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# The Effect of Social Security Benefits on Earnings: Novel Evidence from the Social Security Notch[1]

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## Abstract

We estimate the effect of Social Security benefits on earnings by examining the Social Security "Notch," which cut Old Age and Survivors Insurance (OASI) benefits by around $800 per year for men in the 1917 birth cohort relative to the 1916 cohort. This led to sharply different benefits for similar men born one day apart. Using Social Security Administration microdata on earnings in the full U.S. population by day of birth, we document a very large, visually clear, and statistically significant increase in elderly male earnings when moving from the end of the 1916 cohort to the beginning of the 1917 cohort. This contrasts with previous literature, based on yearly variation in birth timing, which found no evidence that the Notch had an impact on labor outcomes. The evidence suggests that the effect of OASI benefits on earnings is driven primarily by an income effect, and we find no evidence of a substitution effect. The estimates show that for a $1,000 decrease in discounted lifetime OASI benefits, mean discounted earnings in the elderly years rise by $432, and between the ages of 65 and 70 in particular mean yearly earnings rise by $754. Our results suggest that the near-tripling in mean OASI benefits from 1950 to 1985 can account for around one-quarter of the dramatic decline in the elderly male employment rate over this period.

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## 1. Introduction

The effect of incentives on work decisions is one of the central questions in public economics and labor economics. In particular, the effect of pensions on the work decisions of the elderly has been much studied, often reaching the conclusion that these programs cause large distortions (Gruber and Wise 2004). Using administrative data on the U.S. population, and investigating a context that has often been seen as one of the cleanest possible settings for studying the effects of pensions, we come to novel conclusions that offer a new take on the existing literature on the work effects of pensions.

Specifically, we study these questions in the context of the largest retirement program in the U.S., Social Security Old Age and Survivors' Insurance (OASI). OASI is the single largest U.S. federal program—with $679.5 billion in expenditures in 2013, representing nearly 20 percent of federal government spending (Social Security Administration 2014). Using Social Security Administration (SSA) administrative data on the U.S. population, we examine the Social Security "notch" created by the 1977 Social Security Act amendments (Krueger and Pischke 1992). As a result of these amendments, those born on or after the "threshold" date of Jan. 2, 1917 faced sharply different OASI benefits than those born before this date, allowing us to employ a Regression Discontinuity Design (RDD) to study the effect of benefits on men's earnings decisions. We show that for men born after this date, mean yearly real OASI benefits were discontinuously $772.51 lower than for those born before this date.[2]

The accurate data, large sample size, and large, discontinuous variation in Social Security benefits allows precise estimates of the effect of Social Security benefits on earnings and clear graphical documentation of the responses. The variation we investigate represents the largest existing discontinuous change in OASI benefits to our knowledge. For these reasons, the variation created by the notch provides a particularly promising environment for estimating the effects of pension incentives, which is of interest in understanding the earnings decisions of the elderly, and the effects of OASI, more broadly. The cuts in OASI benefits sustained by the notch cohorts are the same order of magnitude as many changes in OASI contemplated by policy-makers today. At the same time, the 1977 Social Security amendments are important to understand in their own right, as they represent one of the major historical changes in OASI policy, with the potential to affect aggregate trends in work decisions substantially.

[2] Except where otherwise noted, all dollar figures are in real 2012 dollars.

We find a very large, visually clear, discontinuous upward jump in mean earnings around the threshold date—*i.e.* moving from the end of the 1916 cohort to the beginning of the 1917 cohort—for elderly and near-elderly men. Over 1978 to 2000, the period over which we have earnings data and when individuals in the 1917 cohort reach ages 61 to 83, mean yearly earnings discontinuously rise by around $299.07 at the threshold ($p<0.01$).[3] Over 1982 to 1987, when individuals in the 1917 cohort reached ages 65 to 70—so they had reached Normal Retirement Age but had not yet reached the older elderly years that are associated with much lower employment rates—average yearly real earnings rise by $613.39 at the threshold ($p<0.01$). As "placebo" tests, we show that the discontinuity in earnings around this birth date does not appear (1) before the policy change was anticipated; (2) among men who were not eligible for Social Security benefits; or (3) at a birth cohort threshold that did not face a discontinuous change in Social Security benefits. We find substantial changes in the "extensive" margin: the probability that men earn a positive amount over 1978 to 2000 rises discontinuously by 0.88 percentage point at the threshold, and the probability over 1982 to 1987 rises discontinuously by 1.30 percentage points.

The core of the paper documents this large effect. We next turn to the mechanisms behind the effect. Gruber and Wise (2004) argue that that Social Security programs reduce the net return to work at older ages and cause substantial, distortionary substitution effects. At the same time, it is also possible that income effects could be large in the context of OASI (as in the unemployment insurance context investigated in Chetty 2008). This distinction is relevant because only substitution effects are associated with welfare losses (in the case of no pre-existing distortions), whereas income effects simply reflect transfers of resources rather than changes in relative prices. At the boundary of the 1916 and 1917 cohorts, the cut in OASI benefits should cause an income effect, but there is also a change in substitution incentives: the incentive to earn more at ages 62 and older decreased. This change in substitution incentives was very large: on average the net lifetime returns to an additional dollar of pre-transfer earnings fell discontinuously at the threshold by around 25 percent. By comparing the size of the earnings discontinuity in adjacent years in which individuals were subject to sharply different substitution

[3] We refer to an individual's "age" in a given calendar year as the highest age they attained over the course of this calendar year.

effects, we find no evidence of a substitution effect. In a baseline calculation, our 95 percent confidence interval rules out a substitution elasticity of 0.066 or higher.

Assuming that the changes in earnings are driven only by income effects, we calculate that a decrease in discounted lifetime OASI benefits of $1,000 causes an increase in mean discounted earnings from age 61 to age 83 of $432.46, and a $1,000 decrease in mean yearly benefits (discounted over the course of an average lifetime) causes an average increase in the probability of earning a positive amount in any given year from age 61 to age 83 of 0.98 percentage point. It is remarkable that during 1982 to 1987, when men in the 1917 cohort are between the ages of 65 and 70, we find nearly one-for-one crowdout, as a $1,000 decrease in yearly benefits (discounted over an average lifetime) leads to a very large, $753.80 increase in average yearly earnings over these years, and an increase in the yearly participation rate of 1.44 percentage point.

Given these large effects, it is possible that increases in OASI benefits over the latter half of the 20$^{th}$ century could be a substantial factor in the dramatic decline in the elderly male employment-to-population ratio over this period.[4] This ratio decreased to around one-third of its initial level, from 43.7 percent in 1950 to 15.3 percent in 1985 (Figure 1). Figure 1 also shows that this decline was coincident with a near-tripling in mean yearly OASI benefits from $4,846.68 to $13,407.72 over the same period (Social Security Administration 2013), and that more generally, periods of relatively faster increases in OASI benefits tend to be associated with periods of relatively faster declines in elderly male labor force participation. Previous literature has concluded that OASI benefits were not an important factor in explaining these trends (*e.g.* Krueger and Pischke 1992; Blau and Goodstein 2007), but our results suggest a new conclusion. If the income effects we estimate apply more broadly, we calculate that the large increase in OASI benefits over 1950 to 1985 would account for around one-quarter of the dramatic decrease in the elderly male employment-to-population ratio over this period. As shown in Figure 1, the stabilization of the male elderly employment rate in the mid-1980s occurs around the same time that the growth rate of mean OASI benefits slowed dramatically—a slowdown that was due to the dramatic changes of the 1977 Social Security Act amendments that we investigate in this paper. This stabilization in the elderly male employment rate stands in contrast to the

[4] The labor force participation rate shows similar trends over time to the employment-to-population ratio. We investigate the employment-to-population ratio because it is a closer to the outcome we observe in our empirical context.

employment rate in younger groups not receiving OASI, which tended to continue a downward trajectory through the 1980s and 1990s.

Our paper builds on the identification strategy in the important work of Krueger and Pischke (1992, hereafter "KP"), who also examine the Notch cohorts and find no evidence that Social Security benefits affect work activity using the Current Population Survey (CPS) data and variation across birth years in OASI benefits and labor force participation. In our view, KP's classic paper examining the Notch was executed as well as allowed by the data available at the time. We find results broadly similar to theirs when we run similar cross-year specifications on our data—leading us to conclude that it is the new data and identification strategy we use that explains the discrepancy in results. Our results isolate variation across cohorts at the day-of-birth level, as opposed to the year-of-birth level.[5] Our finer-grained data are crucial because the true effect on work participation revealed by our data is small relative to the cross-cohort variance in participation; thus, our data suggest that the yearly analysis turns out not to have the statistical power to detect an effect.

Our paper is also related to a large existing body of work on the effects of Social Security and other pensions on retirement decisions (*e.g.* Diamond and Hausman 1984; Fields and Mitchell 1984; Boskin and Hurd 1984; Gustman and Steinmeier 1985; Hausman and Wise 1985; Burtless 1986; Stock and Wise 1990; Costa 1995; Samwick 1998; Coile and Gruber 2000; Coile and Gruber 2004; Brown, Coile, and Weisbenner 2006).[6] Relative to the existing literature on pensions more broadly, our most important new finding is that income effects are very large in the context of a modern pension program.[7] Relative to existing work on OASI specifically, our paper is the first to our knowledge to estimate both income and substitution effects of OASI separately. Our estimates also offer a new take on time series patterns in elderly work decisions. Finally, our data enable us to perform precise estimates of earnings crowdout.

[5] Other literature has examined the effects of the Notch on other outcomes. Engelhardt, Gruber, and Perry (2002) examine the effect of the Notch on elderly living arrangements using variation at the year-of-birth level. Snyder and Evans (2006) estimate the effect of the Notch on mortality using variation at the quarter-of-birth level and a seven percent sample of the population.

[6] Within the literature on the effects of Social Security on retirement decisions, several papers have examined the effect of the Retirement Earnings Test (RET) (Burtless and Moffitt 1985; Friedberg 2000; Song and Manchester 2007; Gelber, Jones, and Sacks 2013). The RET is a substantially different context than ours, for example the RET creates large substitution effects but negligible income effects in the region of the exempt amount that is the focus of this literature (in contrast to the large income effects that we investigate).

[7] In the context of U.S. Civil War veterans, Costa (1995) finds large income effects of pensions on labor supply.

The remainder of the paper is structured as follows. Section 2 describes the policy environment. Section 3 explains our identification strategy. Section 4 describes the data. Section 5 describes our framework for understanding the empirical results and shows our graphical and regression analysis of the main data. Section 6 presents an analysis of the mechanisms that underlie the effect, specifically examining income and substitution effects. Section 7 discusses the relationship between our results and prior work on the Notch. Section 8 concludes.

**2. Policy Environment**

Individuals are eligible to claim OASI benefits as early as age 62, the Early Entitlement Age (EEA). At age 65, individuals reach the Normal Retirement Age (NRA), when they are eligible to claim their "full" OASI benefit.

The 1977 Social Security Act amendments changed the formula that determines OASI benefits as a function of OASI claimants' earnings histories. Prior to 1977, the Primary Insured Amount (PIA), which forms the basis for claimant's monthly Social Security benefit, was based on the Average Monthly Wage (AMW). The AMW was an average of an individual's nominal earnings over their highest-earning years.[8] For OASI beneficiaries who become eligible in 1970, for example, the PIA was calculated as: 81.83 percent of the first $110 of AMW, plus 29.76 percent of the next $290 of AMW, plus 27.81 percent of the next $150 of AMW, plus 32.69 percent of the next $100 of AMW. The 1972 Social Security Act amendments indexed the replacement rate within each bracket to the CPI. This created so-called "double-indexation" of Social Security benefits: inflation both increased the AMW (because AMW was calculated using nominal wages) and increased the replacement rate. In the high-inflation years of the late 1970s, this implied rapidly increasing benefits that were widely seen as financially unsustainable.

The 1977 amendments introduced a new formula that ended double indexation. In the new formula that applied to birth cohorts starting in 1922, PIA was based on Average Indexed Monthly Earnings (AIME). AIME is calculated as a function of past earnings, where earnings prior to age 62 (in the 35 highest-earning calendar years) are inflated by the growth in national earnings (calculated through Social Security's Average Wage Index (AWI)):

$$\text{AIME} = \sum_{t} w_{T-t} \frac{AWI_T}{AWI_{T-t}}$$

[8] The precise years of earnings used depend on the method SSA used. New methods were introduced in 1965, 1967, 1977, and 1978.

PIA is then calculated as 90 percent of earnings in an initial range of AIME; plus 32 percent of earnings in a second, higher range of AIME; plus 15 percent of earnings in a third, still higher range.[9]

This policy change implied sharply lower Social Security benefits for those receiving benefits under the new formula. To ease the transition to the new formula, the 1977 Amendments created a "transitional guarantee" for those born between 1917 and 1921 (inclusive). Claimants in these cohorts received the maximum of benefits calculated two ways:

1. Under the new formula based on the AIME; or
2. Under the old AMW formula with two changes:
    a. Earnings after age 61 are not used in calculating average earnings.
    b. After December 1978, no inflation adjustments are made to benefits until the calendar year in which an individual reaches age 62. Upon reaching age 62, benefits are increased according to inflation. This implies that the claimant does not receive the benefit of inflation adjustments to benefits that would have otherwise occurred between December 1978 and the calendar year when an individual reached age 62.

Method #2 of calculating benefits was called the "transitional guarantee," because it guaranteed claimants an OASI at least this large. The method of calculating the AMW under #2 was called the "transitional AMW."

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 sometimes use the term "cohort boundary" to refer to the boundary between the 1916 and 1917 cohorts defined in this manner.

In the 1917 birth cohort, the majority was covered by the transitional guarantee (McKay and Schobel 1981).[10] As a result, the 1916 cohort faced substantially different OASI benefits than those in the 1917 cohort. Moreover, this change sharply affected individuals by day of birth: those born near the beginning of the 1917 cohort faced a sharply different Social Security benefit structure than those born near the end of the 1917 cohort. This had two effects on earnings,

[9] Further information can be obtained in the *Social Security Annual Statistical Supplement* (Social Security Administration 2012a) and the *Social Security Handbook* (Social Security Administration 2012b).
[10] A very small percentage was covered by other methods, the 1977 Old Start Method (1.9%) or the Regular Minimum (0.6%) (McKay and Schobel 1981).

corresponding to an income effect and a substitution effect.

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 those in the 1917 cohort who worked after age 61 was typically substantially lower than the AMW of an individual with the same earnings history in the 1916 cohort would have been. This caused a substantial decrease in average benefits for those in the 1917 cohort relative to those in the 1916 cohort.[11] We calculate later that as a result of the policy change, on average yearly benefits were discontinuously $772.51 lower for those born near the beginning of the 1917 cohort than for those born near the end of the 1916 birth cohort. Under the typical presumption that leisure is a normal good, the income effect of this decrease in benefits should have led to increases in earnings at the discontinuity.

There was also a substitution effect. Because earnings after age 61 were not taken into account in calculating the AMW under the transitional guarantee, the marginal returns to additional work after age 61 fell. This effect was typically quite substantial. To get a sense of the magnitude, we calculate later that for an average earner in the 1916 cohort, earning an extra dollar after age 61 translated into a lifetime discounted gain in Social Security benefits around 25 cents, whereas earning an extra dollar had little effect on lifetime Social Security benefits in the 1917 cohort.[12] Thus, the net return to work fell by around 25 percent for otherwise similar individuals in the 1917 cohort relative to the 1916 cohort. In a compensated sense, the Slutsky relation implies that this substitution effect should have led to lower earnings in the 1917 cohort. In the Appendix, we show a simple lifecycle framework that guides our empirical analysis; the "substitution effect" we discuss corresponds to a Frisch (*i.e.* intertemporal) earnings elasticity in that framework (specifically comparing the lifetime net returns to additional earnings at ages 62 and 61 in the 1917 cohort relative to the 1916 cohort).

Thus, the net effect of the policy change on earnings at the discontinuity is ambiguous.

[11] Recall that after December 1978, no inflation adjustments are made to benefits until the calendar year when an individual reaches age 62. This factor did *not* discontinuously affect benefits for those born at the beginning of the 1917 birth cohort because individuals in this cohort were born just after December 1978. Thus, this factor is not responsible for the discontinuity in benefits between the 1916 and 1917 birth cohorts. Rather, the discontinuity is due to the lower AMW of those in the 1917 cohort, as explained above.

[12] 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.

*Ceteris paribus* the income effect should have led to discontinuously higher earnings in the 1917 cohort than in the 1916 cohort, whereas *ceteris paribus* the substitution effect should have led to discontinuously lower earnings in the 1917 cohort.

The size of these effects depended on inflation. Consider the income effect at the cohort break: larger inflation led to higher nominal earnings in the 1916 cohort,[13] which in turn led to larger AMWs in this cohort, which in turn led to greater OASI benefits in this cohort. In the 1917 cohort, meanwhile, the transitional AMW was unaffected by inflation subsequent to the age when this cohort reached 61 (i.e. subsequent to 1978). Thus, the size of the discontinuity in benefits paid at the cohort boundary increased as inflation increased. Because of double indexation, the size of the substitution effect should also have increased as inflation increased.

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

Note that there is a lag in calculating OASI benefits as a function of earnings: OASI benefits in a given calendar year are based on the individual's earnings history until the calendar year *two years prior*. Thus, there is no discontinuity across the cohort boundary in benefits paid *in 1979*. Benefits paid in 1979 should only change at the cohort boundary if there is a discontinuity in earnings in 1977, and we show later that there is no evidence for this.

The 1977 amendments also created a discontinuity in benefits at the 1917/1918 cohort boundary. The Appendix discusses this cohort boundary in detail. In brief, because no inflation adjustments are made to benefits until the calendar year in which an individual reaches age 62 under provision 2(b) above, this implied that inflation adjustments were delayed an extra year for those in the 1918 cohort relative to the 1917 cohort. This implied that individuals born in 1918 who were covered by the transitional guarantee had lower benefits than if they had been born in 1917 but otherwise had identical characteristics. However, the discontinuity in benefits at the 1917/1918 cohort boundary is much smaller than that at the 1916/1917 boundary; thus, our

[13] We assume that greater inflation translates into higher nominal earnings.

statistical analysis is mostly underpowered at the 1917/1918 boundary, and our results primarily focus on the 1916/1917 boundary.[14]

Another major policy change specifically affecting the elderly occurred around the same time as the Notch legislation: in 1978, the Age Discrimination in Employment Act (ADEA) was amended so that the ages at which age discrimination in employment was prohibited was extended from 40 to 65 (pre-1978) to 40 to 70 (1978 and after) (Barker and Clark 1980). This could have increased elderly work (Burkhauser and Quinn 1983). However, the ADEA legislation does not have a discontinuous effect on elderly work incentives around the 1916/1917 cohort boundary, as the Notch legislation did, and therefore should not confound our identification strategy. By the time the 1916 and 1917 cohorts reached ages 65 to 70 (*i.e.* 1982 to 1987), all individuals in these cohorts were affected by the change in mandatory retirement.

**3. Empirical Strategy**

For our primary empirical results, we rely on a RDD (see Imbens and Lemieux 2008 and Lee and Lemieux 2010 for surveys). The RDD exploits the discontinuous relationship between day of birth (DOB) and OASI benefits at the boundary of the 1916 and 1917 cohorts, relative to the assumed smooth relationship between DOB and average earnings that would obtain in the absence of the discontinuous change in OASI benefits. Thus, our evidence will effectively document whether we see a sharp change in earnings (and other outcome variables) at the cohort boundary. Specifically, we estimate the following regression:

$$E_i = \beta_1 D_i + \beta_2 DOB_i + \beta_3 (D*DOB)_i + \varepsilon_i \qquad (1)$$

Here $i$ indexes day of birth; $E$ represents an outcome of interest (such as mean real earnings); $D$ is a dummy for being born after January 2, 1917; $DOB$ is a linear trend in day of birth; and ($D*DOB$) is an interaction between $D$ and $DOB$. Because we control for both $DOB$ and the interaction of $D$ and $DOB$, we allow the slope to change at the cohort boundary—which in

[14] In principle, the fact that inflation adjustments are delayed until the calendar year an individual turns 62 under the transitional AMW formula could have created a discontinuity in benefits at other cohort boundaries like the 1918/1919 cohort boundary. However, in practice a sufficient fraction of the population in the 1918 cohort is covered by the AIME formula (rather than the transitional AMW formula) that the delay in inflation adjustments under the transitional AMW formula is largely irrelevant to benefits at the 1918/1919 cohort boundary (and subsequent cohort boundaries), and empirically there is no economically or statistically significant discontinuity in benefits between the 1918 and 1919 cohorts. As a result of these factors, our empirical work focuses on the 1916/1917 cohort boundary, while also examining the 1917/1918 cohort boundary as a secondary analysis. The transitional AMW formula was effectively irrelevant at these later cohort boundaries because realized inflation was sufficiently high that the AIME formula essentially always yielded higher benefits than the transitional AMW for individuals in these later cohorts.

practice makes little difference to our results, relative to constraining the slope to be equal on both sides of the discontinuity. The main coefficient of interest is $\beta_1$, representing the change in the mean level of the outcome variable at the cohort boundary in question. We use data aggregated to the day-of-birth level—rather than at the individual level—in order to estimate conservative standard errors. We weight the regression by the number of non-missing observations on each day of birth to yield the same point estimates as we would estimate using the individual-level data. When we examine a binary dependent variable as the outcome, *i.e.* a dummy for having positive earnings, we use a linear probability model, but we have estimated extremely similar implied point estimates with a probit or logit at the individual level (and clustered at the DOB level).

One issue is the choice of polynomial to estimate the relationship between the assignment and outcome variable. We call model (1) the "linear" specification because the control for DOB is a linear spline. Gelman and Imbens (2014) argue that RD designs should be based on local linear or quadratic polynomials on either side of the discontinuity. To demonstrate the robustness of our results to alternative polynomial controls, we additionally show our results with a quadratic version of model (1), as well as with a cubic version of model (1). In particular, we add a quadratic or cubic trend in DOB to model (1), as well as interactions of these quadratic or cubic trends with a dummy for being in the 1917 cohort.

It is also possible to control for various pre-determined covariates in model (1). In our data, we observe the fraction for which information on birthplace is missing in the SSA data; the fraction that is foreign born on each day; the fraction of the population born on each day that is white; and the fraction of the population born on each day that is male. In some specifications we control for these variables.

In our context, across all of the main outcome variables we investigate, the specification that minimizes the Akaike Information Criterion (AIC) and Bayes Information Criterion (BIC) is always the linear specification without controls. Thus, we choose this specification as our baseline.

On its own, model (1) is simply intended to illustrate the magnitude and statistical significance of the jump in earnings. In other words, considered on its own, (1) is not intended to estimate a parameter relevant to individuals' decisions. When we later explore the mechanisms

relevant to individuals' decisions, we will use the results of these regressions to estimate parameters relevant to individuals' decisions.

Of course, there are many other factors that may have affected the earnings of individuals in these cohorts—such as changes in private pension amounts, changes in health, and so on—but the assumption of the regression discontinuity specification is that such changes would have affected earnings in a way that is smooth 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. For example, the pandemic flu of 1918 could have affected the later earnings of individuals around these ages, but there is no reason why it would have had a sharply different effect on earnings on either side of the discontinuity. Similarly, there are macroeconomic factors that affected the economy as a whole, but there is no apparent reason why these should sharply affect earnings for those born near the beginning of the 1917 cohort relative to earnings for those born near the end of the 1916 cohort, absent the variation created by the Notch. We interpret the discontinuity in earnings at the cohort boundary as reflecting earnings supply, not changes in demand by firms, as workers born near the beginning of 1917 are presumably quite substitutable with those born near the end of 1916. It is important to use our fine-grained data by DOB, as otherwise our analysis could be confounded by other factors that lead to smooth trends in outcomes over the course of the calendar year (Buckles and Hungerman 2013).

As we discuss later, it will also be useful later to compare the magnitude of the effect of the Notch on OASI benefits to the magnitude of the effect on earnings or other outcomes. In order to do so, we can run a similar regression of mean lifetime discounted OASI benefits $B_i$ by DOB $i$ on the covariates above:

$$B_i = \beta_1 D_i + \beta_2 DOB_i + \beta_3 (D*DOB)_i + \varepsilon_i \qquad (2)$$

For illustrative purposes, in those cases in which we discount, benefits or earnings subsequent to 1979 are discounted at a three percent real interest rate, though our results are in the same range under a range of similar interest rates. In forming the measure of discounted lifetime benefits, we use information on benefits from the year the person began collecting OASI benefits to 2012 (or until the year that an individual died, whichever is earlier).

Under the assumption that income effects are the only relevant effects (consistent with our results), we can run a two-stage least squares regression in which we use the notch dummy to instrument for benefits: in other words, model (2) is the first stage, and the second stage is:

$$E_i = \alpha_1 B_i + \alpha_2 DOB_i + \alpha_2 (D * DOB)_i + \varepsilon_i \quad (3)$$

In the two-stage least squares case, we estimate the local average treatment effect of discounted OASI benefits on earnings.

**4. Data**

We use administrative data on the full U.S. male population from the Social Security Master Earnings File and Master Beneficiary Record. For the 1916, 1917 and 1918 birth cohorts, we were able to obtain data on day, month, and year of birth; OASI benefits paid in each year; day, month, and year of death; month and year of initially claiming OASI; gender; race; whether the person is foreign born; and annual FICA earnings in each year separately from 1951 to 2000. Information on annual total wages is also available in the data but only beginning in 1978. The results we show use only information on FICA earnings so that the measure of earnings is held constant from before 1978 to after; when we use information on annual total wages since 1978, we get extremely similar results to those shown.

These data measure pre-tax earnings, which is standard in literature on the effects of public programs on earnings; effects on pre-tax earnings are relevant to welfare evaluation (*e.g.* Chetty 2009). Similarly, our data on OASI benefits measure pre-tax benefits; this makes almost no difference to the results relative to measuring after-tax benefits, because OASI benefits only became taxable in 1984, and at that point the vast majority of individuals in our cohorts of had sufficiently low income that 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 earnings.[15]

Our measure of earnings includes self-employment income, though this is a small fraction of the sample and makes little difference to the results as we show. We drop individuals those who never had positive earnings prior to our period of interest begins in 1977; those who received DI benefits before our period of interest begins; and those who died before our period of interest begins.[16] Dropping those who received disability benefits in the period before the Notch

[15] Because after-tax benefits are slightly lower than pre-tax benefits, the dollar-for-dollar impact after-tax benefits on pre-tax earnings would be slightly larger than the effects of pre-tax benefits that we show.

[16] The effect of Social Security disability benefits on disability beneficiaries' earnings is itself a question of interest. However, our empirical design is not well-suited to study this issue. At the NRA of 65, disability beneficiaries collect Social Security benefits as an OASI beneficiary would. Thus, the period between the policy changes we study and reaching NRA was short for individuals in the cohorts we study, and the change in the incentive to claim disability due to the cohort discontinuities was therefore modest. Only a small fraction of men claim DI in the intervening period.

legislation has a negligible effect on the results, and dropping those who never have positive earnings prior to 1976 leads the coefficient estimates to be negligibly larger (since this represents a negligible fraction of the population). We keep the negligible fraction of the male sample that collects benefits as a dual beneficiary, secondary beneficiary, or survivor, though again this has a negligible effect on the results. After an individual dies, their earnings and benefits are counted as missing. Thus, observations on these individuals from the year after they die are not counted in the averages we compute by DOB or in any of the regression analysis or summary statistics.

Of course, it is equally important to understand women's responses and men's responses, but in most of our analysis we focus on men specifically. Studying women presents particular challenges in these data. Women typically earn less than their husbands in these cohorts, and when one spouse earns less than the other, under the OASI rules the lower-earning spouse in total receives the maximum of either: the benefit to which they are entitled on their own record, or one-half the benefit due to the higher earner. Thus, for those wives who are in total receiving one-half the benefit due to the higher earner, their total OASI benefit is constant no matter what side of the discontinuity their own DOB lies on, because their benefit instead depends on their husband's DOB.[17] For the higher earner (specifically non-dual-entitled primary beneficiaries), OASI benefits are discontinuous at the cohort boundary in their own DOB. Among the men that we study, nearly everyone (over 99 percent of the sample) collected OASI as a non-dual-entitled primary beneficiary, implying that their benefits are discontinuous in their own DOB. However, in these cohorts, women are typically collecting half the benefit of their husband (Social Security Annual Statistical Supplement 2013). We therefore expect the cohort boundary to correspond to a discontinuity in Social Security benefits for only a moderate fraction of women and have less statistical power to estimate effects on women's earnings. We show a basic set of results for women's earnings in this paper but study women's responses in more detail in a companion paper (Gelber, Isen, and Song 2014a).

With ideal data, it would be possible to study a husband's response to a wife's OASI benefit. However, we do not observe the husband's earnings history in our data for the full population, as we only observe husbands linked to their wives when one spouse is collecting through the other's record—which is endogenous to the size of the husband's and wife's separate

[17] This assumes that their earnings history is held constant in both cases (*i.e.* whether their DOB is in 1916 or 1917). It also assumes that in both 1916 and 1917, the OASI benefit associated with their own earnings history does not exceed one-half the benefit due to the primary earner.

benefits and is therefore discontinuous at the cohort boundary, preventing consistent estimation of causal effects.

We investigate nearly the full population, with the exception of the minor sample restrictions mentioned above (which have a negligible effect on the results), in order to avoid selecting the sample on endogenous outcomes. Accordingly, it does not make sense to limit the sample in any given year to men claiming in that year. Claiming OASI is potentially endogenous to one's OASI benefit, and the evidence below suggests such endogeneity (though it is not fully conclusive). Thus, the sample of claimants will be selected, again preventing consistent estimates of causal effects.

Table 1 shows summary statistics. We use data from 10,777,650 observations on the 414,525 individuals born within 100 days of the cohort boundary, averaged by DOB. (The bandwidth that cross-validation selects is always below 100.) Mean earnings over the full period we study, 1975 to 2000, is $11,267.42. Earnings tend to decline over the period of study: in 1975 to 1976, prior to the Notch amendments, mean earnings are $37,681.47. During the period from 1982 to 1987, when individuals in the 1917 cohort had reached NRA but had not yet reached the older elderly years, mean earnings were $7,012.88. Mean earnings fall to $2,217.45 from ages 71 to 83, in years 1988 to 2000. 26 percent of the sample has positive earnings in any given year from 1978 to 2000, and 29 percent has positive earnings in any given year from 1982 to 1987. The mean age of claiming OASI is 63.57. Mean self-employment earnings are $1,776.86, or 22.5 percent of total earnings. Each day on average has 2,072.63 observations, implying that each of the 10-day bins of data we show in our figures below has 20,726.3 observations on average.

## 5. Empirical analysis of effect on earnings

### Illustrative framework

To ground the empirical analysis in a framework, in the Appendix we review a simple lifecycle framework for understanding the effects of the policy change, adapted from Blundell and MaCurdy (1999) to a context in which we observe total earnings (as in our data). Our illustrative framework allows us to clarify conditions under which our estimates later are valid. We show it in the Appendix since the lifecycle labor supply model is standard.

Under the assumptions of this lifecycle model of lifetime labor supply, individuals' earnings should respond to the shock to lifetime income as soon as this shock is anticipated, while the substitution effect should only take effect in 1979 (the calendar year when individuals

in the 1917 cohort turn 62). Thus, it is possible in this framework to distinguish three sets of years:

1. Calendar years before the 1977 reform is anticipated. Empirically, we examine 1975 and 1976 as years before the reform was anticipated, when the discontinuous effects of the reform on the 1916 and 1917 cohorts (that forms the basis of our identification strategy) was not anticipated. We examine 1975 and 1976, rather than earlier years, because these years are more closely comparable to those after the Notch reform, but we have found extremely similar patterns in other years before 1977.
2. Calendar year 1978, which occurs after the 1977 reform is (anticipated and) passed but before individuals in the 1917 cohort reach age 62 in 1979. According to the framework above, earnings should adjust to the discontinuity in OASI benefits in calendar year 1978 because of the change in the marginal utility of lifetime wealth $\lambda$ resulting from the reduction in expected OASI benefits due to the 1977 amendments—in other words, the change in lifetime income associated with the 1977 amendments.
3. Calendar year 1979, when the substitution effect should begin to operate for those born in 1917.

