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

Evidence from the Notch Cohorts

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Notes

- Does not necessarily reflect the views of the U.S. Treasury or the Social Security Administration
- Preliminary and incomplete
- Please do not circulate or cite

Outline

Introduction

Policy Environment

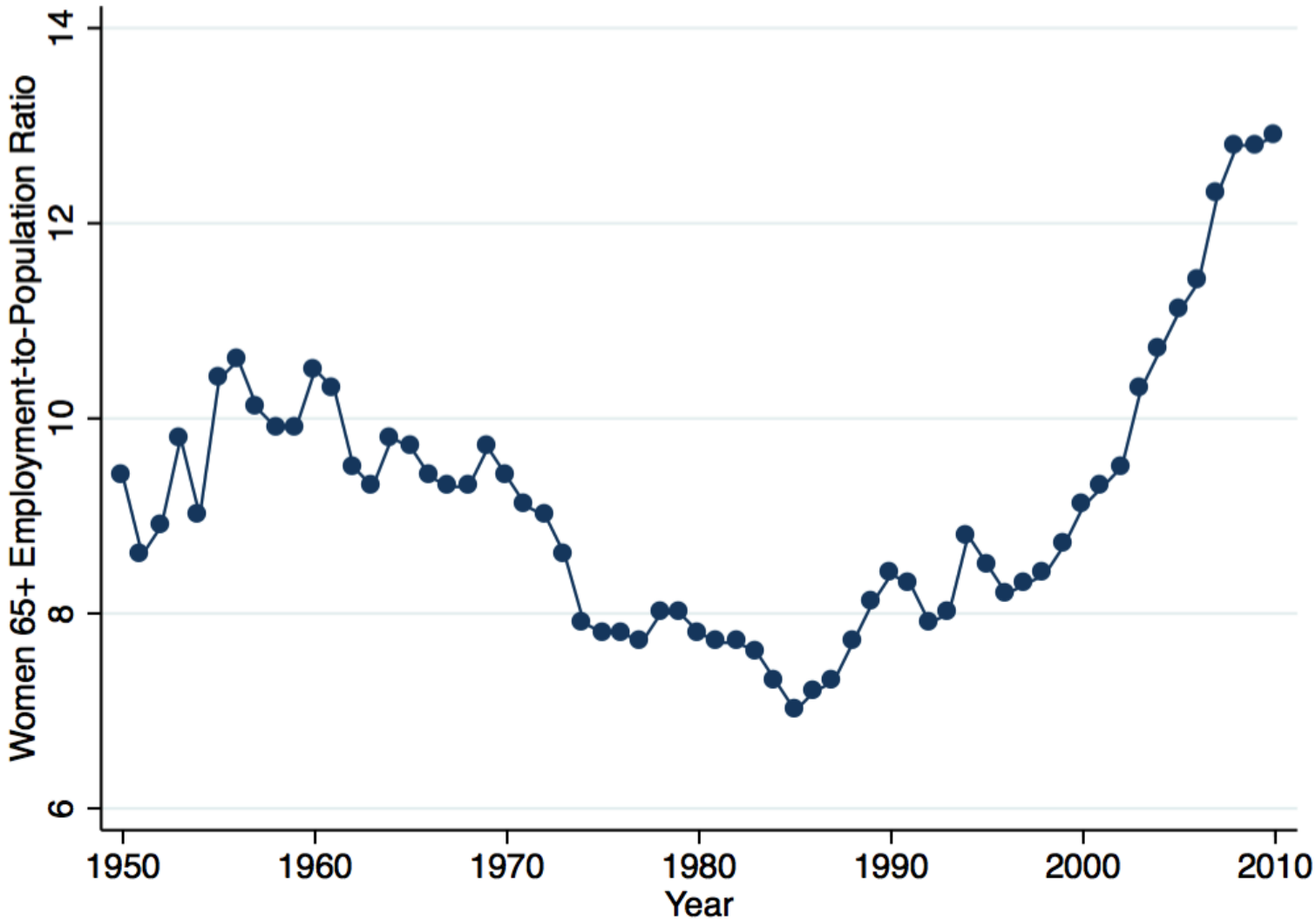
Data

Graphical and Statistical Evidence

Income vs. Substitution Effects

Conclusion

Women working longer: historical perspective on older women's employment rate



Motivation

- Understanding what changed in mid-1980s relevant for understanding secular trends in older women's employment rate
- Many factors likely contributed to reversal in trend
  - e.g. later cohorts typically had higher employment rates earlier in life, rise of DC plans in 1980s could have contributed, etc.

Motivation

- We suggest new, complementary factor: change in growth rate of Social Security benefits/replacement rates
  - Growth rate slowed substantially in mid-1980s largely due to 1977 Social Security amendments
  - OASI could be major determinant of elderly work decisions
  - Intended only as explanation for *change* in slope
    - Upward slope itself can only be explained through other factors
    - Prior to mid-1980s, slope could have been steeper absent rapid OASI benefit/replacement rate growth
- Time series correlations potentially counfounded by other factors changing over time
  - Influenced by many factors other than OASI
  - Turn to microdata

Background

- To assess the role that slowdown in growth of OASI benefits in the mid-1980s could have played, examine effects of 1977 Social Security Act amendments
- 1977 Social Security Act amendments created OASI "Notch"
  - "Notch" cut average lifetime discounted individual benefits by around $2,620 (roughly $115 per year) for women born in 1917 relative to those born in 1916 (including secondary/dual claimants)
  - Also greatly reduced net incentive to earn more for those in 1917 cohort
  - Often seen as one of cleanest settings for studying effects of pensions/OASI (Krueger and Pischke 1992; Engelhardt, Gruber, and Perry 2005; Snyder and Evans 2006)
  - Gelber, Isen and Song (in progress) examines effect of Notch in full population
- Note: "notch" does not refer to budget set

Key empirical strategy and findings

- Each birth cohort year may face its own OASI benefit schedule
- Those born on or after Jan. 2, 1917 faced sharply different OASI benefit structure than those born before this date
  - Allows comparison of otherwise similar women who have sharply different OASI benefits because they are born one day earlier or later
  - To our knowledge, represents largest discontinuous change in OASI benefits of its kind
- Use SSA data on full U.S. female population by exact date of birth
  - Around 13.35 million observations on 381,354 individuals in relevant DOB range
- RDD examines whether this causes discontinuity in earnings, probability of positive earnings
  - No discontinuity in outcomes in placebo samples
  - Robustness to controls, bandwidth, specification of running variable, using other samples to address potential threats to validity

Contribution

- 1977 amendments important to understand in their own right
  - One of major historical changes in OASI policy
  - Potential to cause substantial changes in work behavior that could help explain time series patterns
- Variation created by the Notch more generally useful in understanding determinants of earnings decisions of elderly, effects of pensions, and effects of OASI
  - Setting with large, clean variation for studying such issues and accurate, large administrative dataset

Contribution

- Large existing body of work on effects of OASI, other pensions, and other retirement income on elderly work in the U.S.
  - e.g. Diamond and Hausman 1984; Fields and Mitchell 1984; Burtless and Moffitt 1984; Hurd and Boskin 1984; Hausman and Wise 1985; Gustman and Steinmeier 1985, 1986; Burtless 1986; Sueyoshi 1989; Stock and Wise 1990; Hurd 1990; Berkovec and Stern 1991; Krueger and Pischke 1992; Lumsdaine and Wise 1994; Costa 1995, 2010; Ruhm 1995; Rust and Phelan 1997; Samwick 1998; Anderson, Gustman, and Steinmeier 1999; Coile and Gruber 2000; Coile and Gruber 2004; Coile and Gruber 2007; Kopczuck and Song 2008; Song and Manchester 2008; Liebman, Luttmer, and Seif 2009; Mastrobuoni 2009; Brown, Coile, and Weisbenner 2010; Blau and Goodstein 2010; Behagel and Blau 2012; Manoli and Weber 2015
- Work on recent increase in older men's labor force participation or labor force participation in general
  - Munnell, Cahill, and Jivan 2003; Schirle 2008; Blau and Goodstein 2010; Heiland and Li 2012
  - Trend for older men also reversed in mid-1980s

Contribution

- We find clear, very large income effects in a modern elderly pension program
  - Goldin (2006) emphasized the importance of income and substitution effects in understanding secular trends in women's labor force participation over the 20th century
  - Others have found evidence for income effects in Social Security (*e.g.* Coile and Gruber 2007), but usually of modest size
  - Costa (1995, 2010) finds very large income effects of Union Army pensions
  - Brown, Coile, and Weisbenner (2010) find large inheritance wealth effects on retirement
- Under further assumptions, find that substitution elasticities are at most small
  - Both income and substitution effects needed to predict effects of most policy changes

Contribution

- Krueger and Pischke (1992, "KP") started economics literature on Notch
  - Use CPS data and variation across birth years in OASI benefits and LFP
  - Using time series strategy, find no evidence that Notch change in OASI benefits affected elderly male labor force participation
  - Do not explicitly examine earnings impacts
- We use data on women and different identification strategy
  - Use data on full U.S. population
  - RDD strategy using data at date of birth level

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Social Security Notch: Policy Environment and Brief History

- OASI Early Retirement Age 62, Normal Retirement Age 65 for 1916 and 1917 cohorts
- OASI benefit based on Primary Insurance Amount (PIA)
- Prior to 1977 Social Security Amendments, PIA was increasing and progressive function of Average Monthly Wage (AMW)
- AMW calculated as mean yearly nominal lifetime earnings in highest-earning years
  - No indexation of earnings for inflation

