Access to Retirement Savings and its Effects on Labor Supply Decisions

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Abstract

In the United States, beginning at age 59.5, participants in tax-deferred retirement savings accounts (IRAs, Keogh, 401(k)s, Thrift plans) are granted access to their funds. How does reaching this age threshold affect withdrawal and labor supply decisions? This question is highly relevant to policy-makers who may wish to delay access to such funds to encourage labor force participation in aging populations. Using an event study framework to analyze data from the Survey of Income and Program Participation, I find that for the average participant in such retirement savings programs, gaining access to funds at age 59.5 increases monthly withdrawals by approximately \$23 and decreases hours worked per week by 1.4 hours. An instrumental variables strategy indicates that every additional \$1000 withdrawn annually reduces labor supply by approximately 91 hours that year. Estimates suggest that shifting the access age threshold back by one year would increase aggregate labor supply by an equivalent of 85,500 full-time jobs.

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1 Introduction

Tax-deferred retirement savings accounts (RSAs) are accounts into which pre-tax dollars can be deposited by participants, up to a specified annual limit.[1] These funds grow within the account[2] and can be withdrawn after the participant reaches age 59.5.[3] Only when the funds are withdrawn are they subject to taxation. A participant benefits from such RSA arrangements because taxation of income (both the pre-tax principal deposited and any earnings on that principal) is deferred into the future, when other income sources are presumably smaller and the participant is in a lower tax bracket. Examples of RSAs in the United States which follow such rules include Individual Retirement Accounts (IRAs), Keogh plans for the self-employed, 401(k) plans, and Thrift Savings Plans for government employees.

How does gaining access to funds in RSAs affect withdrawal and labor supply decisions? This paper will answer this question, focusing on RSAs in the United States. The impact of RSA access on labor supply is a highly relevant question to policy-makers. The United States will likely face an aging and declining workforce in the future, similar to experiences seen in other developed countries such as Japan and Germany. Policy-makers may wish to delay access to RSA funds (as they have done for other retirement benefits such as Social Security) in order to encourage or extend labor force participation, thereby increasing labor supply. Moreover, recent policy proposals promote the indexing of age thresholds to life expectancy, causing them to adjust automatically and gradually over time.[4] Shifting age thresholds defined in policies—including those

[1]The tax rules are slightly different for retirement savings accounts that allow post-tax dollars to be deposited. An example of this is the Roth IRA account. Nevertheless, such accounts have similar access rules.

[2]The principal in the account grows through investments in various asset classes, such as stocks, bonds, mutual funds, annuities, etc.

[3]Funds may be withdrawn before age 59.5, but a 10% penalty is incurred. There are a number of exceptions to this penalty which are discussed later.

[4]Denmark is an example where such policies have already been introduced.

delimiting retirement eligibility, social security receipt, and RSA access—affects both hours worked and the decision to participate in the labor market. In order to consider such policy changes, it is imperative that their impact on labor supply be estimated, so that informed policy decisions can be made.

It seems clear that gaining access to RSA funds increases withdrawals, given that the withdrawal choice upper limit changes from zero to the balance of the account. Changes in labor supply decisions, however, are less straightforward. In an inter-temporal labor supply model, a rational agent participating in an RSA would smooth leisure, and therefore labor supply, over all periods. Thus, gaining access to RSA funds should not have an impact on labor supply decisions if the agent is able to perfectly plan across the age threshold. For RSA access to have an impact, one of two special situations must be the case.

Firstly, RSA participants may be financially constrained in that the original solution to the labor supply model is a corner solution. These agents would like to withdraw from RSAs and reduce labor supply, but because of access restrictions, they are up against binding constraints in the optimization problem in the periods prior to the age threshold. Once they gain access to RSAs, these constraints are no longer binding and hence, the pattern of labor supply decisions changes.

Secondly, RSA participants may not be fully rational or dynamically consistent. When making deposits into RSAs, they may not anticipate the consequences of saving or fully plan out the optimal labor supply path—especially since these outcomes occur in periods often decades ahead. Subsequently, they may view access to RSA funds as an unexpected windfall. Thus, gaining access to RSA funds constitutes an income effect for this group, which leads to an increase in demand for leisure and a reduction in labor supply.

I find that for the average RSA participant, gaining access at age 59.5 increases monthly withdrawals by approximately $23 and decreases hours worked per week by 1.4 hours. Using an instrumental variables (IV) identification strategy, I find that every additional $1000 withdrawn each year reduces labor supply by approximately 91 hours for that year. Corresponding effects on labor force participation are also estimated. Further calculations suggest that shifting the access age threshold back by one year would increase aggregate labor supply by an equivalent of 85,500 full-time jobs.

This paper focuses on RSAs in the United States. RSA programs were first established in the 1970s and have gained popularity since, representing a growing fraction of assets set aside for retirement savings by households (Poterba et al., 1994). Much of the economic literature on IRAs has been devoted to the question of whether they encourage savings (see Gravelle (1991) for a survey). There has also been a growing strand of behavioral literature on the importance of default options in nudging workers to save (Beshears et al., 2009).

Another strand of the literature focuses on withdrawal behavior. Sabelhaus (2000) models IRA contributions and withdrawals, and finds that "IRA rules such as penalties for early withdrawals and minimum distribution requirements have predictable effects on IRA flows". Bryant and Gober (2013) document contribution and withdrawal behaviors for IRAs in particular between 2004 and 2010, detailing the impact of the financial crisis within that time period in particular. Brown et al. (2014) examine the consequences of a 2009 one-year suspension of the mandatory withdrawal rule had on withdrawal behavior from TIAA-CREF retirement accounts.[5] There has also been concern among economists regarding early withdrawals taken before the age 59.5 threshold. Using tax data, Argento et al. (2015) study the rise of early withdrawals in recent years and note their correlation with demographic and income shocks. Bryant et al. (2011) investigate similar issues and find that "leakage" from RSAs prior to retirement is mod-

[5]This temporary suspension does not affect the results in this paper because the rule in question deals with a later age 70.5 threshold.

est. However, there is a lack of studies examining the interaction between RSA rules and labor supply in particular. This paper contributes to the literature by being the first to examine how passing the RSA age threshold affects labor supply decisions.