If individuals are liquidity constrained or myopic, then it is possible that OASI benefit payments have effects on contemporaneous earnings that operate over and above the effect that operates through the channel of lifetime income (*e.g.* Blundell and MaCurdy 1999; Card, Chetty, and Weber 2007; Chetty 2008). In this case, it is also possible to distinguish a fourth set of years:

4. Calendar year 1981 and subsequent years, when the substitution effect is operative and contemporaneous OASI benefits paid discontinuously fall at the cohort boundary for the first time. In the lifecycle framework in the Appendix, calendar years 1981 and after should be conceptually indistinguishable from prior years. However, if individuals are myopic or face liquidity constraints, then the introduction of a downward discontinuity in OASI payments in 1981 would have an effect on earnings that does not occur through the channel of lifetime earnings.

Under the assumptions described in the appendix, which essentially specify that other exogenous factors are approximately constant from one calendar year to the next, we can estimate the $\lambda$-constant response of earnings to the substitution effect of OASI. Specifically, we can estimate

the discontinuity at the cohort boundary in the difference in earnings between 1979 and 1978, *i.e.* $E_{1979} - E_{1978}$ in the notation of the Appendix, since the marginal utility of lifetime wealth is constant between these years and therefore the income effect should have affected earnings comparably in the two years. We can separately estimate the discontinuity in the magnitude of the substitution incentives, *i.e.* $\mu_{1979} - \mu_{1978}$ in the notation of the Appendix, in 1979. We can transform this into a Frisch (*i.e.* $\lambda$-constant) elasticity with additional information about mean baseline earnings, *e.g.* mean earnings in the 1916 cohort in 1979, $\overline{E}_{1979,1916}$. Similarly, we can estimate whether liquidity effects play a role by estimating the difference in the level of the discontinuity in earnings in 1980 (when there is no downward discontinuity in benefits paid) and in 1981 (when there is such a downward discontinuity), $E_{1981} - E_{1980}$.

In this lifecycle framework, the marginal utility of lifetime wealth $\lambda$ depends on lifetime income, which in turn depends (among other things) on discounted lifetime OASI benefits.[18] This forms our motivation for investigating the discontinuity in discounted lifetime OASI benefits at the cohort boundary. Our primary specification examines the effect on lifetime discounted earnings, to make an apples-to-apples comparison with lifetime discounted earnings.

In practice, before estimating parameters that correspond more closely to the conceptual framework sketched above and in the Appendix, we begin our analysis by investigating what the raw data say, by simply estimating the discontinuity in average earnings over various sets of years by estimating model (1). In doing so, we often pool years together that occur in conceptually distinct periods. For example, we will often investigate the discontinuity in earnings over 1978 to 2000, in order to investigate the change in earnings over the full set of years of available data after the policy change. Once we have established the raw patterns of earnings, we turn to investigating the mechanisms, including estimating income and substitution effects.

**Preliminary empirical analysis**

We begin our empirical analysis with figures and regressions illustrating the basic patterns in the data. The figures show the means of outcome variables averaged by 10-day bins of DOB around the cohort boundary. 10-day bins represent the largest bin size that passes the two tests of excess smoothing recommended by Lee and Lemeieux (2010) for regression discontinuity designs. The graphical patterns are robust to using bins of other sizes, as we show.

[18] It is the expected discontinuity in lifetime discounted benefits that matters; under rational expectations, we can use the realized discontinuity as a proxy for the expected discontinuity.

We show eight 10-day bins on either side of the boundary because these bins contain 80 days, which roughly corresponds to the number of days contained in the bandwidth we use, 76. After each figure showing the mean of a given variable by DOB bin, we show a figure displaying the point estimates and 95 percent confidence intervals of regression (1) or (2), for all bandwidths between 10 and 100.[19] This allows us to determine the robustness and statistical significance of each effect that we observe in the raw data. The scaling of the y-axis will change across figures; for example, the y-axis spans a broader span in the case of Figure 5 than in the case of Figure 4. We change the scaling in order to illustrate the size of the variation associated with the cohort boundary, *relative* to the underlying variance in earnings across DOB bins in each particular context; the regressions help to assess the magnitude, significance, and robustness of these effects more formally. Corresponding to each main figure, we run regressions in Table 2 and elsewhere, using a bandwidth selected by cross-validation (Imbens and Lemieux 2008; Lee and Lemieux 2010).[20] For the main outcome we investigate—mean real yearly earnings from 1978 to 2000—the selected bandwidth is 76, and for the other outcomes the selected bandwidth is typically very similar. To hold the sample constant across specifications, in Table 2 we use this bandwidth throughout. In Appendix Table 2, we show that the results are comparable when we allow the bandwidth to vary across outcomes and use cross-validation to choose the bandwidth separately for each outcome. Table 2 shows the results for a variety of specifications, alternatively using linear, quadratic, or cubic controls, each shown both when we add the control variables percent white, percent foreign born, percent missing birthplace, and percent male by DOB) and without these controls.

Figure 2 verifies that cumulative discounted OASI benefits from 1978 to 2000 (discounted to 1977, and then expressed in real 2012 dollars) fall discontinuously and quite substantially when crossing the cohort boundary. Table 6 shows that at the baseline bandwidth, discounted lifetime benefits fall discontinuously by \$11,858.95 (standard error 811.60, $p<0.01$). Appendix Table 1 shows that this corresponds to a discontinuous decrease in mean yearly OASI benefits of \$772.51 at the cohort boundary.

Having verified that benefits change discontinuously at the discontinuity, we proceed to verify that the RDD strategy passes a basic validity test. If the number of observations is not

[19] For bandwidths under 10, the regressions typically show insignificant effects because they are underpowered. This makes sense as these regressions have fewer than 20 observations.
[20] The results are very similar when we use optimal bandwidth selection (Imbens and Kalyanaraman 2009).

smooth around the cohort boundary, then the RD strategy may not be valid (McCrary 2008). Figure 3(a) shows that the number of observations appears continuous around the cohort break. The regressions shown in Figure 3(b) and in Appendix Table 3 confirm that there is no significant change in the level of the number of observations at the cohort boundary.

**Main earnings results**

Figure 4 shows a main result: we observe a visually clear, sharp increase of several hundred dollars in mean yearly earnings in years 1978 to 2000 at the cohort boundary. This visual result is robust to using bins of other sizes (as are our other main results). As an example, we show the main results in Appendix Figure 1 using 5-day bins, which clearly also shows a discontinuity at the cohort boundary. Figure 4(b) shows that the discontinuity is always statistically significant. The point estimate of the earnings discontinuity does vary somewhat across bandwidths, though typically the estimates at different bandwidths are statistically indistinguishable. Appendix Figure 2 shows that when we control for covariates, we obtain very similar coefficients and confidence intervals at each bandwidth. Table 2 shows that in the baseline specification mean yearly earnings rise discontinuously by $299.07 at the cohort boundary (standard error 76.99, $p<0.01$). Row B of Table 2 also shows that when the dependent variable is discounted earnings from 1978 to 2000, there is a statistically significant discontinuity of $5,128.49 (standard error 1,216.49, $p<0.01$), which represents a comparable magnitude (since discounted earnings are summed over the 23 years from 1978 to 2000).

The years 1978 to 2000 include many later elderly years, such as ages 71 to 83. This group typically is retired, has low mean earnings, and is in poor health—and thus may have little ability to respond to changes in incentives. Empirically, the discontinuity in earnings appears most strongly in earlier years. To illustrate this, in Figure 5 we show the graph of earnings in 1982 to 1987, when individuals in the 1917 cohort reached ages 65 to 70. Figure 5(a) shows that there is a stark discontinuity in yearly earnings of several hundred dollars over years 1982 to 1987, *i.e.* after individuals in the 1917 cohort have reached the NRA of 65 but before they reach the older elderly years above age 70. Again, Figure 5(b) shows that the discontinuity in earnings is significant across all of the bandwidths considered, and Table 2 shows that in the baseline specification the estimated discontinuity in earnings at the cohort boundary over 1982 to 1987 is $613.39 (standard error 117.56, $p<0.01$). Appendix Figure 3 again shows that when we control for covariates, we obtain very similar coefficients and confidence intervals at each bandwidth.

**Extensive margin results**

Figure 6 shows the extensive margin, *i.e.* the probability that an individual has positive earnings in a given year, with the mean of this variable taken over 1978 to 2000. In panel (a), a clear jump in mean earnings is again visible at the discontinuity. Panel (b) shows that this effect is generally statistically significant. Table 3 shows that in the baseline specification, this variable shows a discontinuity of 0.88 percentage point at the cohort boundary ($p<0.01$). Appendix Figure 4 shows that we find a similar pattern over 1982 to 1987, with a larger point estimate of the discontinuity of 1.3 percentage points in Table 2 ($p<0.01$). Appendix Figure 5 shows the mean of the last calendar year an individual earned a positive amount. This also shows a clear jump at the discontinuity, which Table 3 shows is 0.23 years in the baseline specification.

Appendix Figure 6 shows the intensive margin, *i.e.* mean earnings conditional on having positive earnings. Little jump in mean earnings is visible at the discontinuity. The point estimate shown in Appendix Figure 6(b) is always positive and insignificant. An important limitation of the intensive margin analysis is that it selects the sample based on an outcome variable (*i.e.* whether earnings are positive or negative), which can lead to inconsistent estimates. Another limitation of the intensive margin analysis is that response of calendar year earnings conditional on positive earnings could be driven by later retirement within a calendar year, thus reflecting an *extensive* margin effect that occurs at a different point in the calendar year, rather than an intensive margin effect of the level of earnings while earning a positive amount.

**Other time periods**

In 1978 there is a pure lifetime income effect, with no change in substitution incentives at the cohort boundary. Appendix Figure 7 shows that earnings in 1978 show large point estimates that are generally significantly different from zero at the 5 percent for bandwidths 62 and under, and that are generally significantly different from zero at the 10 percent level (or more significant) at bandwidths 83 and under. At the bandwidth chosen for 1978 in particular, 33, the coefficient on the 1917 dummy is $824.27 with a standard error of 265.44 ($p<0.01$). This income effect is larger in magnitude than the discontinuity in average earnings over 1978 to 2000, and it is comparable in magnitude to the discontinuity in average earnings over 1982 to 1987. At the bandwidth of 76, the coefficient is 369.99 (standard error 195.51, $p<0.10$). Moreover, there is a clear and significant discontinuity in the extensive margin in 1978 at the baseline bandwidth (coefficient 0.0098, standard error 0.0025, $p<0.01$), and this is robust to all other relevant

specifications and all bandwidths between 10 and 100 (Appendix Figure 8). Thus, there is evidence that the lifetime income effect in 1978 has an effect on work behavior.

Appendix Figures 9 and 10 show that over 1978 to 1981, or over 1988 to 2000, there is some visual evidence of a jump in earnings, but it is less robust than it is from 1982 to 1987, or over the full time period from 1978 to 2000. Appendix Table 2 shows that these effects are statistically significant at the chosen bandwidths. In order to estimate an effect on the employment rate at ages 65 and above, which will parallel the information from the CPS over the 20th century, we also estimate an effect on a dummy for having positive earnings from 1982 on; the coefficient in this case again shows a discontinuity of 0.88 percentage point ($p<0.01$) at the bandwidth of 76 (and a similar discontinuity at the chosen bandwidth shown in Appendix Table 2). Extensive margin regressions for 1978 to 1981 or for 1988 to 2000 likewise suggest an effect, though a weaker one than in 1982 to 1987.

Because the legislation was signed on December 20, 1977, and the benefit discontinuity was plausibly only anticipated a short time in advance it is unclear whether we should expect a noticeable effect on earnings in 1977. When we run our regression with 1977 earnings as the dependent variable, the coefficient on the 1917 dummy is $220.89 (standard error 196.69, insignificant at conventional levels).

We also investigate the years 1979 to 2000 pooled, as an alternative to pooling 1978 to 2000 as in our main results—because we might be interested in the period when both the income and substitution effects operate. Appendix Figure 11 shows these results, which are extremely similar to the main results shown in Figure 4.

One potential concern about the extensive margin results in particular is that people could retire precisely on their birthday, perhaps when they become eligible for OASI at age 62, or when they reach full retirement age and become eligible for Medicare at 65. If so, then those born near the beginning of the calendar year (*e.g.* January 2, 1917) would have positive earnings for the calendar year in which they retire (*e.g.* for calendar year 1982 if they retire when they reach 65) because they earn a positive amount on January 1 of that year, whereas those born at the end of the previous year (*e.g.* December 31, 1916) would have zero earnings for that year. This could generate an upward discontinuity in the measured employment rate at the cohort boundary. We have already shown that our extensive margin results are robust to considering other periods (*e.g.* 1982 to 1987) that do not include 1979, the year when individuals in the 1917

cohort reach age 62. To further address this issue, we estimate model (1) when the dependent variable is the average yearly probability of positive earnings in 1983 to 1987, thus excluding age 65. In the baseline specification, the coefficient $\beta_1$ is 0.0123 (standard error 0.003, $p<0.01$), extremely similar to the baseline extensive margin results shown in Table 3. Moreover, when we define the extensive margin dummy as earning more than a modest positive amount (*e.g.* a dummy for earning more than $500 in a year), we again estimate extremely similar results. Note that the issue of retiring on one's birthday is also not a concern for our results on the level of earnings, because retiring precisely on one's birthday would create a smooth pattern of earnings around the cohort boundary.

**Validity, placebo, and robustness checks**

As a validity test, it is relevant to consider whether fixed characteristics of the study population are smooth around the cohort boundary. Appendix Figures 12 through 15 show that the percent foreign born, probability of that the SSA data are missing information on state of birth or country of origin, and the proportion of individuals that are male all appear relatively smooth through this cohort boundary. The regressions in Appendix Table 3 confirm that there is no significant change in the level of any of these variables at the cohort boundaries.[21]

We also show three placebo tests in Table 4. Our first placebo test shows earnings from 1975-1976, prior to the 1977 amendments. Figure 7 shows mean yearly earnings in 1975 to 1976 around the cohort boundary. Panel (a) shows that no discontinuity in earnings is evident near the cohort boundary, and panel (b) verifies that the discontinuity is insignificantly different from zero. Table 4 shows that there is no significant discontinuity in earnings in 1975 to 1976.

[21] One note of caution is in order. While Appendix Table 3 reassuringly shows that the proportion white is *not* discontinuously different on either side of the cohort boundary, Appendix Figure 15 shows that percent white is substantially lower in the bin immediately to the left of the cohort boundary than the surrounding bins. Investigating this pattern by individual day of birth, it turns out that whites represent a substantially lower fraction of births on two particularly salient dates—primarily on December 25 (Christmas day), and to a much smaller extent on January 1 (New Year's day)—but otherwise are a similar fraction of births on other days (see Appendix Figure 16). As we show in Appendix Figures 17 and 18, removing these two dates from our analysis shows nearly identical results to our analysis with these dates included. The results are also nearly identical when we only remove December 25, 1916. Moreover, removing other sets of relevant dates, such as Dec. 24-26 and Dec. 31 to Jan. 2, again yields nearly identical results to the basic analysis. Furthermore, we have already shown that the main regression results are very similar at all bandwidths when we control for the covariates, which include the fraction white. We also show placebo tests below that verify that a discontinuity in earnings at the cohort boundary does not appear in any of three placebo samples. The proportion of the sample that is white is lower on December 25 and January 1 in all three of these placebo samples, and we do not find any significant discontinuity in earnings in any of these placebo samples. We conclude that for all of these many reasons, the discontinuity in earnings that arises after the Notch legislation is unrelated to variation in any of the covariates, including the proportion white.

In our second placebo test, we obtained data on earnings from 1915 and 1916. These cohorts were not affected by the 1977 amendments and faced no discontinuity in Social Security benefit structure at the cohort cutoff. Figure 8 displays these results, which show no discontinuity in earnings, as we would expect. Table 4 confirms that again there is no significant discontinuity. We examine the 1915/1916 cohort boundary, rather than other cohort boundaries, because of the cohort boundaries on which we were able to obtain data, it is the best suited to be a placebo: it is adjacent to our main cohort boundary of interest (*i.e.* 1916/1917), but it is unaffected by the discontinuities in benefit structure that affect other nearby cohort boundaries (*i.e.* 1917/1918).[22]

In our third placebo test, we investigate the earnings of those who are not eligible for Social Security, for example because they do not accumulate sufficient earnings history or are state or local government workers who are ineligible. Figure 9 shows that for these workers, there is no detectable discontinuity in earnings. Table 4 again confirms that there is no significant discontinuity.

In the latter two placebo tests, our graph shows earnings for the period when the younger cohort is ages 65 to 70. That is when the sharpest discontinuity in earnings in the main sample occurs (i.e. in years 1982-1987, when individuals in the 1917 cohort turn ages 65-70); thus, the most "conservative" placebo test examines earnings over this period. Table 4 verifies that there is also no significant discontinuity for the full sample period. In sum, across all of our placebo tests, we find no evidence of a discontinuity in earnings in contexts not affected by the Notch.

**Other outcomes and samples**

The age of claiming Social Security could also respond to the discontinuity in benefits. Interestingly, Appendix Figure 19 shows a small discontinuity in the mean age of initially claiming Social Security, and this discontinuity is robustly significant across bandwidths. However, the jump in claim age is small: at the preferred bandwidth the point estimate only represents a jump of 0.05 years. We show this in the Appendix because these results are subject to an important caveat: we measure the age of first claiming at a *monthly* level, and if individuals tend to claim exactly on their birthday (*e.g.* exactly on their sixty-second birthday or within a few days hence), then we should see a discontinuity in the age of claiming on the first of each month

[22] Shortly after the 1977 amendments, individuals around the 1918/1919 boundary and subsequent cohort boundaries could have expected discontinuities in benefits at the cohort boundaries—before the realization of high inflation in these years—so these later cohort boundaries will not allow suitable "placebo" tests; instead, we turn to three other samples for placebo analyses as we describe later.

because of the coarse (*i.e.* monthly) nature of the data on the time of claiming. In fact, there is some evidence that this is the case in placebo samples at other months, as it is evidence in Appendix Figure 19 that the age of claiming is higher every three bins (beginning with the Jan. 2-11, 1917 bin) than in adjacent bins. Thus, ultimately we lack conclusive evidence on the effect on claiming.

Appendix Figure 20 shows the earnings patterns around the 1917/1918 cohort boundary. This discontinuity featured much smaller changes in average benefits, so the variation in benefits driving the results is less pronounced and we might expect less pronounced and less easily interpretable earnings responses. In fact, we do find some evidence of earnings responses at this cohort boundary, but the evidence is less clear-cut than in the case of the 1916/1917 cohort boundary. In the baseline specification at the bandwidth of 76, the discontinuity in earnings is \$184.54 (standard error 105.59, $p<0.10$), and at the bandwidth chosen for 1917/1918 in particular, the discontinuity is \$207.24 (standard error 109.29, $p<0.10$).

The measure of earnings we have investigated thus far includes self-employment earnings, but we can separately investigate how OASI benefits affect self-employment earnings in particular, as the self-employed may have more flexible earnings for a number of reasons: they can set their own hours, and it is typically believed that they have a particular ability to avoid and evade taxes through changes in reporting earnings. Appendix Figure 21 displays these results, which shows no clear evidence of an effect. When we run our regressions for self-employment earnings, we find insignificant results: when mean yearly self-employment earnings from 1978 to 2000 is the dependent variable and we use the baseline bandwidth of 76, the coefficient on the "1917 cohort" dummy is -\$66.49 with a standard error of 42.05 ($p = 0.12$). The coefficients are similarly insignificant when the dependent variable is self-employment earnings from 1982 to 1987. Under about one-quarter of bandwidths from 10 to 100, the estimates are negative and significant, though the point estimates are modest in these cases (always well under \$100) and the confidence intervals rule out decreases greater in absolute value than at most \$200—and moreover there is no clear visual evidence of an effect. We conclude that there is no convincing evidence of an effect.

As we described above, we are less well-powered to investigate the effect of the notch on the earnings of women. As a preview of our companion paper on women (Gelber, Isen, and Song 2014a), we show a basic set of results for women in Appendix Figure 22, which shows evidence

of a discontinuity in earnings over 1978 to 2000. At the baseline bandwidth of 76, we find a significant discontinuity in mean earnings over 1978 to 2000 of $85.62 (standard error 43.04, $p<0.05$), and a significant discontinuity in mean earnings over 1982 to 1987 of $184.75 (standard error 62.31, $p<0.01$). The discontinuity in earnings is more visually and statistically robust over 1982 to 1987 (Appendix Figure 23), where it is significant at every bandwidth shown.

Another outcome of interest is mortality (Snyder and Evans 2006), which we also address in a companion paper (Gelber, Isen, and Song 2014b). When an individual dies, we set their earnings to zero in our data; thus, some of the effect on earnings we find could in principle be mediated through an effect on mortality, though our companion paper does not find evidence for such a mechanism.

## 6. Analysis of Mechanisms

The previous section demonstrates the central finding of the paper: that the benefit changes caused large, sharp increases in earnings at the cohort boundary. In this section, we explore the mechanisms that underlie this response, specifically income and substitution effects. Our RDD empirical design is very well-suited to investigate the effect of the discontinuous change in the OASI benefit schedule at the cohort boundary on earnings, but our exploration of the mechanisms is necessarily more suggestive and requires further assumptions.

Earnings increase discontinuously for those born after the cohort boundary. The income effect should lead to higher earnings above the discontinuity (assuming that lower OASI benefits lead to higher earnings, consistent with the typical presumption that leisure is a normal good), whereas the substitution effect should lead to lower earnings for this group. We find that individuals born after the discontinuity have higher earnings on average; thus, the income effect outweighs the substitution effect in this sense.[23] Next, we assess whether there is a measurable substitution effect.

### Evidence from Comparison of 1978 and 1979

As explained above and in the Appendix, there was a discontinuously greater incentive to earn more in the 1916 cohort relative to the 1917 cohort in 1979, but not in 1978 (whereas other factors, including lifetime income, can be assumed to be approximately constant from one year to the adjacent year). Thus, if there is a substitution effect, $E_{1979} - E_{1978}$ (*i.e.* earnings in 1979

[23] The evidence is therefore consistent with the presumption that greater unearned income leads to lower earnings, consistent with the usual presumption.

minus earnings in 1978) should show a downward jump at the discontinuity (using the notation of equation (6) of the theory appendix). Note also that because of the substitution effect, there is a downward jump in the incentives to *report* additional income earned; thus, our analysis of the substitution effect could reflect reporting responses as well.

If there is even a moderate substitution elasticity, the downward discontinuity in this variable should be quite large. To get a sense of how large the discontinuity in earnings should be, consider an illustrative back-of-the-envelope calculation. For a typical beneficiary in the 1916 cohort, an increase in earnings of one dollar at ages 62 and above could lead to an increase in lifetime OASI benefits around 25 cents,[24] whereas for a typical beneficiary in the 1917 cohort, an increase in earnings of one dollar at ages 62 and above would lead to no change in lifetime OASI benefits. Thus, the marginal increase in lifetime net consumption from an extra dollar of earnings is 25 percent higher for an individual in the 1916 cohort than for one in the 1917 cohort in 1979, but there is no such discontinuity in 1978. For illustrative purposes, assume for the sake of argument that there is a substitution elasticity of 0.25. As a result, the discontinuity in earnings in $E_{1979} - E_{1978}$ should be 2.13 percent of baseline earnings (where 2.13 percent = 25 percent x 0.25 x 34.01 percent who claim OASI at 62), or around a $640 downward discontinuity in earnings—in the same range as the size of the discontinuity in earnings over 1982 to 1987, which very clearly shows up both graphically and statistically.[25]

In fact, no such discontinuity emerges. For each date of birth $i$, define the difference $\Delta E_i$ in mean earnings between 1978 and 1979:

$$\Delta E_i = \overline{E}_{i,1979} - \overline{E}_{i,1978}$$

Panel (a) of Figure 10 shows a graph of $\Delta E_i$ around the cohort boundary, which shows no evidence of a sharp jump downward in earnings in 1979 relative to 1978; if anything, earnings appear to rise modestly though not discontinuously around the cohort boundary. Panel (b) shows that there is no significant discontinuity in this difference in earnings. Table 5 row A shows the estimates of the discontinuity, which shows no significant effect. The point estimates are

[24] For an average earner, the marginal rate at which PIA replaced AMW was approximately 50 percent in the 1916 cohort. Because AMW was averaged over a mean of around 25 years under the 1978 New Start Method, earning an extra dollar in a given year under the rules affecting the 1916 cohort raised AMW by around 100/25 = 4 cents, which translated into an increase in PIA around 4*50 percent = 2 cents. Over a 16-year expected lifespan (conditional on reaching claiming age), this raised lifetime Social Security benefits by around 25 cents.

[25] "Baseline" earnings are calculated as mean earnings in 1979 of those in the 1916 cohort born within 100 days of the boundary.

positive. The confidence interval does not rule out some downward discontinuity in earnings, though they rule out a more than a small one. One might be concerned that the mean level of earnings also changed from 1978 to 1979; to address this issue, one may wish to run a specification in which we examine the change from 1978 to 1979 in the mean of log earnings. However, estimating exactly this specification would lead to a problem in our context: we would like to include observations in the regressions when individuals have zero earnings, which is frequent in our context, but the log of zero is undefined. Thus, we approximate the log of earnings using the inverse hyperbolic sine (IHS) of earnings, which is similar to log earnings for large values of the argument but is defined at zero and negative values (*e.g.* see related work in Burbidge, Magee, and Robb 1988, Pence 2006, or Gelber 2011). The inverse hyperbolic sine of earnings $E$ is defined as:[26]

$$\sinh^{-1}(E) = \ln(E + \sqrt{1 + E^2})$$

We examine the difference between the mean of the IHS of earnings in 1979 and the mean of the IHS of earnings in 1978 in row B of Table 5. This corresponds to a specification in which the IHS of earnings is related to the independent variables of interest, as explained in the Appendix. Again, we find no evidence of a significant downward discontinuity, with small 95 percent confidence intervals that rule out more than a small downward discontinuity.

We compare the discontinuity in 1979 and 1978 because these adjacent years are therefore the closest comparison years that faced sharply different incentives. Two further assumptions that underlie this strategy appear to be consistent with the available empirical evidence. First, because unanticipated inflation affected the size of the income effect, the discontinuity in expected lifetime OASI benefits at the cohort boundary could have been modestly different in 1979 than in 1978. Note, however, that unanticipated inflation in one year is unlikely to affect the size of the discontinuity in lifetime benefits greatly, as the magnitude of this discontinuity depends on the whole course of inflation over all subsequent years. Moreover, if this did have a substantial effect on earnings decisions, we might expect to see an increase in the size of the discontinuity in earnings at the 1917/1918 cohort from 1978 to 1979. The size of the discontinuity in benefits at the 1917/1918 also increases with (unanticipated) inflation: benefits were lower in the 1918 cohort than the 1917 cohort because under the transitional AMW

[26] A more general form of the inverse hyperbolic sine function adds a scaling parameter; our results are similar when we use other scaling parameters.

formula those born near the beginning of the 1918 cohort do not benefit from the inflation adjustment for an additional year relative to the 1917 cohort (because benefits are frozen at the December 1978 level until the calendar year an individual turns 62, *i.e.* 1980 for the 1918 cohort, or 1979 for the 1917 cohort). Thus, the higher inflation is, the lower benefits will be in the 1918 cohort relative to the 1917 cohort. Crucially, the substitution effect at the 1917/1918 cohort boundary is essentially nonexistent in *both* 1978 and 1979, since the substitution effect in these cohorts only applied beginning in 1980. Appendix Figure 24 shows that there is no visually or statistically apparent discontinuity in this first-difference in earnings. Thus, the evidence does bear out this alternative hypothesis.

Second, looking for a substitution effect at age 62 assumes that individuals were able to react to the incentives within the year. With such a large change in substitution incentives, one might expect that if there were a response at all, then some measurable portion of it would appear in the first year. In fact, in our context we do find evidence of a reaction to other incentives within a year, as we find that there is some evidence that earnings respond to the lifetime income effect in 1978 (*i.e.* within one year of 1977). This is consistent with the findings of Gelber, Jones, and Sacks (2013), who find in the context of the Social Security Earnings Test that the earnings of elderly individuals react almost fully to large policy changes within the year that the policy change occurs. However, we acknowledge that our main empirical evidence in Section 5 requires fewer assumptions than our evidence relating to the mechanisms underlying this effect.

**Testing for liquidity effects**

Chetty (2008) finds that liquidity effects are important in the case of unemployment insurance. In our context, in which individuals are near retirement age and therefore typically have accumulated substantial assets (particularly in these cohorts, in which savings rates were higher than they are today), it would arguably be surprising to find large liquidity effects. Moreover, at these near-elderly ages individuals typically should not want to borrow since their incomes are typically falling thereafter (rather than rising as they would generally have to do in order to make borrowing attractive). Nevertheless, it is illuminating to test for liquidity effects.

Recall that OASI benefits in a given calendar year are calculated on the basis of earnings until two calendar years prior. Since earnings in 1979 and after did not count toward the transitional AMW in the 1917 cohort, benefits discontinuously *fell* at the cohort boundary beginning in 1981. If there are liquidity effects and leisure is a normal good, we would predict

that the earnings discontinuity would become larger in 1981 than in 1980. To examine whether this is the case, we can examine whether there is a discontinuity in the difference in earnings between 1981 and 1980:

$$\Delta E_i = \bar{E}_{i,1981} - \bar{E}_{i,1980}$$

If there are liquidity effects, $\Delta E_i$ would jump upward at the cohort boundary. Appendix Figure 25 shows the results. Panel (a) shows that there is no upward jump in earnings visible at the cohort boundary. Panel (b) shows that there is never a positive and significant coefficient, contrary to the prediction if there are liquidity effects. In fact, the point estimate is always negative.

If hypothetically there were only a liquidity effect, and no effect of lifetime income, we would expect no discontinuity in 1978, when the reform was anticipated but benefit payments were not yet discontinuous at the cohort boundary. We have already demonstrated that there is some evidence of an earnings discontinuity in 1978, and strong evidence of a participation discontinuity in 1978. Thus, our evidence indicates a response to the discontinuity in lifetime income, and we reject the hypothesis that there are *only* liquidity effects (*i.e.* with no separate effect of lifetime income).

Thus, we do not reject the null of the lifecycle model. This serves as justification for examining earnings in 1978 to 2000 pooled. While we do not reject this null given our data, we emphasize that our results do not rule out a liquidity effect; in fact, the 95 percent confidence interval shown in Appendix Figure 25 does not rule out a substantial liquidity effect. Moreover, the discontinuity in benefits in 1980 is modest relative to the discontinuity for those who retire in later years, suggesting that it is possible that we lack the power to detect an effect. We show this evidence not to rule out liquidity effects, but instead simply to test our null of the lifecycle model. Clearly our evidence also does not rule out that liquidity effects are important in other contexts, in which liquidity constraints are more relevant than in our elderly and near-elderly sample. Morover, our results are nearly identical when instead of pooling data from 1978 to 2000, we instead pool data from 1981 to 2000 (reflecting the years when OASI affects liquidity).

