Labor Supply and Savings Responses to Increasing the Pension Eligibility Age in South Korea

Janghyeok An* Devon Gorry† Jonathan M. Leganza‡

December 18, 2025

Abstract

We study how South Koreans responded to an increase in the full pension eligibility age. Using a regression discontinuity design, we document the causal effects of the change on several potential margins of adjustment. We find clear evidence of delayed benefit claiming, consistent with studies in other settings. However, we find little to no statistical evidence of changes in labor supply, in contrast with previous literature. We also find no changes in savings or spending. These muted responses may be, in part, because the South Korean pension is relatively new and benefit replacement rates are comparatively modest.

Keywords: public pensions, social security, retirement ages, labor supply, savings
JEL codes: H55, J26, J22, D14

*John E. Walker Department of Economics, Clemson University. Email: janghya@clemson.edu
†John E. Walker Department of Economics, Clemson University. Email: dgorry@clemson.edu
‡John E. Walker Department of Economics, Clemson University. Email: jleganz@clemson.edu

1 Introduction

Public pensions are some of the largest government programs. They account for about 18% of total public spending on average (OECD, 2023). Facing fiscal pressures and aging populations, these programs are currently being reformed all around the world. One common policy change is to increase pension eligibility ages, the ages at which people become eligible for early or full pension benefits. In fact, eligibility ages are on the rise in more than half of OECD countries (OECD, 2023).

Understanding the effects of increasing eligibility ages is important for the design of pension policy and can inform life-cycle models more generally. The effects could be wide-ranging. In response, people might adjust when they claim pension benefits or when they retire, two decisions that are widely studied in the literature (Gruber and Wise, 2004; Blundell, French and Tetlow, 2016). However, people might also adjust intensive-margin labor supply or consumption and saving decisions, which are comparatively less studied. Moreover, because responses to these reforms are likely to be context-dependent, it is crucial to amass evidence from different settings and to consider the institutional features that might give rise to differences in responses across countries.

In this paper, we provide new evidence from South Korea on how an increase in the pension eligibility age impacted the uptake of pension benefits, labor supply, earnings, consumption, and savings. Like other pension programs, the old-age pension in South Korea is characterized by a full eligibility age, when workers become eligible for full benefits (100% of the amount due based on earnings histories), as well as an early eligibility age, when workers first become eligible for reduced benefits. In 1998, South Korea began reforming their pension system, and we study the first phase of the reform, which increased the full pension eligibility age from 60 to 61 and held the early pension eligibility age constant at 55.[1] Crucially, the increase applied to people born in or after January 1953 but left people born earlier unaffected.

We exploit this sharp discontinuity in policy implementation to estimate the causal effects of the reform. Specifically, we use a regression discontinuity (RD) design and detailed survey data to compare outcomes of people born January 1953 and just after, who faced a full eligibility age of 61, to those of people born just before, who faced the original eligibility age of 60. Leveraging the breadth of our data, we study pension benefit receipt, labor supply outcomes that capture both extensive- and intensive-margin decisions (employment, full-time status,

[1]Note that this approach is similar to the last major U.S. Social Security reform, which incrementally increased the full retirement age from 65 to 67 and held the early retirement age constant at 62.

earnings, and hours worked), consumption, and saving. Since pension reforms could impact outcomes throughout one's life-cycle, we explore the impacts on the affected individuals from their 40s into their late 60s. Altogether, our analysis provides a comprehensive look at how this reform affected the main behaviors that people might adjust. Moreover, we study these behaviors over a critical period, when people age from prime earnings years into retirement and later life.

We first document responses in 2013, which is the critical year when people born around the cutoff date turn 60, the pre-reform full pension eligibility age. We find clear evidence that increasing the eligibility age led to delayed benefit claiming. Specifically, we show large, discontinuous decreases in the probability of receiving pension benefits and in benefit amounts. In contrast, we find little to no evidence that the reform impacted employment, labor market earnings, consumption, or savings at age 60. Standard RD graphical analyses show that these outcomes appear to evolve smoothly through the birthdate cutoff. The point estimates from the corresponding regression analysis are not statistically different from zero, and they do not consistently point to one sign. However, the confidence intervals are wide enough that we cannot rule out moderate effects in either direction.

We next extend our analysis along several dimensions and continue to find no evidence of any labor market, consumption, or savings responses. First, we estimate RD regressions at various ages and find little to no effects as people age through their 40s and 50s and into their 60s. Second, we study cumulative outcomes to investigate whether potentially smaller year-by-year responses that would be difficult to detect statistically amount to a larger and detectable effect overall. However, this analysis also yields no statistically significant findings. Third, we investigate response heterogeneity by gender and find no effects for men or women.