Previous labor supply literature analyzing the "retirement decision" (that is, the decision to reduce labor supply and potentially exit the labor force) has focused on estimating structural dynamic models. Gustman and Steinmeier (1986) and Stock and Wise (1990) are two examples of different structural modeling approaches that have been taken. Aside from RSA age thresholds, various other factors affecting retirement have been considered, including availability of employer pension plans (Stock and Wise, 1990), changes in social security policy (Krueger and Pischke, 1992), and these factors in combination with health (French, 2005) and various other considerations (Fields and Mitchell, 1984).[6] Stewart (1995) deals with social security age thresholds and predicts that workers would delay retirement if the early retirement age increased. Various papers have also looked into "early retirement windows" offered by particular employers to gauge how access to such enhanced incentives to retire can induce retirement. These include Brown (2002), who uses the Health and Retirement Study to look into their effects across firms; on the other hand, Hogarth (1988), Lumsdaine et al. (1990), and Pencavel (2001) investigate the effect at specific individual employers. Others, including Coile and Levine (2010), Gustman et al. (2011), and Goda et al. (2012), use the natural experiment of the Great Recession in 2008 to explore the impact of wealth, savings, and labor market conditions on retirement decisions. I connect this labor supply literature to the retirement savings literature by focusing on the RSA age 59.5 threshold as an impetus for labor supply responses.

[6] For a summary of social security-like systems in other countries, see Gruber and Wise (2008).

2 Data

The panel data used in the analyses to follow come from the 2008 panel of the Survey of Income and Program Participation (SIPP). The SIPP follows a panel of members of randomly sampled households who are interviewed every four months for 16 "waves" of interviews (thus far). Survey questions are asked during each interview wave about each person's preceding four months.[7] The data are then reorganized such that each unit of observation is a person-month. The sample of analysis is restricted to observations where the person is between the ages 57 and 62 whose household has never owned a business.[8] All dollar figures are adjusted for inflation using the Consumer Price Index (CPI) and are in May 2008 dollars, the first month of Wave 1 of the 2008 SIPP. Labor supply is measured in usual hours worked per week or as an indicator for working positive hours.

Table 1 shows summary statistics for the sample. Column (1) contains statistics calculated over person-month observations for the entire sample; column (2) uses person-month observations of RSA participants only (persons who have ever owned any type of RSA); and column (3) uses person-month observations of non-participants only. The columns in the table show several differences between RSA participants and non-participants. RSA participants are more likely to be white and married. On average, they have acquired more education compared to non-participants. Owning an RSA is also associated with greater incomes and higher labor supply—both in terms of hours worked and labor force participation. Moreover, conditional on working, usual hours worked are higher. In other respects such as sex and household size, RSA partici-

[7] For example, a person being interviewed in April will be asked about his or her monthly earnings for January, February, March, and April. The next wave of the interview is then conducted four months later in August, with questions about monthly earnings in May, June, July, and August.

[8] I choose age 62 as the upper age limit of the sample in order to avoid the impact from social security reduced benefits eligibility; age 57 is chosen as the lower age limit for symmetry. I exclude business-owning observations because of complications with the reporting of income and hours worked. The analysis thus focuses only on those who work for earned income.

pants seem similar to the overall population. In later regression specifications, including person fixed effects which capture the impact of these covariates will be essential for ensuring that non-participants serve as a viable comparison group.

Figure 1 presents histograms depicting distributions of age and usual hours worked per week in the sample. Sample attrition occurs gradually from age 57 to 62. For usual hours worked, there is bunching at 0 and 40 hours per week, but also an appreciable number of observations at other points of the hours worked distribution.

Figure 2 shows the proportion of RSA participants making withdrawals in any given month over the ages in the sample period. There is a discrete increase just before the age threshold, demarcated by the vertical line. The proportion withdrawing increases from zero to about 1.5% at age 59.5. Subsequently, the proportion withdrawing each month increases gradually to almost 3.5% by age 62. These percentages may seem low, but that is because RSA participants do not necessarily make withdrawals from their RSAs every month—for example, they may choose to make a large withdrawal once a year.

A withdrawal in the SIPP data is defined as the "amount [the survey respondent] received from [a] draw on an IRA, KEOGH, 401(k) or Thrift Plan in this month". There are several exceptions which allow for penalty-free withdrawals before age 59.5, including death or disability of the RSA owner, qualified education and medical expenses, and (for 401(k) plans after age 55) separation from employer.[9] In addition to these exceptions, the slight uptick in withdrawals just prior to age 59.5 could be due to measurement error (in either withdrawals or age) or RSA participants being imperfectly informed about the age threshold (thinking that it is at 59 instead of at 59.5). There also exists an annuity payments scheme called the "Series of Substantially Equal

[9]For a full list of exceptions and a comparison of these exceptions between different plan types, see
http://www.irs.gov/Retirement-Plans/Plan-Participant,-Employee/Retirement-Topics—Tax-on-Early-Distributions.

Payments" allowing for withdrawals before age 59.5. Unfortunately, the data do not disaggregate the total withdrawal amount further into amounts for the different plan types. While there are minor differences, the rules and exceptions of the various plan types are more or less identical, with the main similarity being that the access age threshold for all these plans are the same: at age 59.5.[10]

Panel (a) of Figure 3 depicts a similar pattern in the (unconditional) average monthly withdrawal amount. However, when averaged over only RSA participants who withdrew some positive amount, the conditional monthly withdrawal varies between \$3000 and \$4000 dollars near the age 59.5 threshold, gradually declining to around \$2500 in later years. This is depicted in panel (b) of Figure 3.

Across the age threshold, the labor supply decisions of RSA participants follow markedly different patterns over time compared to non-participants.

Figure 4 plots the labor force participation rate (proportion who report positive usual hours worked per week) by age and RSA participation. The thicker line is the rate for RSA participants, while the thinner line is that of non-participants. As expected, both groups gradually reduce labor force participation as they age. RSA participants are more likely to be working compared to non-participants at every age level. This is likely due to the fact that many RSAs are offered through employers (e.g. 401(k)s) or require contribution of earned wages (e.g. IRAs).

However, when RSA participants pass the age 59.5 threshold and gain access to retirement funds, their decline in labor force participation is much steeper relative to similarly-aged non-participants. In the two-and-a-half-year period before the age 59.5 threshold, both groups experience a 4 to 5 percentage point decline in labor force

[10]One concern is that Roth IRA withdrawals can be taken penalty free before age 59.5 as long as it is part of the basis that was contributed after-tax. Even though there is no way to distinguish between Roth and Traditional IRA withdrawals in the data, since Roth IRAs were invented only in 1997, even the youngest members of the sample passing the 59.5 threshold would have been in their late 40s at the time. Hence, Roth IRA withdrawals may not matter as much because participants in the sample only had limited time to contribute into Roth IRAs.

participation. Yet in the two-and-a-half-year period after the threshold, the proportion working declines by 11.2 percentage points for RSA participants (from 0.764 to 0.651), but only by 6.7 percentage points for non-participants (from 0.357 to 0.285).