Social Security Notch: Key Policy Changes

- 1972 Social Security Amendments indexed PIA to Consumer Price Index (CPI) ("double indexation"), adjusted in each year
- PIA increased with inflation due to two factors
  - 1) PIA depended on AMW; increased when inflation increased earnings
  - 2) PIA also depended on CPI; clearly increased from inflation
- Amid high inflation of late 1970s, double indexation led to sharp increases in OASI benefits over time
- 1977 Social Security Amendments addressed this fast benefit growth
  - Signed into law December 20, 1977
  - DOB discontinuity could not have been anticipated until earlier in 1977
    - Mostly exclude 1977 from analysis because expectations unclear
- 1922 and later cohorts: PIA depends on Average Indexed Monthly Earnings (AIME)
  - AIME in turn depends on wages indexed for wage growth

Social Security Notch: Key Policy Changes

- Under 1977 Amendments, 1917-1921 cohorts faced "transitional guarantee," which gave beneficiaries the greater of:
  - 1) Benefits based on AIME formula
  - 2) Benefits based on AMW, but modified:
    - (i) earnings in calendar years after age 61 not used to calculate AMW
    - (ii) after 1978, no CPI adjustment of benefits until calendar year of turning age 62
- Majority of 1917 cohort was covered under #2
  - Because of (i), income effect: mean yearly benefit amounts cut over $500 from 1916 cohort to 1917 cohort
  - Also because of (i), substitution effect: substantially smaller average incentive to earn more after age 61 in 1917 cohort than 1916 cohort
    - Earnings after 61 irrelevant to PIA under modified AMW calculation
    - Average 24 percent decrease in net returns to pre-tax, pre-transfer earnings in 1979 and after, in 1917 cohort relative to 1916 cohort
- Discuss inflation later

Public understanding of the policy

- Lots of publicity around Notch cuts in benefit levels
  - e.g. "Dear Abby" column
- Colorful stories, e.g. this Congressional testimony (KP 1992): "Two sisters, Edith and Audrey, started work at the same book bindery in Bell Gardens, CA on the same day in October 1957. Audrey was...born in March 1916...Edith...was born in June 1917. The two worked together at similar pay for 25 years...The total lifetime earnings of the pair was almost identical, differing only by about four percent (in favor of the younger Edith)...Edith (born in 1917) received a $512.60 monthly award or $111.80 per month less than Audrey (born in 1916), who received a higher benefit of $624.40 per month."
- Even absent publicity, many aware of size of their own income or OASI income

Understanding the policy changes

- Substitution incentive complex to understand and may not have been understood by many beneficiaries (Blinder, Gordon, and Wise 1980)
  - Requires understanding linkage between current earnings and future OASI benefits
- Lack of transparency endemic to many incentives created by OASI and other aspects of pensions (*e.g.* Lusardi and Mitchell 2007; Liebman and Luttmer 2015)
  - But substitution incentives transparent to some in certain contexts
    - *e.g.* Earnings Test where moderate substitution elasticities are evident
    - Liebman, Luttmer, and Seif (2009) find that marginal OASI benefits influence retirement

Other policies

- 1978 Age Discrimination in Employment Act (ADEA) amendment: discrimination (including mandatory retirement) prohibited up to age 65 (pre-1978), up to 70 (1978 and after)
  - No discontinuous effect around the 1916/1917 cohort boundary
  - 1916 and 1917 cohorts ages 65 to 70 in 1982 to 1987, all affected
- Mandatory schooling policies could imply that first quarter births discontinuously have lower schooling on average than fourth quarter births (Angrist and Krueger 1991)
  - Should push toward *lower* earnings in 1917 cohort, working against our later finding of large income effects
  - Would *strengthen* our conclusion that the lower bound on income effects is large
  - Three placebo tests to rule out discontinuity in other contexts

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Data

- Social Security administrative data on universe of U.S. earnings records
- Complete yearly FICA earnings history beginning in 1951
  - Earnings observed in each calendar year
  - Exclude self-employment earnings (subject to manipulation)
- Key variables: earnings, date of birth (DOB), when claiming began, gender, OASI benefits
  - OASI benefits calculated using OASI rules
  - More aggregate data (*e.g.* quarterly) could be confounded by smooth trends in outcomes over DOBs (Buckles and Hungerman 2013)
- Key outcomes: earnings, participation
  - Main focus on earnings
    - Can be relevant to effects on government budget (which we estimate), welfare (Chetty 2009)
  - "Participation": probability of positive earnings
    - Different than labor force participation in CPS

Social Security Administrative Data

- Dropped: pre-1977 never have positive earnings, go on DI, or die
- Pool claimants and non-claimants (claiming potentially endogenous)
- After death, observations set to missing (except yearly graphs)
- Measure pre-tax benefits and earnings
  - SS benefits generally not taxed here (and can't measure tax rate)
  - Pre-tax benefits answer policy-relevant question
- All dollar amounts in 2012 terms
- Where relevant, discount earnings and benefits at 3 percent real rate
  - Average real Treasury rate over 1979-2012 (rounded to nearest percent)
  - Estimates in same range between 1 and 5 percent
  - Discounted version relevant for effects on government budget
- Days of birth can be misreported in the data
  - More born on first of month, January 2 birthdays have different incentives to claim (Kopczuk and Song 2008)
  - Results robust to removing January 1 and/or surrounding few days
  - Look at placebo years and samples

Social Security Administrative Data

- Focus on those born within 100 days of cohort boundary
  - Cattaneo, Calonico, and Titiunik (2014) bandwidth: 62
  - Similar results with Imbens-Kalyanaraman bandwidth
- Show seven 10-day bins around 1916/1917 cohort boundary
  - Visual patterns robust to other bin sizes

Local Effect

- Women typically claim as secondary or dual in 1916-1917 cohorts
  - 61% of women claim as secondary or dual in 1916 cohort
  - When own OASI benefit is less than 50% of spouse's, claim as secondary (own benefit zero)/dual (own benefit>0)
  - Secondary/dual wives' total benefits determined by husband's DOB
  - For these women, no discontinuity in woman's benefit when moving from woman's DOB in 1916 to woman's DOB in 1917
- Important: effect will be local to women claiming as primary beneficiaries
  - Primarily women with relatively high earnings relative to their husbands
    - PIA progressive function of lifetime earnings–>on average primary married female beneficiaries still had lifetime earnings well under half of their husbands'
  - Also includes always-unmarried women

Issues our strategy does not attempt to address

- We investigate how a woman's benefit affects *her* earnings
  - Cannot assess how one spouse's benefit affects other spouse's earnings
  - Only observe linked spouses when one spouse claims on other spouse's record
  - Claiming on spouse's record endogenous
    - Higher husband benefit increases probability that wife claims on husband's record, and vice versa
- Available data underpowered to estimate effect on consumption or savings
  - In theoretical framework we specify (lifecycle model or adding myopia), do not need consumption or asset data to estimate substitution effect or income effect
- Cannot investigate GE effects with our RDD
  - Interpret results as movement along earnings (labor) supply curve
- Estimate responses net of frictions

Summary statistics: mean (SD) of means by DOB

	Mean (SD)
Discounted earnings, 1978 to 2012 ($)	53,131.83 (2,372.61)
Fraction of years with >0 earnings, 1978 to 2012	0.097 (0.0033)
Lifetime discounted OASI benefits	84,851.52 (1,548.97)
Individuals	381,354
Individuals per DOB	1,906.77 (258.85)
Observations	13,347,390

100% sample, born 1916 or 1917 within 100 days of boundary, 1978 to 2012, 2012 dollars.

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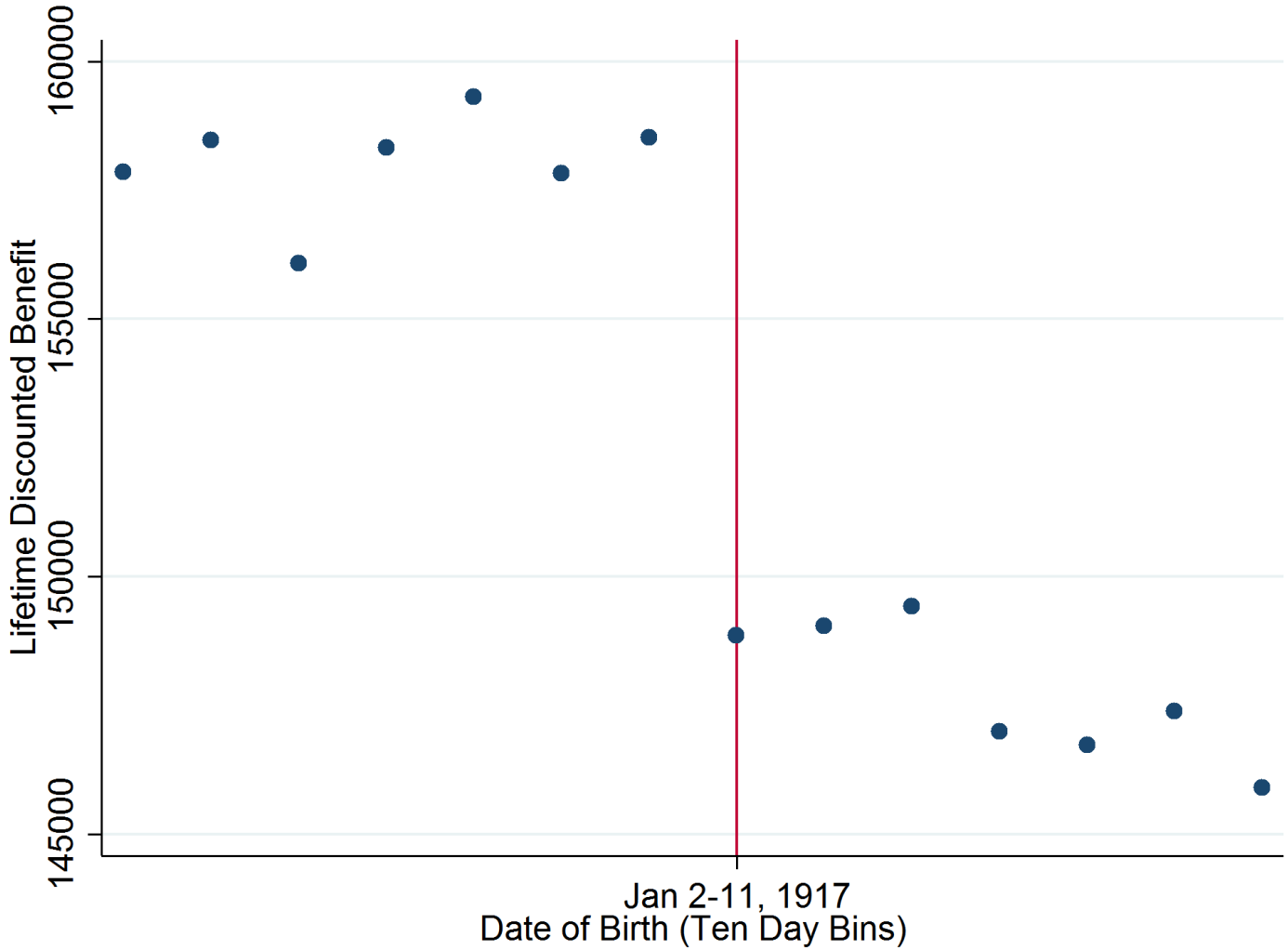
Data