Overall, we find strong evidence that the initial increase in the pension eligibility age in South Korea caused people to delay benefit claiming. However, we find much less evidence of any meaningful changes in labor supply, consumption, or saving decisions. Given the strong evidence of labor supply adjustments in other settings (e.g., Staubli and Zweimüller, 2013; Nakazawa, 2025), it is worth reflecting on the differences in the South Korean context.

There are two institutional features that we speculate are important for understanding the results in our setting. First, the old-age pension in South Korea is relatively new. The program began in 1986 and had only existed for 12 years before this reform was implemented. The relatively late implementation of the public pension system limited the contribution period for the individuals in our study, which resulted in lower benefit levels. Second, the program replaces a comparatively modest degree of income relative to other pension

programs. Across OECD countries, average full-career workers entering the labor market in recent years are projected to receive a pension with a net replacement rate of 61%, but the corresponding figure in South Korea is less than 40% (OECD, 2023). These relatively modest benefits might mean that the change in the eligibility age was not impactful enough to generate responses in labor supply, spending, or saving, even though it was significant enough to lead to delays in benefit claiming.

Our paper relates to a large literature that studies the influence of public pensions on economic outcomes (Feldstein and Liebman, 2002; Krueger and Meyer, 2002; Gruber and Wise, 2004; Blundell, French and Tetlow, 2016; Coile, 2016). We relate closely to other studies using quasi-experimental methods to evaluate increases in pension eligibility ages. Many papers focus on benefit claiming, retirement, or employment responses to this type of reform (e.g., Mastrobuoni, 2009; Behaghel and Blau, 2012; Manoli and Weber, 2016; Atalay, Barrett and Siminski, 2019; Geyer et al., 2020; Geyer and Welteke, 2021; Seibold, 2021; Gruber, Kanninen and Ravaska, 2022; Morris, 2022; Lalive, Magesan and Staubli, 2023; Deshpande, Fadlon and Gray, 2024; García-Miralles and Leganza, 2024*a*; Nakazawa, 2025). Relatively few papers examine consumption or savings responses (e.g., Attanasio and Brugiavini, 2003; Lachowska and Myck, 2018; Slavov et al., 2019; Etgeton, Fischer and Ye, 2023; García-Miralles and Leganza, 2024*b*). On the whole, the evidence from other countries indicates that increasing eligibility ages tends to delay benefit claiming and retirement.

We contribute by providing evidence on this important topic from a new setting, South Korea. We are aware of only two other working papers that study the increases in the South Korean eligibility age, although their focus is different than ours (Kim, 2024; Han and Qian, 2025).[2] We also contribute by providing a more comprehensive analysis that includes several labor supply outcomes, as well as spending and saving outcomes. Looking at a wide range of behaviors is important for developing a better understanding of how people respond to increasing eligibility ages in practice. While structural work often emphasizes multiple response margins (e.g., French, 2005; Gustman and Steinmeier, 2005), reduced form work like ours often focuses on estimating either retirement or consumption and savings responses.

[2]Kim (2024) studies labor supply responses to a later phase of the reform that we do not study because it occurs at the same time as another policy that limits how firms could dismiss workers based on age. Han and Qian (2025) study both the first and later phase of the reform to highlight how the increase in the eligibility age can interact with other pension rules. They use a difference-in-differences design to compare the evolution of benefit claiming and retirement for the affected 1953 cohort to that of the unaffected 1952 cohort, before and after age 50. In contrast, we use an RD design that uses data on exact date of birth to compare responses in the same year for people born on either side of the birthdate cutoff. This approach allows us to estimate responses at many different ages, including earlier ones, which is important because the reform might have impacted people throughout the life cycle, even before typical retirement ages.

2 Policy Environment

2.1 The Old-Age Pension in South Korea

The National Pension Act of 1986 began establishing a comprehensive public pension scheme in South Korea. The scheme was originally implemented in 1988 and applied to workers at firms with more than ten employees. Several extensions over the next decade expanded eligibility. Near-comprehensive coverage began in 1999, when all income-earning people between the ages of 18 and 59 were required to enroll in the pension system.[3] We emphasize that the timing of the introduction of the national old-age pension means that people in our sample are making labor supply and consumption-savings decisions at older ages in the face of a relatively new pension program that is changing incentives as they are reaching older ages.

2.2 Benefit Eligibility and Calculations

To become eligible for pension benefits, a person must have contributed to the system for a sufficient period, originally 15 years. Benefit amounts are then calculated using a formula that depends on earnings and contribution histories, like in many other countries, but also on the number of dependents a person has, the average earnings of all other members enrolled in the pension program, and on a government-specified factor that is the same for everyone and is designed to adjust benefits to target an overall replacement rate for the system.