**Calculation of Implied Substitution and Income Effects**

Using the regression estimates, we can calculate the implied substitution and income effects that are consistent with the data. When the difference between 1979 and 1978 earnings is the dependent variable in row A of Table 5, the lower end of the 95 percent confidence interval is -168.60. This represents a 0.56 percent change in earnings relative to mean earnings in 1979 in

the 1916 cohort within 76 days of the cohort boundary. We also calculate the mean change at the discontinuity in the net return to work $\mu_{1979} - \mu_{1978}$, by calculating the mean increase in lifetime discounted OASI benefits from a dollar of extra earnings separately in each of these years for an individual who claims at 62, and we let this be the dependent variable in model (1). The discontinuity (*i.e.* coefficient $\beta_1$ in this regression) is 0.25 (standard error 0.0056). Since 34.01 percent of individuals claim OASI at age 62 in these cohorts, this implies that at the sample means, the maximum substitution elasticity consistent with the data is 0.066. Similarly, using the results of the IHS specification (Table 5 row B), we calculate that the maximum substitution elasticity consistent with the 95 percent confidence interval is 0.046. For this IHS calculation, we do not need to specify that the elasticity is calculated at the mean of earnings but instead can assume that the elasticity represents the mean throughout the earnings distribution.

We also calculate the income effects consistent with the data, in several different ways. First, as a benchmark, we calculate the income effect associated with the changes in OASI benefits at the cohort discontinuities, assuming that there is no substitution effect—a scenario that we cannot rule out with the analysis above. In this context, we can use regressions (2) and (3) to perform a two-stage least squares (2SLS) estimate of the effect of OASI benefit levels on earnings. As in all of our other regression analysis, in both stages of the 2SLS regression, we use data averaged at the day-of-birth level.

Table 6 shows the first stage, i.e. model (2). The first two columns show that actual lifetime discounted OASI benefits discontinuously fall by around $12,000 at the cohort boundary. This is relevant to a two-stage least squares specification that estimates the effect of the change in actual OASI benefits on earnings.

A different specification in column 3 estimates the discontinuity in the *scheduled* lifetime benefit. This is relevant to a two-stage least squares specification that estimates the effect of a *scheduled* change in lifetime OASI benefits on lifetime earnings (taking inflation as given). In other words, the observed discontinuity in benefits at the cohort boundary is influenced by both the discontinuity in scheduled benefits and the endogenous response to this change in benefits. Policy-makers may be interested in the effect on earnings of the cuts in benefits that they schedule, holding behavior fixed. Thus, in the specification in columns 3 and 4, we estimate the first stage of a two-stage least squares regression in which the cohort boundary dummy serves as an instrument for "simulated" OASI benefits that an individual could have if their behavior were

unaffected by the 1977 amendments. Specifically, we simulate earnings in calendar years *t* =1977, 1978…2000, by using the 1916 cohort data within 100 days of the cohort boundary, and separately regressing earnings in each year *t* on earnings in 1975 and its square, as well as earnings in 1976 and its square:

$$E_{it} = \beta_0 + \beta_1 E_{i,1975} + \beta_2 E_{i,1975}^2 + \beta_3 E_{i,1976} + \beta_4 E_{i,1976}^2 + \varepsilon_{it}$$

In *both* the 1916 and 1917 cohorts, we then simulate earnings for each individual *i* in each calendar year *t* by using the estimates $\hat{\beta}_1$, $\hat{\beta}_2$, $\hat{\beta}_3$, and $\hat{\beta}_4$ to obtain the fitted values $\hat{E}_{it}$, and add a random term $\tilde{u}_{it}$ that reflects the within-person variance of earnings in the 1916 cohort. Thus, we obtain "simulated" earnings $\tilde{E}_{it} = \hat{E}_{it} + u_{it}$.[27] We then calculate "scheduled" OASI benefits $\tilde{B}_{it}$ for each individual *i* in the 1916 and 1917 cohorts in each calendar year *t* by assuming that each individual in both of these cohorts had simulated earnings $\tilde{E}_{it}$ in each year; this forms our measure of "scheduled" OASI benefits. The first-stage estimates in columns 3 and 4 of Table 6 relying on scheduled benefits are modestly larger than those relying on actual benefits.[28]

In Table 7, we show the estimates of the two-stage least squares regression model, (2) and (3). In the baseline specification in column 1 row A, this regression indicates that a dollar of extra actual lifetime OASI benefits is associated with a decrease in mean discounted lifetime earnings of 43 cents, which is statistically significantly different from zero ($p$<.01). Column 2 shows that this estimate is very similar when we add controls. Columns 3 and 4 show that the

[27] This random term is conceptually necessary because higher moments (beyond the mean) of the earnings distribution help to determine the path of OASI benefits, and therefore we must model the year-to-year variance of earnings in order to correctly capture the path of OASI benefits. Specifically, we need to capture the fact that for some individuals in the 1916 cohort, earnings are sufficiently high in the post-retirement years to raise their AMW and therefore raise their OASI benefit—whereas this is not true for other individuals. Without the random term, the model predicts only mean earnings, with no variance, and therefore counterfactually under-predicts OASI benefits in the 1916 cohort (on which the simulated earnings measure is based), as mean earnings tend to fall from year to year in the elderly and near-elderly years.

[28] It is not surprising that the discontinuity in scheduled benefits is similar to the discontinuity in actual benefits. In the 1916 cohort, scheduled and actual benefits should be very similar if we have modeled earnings appropriately (since simulated benefits are calculated using actual earnings in 1916). In the 1917 cohort, earnings in 1979 and after should not affect actual benefits through the channel of the transitional AMW, because the transitional AMW is unaffected by earnings in 1979 and after. Thus, the only ways in which earnings after 1977 affect the benefits of the 1917 cohort are: (a) earnings in 1977 and 1978 can affect benefits (this factor should *ceteris paribus* decrease the discontinuity in scheduled benefits relative to the discontinuity in actual benefits), (b) claim age could have responded, though given that the actuarial adjustment is roughly actuarially fair this should have had a small effect on lifetime discounted benefits; and (c) through an interaction with the Earnings Test, earnings can affect the Delayed Retirement Credit, which was less generous than actuarially fair (and thus this factor should *ceteris paribus* increase the discontinuity in scheduled benefits relative to the discontinuity in actual benefits). However, these are relatively minor factors, and they offset each other to some extent.

estimates are only slightly smaller when the endogenous variable is scheduled OASI benefits (rather than actual OASI benefits).

A different way to calculate income effects is under the assumption that the substitution elasticity is 0.066, the maximum allowed by the intensive margin confidence interval under the strategy comparing earnings in 1979 and 1978. Specifically, we calculate $\mu_{it}$ for each individual $i$ in each year $t$; we multiply this by the substitution elasticity $\varepsilon$=0.066 to calculate the implied effect on earnings; and we subtract this from actual earnings $E_{it}$ to form a transformed measure of earnings $\tilde{E}_{it} = E_{it} - 0.066\mu_{it}$. We then discount $\tilde{E}_{it}$ over the course of an individual's lifetime and let this be the dependent variable in model (2)-(3).

Row B of Table 7 shows these results, which are similar to the results when we assume no substitution effect. Note that these results show slightly *larger* income effects (in absolute value) than the results under the assumption of no substitution effect, and that any larger substitution effect would imply even larger income effects (in absolute value). Thus, we could consider the estimates in row A to be a lower bound on the income effect, and under the further assumptions detailed in the Appendix, we could consider the estimates in row B to be an upper bound on the income effect.

It is notable that these estimates are very similar to the implied income effects at the 1917/1918 cohort boundary. The discontinuity in discounted lifetime benefits at the 1917/1918 cohort boundary is \$7,139.20 ($p$<0.01), implying that a \$1 increase in lifetime discounted benefits leads to a 40-cent reduction in discounted lifetime earnings (assuming a lack of substitution effects at the 1917/1918 cohort boundary, consistent with the results at the 1916/1917 boundary), very close to the 43-cent crowdout found at the 1916/1917 cohort boundary.

Though in many cases these various estimates refer to different objects of interest, it is worth noting that throughout all of the estimates in Table 7, the estimated income effects are quite large. They all show that an increase in lifetime discounted OASI benefits causes a decrease in mean yearly earnings in the range of 0.35 to 0.45 cents.

It is also possible to consider the substitution effect at the extensive margin. If individuals face a fixed cost of earning a positive amount, then they can make a choice between earning nothing and earning a substantial positive amount (*e.g.* Cogan 1981). This is particularly relevant in the elderly years as individuals consider whether to retire. In this case, we must consider the

net return to working as opposed to not working. Like in the intensive margin case, on the extensive margin the substitution effect leads to a lower incentive to work for those in the 1917 cohort than for those in the 1916 cohort. We can calculate the extensive margin substitution effect at age 62 by calculating the change in lifetime discounted OASI benefits from having zero earnings at age 62, rather than having the individual's simulated earnings at age 62. With this variable as the dependent variable, we run model (1); the coefficient $\beta_1$ is equal to -4,200.78, or 14 percent of mean simulated earnings at age 62. The implied upper bound on the elasticity of the probability of participation with respect to the extensive margin substitution effect—calculated using the lower bound of the 95 percent confidence interval on $\beta_1$ from a regression in which the dependent variable is the difference in the mean probability of working between 1979 and 1978 by DOB—is 0.019.

In principle, it would be possible to estimate a model in which data from all ages were used to estimate the income and substitution effects in a differences-in-differences discontinuity framework (*i.e.* comparing the magnitude of the earnings discontinuity with the magnitude of the discontinuity in income and substitution incentives, across all ages). Conditional on a DOB fixed effect, the substitution elasticity (*i.e.* Frisch elasticity) would effectively be identified from changes across ages in the net returns to work (and the differential changes in these net returns across the 1916 and 1917 cohorts). However, this model would run into problems if the substitution elasticity were heterogeneous across ages and the size of the substitution incentives (i.e. $\mu$) were correlated across age with the substitution elasticity. In this case, the model would estimate a weighted average of the elasticity in different population groups. This is likely to be an important worry, as the resulting estimates may be a very misleading estimate of the population average elasticity. For example, the net returns to extra work $\mu$ are highest in the 1916 cohort at younger ages, when the lifetime consequences for OASI benefits of a given change in earnings tend to be largest because they are discounted over a maximum of future years of benefit receipt. At the same time, those in the late elderly years tend to have very low earnings and may be less responsive to changes in incentives than those at younger ages (as our results suggest). Thus, there could be a *negative* correlation between the incentive for extra work and individuals' earnings, despite the fact that no individual in the population has a negative elasticity in a standard model (as this would violate the restrictions of the Slutsky equation, specifically that the Slutsky matrix is negative semidefinite). Because of such worries, we

instead estimate the substitution effect by comparing behavior at adjacent ages that appear more closely comparable than ages separated by decades.

**Heterogeneity in income effects**

It is of interest to investigate whether the income effects are heterogeneous in different populations. In this analysis, we are restricted by the limited set of demographic variables we have in our data: we are underpowered to investigate the effect among those in some of the demographic groups defined by our control variables (non-whites, foreign-born, and those missing birthplace information, each of which forms only a modest fraction of the full sample). However, we are able to investigate how effects vary among those with above- or below-median average earnings in the SSA data prior to 1977, *i.e.* from 1951 to 1976. The discontinuity in benefits was different in these groups, implying that in order to calculate income effects, we must also estimate the discontinuity in lifetime discounted benefits separately in these groups. Table 8 shows that the point estimate of the discontinuity in both earnings and in discounted lifetime benefits is larger in the high lifetime earnings group. The implied income effect (again assuming no substitution effect) is moderately larger in the high lifetime earnings group, though it is insignificantly different from that in the lower lifetime earnings group. We show results for the baseline specification, but as before they are very similar under other specifications such as adding controls.

**7. Relationship to Krueger and Pischke (1992)**

The work of Krueger and Pischke (KP, 1992) started the literature on the Notch, and it is worth considering the reasons for the apparent discrepancy between their results and ours. There are three primary differences between their empirical strategy and ours:

1. We use SSA administrative data on the full population, whereas KP use CPS survey data on a population sample that represents less than 0.1 percent of the population (because these were the data available at the time);
2. Our variation is based on daily variation in Social Security benefits across birth cohorts and an RDD design, whereas KP's identification is primarily based on variation in Social Security benefits across yearly birth cohorts—again because these were the best data available at the time;
3. KP's primary outcome of interest is the log odds ratio of labor force participation of men in these cohorts and this age group, whereas we are unable to investigate labor force

participation in our data (as we do not have data on those looking for work that are included in the CPS). However, we can examine the effect of OASI benefits on the probability of having positive earnings as above, or on the log odds ratio of the probability of having positive earnings as below.[29]

To illuminate the reasons for the difference between KP's results and ours, we can assess whether we obtain empirical results similar to those of KP when we run specifications parallel to theirs using our SSA data. Specifically, using our data aggregated to the birth cohort-calendar year level (as KP do), we keep the same sample of ages and years as Krueger and Pischke (1992) and implement the same specifications:

$$E_{ij} = \alpha B_{ij} + \Gamma_k + \Psi_j + \varepsilon_{ij} \qquad (4)$$

where $E_{ij}$ represents the log participation rate in cohort $i$ in year $j$; $B_{ij}$ is log mean lifetime discounted OASI benefits in this cohort and year; $\Gamma_k$ represent age fixed effects; $\Psi_j$ represent year fixed effects; and $\varepsilon_{ij}$ is an error. We alternatively omit or include year dummies, paralleling their specifications. We choose the same sample as Krueger and Pischke, which they call their "Notch" sample: men born 1916 to 1922 (inclusive) observed between ages 60 and 68.

In Appendix Table 5, the first two columns show the estimated effects of log OASI benefits on the log odds ratio of the participation rate (where participation is defined as having positive earnings). We do not find negative and significant coefficients, as in the results of KP. The last two columns of the table show the estimated effect of log OASI benefits on log mean earnings (which we show in order to investigate an outcome closer to the main outcome we examine in our study, *i.e.* earnings). Again, we do not find negative and significant coefficients. We also do not find negative and significant coefficients when the dependent variable is mean earnings, or when the dependent variable is the mean of the IHS of earnings. Moreover, when we use a 0.1 percent sample of the SSA data—much more similar to the sample size in KP—our point estimates are similar to those shown in Appendix Table 5, but the standard errors are several times larger—and we never estimate a significant coefficient on log lifetime OASI benefits.

We conclude that when identifying using cross-cohort variation (as KP do), both KP and we find no evidence of the negative effect of OASI benefits on earnings that we demonstrate

[29] In a different specification, KP estimate the effect of OASI benefits on log hours worked; we cannot estimate the effect on this outcome in our data because we do not observe hours worked.

using day-of-birth data. This suggests that a primary reason for the difference in results is the difference in the level of variation examined (*i.e.* daily vs. yearly). KP show that elderly male labor force participation trended downward across birth cohorts prior to 1917, and such trends may be difficult to disentangle from the true effects of policies like the 1977 Social Security amendments. This factor would affect the analysis of yearly data, but it would not affect our earlier analysis of data by day of birth (assuming that we are able to control adequately for trends in the outcome across day of birth, as appears likely given that our results are generally similar regardless of the controls for DOB that we use).[30]

Compounding this issue, in the cohorts and years considered in KP, the variation across days of birth in the participation rate is large relative to the variation induced by the benefit discontinuity. Our RDD regression results applied to the ages KP investigate (60 to 68) show that the cut in benefits at the cohort discontinuity led to a 1.03 percentage point increase in the participation rate in these years (standard error 0.0025, $p<0.01$), on a mean base of 56.7 percent over these ages. Meanwhile, in the cohorts and years KP consider, the standard deviation of the participation rate at the yearly level is 0.19 in our data, which is large relative to a change in the rate of 0.0103.[31] Despite the fact that this change in the participation rate had large implications for average earnings—as we show using our RDD strategy—the cross-cohort empirical design turns out to be under-powered to uncover this effect on participation and separate it from unrelated variation across cohorts. Moreover, measurement error in the self-reported CPS data used in KP, and the CPS sample size that is approximately 1/2000 as large as ours, could further

[30] While the primary analysis of Snyder and Evans (2006) concerns the effect of the Notch on mortality, as a secondary and brief analysis these authors also examine the effect of the Notch on labor supply in the CPS and find that the Notch increased labor supply, as we do. However, Snyder and Evans use data at the birth cohort level (as opposed to the day of birth data we use), their sample size is less than a 0.1 percent sample of the population (as opposed to the full population data that we use), and as they discuss there is substantial measurement error in the measure of year of birth in the CPS. Snyder and Evans' sample differs from KP's because Snyder and Evans use a different set of birth cohorts and years; thus, the evidence based on yearly time series variation conflicts between KP and Snyder and Evans (depending on which years of data are included), and our evidence is useful in sorting out these conflicting results in previous literature. The Snyder and Evans estimated effect of the Notch on the probability of working is four times as large as ours, again demonstrating the importance of our more granular data. Moreover, Snyder and Evans estimate a very large 5 percentage point discontinuity in the probability of working at ages 68-70, with no economically or statistically significant change in the probability of working at ages 65-67, whereas we have found a slightly larger discontinuity in the employment probability at ages 65-57 than at ages 68-70, again illustrating the importance of the finer-grained data. Snyder and Evans do not estimate the effect on earnings, which is our primary focus, and thus we also contribute precise crowdout estimates and well as many other contributions such as estimates of income and substitution effects.

[31] Conditional on age and year fixed effects, the residual standard deviation is 0.0037. However, with 54 observations in the regression (six cohort over nine ages), the regressions are not apt to pick up an effect of magnitude 0.0088 and separate it from unrelated trends across cohorts, as our empirical analysis shows.

obscure an association between Social Security benefits and labor outcomes. Indeed, our results on the employment rate are well within the 95 percent confidence intervals of KP.

In sum, our view is that KP's classic paper examining the Notch was both an important innovation and was executed as well as allowed by the data available at the time. Our analysis of more newly available data at a more granular (*i.e.* DOB) level has shown patterns that the earlier data turn out to be underpowered to detect—though it was not possible to verify using only the earlier data that they would be underpowered.

**8. Conclusion**

How OASI benefits affect beneficiaries' work activity is a key question for policy and for understanding patterns of work activity among the elderly. We find clear evidence that the effect of OASI benefits is substantial: sharp decreases in OASI benefits at the cohort boundary led to sharp increases in earnings. We know, therefore, that the income effect must be strong and must have outweighed the substitution effect at this discontinuity: average earnings increase substantially at the cohort boundary despite a substitution effect that could in principle have reduced earnings. In fact, we find that there is no evidence of a substitution effect, and we are able to rule out a substitution elasticity greater than 0.066. In a baseline calculation of the income effect, we calculate that a $1,000 increase in discounted lifetime OASI benefits is associated with a decrease in earnings of $754. It is remarkable that during 1982-1987, when individuals in the 1917 cohort are between the ages of 65 and 70, we find nearly one-for-one crowdout of earnings by OASI benefits, as a $1,000 increase in discounted lifetime benefits leads to a $754 decrease in mean yearly earnings over these years.

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. This particular variation is of great interest in understanding the stabilization of the employment-to-population ratio in the mid-1980s, as noted in Figure 1, as our results suggest that the Notch legislation may have been an important factor in explaining this pattern in the data (more on which below). Thus, while our results concern a particular episode, this episode is of substantial interest in explaining important patterns in the data. Moreover, it is worth noting that we estimate similar results in the low- and high-income samples. To understand trends in employment in the latter half of the 20$^{th}$ century, it is relevant to consider the effects on a sample with the lower average incomes that prevailed earlier than 1977; our results by income suggest that the effects could be similar in

samples with lower (or higher) income. Furthermore, we find very similar point estimates at the 1917/1918 cohort boundary, further suggesting the generalizability of our results to other similar contexts.

Under the assumption that the income effect generalizes to the elderly in other time periods, we can calculate how much of the variation in elderly male participation rates from 1950 to 1985 can be accounted for through the increases in OASI benefits over this time period. From 1950 to 1985 the elderly male employment-to-population ratio decreased by 28.4 percentage points.[32] Meanwhile, from 1950 to 1985, the mean real annual male OASI benefit rose by $8,561.04 (Social Security Administration 2013).[33] Assuming that a $772.51 increase in yearly OASI benefits leads to a decline in the elderly male participation rate of 0.88 percentage point, as our empirical results show, we find that the rise in mean Social Security benefits from 1950 to 1985 should have led to an 8.1 percentage point decrease in the participation rate over this period (*ceteris paribus*), equal to 28.5 percent of the actual decline in the participation rate.[34] One limitation of this analysis is that we only have data until 2000, when the 1917 cohort reaches 83, whereas the CPS data on the elderly employment-to-population ratio includes still older individuals. Using SSA life tables we calculate that 17.05 percent of the population over age 65 was also over 83; assuming that in a worst-case scenario the effect on the employment probability is zero for ages over 83, this would imply that the rise in OASI benefits was responsible for a 6.7 percentage point decline in the employment-to-population ratio, or 23.7 percent of the actual decline.[35]

When considering the time series variation, it is also relevant to note that the stabilization in the elderly male employment-to-population ratio shown in Figure 1 occurs in the mid-1980s. This does not match perfectly with the year in which the Notch legislation occurred (1977), but it

[32] These statistics are from the Current Population Survey, which is different than our SSA data. While acknowledging this limitation, our calculation illustrates that much of the variation in elderly male participation could be accounted for through changes in OASI benefits. Our data are only for the cohorts near the Notch cohorts, so we are unable to calculate the fraction with positive earnings in 1950 or other earlier years in our data.
[33] The mean male OASI benefit reported by SSA is higher than the mean benefit in our data because the mean male OASI benefit reported by SSA is greatly influenced by the lower benefits of other birth cohorts.
[34] To perform this calculation, we divide the increase in OASI benefits over this period ($8,561.04) by the absolute value of the discontinuous decrease in mean yearly benefits in our data ($772.51), and multiply by estimated discontinuous increase in the elderly male participation rate (0.88 percentage point).
[35] Women's average OASI benefits also increased over this period. If husbands' and wives' leisure is complementary (substitutable), then this would allow us to explain a greater (smaller) fraction of the change in the male employment-to-population ratio. The literature has typically suggested complementarity in the spousal retirement decision.

matches very well with the years when our RDD strategy shows that the big effect on employment occurs, *i.e.* 1982 to 1987. Thus, the Notch variation caused *future* impacts on elderly employment that match very well with the time period when the stabilization in the elderly male employment-to-population ratio (and labor force participation rate) occurred. While mean OASI benefits continued to increase after the early 1980s, their rate of growth slowed quite substantially, coinciding with the change in slope in Figure 1 of the elderly male employment-to-population ratio that occurs in the mid-1980s. Moreover, the overall elderly employment-to-population ratio is affected by the full elderly population; thus, the effect of a higher employment rate in newly-elderly cohorts on the overall elderly employment-to-population ratio should appear only slowly, as new cohorts enter the elderly sample.

The income effects that we find are quite large. The cut in benefits from the 1916 cohort to subsequent cohorts was well-publicized (GAO 1988), and OASI claimants may have a good sense of their overall income—both in the context of the Notch variation and in other contexts. While this is true in other OASI contexts, our results do not imply that income effects are very large in all contexts. Estimates of income elasticities are often smaller (e.g. Imbens, Rubin, and Sacerdote 2001). Elderly individuals are often on the margin of retirement or partial retirement, which could make them more responsive to exogenous changes in unearned income than other groups; indeed, much previous literature has found large labor supply responses among the elderly in particular.

It is perhaps unsurprising that we do not find evidence of a substitution effect in our particular context. In contrast to the wide publicity surrounding the Notch's cut in OASI benefits from the 1916 cohort to subsequent cohorts, and the ease of understanding the fact that there was a large cut in benefits, the policy that underlies the substitution effect is complex—it requires understanding the linkage between current earnings and future OASI benefits, and how this worked under a new policy (the transitional AMW) that applied to only five birth cohorts and was prevalent among only two (1917 and 1918). Liebman and Luttmer (2011) have documented that many individuals do not understand important Social Security rules. If this is the reason for the absence of a larger substitution effect in our context, then it is possible that in other contexts in which substitution incentives are not well-known or not salient, we also might not see large substitution effects. Nonetheless, we do not have direct evidence on the reasons for the small substitution effect in this context. Our results do not rule out the possibility that substitution

effects are larger in other contexts, perhaps in which substitution incentives are more transparent, including in other Social Security contexts like the Retirement Earnings Test where moderate substitution elasticities are evident (Burtless and Moffitt 1985; Friedberg 2000; Song and Manchester 2007; Gelber, Jones, and Sacks 2013).

Our results are clearly relevant to policy-makers as they contemplate changes to OASI such as changing the Cost of Living Adjustment or the retirement age. The distinction between income and substitution effect is important in part because only substitution effects are associated with distortions in a standard public finance setting (in the absence of a pre-existing distortion). If applicable more broadly, our finding of large income effects would imply that an important part of the effect of pensions would not be associated with large distortions, in contrast to the received wisdom. (Of course, OASI is financed through taxation, which can cause deadweight loss through this separate channel.) Another policy-relevant implication of these results is for the fiscal consequences of changes in OASI benefits. For example, if a $1 increase in lifetime OASI benefits leads to a decrease in lifetime earnings of 43 cents, this would have implications for the fiscal implications of an increase in OASI benefits. Assuming for the sake of argument that earnings are taxed at a 25 percent marginal rate on average in this population (including both income and FICA taxes), a $1 increase in OASI benefits would cost $1.11, net of the resulting decrease in revenue. The OASI Trust Fund alone would lose 4.6 cents.

How OASI affects consumption, savings, or assets is a very important complementary question. Data are not available to provide an illuminating analysis of these issues. The available data, on consumption from the Consumer Expenditure Survey, or on savings or assets from the Survey of Consumer Finances or the Health and Retirement Study, have much smaller sample sizes than those we are able to use in the full population data—at least two orders of magnitude smaller. This extra sample size is crucial. For example, when we limit the sample size in our data to a one percent sample of the population, we no longer find a significant effect even on earnings—even though this effect is very clear when we use the 100 percent sample of the male population. Exploratory work on datasets with information on consumption, savings, or assets has shown that indeed, measures of these outcomes are unsurprisingly very noisy around the cohort boundary, and power calculations show that it would not be possible to estimate significant effects even with large effect sizes. Thus, we are underpowered to investigate the effect on these outcomes.

Another limitation is that our design does not allow us to investigate general equilibrium effects of the policy change, for example on the level of wages. As with other macroeconomic variation, such changes would be expected to affect individuals similarly on both sides of the discontinuity.

A final issue is that the magnitude of the discontinuity depended on the magnitude of inflation. Federal Reserve data show that inflation expectations were far below realized inflation in 1978 and 1979, but expectation generally closely matched realized inflation in all other years (Mehra and Reilly 2008). However, it is possible that the unanticipated inflation of the late 1970s and early 1980s led to a larger realized discontinuity in benefits than was anticipated in 1977. If so, then we might expected the measured discontinuity in these early years to *understate* the discontinuity that would have occurred if the full magnitude of the discontinuity in benefits were known throughout. In this sense, our estimates of the income effects could be considered lower bounds on the effect that would have occurred if the realized discontinuity in benefits were known with certainty in 1977—thus reinforcing our conclusion that the income effects are very large.

Future research could fruitfully the effect of OASI benefits on women's earnings. Understanding how OASI benefit changes affect joint family earnings decisions, using appropriate data on linked spouses that we do not have, would also be valuable.

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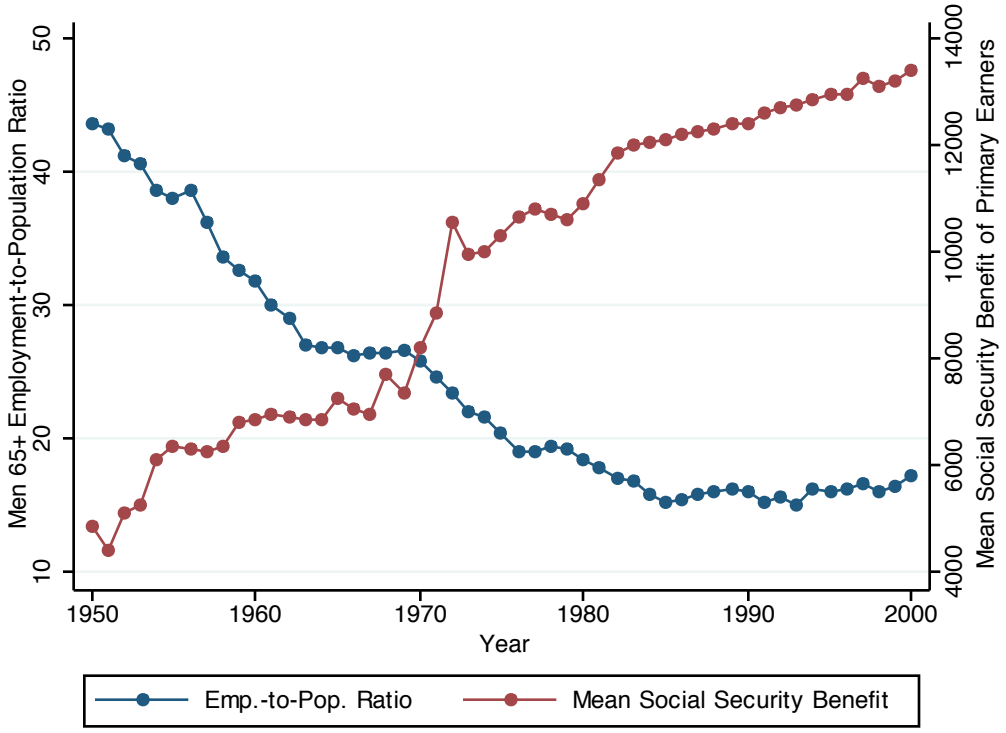
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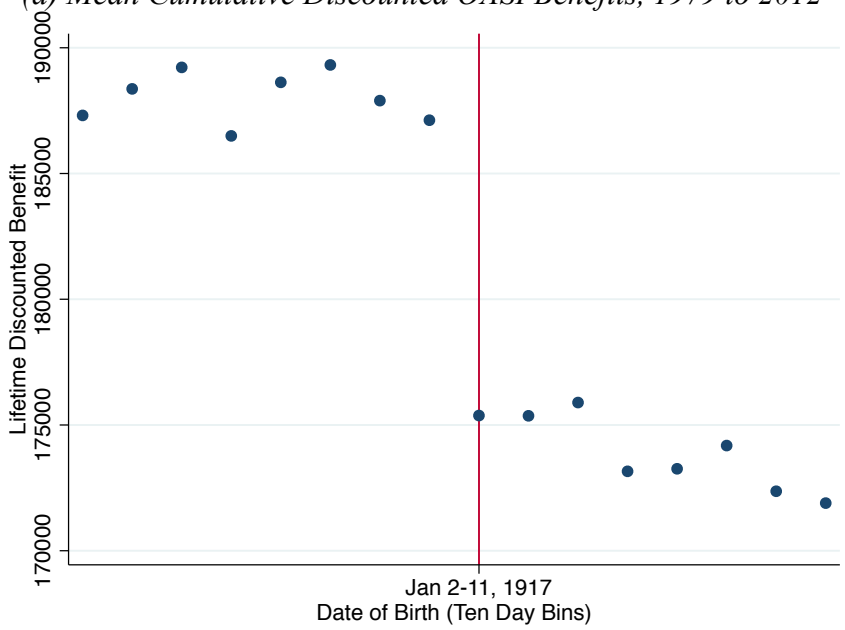
**Figure 1.** *Mean Social Security Benefit of Primary Earners and Elderly Male Employment-to-Population Ratio*



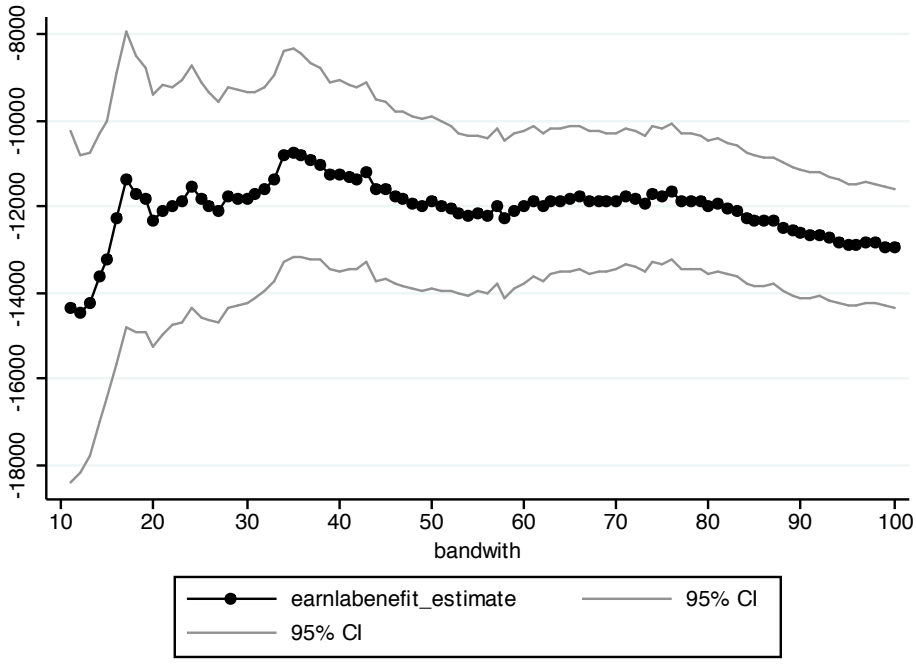

Notes: the figure shows the employment-to-population ratio of men 65 and over, as well as the mean OASI benefit, by year from 1950 to 2000. The data on the employment-to-population ratio come from the Current Population Survey. The data on mean OASI benefit of male primary earners come from the Social Security Administration Annual Statistical Supplement (Social Security Administration 2012a).