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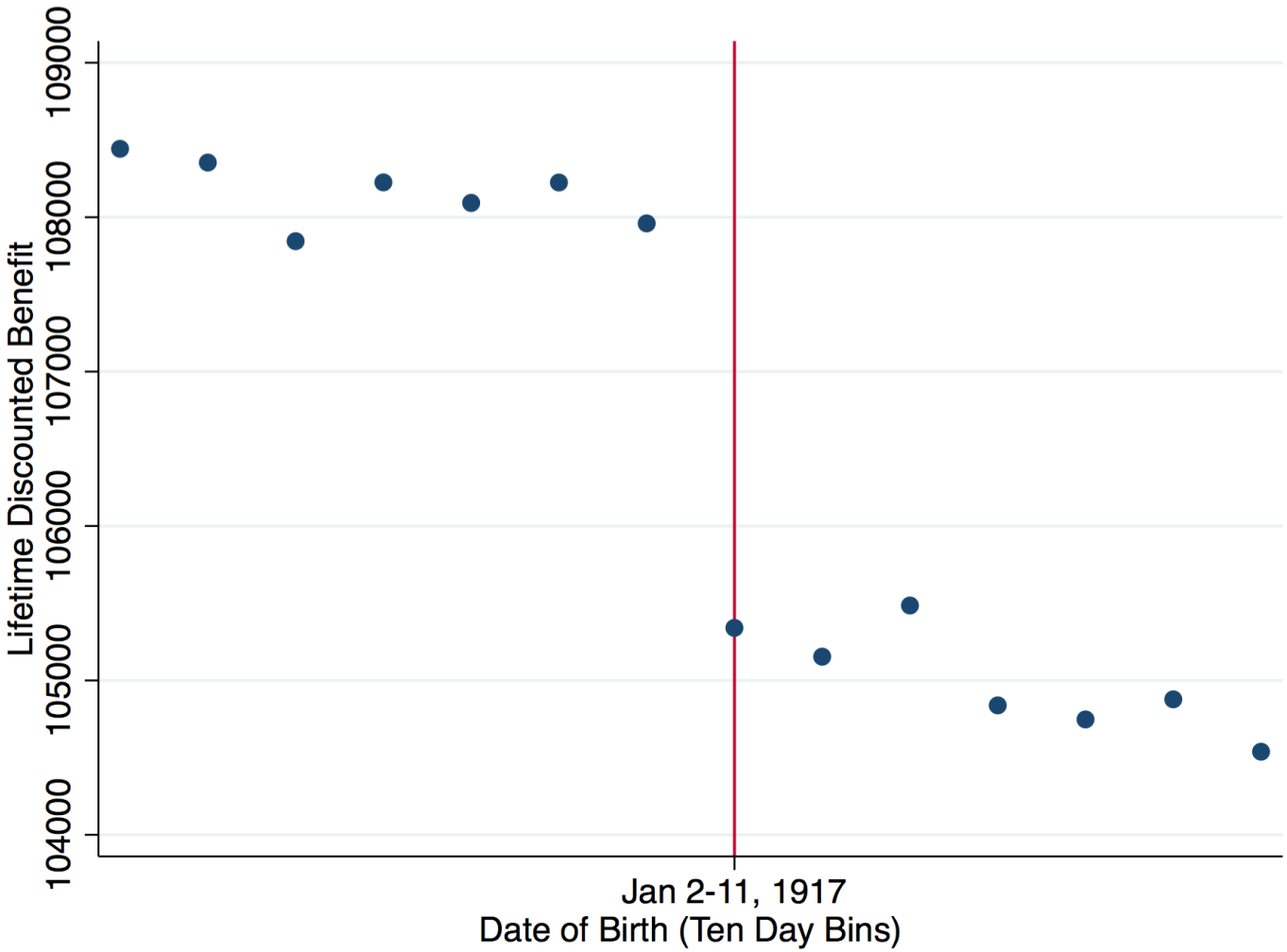
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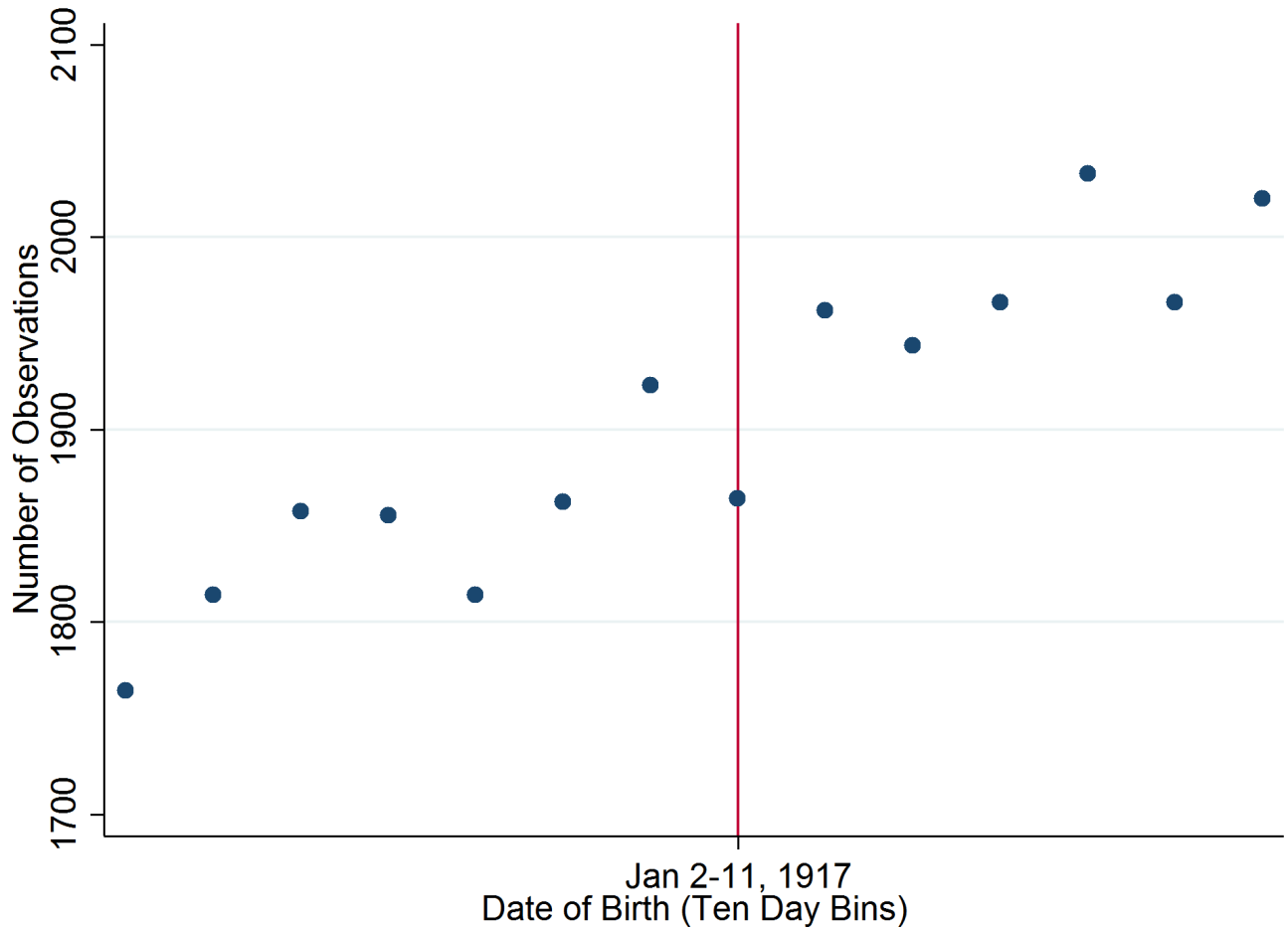
Mean Cumulative Discounted OASI Benefits 1978 to 2012 (Ages 61 to 95), by DOB Bin, Men