The full pension eligibility age—the age at which a person can claim 100% of the benefits they are due—was originally set to 60. The early eligibility age—the earliest age at which a person can claim reduced benefits—was originally set to 55. For each year of early claiming, benefits were reduced by 5%. Originally, benefits did not increase for claiming after the full pension eligibility age.

However, individuals earning high incomes received reductions in benefits. People with sufficiently high earnings could not claim benefits before reaching the full pension eligibility age. Upon reaching eligibility, they also experienced up to 50% reductions in benefits received before age 65.[4] This policy discouraged early claiming for higher-income individuals.

[3]Individuals covered by alternative pension programs are exempt from mandatory participation in the public pension program. These exemptions primarily apply to government employees, private school faculty and staff, members of the military, and postal service personnel.

[4]Workers with earnings that exceeded the average monthly earnings of all contributors to the system faced these benefit reductions. Benefits received at the full pension eligibility age were reduced by 50% but gradually increased back to full amounts by age 65.

2.3 Pension Reforms

Facing fiscal pressures and an aging population, South Korea began major reforms to their pension system in the late 1990s. The first reform, which we exploit in our analysis, was the 1998 National Pension Reform. From the perspective of our study, the key component of the reform was an increase in the full pension eligibility age. In 1998, the government announced one-year, step-wise increases in the full pension eligibility age from 60 to 65 to be phased in over several birth cohorts. The first phase of the increase in the full pension eligibility age applied to the 1953 birth cohort. People born before 1953 faced the original full pension eligibility age of 60. In contrast, people born on or after January 1, 1953 (and through 1956) faced a new eligibility age of 61. We focus on this initial increase. Importantly, while other components of the reform applied to everyone, the increase in the pension age applied only to people born in or after 1953, which provides useful variation that we can exploit for identification.[5]

Still facing budgetary pressures, the government introduced another reform in 2007. This reform further decreased benefits for everyone by adjusting the replacement-rate constant used to calculate benefits. It also made important changes to the way benefits were adjusted based on claiming age, with the goal of encouraging delayed claiming. Specifically, it increased the penalty for early claiming from 5% per year to 6% per year, and it introduced a credit for delaying claiming past the full pension eligibility age. Originally, delaying claiming past the full pension eligibility age yielded a 6% annual increase in benefits up to age 65, but this annual increase was further raised to 7.2% in 2012.

2.4 Reform Implementation and Economic Incentives

Our empirical analysis exploits the discontinuous nature of the increase in the full pension eligibility age by birth cohort. While the reforms made several changes to the pension system overall, the crucial change from the perspective of our research design is the difference between the full pension eligibility age for those born on or after January 1, 1953 compared to those born earlier.

[5]This reform also made adjustments to benefit calculations such as changing the government-specified replacement-rate constant. It also altered the number of required contribution years such that people with 10 years of contributions could claim a (reduced) pension, and contribution-years required to receive a non-reduced pension increased to 20. Importantly, these changes applied to all birth cohorts. Moreover, later cohorts experienced additional rule changes that we do not study. For example, cohorts born in 1954 and beyond faced different benefit reduction rules for high earners. In addition, cohorts born in 1957 and beyond experienced further increases in the full pension eligibility age.

Figure 1 illustrates the economic incentives of these reforms for relevant birth cohorts. It also highlights an important distinction between how the reforms were intended to be implemented and how they played out in practice.

Panel (a) illustrates the effects of the initial increase announced in 1998, when people born in 1953 were 45 years-old. The graph plots pension benefits (as a percentage of the full amount) against claiming age. The gray line corresponds to the 1949 through 1952 cohorts, who can be thought of as our control group. The black line corresponds to the 1953 through 1956 cohorts, who can be thought of as our treatment group. A person from the control cohort would receive 100% of their benefits if they were to claim at age 60. In contrast, a person from the treatment cohort would have to wait to claim until age 61 to receive 100% of their benefits. For treated individuals, claiming at 60 would be claiming one year early and would thus be subject to the originally-specified 5% decline in pension benefits. Each additional year of early claiming would result in reduced benefits. In this year, because there was not yet a benefit for delaying claiming, the lines for the treatment group and the control group are both flat at 100% after age 61.

Panel (b) illustrates the effects of the 2007 reform. This reform took place when people born in 1953 were 54 years-old, or right before they reached the early pension eligibility age. Importantly, this reform did not introduce cohort-specific changes, but rather (i) increased the penalty for claiming early to 6% and (ii) introduced a 6% increase in benefits for delaying claiming. The effect of this reform can be summarized as straightening out the benefit-claiming-age profiles for both our treatment and control groups, as well as a change in the slope of the lines from 5% per year to 6% per year. This graph highlights how the increase in the full pension eligibility age was a benefit cut: holding claiming age constant, the increase in the eligibility age would have reduced pension wealth by 6% for the treated cohorts.

