Volume Title: Women Working Longer: Increased Employment at Older Ages

Volume Author/Editor: Claudia Goldin and Lawrence F. Katz, editors

Volume Publisher: University of Chicago Press

Volume ISBNs: 978-0-226-53250-9 (cloth); 978-0-226-53264-6 (e-ISBN)

Volume URL: http://www.nber.org/books/gold-12

Conference Date: May 21-22, 2016

Publication Date: April 2018

Chapter Title: The Role of Social Security Benefits in the Initial Increase of Older Women's Employment: Evidence from the Social Security Notch

Chapter Author(s): Alexander Gelber, Adam Isen, Jae Song

Chapter URL: http://www.nber.org/chapters/c13802

Chapter pages in book: (p. 239 – 268)

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The Role of Social Security Benefits in the Initial Increase of Older Women's Employment

Evidence from the Social Security Notch

Alexander Gelber, Adam Isen, and Jae Song

8.1 Introduction

One of the most intriguing phenomena in the US labor market over the past three decades is the striking rise of older women's employment. Current Population Survey (CPS) data in figure 8.1 show that the employment-to-population ratio of women sixty-five and older has more than doubled in less than thirty years, rising from 7.0 percent in 1985 to 14.2 percent in 2013. The 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, such as compositional changes across birth cohorts including increases in education and prior employment across successively later cohorts of women (Goldin and Katz, chapter 1, this volume), changes in

Alexander Gelber is an assistant professor at the Goldman School of Public Policy at the University of California, Berkeley, and a faculty research fellow of the National Bureau of Economic Research. Adam Isen is an economist in the Office of Tax Analysis at the US Department of the Treasury. Jae Song is an economist in the Division of Economic Research at the Social Security Administration.

We thank Claudia Goldin, Larry Katz, Erzo Luttmer, and other participants in the Women Working Longer working group for helpful comments. We are grateful to Patricia Jonas and Gerald Ray at the Social Security Administration for their help and support. This chapter does not necessarily reflect the views of the Social Security Administration or the US Treasury. We thank the UC Berkeley IRLE, CGIF, CEDA, and Burch Center, NIH (2P30AG012839), and the Wharton Pension Research Council and Trio grant for support. We are grateful to Nicole Danna, Gita DeVaney, Jonathan Holmes, and Harsha Mallajosyula for outstanding research assistance. All errors are our own. For acknowledgments, sources of research support, and disclosure of the authors' material financial relationships, if any, please see http://www.nber.org/chapters/c13802.ack.

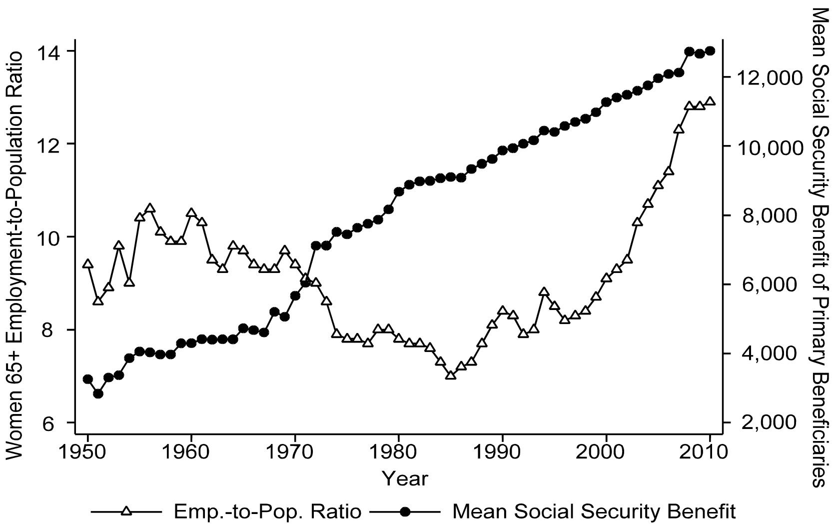

Fig. 8.1 Mean OASI benefits and employment-to-population ratio of older women

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

private-pension arrangements like the increase in the 1980s of defined-contribution pensions relative to defined-benefit pensions (see Fitzpatrick, chapter 7, this volume, on defined-benefit pensions among teachers, as well as Munnell, Cahill, and Jivan [2003]), increases in debt (Lusardi and Mitchell, chapter 6, this volume), changes in marriage and divorce (Olivetti and Rotz, chapter 4, this volume), improvements in health, or other factors.[1]

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 US 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 older Americans' work decisions, as it is a major source of their income, providing the majority of income for 65 percent of older beneficiaries (SSA 2015). Largely due to the 1977 Social Security Act amendments, OASI benefits and replacement rates grew far less rapidly beginning in the mid-1980s than prior to this time (Clingman, Burkhalter, and Chaplain 2014; Social Security Administration [SSA] 2013a). These changes should push toward older women's employment rates growing more rapidly starting in the mid-1980s, consistent with the evidence in figure 8.1.

1. Blau and Goodstein (2010), Gustman and Steinmeier (2009), and Schirle (2008) explore trends among men.

This observation about Social Security generosity serves as a motivation for investigating the microdata to assess the extent to which changes in Social Security played a role, relative to other factors, in explaining the turnaround observed 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 the 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, our measure of mean lifetime discounted real OASI benefits was discontinuously \$2,094 lower.[2] The variation we investigate represents the largest discontinuous change in OASI benefits in its history 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 relationship 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 sixty-one to ninety-five of 1.24 percentage points. From ages sixty-two to seventy-five, when beneficiaries experience contemporaneous benefit cuts and have not reached older ages with very low participation rates, this effect is 2.36 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 reduction in the growth rate of OASI benefits. Under our RDD estimates, in a baseline specification we calculate that the reduction in the growth rate over time of OASI benefits around 1985 can account for around 28 percent of the contemporaneous increase in the growth rate of the employment rate of those over sixty-five, relative to the counterfactual that benefit growth continued at the same rate in real dollars per year. For the sixty-five- to sixty-nine-year-old population, an even larger turnaround in the employment rate is observed in the mid-1980s (figure 8.2). We calculate that the decrease in the growth over time of OASI benefits around 1985 can account for around 34 percent of the contemporaneous increase in the growth of the employment rate of sixty-five- to sixty-nine-year-olds.

