Feeling Squeezed? Impact of Social Security Benefit Cuts on Labor Market Outcomes of the Elderly*

Christopher John Cruz†

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Abstract

This paper examines how a negative retirement wealth shock affects the labor market outcomes of older workers at different stages of the lifecycle. Exploiting the variation from the nonlinear design of the 1983 Social Security benefit reduction, I find that affected cohorts increased their labor supply, both at the extensive and intensive margins, but only when they were very close to the normal retirement age. Evidence also shows that the wealth shock had effects beyond the normal working years of the elderly and that the lower-educated workers appear to be hit harder.

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†Correspondence: Department of Economics, Seidman College of Business, Grand Valley State University, 50 Front Ave SW, Grand Rapids, MI 49504 USA; cruzch@gvsu.edu; (616) 331-7122

1 Introduction

There is a general consensus among economists and policymakers that reforms are needed to improve the financial viability of the Social Security program. Reform proposals tend to be characterized by a reduction in the prospective Social Security wealth of beneficiaries and/or an increase in retirement age. Workers can respond along different dimensions and the extent of the effects can vary widely, some of which may be unintended. As noted by (Krueger & Pischke, 1992), standard labor-supply analysis would predict that the negative wealth shock will increase labor force participation (assuming leisure is a normal good). However, the empirical question on when workers actually respond to a negative retirement wealth shock remains unsettled. There is also considerable uncertainty on the other margins where workers could have responded and the welfare implications of the reforms.

By exploiting the variation from the nonlinear design of the 1983 Social Security reforms, I examine how a large, potentially unanticipated wealth shock affects the labor supply of elderly workers. The reforms raised the normal retirement age (NRA) from 65 to 67 for workers born after 1937 and correspondingly increased by a range of 20 to 30 percent the penalty for claiming benefits earlier than the relevant full eligibility age. This paper examines the labor supply response both at the extensive and intensive margins and as workers approach or go past the traditional retirement age. Specifically, the paper studies three age groups: the 55-60 age group, which can be labeled the "pre-retirement" age; the near-retirement age group of 61-65, and the 66-70 age group, which can be thought of as the "post-retirement" group. The identification makes the assumption that the cohort effects are changing linearly in both the treated and control cohorts.

Previous research on the effects of Social Security reforms estimate structural models to predict the impact on retirement rate (Gustman and Steinmeier (1985), Anderson, Gustman, and Steinmeier (1999), Gustman and Steinmeier (2009)) while another strand is comprised of ex-post evaluation of the reforms using reduced-form models. These papers focus on labor supply effects but only among those aged 61-65 (Mastrobuoni (2009)), the age at which people claim Social Security benefits (Song and Manchester (2009)), and Security Disability Insurance claims (Duggan, Singleton, and Song (2007)), while others exploit the nonlinear design of the 1983 reforms to study the impact of retirement on health (e.g., Yuanrong (2018)).

This paper contributes to the literature in several ways. First, it examines how men adjusted their labor supply not only at the extensive margin, but also at the intensive margin. Second, it analyzes the effects on different subpopulations to get a sense of the heterogeneous effects and welfare implications of the policy. Third, it stresses that the effects are highly dependent on where the workers are in the lifecycle.

Broadly, the evidence points to a strong response among male workers who are very close to their retirement ages. Results indicate a 3-percentage-point reduction in the retirement rate of male workers in the near-retirement age of 61-65. As the fraction of part-time workers rose by about 1.5 percentage points, men in this age group have also clocked in one hour more of work per week (an increase of about 6 percent relative to baseline). There is also some evidence, though weak, pointing to the persistence of labor supply effects. In the post-retirement age group of 66-70, the fraction of workers claiming to be unemployed — even though they have passed their normal retirement ages — rose by more than 1 percentage point, double the baseline mean. These labor supply effects seem to be driven largely by low-educated workers. Meanwhile, results show increased retirement among workers aged 55-60, but the effect can be traced to highly-educated workers who may have found the change in Social Security rules a reduction in the reward for working.

Overall, the findings in this paper suggest that, on average, the 1983 Social Security reforms have been successful in inducing male workers to increase labor supply. Nonetheless, there is evidence that some low-educated workers were more adversely affected by the wealth shock as indicated by their need to remain in the labor force beyond their normal retirement ages. This paper contributes to the discussion in public policy on how to mitigate the impact of future Social Security reforms, particularly on vulnerable segments of the society.

The remainder of the paper is organized as follows. Section 2 provides some background on the 1983 reforms while Section 3 presents the empirical approach, including a description of the data used. Section 4 presents the empirical evidence while Section 5 presents some policy implications and concludes.

2 Background on the Social Security reforms

To improve the financial viability of the the Social Security program, the Social Security Amendments of 1983 were signed into law in April 1983. The most important component of the amendments was a two-year increase in the NRA from 65 to 67 years old, as well as the related increase in the penalty for early benefit claimants from 20 percent to 30 percent (Table 1).[1] The amendments raised in a nonlinear manner the NRA for those born in 1938 or later. Workers born in 1937 or earlier were not affected by the reforms and maintained their NRAs at 65 years old. The NRA was increased in two-months increments starting with the 1938 birth cohort until it reached 65 and 10 months for the 1942 cohort. The NRA is kept steady at 66 years old for those born between 1943 and 1954. The NRA then increased gradually, again in two-months increments, for those born after 1954 until it reached 67 for the 1960 and later birth cohorts.