However, we note that the reform was not ultimately implemented in this way. If people born in 1953 claimed early pension benefits when they reached age 55 in 2008 (only one year after the 2007 reform was announced), they received 70% of their full pension benefits, instead of the 64% illustrated in graph (b). In practice, the 2007 reform was not implemented until 2013, when the 1953 cohort was 60 years-old. This situation meant that the reform did not impact the 1953 cohort if they claimed before age 60.

Graph (c) of Figure 1 shows the economic effects of all the reforms for the 1953 affected cohort compared to the 1949-52 unaffected cohorts in practice. The increase in the full pension eligibility age did not change benefits for people who would have otherwise claimed early, before age 60. For people who would have claimed at 60, the reform ultimately

resulted in a 6% decrease in pension wealth. Furthermore, because the 2012 reform changed the credit for delaying claiming from 6% to 7.2%, people who would have claimed at or after 61 experienced a 7.2% decrease in pension wealth.

In response to the reduction in pension wealth, people might (i) delay the age at which they claim benefits, (ii) work longer on the extensive margin or more on the intensive margin, or (iii) save more or consume less. While one might expect a strong role for extensive-margin labor supply responses through delayed retirements, it is important to consider all of these potential response margins, as we do in our empirical analysis below.[6]

3 Data

We use longitudinal data from the Korean Labor and Income Panel Survey (KLIPS). The survey began in 1998 and initially included about 13,000 individuals in 5,000 households from urban areas. In 2009 and 2018, the survey incorporated an additional 1,415 and 5,044 households, respectively. These additions made the data nationally representative. To access the data, we use publicly-available files for each survey wave that correspond to 1998 through 2019. We conduct our analysis at the individual level, and almost all of our variables are defined at the individual level; however, we use household-level data files to define spending and saving variables.

3.1 Key Variables

To study pension benefits, we use two variables. The first is an indicator variable that takes the value of one if the individual receives old-age pension benefits. The second is a continuous variable for the amount of old-age pension benefits received (including zeros). We convert this variable and all other monetary variables to U.S. dollars using year-specific exchange rates from FRED and then adjust for inflation to express all values in 2013 USD.[7]

To study labor supply, we use four variables. The first variable is an employment indicator variable that equals 1 if the respondent reported working either during the past week or at

[6]Recent work suggests multiple reasons to expect extensive-margin labor supply responses. For example, Caliendo et al. (2025) show how adjusting retirement can be a dominant way to insure consumption in response to wealth shocks. Moreover, Seibold (2021) shows how, even in the absence of financial incentives, pension eligibility ages can impact retirement through reference dependence. Still, other margins could be important. A longstanding literature on social security and saving traditionally focuses on the extent to which public pension wealth displaces private savings (e.g., Feldstein, 1974; Munnell, 1974; Blinder, Gordon and Wise, 1983), making spending and saving crucial outcomes to consider.

[7]See here: https://fred.stlouisfed.org/series/AEXKOUS.

present, including those who are temporarily absent from their regular job. The second is a continuous variable for the amount of monthly earnings received after taxes. The third is an indicator variable that takes the value of one for individuals who work full-time, and the fourth is a continuous variable for the number of hours typically worked in a week.

To study consumption and savings decisions, we use continuous variables for typical monthly household level spending and saving.[8]

Finally, we use several other variables to conduct our regression discontinuity analysis. Importantly, we have a variable for exact date of birth, which we use as the running variable in our design. We also use information on gender, marital status, and college education as control variables in a robustness check.

3.2 Analysis Sample

To construct our analysis sample, we begin with all individuals in the KLIPS data between 1998 and 2019, which includes 302,105 observations on 34,740 unique individuals. We first limit our sample to relevant birth cohorts by keeping only individuals born between 1949 and 1956. This restriction leaves us with 38,019 observations on 3,765 individuals. Next, we drop a small number of observations with missing information on birth month and day, leaving us with 38,008 observations on 3,758 individuals born between 1949 and 1956. However, many of these individuals, namely those added to the survey in 2018, are only in the sample for two years, and our core analysis focuses on 2013, when those affected by the reform turn 60. When we study this critical year, we have observations on 1,668 unique individuals.