Our chapter examines only women, whereas the earlier work that innovated the use of the Notch to study economic outcomes, Krueger and Pischke (1992), examines only men.[3] The research complements Gelber,

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 older Americans' living arrangements (Engelhardt, Gruber, and Perry 2005), 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).

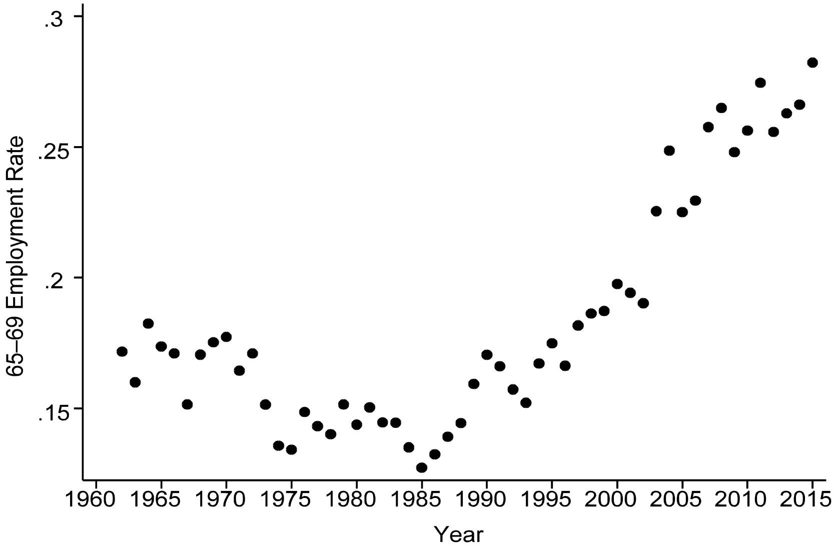

Fig. 8.2 Women's employment-to-population ratio by calendar year, ages sixty-five to sixty-nine

Notes: The figure shows the employment-to-population ratio for women ages sixty-five to sixty-nine by year from 1962 to 2015. The data on the employment-to-population ratio come from the Current Population Survey.

Isen, and Song (2016), who investigate the effects of the Notch in the full population of men and women combined (with only a limited separate analysis of women). More broadly, our chapter is related to other work on the effects of pensions for older individuals and other retirement income on employment decisions (e.g., Behagel and Blau [2012]; Coile and Gruber [2004, 2007]; Costa [1995]; Fetter and Lockwood [2016]; Manoli and Weber, forthcoming; Mastrobuoni [2009]; see Feldstein and Liebman [2002] for a review of earlier literature, and Gruber and Wise [1999] for a broad discussion of relevant evidence).

We proceed as follows: section 8.2 describes the policy change we study. Section 8.3 discusses the data. Section 8.4 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 8.5 discusses implications for understanding the time series of older women's participation decisions. Section 8.6 concludes. Throughout much of the chapter, particularly in sections 8.2, 8.3, and part of 8.4, we draw on the description of the policy environment, data, and empirical specification from Gelber, Isen, and Song (2016).[4]

4. In some cases the description is nearly identical, which is natural because the policy environment, data, and some of our specifications overlap. Relative to that work, the current

8.2 Policy Environment

Eligible individuals can claim their OASI benefit through their own earnings history beginning at age sixty-two, 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 sixty-five.

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. Inflation thereby 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. Since inflation was high in the mid- and late 1970s, this "double indexation" as it was called, led to benefits that increased very quickly, and policymakers 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 sixty-two are inflated by the growth in national earnings.

The 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, policymakers 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: (a) under the new formula based on the AIME; or (b) under the old AMW formula with one change relevant for the 1917 cohort: earnings after age sixty-one are not used in calculating average earnings: $AMW = \Sigma_{t \in T \text{ and } t<62} w_t/N$.[5] The second method was called the "transitional guarantee."

Social Security rules in a given birth cohort apply to individuals born January 2 or later in that cohort. For example, the rules affecting what we

chapter focuses on women's employment decisions and the implications of these results for understanding the time series of women's employment rate. In the two cases in which results overlap between the two papers, we cite Gelber, Isen, and Song (2016) as the primary source of these estimates.

5. 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 sixty-two and following years. However, since those in the 1917 cohort reached age sixty-two in 1979 (that is 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.

call the “1916 cohort” apply to individuals born January 2, 1916, through January 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, a larger fraction was covered by the transitional guarantee than by the AIME formula (McKay and Schobel 1981).[6] 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.

The policy change could create both income and substitution effects on participation. Because earnings after age sixty-one 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 sixty-one can only cause an increase—but cannot cause a decrease—in an individual’s PIA, the AMW of someone in the 1916 cohort whose earnings after age sixty-one were in their highest-earning years would be higher than the AMW of an individual with the same earnings history in the 1917 cohort. The average benefits for those in the 1917 cohort relative to those in the 1916 cohort were in consequence substantially lower. 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.[7] 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 earnings after age sixty-one were not taken into account in calculating the AMW under the transitional guarantee, the net marginal returns to additional earnings after age sixty-one fell at the boundary. In other words, additional earnings after age sixty-one 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 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 (DRC) (as they interact with the earnings test), a change in earnings

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

7. 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.

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 pretax, pretransfer 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.[8]

8.3 Data

We obtained administrative data on the full US 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-2 forms, 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—for example, 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 DRC, the earnings test

8. Because the transitional guarantee formula specified that after December 1978 no inflation adjustments were to be made to benefits until the calendar year in which an individual reaches age sixty-two, the 1977 amendments also created small discontinuities in benefits at the 1917/1918, 1918/1919, 1919/1920, 1920/1921, and 1921/1922 cohort 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. Indeed, even when pooling results from the other boundaries, we estimate insignificant results.

(and its interaction with the actuarial adjustment and DRC), and so forth—we calculated an approximate measure of OASI benefits on the basis of earnings, claiming histories, and spousal benefit rules.[9]

Our data allow us to calculate a measure of pretax 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 pretax 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 disability insurance (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 data set (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 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 date of birth (DOB) lies on, because their total benefit received depends only on their husband's DOB.[10] 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 married 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. Whether one is a dual or secondary beneficiary is endogenous to the size of the husband's and wife's separate benefits.