Mean Cumulative Discounted OASI Benefits 1978 to 2012 (Ages 61 to 95), by DOB Bin, Women



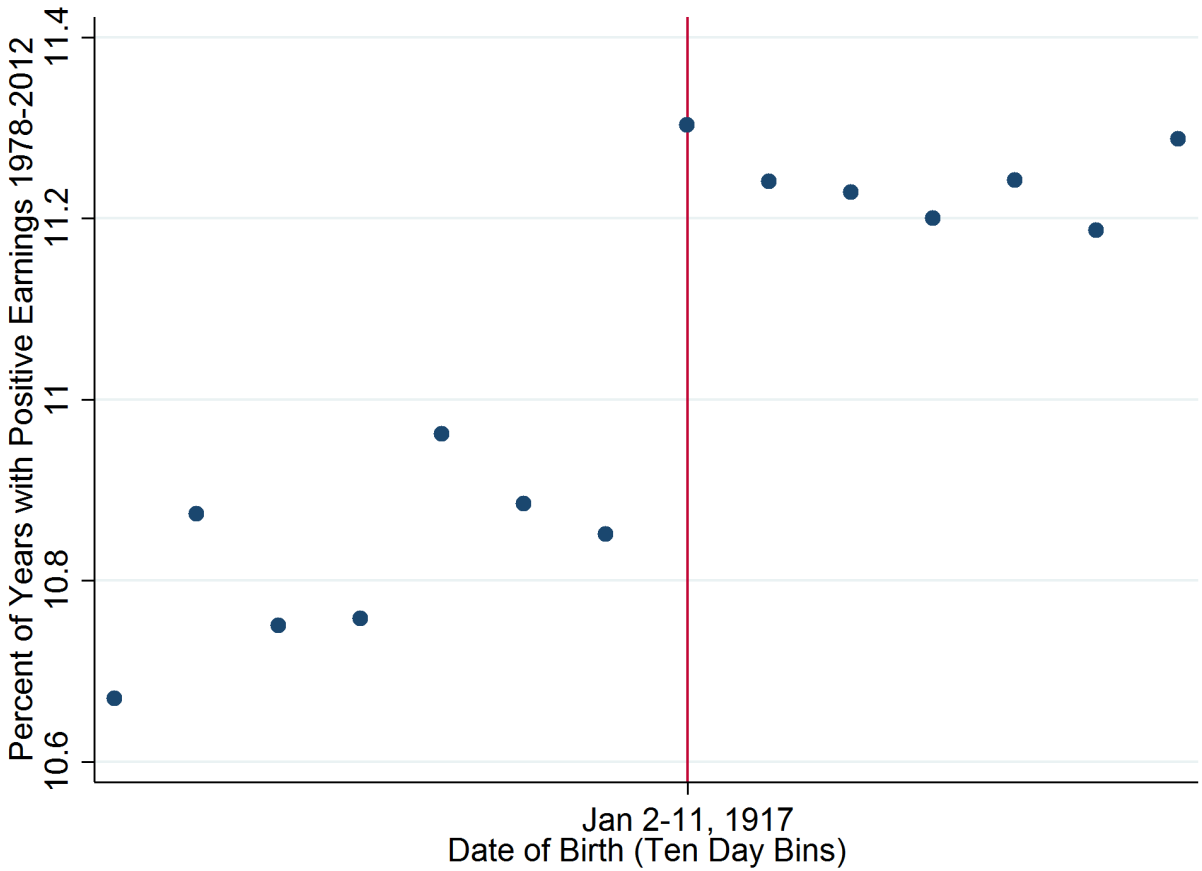
Mean number of daily observations, by DOB bin, women



Other predetermined characteristics

- Insignificant discontinuity in number of observations
  - Regress # obs on 1917 cohort dummy, linear DOB trend, and interaction of trend with 1917 dummy (p=0.40)
- Other predetermined characteristics also show insignificant discontinuity:
  - Fraction white
  - Fraction missing birthplace
  - Fraction male
  - Fraction foreign born

Extensive margin: mean by DOB Bin, percent of years with positive earnings from 1978 to 2012, full population



Initial regression specification

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

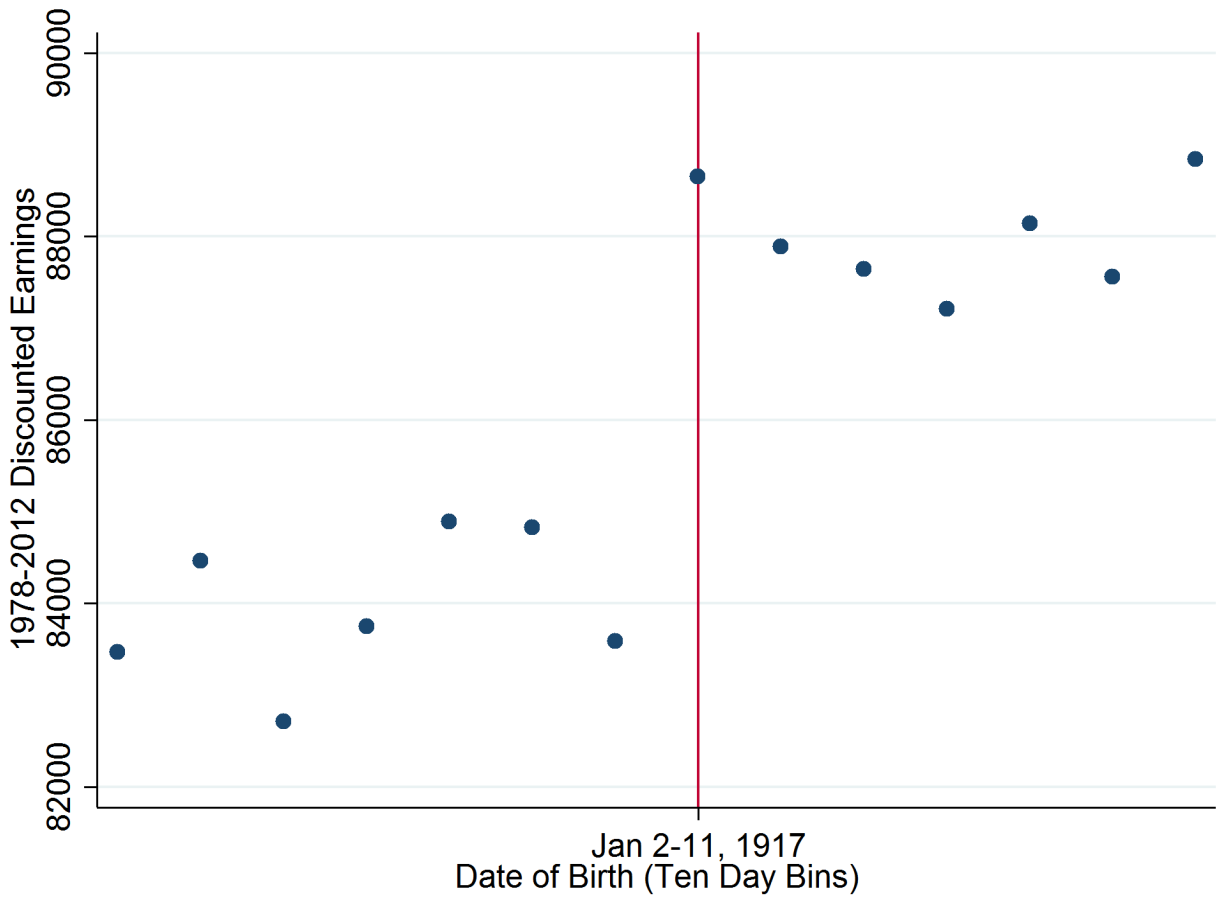
- i indexes DOB; L_i mean fraction of positive-earning years 1978 to 2012; D_i dummy for being born on or after Jan. 2, 1917; DOB_i linear trend in DOB; $(D * DOB)_i$ interaction between D and DOB
  - Regression weighted by number of observations on each DOB
- Documents magnitude and statistical significance of jump in earnings
  - Not intended to estimate a parameter relevant to individuals' decisions
- Use means at DOB level to estimate "conservative" standard errors
  - Discuss later choice to analyze at DOB (rather than individual) level
  - Use robust standard errors
- Many other factors could have affected earnings
  - RDD assumes they affect earnings smoothly through cohort boundary
- Following figure shows β_1 and confidence interval, for each bandwidth from 20 to 100, measured in days