Table 1 presents summary statistics for our sample in 2012, the year immediately preceding the critical year 2013. Consistent with our sample being centered on the 1953 birth cohort, the average age of a person in our sample in 2012 was 59. The sample is about 46% male and 82% married, and only 13% of people completed college. Roughly 61% of the sample was employed, monthly earnings were $1,004 USD, typical monthly spending was $1,873, and typical monthly savings were $534.[9]

[8]The monthly spending variable comes from a survey question that asks about household living expenses including expenditures on food, housing, clothes, education, health and medical fees, and other utilities. The monthly saving variable comes from a survey question that asks about savings including bank deposits, installment savings, insurance, savings in individual pension accounts, and savings in private mutual savings clubs. We note that these household-level variables have a one-year look-back reference period. For example, in the 1999 survey wave, the survey asks about household spending and saving during 1998. We therefore merge these household-level variables in year t with the individual-level data from year $t-1$, so that the timing of all our outcomes is consistent and corresponds to the year of the survey.

[9]Appendix Table A.1 presents summary statistics by gender. The data show that men are more attached to the labor force than women. We explore heterogeneity by gender after presenting the full-sample results.

4 Identification Strategy

4.1 Regression Discontinuity Design

To identify the causal effects of increasing the full pension eligibility age, we use an RD design. The idea is to derive identification from the discontinuous change in the eligibility age that is contingent on birthdate. People born before January 1953 face the original pension eligibility age of 60. In contrast, people born in or after January 1953 face a full pension eligibility age of 61. Our RD approach compares outcomes of people born just after the cutoff to those of people born just before the cutoff.

To make these comparisons, we estimate

$$y_i = \alpha + \beta \cdot 1[x_i \geq c] + \gamma \cdot (x_i - c) + \delta \cdot (x_i - c) \cdot 1[x_i \geq c] + \varepsilon_i, \tag{1}$$

where y_i is an outcome for individual i (like an indicator for being employed), x_i is the running variable, birthdate, c is the birthdate cutoff, and ε_i is an error term. The coefficient of interest is β. It is an estimate of the discontinuous change in the outcome at the birthdate cutoff and captures the effect on an increase in the full pension eligibility age. We sometimes refer to it as the "RD estimate."

In the baseline specification, we cluster standard errors at the household level. Because we are using survey data with relatively limited sample sizes, we use a bandwidth equal to four birth cohorts on either side of the cutoff. This approach allows us to use as much data as possible, as people born four years after the cutoff date are affected by a later phase of the reform that introduces further increases to the full eligibility age and other major changes. After presenting our main estimates, we assess the sensitivity of our results to specification choices and find that our takeaways are robust.

4.2 Assessment of Validity

The identifying assumption for a causal interpretation of our RD estimates is that other factors that influence the outcomes do so smoothly through the birthdate cutoff. We use our design to estimate discontinuities in outcomes at the cutoff. To interpret those discontinuities as the causal effect of the increase in the full pension eligibility age, we need to assume that the outcomes would have evolved smoothly absent the policy change.

We assess standard threats to the validity of this assumption. First, we note that one key threat to most RD designs is manipulation of the running variable. However, the run-

ning variable in our design is birthdate, which cannot be manipulated at the time of the policy. It is therefore likely that classical issues with manipulation are unlikely in our setting. Nonetheless, we still analyze the density of the running variable. Appendix Figure A.1 presents a histogram of birthdates. In the spirit of McCrary (2008), we superimpose on the histogram smoothed values and confidence intervals from local polynomial regressions of the number of individuals on month of birth on both sides of the cutoff. We conclude that the density is smooth through the cutoff. Second, we test for discontinuous changes in covariates at the cutoff. Appendix Table A.2 reports the results from estimating equation (1) using control variables as outcomes. Reassuringly, we find no evidence of potentially problematic changes in control variables.

5 Results

5.1 Preliminary Analysis of Trends

We begin with a preliminary investigation of age trends. Our goals are to provide (i) initial evidence on how two key outcomes, pension benefit receipt and employment, evolved across ages for people in affected versus unaffected cohorts and (ii) a broader look at the data before we zoom in on estimating causal effects using the RD design.

Figure 2 plots these age trends. Graph (a) plots receipt of pension benefits and graph (b) plots employment. Each graph shows the average of the outcome across ages for two different groups: the black line represents those affected by the reform (born between 1953 and 1956) and the gray line represents those unaffected by the reform (born between 1949 and 1952).

There are a few key takeaways. First, graph (a) shows that not many people claimed pension benefits early, before the full eligibility age. For example, the fraction of people in the unaffected birth cohorts who receive public pension benefits increased only modestly from ages 55 to 59 before increasing sharply at age 60. This pattern contrasts with that in other countries where there are sharp and large increases in benefit claiming at the earliest eligibility age. Second, graph (a) provides some prima facie evidence that the reform impacted benefit claiming around the full pension eligibility age. The black line tracks the gray line through the early claiming period but benefit claiming for the affected cohort did not increase sharply until age 61, which is their full eligibility age.