A similar pattern between RSA participants and non-participants emerges when comparing hours worked per week. Panel (a) of Figure 5 depicts hours worked per week among all respondents, including those working zero hours. In the two-and-a-half-year period prior to the age 59.5 threshold, both groups experience a gradual decline in hours worked. For RSA participants (thicker line), this decline is 2.6 hours per week; for non-participants (thinner line), it is 1.4 hours per week. However, in the two-and-a-half-year period after the age threshold, the decline for RSA participants accelerates to 5.3 hours per week, while non-participants see a decline of only 2.7 hours per week.

Panel (b) of Figure 5 depicts hours worked per week conditional on working positive hours. On average, RSA participants put in more hours (conditional on working) compared to non-participants. Given the reduced sample size over which these averages are calculated, the line graphs appear noisier, especially for non-participants. From these graphs, it is difficult to say definitively whether the declining trend diverges after age 59.5 between RSA participants and non-participants. If one were to presuppose that the decline amongst the two groups are identical, this would indicate that most of the reduction in hours worked results from changes on the extensive margin as workers stop working and move to zero hours. The regression analysis in the next section should shed further light on this.

3 Regression Analysis

Three sets of regression results are presented in this section. First, I show that reaching the age threshold of 59.5 induces monthly withdrawals from RSAs. Second, I show that reaching the age threshold reduces labor supply in terms of hours worked per week. Finally, I use IV to estimate the relationship between monthly withdrawal amounts and hours worked per week. The sample of analysis is the five-year window around the age 59.5 threshold—person-month observations where age is between 57 and 62. Standard errors are clustered at the state level to account for SIPP's survey design.[11] Checks for robustness to a variety of alternative specifications are presented in the appendix.

RSA Access and Withdrawal Amount To estimate how reaching the age 59.5 threshold affects the withdrawal behavior of RSA participants, I use ordinary least squares (OLS) to estimate the regression

$$withdrawal_{it} = \beta\, post_{it} + \alpha_1 age_{it} + \alpha_2 age_{it}^2 + \mu_t + \mu_i + \mu_{SIPP} + \varepsilon_{it} \qquad (1)$$

where:

- $withdrawal_{it}$ is the dollar amount withdrawn by person i in period t from their RSAs
- $post_{it}$ is an indicator which takes the value of one when person i is older than 59.5 in period t
- age_{it} is person i's age in period t, normalized to 0 at age 59.5
- μ_t and μ_i are a set of time- and person-level fixed effects
- μ_{SIPP} are a set of SIPP reference month, SIPP wave, and state fixed effects, included for identification and inference purposes to account for the SIPP survey structure and seam bias

[11]For respondents reporting multiple states over time, the modal state is used.

- ε_{it} are error terms

Because only RSA participants have non-zero positive withdrawals, the sample of analysis for this set of regressions excludes non-participants. This specification is akin to an event study, where the event is gaining access to RSA funds as each person i reaches age 59.5 over time t. The identification strategy measures the change in withdrawal decisions across the age threshold as depicted in panel (a) of Figure 3. The estimate of the coefficient β is the marginal effect of being granted access to RSA funds at age 59.5 on monthly withdrawal amount.

Table 2 presents the coefficient estimates for different specifications of equation (1). Column (1) is a basic OLS specification which includes the age quadratic function and all fixed effects. Gaining access to RSA funds at age 59.5 increases withdrawals by about $23 per month on average. This estimate is statistically significant at a 1% level.[12]

This effect estimate of passing the age threshold on withdrawal amount may mask differences in withdrawal patterns among different demographics of RSA participants. For example, withdrawal patterns may change over time as RSA participants age. Alternatively, liquidity-constrained participants may want to make larger withdrawals. Another possibility is that better-educated RSA participants may be more salient of RSA rules and the tax implications of timing withdrawals without accruing penalties. Lastly, the sex of the account holder may also influence withdrawal behavior. To investigate potential heterogeneity in the effects of passing the age thresholds on withdrawal patterns, I augment the regression specification in equation (1) with additional interaction terms.

[12]One concern is that instead of smoothing withdrawals across months, RSA participants can make one-time lump sum withdrawals of the entire amount of RSA funds, or make larger sporadic withdrawals punctuating many months of zero withdrawals. The inclusion of person fixed effects accounts for differences in this behavior because identification is based off of within-person variation, allowing one to interpret the estimates as average treatment effects on withdrawal on a monthly basis.

To investigate whether withdrawal behavior changes with age, I add an interaction term between $post_{it}$ and age_{it} to the specification in equation (1). The result is presented in column (2) of Table 2. The coefficient estimate on the interaction term is not statistically significant, and the coefficient estimate on $post_{it}$ remains almost identical to the previous specification in column (1). This suggests that the average size of monthly withdrawals do not change as RSA participants age.

To investigate whether liquidity-constrained participants make larger withdrawals, I add an interaction term between $post_{it}$ and a measure of liquidity to the specification in equation (1). This liquidity measure is derived from data on SIPP survey respondents' liquid assets in various financial accounts during specific "topical" waves of the survey when such questions were asked.[13] It is calculated as the mean across all topical SIPP waves of the inflation-adjusted summed dollar amount in all checking, savings, money market, certificates of deposit, and brokerage[14] accounts. Column (3) of Table 2 report the coefficient estimates for this specification with the liquidity interaction term. The estimate of the interaction term coefficient is close to zero and not statistically significant. This suggests that liquidity constraints do not influence RSA withdrawal behavior, and that RSA participants making withdrawals may in fact be irrational or dynamically inconsistent.

Another source of heterogeneity in effect sizes is that better-educated RSA participants may be more salient of RSA rules and the tax implications of withdrawals. The effect of financial literacy (which is correlated with education) on retirement planning has been documented in a growing literature. Clark et al. (2012) find that financially illiterate workers tend to be unaware of the parameters of both public and private retirement plans, such as benefits and age thresholds; this in turn affects their labor supply

[13] These are waves 4, 7, and 10 of the 2008 SIPP.

[14] "Brokerage" here refers to the value held in stocks and mutual funds only. I count these as liquid assets because they can usually be liquidated for cash balances in a short amount of time.

plans in terms of expected age of retirement. While the sample in Clark et al. (2012) is limited in scope, Lusardi and Mitchell (2011) use a more representative dataset and similarly conclude that there is great variation in financial literacy in the population, and that it is positively correlated with making plans for retirement. Given that financial literacy encourages retirement planning *before* retirement, it seems reasonable to argue that financial literacy would also be positively correlated with financial planning *during* retirement, in regards to the withdrawal and use of different retirement assets.

To investigate whether financial literacy influences the size of RSA withdrawals, I add two interaction terms to the specification in equation (1): one between $post_{it}$ and a dummy for having a two- or four-year college degree, and another between $post_{it}$ and a dummy for having a high school diploma.[15] (The baseline is no high school diploma.) These results are reported in column (4) of Table 2, and suggest that RSA participants with college degrees on average withdraw \$29 more per month upon reaching the age 59.5 threshold and gaining access to their RSA funds, compared to those with only high school diplomas. The difference a high school degree has on the effect of RSA access (relative to no high school) is positive but not statistically significant (\$13). These findings suggest that knowledge of RSA rules and thresholds may play a role in determining withdrawal behavior. On the other hand, education may be correlated with the size of RSA balances, and this difference between participants with varying education levels may just be accounting for differences in available RSA funds for withdrawal.