For illustrative purposes, in those cases in which we discount, in the base-

9. We lack population data on earnings or quarters of coverage before 1951, necessitating imputation. Claiming as primary is endogenous to the spouse's benefits, so we impute average benefits for nonprimary women based on halving men's benefits.

10. 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, when the wife is born both in 1916 and in 1917.

Table 8.1 **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, 1978 to 2012	9.70 (0.33)
Discounted OASI benefits, 1978 to 2012	$85,144.80 (1,548.97)
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 US 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).

line benefits are discounted at a 3 percent real interest rate (the average real ten-year 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 8.1 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 are $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 $85,144.80. Each DOB on average has 1,907 observations; this is smaller than counts for the full US female population due to our sample restrictions.

8.4 Effects of Notch on Participation

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.

8.4.1 Basic Empirical Strategy for Documenting Effect of Notch

To estimate the effect of the Notch policy, we use an RDD as in Gelber, Isen, and Song (2016), exploiting 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:

$$E_j = \beta_1 D_j + \beta_2 \mathrm{DOB}_j + \beta_3 (D \times \mathrm{DOB})_j + \varepsilon_j. \quad (1)$$

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 ($D \times$ 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 chapter.

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), and macroeconomic factors. 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 older Americans' work (Burkhauser and Quinn 1983). However, neither of these changes to the ADEA has a discontinuous effect on older Americans' 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 led 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 nonmissing observations on each day of birth.

We use the procedure of Calonico, Cattaneo, and Titiunik (2014; hereafter CCT) to select the bandwidth. For our main outcome—the percent of years from 1978 to 2012 with positive earnings—CCT selects a bandwidth of sixty-two days. To hold the sample constant across specifications, in our main results we use this bandwidth throughout.

We call (1) 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 nonwhite by DOB.

We interpret the discontinuity in earnings at the cohort boundary as reflecting movements in an earnings supply curve (in the case of income effects) or movements along an earnings supply curve (in the case of substitution effects)—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 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 as $B_{jPDV} \equiv \Sigma_{i \in I} \Sigma_{t=t_0}^{T} B_{ijt}/n$, 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:

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

8.4.2 Validating the Regression Discontinuity Design

Our figures show the means of outcome variables averaged by ten-day bins of DOB around the cohort boundary. We show seven bins on either side of the boundary to display at a minimum the variation within the CCT bandwidth of sixty-two days of the boundary.

Figure 8.3 shows that the number of observations appears continuous at the boundary (following McCrary 2008). Table 8.2 confirms that there is no significant discontinuity. Table 8.2 and figure 8.4 show that the proportion male (in the combined male and female population) and the proportion white are also smooth through the boundary.

Figure 8.5 verifies that discounted OASI benefits from 1978 to 2012 ("lifetime benefits") decrease discontinuously and quite substantially when crossing the cohort boundary. Table 8.3, row A, shows that in the baseline specification, lifetime benefits fall discontinuously by \$2,094.

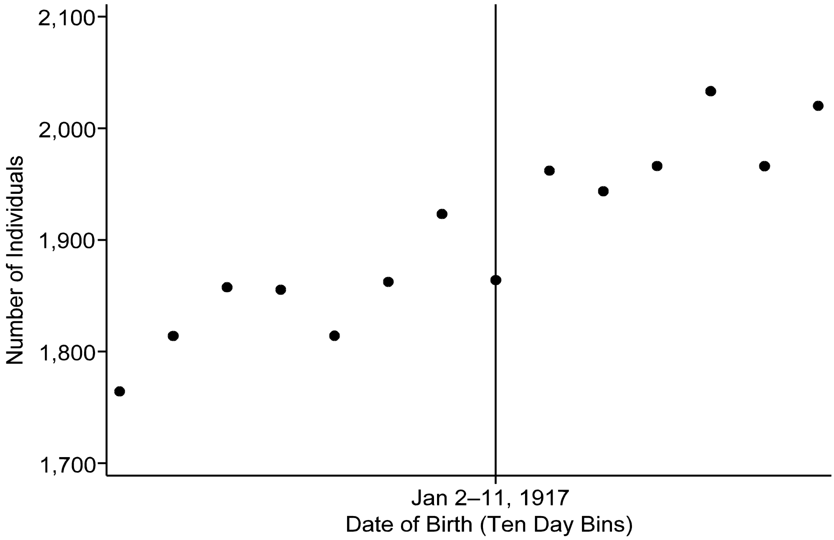

Fig. 8.3 Number of observations by DOB bin

Notes: The figure shows the mean number of observations per DOB in ten-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 percent sample of women from the Social Security Administration Master Earnings File and Master Beneficiary Record, with the sample restrictions described in the text.

Table 8.2 Testing smoothness of predetermined variables

Specification	(1) Percent white	(2) Percent male	(3) Number of observations
Coefficient (SE) on Jan. 2, 1917	0.34	−0.17	−47.55
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 January 2, 1917). We use robust standard errors in table 8.2 and throughout the other tables. We show the results for the bandwidth of sixty-two, 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 8.1.)