The nonlinear increase in the NRA is mirrored in the fraction of Social Security benefits that workers are entitled to should they decide to claim their benefits earlier than their NRA. Prior to the reforms, workers who decide to retire at the early retirement age (ERA) of 62 can claim 80 percent of their primary insurance amount (PIA), which is the total amount a worker receives if s/he decides to claim retirement benefits at his/her NRA.[2] This fraction is reduced gradually in 0.8-percentage-point increments starting with the 1938 birth cohort until it reached 75 percent for those born in 1943-1954. The amount was reduced again by the same increment until it settled to 70 percent for those born in 1960 or later. The policy design resulted in kinks in the benefit reduction path across birth cohorts.

Because of their potential confounding effects, two other Social Security policies are considered carefully in the paper — the Delayed Retirement Credit (DRC) and the removal of the Retirement Earnings Test (RET). The DRC raises by a certain percentage the Social Security benefits of workers who chose to delay their retirement beyond the normal retirement age up to the age of 70. The annualized DRC rate increases with birth cohort. It rises from a range of 1-6 percent for those born between 1900 and 1936 to reach 6.5 percent for the 1937-1938 birth cohorts, which includes the first cohorts affected by the 1983 Social Security reforms. The

[1] Other components include an increase in the number of workers covered by the program, higher payroll tax rate, and a rise in the actuarial adjustment factors beyond the NRA.

[2] According to the Social Security Administration, the retirement benefit is neither reduced for early retirement nor increased for delayed retirement at this age.

rate continues to rise in 0.5-percentage-point increments to reach 7.0 percent for the 1939-1940 cohorts, 7.5 percent for the 1941-1942 cohorts, and finally to 8 percent for those born in 1943 or later. Meanwhile, under the RET, the Social Security Administration withholds a fraction of benefits should beneficiaries' earnings exceed a certain threshold (the retirement earnings test exempt amount). This reduction in benefits is offset by higher benefits in the future at an actuarially fair rate. The RET thus served as a disincentive for beneficiaries to continue working to augment their retirement income. Prior to 2000, the RET applies to beneficiaries aged 62-69. However, the passage of the Senior Citizens Freedom to Work Act of 2000 removed the RET for beneficiaries who have reached their full retirement ages.

3 Empirical Strategy

The Social Security reforms affect workers born starting in 1938 (the treated cohorts) but not those born in 1937 or earlier (control cohorts). The most straightforward identification strategy, as has been done in some papers in the literature, is to essentially estimate the social security income of the treated and control cohorts, and add age fixed effects to ensure within-age comparison and year effects to partial out changes in aggregate economic conditions over time. This strategy does not take into account potential cohort effects unrelated to the Social Security reforms that could bias the estimates.

Cohort effects describe the variation in economic outcomes due to variation in common experiences or life events. In this case, workers born in the same year may be expected to have experienced broadly the same shocks that could influence their outcomes. For instance, Abrahamsen (2015) notes that over the life cycle, the differences in labor supply are primarily a function of age, time, and birth year.[3] After controlling for potential demographic changes and policy shocks, life cycle patterns among birth cohorts should be the same if there are no time and cohort effects. Suppose there are no cohort effects. Then, the variation in the life cycle can only be attributed to time effects, e.g., economic crisis, that affect cohorts at different points in the lifecycle. Suppose there are no time effects but only cohort effects. In this case, the lifecycle pattern, e.g., retirement, of one cohort may be higher or lower than another cohort due purely to

[3] She defines *age effects* as the effects of differences in ages of individuals over time; *period effects* as the effects of differences in time periods in the measurement of observations; and *cohort effects*, the effects due to differences in year of birth and some shared life events.

experiences or life events uniquely experienced by that cohort. A simple comparison of treated and control cohorts, which only controls for age and time effects, could potentially be biased by cohort effects. Previous studies assume that the cohorts under study are otherwise identical except for the retirement benefit change. This assumption may be defensible if the comparison is made between adjacent birth cohorts. But this may prove to be a very strong assumption if the birth cohorts under study are quite distant.

3.1 Difference-in-differences model

This paper takes a different approach and exploits the kink in the Social Security benefit rules, which avoids making the strong assumption that cohorts under study are identical absent the benefit reduction. It employs a difference-in-differences (DID) model which first compares the outcomes for the first few cohorts affected by the amendments, i.e., those born between 1938 and 1942 versus those born in 1937 or earlier. The second difference arises from the comparison of 1943-1952 birth cohorts which are in the flat portion of the Social Security benefit path (Figure 1). Note that compared to the 1937 or earlier cohorts, these cohorts also suffer from lower prospective social security benefit with the 1983 amendments. However, within this group, there is no variation in the new normal retirement age or prospective reduction in benefits for early claimants. In principle, therefore, dividing the 1943-1952 birth cohort into two subsamples and comparing their outcomes should only yield changes in cohort effects which are unrelated to the benefit reduction. It may be noted that the analysis focuses on the first few treated cohorts because these workers were about 41-45 years old when the amendments were passed in 1983. They were in the middle of their careers and had relatively less time to smooth consumption compared to the later cohorts. In particular, they had only about 17-21 years before they reached the early retirement age of 62. In comparison, the 1960 birth cohort — the first cohort to be subject to the new NRA of 67 — had almost 40 years to prepare before reaching 62. This implies that in principle, the policy response in terms of labor supply or financial reallocation as workers approach retirement may be strongest for the older cohorts.