One last potential source of heterogeneity is sex. The specification in column (5) of Table 2 augments equation (1) with an interaction term between $post_{it}$ and a female indicator variable. The coefficient estimate shows that after gaining access to RSA funds at age 59.5, compared to males with a baseline withdrawal increase of \$32 per month, females withdraw on average \$17 less per month than males, though this difference is

[15] The dummy variables are cumulative, in that all those with college degrees are also always coded as 1 for the high school diploma dummy.

statistically significant only at the 10% level. One explanation for this difference may be that female workers saved less during their working years because they received lower wages or were absent from the labor force for some time periods, leading to lower RSA balances to draw from. Another reason may be that females are more frugal with their RSA savings because of time preferences that value future periods more highly.[16]

RSA Access and Labor Supply To estimate how reaching the age 59.5 threshold affects hours worked and labor force participation, I use the regression

$$outcome_{it} = \beta_1 \left(post_{it} \times RSA_i\right) + \beta_2 post_{it} + \alpha_1 age_{it} + \alpha_2 age_{it}^2 + \mu_t + \mu_i + \mu_{SIPP} + \varepsilon_{it} \quad (2)$$

where

- $outcome_{it}$ is either usual hours worked per week or an indicator for positive non-zero hours worked, for person i in period t
- RSA_i is an indicator for person i participating in an RSA program

and the remaining notation is defined as before. The RSA_i term alone is excluded because it is perfectly collinear with the included person fixed effects μ_i.

I use this difference-in-differences event study approach because RSA non-participants ($RSA_i = 0$) have non-zero labor hours, unlike the previous regressions involving equation (1) with withdrawal amount as the dependent variable, where RSA non-participants always had zero withdrawals. By using non-participants as a comparison group for RSA participants, the estimate of β_1 measures the labor supply effects of being granted access to RSA funds for participants, independent of age trends in labor supply patterns common to both RSA participants and non-participants. In doing so, I make the following identification assumptions: 1) there are no differential age trends for both

[16] Warner and Pleeter (2001) present some evidence of gender differences in discount rates.

groups conditional on within-person characteristics controlled for in the included person fixed effects, and 2) the only policy change affecting RSA participants at the age 59.5 threshold is the one imposed by the RSA access rule.[17] Thus, β_1 measures the difference in labor supply changes across the age threshold between RSA participants and non-participants as depicted in panel (a) of Figure 5. Moreover, since individuals cannot manipulate their age, being on either side of the age threshold ($post_{it}$) is exogenous. Thus, as long as the difference-in-differences identification assumptions hold, the estimate can be interpreted as the causal effect of gaining RSA access on labor supply.

The first two columns of Table 3 present the coefficient estimates for specifications of equation (2) across the age 59.5 threshold. Column (1) reports results for the specification where usual hours worked per week is the dependent variable. These suggest that gaining access to RSA funds reduces hours worked by 1.4 hours per week for the average RSA participant over the 2.5 year period after age 59.5. This estimate is statistically significant at the 1% level.

While these labor supply reduction estimates measure the effect averaged across multiple periods and over many individuals after passing the age threshold, in reality, adjustments to labor supply may not be as abrupt or as flexible. Firstly, workers may not be free to choose hours precisely; in many vocations, the choice of hours is discrete (e.g. 40 hours or 0 hours) rather than continuous. Secondly, workers may respond to passing the age threshold gradually, reducing labor supply over a protracted period of time.

The first consideration is that workers may not be able to choose hours flexibly. While researchers have examined specific situations[18] where precise control of hours is possible, this question of flexibility in hours is an issue arising in the labor supply

[17] To the best of my knowledge, there exists no other policy change occurring at this rather unique age of 59.5.

[18] One such example is taxi drivers. See Camerer et al. (1997) and Farber (2005).

literature more generally. Nonetheless, even in the extreme case where all workers can only work 40 or 0 hours (and no other number of hours), the estimates with hours as the dependent variable can still be interpreted as the average reduction in labor supply, averaged over multiple workers with this binary choice. This estimate is still policy relevant in the aggregate for those wishing to calculate the aggregate labor supply reduction due to treatment.

One way to get a sense of how labor supply responds on the extensive margin is to set labor force participation as the dependent variable. Column (2) of Table 3 presents linear probability estimates of equation (2) where $outcome_{it}$ is an indicator variable which takes on 1 if hours worked that week is positive. The results suggest that gaining access to RSA funds reduces the probability of working by 2.7 percent for the average RSA participant over the 2.5 year period after age 59.5. This estimate is statistically significant at the 1% level.

Together, the estimates from columns (1) and (2) indicate that workers reduced labor supply on both the intensive and extensive margins. This can be seen by multiplying 0.0268 by 40 hours per week, the modal usual hours worked per week as observed in panel (b) of Figure 1. The product, 1.072 hours per week, represents the labor supply reduction on the extensive margin, expressed in hours. This is slightly less than the 1.425 hours per week reduction found in column (1), which represents the labor supply reduction on both the intensive and extensive margin. Clearly, there are workers who are able to shift to fewer (but still positive) hours, but there are also those who exit the labor force entirely by dropping to zero hours.

The second consideration is that a worker may choose to "ease" into retirement by slowly reducing hours over several periods after gaining access to RSA funds. Between persons, even if every worker can only adjust between 40 and 0 hours per week, averaged over the entire population, the effect would still be stretched over a period of time as workers exit the labor force gradually as opposed to everyone simultaneously doing so at the age threshold. This would lead to heterogeneity in effect sizes spread over the different age levels after 59.5 years old.

To investigate how the effect size may vary over time, I use a modified version of the regression specification in equation (2). First, I replace the age quadratic function with a full set of indicator variables for each age level (by month). Second, I replace the interaction term ($post_{it} \times RSA_i$) with interaction terms between the RSA_i dummy and a set of indicator variables for each age level (by month) on both sides of the age 59.5 threshold. Thus,

$$outcome_{it} = \sum_{j=57}^{62} \beta_j \left(RSA_i \times 1\left\{age_{it} = j\right\}\right) + \sum_{j=57}^{62} \alpha_j 1\left\{age_{it} = j\right\} + \mu_t + \mu_i + \mu_{SIPP} + \varepsilon_{it} \tag{3}$$

where

- $1\left\{age_{it} = j\right\}$ is the age-month indicator variable and $j = \left\{57, 57\frac{1}{12}, 57\frac{2}{12}, \ldots\right\}$ is an index by months

and the remaining notation is defined as before. The coefficient β_j on each interaction term measures the effect on labor supply of being in a particular age-month for RSA participants relative to non-participants at the same age-month level.