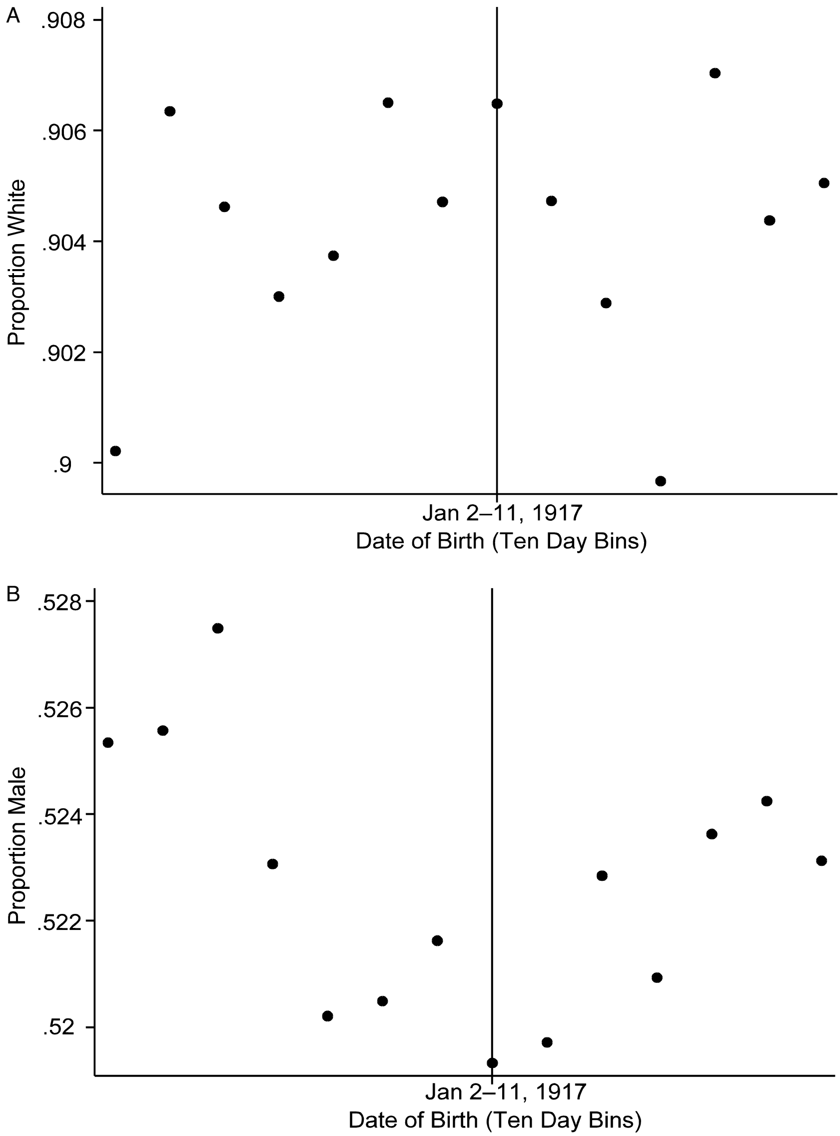

Fig. 8.4 Predetermined demographic outcomes

Notes: See notes to figure 8.3. In panel (B) the dependent variable is the fraction male in the full population of both men and women.

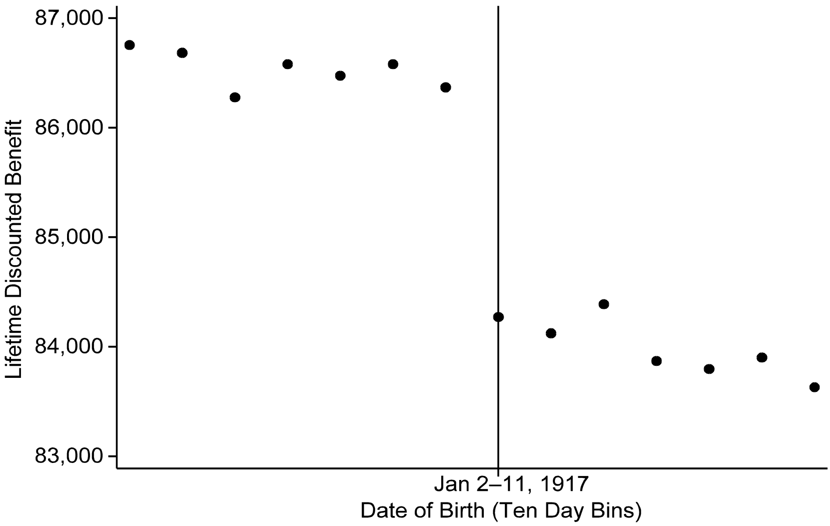

Fig. 8.5 Mean discounted real OASI benefits, 1978 to 2012 (ages sixty-one to ninety-five)

Notes: The figure shows individuals' mean discounted OASI benefits from 1978 to 2012, in ten-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 sixty-one to ninety-five during the calendar years 1978 to 2012, respectively. (See other notes to figure 8.3.)

8.4.3 Discontinuities in Participation Rates at the Cohort Boundary

Our main outcome of interest for understanding the impacts of OASI benefits on women's employment patterns is the "participation rate," defined as the percent of individual-calendar year observations from 1978 to 2012 with positive earnings by DOB. Figure 8.6 shows a main result: at the cohort boundary, we observe a sharp increase in the participation rate from 1978 to 2012. Table 8.3 shows that in the baseline the participation rate increases by 0.26 percentage points at the boundary ($p < 0.05$). For ages sixty-five and over, which we will use to analyze the changes in older women's employment over the twentieth century, we find an increase of 0.25 percentage points at the boundary ($p < 0.01$). Beneficiaries first begin to experience contemporaneous cuts in benefits at age sixty-two, and mean participation rates reach very low levels after age seventy-five; from ages sixty-two to seventy-five, we find a larger increase at the boundary of 0.47 percentage points.

To illustrate how the effects vary across ages, in figure 8.7 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

Table 8.3 **Effect of Notch on benefits 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 positive earnings 1978 to 2012	0.26	0.26
	(0.12)**	(0.12)**
(C) Log odds of fraction years with positive earnings 1978 to 2012	0.030	0.030
	(0.014)**	(0.014)**
(D) Percent years with positive earnings 1982 to 2012	0.25	0.25
	(0.089)***	(0.091)***
(E) Percent years with positive earnings 1979 to 1992	0.47	0.48
	(0.22)**	(0.23)**
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 January 2, 1917), as well as a linear spline in DOB with a knot at the cohort boundary. 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. (See other notes to table 8.2.)

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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 sixty-four to seventy-five years old. The effects decline to insignificant in 1993 and after, corresponding to ages seventy-six and older for the 1917 cohort, when individuals typically have low participation rates (in all cohorts).

We can run a number of placebo tests that help establish that the discontinuity in participation was due to the causal effect of the Notch. First, figure 8.7 shows that the discontinuity in participation does not appear in our sample before the policy change could have been anticipated.