Third, graph (b) shows that employment is generally falling at older ages, but it does not indicate sharp breaks or rapid declines in employment after age 60, when benefit claiming

begins to increase. Interestingly, the employment trend for unaffected cohorts around 60 looks broadly similar to that for affected cohorts. If reaching the full pension eligibility age is not a key driver of aggregate employment for pre-reform cohorts, then changing the pension eligibility age might not generate strong employment responses. On the whole, these descriptive patterns suggest that the reform induced major changes in benefit claiming but did not affect aggregate employment around pension eligibility ages.

5.2 Responses at Age 60

Next, we turn to our RD design to document the causal effects of the reform. We begin our RD analysis by documenting the effects in 2013, the critical year when people born at the cutoff reach age 60. If not for the reform, those born at the cutoff and later would have been able to claim their full pension benefits at this age.

Before estimating the regressions detailed above, we conduct a standard RD graphical analysis, which provides a visual assessment of the effects of the reform. Specifically, we plot binned means of outcome variables against the running variable, birthdate, and we superimpose on these plots regression lines and confidence intervals from estimating linear relationships between the outcomes and the running variable using the underlying, unbinned data on either side of the cutoff.

Figure 3 presents RD graphs for pension benefit claiming. Each graph corresponds to a different outcome variable. Graph (a) shows pension benefit receipt and graph (b) shows the amount of pension benefits in U.S. dollars. The graphs illustrate large, sharp, and visually clear declines in the receipt and amount of pension benefits at the cutoff. These patterns are consistent with the reform leading to delayed benefit claiming. People born after the cutoff are still eligible to claim their benefits, but their full pension eligibility age is not until one year later, and very few of them claim at age 60.

Figure 4 presents RD graphs for labor outcomes, spending, and saving, which capture other key margins that people could adjust in response to the reform. Graph (a) shows there is some evidence of an increasing trend in employment, indicating that people born later tend to be more likely to work at older ages. But the employment rate was roughly 60% on either side of the cutoff, and there is no visual evidence of a discontinuous change in this outcome. Similarly, graphs (b), (c), and (d) show little to no evidence of discontinuous changes in monthly earnings, hours worked, or the probability of working full-time. Overall, the patterns suggest that the reform had little impact on labor supply at age 60.

Graphs (e) and (f) of Figure 4 show monthly household spending and saving at age 60,

respectively. There is some evidence of an overall upward trend in both spending and saving across birth cohorts, as is the case for employment, but there is no evidence of discontinuous changes at the cutoff. Households on either side of the cutoff spent roughly $1,800 per month and saved approximately $450 a month.

Next, we turn to our regression analysis to quantify magnitudes and assess the statistical significance of the discontinuities in outcomes. Table 2 presents the results from estimating equation (1). Each column in the table corresponds to a regression for a different outcome variable at age 60. For each regression we report the RD estimate and standard error, the mean of the dependent variable for people born before the cutoff, and the number of households and observations.

Outcomes in columns (1) and (2) are for the receipt of pension benefits. Column (1) indicates that the reform caused a 16.8 percentage point decline in the probability of receiving pension benefits at age 60. This estimate is statistically different from zero and represents a 56% decline compared to the mean for the control group of people born before the cutoff (0.302). Column (2) indicates an average decline in monthly benefits of $59. This estimate represents a 55% decline compared to the control group mean ($107). While large in percentage terms, the magnitude of this estimate highlights the fact that the old-age pension benefits are not likely a dominant source of income for the individuals in our sample. Even so, the graphical results and the corresponding point estimates provide strong evidence that people responded to the increase in the full pension eligibility age by delaying benefit claiming.

Outcomes in columns (3) through (6) correspond to labor market outcomes. Consistent with the graphical evidence showing no labor supply response, none of these point estimates are statistically different from zero. Taken at face value, the point estimate for employment would suggest, if anything, a modest 2.9 percentage point decline in employment. The estimates for earnings, hours worked, and full-time status are not particularly small in magnitude, but they also do not consistently point to one sign (the estimates for earnings and full-time employment are positive but the estimate for hours worked is negative). The estimate for monthly earnings would suggest an increase that more than offsets the decline in monthly pension benefits, but it is imprecise and not statistically different from zero.

Finally, columns (7) and (8) correspond to household spending and saving, respectively. We find no statistically significant change in either of these outcomes, consistent with the smooth patterns in the graphs. However, again note that the magnitudes of the estimates are not small relative to the measured decrease in monthly pension benefits received.

Overall, the results do not show evidence of meaningful adjustments to labor supply, spending, or saving at age 60 in response to the increase in the full pension eligibility age, despite clear and strong benefit-claiming responses. Next, we conduct robustness checks and then extend our analysis along several dimensions.