Panel (a) of Figure 6 plots these interaction term coefficient estimates of the effect on hours worked over time around the age 59.5 threshold. The plot shows that there is indeed a gradual growth in the reduction in hours worked to the right of the age threshold. Six months after gaining access to RSA funds, there is a slight but statistically insignificant labor supply reduction of 0.5 hours worked per week for RSA participants relative to their non-participating counterparts. Over time, this reduction increases in magnitude and becomes statistically significant, reaching approximately 1.5 hours per week one year after the age 59.5 threshold. The size of the reduction continues to grow, reaching almost 3 hours per week 30 months after the age 59.5 threshold.

A similar pattern is observed in panel (b) of Figure 6, which plots these interaction term coefficient estimates of the effect on labor force participation over time around the age 59.5 threshold. This plot also shows a gradual growth in the reduction in the probability of working positive hours to the right of the age threshold. One year after gaining access to RSA funds, there is a statistically significant decline of 2.5 percent in the probability of working. This decline almost reaches 5 percent two years out.

Another aspect of the plots worth noting is the pre-threshold trends. While there seems to be a slight downward trend, there is no statistically significant effect on RSA participants relative to non-participants prior to the threshold, which is to be expected. This lends credence to the difference-in-differences assumption regarding non-differential trends, affirming RSA non-participants as a reasonable comparison group. The decision to participate in RSAs may be endogenous, but this suggests that it may not be a major issue conditional on factors accounted for by person fixed effects. The break in trend is quite clear around the 59.5 threshold, with the reduction in labor supply accelerating after passing it.

* * *

The regression results in the previous two subsections highlight the reduced-form effects of reaching the age threshold on withdrawal and labor supply decisions. These estimates are relevant when considering the macro-level impact of shifting the age threshold to a later age, should the policy aim be to increase the aggregate labor supply of aging populations or contain the aggregate amount of funds withdrawn from RSAs. On the other hand, having ruled out liquidity constraints as a driver for withdrawals, if dynamically-inconsistent RSA participants view withdrawals as an unexpected boost in income, then finding the direct "income effect" of each dollar withdrawn on labor supply would be of interest. The next subsection explores this effect using an IV estimation strategy.

Effect of withdrawal amount on labor supply. To estimate the effect of withdrawals on labor supply, I use the regression

$$outcome_{it} = \gamma \, withdrawal_{it} + \alpha_1 age_{it} + \alpha_2 age_{it}^2 + \mu_t + \mu_i + \mu_{SIPP} + \varepsilon_{it} \qquad (4)$$

The coefficient γ in this equation estimates the average effect of one additional dollar of RSA withdrawal on labor supply (hours worked per week or the probability of working positive hours). However, using OLS to estimate this equation may result in biased estimates. This can be either because withdrawal amounts may be correlated with unobservables in the error term (selection bias), or because $withdrawal_{it}$ is measured with error (attenuation bias).

To mitigate this bias, I take an IV approach. I instrument withdrawal amount with two instruments: the post age threshold dummy interacted with the RSA participation indicator ($post_{it} \times RSA_i$), and the post age threshold dummy alone ($post_{it}$). The first stage regression is

$$withdrawal_{it} = \pi_1 \left(post_{it} \times RSA_i \right) + \pi_2 post_{it} + \lambda_1 age_{it} + \lambda_2 age_{it}^2 + \mu_t + \mu_i + \mu_{SIPP} + \eta_{it} \quad (5)$$

which is identical to equation (2) except withdrawal amount is now the dependent variable. This regression can also be seen as an extension of equation (1) into the difference-in-differences form by including in the analysis the sample's RSA non-participants, who have zero withdrawals throughout.

I argue that the identifying assumptions of IV are satisfied. First, the exclusion restriction holds for both instruments. The post age threshold dummy $post_{it}$ itself is exogenous because individuals cannot manipulate which side of the age threshold they are on. By the same reasoning, the interacted term ($post_{it} \times RSA_i$) is also exogenous conditional on RSA_i, which is captured by the person fixed effects included in the regression. Moreover, the age threshold affects labor supply only through their effect on withdrawal decisions, since there are no other policies or phenomena that change at age 59.5 and that would have a direct impact on labor supply. Secondly, passing age 59.5 does have an impact on withdrawal decisions, thus satisfying the non-weak instruments requirement. This can be seen in the statistically significant first stage coefficient estimates reported in column (3) of Table 3.

Given this IV identification strategy, γ can be interpreted as the causal effect of withdrawals on labor supply for RSA participants who "complied" with the age threshold rule and were induced to make withdrawals because they passed the age threshold and were granted penalty-free access to RSA funds. This local average treatment effect (Imbens and Angrist, 1994) is the policy-relevant estimate necessary to predict the aggregate labor supply adjustment to a shift in this age 59.5 threshold, since it is precisely this group of "compliers" who will respond to such a shift.

Columns (4) and (5) of Table 3 report different specifications of the second stage regression in equation (4). The specification in column (4) sets hours worked per week as the dependent variable. The IV estimate suggests that a \$100 increase in withdrawals per month reduces labor supply by 2.1 hours per week. The estimate is statistically significant at the 1% level. This translates to an annual 91-hour reduction in labor supply for the RSA participant who withdraws \$1000 more that year.

Column (5) of Table 3 presents analogous IV results for labor force participation, replacing the dependent variable with an indicator for positive hours worked. The linear probability specification estimates that a \$100 increase in withdrawals per month reduces the probability of working by 4.3 percent on average. The estimate is statistically significant at the 1% level. This translates to an annual 3.6 percent reduction in the probability of working for the RSA participant who withdraws $1000 more that year.[19]

4 Conclusion

The findings from the regression analysis reinforce the empirical patterns presented graphically earlier. They suggest that delaying access to RSA funds can have appreciable effects on both RSA withdrawal patterns and labor supply decisions. These estimates are especially relevant to policy-makers who may wish to delay access to such funds in order to encourage labor force participation in aging populations. Not only will shifting the age threshold have an effect on aggregate labor supply, but the impact on withdrawals will reduce aggregate savings levels as well, which is likely to be a concern to central bankers and financial institutions holding RSA deposits. Any policy decision to shift the access age threshold should be approached and considered in a thoughtful manner. These results may also be relevant in policy contexts pertaining to other retirement plans with access age thresholds, such as social security and private pensions.