Participation coefficient and confidence interval by bandwidth, full population

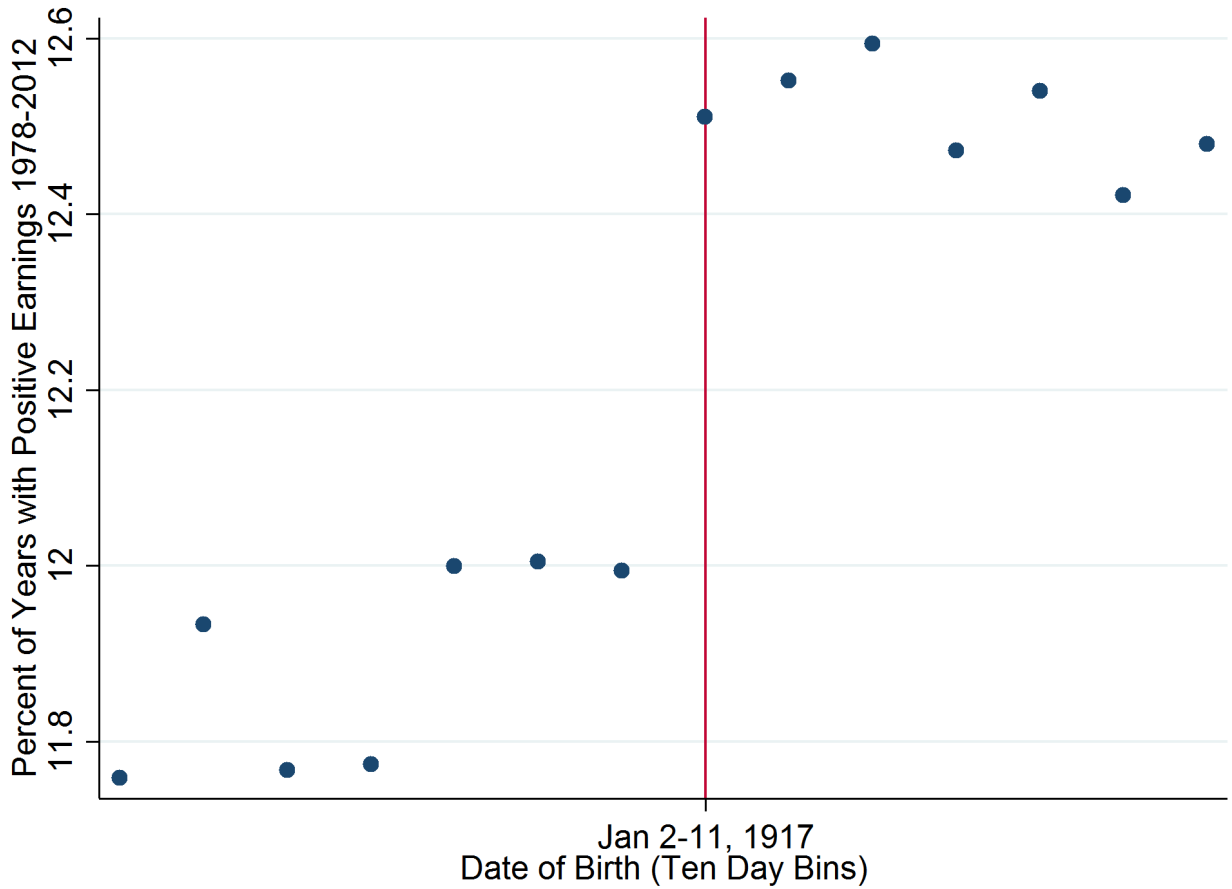


- Similar with controls for demographics and/or quadratic polynomial

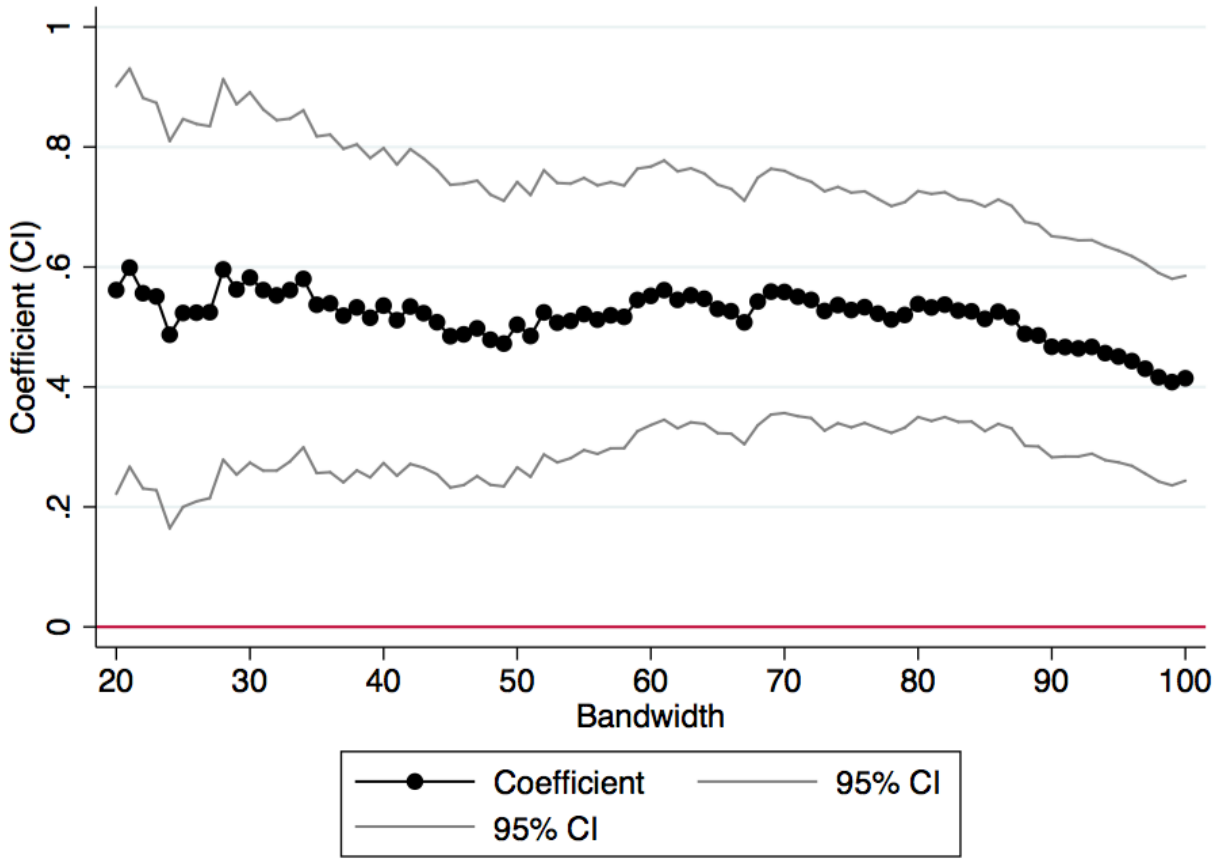
Mean Discounted Earnings 1978 to 2012 (Ages 61 to 95), by DOB Bin, Full Population



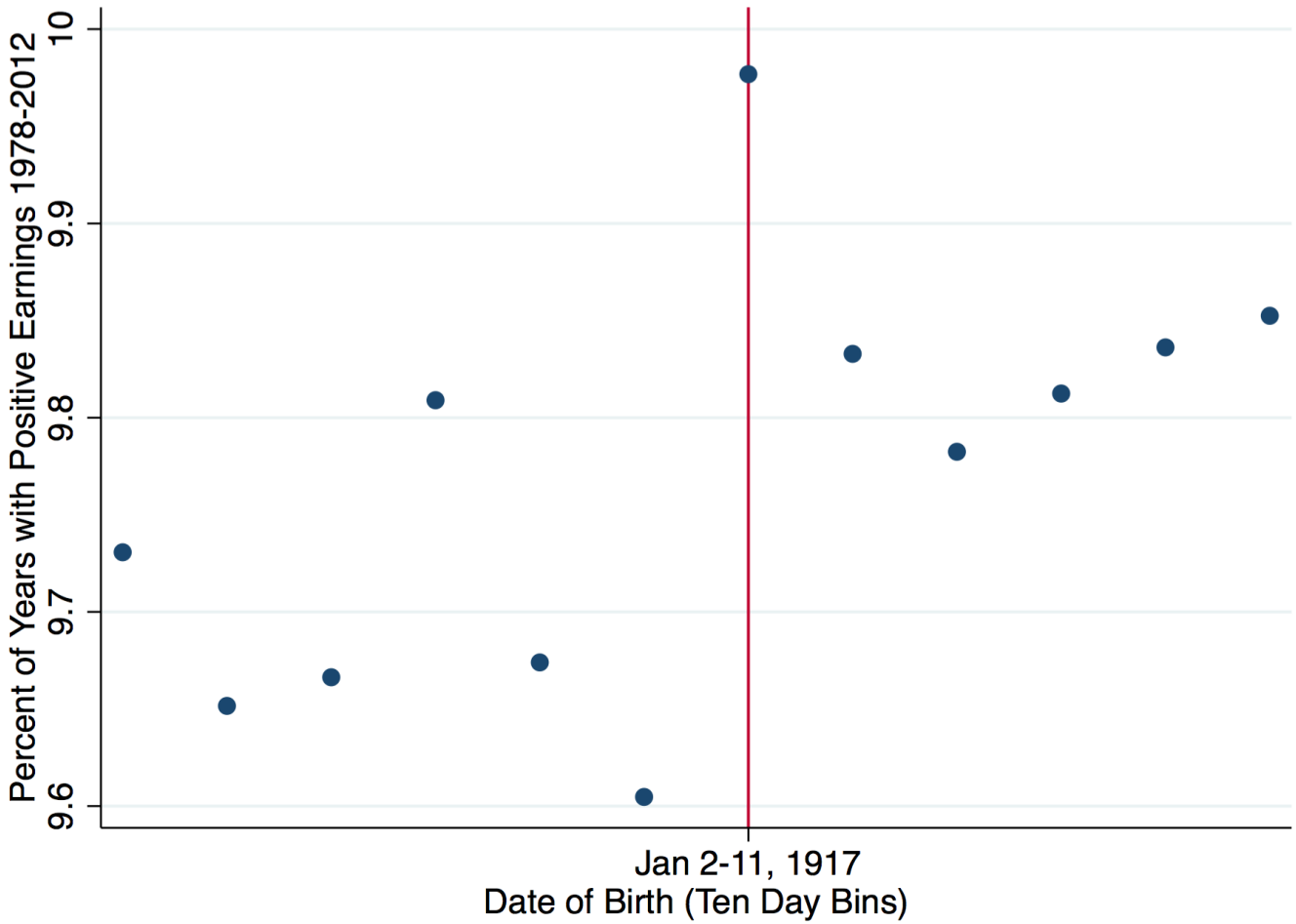
Extensive margin: mean by DOB Bin, percent of years with positive earnings from 1978 to 2012, men