Second, in table 8.4 we show that no systematic discontinuity in participation occurs at thresholds between other birth cohorts that were not subject to a discontinuous change in Social Security benefits. If some individuals retire exactly on their birthday, a discontinuity in our measure of participation would be observed if people then receive positive earnings in an extra calendar year. However, the placebo tests in table 8.4 help rule out this scenario. We were able to obtain W-2 wage earnings data from IRS on the full US 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—

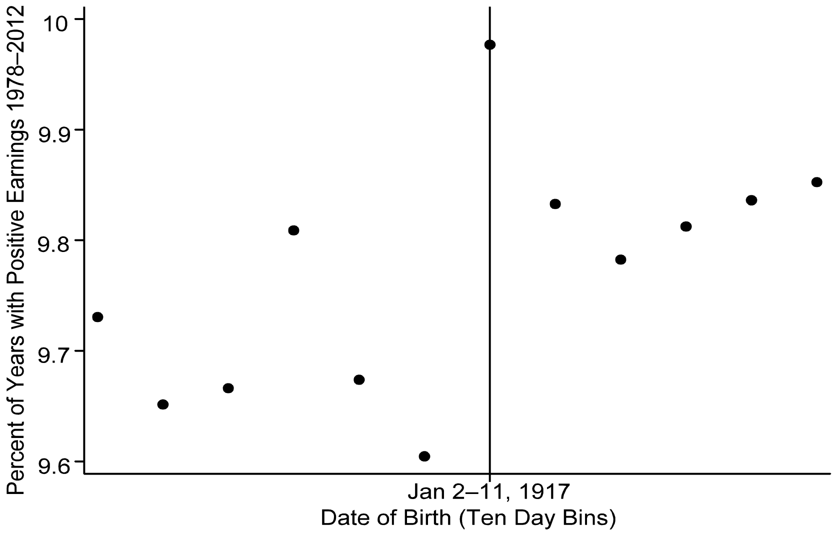

Fig. 8.6 Percent of years with positive earnings, 1978 to 2012 (ages sixty-one to ninety-five)

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 8.3.)

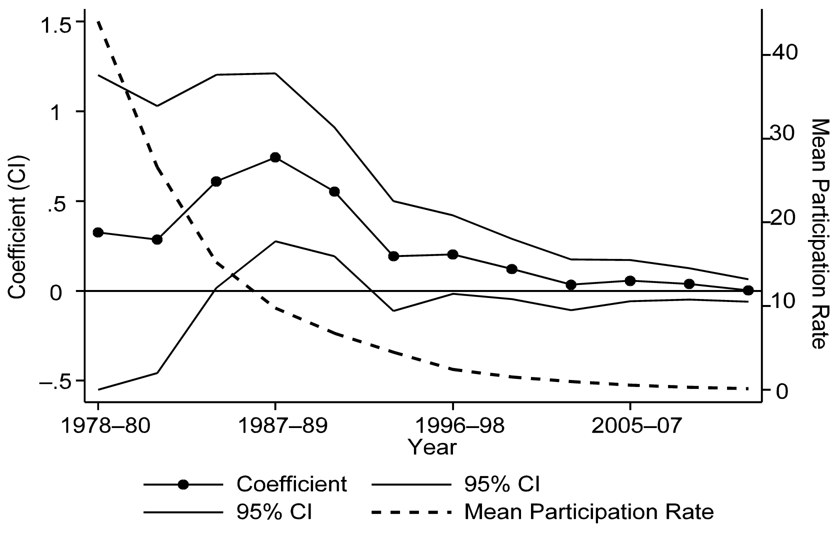

Fig. 8.7 Effects on participation by time period

Notes: The figure shows the discontinuity at the boundary in mean participation by three-year periods. It illustrates that the effects of the Notch on participation are largest in the 1980s and early 1990s when individuals are sixty-four to seventy-five years old, and decline to insignificant at later ages. Specifically, the y-axis (circles, left-hand scale) 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). For context, we also show the mean participation rate in each three-year period (dotted line, right-hand scale). The x-axis shows the time period in question.

Table 8.4 **Discontinuity in earnings and participation at placebo boundaries and the 1916/1917 boundary**

Age range (cohort boundary)	(1) SSA data, % of years with earnings > 0, 1916/17 boundary	(2) IRS data, % of years with earnings > 0, 1999–2013
(A) 75 to 89	0.20	0.081
(1923/1924)	(0.095)**	(0.062)
(B) 73 to 87	0.29	0.13
(1925/1926)	(0.11)**	(0.079)*
(C) 71 to 85	0.29	0.041
(1927/1928)	(0.11)***	(0.13)
(D) 69 to 83	0.38	−0.14
(1929/1930)	(0.13)***	(0.16)
(E) 67 to 81	0.42	−0.35
(1931/1932)	(0.15)***	(0.20)*
(F) 65 to 79	0.45	0.16
(1933/1934)	(0.17)**	(0.24)
(G) 63 to 77	0.46	−0.43
(1935/1936)	(0.20)**	(0.30)

Notes: The table shows using a 100 percent 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 percent 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 seventy-six to ninety for the 1923 cohort, ages seventy-five to eighty-nine for the 1924 cohort, and so forth. 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 8.4 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 at the 5 percent level we do not find significant discontinuities in the IRS data. 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 seventy-five to eighty-nine" refers to the fact that around the 1923/1924 boundary, those born in 1924 attained ages seventy-five to eighty-nine in 1999 to 2013, respectively. It makes sense that the standard errors are larger on the estimates for cohorts in the IRS data than those in the SSA data for 1916/1917 over the comparable set of ages; the means and standard deviations of earnings are larger in the IRS data due to the secular trend of increasing participation and earnings among older Americans across cohorts from 1917 to 1937 (see Gelber, Isen, and Song 2016). (See other notes to table 8.3.)

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

have no discontinuity in the DRC or another policy. 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 seventy-six to ninety for the 1923 cohort, ages seventy-five to eighty-nine for the 1924 cohort, and so forth. 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.

Table 8.4 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 at the 5 percent significance level we do not find significant discontinuities in the IRS data around any of the seven boundaries.[11] When pooling all seven boundaries in the IRS data and defining a dummy for being born after January 1 around any of the boundaries, the coefficient on this dummy in the resulting pooled regression is insignificant ($p = 0.51$).[12] Moreover, the discontinuities in 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.[13]

Furthermore, we have tried limiting the sample to those born January 1, 1917, or up to sixty-two days prior and test whether those born January 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 8.3.