To gain an understanding of the magnitude of a potential policy change in the aggregate, consider the following calculations. Suppose the government delays RSA access by one year, moving the age 59.5 threshold to age 60.5, thereby causing those affected to increase labor supply. According to the 2010 census, there are approximately

[19] OLS estimates of equation (4) are much smaller in magnitude compared to these IV estimates. For hours worked as the dependent variable, the OLS coefficient estimate on $withdrawal_{it}$ is -0.00026 (standard error 0.000078); for labor force participation, the OLS estimate is -0.0000056 (standard error 0.0000018). This is consistent with a positive bias induce by an omitted variable which is positively correlated with both hours worked and withdrawal amount. For example, high ability workers may choose to work more hours, but have more RSA funds to withdraw because of higher earnings stemming from their more-productive abilities. This positive bias is also consistent with attenuation bias resulting from measurement error in withdrawal amount.

3.6 million adults aged 60.[20] Of these, about 66% (see Table 1) or 2.4 million would be RSA participants affected by this policy change. The estimate from column (1) of Table 3 suggests that, on average, each worker reduces labor supply by 1.425 hours per week in response to gaining access to RSA funds, which translates to 74.1 hours per person during the one-year period the policy change pushes the age threshold back. In the aggregate, with a one-year delay in RSA access, this adds approximately 177.84 million labor-hours per year, which is equivalent to 85,500 full-time 40-hours-per-week jobs. While these calculations assume a constant labor supply effect and do not take into account general equilibrium considerations, they do provide scope for what is at stake in the event of any policy change.

This paper contributes to the literature by being the first to examine how passing the RSA age threshold leads to reductions in labor supply as well as to withdrawals. There are several avenues that warrant further investigation. Labor supply decisions are often made jointly between both spouses within a household, and it would be interesting to see how one spouse reaching an age threshold affects the labor supply decision of the other spouse. The SIPP is a trove of data with special topical modules for certain waves of the survey. Using these additional data may offer further insight into how reaching the RSA age threshold affects other outcomes. Furthermore, similar analyses can be conducted with SIPP panels prior to 2008, as long as the variables in the data are measured consistently across SIPP panels. Exploiting richer data sources, such as administrative records, would also greatly add knowledge to the question of how RSA age threshold policy affects withdrawal behavior and labor supply decisions.

[20]Source: "Annual Estimates of the Resident Population by Single Year of Age and Sex for the United States: April 1, 2010 to July 1, 2014" (U.S. Census Bureau, Population Division). Available at http://factfinder2.census.gov/bkmk/table/1.0/en/PEP/2014/PEPSYASEXN

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Appendix

Robustness checks. This appendix documents checks conducted to ensure robustness of the estimates presented in the main paper. These are done for the five regressions presented in columns (1) through (5) in Table 3. The original results to each of these regressions are reprinted in row (A) of Table A1 for ease of comparison. The robustness checks are conducted for five alternative specifications of these regressions, with results presented in rows (B) through (F) in Table A1. Each cell in this table reports the point estimate and standard error (in parentheses below) for the coefficient on the explanatory variable of interest—either $(post_{it} \times RSA_i)$ or $withdrawal_{it}$, depending on the column.

Row (B) of Table A1 allows for the controlled-for age quadratic function to vary across the age 59.5 threshold by augmenting the regressions with interaction terms between the quadratic terms and a $post_{it}$ indicator. Row (C) uses a higher-order cubic polynomial function in age instead. Both these changes allow for more flexibility in how the outcomes of each specification trend with age. The estimates across all five regressions in both rows (B) and (C) are very similar to the original baseline estimates.

Row (D) omits person fixed effects from the regressions. Without them, the estimates are identified off of between- and within-individual variation in the explanatory variable of interest. Compared to the original baseline estimates (which only account for within-individual variation), the estimates in row (D) are slightly larger in magnitude, but nonetheless qualitatively similar.

Row (E) excludes person-month observations between ages 59 and 60, creating a "doughnut" sample with a one-year hole around the access age threshold. This specification addresses concerns that some RSA participants make positive withdrawals just before the age 59.5 threshold, perhaps because of exceptions allowing penalty-free access, measurement error in the withdrawal or age variables, and imperfect information.

Excluding the person-month observations immediately surrounding the age threshold allows for a cleaner identification of the effects. The estimates in columns (1) through (3) of row (E) are slightly larger in magnitude compared to the original baseline estimates. This is because the effects of passing the age threshold grow over time, as can be seen from the discussion regarding Figure 6. The IV estimates in columns (4) and (5) are now smaller in magnitude because the first stage coefficient on the instrument (column (3)) is larger.[21] Nonetheless, the five estimates in this row do not change much in relation to the original baseline estimates.

Row (F) restricts the analysis sample to person-month observations between ages 58 and 61, reducing the original sample window by two years. The opposite of what happened in row (E) now occurs here. The magnitudes of the estimates in columns (1) through (3) are smaller than the original baseline estimates. Again, this is because the effects of passing the age threshold grow over time. The IV estimates in columns (4) and (5) are also smaller in magnitude because even though the first stage coefficient on the instrument (column (3)) is smaller, the shrinking of the reduced-form effects (columns (1) and (2)) outweighs this change in the first stage.[22]

Overall, the estimates presented in Table A1 are qualitatively similar to the original results, and are more or less robust to the varying specifications.

[21] This is akin to increasing the denominator in the Wald estimator.

[22] This is akin to increasing the numerator faster than the denominator in the Wald estimator.

Table 1: Sample Summary Statistics

	(1) Overall Sample	(2) RSA Participants	(3) Non-Participants
% Female	0.551	0.541	0.569
	(0.497)	(0.498)	(0.495)
% White	0.805	0.852	0.714
	(0.396)	(0.356)	(0.452)
% Black	0.131	0.094	0.204
	(0.337)	(0.291)	(0.403)
% Asian	0.035	0.033	0.039
	(0.183)	(0.178)	(0.194)
% High School	0.897	0.956	0.779
	(0.304)	(0.204)	(0.415)
% College	0.383	0.485	0.180
	(0.486)	(0.500)	(0.384)
% Married	0.659	0.717	0.544
	(0.474)	(0.451)	(0.498)
Household Size	2.41	2.36	2.50
	(1.28)	(1.14)	(1.51)
Owns RSA	0.664	1	0
	(0.472)		
% Working	0.613	0.747	0.349
	(0.487)	(0.435)	(0.477)
Usual Hours/Week	23.90	29.63	12.56
	(20.85)	(19.46)	(18.74)
Hours if Working	38.96	39.66	36.00
	(11.03)	(10.45)	(12.77)
Total Income	2982.2	3765.7	1431.7
	(3598.2)	(4021.3)	(1717.2)
Earned Income	2315.5	3118.1	727.4
	(3592.6)	(4037.5)	(1546.6)
N	285079	189375	95704

Notes: Standard deviations in parentheses.