5.3 Robustness Checks

We assess the robustness of our estimates along two broad dimensions. First, we examine the sensitivity of our estimates to our choice of bandwidth (48 months). Figure 5 shows how our estimates change when we use different bandwidths. Each graph corresponds to a different outcome variable and plots RD estimates and 95-percent confidence intervals for a wide range of bandwidths from 24 months to 48 months.[10] Our results are insensitive to the choice of bandwidth.

Second, we examine the sensitivity of our estimates to other regression specification choices. Appendix Table A.3 shows how our baseline regression estimates from Table 2 compare to the estimates we obtain when we add control variables, use triangular weights, or use survey weights. Across specifications, the point estimates are broadly similar to one another, especially considering the standard errors. The statistical significance of our findings is also unaffected by these regression specification choices. We continue to conclude that there is strong evidence that the reform impacted benefit claiming but little to no evidence that the reform impacted labor supply, spending, or saving at age 60.

5.4 Responses at Different Ages

Next, we leverage our RD design and the time horizon of our data to document responses at different ages, with the idea that the reform could impact behaviors throughout the life cycle. Figure 6 illustrates the effects at different ages. Each graph within the figure corresponds to a different outcome and plots RD estimates and 95-percent confidence intervals from separate regressions estimated using data from different years. The x-axis indicates the age of a person born just after the cutoff in that year. The vertical dashed line draws attention to the point estimates for 2013, when the treated individuals reach age 60; these estimates are the same as those discussed above and presented in Table 2.

Graphs (a) and (b) show pension benefit claiming. The graphs illustrate large declines in pension benefits at age 60 and 61 as well as smaller reductions in pension benefits at other

[10]Note that we cannot go beyond 48 months because those cohorts are affected by another increase in the pension eligibility age.

close ages. These patterns are consistent with the reform not just inducing delayed claiming among people who would have otherwise claimed at 60, but also for people who would have otherwise claimed at 59, 61, or 62.

Graphs (c), (d), (e), and (f) show employment, monthly earnings, hours worked, and full-time status, respectively. Overall, the results indicate a lack of evidence for a meaningful labor supply response at other ages. On the one hand, graph (d) might point to weak evidence of an increase in monthly earnings as people approach 60 and reach the early pension eligibility ages. On the other hand, only one of those estimates is statistically different from zero at the 95-percent level. Zooming further out, of the 88 point estimates in the four labor supply graphs, only one is statistically different from zero.

Graphs (g) and (h) show spending and saving. We also find no statistically significant evidence that people adjust these behaviors at other ages. Not one of the point estimates is statistically different from zero, and the pattern of the estimates does not reveal a meaningful trend.

The evidence from these estimates across ages indicates that people responded to the reform primarily by delaying the claiming of benefits. The results do not point to clear adjustments to work, savings, or spending in the years leading up to or after pension eligibility ages. However, the estimates are not precisely-estimated zeros that would allow us to rule out any meaningful effects. For example, the upper bounds of the confidence intervals on the estimates for monthly earnings when individuals are in their late 50s are just-under $500. It would be difficult to detect relatively modest year-by-year responses with our sample sizes. Therefore, we next study overall, within-person responses over a number of years to investigate whether potentially smaller adjustments over time translate to detectable changes in outcomes that reflect overall behaviors.

5.5 Overall Responses Before Reaching Age 60

To assess whether overall behaviors change, we construct outcome variables that capture within-person averages of key outcomes over time. The basic idea is to study a cumulative outcome, but because individuals in our data are observed for a different number of years, we construct average outcomes instead. Specifically, for employment, earnings, savings, and spending, we define individual-level outcomes that are the average of these variables between 1998 (the first year of our data) and 2012 (the year people born at the cutoff turn 59). We then estimate our RD regressions using these average outcomes as the dependent variables of interest.

Table 3 displays the results. We find positive coefficients for average monthly earnings and average monthly spending, but negative coefficients for average employment (i.e., the fraction of years employed) and average monthly savings. None of the point estimates are statistically different from zero, although the magnitude of the estimate for average monthly earnings is, consistent with the by-age results above, meaningful in size.

Still, we conclude that there is no statistical evidence of overall responses. Next, we investigate response heterogeneity by gender to assess whether the full-sample results are potentially masking different types of responses by men and women.

5.6 Heterogeneous Responses by Gender

We estimate our RD regressions separately for men and women. While men and women could respond differently for many reasons, we emphasize that men in South Korea have traditionally been more attached to the labor force than women. This fact means that the men in our sample are more likely than women to have contribution histories that make them eligible for public pension benefits.