The birth cohorts can be grouped as follows: Group A: 1933-1937; Group B: 1938-1942; Group C: 1943-1947; and Group D: 1948-1952 (Figure 1 marks the relevant Social Security benefits for the relevant cohort groups.).[4] The difference in outcomes for groups A and B yields a

[4] In the estimation for financial outcomes, the comparison groups are modified slightly due to data limitations.

combination of policy effects and changes in cohort effects. Meanwhile, the difference in outcomes between groups C and D can be argued to represent changes in cohort effects only. Under the assumption that the cohort effects are changing linearly across years, the DID estimator can be written as follows:

$$\begin{aligned} \text{DID} &= \big(E[Y_{1938-1942}] - E[Y_{1933-1937}]\big) - \big(E[Y_{1943-1947}] - E[Y_{1948-1952}]\big) \\ &= (\text{Policy Effect} + \text{Cohort Effect Change}) - (\text{Cohort Effect Change}) \\ &= \text{Policy Effect} \end{aligned} \tag{1}$$

Considering the predictions of the structural life-cycle retirement model by Gustman and Steinmeier (1985) and the key age groups examined by previous work, this paper analyzes the effects of the 1983 reforms on age groups representing various points in the life cycle: (1) "pre-retirement age" of 55-60, when workers are at the peak of their careers and when they are shown to be increasingly more aware of their social security benefits (Helman, VanDerhei, & Copeland, 2007); (2) "near-retirement" age of 61-65; and (3) age 66-70, which can be thought of as "post-retirement" period since the youngest cohort in the treated group (the 1942 birth cohort) has NRA of 65 years and 10 months. The CPS sample in this paper is limited to male workers to simplify the analysis.

3.2 Data

The paper utilizes data for the period 1993-2015 from the Current Population Survey (CPS), a monthly survey of about 60,000 households and the source of the official US labor statistics. The large sample in this nationally-representative survey provides power to exploit variation across birth cohorts and precisely identify the effect of wealth shocks on labor outcomes. In this research, treatment is determined by respondents' birth year. However, the monthly CPS data presents a misclassification issue since the CPS reports the respondent's age as of the survey week and not the birth year. Using information on age and survey month, one can estimate the birth year as the difference between the survey year and age. As described in Mastrobuoni (2007), the probability of misclassifying an individual's birth year using a simple difference between survey year and age is about 11/12 in a January survey because s/he was likely born later in

Specifically, Group C is composed of 1943-1945 birth cohorts while Group D is comprised of 1946-1948.

the year, 10/12 in February, and so on. By December, the misclassification probability is close to zero. This paper attempts to minimize the measurement error by restricting the sample to the December and January surveys. For the December sample, birth year is computed as the difference between survey year and age. For the January sample, it is estimated as the same difference minus one. These computations assume that (1) those born in December already celebrated their birthday prior to the survey and (2) those born in January have yet to celebrate their birthdays.[5]

I examine various indicators of labor force status from the CPS survey; (1) retired, (2) working part time, (3) unemployed, (4) disabled, and (5) weekly work hours to measure work intensity. It may be noted that the *work hours* variable represents the number of hours per week usually worked at all jobs. For those who respond that their work hours vary, I estimate their weekly hours depending on whether they usually work full-time or part-time.[6]

3.3 Specification

The following reduced-form model isolates the variation that can be attributed to the Social Security benefits reduction by partialing out confounding effects of cohort, race, marital status, age, overall time trends, and other policies:

$$\mathrm{y}_{it} = \beta_0 + \beta_1 \mathrm{Notch}_{it} + \beta_2 \mathrm{Post}_{it} + \beta_3 \mathrm{Notch{*}Post}_{it} + \lambda_{\mathrm{Age}} + \mathbf{X\Gamma} + \varepsilon_{it} \qquad (2)$$

In this model, y_{it} represents the labor force status of individual i at year t. In order to implement a difference-in-differences regression, I define the variable Notch_{it} to be a dummy variable equal to one if an individual is born either in 1938-1942 or 1933-1937, and zero if born in 1948-52 or 1943-47.[7] I define Post_{it} as a dummy variable equal to one for those born in either 1938-1942 or 1948-1952 and zero for those born in either 1933-1937 or 1943-1947.[8] The coefficient β_3 represents the DID estimate, the effect of being subject to a higher normal retirement age or a

[5]Given that the survey week always contains the 19th of the month, the resulting probabilities of misclassification under this approach are (1) (30-19)/365=3 percent, and (2) 19/365=5 percent, respectively.

[6]I assign 40 hours to workers who usually work full-time, and 20 hours to those who usually work part-time.

[7]This is equivalent to the typical "TREAT" variable in a difference-in-differences analysis, where Notch=1 if the group is treated by the policy.

[8]The main consideration in defining the cohorts to be included in each group is the number of cohorts affected by the policy.

lower prospective Social Security benefit on labor supply.

The λ_{Age} variable represents age dummies to ensure same age comparison of treatment and control groups. The vector **X** contains individual characteristics such as race, education, and marital status. To control for local labor and economic conditions, I also include census region fixed effects and the state-level unemployment rate. To capture the effects of the global financial crisis, I add a dummy for the year 2008. I control for time trends with a linear year effect. I impose this restriction in lieu of survey year dummies to conserve statistical power.

The vector **X** also includes controls for the DRC and the removal of the RET. In theory, the DRC incentivizes workers to continue to work past 65 years old (or their relevant NRA) up to age 70. Since this varies by cohort similar to the treatment, it can confound the estimated effects particularly for the post-retirement age sample. With respect to retirement, it can bias my estimate downwards as it incentivizes workers to delay their retirement past the normal retirement age. Thus, I include the DRC rate as an additional control for those aged 66-70.