8.4.4 Estimating an Income Effect

The fact that participation increases at the boundary means that the income effect must dominate the substitution effect in our context. Because, ceteris paribus, the substitution effect should unambiguously push participa-

11. Two of the coefficients are significant at the 10 percent level, but they are of opposite signs (one is positive while the other is negative).

12. 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.

13. 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 8.7).

tion 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

$$E_j = \alpha_1 B_j + \alpha_2 \mathrm{DOB}_j + \alpha_3 (D \times \mathrm{DOB})_j + \eta_j. \tag{3}$$

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 8.5 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 (recapitulating the estimates in Gelber, Isen, and Song [2016]). 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. From ages sixty-two to seventy-five, a $10,000 increase in lifetime-discounted benefits causes the

Table 8.5 **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	(3) Percent of years with pos. earnings 1979 to 1992	(4) Percent of years with pos. earnings 1979 to 1992
	−1.24	−1.23	2.36	2.54
	(0.59)***	(0.58)***	(0.78)***	(0.84)***
Controls?	N	Y	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 4.77 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 life cycle model. (See other notes to table 8.3.)

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 8.6 **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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

yearly participation probability to decrease by 2.36 percentage points. As we show and discuss in greater detail in Gelber, Isen, and Song (2016), when we investigate the income effect on earnings among women, we find that a one-dollar increase in OASI benefits leads to a decrease in discounted real earnings from 1978 to 2012 of eighty-nine cents (standard error forty-three cents), using a discount rate of 3 percent.

Different groups could show different-sized effects. Table 8.6 estimates the effects among those with average earnings prior to 1977 (from 1951 to 1976) that are below as opposed to 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. 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 smaller first-stage regression, so it is not surprising to estimate insignificant effects in the below-median group. Indeed, the graph of participation by DOB for the high lifetime income group shows a much clearer visual discontinuity in mean participation from 1978 to 2012 (figure 8.8). Given the larger first stage in this sample, as robustness checks it also makes sense to show that in this above-median sample: (a) in a wider range of DOBs, the discontinuity at the cohort boundary is unusual given the variation elsewhere in the range of DOBs (appendix figure 8A.1); and (b) when we use three-day bins of DOB, there is naturally more noise in each bin, but there still appears to be a clear shift upward in the level of the dependent variable—that is not a continuation of the trend on either side of the cohort boundary—from below to above the boundary (appendix figure 8A.2).

In most parameterizations of this life cycle model, the effect on the annual

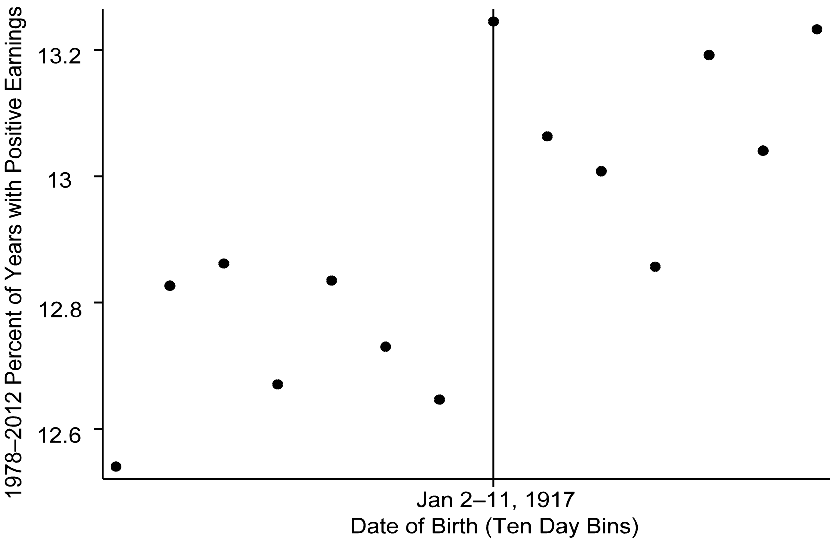

Fig. 8.8 Percent of years with positive earnings, 1978 to 2012 (ages sixty-one to ninety-five), above-median average prior earnings

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 among the group with above-median earnings prior to 1977. (See other notes to figure 8.3.)

participation rate should be larger when an unanticipated cut in benefits occurs closer to retirement rather than earlier in life (Imbens, Rubin, and Sacerdote 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 to alter consumption, and therefore adjusts participation more. In this light, our results would be most similar to evaluating the effects of unanticipated cuts in benefits that occur close to retirement age. Our estimates are most pertinent to contexts with an unanticipated change in OASI benefits experienced close to retirement age, relevant to policymakers interested in the effects of such changes along the transition path to a new steady-state OASI system.

In Gelber, Isen, and Song (2016) 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.

Gelber, Isen, and Song (2016) also show that the point estimates of the income effect on participation among women are around twice as large as those for men—consistent with the typical finding that women's labor supply is more elastic than men's, perhaps due to women's weaker historical attachment to the labor force. At the same time, the estimates are less statistically significant among women than among men: among men, the estimates are significant at the 1 percent level, but they are significant only at 5 percent among women. (The estimates are insignificantly different across the genders.) The finding of larger but less significant estimates among women may occur because the first-stage change in women's average benefits is smaller than men's—in part because many women's total benefits do not depend on their own DOB—thus driving a weaker and less statistically robust discontinuity in earnings in the reduced-form regression (1). The estimates among men and women combined are likewise more significant and robust, and a bit less than half as large, than among women alone.

8.5 Implications for the Time Series

8.5.1 Basic Calculations

Using these results, we can perform a simple calculation of the fraction of the change in the growth rate of the employment rate in the mid-1980s that can be accounted for with the reduction 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—1981 to 1989. The mid-1980s occur several years after when the Notch legislation occurred (1977), but the 1917 cohort reached age sixty-five and thus became included in the older group shown in figures 8.1 and 8.2 only in 1982.

In a life cycle model, only unanticipated changes in benefits should have mattered to employment decisions through the income effect. Since the growth in benefits due to double indexation was in fact unanticipated, as were the cutbacks in the 1977 amendments, this is applicable in our setting.