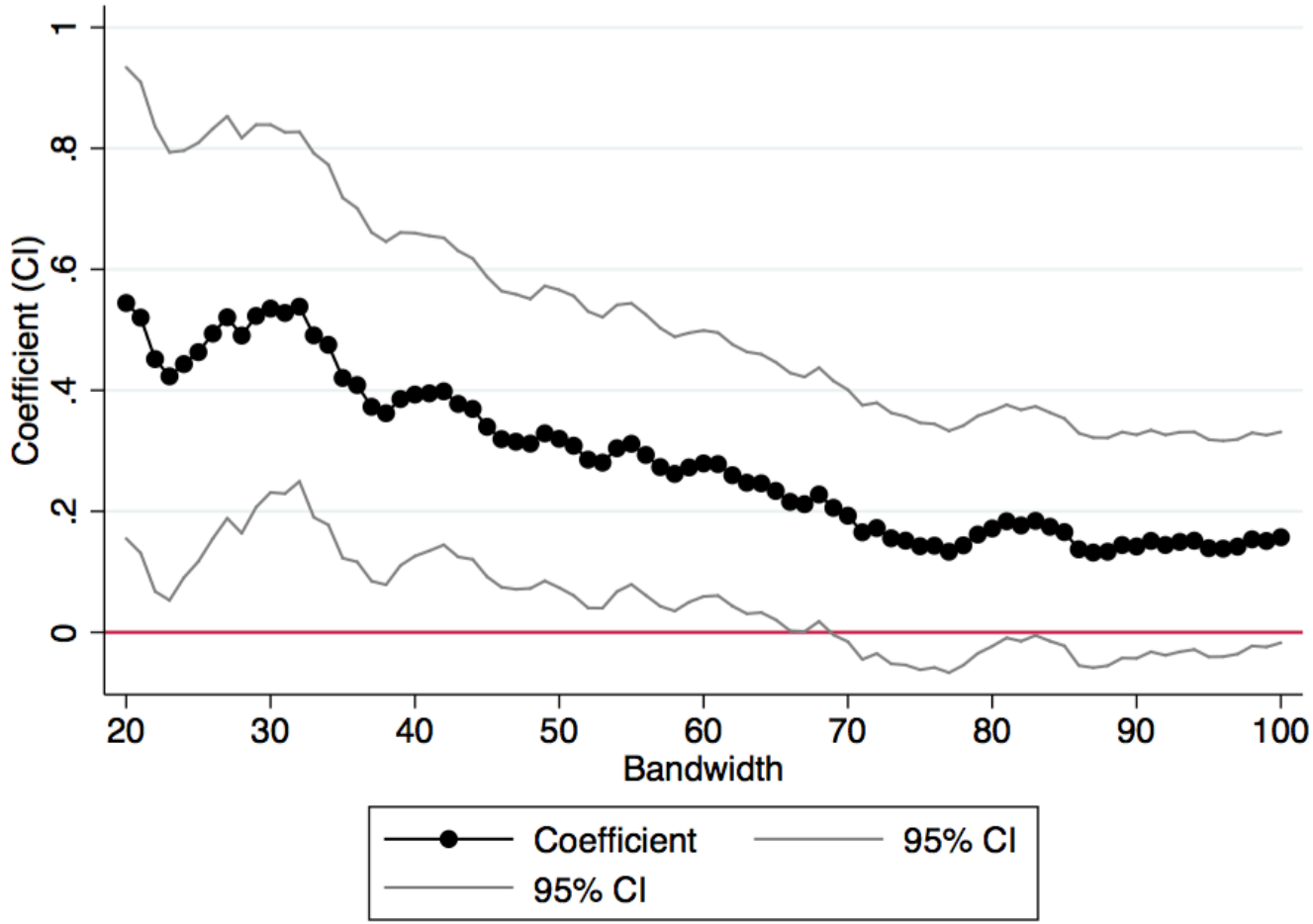
Extensive margin: mean by DOB Bin, percent of years with positive earnings from 1978 to 2012, men



Extensive margin: mean by DOB Bin, percent of years with positive earnings from 1978 to 2012, women

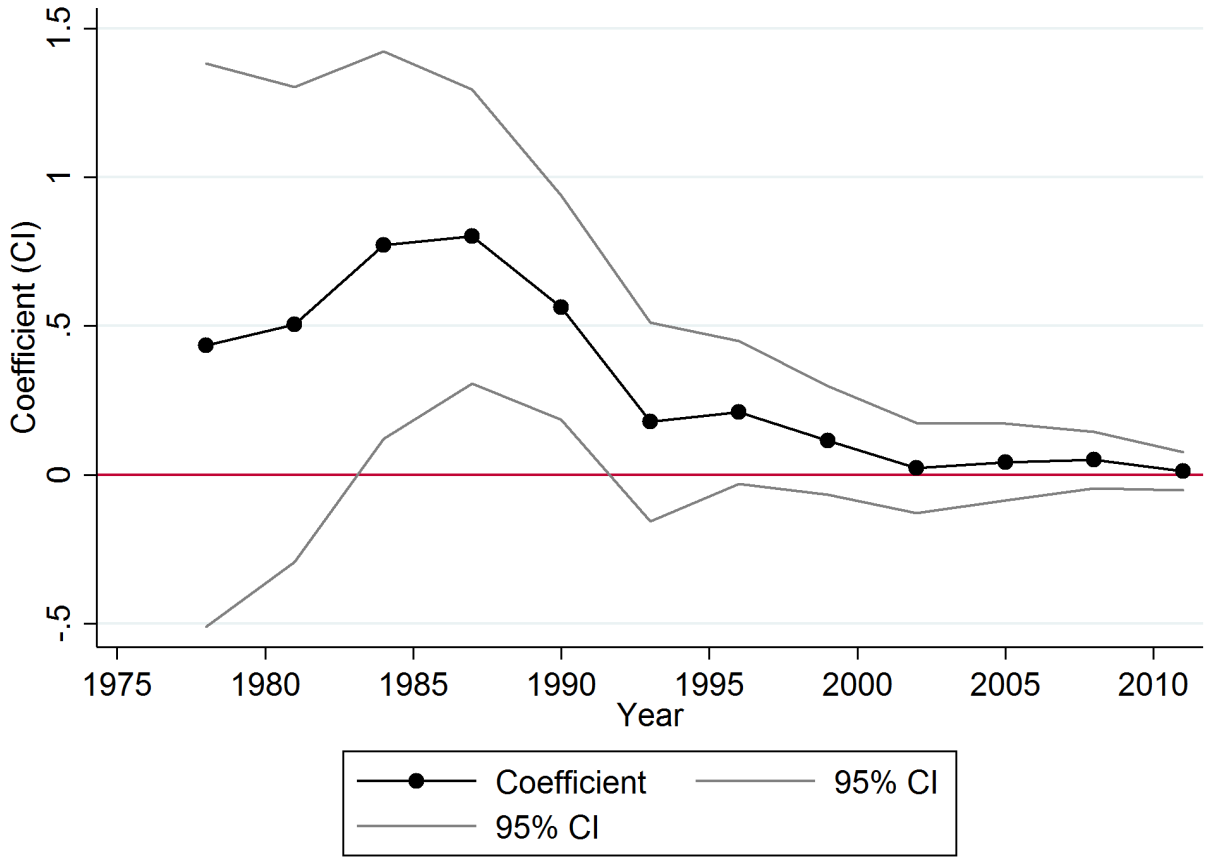


Coefficient and confidence interval by bandwidth



- Similar with controls for demographics and/or quadratic polynomial

Percentage point effect on mean participation rate in each year, by 3-year time period, women



- Time patterns similar with: lived until at least 75 or at least 80

Effects on placebo outcomes

	(1) 1975-76 mean participation	(2) 1922/3, 1978-2012 mean participation
β_1	0.17	0.15
	(0.43)	(0.49)

- Using 100% sample from 1999 to 2013 for each boundary from 1923/4 to 1935/36 (excluding those where the Delayed Retirement Credit changed), estimate significant effects in main sample but no significant effect in placebos
- Unable to use 100% sample prior to 1916

Other results

- No conclusive evidence on age of claiming OASI
  - See Kopczuk and Song (2008)
- 1917/1918, 1918/1919, 1919/1920, 1920/1921: underpowered
- Similar results when exclude birthdays from December 30 to January 4 (or similar dates)

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Mechanisms

- Next, we analyze income and substitution effects
- Results so far show income effect "outweighs" substitution effect
  - Substitution effect pushes toward decrease in earnings, whereas income effect pushes toward increase in earnings at cohort boundary
- Discontinuity should reflect *lower bound* on income effect in broad range of lifecycle models

Regression estimates part one: lower bound on income effect

- Can estimate 2SLS regression:

$$B_i = \gamma_1 D_i + \gamma_2 DOB_i + \gamma_3 (D * DOB)_i + \varepsilon_i$$

$$L_i = \alpha_1 B_i + \alpha_2 DOB_i + \alpha_3 (D * DOB)_i + \eta_i$$

- B_i lifetime discounted benefits, L_i participation
- Estimates LATE for those at cohort boundary
- Since theory tells us substitution effect weakly positive, α_1 will be *lower bound* on income effect
  - α_1 estimates income effect, assuming substitution effect is zero
  - Slutsky matrix negative semidefinite–>α_1 lower bound on income effect
- Central finding of the paper: lower bound large–>income effects large