Meanwhile, the RET could induce workers who are aged 62 or older to remain in the labor force until they reach the age at which they will not be subject to tax. In particular, for a worker aged 62 who is deciding to file for retirement, his decision could be driven by his prospective social security income as well as by the deferment in his benefits should he decide to do other part-time work in which his earnings exceed the RET threshold. The elimination of the RET for workers who have reached their normal retirement ages in the year 2000 could confound my estimates in two ways. First, RET no longer discourages labor force participation for workers who are in my post-retirement age group with the RET elimination in 2000. At the same time, the RET effect remains in place for younger workers. The main treated group in this paper, those born between 1938 and 1942, are about 58-62 years old in 2000 and so may have considered this policy change in their labor supply decisions. To control for this, I add a dummy variable equal to one for the years 2000 and onwards, zero otherwise. The regression models are estimated by OLS and standard errors are clustered at the birth cohort level.

The identification rests on the assumption that cohort effects are changing linearly and that after controlling for individual characteristics, time trends, macroeconomic effects, and confounding Social security policies, any observed trend-discontinuity in the labor supply and savings of workers born before and after 1938 is due to the corresponding change in their prospective social security benefits.

4 Empirical Evidence

Before proceeding to the estimation results, it is useful to examine the summary statistics for the various cohorts considered in the analysis. Table (2) in the Appendix section compares the four cohort groups used in the labor supply analysis and analyzed separately for three age groups - age 55-60, age 61-65, and age 66-70. The cohort groups are broadly similar. About 80 percent of the male workers in the sample have partners, more than 80 percent are white, while about 8 percent are black. With respect to education, younger cohorts tend to be slightly more educated than older cohorts. For the 1938-1942 male cohorts, the first set of treated cohorts, about one-third has high-school diploma, about 20 percent have some college, while close to a third have at least a college degree. The basic characteristics of the sample are broadly similar across age groups.

4.1 Cohort Effects

Figure (2) shows the labor force participation of men for the various cohort groups at various ages. Prior to age 55, the fraction of retired workers are fairly similar across cohort groups. However, there seems to be a marked divergence in labor force participation as workers reach age 55 until about 70, after which participation tends to converge again. At the 55-70 age range, the 1933-1937 cohort showed higher retirement rate compared to the 1938-1942 cohorts. The younger (control) groups, the 1942-1952 birth cohorts, tend to have lower retirement rates suggesting rising labor supply across cohorts over time. However, within the younger (control) group, there appears to be divergence in levels which could be evidence of cohort effects.[9]

To implement a formal test of the presence of cohort effects, I run a regression model of the labor force outcomes y against birth cohort dummies ($\mathbf{\Phi}_{\text{Cohort}}$) and controls ($\mathbf{X}$), which include race, education, age, and time trend.

$$\mathrm{y}_{it} = \beta_0 + \mathbf{\Phi}_{\text{Cohort}} + \mathbf{X\Gamma} + \varepsilon_{it} \tag{3}$$

[9]The same has been observed when analysis is done on full-time/part-time workers and even work hours.

I then plot the birth cohort fixed effects for the different outcomes and sample age groups. Figure (3) shows the results in the 61-65 age group, which covers the early retirement age of 62 and the pre-amendment normal retirement age of 65.[10] Overall, the statistical significance of the cohort dummies points to the existence of cohort effects, which seem to be more glaring starting from the 1938 cohorts. In addition, the cohort effects seem to be increasing in a linear manner as cohorts become younger.[11] This lends support to the estimation of a difference-in-differences model which partials out changes in cohort effects, thus potentially yielding the causal effect of interest under certain assumptions.

4.2 Main Results

The main specification results for male workers are shown in Table 3. Each figure represents the difference-in-differences estimate with the rows representing the different labor outcomes and the columns representing the estimates for the various age groups. The preferred specification includes age fixed effects, census region fixed effects, state unemployment rate, time trend, individual characteristics, control for the global financial crisis, and controls for confounding Social Security policies.

The first key question I attempt to answer is whether the reduction in prospective retirement wealth had any significant effects on labor supply at the extensive margin. Estimates in Table (3) suggest that the negative wealth shock appears to have discouraged continued stay in the labor force, consistent with the findings of Krueger and Pischke (1992) who note that the similar Social Security policy in 1977 reduced rewards for working. The DID model estimates an increase in retirement rate among men aged 55-60 by about 2 percentage points (ppts), coming from a baseline of 14.5 percent for the 1933-1937 cohorts.[12] Meanwhile, for the age group which spans the early retirement age of 62 and the previous normal retirement age of 65, results indicate that more workers in the treatment group decided to stay in the labor force. The decline in prospective retirement wealth caused more than 3-ppt decline in the retirement rate, relative to the baseline mean of almost 44 percent. Results also show a reduction in retirement for the 66-70 age group of about 2.8 ppts but this is not statistically significant.

[10] I set the 1937 birth cohort as the base for the cohort dummies.

[11] The trend in cohort effects is most evident in the pre-retirement and near-retirement age groups.

[12] Later analysis by education level reveals which subpopulation drives the results.

Next, I ask if the negative wealth shocks had significant effects on the intensive margin of labor supply. As shown in Table (3), there appears to be no considerable intensive margin effects for the 55-60 age group. The estimated effect on work hours is not statistically significant. However, the fraction of part-time workers seems to have declined significantly, potentially reflecting in part the increase in retirement in this age group. Meanwhile, among male workers who are very close to retirement (aged 61-65), evidence points to a substantial increase in labor supply as a result of the 1983 amendments. The fraction of part-time workers rose by about 1.5 ppt from a baseline of 13 percent. Male workers also clocked in an hour more of work or an increase of 6 percent from the baseline average of 18 hours per week. Workers' response thus may be argued to be the strongest in the near-retirement age group of 61-65. Meanwhile, in the age group where affected workers have already reached their new NRAs (66-70 years old), results do not indicate significant intensive margin effects. However, it is worth noting that the proportion of unemployed workers rose by more than 1 ppt (from a baseline of about 1 ppt) among this supposed retirees. The doubling in the fraction of unemployed workers suggests financial insecurity among workers who have already reached their normal retirement ages. This insecurity may be driving them to either remain in or return to the labor force, but are unable to find a job.[13]