**Figure 2.**

*(a) Mean Cumulative Discounted OASI Benefits, 1979 to 2012*



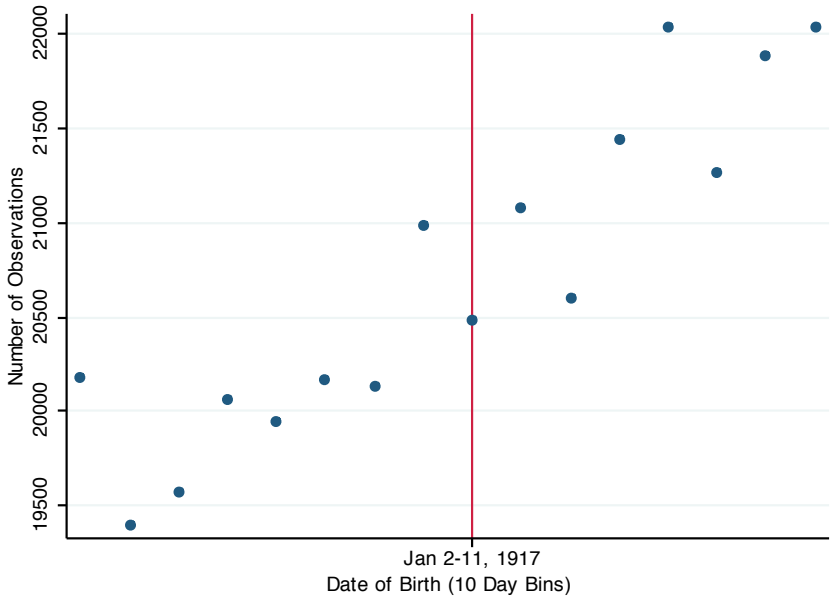

*(b) Coefficients and confidence intervals by bandwidth*



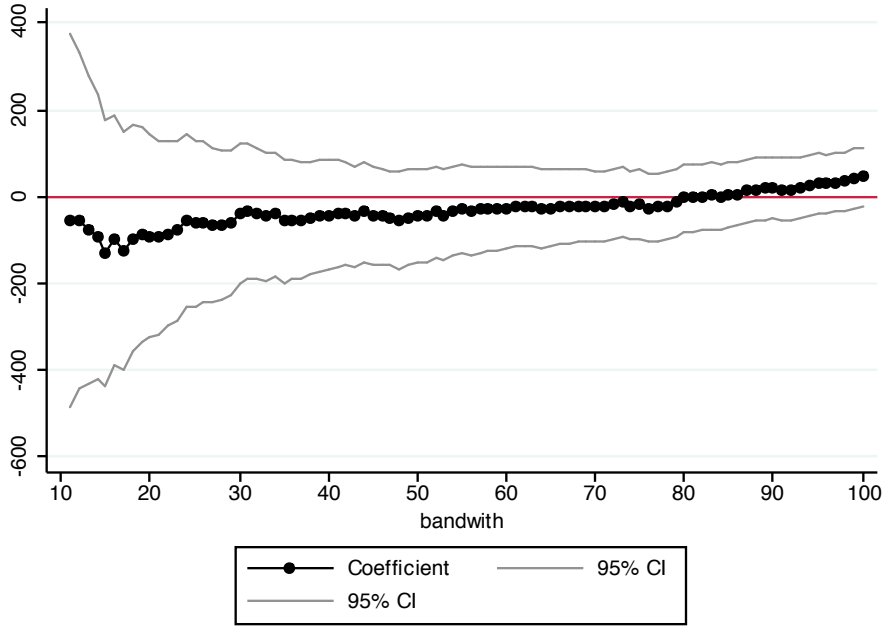

Note: Panel A shows mean lifetime discounted OASI benefits from 1978 to 2012, in 10-day bins around the discontinuity separating the 1916 birth cohort from the 1917 birth cohort. Panel B shows the coefficients and 95 percent confidence intervals on a dummy for being born on or after January 2, 1917 from model (1), as a function of the bandwidth (in days) chosen around this cutoff, for all bandwidths from 10 to 100. The data are a 100% sample of men from the Social Security Administration Master Earnings File and Master Beneficiary Record. 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.

**Figure 3.**

*(a) Number of observations, by DOB Bin*



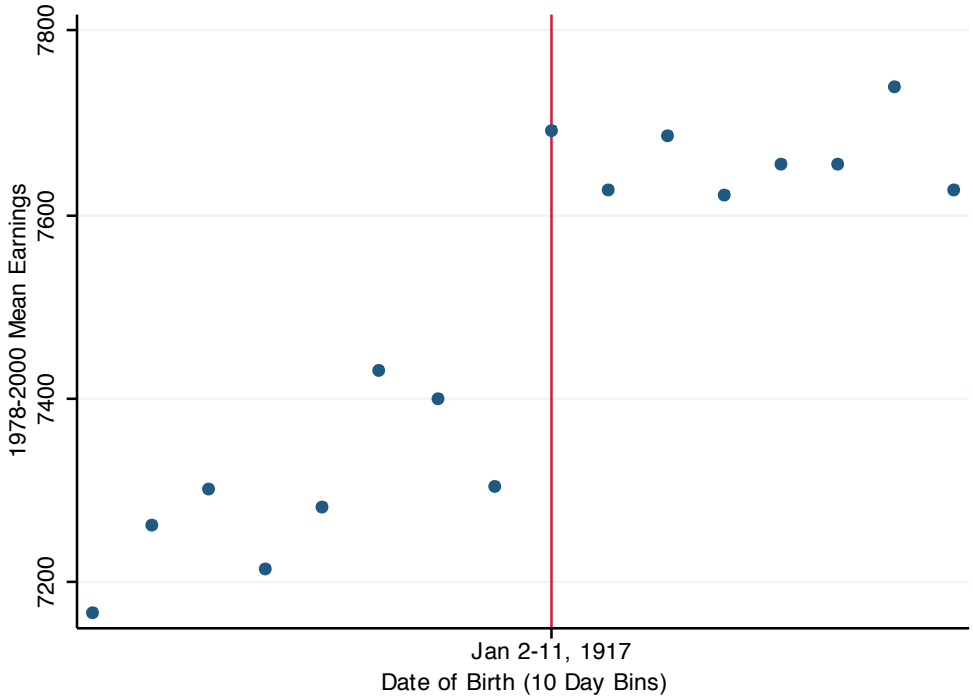

*(b) Coefficients and confidence intervals by bandwidth*



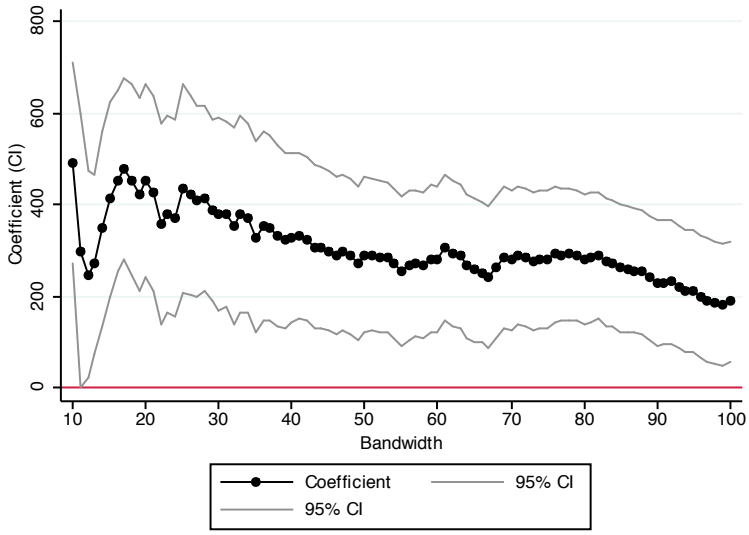

Note: Panel A shows the number of observations in 10-day bins around the discontinuity separating the 1916 birth cohort from the 1917 birth cohort. Panel B shows the coefficients and 95 percent confidence intervals on a dummy for being born on or after January 2, 1917 from model (1), as a function of the bandwidth (in days) chosen around this cutoff.

**Figure 4.**

*(a) Mean male real yearly earnings by DOB, 1978 to 2000 (1917 cohort ages 61 to 83)*



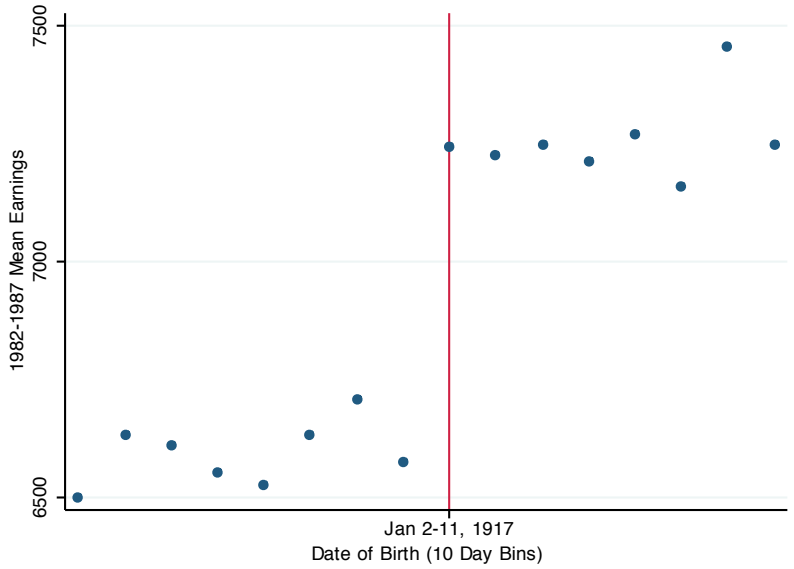

*(b) Coefficients and confidence intervals by bandwidth*



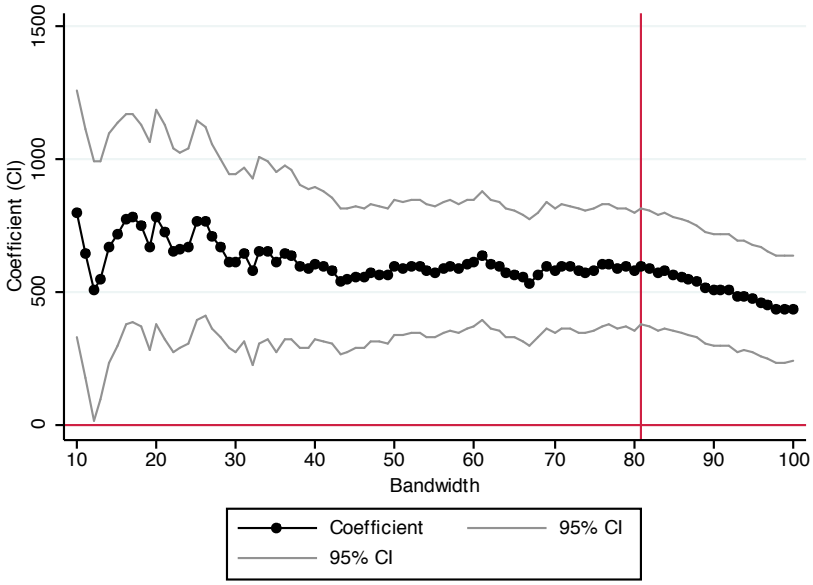

Note: Panel A shows mean yearly earnings from 1978 to 2000, in 10-day bins around the discontinuity separating the 1916 birth cohort from the 1917 birth cohort. Panel B shows the coefficients and 95 percent confidence intervals on a dummy for being born on or after January 2, 1917 from model (1), as a function of the bandwidth (in days) chosen around this cutoff.

**Figure 5.**

*(a) Mean male real yearly earnings by DOB, 1982 to 1987 (1917 cohort ages 65 to 70)*



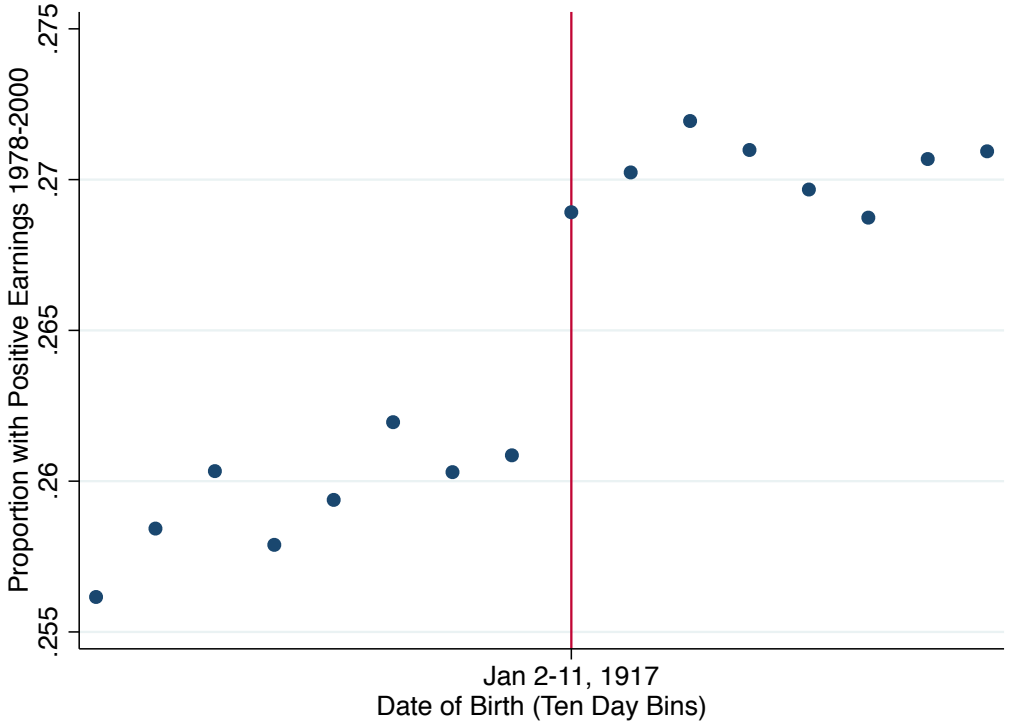

*(b) Coefficients and confidence intervals by bandwidth*



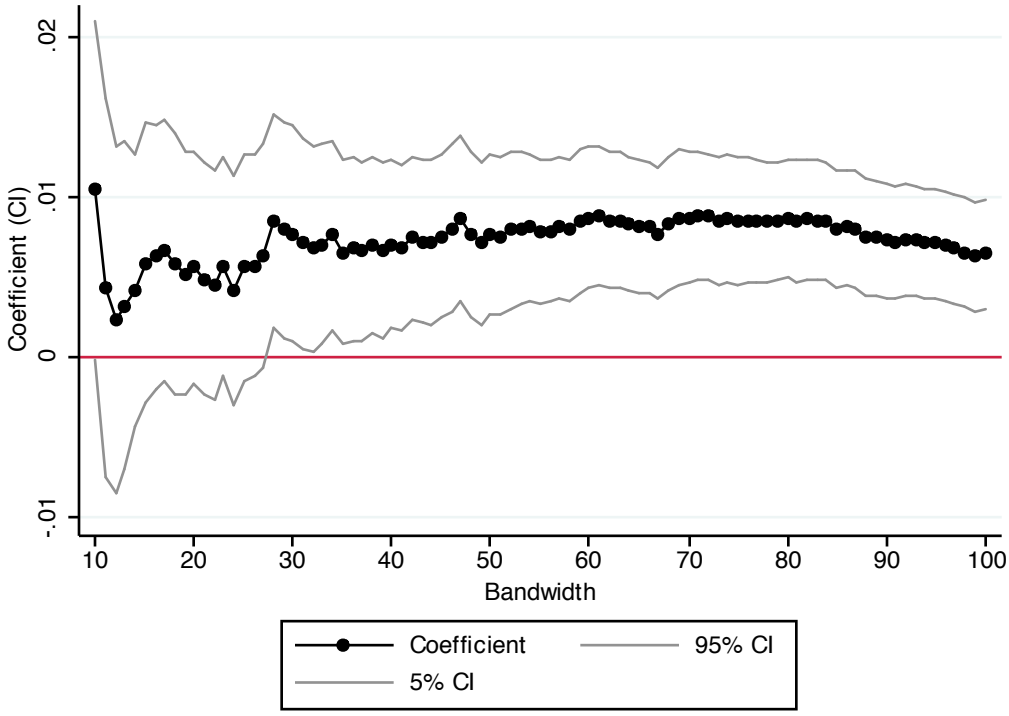

See notes to Figure 4.

**Figure 6.**

*(a) Extensive margin: mean of yearly male positive earnings dummy, 1978 to 2000 (1917 cohort ages 61 to 83)*



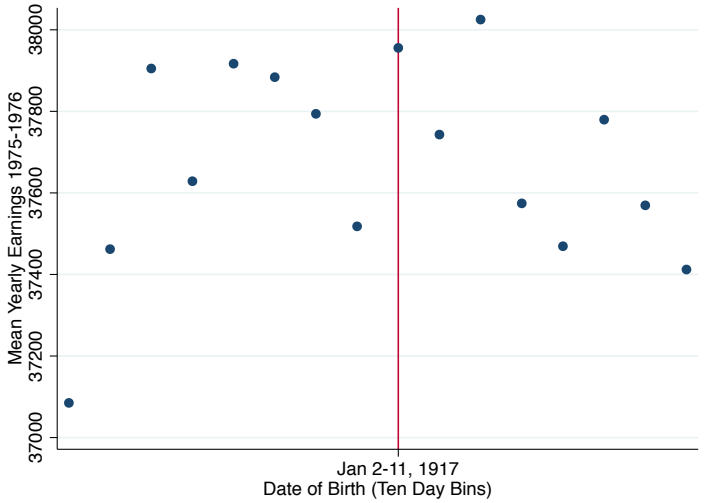

*(b) Coefficients and confidence intervals by bandwidth*



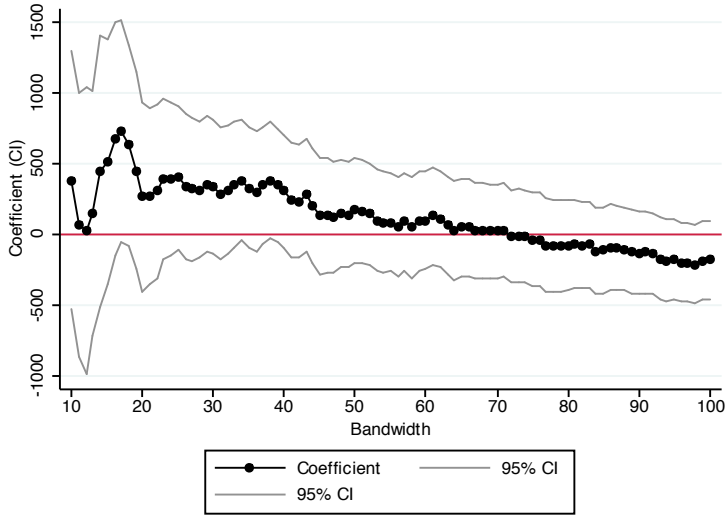

See notes to Figure 4.

**Figure 7.**

*(a) Mean male yearly earnings by DOB, 1975 to 1976 (prior to 1977 amendments)*



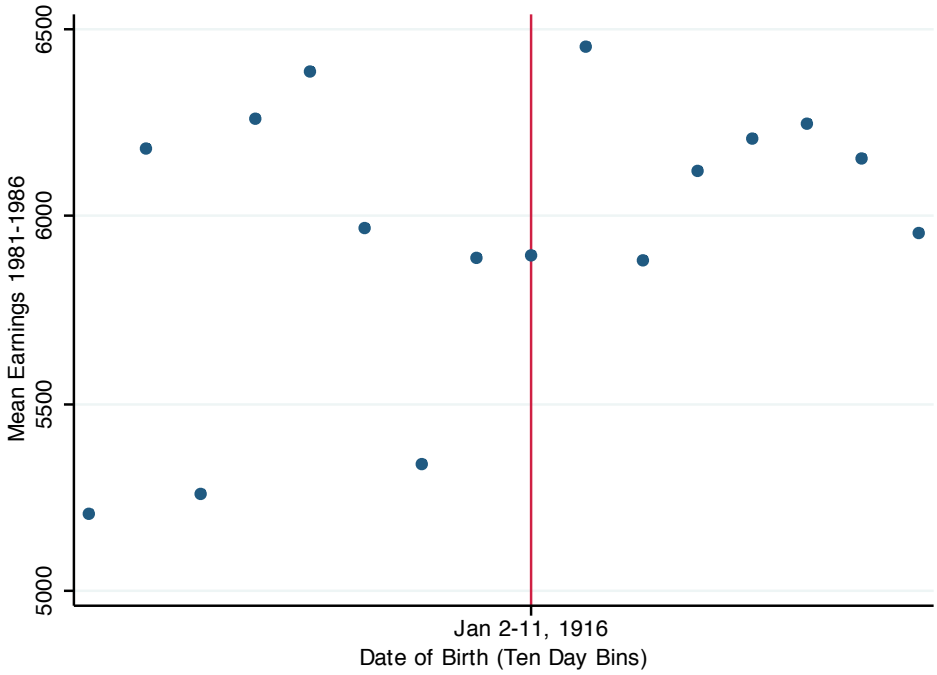

*(b) Coefficients and confidence intervals by bandwidth*



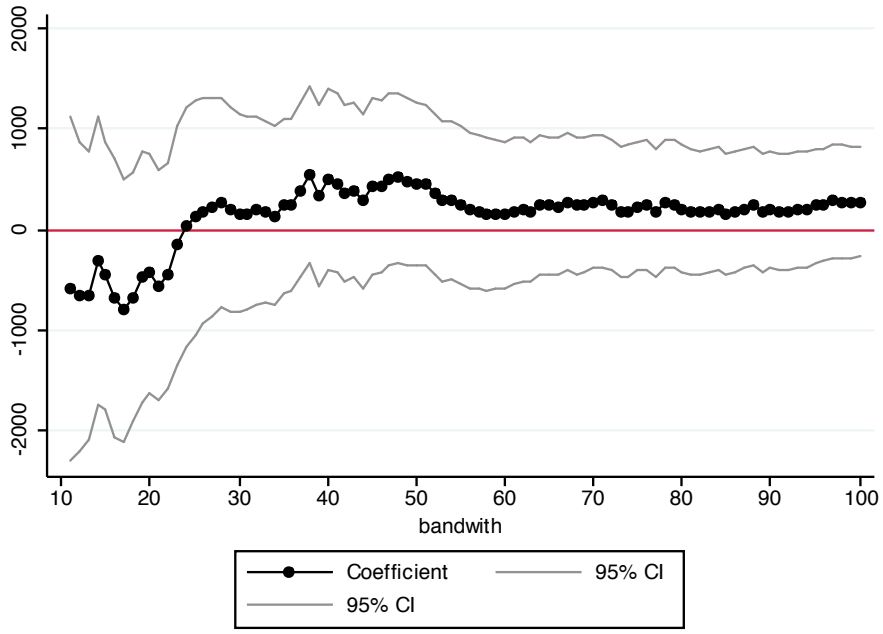

See notes to Figure 4.

**Figure 8.**

*(a) Mean yearly earnings, 1981-1986, 1915/1916 cohort boundary*



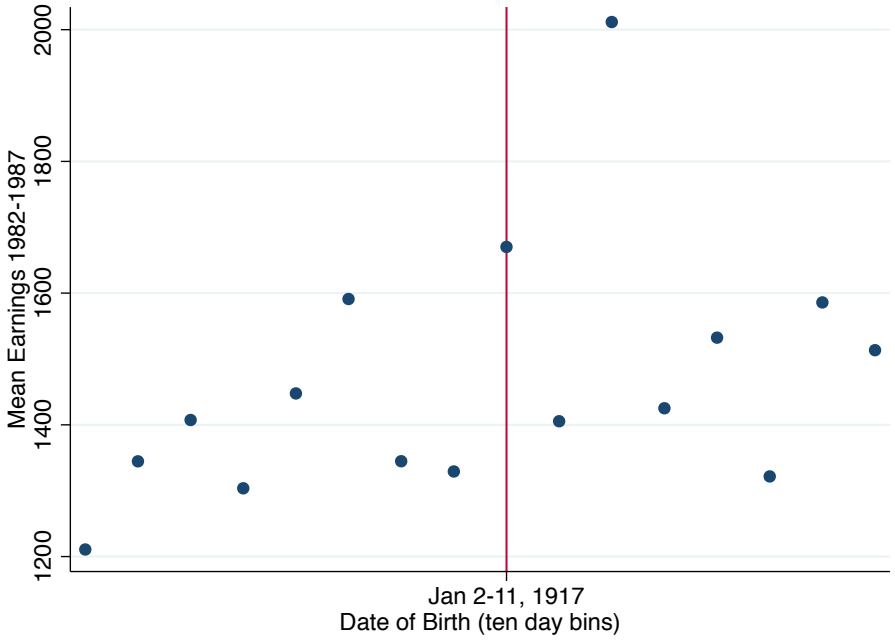

*(b) Coefficients and confidence intervals by bandwidth*



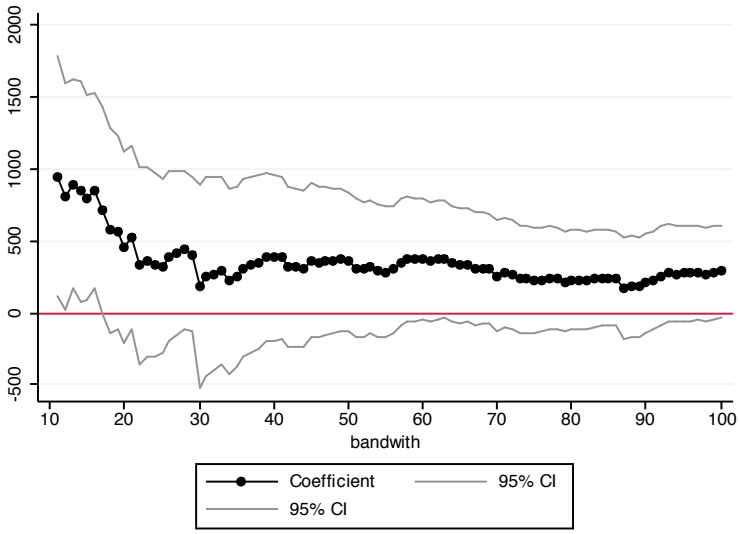

See notes to Figure 4.

**Figure 9.**

*(a) Mean earnings of men ineligible for OASI, 1982 to 1987, by DOB bin*



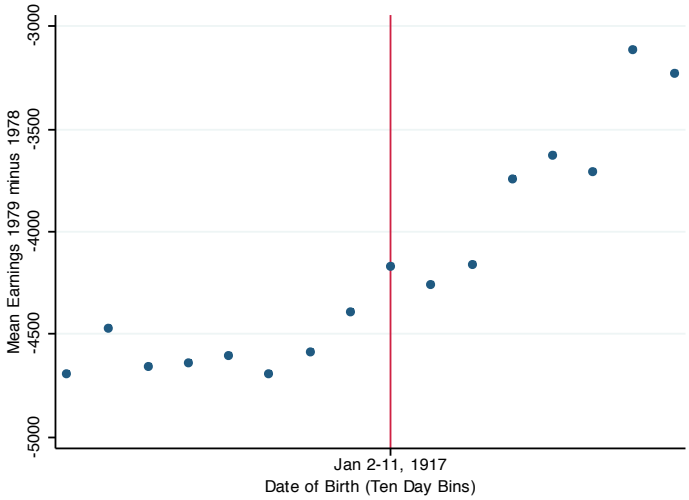

*(b) Coefficients and confidence intervals by bandwidth*



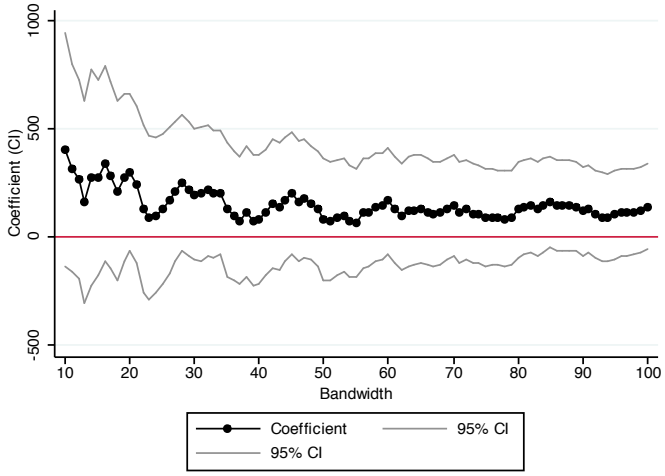

Note: See notes to Figure 4. Mean earnings are low for this group, which is not surprising because many of these individuals did not accumulate sufficient work history to qualify for OASI.

**Figure 10.**

*(a) Mean yearly earnings, 1979 minus 1978*




*b) Coefficients and confidence intervals by bandwidth, 1979 minus 1978*




See notes to Figure 4.

