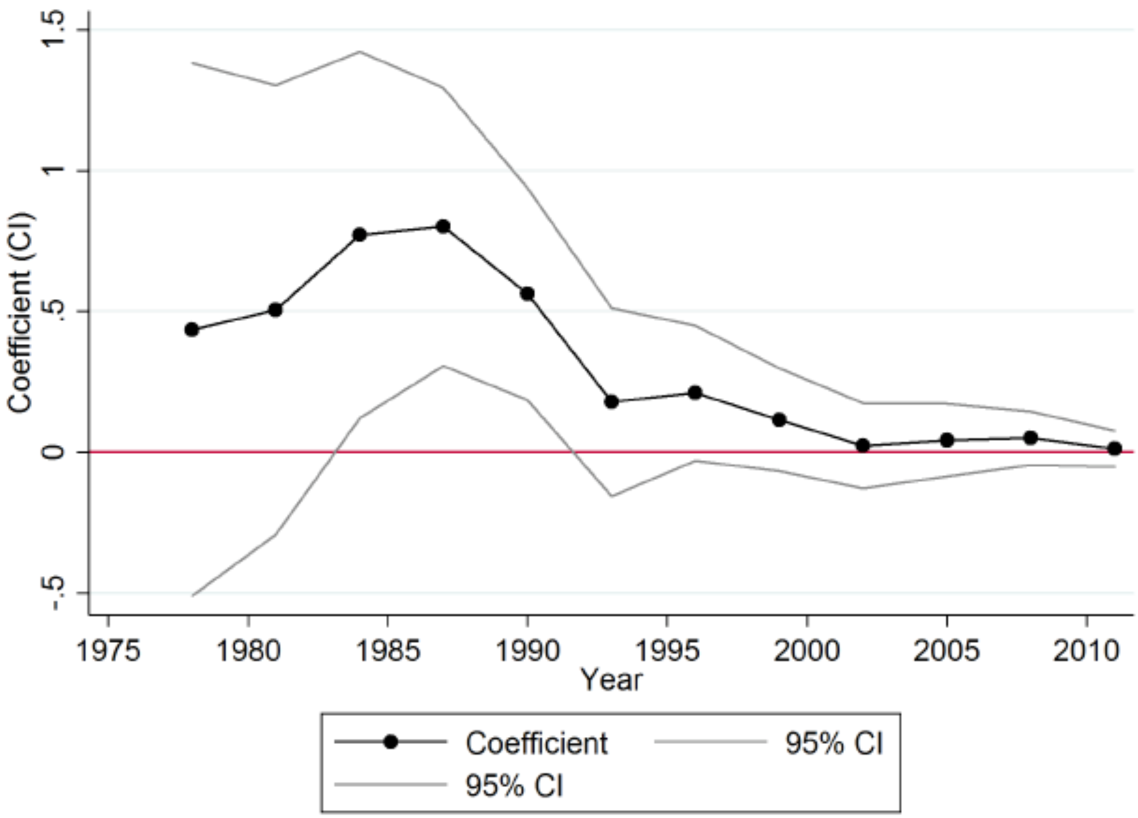
Notes: the figure shows individuals' mean discounted OASI benefits from 1978 to 2012, in 10-day bins around the discontinuity separating the 1916 birth cohort from the 1917 birth cohort. We discount to 1977 terms and then express all dollar amounts in real 2012 dollars. For illustrative purposes we use a 3 percent real discount rate. The 1917 birth cohort reaches ages 61 to 95 during the calendar years 1978 to 2012, respectively. See other notes to Figure 3.

Figure 6. *Percent of years with positive earnings, 1978 to 2012 (ages 61 to 95)*



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

Figure 7. *Effects on participation by time period*



Notes: The figure shows the discontinuity at the boundary in mean participation, by 3-year period. It illustrates that the effects of the Notch on participation 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 β_1 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 the mean percent of years with positive earnings (left axis). The x-axis shows the time period in question.

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

Variable	Mean (SD)
Discounted Earnings, 1978 to 2012	$53,131.83
	(2,372.61)
Percent of years with positive earnings,	9.70
1978 to 2012	(0.33)
Discounted OASI benefits, 1978 to 2012	$106,534.04
	(1,630.48)
Number of individuals per day of birth	1,906.77
	(258.86)

Notes: The source is SSA administrative data from the Master Earnings File and Master Beneficiary Record on the universe of U.S. data on women, with the other 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 381,354 individuals within 100 days of the cohort boundary, or 13,347,390 individual-year observations from 1978 to 2012 (inclusive).

Table 2. *Testing smoothness of predetermined variables*

Specification	(1) Percent white	(2) Percent male	(3) Number of observations
Coefficient (SE) on Jan. 2,	0.34	-0.17	-47.55
1917 dummy (linear)	(0.61)	(0.28)	(81.45)

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. We show 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). We use robust standard errors in Table 2 and throughout the other tables. We show the results for the bandwidth of 62, chosen using the CCT procedure when the outcome is our primary outcome (percent of years with positive earnings from 1978 to 2012), to hold the sample constant across regressions. Thus, all regressions have 124 observations. Percent male by DOB is calculated from the combined male and female population. 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
A) Discounted benefits 1978 to 2012	-2,093.66	-2,122.61
	(268.14)***	(272.26)***
B) Percent years with pos. earnings	0.26	0.26
1978 to 2012	(0.12)**	(0.12)**
C) Log odds of fract. years with pos.	0.030	0.030
earnings 1978 to 2012	(0.014)**	(0.014)**
Controls?	N	Y

Notes: The table shows the results of OLS regressions corresponding to the RDD model (2) (Row A) or model (1) (Rows B and C) 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 Bayes 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. *Lower bound income effect of discounted lifetime benefits on participation*

	(1) Percent of years with pos. earnings 1978 to 2012	(2) Percent of years with pos. earnings 1978 to 2012
	-1.24	-1.23
	(0.59)***	(0.58)***
Controls?	N	Y

Notes: The table shows the results of two-stage least squares regressions corresponding to regressions (2) and (3) in the text, estimating the effect of discounted lifetime OASI benefits on the percent of years with positive earnings from a linear probability model. The excluded instrument is the dummy for being in the 1917 cohort. The dependent variable is the percent of years with positive earnings from 1978 to 2012. 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 3.82 times larger than the actual 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 a lifecycle model. See other notes to Table 3.

Table 5. *Heterogeneity analysis*

	(1) Below-median pre-1977 earnings	(2) Above-median pre-1977 earnings
Coefficient	-0.27	-2.38
	(0.60)	(0.89)***

Notes: The table shows the results of two-stage least squares regressions corresponding to regressions (2) and (3) in the text, estimating the effect of discounted lifetime OASI benefits on the percent of years with positive earnings from 1978 to 2012. The dependent variable is the percent of years with positive earnings from 1978 to 2012 in the group shown in the column heading. Columns (1) and (2) 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.