I also investigate if the reduction in the generosity of Social Security could have induced some workers to file for disability instead of retirement benefits. It may be noted that workers cannot collect retirement and disability at the same time. Workers over the age of 65 who do not have the financial capability to retire but become disabled may choose instead to apply for disability benefits instead of start collecting retirement benefits. Evidence does not find any significant effects on the fraction of workers claiming disability. The estimates are not statistically different from zero and are much smaller than those of Duggan et al. (2007) who find that an additional 0.6 percent of men between the ages of 45 and 64 received SSDI benefits as a result of the 1983 reforms. Overall, my results suggest that workers did not opt for disability benefits instead of retirement benefits.

[13] The analysis by education level shows which subpopulation could be driving this finding.

[13] They use Social Security Disability Insurance enrollment and limited their sample to those aged 45-64.

4.3 Specification Check

The novel type of difference-in-differences estimator I used in this paper does not conform to the vanilla-type DID models. The most basic form of a DID model essentially computes the difference in the average outcome in the treatment group before and after some treatment effectivity date minus the difference in average outcome in the control group before and after the said date. However, instead of simply looking at two groups over time, I look at four different cohort groups. I exploit the nonlinearity in the policy design to come up with the "control groups," which span the 1943-1952 birth cohorts. Comparison within these younger cohorts addresses potential cohort effects in my model. Even then, there may be unit-specific trends that need to be controlled for in the DID models.

The different specifications which use various controls are shown in Tables (4) and (5). The most basic specification is a DID model with age fixed effects (Column 1). Because I analyze the average labor force participation of worker at a specific age group, the age dummies ensure I am comparing workers of the same age. Column (2) reflects the results when individual characteristics (race, education, and marital status) are added in the model. Column (3) adds the controls for local economic conditions (census region fixed effects and state unemployment). I then augment the model with controls for time trend and the RET removal separately (Columns 4 and 5). The estimated coefficients change marginally but their statistical significance generally remains. The final and preferred specification for the pre- and near-retirement age groups (Column 6) adds the GFC dummy and controls for the joint effect of time trend and the RET removal. The DRC variable is included as additional control for the preferred specification (Column 7) for the post-retirement age group.

4.4 Heterogeneity

To test for potential heterogenous treatment effects, I estimate the main specification, excluding the education control, for the various educational attainment levels: (1) less than high school or LTHS, (2) with high school diploma, (3) with some college, and (4) with at least a college degree, i.e., including graduate school.

Figures (4) and (5) show the labor supply effects by education level. In the 55-60 age group, the 2-ppt increase in the retirement rate appears to be driven primarily by highly-educated workers, i.e., those with at least a college degree. These workers may have been discouraged to remain in the labor force if they have interpreted the Social Security amendments as a reduction in the reward for working. Meanwhile, for workers aged 61-65, the 3-ppt reduction in retirement is driven by lower-educated workers, particularly those with some college or less. On the intensive margin effects, increased work effort is also seen primarily among lower-educated workers.

There is also some evidence that the effect of the policy persisted post-retirement. In particular, lower-educated workers appear to have increased their labor supply in response to lower retirement wealth. Results point to additional 2.8 percent of those with less than high school and 2.5 percent of those with some college are doing part-time work. These increases come from a baseline of about 10 percent and 13 percent, respectively. In addition, more workers appear to have decided to either remain or go back to the labor force in their post-retirement years. The fraction of unemployed workers rose by 1 ppt, and this effect is attributable mainly to low-educated workers, i.e., those with high school degree or less.

4.5 Placebo Test

One potential way to check that the DID model is capturing the policy effect is to estimate the model using a sample where there should be no meaningful change in labor force outcomes attributable to the Social Security reforms. Noting that there is no variation in the Social Security benefit among those born between 1943-1952, I estimate the main specification using the cohorts within this group.[14] It may be recalled that the DID model estimates assume that the cohort effects change linearly across cohorts. Under this assumption, the DID estimates for the placebo samples should not be statistically significant from zero. If they turn out to be significant, one can argue that the cohort effects are not changing perfectly linearly in the sample groups. This implies that the DID estimates could be capturing this differential trend in cohort effects. Results shown in Figure (6) show that the DID estimates are not significantly different from zero, providing support to the empirical strategy used in the paper.

[14] I assign 1943-1944 as Cohort A, 1945-1946 as Cohort B, 1947-1948 as Cohort C, and 1949-1950 as Cohort D. The first difference comes from Cohorts A and B, while the second comes from Cohorts C and D.

5 Discussion and Conclusion

This paper examines the impact of a large, potentially unanticipated wealth shock on the labor supply of older workers. It exploits an exogenous variation of wealth across birth cohorts arising from the Social Security reforms of 1983 to estimate the causal effects of a wealth shock in the pre-retirement, near-retirement, and post-retirement years. It takes advantage of the nonlinear design of the reduction in prospective Social Security wealth to estimate difference-in-differences models between the cohorts affected by the wealth shock and the earlier cohorts. By utilizing unique cohorts who were affected by the policy change differently, the DID model addresses potential cohort effects and other unobservables that could bias the estimates. The identification rests on the assumption that cohort effects are changing linearly and that after controlling for individual characteristics, time trend, macroeconomic effects, and confounding Social security policies, any observed trend-discontinuity in the labor supply of workers born before and after 1938 is due to the corresponding change in the prospective social security benefits.