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

| Variable | Mean (SD) |
|---|---|
| Earnings, 1975 to 2000 | $11,267.42 |
| | (352.18) |
| Earnings, 1975 to 1976 | $37,681.47 |
| | (588.08) |
| Earnings, 1978 to 2000 | $7,881.55 |
| | (355.26) |
| Earnings, 1982 to 1987 | $7,012.88 |
| | (549.24) |
| Earnings, 1988 to 2000 | $2,217.45 |
| | (241.01) |
| Earnings, 1978 to 1981 | $27,592.85 |
| | (816.73) |
| Positive earnings dummy, 1982 to 1987 | 0.29 |
| | (.013) |
| Positive earnings dummy, 1978 to 2000 | 0.26 |
| | (0.0091) |
| Discounted OASI benefits, 1979 to 2012 | $181,126 |
| | (75,503.75) |
| Last calendar year earned positive amount | 1982.53 |
| | (0.26) |
| Foreign born dummy | 0.036 |
| | (0.010) |
| Missing birthplace dummy | .014 |
| | (0.0059) |
| Male dummy | 0.52 |
| | (0.0083) |
| White dummy | 0.91 |
| | (0.014) |
| Age of initial OASI claim | 63.57 |
| | (0.045) |
| Self-employment earnings, 1978 to 2000 | $1,776.86 |
| | (132.96) |
| Number of individuals per day of birth | 2,072.63 |
| | (138.46) |

Notes: The source is SSA administrative data on the universe of U.S. FICA earnings and other outcomes, from the Master Earnings File and Master Beneficiary Record. The table shows means and standard deviations of the main variables in our sample. We report the means and standard deviations of the variables that we use for our regression analysis, *i.e.* the mean and standard deviation of mean earnings by DOB, rather than reporting the mean and standard deviation in the raw (*i.e.* un-averaged) SSA data. The sample is men born in 1916 and 1917 within 100 days of January 2, 1917. After an individual dies, their earnings and benefits are counted as missing prior to averaging by DOB; thus, these individuals' earnings are not counted in the averages shown above. Since we compute means by DOB, the means and standard deviations shown above are based on 200 observations in every case. 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 appear in 1978. This corresponds to 414,525 individuals within 100 days of the cohort boundary, or 10,777,650 individual-year observations from 1975 to 2000 (inclusive).

**Table 2.** *Effect of Notch on earnings outcomes: OLS regressions of earnings outcomes on dummy for being affected by the Notch and control variables*

| (1) Outcome | (2) Linear | (3) Linear | (4) Quadratic | (5) Quadratic | (6) Cubic | (7) Cubic |
|---|---|---|---|---|---|---|
| A) Mean earnings | 299.07 | 279.36 | 307.63 | 289.80 | 457.34 | 414.44 |
| 1978 to 2000 | (76.99)*** | (79.45)*** | (101.51)*** | (105.93)*** | (120.15)*** | (125.53)*** |
| B) Disc. earnings | 5,128.49 | 4,682.75 | 5,722.22 | 5,361.31 | 7,652.45 | 6,916.35 |
| 1978 to 2000 | (1,216.49)*** | (1,245.27)*** | (1,585.93)*** | (1,616.46)*** | (1,884.81)*** | (1,936.80)*** |
| C) Earnings 1982 | 613.39 | 606.20 | 584.80 | 573.43 | 725.63 | 694.19 |
| to 1987 | (117.56)*** | (121.79)*** | (155.90)*** | (161.69)*** | (201.06)*** | (202.63)*** |
| Controls? | N | Y | N | Y | N | Y |

Notes: The table shows the results of OLS regressions corresponding to model (1) in the text. The dependent variable is mean yearly earnings at the day-of-birth level in the years shown. In the "linear" specifications, the independent variables are a dummy for being born after January 2, 1917, a linear trend in day of birth (DOB), and the interaction of this linear trend with the dummy for being born after January 2, 1917. The "controls" columns show the regressions with controls for percent white, percent foreign born, percent missing birthplace, and percent male by DOB. We also show the regressions separately for linear, quadratic, and cubic controls, where the latter two specifications include quadratic and cubic controls as well as interactions of these controls with the dummy for being in the 1917 cohort. In this table we show the results for the bandwidth chosen using cross-validation for earnings in 1978 to 2000 (*i.e.* for bandwidth of 76), in order to hold the sample constant across regressions; in Appendix Table 2 we show the results when the bandwidth is chosen separately for each outcome. In all cases, the specification that minimizes the Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC) is the linear specification without controls. *** refers to significance at the 1% level; ** at the 5% level, and * at the 10% level.

**Table 3.** *Effect of Notch on earnings outcomes: OLS regressions of participation outcomes on dummy for being affected by the Notch and control variables*

| (1) Outcome | (2) Linear | (3) Linear | (4) Quadratic | (5) Quadratic | (6) Cubic | (7) Cubic |
|---|---|---|---|---|---|---|
| A) Positive earnings | 0.0088 | 0.0085 | 0.0083 | 0.0075 | 0.0072 | 0.0064 |
| dummy, 1978 to 2000 | (0.0019)*** | (0.0019)*** | (0.0027)*** | (0.0028)*** | (0.0033)** | (0.0035)* |
| B) Positive earnings | 0.013 | 0.013 | 0.014 | 0.013 | 0.011 | 0.011 |
| dummy, 1982 to 1987 | (0.0030)*** | (0.0031)*** | (0.0044)*** | (0.0044)*** | (0.0057)* | (0.0057)* |
| C) Last year earned | 0.23 | 0.20 | 0.24 | 0.20 | 0.23 | 0.19 |
| positive amount | (0.059)*** | (0.061)*** | (0.089)*** | (0.09)** | (0.12)* | (0.12) |
| Controls? | N | Y | N | Y | N | Y |

Notes: The table shows the results of OLS regressions corresponding to model (1) in the text. See notes to Tables 1 and 2.

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

| Outcome | (1) Linear | (2) Linear |
|---|---|---|
| A) Mean earnings 1975 to 1976 | 144.81 (206.19) | -80.12 (174.13) |
| B) Mean earnings 1981 to 1986, 1915/1916 cohort boundary | 172.74 (312.28) | 155.79 (328.53) |
| C) Mean earnings 1977 to 1999, 1915/1916 cohort boundary | 2.78 (222.32) | -30.41 (233.02) |
| D) Mean earnings 1982 to 1987 among those ineligible for OASI | 186.14 (185.35) | 197.66 (193.40) |
| E) Mean earnings 1978 to 2000 among those ineligible for OASI | 96.03 (72.63) | 101.70 (75.98) |
| Controls? | N | Y |

Notes: The table shows the results of OLS regressions corresponding to model (1) in the text. We demonstrate that in three placebo samples not affected by the discontintinuity in OASI benefits, there is no discontinuous change in earnings at placebo cohort boundaries. In row A, the dependent variable is mean earnings in 1975-1976, prior to the 1977 Social Security amendments that changed benefits. In rows B and C, we examine those ineligible for OASI. In rows D and E, we examine earnings around the 1915/1916 cohort boundary. We use the linear specification from Table 2. See other notes to Table 2.

**Table 5.** *Effect of Notch on earnings outcomes: OLS regressions of labor outcomes on dummy for being affected by the Notch and control variables, over other time periods or specifications*

| Outcome | (1) Linear | (2) Linear |
|---|---|---|
| A) Earnings 1979 minus 1978 (levels) | 72.54 (123.03) | 106.89 (123.99) |
| B) Earnings 1979 minus 1978 (IHS) | 0.0039 (0.0040) | 0.0043 (0.0041) |
| Controls? | N | Y |

Note: IHS refers to the inverse hyperbolic sine of earnings, as described in the text. In row A, the dependent variable is the mean of earnings in 1979 minus the mean of earnings in 1978. In row B, the dependent variable is the mean of the inverse hyperbolic sine of earnings in 1979 minus the mean of the inverse hyperbolic sine of earnings in 1978. See other notes to Table 2.

**Table 6.** *OLS regressions of OASI benefits on dummy for being affected by the Notch*

| Outcome | (1) Discounted OASI benefits, 1978-2012 | (2) Discounted OASI benefits, 1978-2012 | (3) Discounted scheduled OASI benefits, 1978-2012 | (4) Discounted scheduled OASI benefits, 1978-2012 |
|---|---|---|---|---|
| Coefficient (SE) on Jan. 2, 1917 dummy | -11,858.95 (811.60)*** | -12,597.51 (795.55)*** | -12,546.62 (780.58)*** | -13,269.85 (750.43)*** |
| Controls? | N | Y | N | Y |

Notes: The table shows the results of OLS regressions corresponding to model (2) in the text, showing the effect of the Notch dummy on measures of OASI benefits. We use the same sample and specification considered in Table 2 but instead with the dependent variable shown in the column heading. Columns 1 and 2 show the results when the dependent variable is realized discounted OASI benefits, and columns 3 and 4 show the results when the dependent variable is scheduled discounted OASI benefits, as described in the text. We use the linear specification from Table 2. See other notes to Table 2. *** refers to significance at the 1% level; ** at the 5% level, and * at the 10% level.

**Table 7.** *Effect of discounted lifetime benefits on discounted lifetime earnings*

| Outcome | (1) Discounted OASI benefits, 1978-2000 | (2) Discounted OASI benefits, 1978-2000 | (3) Discounted scheduled OASI benefits, 1978-2000 | (4) Discounted scheduled OASI benefits, 1978-2000 |
|---|---|---|---|---|
| A) 2SLS | -0.43 (0.12)*** | -0.37 (0.11)*** | -0.41 (0.11)*** | -0.35 (0.10)*** |
| B) Implied income effect, assuming substitution elasticity of 0.066 | -0.45 (0.12)*** | -0.38 (0.11)*** | -0.42 (0.11)*** | -0.36 (0.10)*** |

Notes: The table shows the results of two-stage least squares regressions corresponding to regressions (2) and (1) in the text, showing the effect of discounted lifetime OASI benefits on discounted lifetime earnings. The excluded instrument is the dummy for being in the 1916 cohort. Columns 1 and 2 show the results when the endogenous variable is realized discounted OASI benefits, and columns 3 and 4 show the results when the endogenous variable is scheduled discounted OASI benefits, as described in the text. In row A, the dependent variable is discounted lifetime earnings. In row B, the dependent variable is the measure of discounted "transformed earnings" $\tilde{E}_{it}$, as described in the text, which accounts for a substitution effect of the maximum size allowed by our 95 percent confidence interval. Thus, row A shows income effects when we assume that substitution effects are zero (*i.e.* a lower bound on the absolute value of the income effect), and row B shows income effects when we assume that the substitution effect is at its maximum possible level (*i.e.* an upper bound on the absolute value of the income effect) We use the linear specification from Table 2. See other notes to Table 2. *** refers to significance at the 1% level; ** at the 5% level, and * at the 10% level.

**Table 8.** *Heterogeneity analysis: OLS regressions of outcomes on dummy for being born after Jan. 2, 1917, and control variables*

| Dependent variable | (1) Low lifetime earnings | (2) High lifetime earnings |
|---|---|---|
| A) Mean earnings 1978 to 2000 | 175.21 (63.57)*** | 363.21 (100.41)*** |
| B) Discounted lifetime benefits | -8,129.60 (995.73)*** | -15,492.33 (919.48)*** |
| C) Implied income effect (2SLS) | -0.35 (0.13)*** | -0.45 (0.16)*** |
| Controls? | N | N |

Notes: The table shows the results of OLS regressions corresponding to model (1) in the text. The dependent variable is mean earnings in the group shown in the column heading. The group considered in column (1) is those with mean earnings in years prior to 1977 that are below the median. The group considered in column (2) is those with mean earnings in years prior to 1977 that are above the median. We use the linear specification from Table 2. See other notes to Table 2. *** refers to significance at the 1% level; ** at the 5% level, and * at the 10% level.

## Appendix 1. Policy environment at other Notch cohort boundaries

The 1977 Social Security amendments affected OASI benefits discontinuously for the 1918 cohort relative to the 1917 cohort. Many individuals in the 1918 cohort were covered under the transitional guarantee. The transitional guarantee was lower in the 1918 cohort than in the 1917 cohort, because after December 1978, no inflation adjustments are made to benefits until the calendar year an individual reaches age 62. Thus, those born in the 1918 cohort benefited from one less year of inflation adjustments than those born in the 1917 cohort. This implied that individuals born in 1918 who were covered by the transitional guarantee had lower benefits than if they had been born in 1917 but otherwise had identical characteristics.

This also implied that many individuals in the 1918 cohort were covered under the AIME formula rather than the transitional guarantee, because benefits calculated through the AIME formula exceeded those under the transitional guarantee. Only a modest fraction of the 1918 cohort was covered by the transitional guarantee. Others in this cohort were covered by the new AIME formula.

This created both income and substitution effects at the 1917/1918 cohort boundary. Those in the 1918 cohort received discontinuously lower benefits on average, because they lost a year of inflation adjustments under the transitional guarantee (and were paid the maximum of the benefits under the transitional guarantee and the AIME formula). Under the presumption that leisure is a normal good, this should have led to an increase in earnings in the 1918 cohort relative to the 1917 cohort.

Those in the 1918 cohort who were covered by the AIME formula experienced a positive substitution effect—greater incentives to earn more—relative to similar individuals in the 1917 cohort who were covered by the transitional guarantee: under the transitional guarantee, earnings after age 61 are irrelevant to the PIA, but under the AIME formula, the PIA weakly increases in an individual's earnings after 61. In a compensated sense, this substitution effect would be expected to increase earnings. Thus, on average relative to the 1917 cohort, the 1918 cohort experienced an income effect that should have caused them to earn more and a substitution effect that caused them to earn more. Thus, at the 1917/1918 boundary, the income and substitution effects work in the same direction—both leading to greater earnings in the younger cohort—beginning in 1980 when the 1918 cohort turns 62. Note that our empirical results show very similar point estimates of earnings crowdout at the 1917/1918 cohort boundary as at the 1916/1917 cohort boundary, again consistent with the absence of meaningful substitution effect.

At the 1917/1918 discontinuity, the size of the income effect depends on inflation. Specifically, the 1918 cohort did not receive the benefit of an extra year of inflation adjustment that the 1917 cohort received. The size of this inflation adjustment was known by 1980, the time that these individuals reached age 62.

By the 1919 birth cohort, essentially everyone was covered by the new AIME formula (GAO 1988). In principle, the fact that inflation adjustments are delayed until the calendar year an individual turns 62 under the transitional AMW formula could have created a discontinuity in benefits at other cohort boundaries like the 1918/1919 cohort boundary—because the inflation

adjustment was delayed an extra year for the 1919 cohort relative to the 1918 cohort under the transitional AMW formula. However, essentially all of the 1919 cohort is covered by the AIME formula, and in practice a sufficient fraction of the population in the 1918 cohort is covered by the AIME formula (rather than the transitional AMW formula) that the delay in inflation adjustments under the transitional AMW formula is largely irrelevant to benefits at the 1918/1919 cohort boundary (and subsequent cohort boundaries). Empirically, there is no economically or statistically significant discontinuity in benefits between the 1918 and 1919 cohorts. As a result of these factors, our empirical work focuses on the 1916/1917 cohort boundary, while also examining the 1917/1918 cohort boundary as a secondary analysis. The transitional AMW formula was effectively irrelevant at these later cohort boundaries because realized inflation was sufficiently high that the AIME formula essentially always yielded higher benefits than the transitional AMW for individuals in these later cohorts. Shortly after the 1977 amendments, individuals around the 1918/1919 boundary and subsequent cohort boundaries could have expected discontinuities in benefits at the cohort boundaries—before the realization of high inflation in these years—so these later cohort boundaries will not allow suitable "placebo" tests.

**Appendix 2: Illustrative framework for analyzing responses**

To ground the empirical analysis in a framework, in this Appendix we review a simple lifecycle framework for understanding the effects of the policy change. Our illustrative framework allows us to clarify conditions under which our estimates correspond to theoretically-grounded parameters. Our framework is adapted from Blundell and MaCurdy (1999) to our particular context in which we observe pre-tax-and-transfer earnings as the key outcome of interest. Thus, rather than modeling the tradeoff between consumption and hours worked as in Blundell and MaCurdy, we model the tradeoff between consumption and pre-tax earnings. In other words, following Saez (2010) and other papers that have access to administrative data on earnings but not on hours worked, we model individuals as trading off consumption, in which utility is increasing, against pre-tax earnings, in which utility is decreasing because it requires effort to produce earnings.

Assume, then, that each individual has a quasi-concave utility function that is separable across time from periods $t$ to $\tau$: $U_t = U(U^t(C_t, \mathrm{E}_t, X_t), U^{t+1}(C_{t+1}, E_{t+1}, X_{t+1}) ... U^{\tau}(C_{\tau}, E_{\tau}, X_{\tau}))$, where $C_t$ and $E_t$ are respectively consumption, pre-tax and benefit earnings, and individual attributes in period $t$. Utility is maximized subject to the intertemporal budget constraint:

$$A_{t+1} = (1 + r_{t+1})(A_t + B_t + T_t + E_t(1 - \mu_t - \tau_t) - C_t) \quad (5)$$

where $A_t$ represents assets in period $t$, $B_t$ is OASI benefit income, $T_t$ is other non-asset unearned income (where $T_t$ has been appropriately adjusted so that virtual income is correctly specified), $C_t$ is consumption, $E_t$ is pre-tax-and-transfer earnings, $\tau_t$ represents the net marginal tax rate *absent* the effects of OASI benefits (so including the effects of FICA and personal income taxes, for example),[36] $\mu_t$ represents the net marginal tax rate associated with OASI benefits (which is negative when OASI benefits are increasing in an individual's earnings in year $t$, as is typically the case, for example, in the 1916 cohort), and $r_{t+1}$ is the interest rate.

[36] $\tau_t$ is subtracted from $\mu_t$, rather than entering multiplicatively, since OASI benefits are untaxed before 1984 and are rarely subject to taxation in our elderly sample after 1984.

Standard dynamic programming techniques yield the following first-order conditions:

$$U_C(C_t, E_t) = \lambda_t$$
$$U_E(C_t, E_t) \geq \lambda_t(1-\tau)_t$$
$$\lambda_t = \kappa(1+r_{t+1})\lambda_{t+1}$$

Here $\lambda_t$ represents the marginal utility of lifetime wealth in period *t*. Under the assumption that the discount factor $\kappa$ is equal to $(1+r_{t+1})$, then $\lambda_t = \lambda_{t+1}$ for all *t*, and we have a constant $\lambda$.[37]

In our empirical context, it is possible to distinguish four sets of years:

1. Calendar years before the 1977 reform is anticipated. Empirically, we examine 1975 and 1976 as years before the reform was anticipated, when the discontinuous effects of the reform on the 1916 and 1917 cohorts (that forms the basis of our identification strategy) was not anticipated. We examine 1975 and 1976, rather than earlier years, because these years are more closely comparable to those after the Notch reform, but we have found extremely similar patterns in other years before 1977.

2. Calendar year 1978, which occurs after the 1977 reform is (anticipated and) passed but before individuals in the 1917 cohort reach age 62 in 1979. According to the framework above, earnings should adjust to the discontinuity in OASI benefits in calendar year 1978 because of the change in the marginal utility of lifetime wealth $\lambda$ resulting from the reduction in expected OASI benefits due to the 1977 amendments—in other words, the change in lifetime income associated with the 1977 amendments.

3. Calendar year 1979, when the substitution effect should begin to operate for those born in 1917.

If individuals are liquidity constrained or myopic, then it is possible that OASI benefit payments have effects on contemporaneous earnings that operate over and above the effect that operates through the channel of lifetime income (*e.g.* Blundell and MaCurdy 1999; Card, Chetty, and Weber 2007; Chetty 2008). In this case, it is also possible to distinguish a fourth set of years:

4. Calendar year 1981 and subsequent years, when the substitution effect is operative and contemporaneous OASI benefits paid discontinuously fall at the cohort boundary for the first time. In the lifecycle framework in the Appendix, calendar years 1981 and after should be conceptually indistinguishable from prior years. However, if individuals are myopic or face liquidity constraints, then the introduction of a downward discontinuity in OASI payments in 1981 would have an effect on earnings that does not occur through the channel of lifetime earnings.

It will be possible to make progress in estimation if we assume that some factors are approximately constant from one calendar year to an adjacent calendar year. It is reasonable to

[37] This framework shows the results for Frisch earnings supply under certainty. The only difference between the cases with certainty and uncertainty is that the marginal utility of lifetime wealth is a random variable that is realized only at the start of each period. In our context, a common time effect should be removed by the RDD setup comparing similar individuals in each year.

assume that in adjacent calendar years the utility function is approximately equal for a given value of the arguments: $U^t(C, E, X) \approx U^{t+1}(C, E, X)$. We can also assume that in adjacent calendar years, all of the variables that appear in the budget constraint (5) other than $B_t$, $C_t$, $E_t$, and $\mu_t$ are approximately equal in adjacent calendar years, so that the difference between earnings in 1978 and 1979, for example, reflects a response with the marginal utility of lifetime wealth held constant. In fact, for our purposes it is sufficient to make these assumptions about calendar years 1978 and 1979 (for the purpose of estimating the substitution effect), and separately for 1980 and 1981 only (for the purpose of determining whether there is evidence of a liquidity effect).

Under the assumptions just described, and assuming a linear relationship between the net marginal tax rate $\mu$ associated with OASI benefits as is common in the labor supply context (Blundell and MaCurdy 1999), one can estimate the $\lambda$-constant response of earnings to $\mu$ by comparing earnings in 1978 and 1979, as the marginal utility of lifetime wealth is constant between these years:

$$E_{1978} = \alpha(1-\mu_{1978}) + \beta\lambda + \varepsilon_t \quad (6)$$

$$E_{1979} = \alpha(1-\mu_{1979}) + \beta\lambda + \varepsilon_t \quad (7)$$

Subtracting (6) from (7), we have:

$$E_{1979} - E_{1978} = \alpha(\mu_{1978} - \mu_{1979}) \quad (8)$$

In practice, we can estimate $\alpha$ by estimating the discontinuity in the difference in earnings between 1979 and 1978, and comparing this to the discontinuity in the substitution effect in 1979 (note that there is no discontinuity in the substitution effect in 1978 as the transitional AMW formula only is affected by earnings beginning in 1979). We can transform this into a Frisch (*i.e.* $\lambda$-constant) elasticity with additional information on mean baseline earnings, *e.g.* mean earnings in the 1916 cohort in 1979, $\overline{E}_{1979,1916}$. Similarly, we can estimate whether liquidity effects play a role by estimating the difference in the level of the discontinuity in earnings in 1980 and in 1981, as the downward discontinuity in benefits paid appears only in 1981.

We could also estimate the relevant parameters in a specification in which the dependent variable is log earnings:

$$\ln(E_{1978}) = \alpha(1-\mu_{1978}) + \beta\lambda + \varepsilon_t \quad (9)$$

$$\ln(E_{1979}) = \alpha(1-\mu_{1979}) + \beta\lambda + \varepsilon_t \quad (10)$$

Subtracting (9) from (10), we have:

$$\ln(E_{1979} / E_{1978}) = \alpha(\mu_{1978} - \mu_{1979}) \quad (11)$$

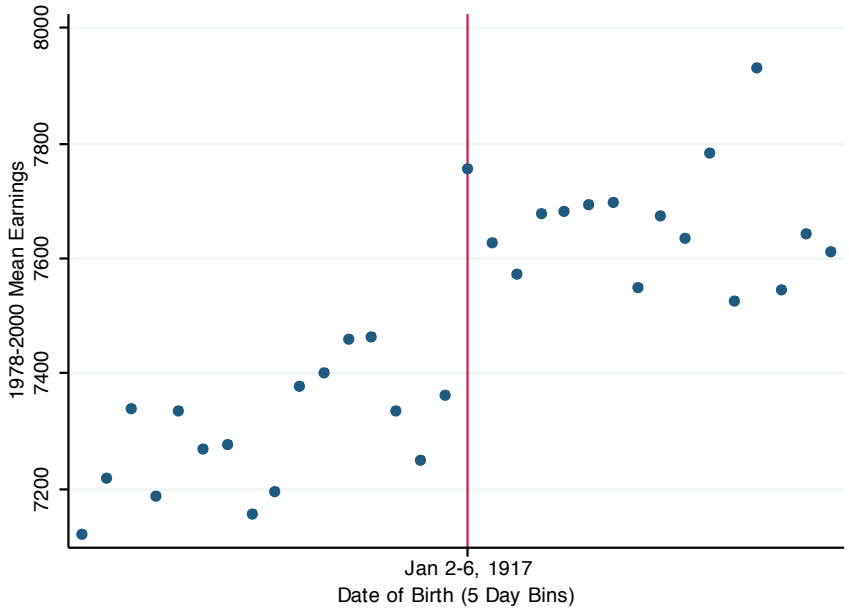
However, in our context, estimating exactly this specification would lead to a problem: we would like to include observations in the regressions when individuals have zero earnings, which is frequent in our context, but the log of zero is undefined. Thus, we approximate the log of earnings using the inverse hyperbolic sine (IHS) of earnings, which is defined at zero and negative values (*e.g.* see related work in Burbidge, Magee, and Robb 1988, Pence 2006, or Gelber 2011). The inverse hyperbolic sine of earnings $E$ is defined as:[38]

[38] A more general form of the inverse hyperbolic sine function adds a scaling parameter; our results are similar when we use other scaling parameters.

$$\sinh^{-1}(E) = \ln(E + \sqrt{1 + E^2})$$

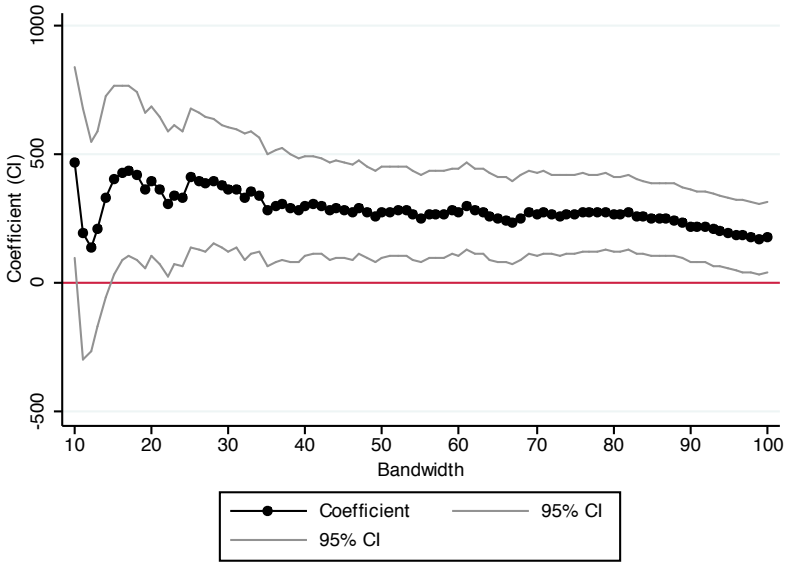
Thus, we can examine the difference between the mean of the IHS of earnings in 1979 and the mean of the IHS of earnings in 1978.

**Appendix Figure 1.** *Mean yearly earnings, 1978 to 2000, 5-day bins*



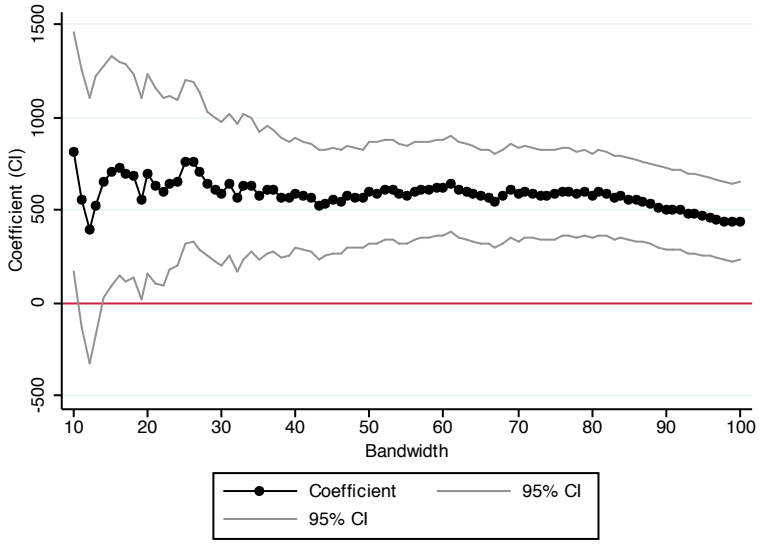

See notes to Figure 4.

**Appendix Figure 2.** *Coefficients and confidence intervals by bandwidth for mean earnings 1978 to 2000, controlling for covariates*




See notes to Figure 4.

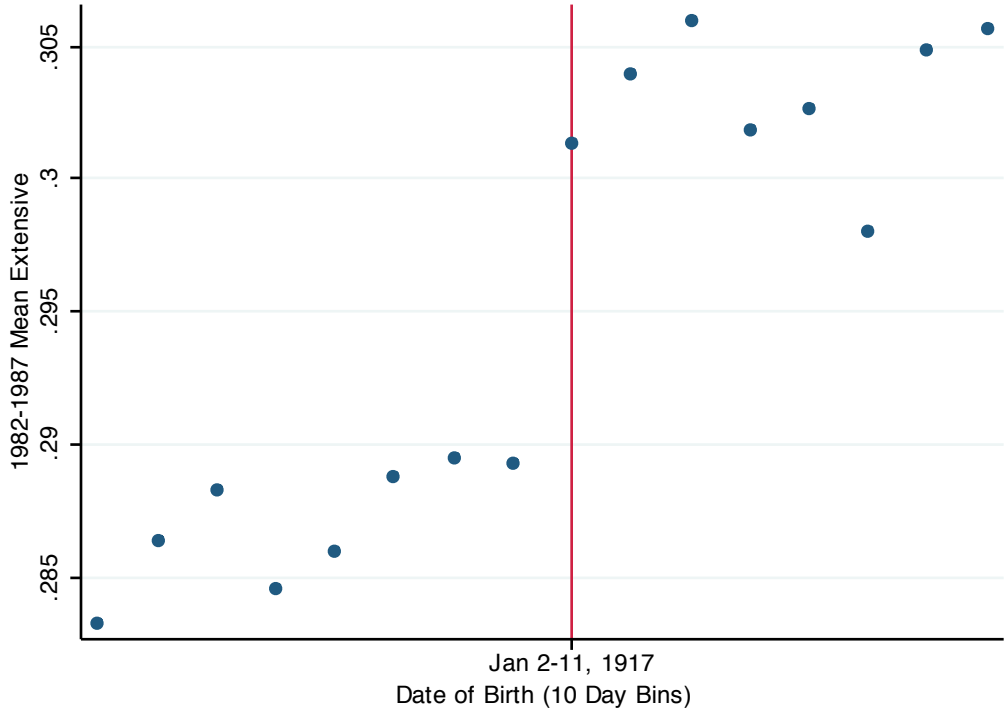
**Appendix Figure 3.** *Coefficients and confidence intervals by bandwidth for mean earnings 1982 to 1987, controlling for covariates*



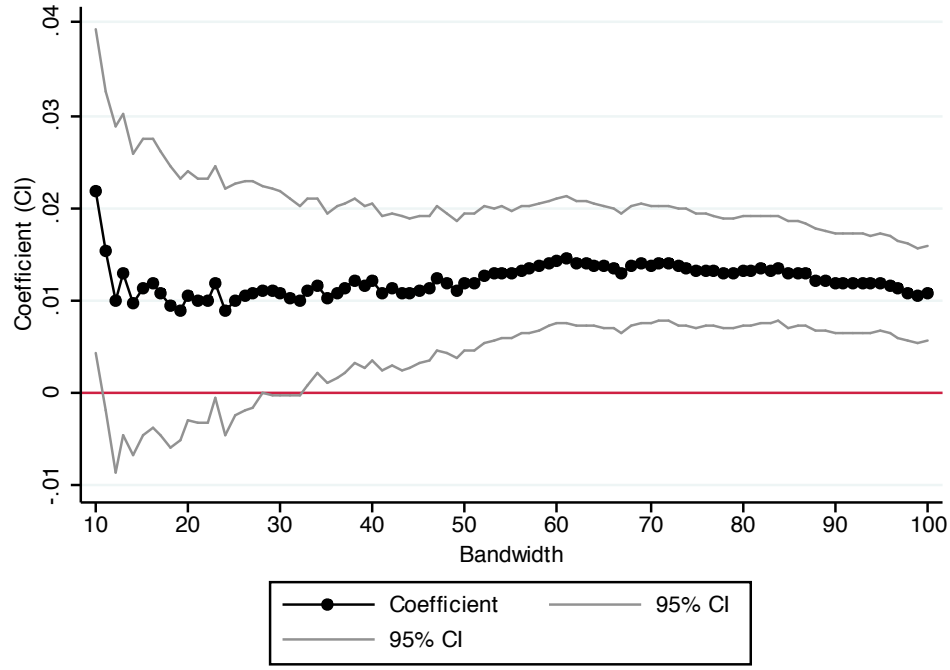

See notes to Figure 4.