From 1973 to 1984 the employment-to-population ratio among those age sixty-five and older 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 less quickly on average from 1985 to 2010, by only $148.02 per year (Social Security Administration 2013a). Discounted over the average of twenty years over which women collect OASI benefits after claiming in our data, this implies moving from an increase in discounted lifetime benefits from $2,932.21 per year (where $2,932.21 is the presented discounted value of annual payments of $191.35 for twenty years, using a 3 percent discount rate) to $2,268.23 per year (the presented discounted value of annual payments of $148.02 for twenty years). We estimate an effect of the Notch on the annual female participation rate from 1982 to 2012 of

0.25 percentage points, and in table 8.3 we find a discontinuity in lifetime-discounted benefits from the Notch of −$2,093.66.

To calculate the fraction of the post-1985 employment rate turnaround that can be accounted for with the slowdown in OASI benefit growth, we use these estimates as follows. First, we take the Notch-based estimates of how a dollar more in lifetime OASI benefits affects the employment rate, which is 0.25 divided by $2,093.66. Second, we multiply this by the change in the growth rate of lifetime benefits over the two periods, $2,932.21 per year minus $2,268.23 per year, to obtain the implied change in the growth of participation in annual percentage point terms. Third, we divide this by the actual annualized change in the participation growth rate in percentage points, 0.22 minus −0.059, or 0.28. Thus, we find that the slowdown in the growth rate of OASI benefits can account for 28 percent of the actual change in the participation growth rate around 1985 ($0.25 \times [2{,}932.21 - 2{,}268.23]/(2{,}093.66 \times [0.22 - (-0.059)]) = 28$ percent). For the sixty-five- to sixty-nine-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 34 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.[14] 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. Although the point estimates are notable, it is important to note that the confidence intervals on the estimates are large enough that we cannot rule out that the true fraction is small (9 percent at the bottom end of the 95 percent confidence interval) or nearly half (48 percent at the top end of the 95 percent confidence interval).

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 the reduction in the OASI benefit growth rate can account for an important part of the increase in the growth rate of the employment-to-population ratio.

14. 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.

8.5.2 Extrapolating Local Estimates

A number of other issues could arise in determining the implications of our estimates for the time series of the employment rate. 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 whose benefits were affected by the Notch legislation. Importantly, we extrapolate our RDD estimate to the full population, but we do not have direct evidence on whether our local estimate generalizes to the full population. Indeed, it is worth noting that in the structural retirement models estimated in Coile and Gruber (2004, 2007), the effects of Social Security wealth on female employment appear smaller than those we have estimated.[15] One important issue is that because the Social Security benefits of women who have relatively low lifetime income in relation to their spouses are unaffected by the policy variation, our RDD estimate applies only to the combined population of single women and married women with relatively high lifetime income in relation to their spouses, but our extrapolation implicitly assumes that our results generalize. Our extrapolation also implicitly assumes that our results generalize beyond just those around the 1916/1917 birth cohort cutoff.

Several further assumptions are necessary to extrapolate our estimates. 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 also 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 older Americans' 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.

8.5.3 Evaluating Other Counterfactuals

It is worth considering the counterfactual we are assuming in our estimates of the fraction of the increase in the growth rate of older female labor force participation around 1985 that can be accounted for by the reduction in the growth rate of OASI benefits. Our counterfactual effectively assumes that the fast benefit growth under double indexation in the 1970s and early

15. The estimates of Mastrobuoni (2009) show substantial effects of the increase in the normal retirement age on women's employment decisions. However, the increase in the normal retirement age both decreased Social Security wealth and also could have changed the focal retirement age (Behagel and Blau 2012), and thus is not directly comparable to our setting.

1980s would have continued from 1985 to 2010. A key takeaway from this exercise is that this benefit growth would have otherwise caused women's employment rate to grow significantly less quickly. Phrased differently, much of the downward trend in women's employment rate prior to the mid-1980s was due to the sharp upward trend in OASI benefits, and was greatly lessened by the slower OASI benefit growth beginning in the mid-1980s. Thus, this counterfactual illustrates the role that fast OASI benefit growth played in explaining the downward trend in women's employment rate prior to 1985.

Of course, the fast benefit growth under double indexation was unsustainable absent significant tax increases, which indeed was the rationale for the cuts in OASI in the 1977 amendments. Figure 8.1 does show that comparable benefit growth occurred for much of the rest of the period from 1950 to 1980, most of which was financed through repeated payroll tax increases (Social Security 2013b). It is not unreasonable to believe that further sustained benefit growth could have occurred, though perhaps that was significantly less likely amid the fast benefit growth driven by the high inflation of the late 1970s.

Of course, other counterfactuals are possible, as we show in table 8.7. In the baseline, we choose the periods 1973 to 1984 and 1985 to 2010 because benefits and older women's employment rate usually changed in relatively smooth ways over each of these periods. However, it is possible to choose other historical time periods over which to make this comparison, and other choices usually yield comparable conclusions. If we consider the full time period shown in figure 8.1, 1950 to 2010, we can separate this into the period from 1950 to 1985 when OASI benefits grew faster on average and women's employment trended down overall, and the period from 1985 to 2010 when benefits grew more slowly on average and women's employment trended up. In this case, performing an analogous calculation to the one above shows that using the slowdown in the growth rate of OASI benefits we can account for 25 percent of the turnaround in the women's employment rate (0.25 ×

Table 8.7 **Evaluating fraction of turnaround explained under other counterfactuals**

	(1) Baseline	(2) 1950–2010	(3) Percentage increases in benefits
Fraction of turnaround explained	28.42%	25.33%	77.38%

Notes: The table shows the percentage of the turnaround in the older women's employment rate around 1985 that can be accounted for given the slowdown in OASI benefit growth rate around 1985, under different assumptions described in the column headings. Column (1) shows the baseline, in which we compare the growth of the absolute level of benefits and older women's employment in 1973–1984 and 1985–2010. Column (2) shows the analogous calculations, but expands the earlier time period to 1950–1984. Column (3) shows the calculations when we assume that benefits continued to grow from 1985 to 2010 at the same yearly percentage rate as they grew from 1973–1984. See the main text for details of these calculations.