The evidence points to considerably higher labor force participation among male workers, but only in the near-retirement age group of 61-65 years old. The fraction of workers retiring has declined significantly while workers seem to have rendered more work hours in this age group. This is consistent with the predictions of a standard labor supply model that labor force participation and reduced retirement wealth shock are negatively correlated. Results also show that for the 66-70 age group, the fraction of workers who are doing part-time work or unemployed rose significantly. These labor supply effects seem to be driven largely by low-educated workers. This finding appears to be consistent with the survey results that some workers seem to be unaware of the reduced generosity of Social Security, and are thus, likely taken by surprise when they visit SSA offices to check their retirement benefits (Helman et al. (2007), Bernheim, Skinner, and Weinberg (2001)).

Overall, the evidence in this paper suggests that, on average, the Social Security reforms (which tend to reduce the prospective retirement wealth) may have been successful in inducing workers to increase labor supply. Nonetheless, there is evidence that some low-educated workers are not able to sufficiently respond earlier as indicated by the need to remain in the labor force beyond their normal retirement ages. This paper contributes to the discussion in public policy on how to mitigate the adverse effects of future Social Security policy changes. This is particularly salient among lower-educated workers who seem to have been affected disproportionately by the 1983 reforms.

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A Tables

Table 1: Social Security Benefit Changes by Cohort

Year of Birth	NRA/FRA	Months between age 62 and NRA/FRA	Reduction at age 62: Primary beneficiary		Spouse	
			From $1000	In %	From $500	In %
1937 or earlier	65 yrs	36	800	20.00	375	25.00
1938	65 and 2 mths	38	791	20.83	370	25.83
1939	65 and 4 mths	40	783	21.67	366	26.67
1940	65 and 6 mths	42	775	22.50	362	27.50
1941	65 and 8 mths	44	766	23.33	358	28.33
1942	65 and 10 mths	46	758	24.17	354	29.17
1943-1954	66	48	750	25.00	350	30.00
1955	66 and 2 mths	50	741	25.83	345	30.83
1956	66 and 4 mths	52	733	26.67	341	31.67
1957	66 and 6 mths	54	725	27.50	337	32.50
1958	66 and 8 mths	56	716	28.33	333	33.33
1959	66 and 10 mths	58	708	29.17	329	34.17
1960 and later	67	60	700	30.00	325	35.00

Notes: Source of basic data: Social Security Agency (https://www.ssa.gov/planners/retire/agereduction.html). NRA/FRA means normal/full retirement age.

Table 2: Summary Statistics: Group Mean and Standard Deviation*

	1938-42	1933-37	1948-52	1943-47
	Age 55-60			
Age	57.95	58.67	57.49	57.71
	(1.685)	(1.038)	(1.680)	(1.711)
Married	0.797	0.816	0.728	0.771
	(0.402)	(0.388)	(0.445)	(0.420)
White	0.880	0.877	0.865	0.877
	(0.325)	(0.328)	(0.342)	(0.329)
Black	0.0766	0.0803	0.0772	0.0736
	(0.266)	(0.272)	(0.267)	(0.261)
LTHS	0.163	0.221	0.0956	0.121
	(0.369)	(0.415)	(0.294)	(0.327)
HS	0.340	0.325	0.284	0.297
	(0.474)	(0.468)	(0.451)	(0.457)
Some College	0.215	0.204	0.271	0.245
	(0.411)	(0.403)	(0.444)	(0.430)
College	0.283	0.250	0.350	0.337
	(0.450)	(0.433)	(0.477)	(0.473)
Obs	13308	3610	48458	32141
	Age 61-65			
Age	63.22	63.23	62.77	62.97
	(1.408)	(1.482)	(1.372)	(1.390)
Married	0.791	0.799	0.725	0.757
	(0.406)	(0.400)	(0.446)	(0.429)
White	0.873	0.878	0.852	0.872
	(0.333)	(0.328)	(0.355)	(0.334)
Black	0.0776	0.0830	0.0866	0.0759
	(0.268)	(0.276)	(0.281)	(0.265)
LTHS	0.167	0.210	0.0967	0.119
	(0.373)	(0.407)	(0.296)	(0.324)
HS	0.328	0.333	0.280	0.295
	(0.469)	(0.471)	(0.449)	(0.456)
Some College	0.221	0.198	0.275	0.244
	(0.415)	(0.398)	(0.446)	(0.430)
College	0.285	0.260	0.348	0.342
	(0.452)	(0.438)	(0.476)	(0.474)
Obs	20895	10000	30730	32139
	Age 66-70			
Age	67.92	68.11	66.32	67.72
	(1.398)	(1.405)	(0.465)	(1.339)
Married	0.768	0.785	0.736	0.739
	(0.422)	(0.411)	(0.441)	(0.439)
White	0.871	0.875	0.857	0.871
	(0.336)	(0.330)	(0.350)	(0.335)
Black	0.0734	0.0770	0.0799	0.0749
	(0.261)	(0.267)	(0.271)	(0.263)
LTHS	0.159	0.214	0.0946	0.119
	(0.366)	(0.410)	(0.293)	(0.324)
HS	0.329	0.335	0.249	0.300
	(0.470)	(0.472)	(0.433)	(0.458)
Some College	0.223	0.195	0.301	0.238
	(0.416)	(0.396)	(0.459)	(0.426))
College	0.289	0.256	0.355	0.343
	(0.453)	(0.436)	(0.479)	(0.475)
Obs	23326	16324	4230	23487