**Appendix Figure 4.**

*(a) Extensive margin: mean of male positive earnings dummy, 1982 to 1987*



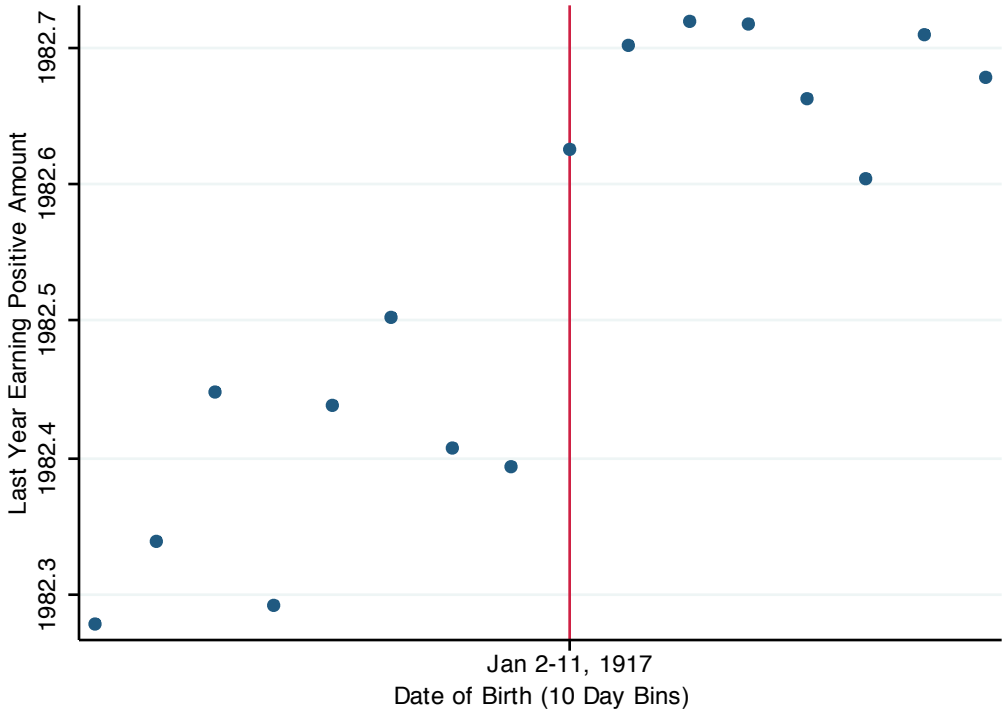

*(b) Coefficients and confidence intervals by bandwidth*



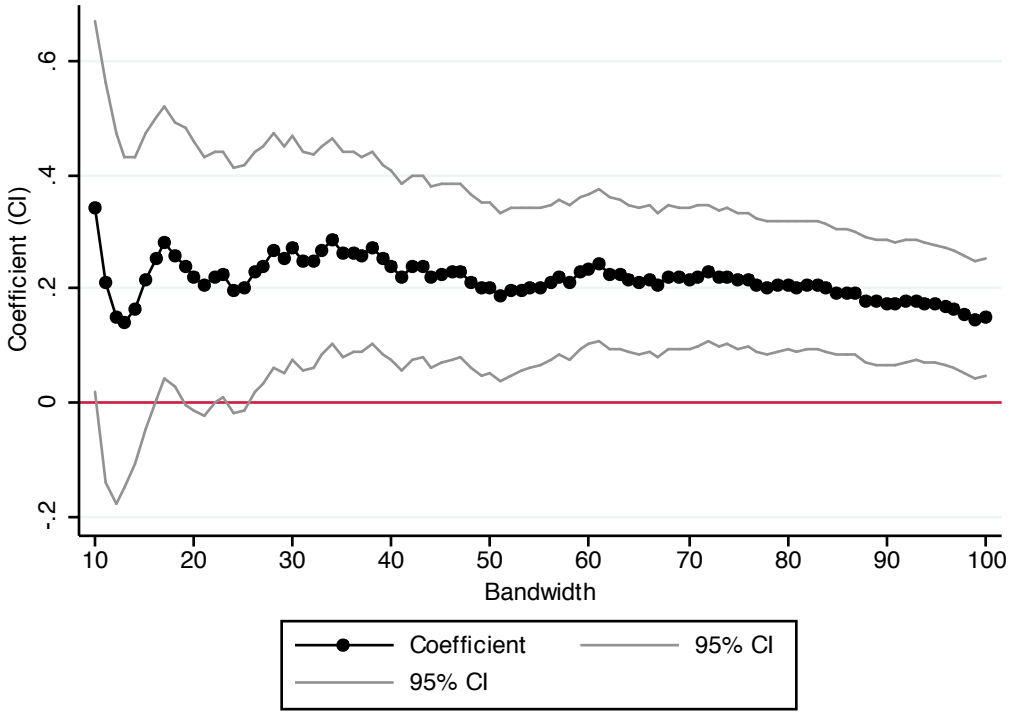

See notes to Figure 4.

**Appendix Figure 5.**

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



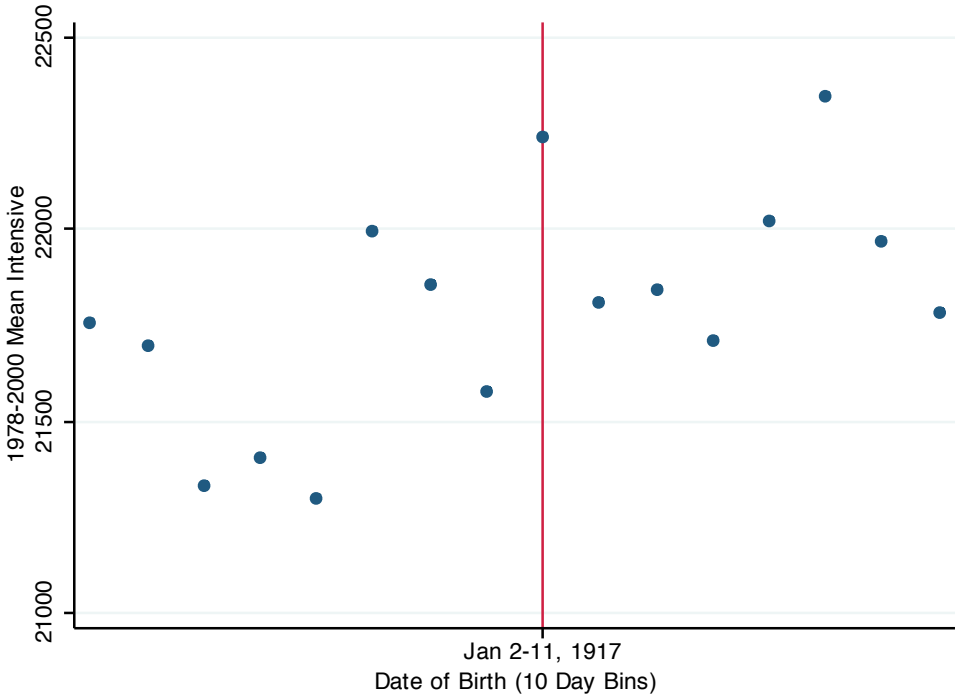

*(b) Coefficient and confidence interval by bandwidth*



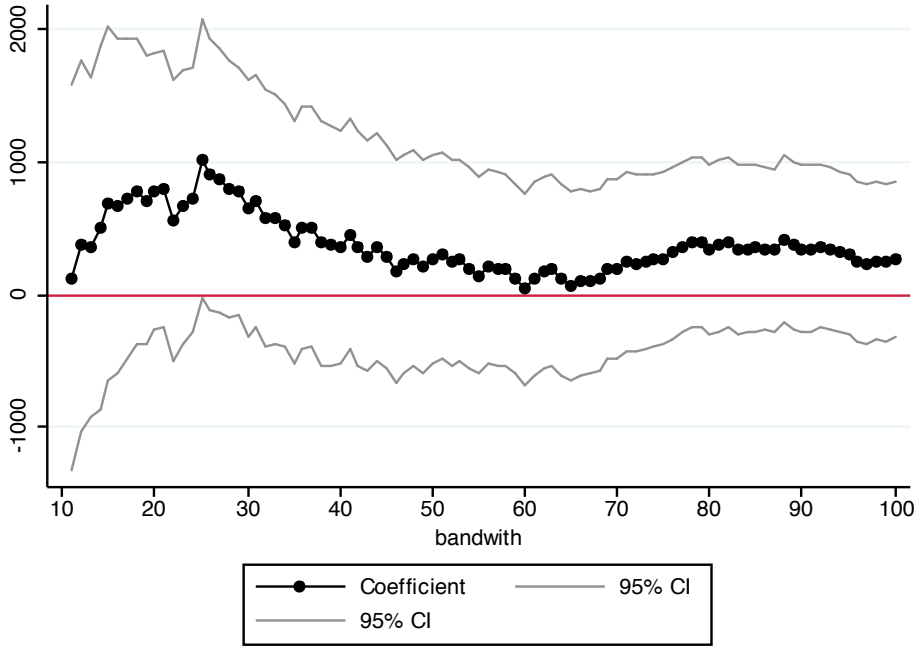

See notes to Figure 4.

**Appendix Figure 6.**

*(a) Intensive margin: mean yearly male earnings (conditional on positive), 1978 to 2000*



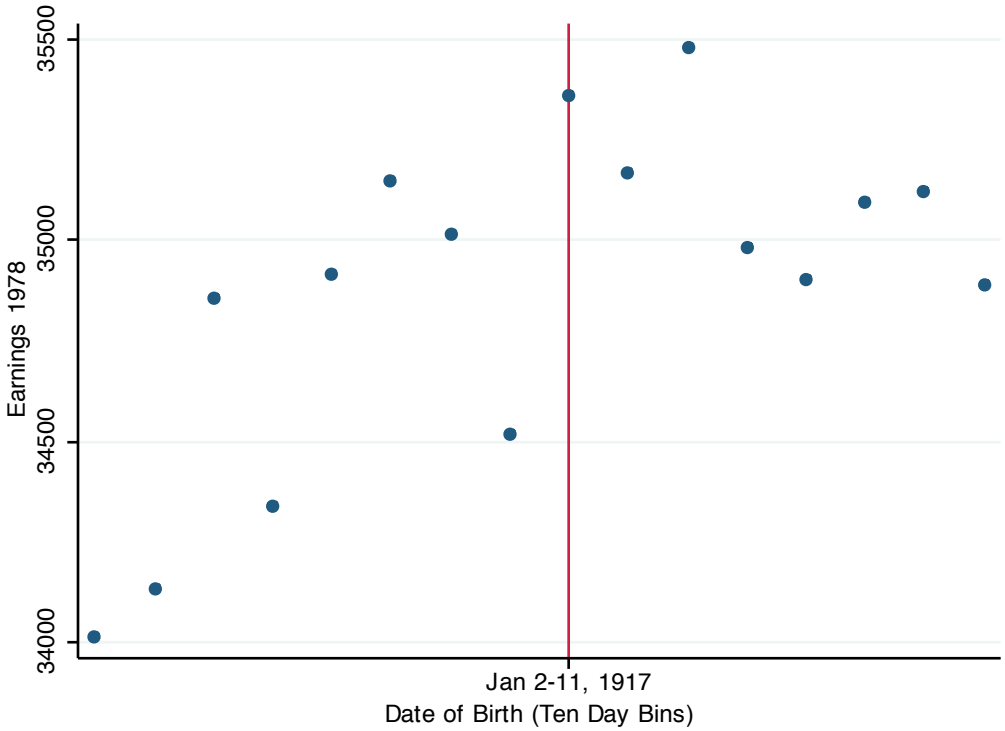

*(b) Coefficients and confidence intervals by bandwidth*



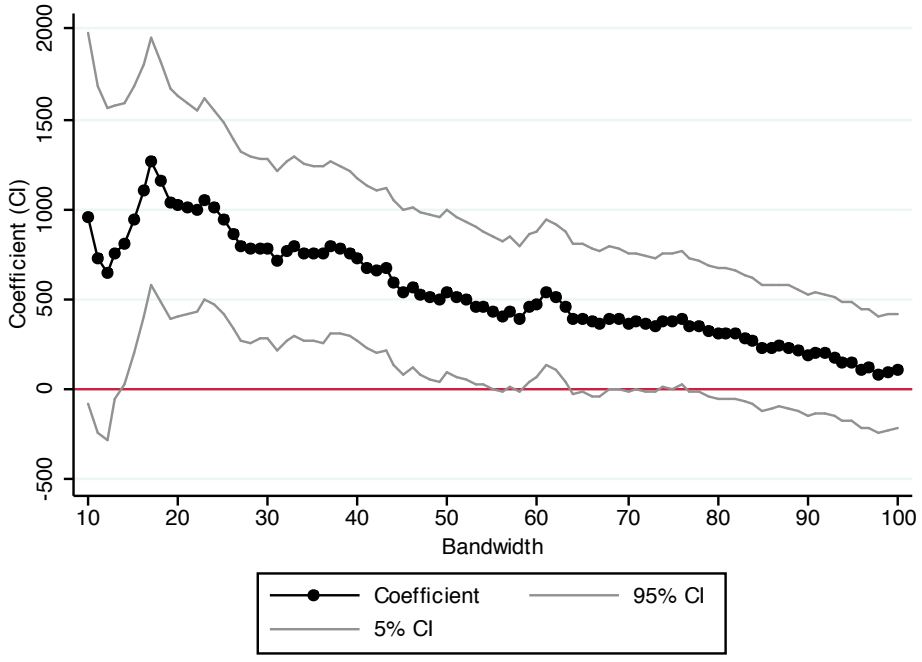

See notes to Figure 4.

**Appendix Figure 7.**

*(a) Mean yearly earnings, 1978*



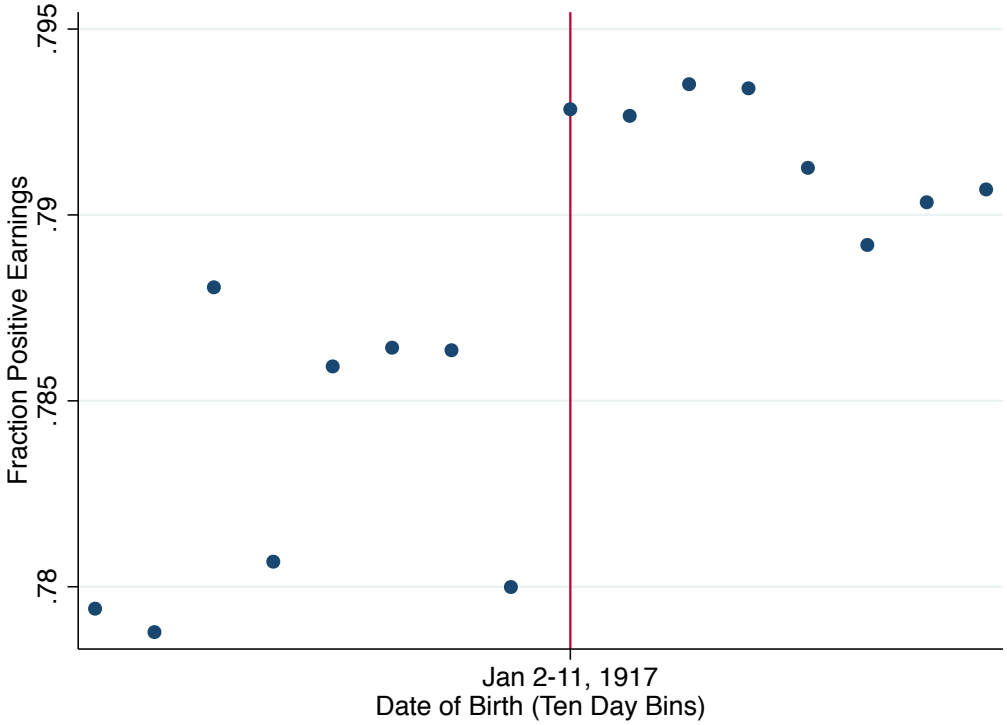

*(b) Coefficients and confidence intervals by bandwidth*



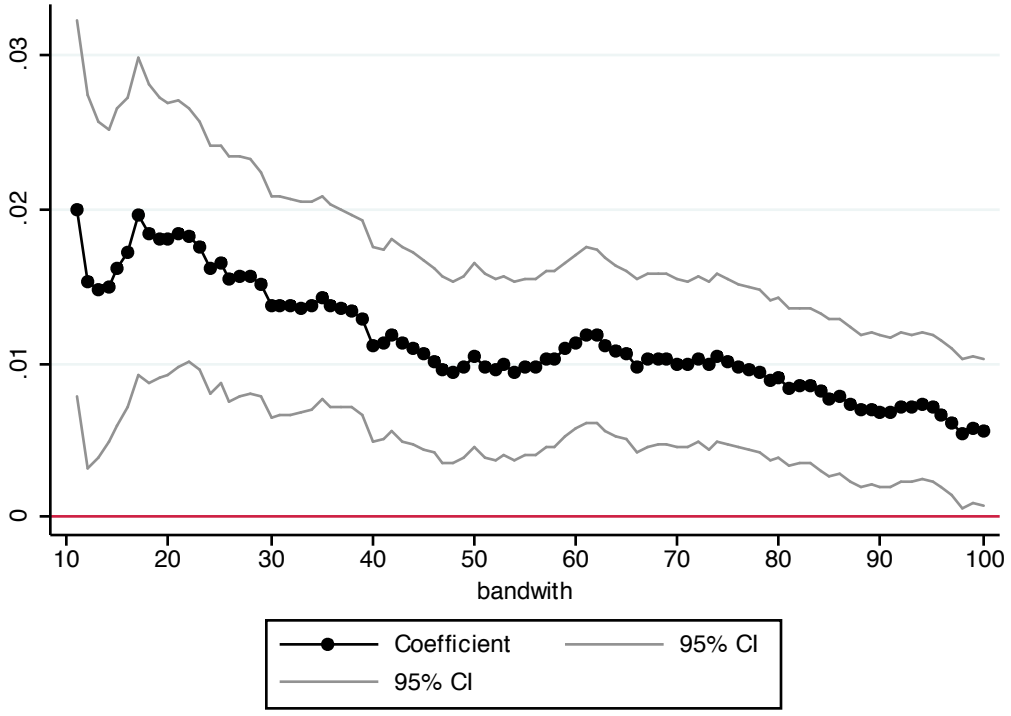

See notes to Figure 4.

**Appendix Figure 8.**

*(a) Fraction with positive earnings, 1978*



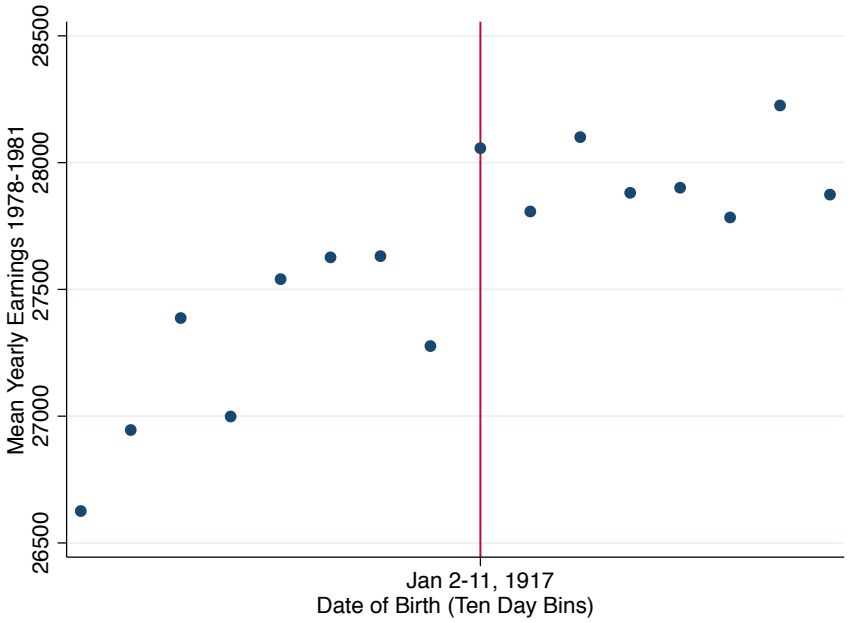

*(b) Point estimates and confidence intervals by bandwidth*



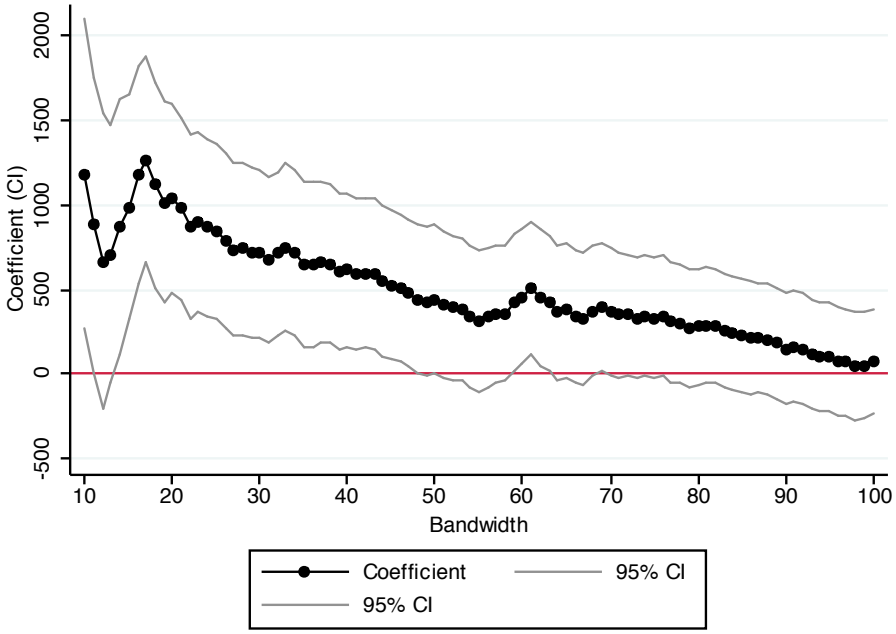

See notes to Figure 4.

**Appendix Figure 9.**

*(a) Mean yearly earnings, 1978 to 1981*



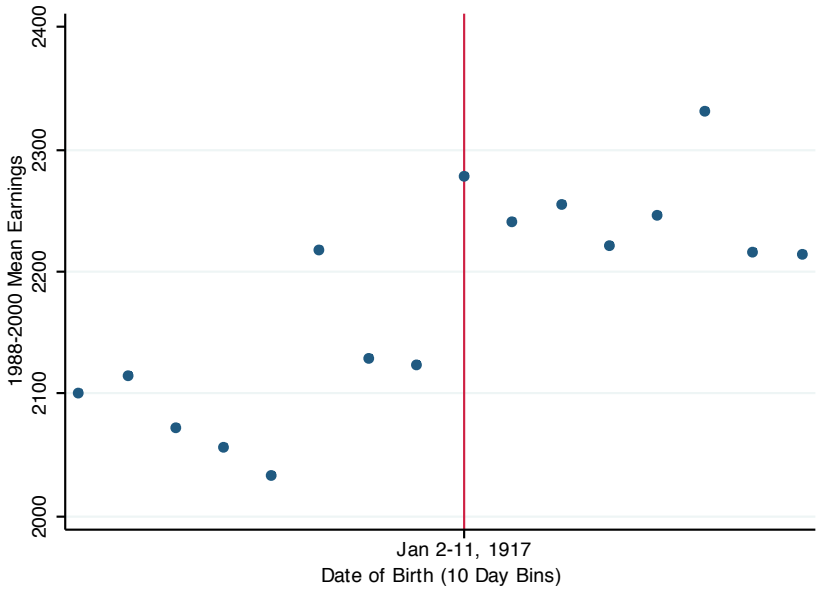

*(b) Coefficients and confidence intervals by bandwidth*



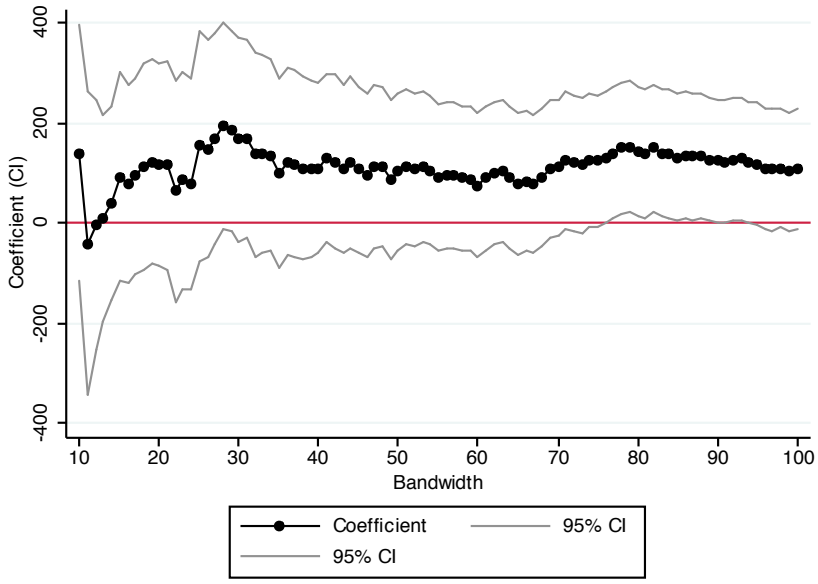

See notes to Figure 4.

**Appendix Figure 10.**

*(a) Mean yearly earnings, 1988 to 2000*



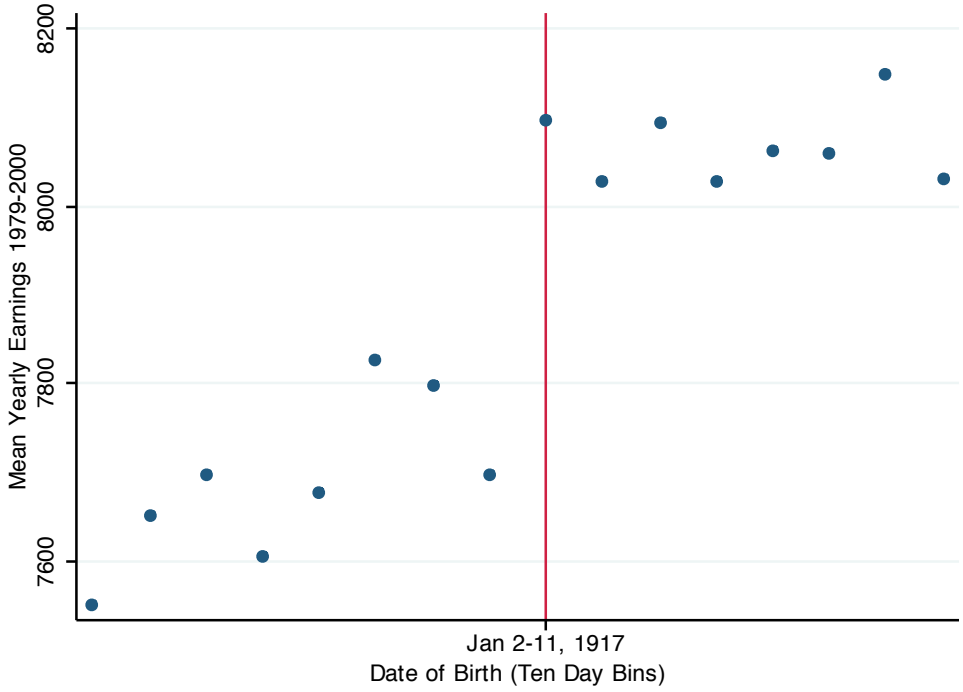

**(c)** *Coefficients and confidence intervals by bandwidth*



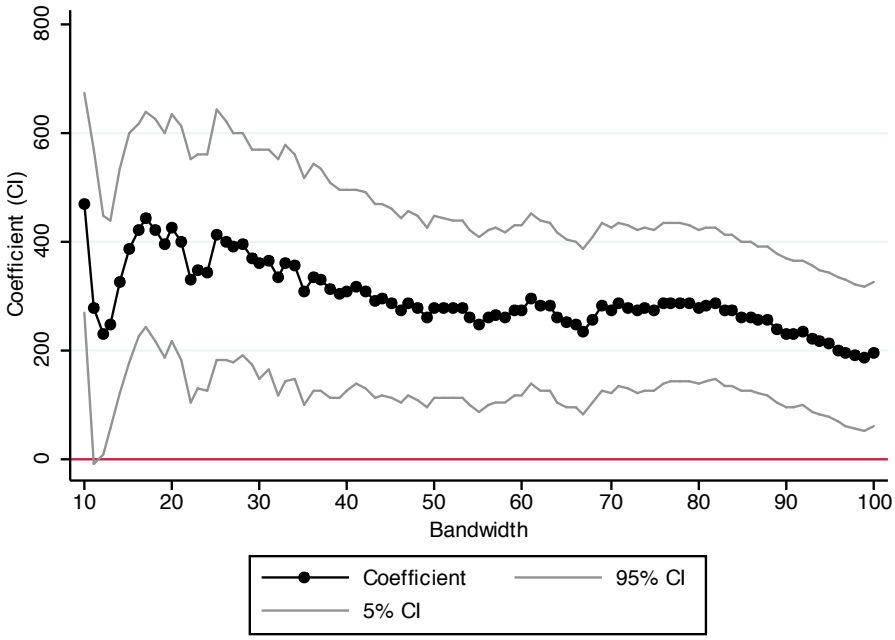

See notes to Figure 4.

**Appendix Figure 11.**

*(a) Mean yearly earnings, 1979 to 2000*



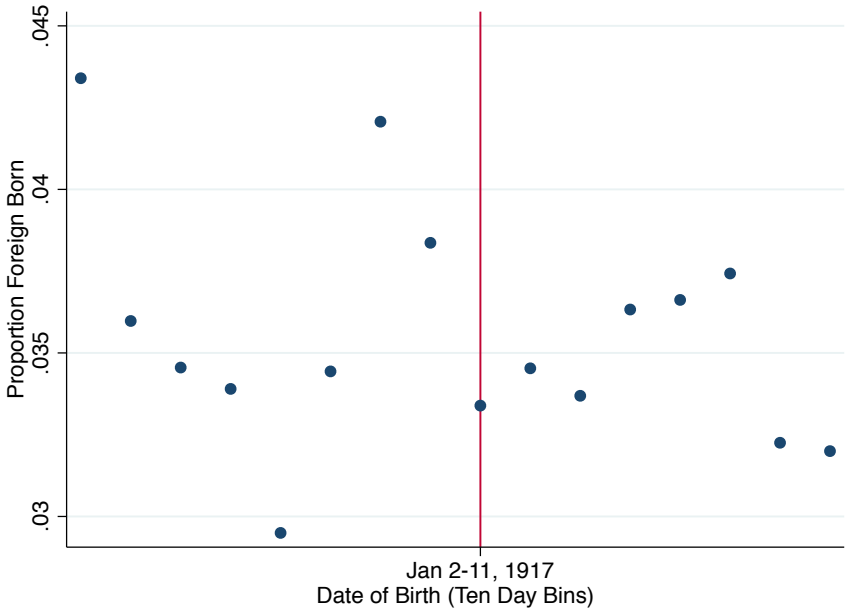

*(b) Coefficients and confidence intervals by bandwidth*



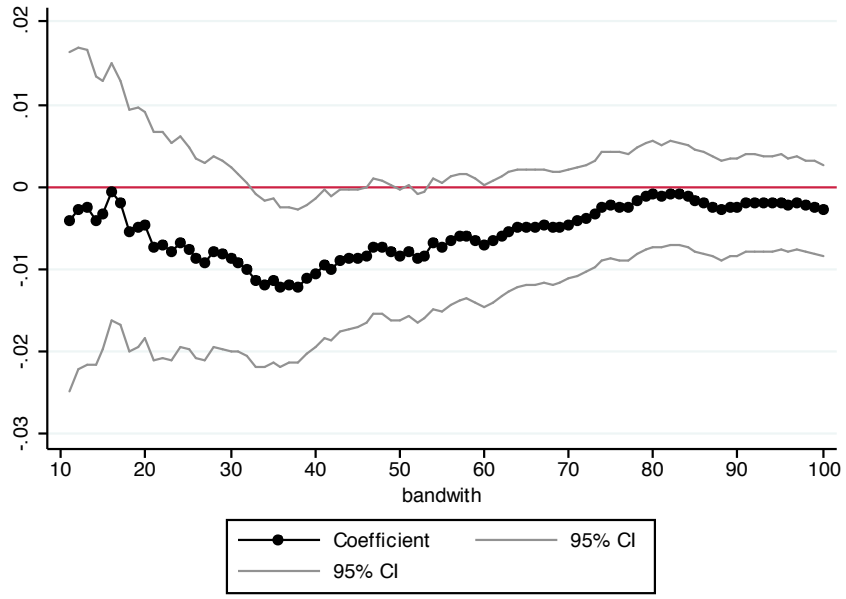

See notes to Figure 4.