Table 2: Effect of RSA Access on Withdrawal Amount

Dep. Var.: Withdrawal Amount	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS
Post	22.58***	21.41**	15.14	-4.14	32.06***
	(7.70)	(8.06)	(10.37)	(12.56)	(10.29)
Post × Age		-26.75			
		(20.25)			
Post × Liquidity			0.00024		
			(0.00017)		
Post × High School				13.08	
				(12.58)	
Post × College				29.41**	
				(12.62)	
Post × Female					-17.47*
					(9.49)
Age Quadratic	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	189375	189375	185182	189375	189375
R-Square	0.002	0.002	0.003	0.003	0.002

Significance Levels: *** = 1%; ** = 5%, * = 10%

Notes: The sample of analysis comprise 2008 SIPP RSA participants age between 57 and 62 whose household has never owned a business. Withdrawal amount in May 2008 dollars per month. Age variable is normalized, with a value of 0 at age 59.5. All regressions include age quadratic function and fixed effects at the time, person, state, SIPP reference month and SIPP wave levels. Standard errors in parentheses are clustered at the state level.

Table 3: Effect of RSA Access on Labor Supply

	OLS		First Stage	IV	
	(1)	(2)	(3)	(4)	(5)
Dep. Var.:	Hours	LFP	Withdrawal	Hours	LFP
Post×RSA	-1.425***	-0.0268***	56.618***		
	(0.313)	(0.0072)	(7.647)		
Withdrawal				-0.0210***	-0.00043***
				(0.0058)	(0.00012)
Post	1.112***	0.017***	-22.553***		
	(0.22)	(0.005)	(4.525)		
Age	-1.596***	-0.031***	7.045*	-1.311***	-0.027***
	(0.073)	(0.002)	(3.641)	(0.111)	(0.003)
Age Squared	-0.124***	-0.003***	-1.185	-0.148***	-0.004***
	(0.027)	(0.001)	(0.755)	(0.031)	(0.001)
Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	285079	285079	285079	285079	285079
R-Square	0.035	0.033	0.002	0.001	0.002

Significance Levels: *** = 1%; ** = 5%, * = 10%

Notes: The sample of analysis comprise 2008 SIPP RSA participants and non-participants age between 57 and 62 whose household has never owned a business. Hours worked are usual hours per week for that month. Labor force participation (LFP) is an indicator for positive non-zero hours worked. Withdrawal amount in May 2008 dollars per month. Age variable is normalized, with a value of 0 at age 59.5. The RSA indicator alone is omitted because of the included person-level fixed effects. All regressions include fixed effects at the time, person, state, SIPP reference month and SIPP wave levels. Standard errors in parentheses are clustered at the state level.

Figure 1: Histograms of Age and Hours Worked Distribution

(a) Age

(b) Usual Hours Worked per Week

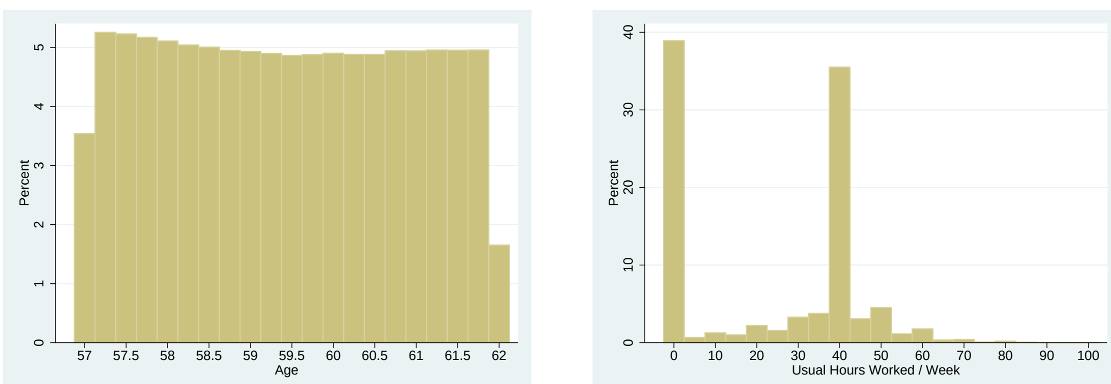


Figure 2: Proportion of RSA Participants Withdrawing by Age

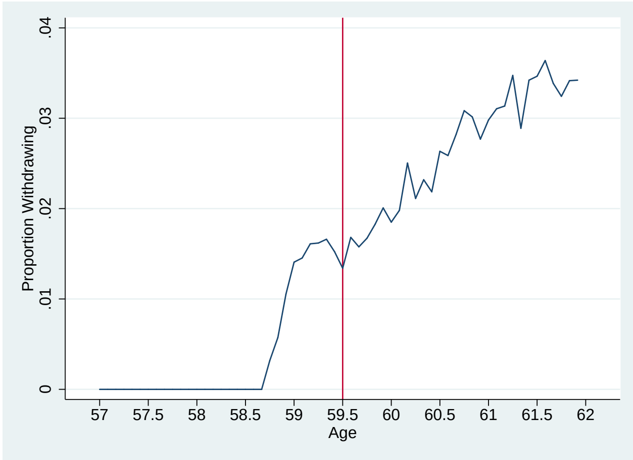
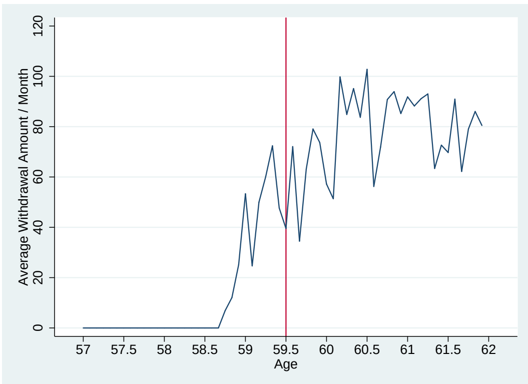
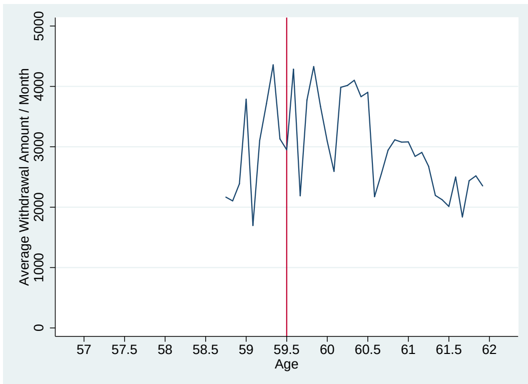


Figure 3: Average Monthly Withdrawal Amount for RSA Participants by Age

(a) Unconditional

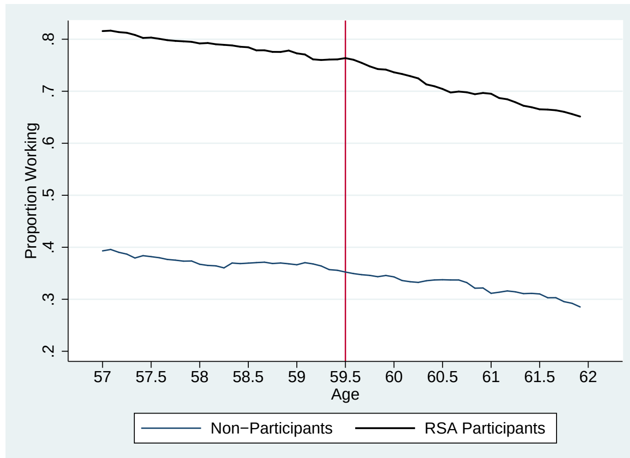


(b) Conditional on Withdrawing



Note: Withdrawal amount in May 2008 dollars per month.