*Enclosed

Table 3: DID Estimates

	Age 55-60	Age 61-65	Age 66-70
Retired	0.0201**	-0.0332***	-0.0279
	(0.00802)	(0.00654)	(0.0162)
Part-time	-0.0266***	0.0151***	-0.00575
	(0.00890)	(0.00392)	(0.00948)
Work Hours	-0.409	1.045***	0.462
	(0.532)	(0.347)	(0.444)
Unemployed	0.000873	0.00322	0.0110***
	(0.00322)	(0.00224)	(0.00287)
Disabled	-0.00941	-0.00331	0.00991
	(0.00682)	(0.00419)	(0.00892)
Baseline Mean of Dependent Variable			
Retired	0.145	0.439	0.644
Part Time	0.144	0.129	0.129
Work Hours	28.87	17.52	9.420
Unemployed	0.0296	0.0164	0.00956
Disabled	0.106	0.0741	0.0454
Obs	97517	93764	67367

Notes: Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Specifications control for the following: Age FE, Marital Status, Education, Race, Region, State unemployment, Trend, RET, DRC (for the age 66-70 group). The number of observations refers to the specification in which retirement is the dependent variable. The baseline mean of the dependent variable refers to the 1933-1937 cohorts.

Table 4: Sensitivity Estimates: Age 55-60 and Age 61-65

	(1)	(2)	(3)	(4)	(5)	(6)
			Age 55-60			
Retired	0.0206**	0.0205**	0.0248**	0.0261***	0.0217**	0.0201**
	(0.00862)	(0.00879)	(0.00866)	(0.00832)	(0.00899)	(0.00802)
Part-time	-0.0322***	-0.0326***	-0.0301***	-0.0302***	-0.0264***	-0.0266***
	(0.00861)	(0.00863)	(0.00888)	(0.00893)	(0.00908)	(0.00890)
Work Hours	1.180**	0.616	-0.810	-0.855	-0.439	-0.409
	(0.445)	(0.430)	(0.565)	(0.575)	(0.562)	(0.532)
Unemployed	-0.0113***	-0.0101**	0.00189	0.00143	0.000494	0.000873
	(0.00354)	(0.00354)	(0.00306)	(0.00314)	(0.00295)	(0.00322)
Disabled	-0.0258***	-0.0168***	-0.00986	-0.00980	-0.0101	-0.00941
	(0.00473)	(0.00490)	(0.00575)	(0.00584)	(0.00645)	(0.00682)
			Age 61-65			
Retired	-0.0298***	-0.0290***	-0.0317***	-0.0311***	-0.0277***	-0.0332***
	(0.00967)	(0.00972)	(0.00938)	(0.00588)	(0.00797)	(0.00654)
Part-time	0.00919*	0.00756	0.00881*	0.00901**	0.0163***	0.0151***
	(0.00445)	(0.00467)	(0.00460)	(0.00422)	(0.00428)	(0.00392)
Work Hours	0.859*	0.495	0.797*	0.787**	0.894**	1.045***
	(0.481)	(0.480)	(0.416)	(0.373)	(0.347)	(0.347)
Unemployed	0.00513	0.00549	0.00271	0.00266	0.00250	0.00322
	(0.00352)	(0.00352)	(0.00241)	(0.00241)	(0.00220)	(0.00224)
Disabled	-0.00421	0.00235	0.00182	0.00147	-0.00453	-0.00331
	(0.00414)	(0.00487)	(0.00480)	(0.00321)	(0.00559)	(0.00419)
Controls:						
Age FE	✓	✓	✓	✓	✓	✓
Married, Educ, Race	X	✓	✓	✓	✓	✓
Region, State UE	X	X	✓	✓	✓	✓
Time Trend	X	X	X	✓	X	✓
RET	X	X	X	X	✓	✓
GFC	X	X	X	X	✓	✓
Obs: Age 55-60	97517	97517	97517	97517	97517	97517
Obs: Age 61-65	93764	93764	93764	93764	93764	93764

Notes:
Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The number of observations refers to the specification in which retirement is the dependent variable.

Table 5: Sensitivity Estimates: Age 66-70

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Retired	-0.0363**	-0.0357**	-0.0427**	-0.0364**	-0.0427**	-0.0387**	-0.0279
	(0.0146)	(0.0145)	(0.0164)	(0.0144)	(0.0164)	(0.0148)	(0.0162)
Part-time	0.000207	-0.00102	0.00316	0.00482	0.00316	0.00657	-0.00575
	(0.00650)	(0.00708)	(0.00865)	(0.00879)	(0.00865)	(0.00931)	(0.00948)
Work Hours	0.518	0.403	1.177**	0.930*	1.177**	0.987*	0.462
	(0.582)	(0.536)	(0.551)	(0.464)	(0.551)	(0.467)	(0.444)
Unemployed	0.0135***	0.0135***	0.00755**	0.00697**	0.00755**	0.00663**	0.0110***
	(0.00301)	(0.00304)	(0.00261)	(0.00262)	(0.00261)	(0.00255)	(0.00287)
Disabled	0.00712	0.00943	0.00554	0.00422	0.00554	0.00493	0.00991
	(0.00654)	(0.00680)	(0.00713)	(0.00726)	(0.00713)	(0.00714)	(0.00892)
Controls:							
Age FE	✓	✓	✓	✓	✓	✓	✓
Partnered, Educ, Race	X	✓	✓	✓	✓	✓	✓
Region, State UE	X	X	✓	✓	✓	✓	✓
Time Trend	X	X	X	✓	X	✓	✓
RET	X	X	X	X	✓	✓	✓
DRC	X	X	X	X	X	X	✓
GFC	X	X	X	X	X	X	✓
Obs	67367	67367	67367	67367	67367	67367	67367

Note: Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes:

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The number of observations refers to the specification in which retirement is the dependent variable.