**Appendix Figure 12.**

*(a) Fraction foreign born, by DOB bin*



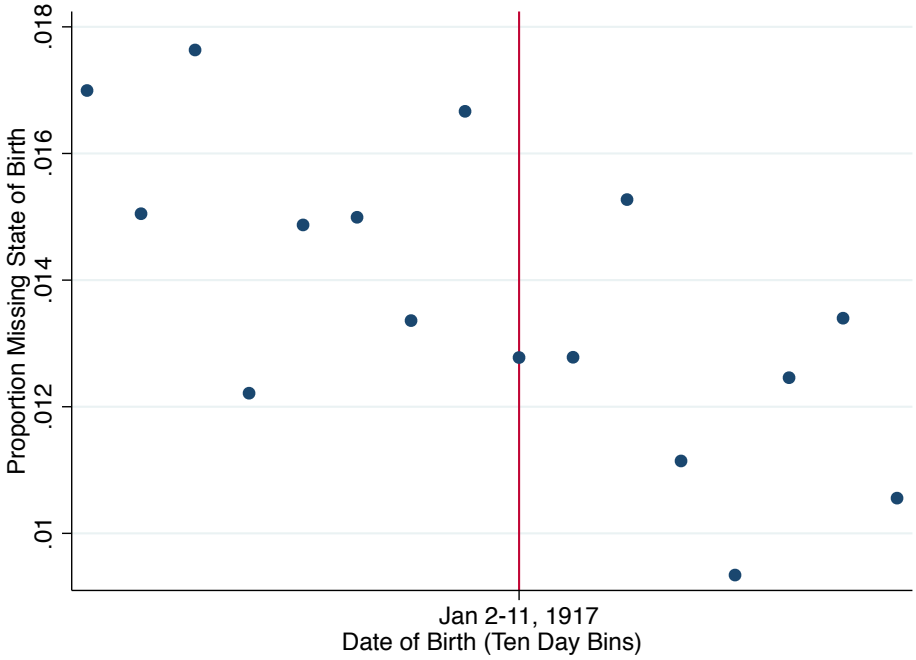

*(b) Coefficients and confidence intervals by bandwidth*



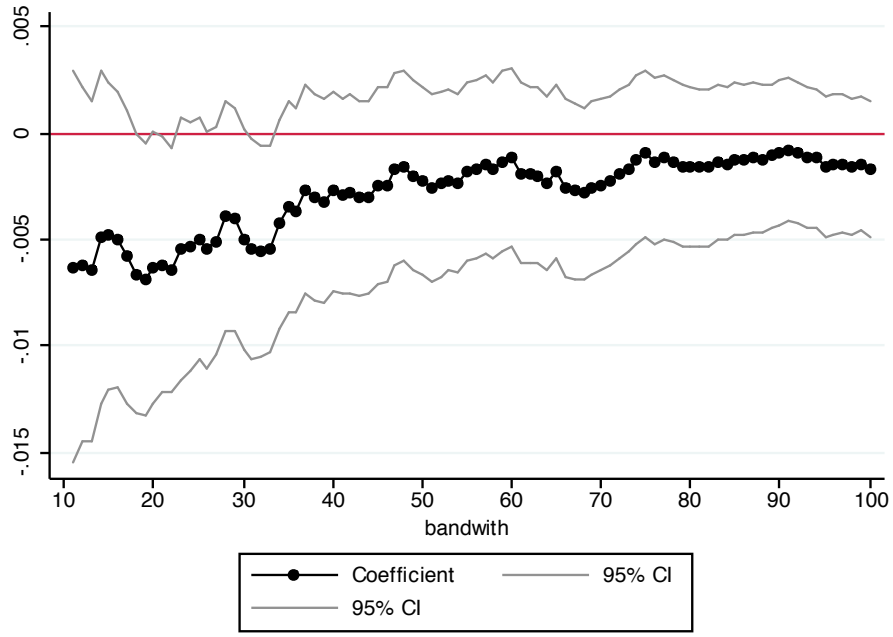

See notes to Figure 4.

**Appendix Figure 13.**

*(a) Fraction missing birthplace, by DOB bin*



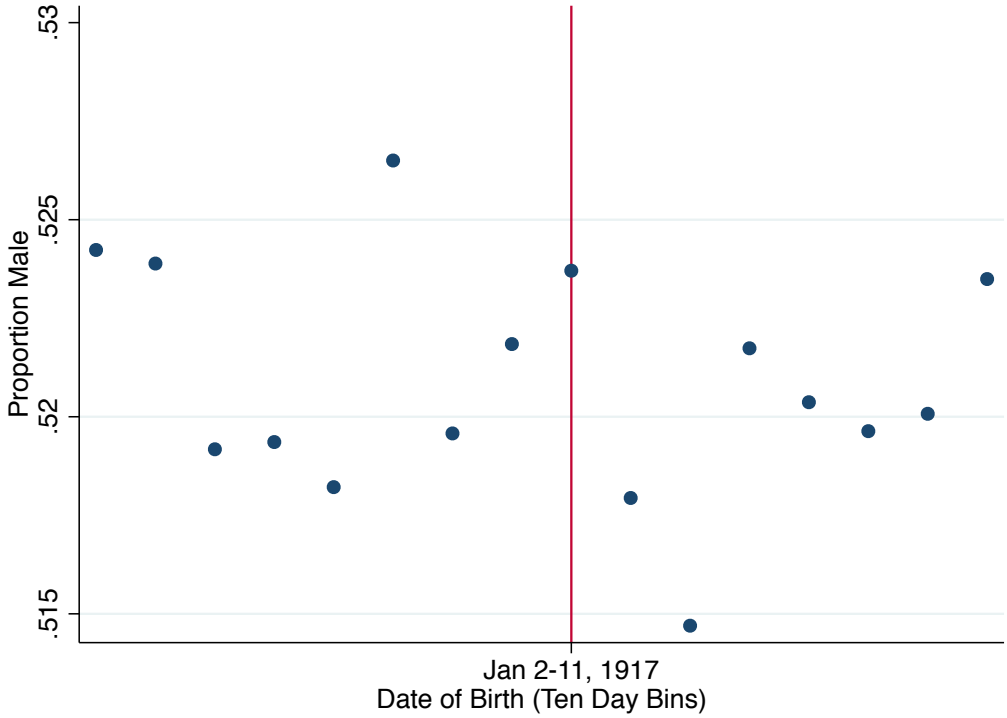

*(b) Coefficients and confidence intervals by bandwidth*



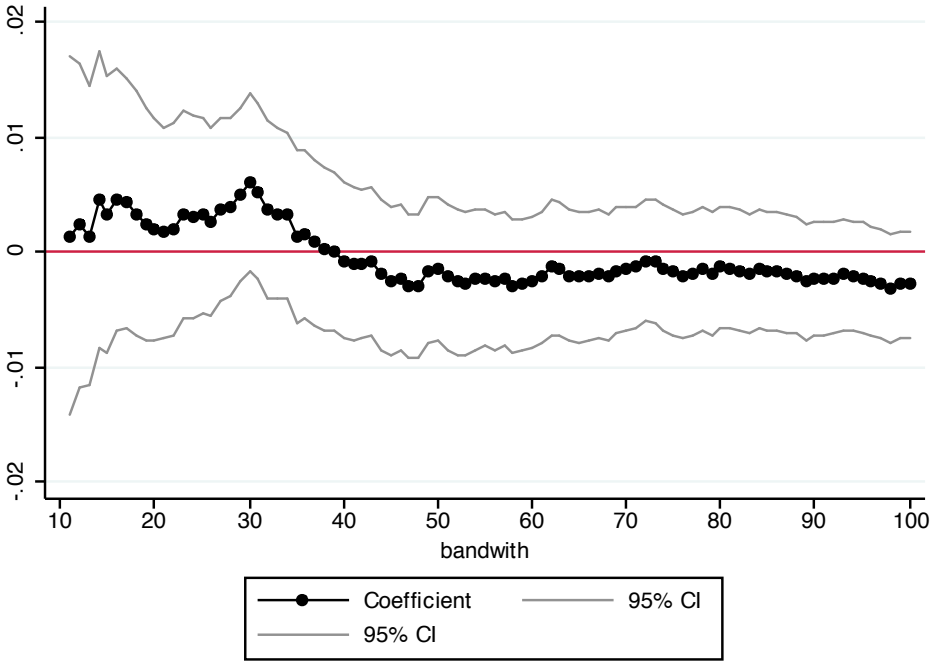

See notes to Figure 4.

**Appendix Figure 14.**

*(a) Proportion male, by DOB*



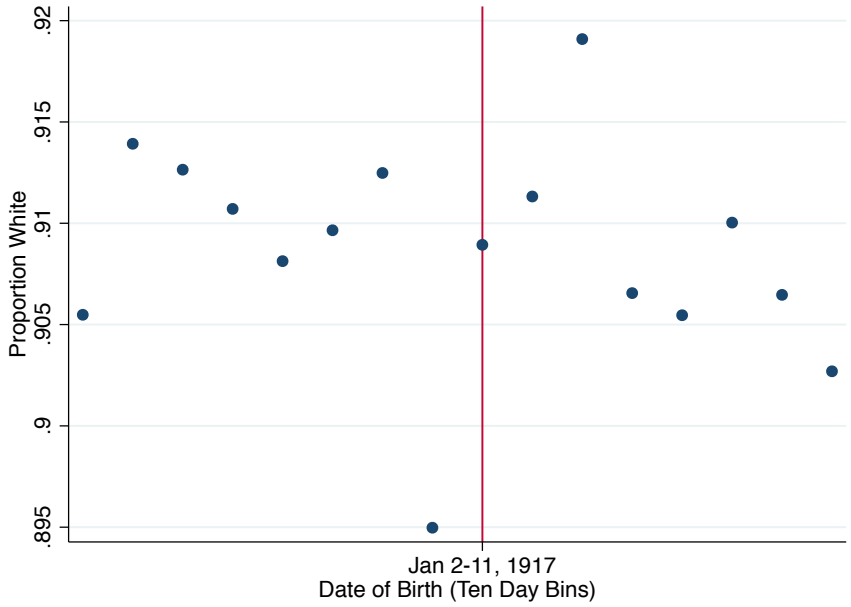

*(b) Coefficients and confidence intervals by bandwidth*



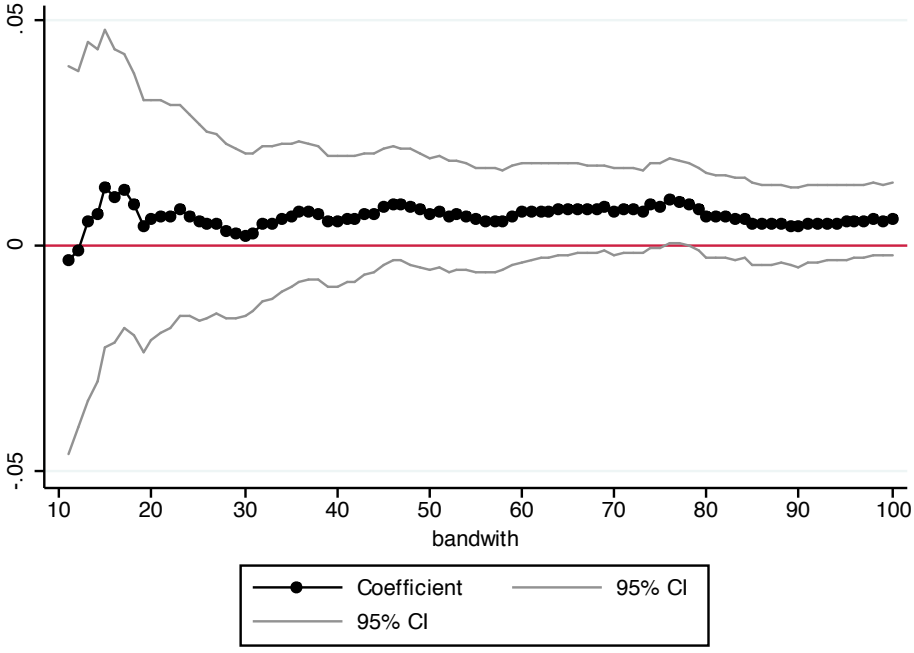

See notes to Figure 4.

**Appendix Figure 15.**

*(a) Proportion White, by DOB*

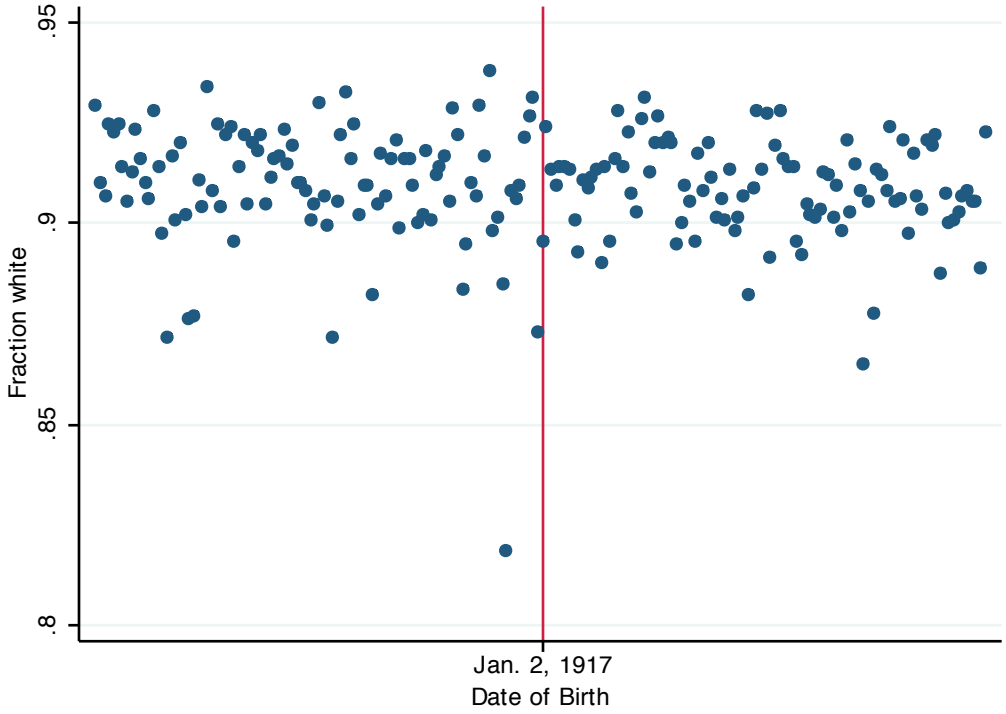



*(b) Coefficients and confidence intervals by bandwidth*




See notes to Figure 4.

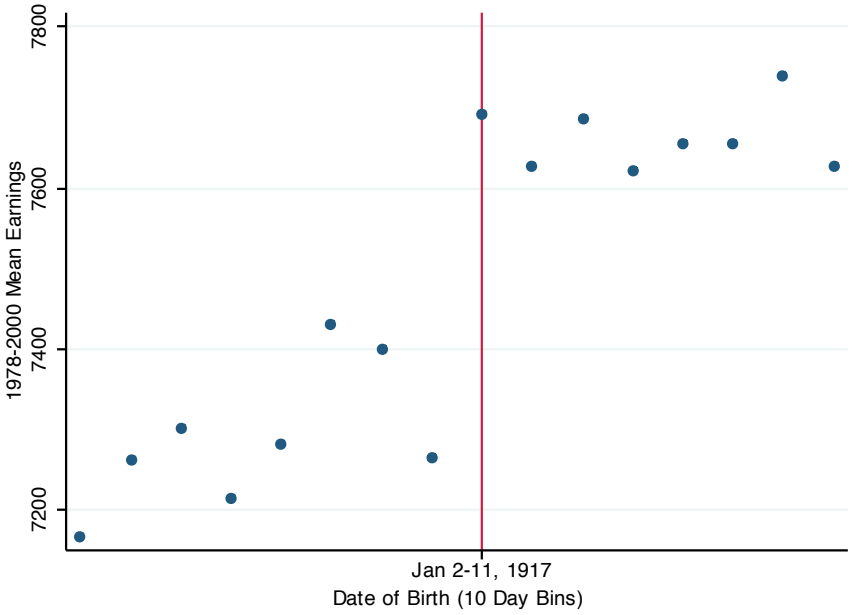
**Appendix Figure 16.** *Fraction of the sample that is white, by single day of birth*



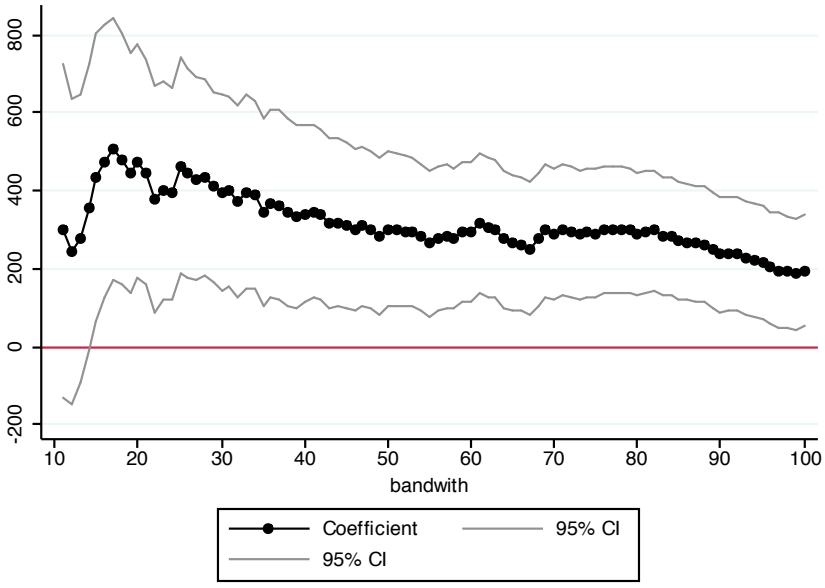

Note: the figure shows that the smaller fraction white in the 10-day bin immediately before the cohort break is due primarily to the much lower fraction white born on December 25, 1916. The following two figures (Appendix Figures 17 and 18) show that the results are robust to excluding this date and a second date on which there is also a more modestly smaller fraction white (January 1, 1917).

**Appendix Figure 17.**

*(a) Mean yearly earnings, 1978 to 2000, removing 12/25/1916 and 01/01/1917*



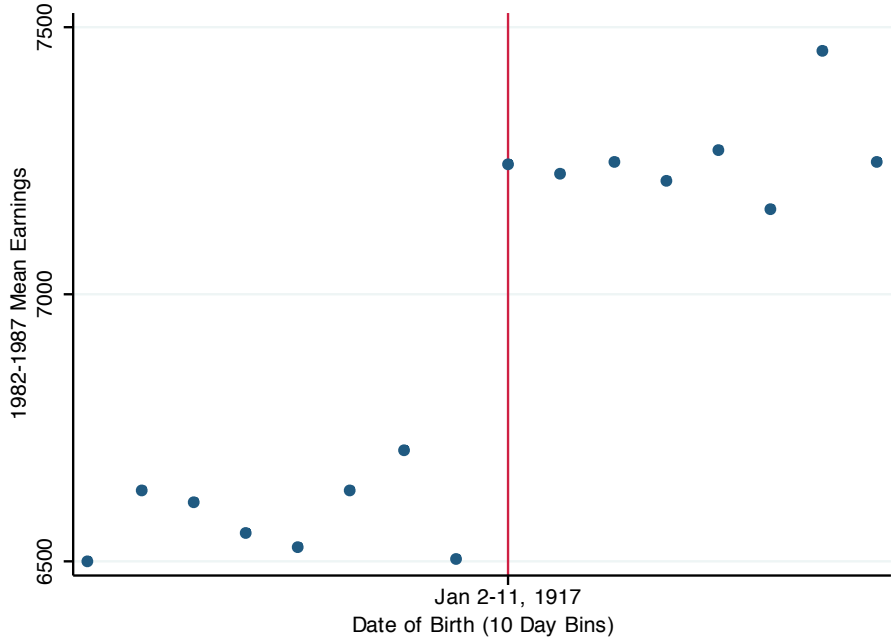

*(b) Point estimates and confidence intervals by bandwidth*



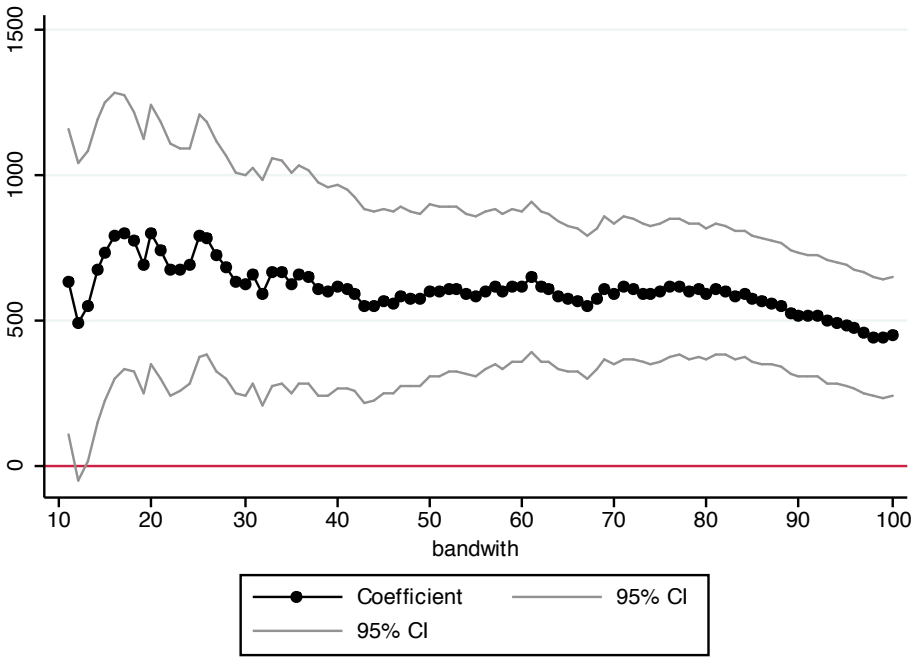

See notes to Figure 4.

**Appendix Figure 18.**

*(a) Mean yearly earnings, 1982 to 1987, removing 12/25/1916 and 01/01/1917*



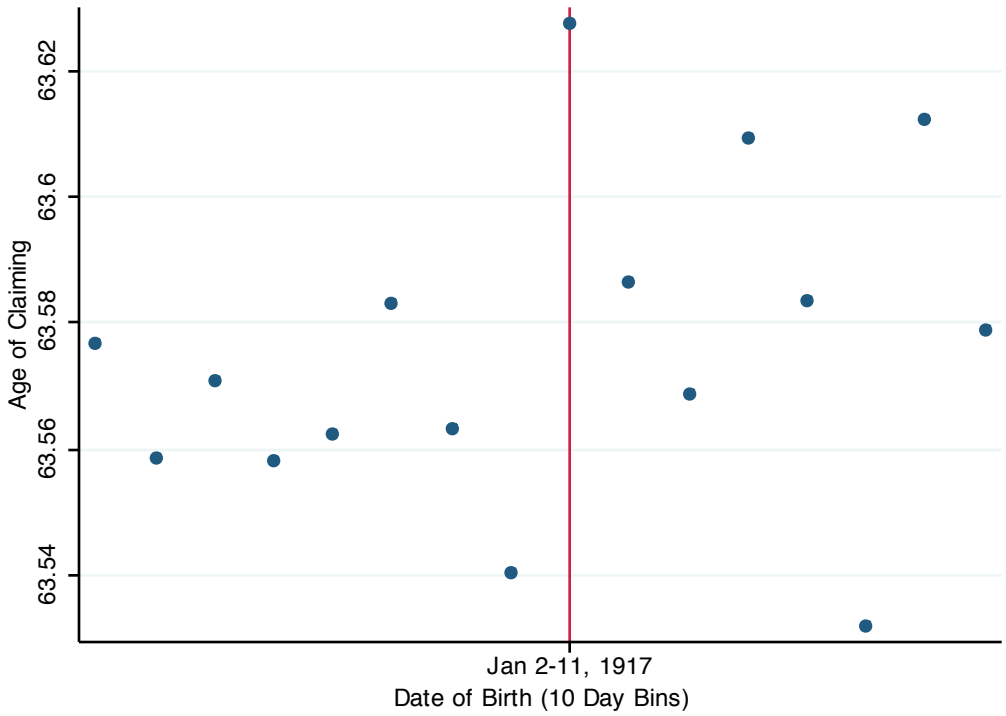

*(b) Point estimates and confidence intervals by bandwidth*



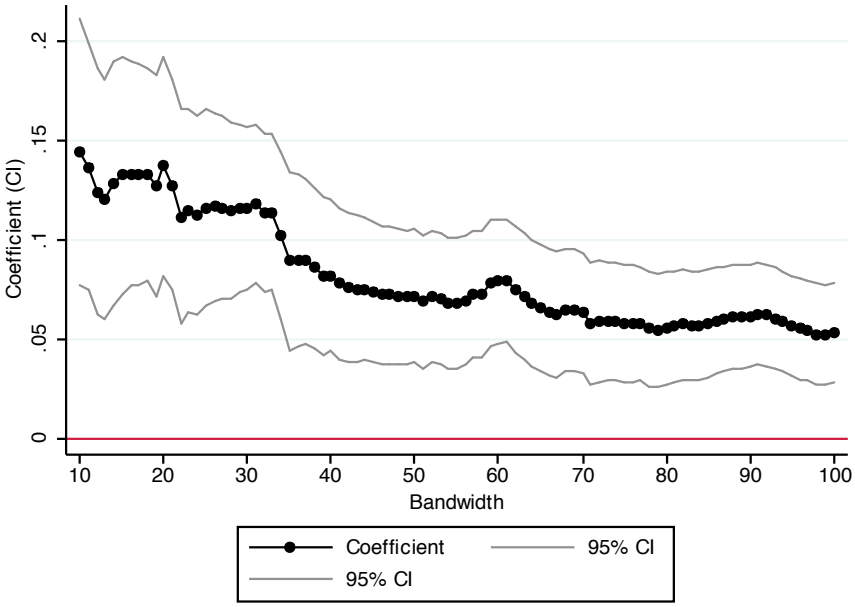

See notes to Figure 4.

**Appendix Figure 19.**

*(a) Mean age of initial OASI claim by DOB bin*



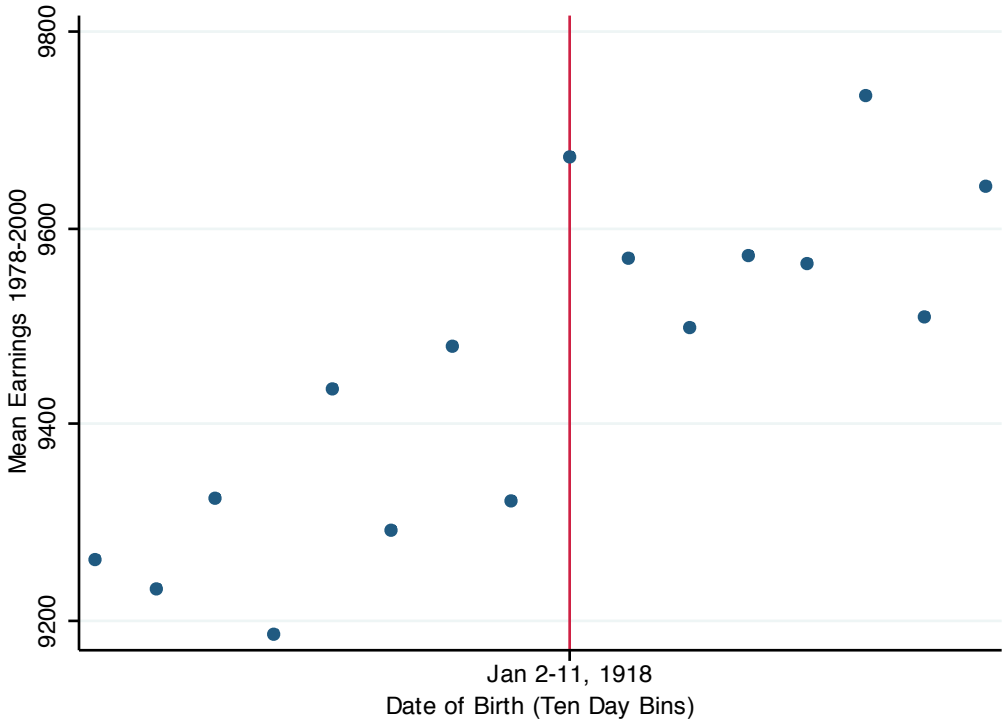

*(b) Coefficients and confidence intervals by bandwidth*



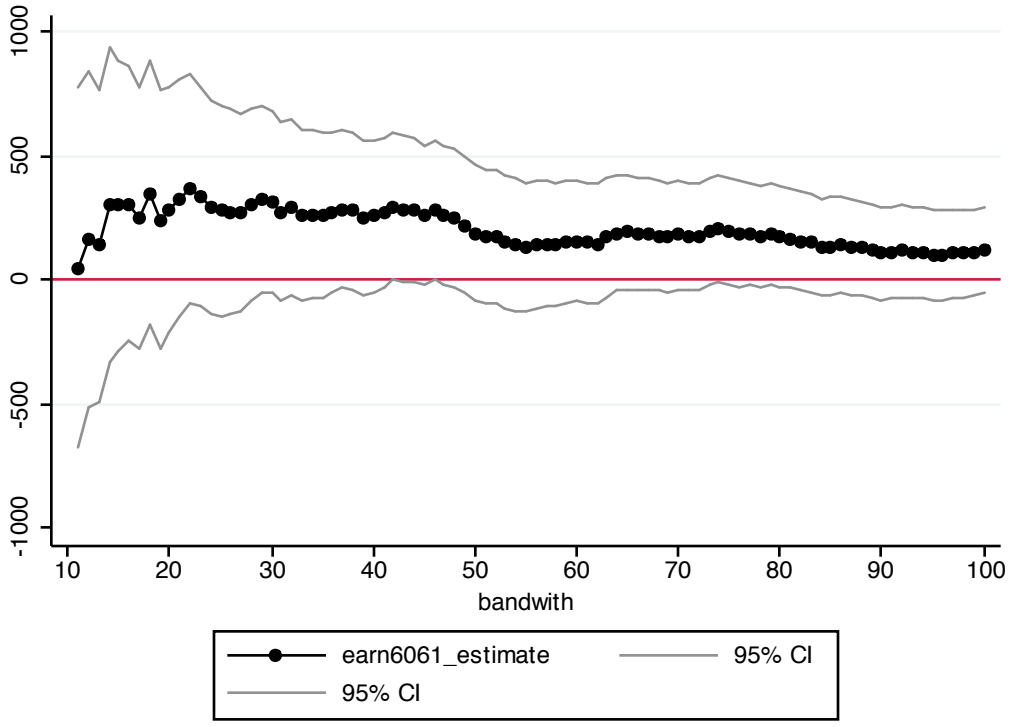

See notes to Figure 4.