[2,891.91 − 2,268.23]/(2,093.66 × [0.22 + 0.074]) = 25 percent) as shown in column (2) of table 8.7. This calculation involves extrapolating the estimates further back in time, when the setting may not have been as comparable. It is notable that the explained fraction of the turnaround, 25 percent, is similar to our calculation of 28 percent in the baseline. In other words, since the mid-1980s turnaround we investigate, older women's employment rates have largely continued to increase at a rapid rate until the time of this writing, with certain pauses but also a clear and striking upward trend (figure 8.1 and Goldin and Katz, chapter 1, this volume). Our calculations suggest that the slower growth rate of OASI benefits could potentially help account not only for the turnaround in the older women's employment rate in the mid-1980s, but also the continued growth today. However, this involves extrapolation of the estimates to a wider time period.

As another possible counterfactual, if OASI benefits had grown at the same rate in percentage terms as from 1973 to 1984, this would have implied still higher growth in the absolute level of OASI benefits from 1985 to 2010, since the baseline level of benefits grew over time. This would make the slowdown in benefits appear still starker, and therefore imply that we could account for still more of the turnaround in older women's employment rate relative to this counterfactual. We show this in column (3) of table 8.7. In percentage terms, mean benefits grew by an average of 2.22 percent per year over our baseline period from 1973 to 1984. If benefits had instead continued their growth rate of 2.22 percent over 1985 to 2010, then benefits would have been $15,767.51 in 2010, implying annual benefit growth in absolute terms of $266.43 per year, or growth in discounted lifetime benefits of $4,082.72 per year. As a result, the implied change in the participation growth rate, from the world in which discounted benefits rise at $4,082.72 per year to the reality where they rose $2,268.23 per year, is 0.25 × (4,082.72 − 2,268.23)/2,093.66 = 0.22 percentage points per year. Dividing by the true change in the participation growth rate, 0.28 percentage points per year, we can account for 77 percent of the turnaround in the employment growth rate relative to this counterfactual. However, this counterfactual implicitly makes the assumption that the increases in benefits were sustainable in yearly percentage terms, which implies still faster benefit growth than the baseline and therefore is still less realistic for the reasons described above.

A final possible counterfactual is that benefit levels would have stayed at their 1985 level. This is unrealistic, primarily because OASI benefits grow in real terms through the fact they are based on earnings (in the AIME and AMW formulae), which have on average grown in real terms over time. 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 older Americans' employment rates. It is possible, for example, that factors such as greater average educational attainment and prior labor market experience led to significant increases in older women's employment rates beginning around the same time, but that growth in OASI benefits led these increases to be slower than they otherwise would have been—and that the even faster OASI benefit growth prior to 1985 helped contribute to the downward slope in older women's employment rates over this period.[16]

8.6 Conclusion

We propose that a reduction in the growth rate of 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 sixty-one to 95. If these results apply more broadly, we calculate that the reduction in the growth rate of Social Security benefits can account for over one-quarter of the turnaround in the trend in older women's employment rates in the mid-1980s, relative to the counterfactual that benefit growth continued at the same rate in real terms. 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. In investigating the role that OASI may have played in explaining recent trends in older workers' employment, it would be valuable to complement this work by investigating further the potential role of substitution effects of OASI in explaining recent trends in older Americans' employment rates.

16. If the level of OASI benefits relative to prior income or wealth matters for the magnitude of the income effect—as we might expect, for example, if individuals display "habit formation" in their consumption and grow accustomed to their prior income—then the growth of prior income over time could help explain why employment grew after 1985 despite the contemporaneous rise in benefits.

Appendix

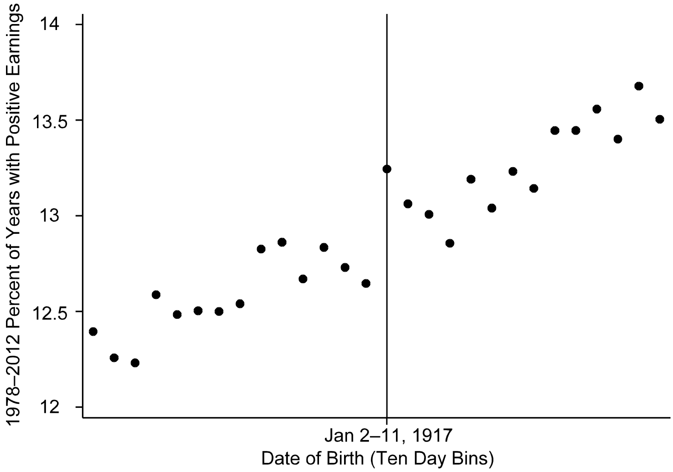

Fig. 8A.1 Percent of years with positive earnings, 1978 to 2012 (ages sixty-one to ninety-five), above-median average prior earnings, wider DOB range

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, among the group with above-median earnings prior to 1977, in a wider range of ten-day bins of DOB. (See other notes to figure 8.3.)

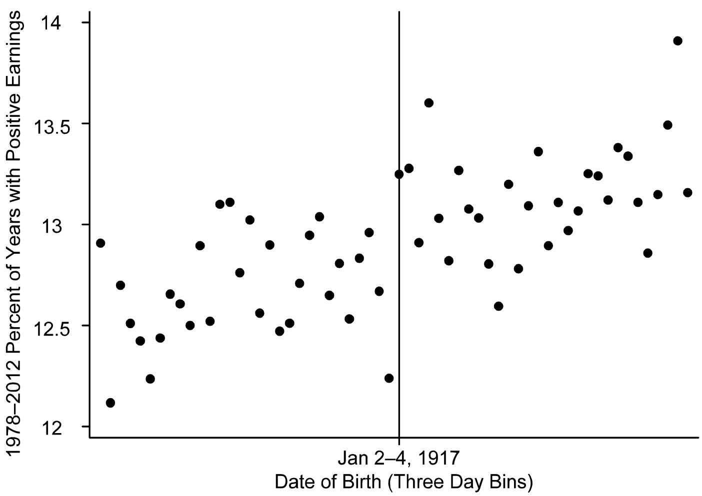

Fig. 8A.2 Percent of years with positive earnings, 1978 to 2012 (ages sixty-one to ninety-five), above-median average prior earnings, three-day bins of DOB

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, among the group with above-median earnings prior to 1977, in a wider range of ten-day bins of DOB. (See other notes to figure 8.3.)

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