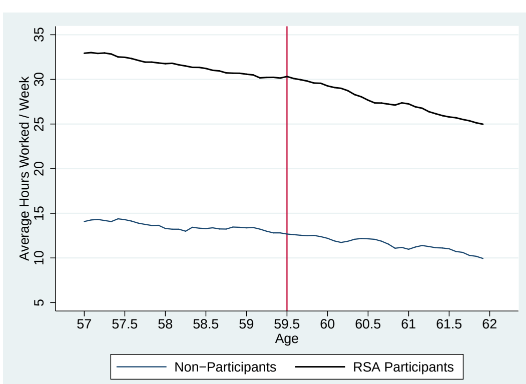
Figure 4: Labor Force Participation by Age and RSA Participation



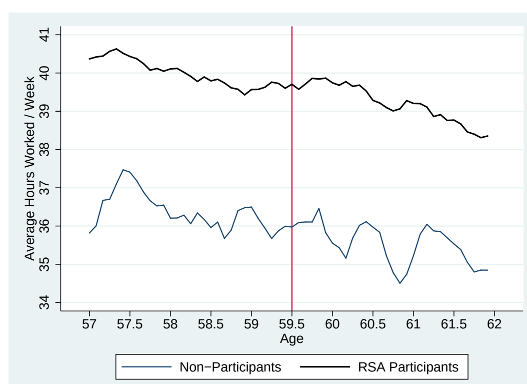
Note: Labor force participation is an indicator for positive non-zero hours worked.

Figure 5: Hours Worked per Week by Age and RSA Participation

(a) Unconditional



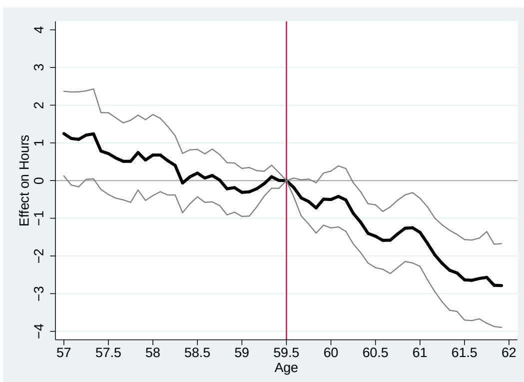
(b) Conditional on Working



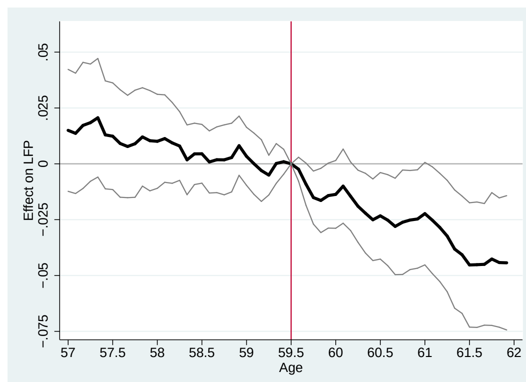
Note: Hours worked are usual hours per week for that month.

Figure 6: Effect on Labor Supply by Age

(a) Hours Worked per Week



(b) Labor Force Participation



Note: Hours worked are usual hours per week for that month. Labor force participation is an indicator for positive non-zero hours worked. The regression specification used to calculate these effects over time replaces the interaction term $post_{it} \times RSA_i$ with an interaction term between the RSA_i dummy and a set of indicator variables for each age level (by month). All regressions include age indicator variables for each age level and fixed effects at the time, person, state, SIPP reference month and SIPP wave levels. Standard errors are clustered at the state level; the gray lines plot 5% confidence intervals.

Table A1: Robustness Checks

		OLS		First Stage	IV	
		(1)	(2)	(3)	(4)	(5)
	Dep. Var.:	Hours	LFP	Withdrawal	Hours	LFP
	Coefficient on:	Post×RSA	Post×RSA	Post×RSA	Withdrawal	Withdrawal
(A)	Original Baseline Specification	-1.425***	-0.0268***	56.618***	-0.0210***	-0.00043***
		(0.313)	(0.0072)	(7.647)	(0.0058)	(0.00012)
(B)	Age Quadratic Varies Across 59.5	-1.426***	-0.0268***	56.373***	-0.0245***	-0.00045***
		(0.313)	(0.0072)	(7.638)	(0.0055)	(0.00012)
(C)	Cubic Polynomial in Age	-1.426***	-0.0268***	56.407***	-0.0253***	-0.00047***
		(0.313)	(0.0072)	(7.629)	(0.0057)	(0.00012)
(D)	Omit Person Fixed Effects	-1.967***	-0.0378***	64.855***	-0.0268***	-0.00054***
		(0.394)	(0.0108)	(5.654)	(0.0062)	(0.00016)
(E)	Sample Excluding Age 59 to 60	-1.934***	-0.0338***	75.300***	-0.0178***	-0.00036***
		(0.379)	(0.0089)	(10.405)	(0.0054)	(0.00011)
(F)	Restrict Sample to Age 58 to 61	-0.772**	-0.0176**	48.803***	-0.0159**	-0.00036***
		(0.302)	(0.0069)	(7.605)	(0.0062)	(0.00014)

Significance Levels: *** = 1%; ** = 5%, * = 10%

Notes: Each row presents estimates of the coefficient on the variable stated in the header, for the regression specification in that column. The regression specifications of the five columns correspond to those in the five columns in Table 3. The baseline sample of analysis comprise 2008 SIPP RSA participants and non-participants age between 57 and 62 whose household has never owned a business. Hours worked are usual hours per week for that month. Labor force participation (LFP) is an indicator for positive non-zero hours worked. Withdrawal amount in May 2008 dollars per month. Unless otherwise noted, all regressions include age quadratic function and fixed effects at the time, person, state, SIPP reference month and SIPP wave levels. Standard errors in parentheses are clustered at the state level.