**Appendix Figure 20.**

*(a) Mean Earnings, 1978 to 2000, 1917/1918 cohort boundary*



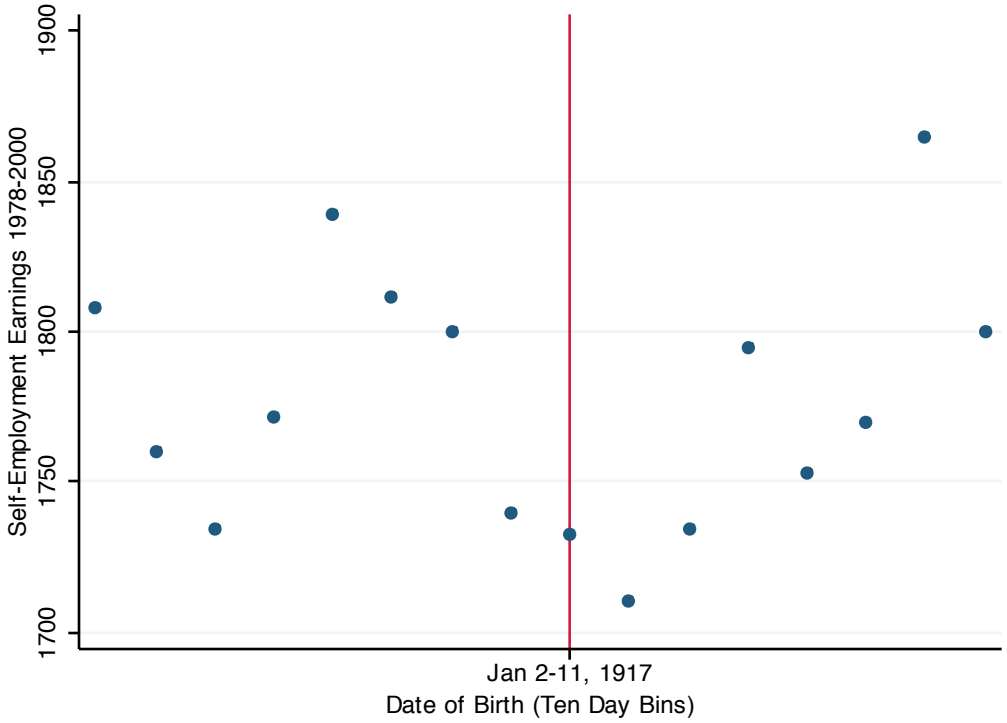

*(b) Coefficients and confidence intervals by bandwidth*



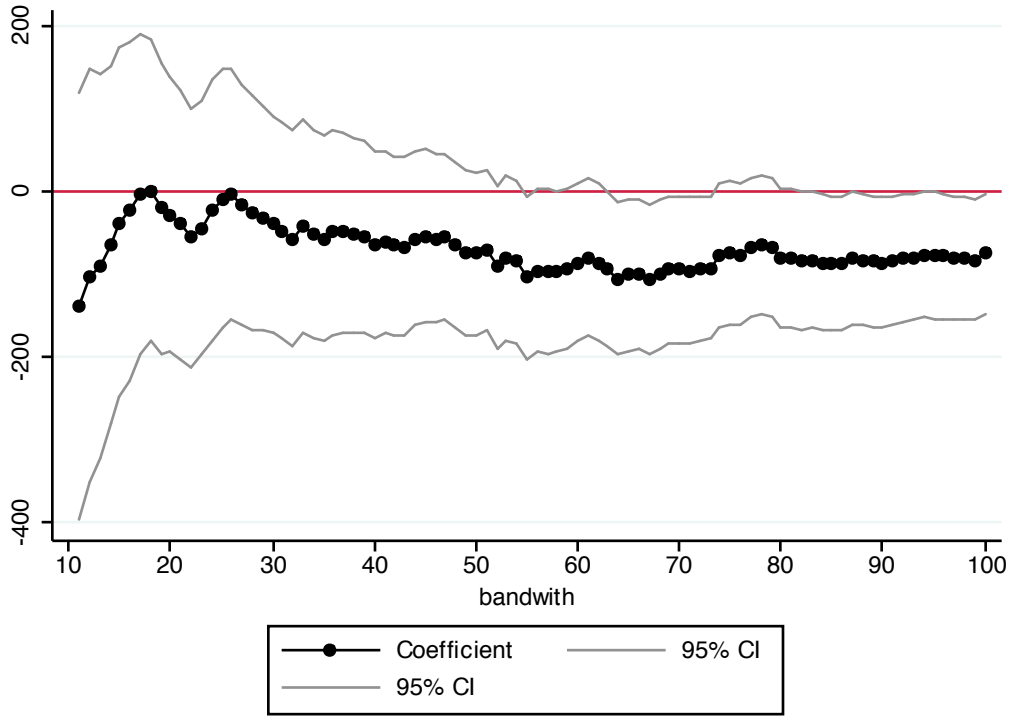

See notes to Figure 4.

**Appendix Figure 21.**

*(a) Self-Employment Earnings, 1978 to 2000*



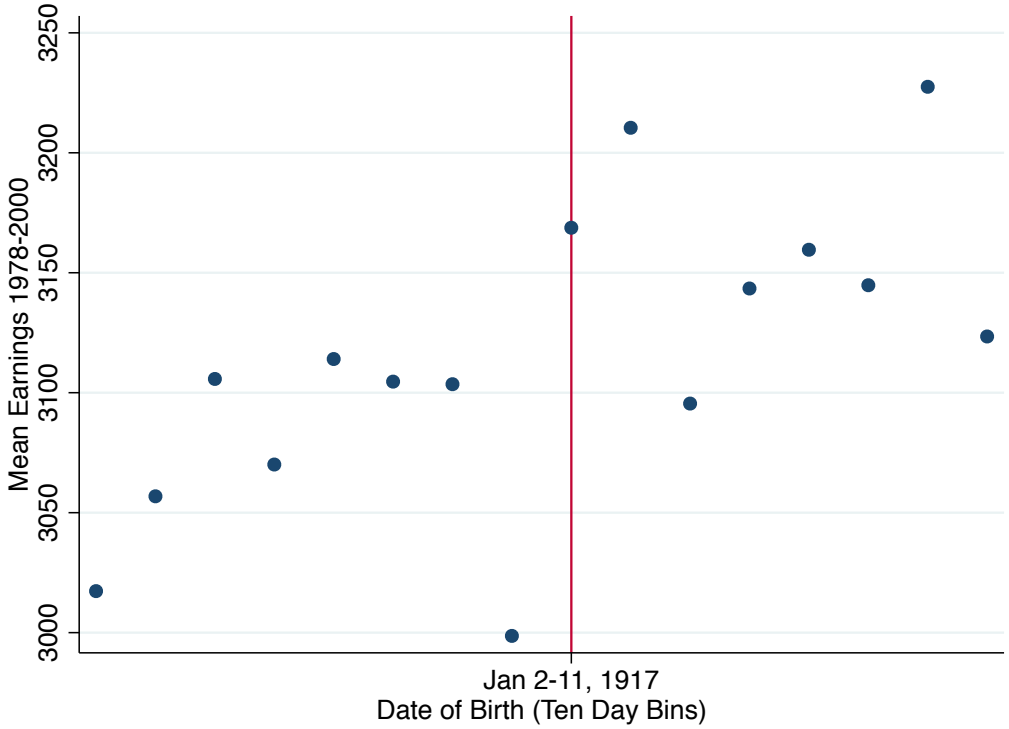

*(b) Coefficients and confidence intervals by bandwidth*



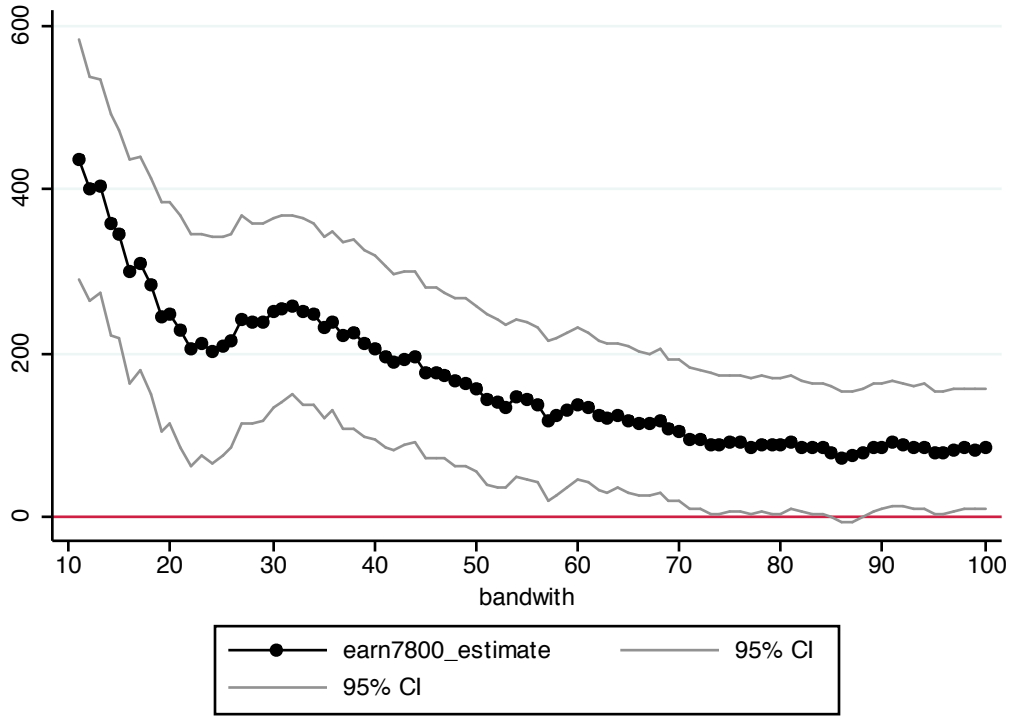

See notes to Figure 4.

**Appendix Figure 22.**

*(a) Mean earnings of women, 1978 to 2000*



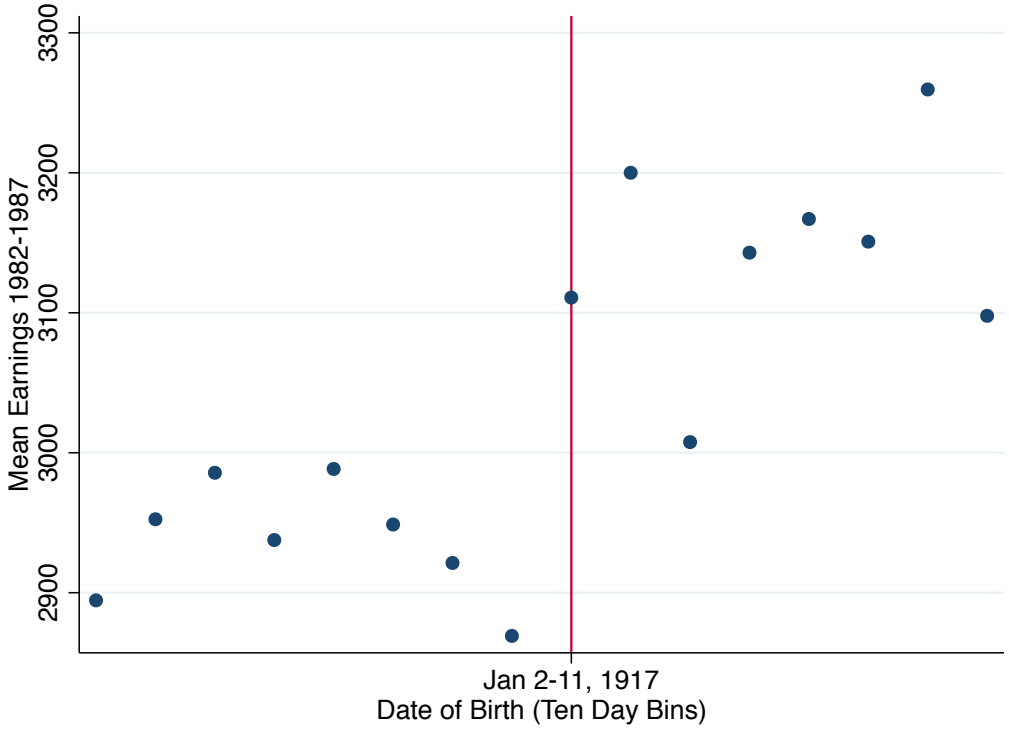

*(b) Coefficients and confidence intervals by bandwidth*



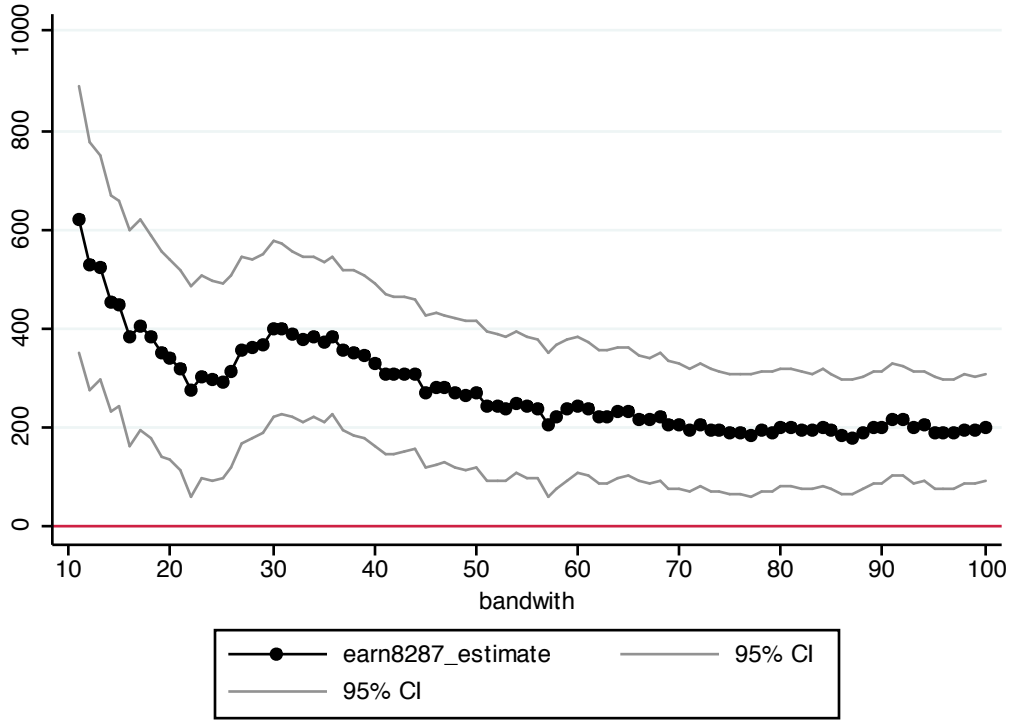

See notes to Figure 4.

**Appendix Figure 23.**

*(b) Mean earnings of women, 1982 to 1987*



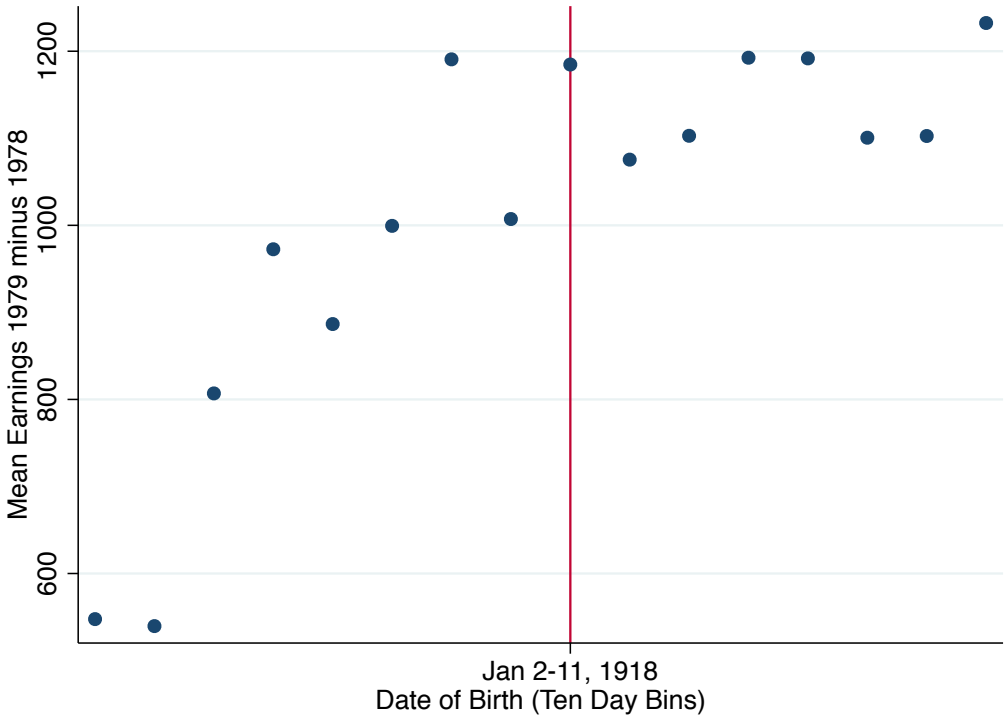

*(b) Coefficients and confidence intervals by bandwidth*



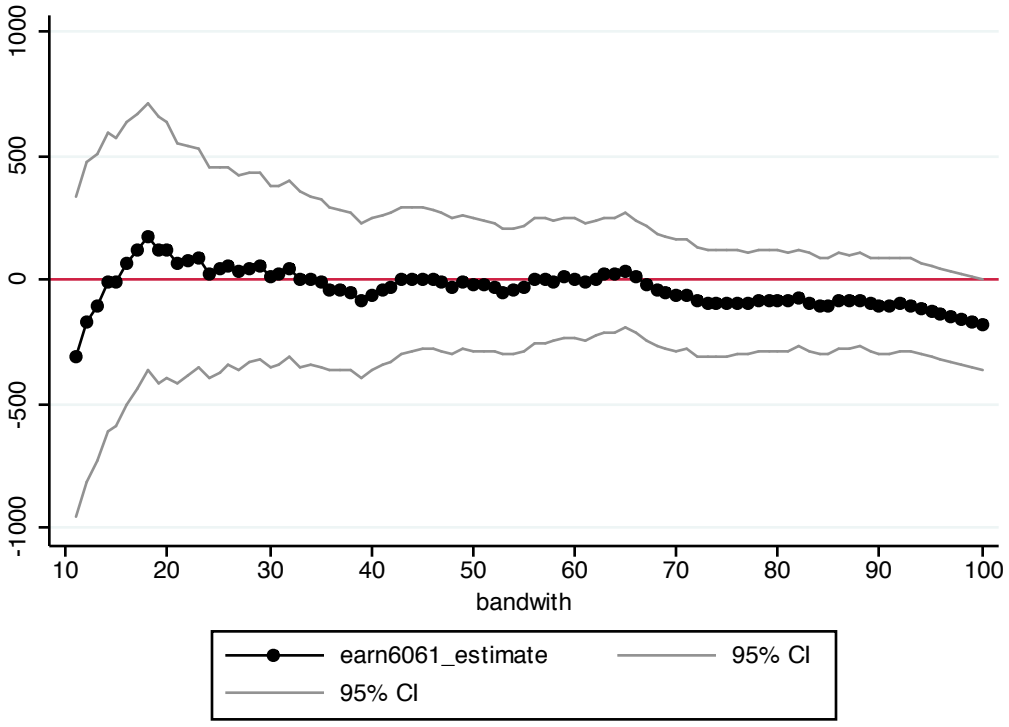

See notes to Figure 4.

**Appendix Figure 24.**

*(a) Mean yearly earnings, 1979 minus 1978, 1917/1918 cohort boundary*



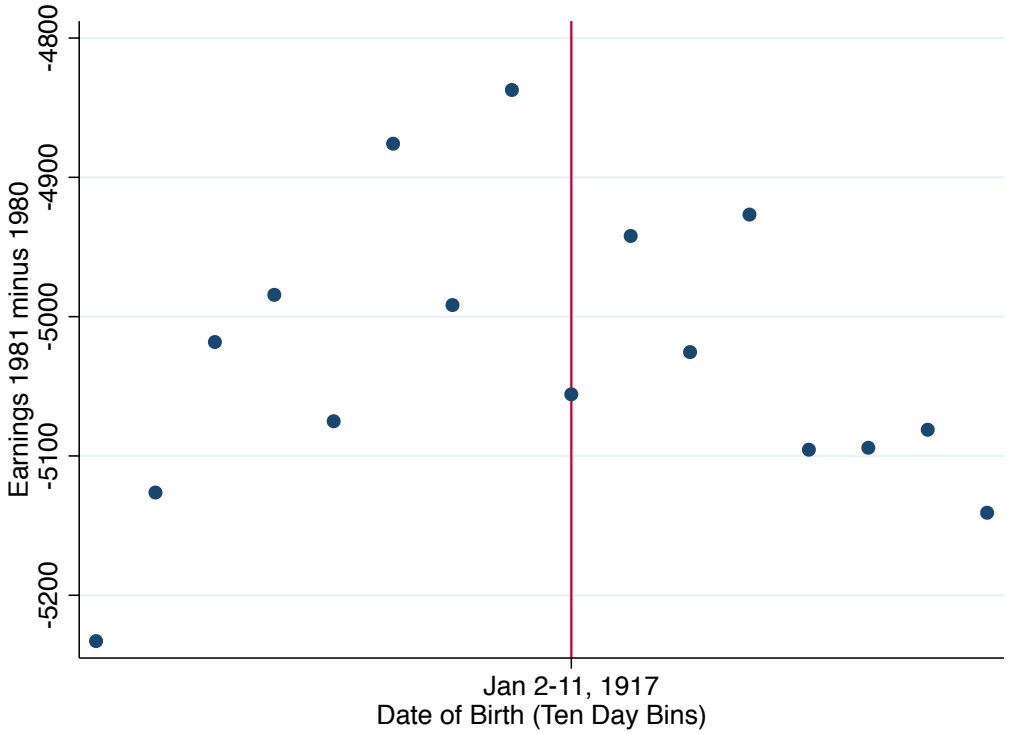

*(b) Coefficients and confidence intervals by bandwidth*



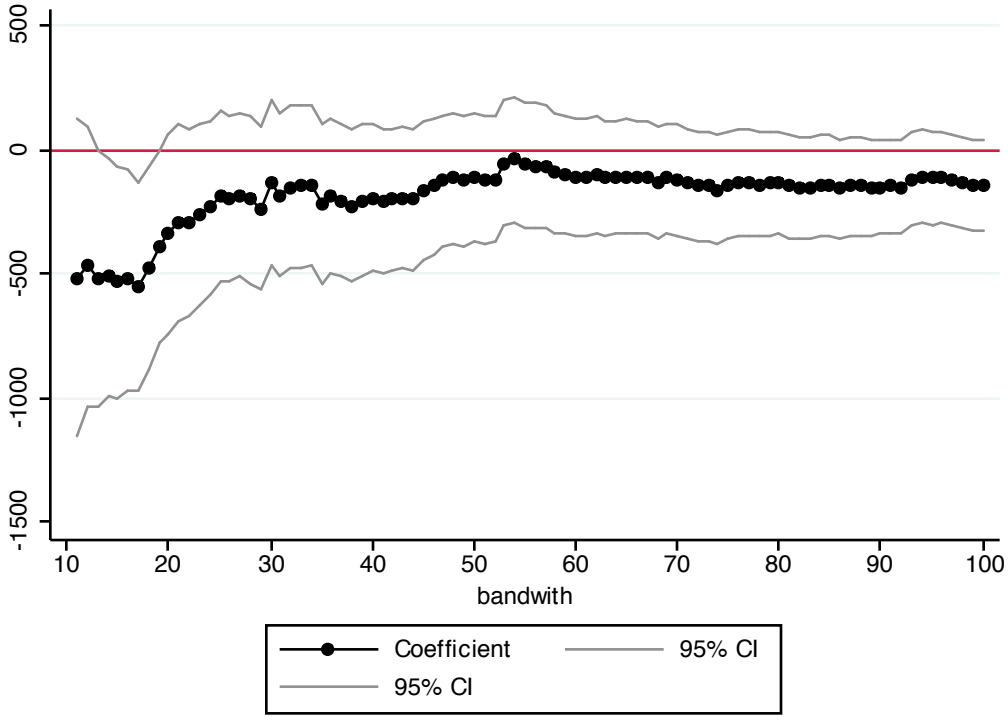

See notes to Figure 4.

**Appendix Figure 25.**

*(a) Mean yearly earnings, 1981 minus 1980, 1916/1917 cohort boundary*




*(b) Coefficients and confidence intervals by bandwidth*




See notes to Figure 4.

**Appendix Table 1.** *Discontinuity in mean yearly benefits*

| Outcome | (1) Mean yearly OASI benefits, 1978-2000 | (2) Mean yearly OASI benefits, 1978-2000 | (3) Mean yearly scheduled OASI benefits, 1978-2000 | (4) Mean yearly scheduled OASI benefits, 1978-2000 |
|---|---|---|---|---|
| Coefficient (SE) on Jan. 2, 1917 dummy | -772.51 (50.73)*** | -816.59 (49.48)*** | -778.98 (45.87)*** | -822.67 (42.94)*** |
| Controls? | N | Y | N | Y |

Note: the dependent variable is specified in the column heading. See notes to Table 2.

**Appendix Table 2.** *Effect of Notch on labor outcomes: OLS regressions of labor outcomes on dummy for being affected by the Notch and control variables*

| Outcome | Chosen bandwidth | Coefficient (SE) |
|---|---|---|
| A) Earnings 1975 to 1976 | 60 | 296.31 (228.91) |
| B) Earnings 1978 to 1981 | 32 | 811.67 (275.08)*** |
| C) Earnings 1982 to 1987 | 76 | 613.39 (117.56)*** |
| D) Earnings 1988 to 2000 | 83 | 142.20 (66.16)** |
| E) Positive earnings dummy, 1978 to 2000 | 92 | 0.0074 (0.0017)*** |
| F) Positive earnings dummy, 1982 to 1987 | 95 | 0.012 (0.0026)*** |
| G) Positive earnings dummy, 1982 to 2000 | 93 | 0.0074 (0.019)*** |
| H) Last year earned positive amount | 86 | 0.21 (0.054)*** |
| I) Earnings 1979 minus 1978 (levels) | 97 | 99.48 (109.48) |
| J) Earnings 1979 minus 1978 (IHS) | 97 | 0.0039 (0.0036) |
| K) Mean earnings 1981 to 1986, 1915/1916 cohort boundary | 93 | 196.55 (282.38) |
| K) Mean earnings 1977 to 1999, 1915/1916 cohort boundary | 98 | 50.35 (191.51) |
| L) Mean earnings 1982 to 1987 among those ineligible for OASI | 80 | 80.68 (71.31) |
| M) Mean earnings 1978 to 2000 among those ineligible for OASI | 81 | 181.33 (178.04) |

Notes: The table shows the results of OLS regressions corresponding to model (1) in the text. These results differ from Table 2 only because in Table 2 we show the results for the bandwidth is chosen using cross-validation for earnings in 1978 to 2000 (*i.e.* a bandwidth of 76), in order to hold the sample constant across regressions), whereas in Appendix Table 2 we show the results for the bandwidth chosen using cross-validation for each outcome separately. See other notes to Table 2. The estimates are extremely similar with controls.

**Appendix Table 3.** *Testing smoothness of predetermined variables: OLS regressions of outcomes on dummy for being affected by the notch*

| Outcome | (1) Percent male | (2) Percent missing birthplace | (3) Percent foreign born | (4) Percent white | (5) Number of observations |
|---|---|---|---|---|---|
| A) Coefficient (SE) on Jan. 2, 1917 dummy (without controls) | -0.0020 | -0.0012 | -0.0025 | 0.011 | -45.85 |
| | (0.0026) | (0.0017) | (0.0035) | (0.0066) | (65.62) |
| B) Coefficient (SE) on Jan. 2, 1917 dummy (with controls) | -0.00053 | -0.0016 | -0.0016 | 0.0010 | 22.32 |
| | (0.0026) | (0.0017) | (0.0036) | (0.0063) | (42.28) |

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 use the linear specification from Table 2. The version "with controls" in row B includes controls for the four covariates (percent male, percent missing birthplace, percent foreign born, and percent white) when number of observations is the dependent variable in column (5). In row B for the other four regressions in columns (1) through (4), in each regression we control for the three of these four variable that are not the same as the dependent variable in each regression. See other notes to Table 2.

**Appendix Table 4.** *Effect of Notch on labor outcomes: OLS regressions of labor outcomes on dummy for being affected by the Notch and control variables*

| Outcome | (1) Linear | (2) Linear |
|---|---|---|
| A) Earnings 1975 to 1976 | 144.81 | -80.12 |
| | (205.15) | (172.05) |
| B) Earnings 1978 to 2000 | 299.07 | 279.36 |
| | (76.26)*** | (77.66)*** |
| C) Earnings 1978 to 1981 | 334.90 | 189.50 |
| | (194.02)* | (174.66) |
| D) Earnings 1982 to 1987 | 613.39 | 606.20 |
| | (116.58)*** | (119.40)*** |
| E) Earnings 1988 to 2000 | 142.98 | 156.17 |
| | (67.18)** | (69.12)** |
| F) Positive earnings dummy, 1978 to 2000 | 0.0088 | 0.0085 |
| | (0.0019)*** | (0.0019)*** |
| G) Positive earnings dummy, 1982 to 1987 | 0.013 | 0.013 |
| | (0.0029)*** | (0.0030)*** |
| H) Positive earnings dummy, 1982 to 2000 | 0.0088 | 0.0088 |
| | (0.0020)*** | (0.0020)*** |
| Controls? | N | Y |

Notes: The table shows the results of OLS regressions corresponding to model (1) in the text. These results differ from those shown in the main text only because in the main text we show the results when the dependent variable is mean earnings by DOB in the set of years examined, whereas in Appendix Table 4 we show the results when we treat mean earnings by DOB in a given year as a separate observation and cluster by DOB. See other notes to Table 2.

**Appendix Table 5.** *Specifications parallel to Krueger-Pischke: coefficients and standard errors from OLS estimates of log odds ratio of participation rate or log earnings on log mean OASI benefits and fixed effects, at the cohort-year level*

| | Log Odds Ratio of Participation Rate | | Log Earnings | |
|---|---|---|---|---|
| Log Lifetime OASI benefits | 1.79 (0.31)*** | -0.06 (0.09) | 0.28 (0.16)* | .03 (0.12) |
| Age dummies | X | X | X | X |
| Year dummies | | X | | X |
| Observations | 53 | 53 | 53 | 53 |

Notes: The table reports estimates of the coefficient $\alpha$ from model (4). The data are yearly data on participation rates (columns 1 and 2) or earnings (columns 3 and 4) of men born 1916 to 1922 (inclusive), observed between ages 60 and 68. These yearly averages are calculated using the SSA Master Earnings File. This is the same set of birth cohorts and ages used in Krueger and Pischke (1992), who instead use the CPS. Participation is defined as having positive earnings. Age is defined in our data as the highest age an individual turns during a calendar year; if instead we define age as the age an individual initially has during the calendar year, we obtain similar results. *** refers to statistical significance at the 1% level; ** at the 5% level; * at the 1% level.