2SLS regression estimates

	Fraction of Years with Earnings>0, 1978-2012
α_1	-1.16
	(0.59)**

- Participation coefficient scaled to represent yearly percentage point effect on employment of $10,000 increase in lifetime discounted OASI benefits
  - Similar results with log odds specification
- Similar results with quadratic, demographic controls
- With higher (lower) discount rate, crowdout estimate modestly larger (smaller)

Effect of actual vs. simulated benefits

- Actual benefits could be endogenous
  - But very little endogeneity in 1917 cohort since earnings did not affect AMW
    - Possible channels: claiming, mortality, Earnings Test
- Also estimated effect of "simulated" benefit change
  - Simulated post-1977 earnings for both 1916 and 1917 cohort, using relationship in 1916 cohort between pre-1977 and post-1977 earnings
  - Used simulated earnings to calculate simulated benefit
- Effect of actual benefit *vs.* effect of simulated benefit
  - Different objects
  - Both of interest
- Effect of simulated benefit very similar to effect of actual benefit

Effects by pre-1977 average earnings

Above-median	-0.39
	(0.15)***
Below-median	-0.96
	(2.55)

Lifecycle framework: substitution elasticity

- Three periods of interest:
  - Calendar years before 1977 reform is anticipated (may include 1977)
  - Calendar year 1978
    - Occurs after the 1977 reform is (anticipated and) passed but before individuals in 1917 cohort reach age 62 in 1979
    - Earnings should adjust immediately in 1978, because of anticipation of future policy changes
  - Calendar year 1979 and after, when substitution effect should also operate for those born in 1917
- Can assume that exogenous factors constant on average in adjacent calendar years

Substitution incentive by calendar year and birth cohort for those on AMW

	1916 cohort	1917 cohort
1978	$\mu_{78,i}$	$\mu_{78,i}$
1979	$\mu_{79,i}$	0

- Table considers *identical* individual (with identical earnings history) who is born in 1916 vs. 1917
- Llifetime income and OASI benefits continuous across 1916/1917 cohort boundary in both 1978 and 1979
- Note: break between 1916 and 1917 is discontinuous, so variation is difference-in-discontinuities
  - Not differences-in-differences

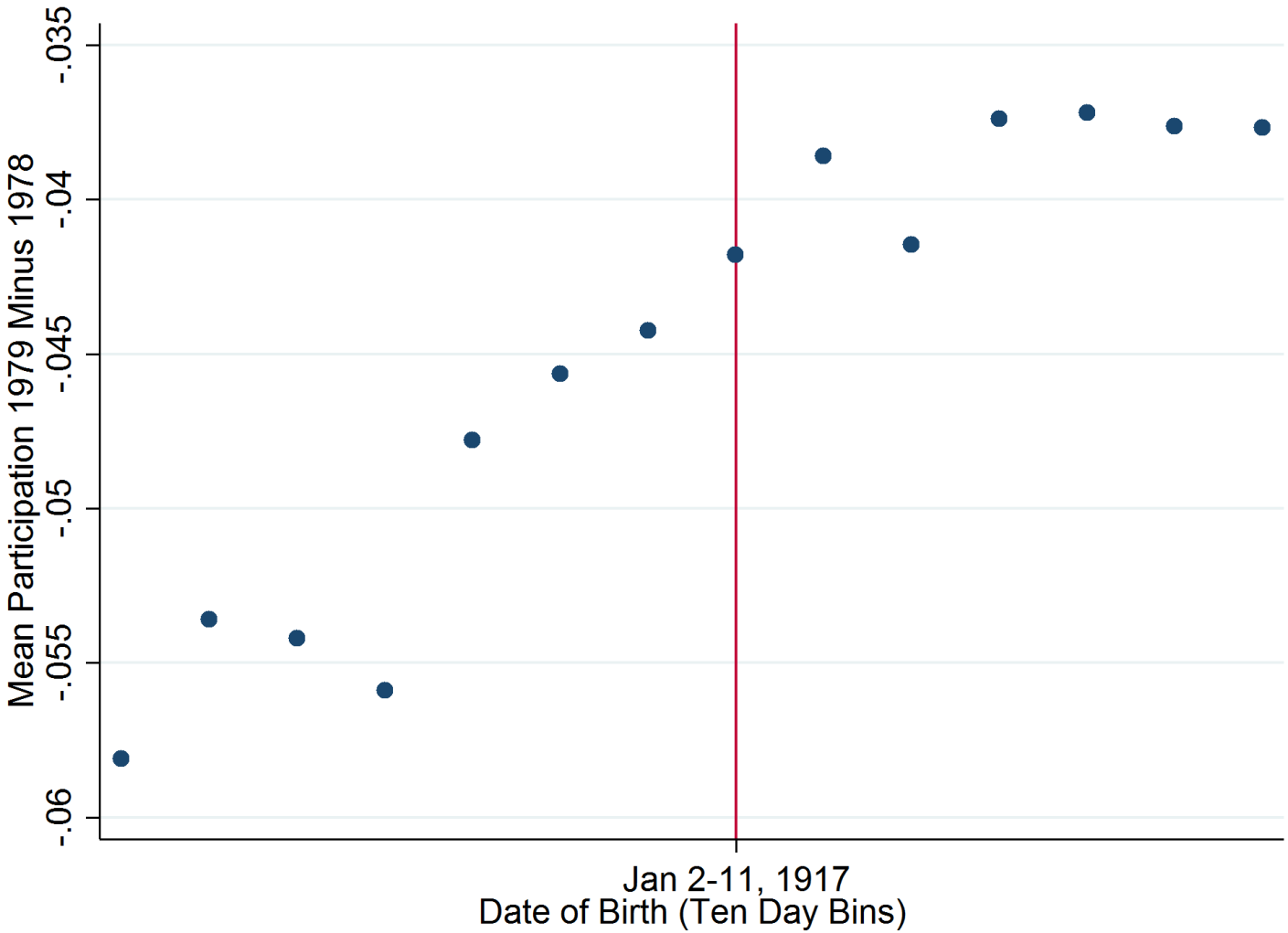
Searching for substitution effect

- Thus, for each DOB i near the 1916/1917 cohort boundary, we calculate:

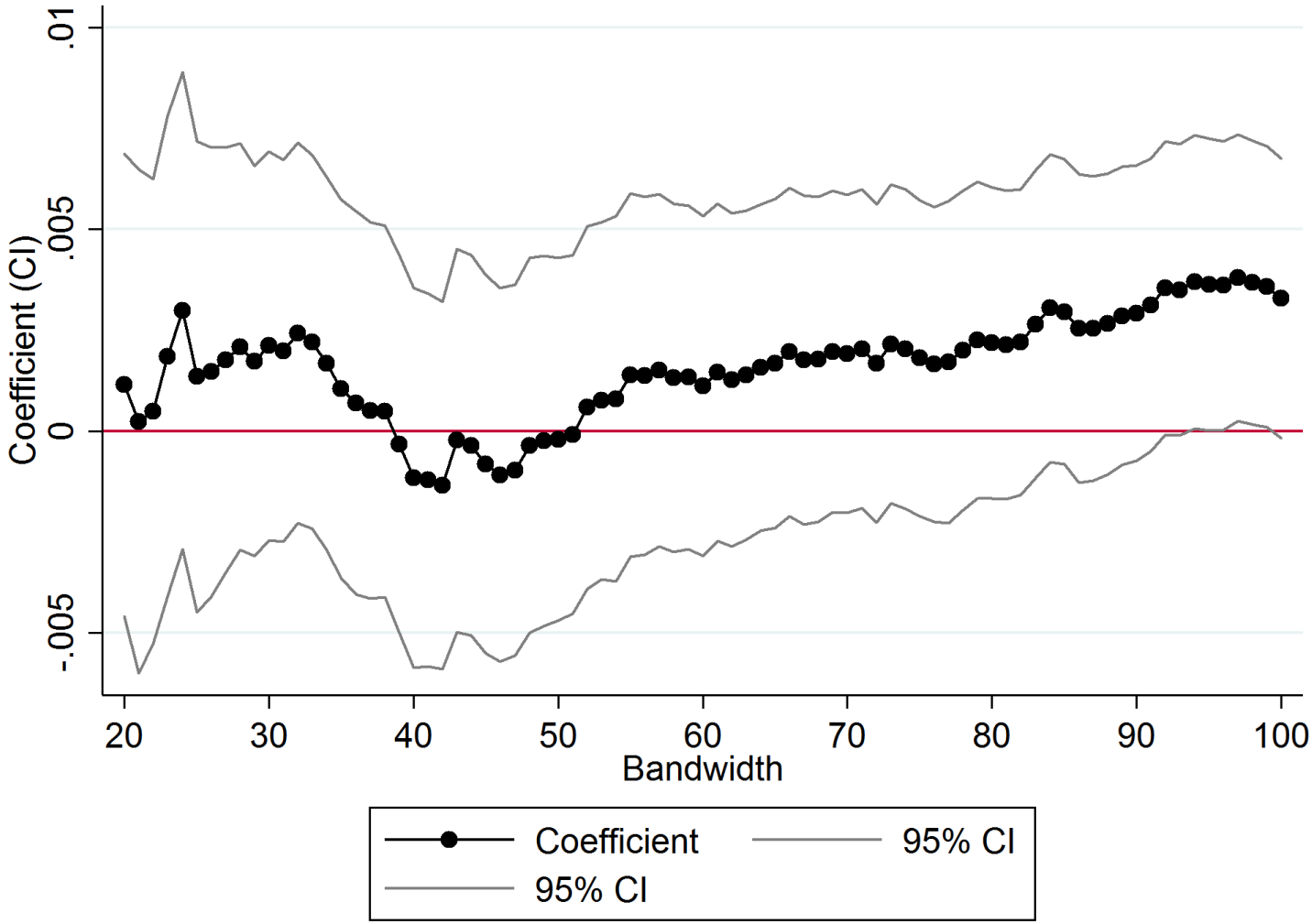
$$\Delta L_i = L_{i,1979} - L_{i,1978}$$

- Here L_{it} represents mean earnings on the DOB in year t
- The following graph shows ΔL_i by DOB bin
- With non-zero substitution effect, would expect a downward jump in ΔL_i at the cohort boundary
  - With even modest substitution elasticity, should be very large (because of 24% change in substitution incentive)
  - Example: with substitution elasticity of 0.25, should expect downward discontinuity in ΔL_i of 0.027.