B Figures

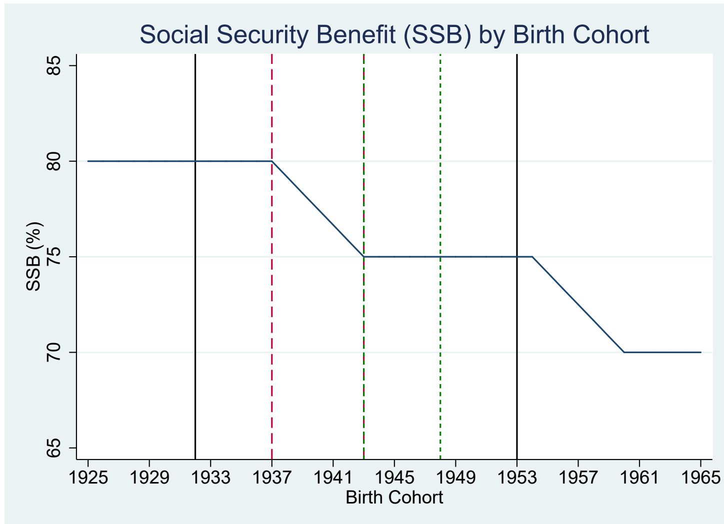

Figure 1

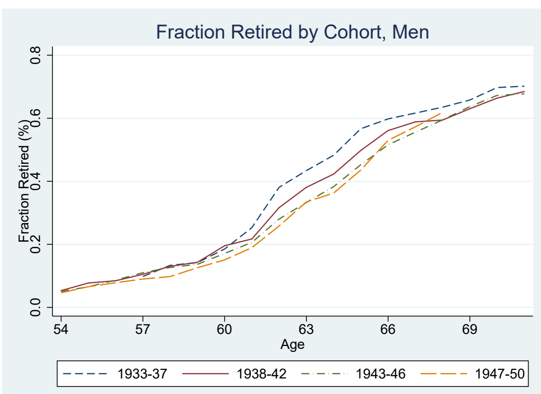

Figure 2: Retirement Trend

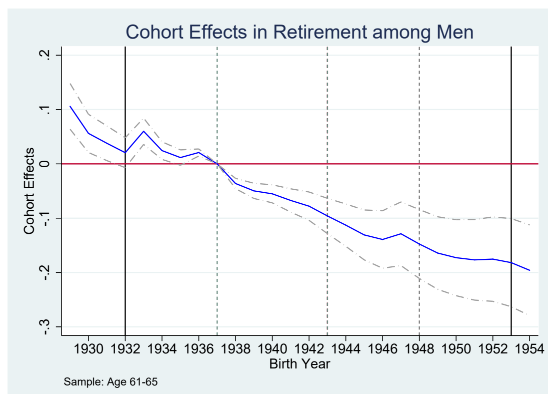

Figure 3: Cohort Effects

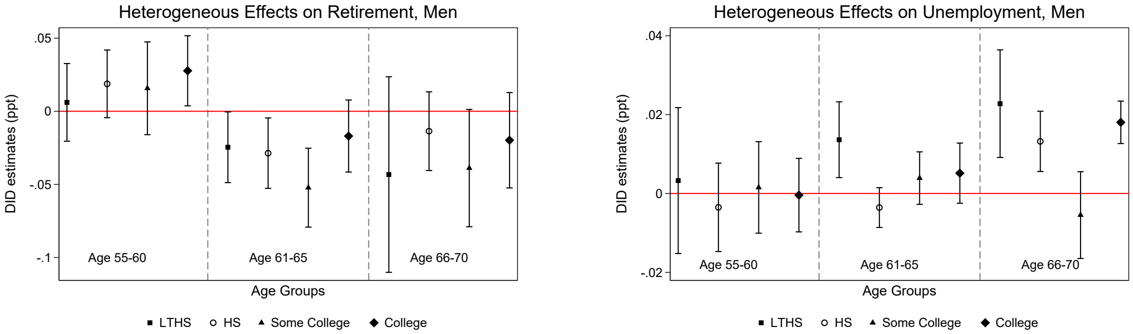

Figure 4: Heterogeneity - Retired and Unemployed

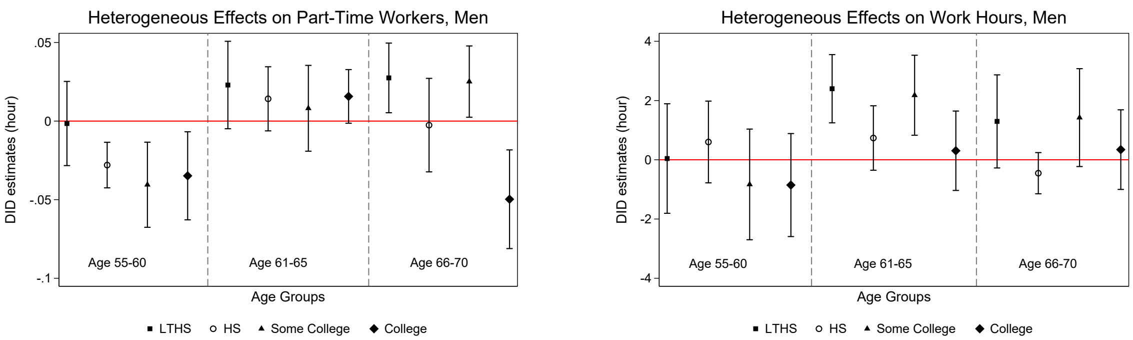

Figure 5: Heterogeneity - Part-time and Work Hours

Figure 6: Placebo Test