1979 Mean Participation Minus 1978 Mean Participation



1979 Mean Participation Minus 1978 Mean Participation



Substitution Elasticity

	Participation
β_1	-0.046
	(0.13)

- Participation elasticity calculated with respect to net return to earning actual (or simulated) earnings rather than 0 in 1979 (and 1978)
- No significant effects (or differences) in high vs. low earnings groups

Substitution Elasticity

- Two presumptions appear consistent consistent with data
- First, strategy assumes individuals could react within a year
  - With very large change in substitution incentives, if there is response at all, might expect a measurable portion of it to appear in first year
  - Consistent with fast reaction in Gelber, Jones, and Sacks (2013)
- Second, unanticipated inflation affected size of income effect
  - Could affect change in earnings from 1978 to 1979
  - If this is major driver of results, would expect to see discontinuity arise in $L_{i,1979} - L_{i,1978}$ at 1917/1918 cohort boundary
    - At 1917/1918 boundary, no change in substitution incentives from 1978 to 1979
    - No evidence of discontinuity in $L_{i,1979} - L_{i,1978}$ at 1917/1918 boundary

Results with liquidity effects or myopia

- Results in progress

Rationale for empirical strategy

- Alternative empirical strategies:
  - (1) Estimating at individual level, and/or...
  - (2) Estimating income and substitution effects in same regression
  - (3) Estimating using data from all ages
- Compare earnings discontinuity with discontinuity in incentives
- Re #1/2: could generate results with not-necessarily-useful weighting if response heterogeneous across groups and correlated with size of substitution or income incentive
  - Standard issue in DD: weighted average of responses across groups
    - Example: those with largest cut in OASI payments tend to have high earnings near retirement age, who could be more (less) responsive
    - Example: those with largest substitution incentives tend to have low lifetime income (pre-1977), who could be more or less responsive
  - Regressions at DOB-mean level weight individuals to estimate population average effect on earnings for those at cohort boundary
    - Relevant e.g. to government revenue implications for those at boundary
- Re #3: *Same group across adjacent years* most comparable

Using individual-level data

- Use discontinuity in benefit formula to drive regression estimates
- Step 1: Run regressions in the 1916 cohort to obtain fitted values $\hat{E}_{it}$ in both 1916 and 1917:

$$E_{it} = \beta_0 + \beta_1 E_{i1975} + \beta_2 E_{i1975}^2 + \beta_3 E_{i1976} + \beta_4 E_{i1976}^2 + \varepsilon_{it}$$

- Step 2: Calculate simulated benefits $B_{it}^{sim,T}$ by applying cohort T benefit rules to $\hat{E}_{it}$
- Step 3: For 1917 cohort, change in yearly benefit amount due to policy variation is: $\Delta B_{it}^{sim} = B_{it}^{sim,1917} - B_{it}^{sim,1916}$
  - ΔB_{it}^{act} is the difference under the actual earnings history
- Step 4: 2SLS, cluster by DOB

$$\Delta B_{it}^{act} = \alpha_1 \Delta B_{it}^{sim} + \alpha_2 DOB_i + \alpha_3 (D * DOB)_i + \Gamma_t \alpha_4 + \varepsilon_i$$

$$E_{it} = \beta_1 \Delta B_{it}^{act} + \beta_2 DOB_i + \beta_3 (D * DOB)_i + \Gamma_t \beta_4 + \eta_i$$

Participation estimates using individual-level data

- Results in progress

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Data

Graphical and Statistical Evidence

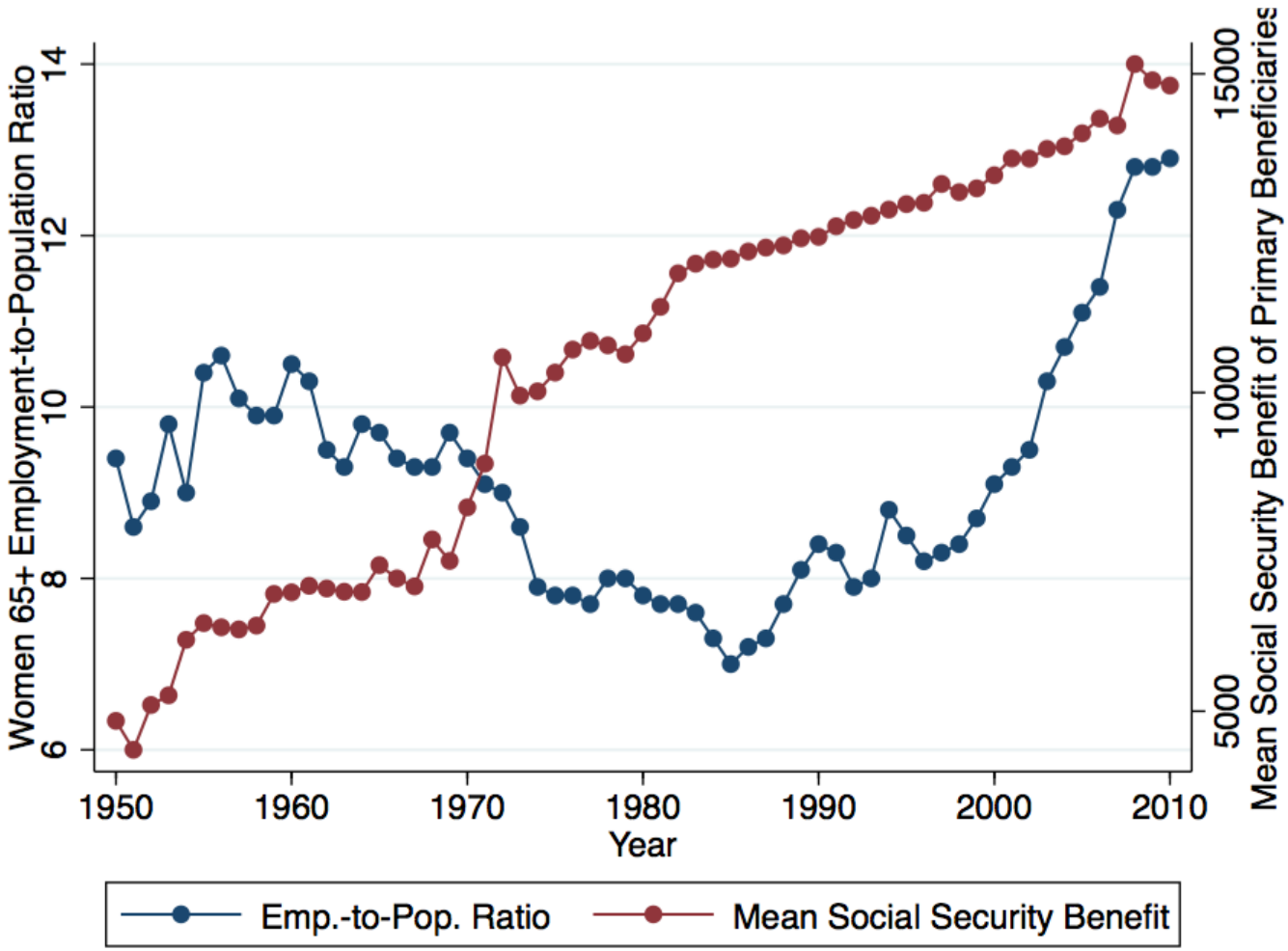
Income vs. Substitution Effects

Conclusion

Conclusion

- Cut in OASI benefits for the 1917 cohort led to large increase in women's earnings and participation
- Income effect very large
  - Lower bound point estimate: $10,000 increase in discounted lifetime benefits leads to 1.16 percentage point decrease in yearly participation
- Under further assumptions, no evidence for substitution effect

Historical perspective: OASI benefits and women's elderly employment rate



Implications for time series

- Change in 1985 in slope of older women's participation rate (as function of year): 0.255
  - Compare 1973-1984 to 1985-2010
  - Note: results vary moderately depending on choice of years
    - These calculations are illustrative and reflect typical results
- Change in slope of OASI benefits: -43.75
  - Corresponds to $670 in lifetime benefits
- Our point estimates suggest change in slope of mean OASI benefits from 1950 to 1985 should have led to change in slope of 0.078
- Accounts for 30 percent of actual decline
- Note stabilization in elderly (65+) employment-to-population ratio occurs in the mid-1980s
  - Does not match perfectly with year of 1977 amendments
  - But 1917 cohort reaches age 65 (so is included in CPS 65+ data) only in 1982
  - Timing of stabilization matches very well with years when RDD strategy shows big effect on participation

Generalizability

- Notch episode of great interest on its own
- Goldin (2006): income effects on women's labor supply more important earlier in the 20th century

Further caveats to generalizability

- Results do not imply that substitution effects also small in all other contexts
  - Substitution effects complex to understand in this context
  - Could be larger in other contexts, *e.g.* when easier to understand
- Results do not imply income effects very large in all earnings or labor supply contexts
  - e.g. lifecycle model suggests effects of unexpected change near retirement age likely larger than effects of unexpected change earlier in life