Appendix Table A.4 reports the estimates for age 60 by gender. Here we see significant declines in pension uptake and monthly benefits for both men and women, consistent with the main results. These declines are larger in magnitude for men, but the percent decline relative to the control group mean is similar. This pattern is consistent with the idea that men are more likely to be eligible for the pension program and thus more directly affected by the reform. We also find no significant effects for men or women on labor market outcomes, spending or saving.

Appendix Figure A.2 presents the by-age RD estimates for men, and Appendix Figure A.3 presents the by-age RD estimates for women. Broadly speaking, the patterns for both men and women look similar to the main sample: we find decreases in pension benefits for men and women around age 60, consistent with delayed benefit claiming, but we find little to no statistical evidence of changes in the other outcomes at any age.

Moreover, while still not statistically different from zero, the point estimates for monthly earnings and the probability of working full-time are more elevated for men than for women, which might provide some weak evidence of increased labor supply by some men who were affected by the reform. Overall though, the lack of statistically significant estimates across genders reinforces our general conclusion that the reform seems to have had little, if any, impact on labor supply, consumption, or savings.

6 Conclusion

In this paper, we use detailed survey data and a regression discontinuity design to provide new evidence from South Korea on how increasing the full pension eligibility age impacts the receipt of pension benefits, labor supply, saving, and spending. Consistent with prior research, we find strong evidence of delayed benefit claiming. However, we find little to no evidence of changes in labor supply. We also find little to no evidence of changes in saving or spending.

On the one hand, our results could be surprising, as other research has tended to find strong labor supply responses to changes in pension eligibility ages. On the other hand, we have shown that the full pension eligibility age does not appear to be a major driver of aggregate employment rates at older ages in South Korea since there are not steep declines in employment at pension ages, at least for the birth cohorts we study. Thus, perhaps increasing the eligibility age should not have been expected to have an outsized influence on labor market exits. Moreover, our point estimates are imprecise, and some of the magnitudes of the estimates at age 60 (like that for earnings) are reasonably similar in size to the estimated decline in benefits received at 60.

Taken together, our results indicate that people in South Korea responded to the increase in the full pension eligibility age primarily by waiting longer than they otherwise would have to claim their public pension benefits. More broadly, our study highlights the importance of accumulating evidence from a wide variety of settings on policy questions surrounding public pension and retirement reform. The way in which people respond and how strongly they may or may not react to policy changes is likely to vary across countries. This variation will depend on features of the pension system, like its overall level of generosity, as well as the broader economy, like how attached people are to the labor market at older ages.

References

Atalay, Kadir, Garry F Barrett, and Peter Siminski. 2019. "Pension incentives and the joint retirement of couples: Evidence from two natural experiments." *Journal of Population Economics*, 32(3): 735–767.

Attanasio, Orazio P, and Agar Brugiavini. 2003. "Social security and households' saving." *Quarterly Journal of Economics*, 118(3): 1075–1119.

Behaghel, Luc, and David M Blau. 2012. "Framing social security reform: Behavioral responses to changes in the full retirement age." *American Economic Journal: Economic Policy*, 4(4): 41–67.

Blinder, Alan S., Roger H. Gordon, and Donald E. Wise. 1983. "Social Security, Bequests and the Life Cycle Theory of Saving: Cross-sectional Tests." *The Determinants of National Saving and Wealth*, , ed. Franco Modigliani and Richard Hemming, 89–122.

Blundell, Richard, Eric French, and Gemma Tetlow. 2016. "Retirement incentives and labor supply." In *Handbook of the Economics of Population Aging*. Vol. 1, 457–566. Elsevier.

Caliendo, Frank N, Erin Cottle Hunt, Aspen Gorry, and Jonathan M Leganza. 2025. "Retirement and Consumption Insurance."

Coile, Courtney C. 2016. "Economic determinants of workers' retirement decisions." *A Collection of Reviews on Savings and Wealth Accumulation*, 271–296.

Deshpande, Manasi, Itzik Fadlon, and Colin Gray. 2024. "How Sticky Is Retirement Behavior in the United States?" *Review of Economics and Statistics*, 106(2): 370–383.

Etgeton, Stefan, Björn Fischer, and Han Ye. 2023. "The effect of increasing retirement age on householdsâ savings and consumption expenditure." *Journal of Public Economics*, 221: 104845.

Feldstein, Martin. 1974. "Social security, induced retirement, and aggregate capital accumulation." *Journal of Political Economy*, 82(5): 905–926.

Feldstein, Martin, and Jeffrey B Liebman. 2002. "Social security." *Handbook of Public Economics*, 4: 2245–2324.

French, Eric. 2005. "The effects of health, wealth, and wages on labour supply and retirement behaviour." *The Review of Economic Studies*, 72(2): 395–427.

García-Miralles, Esteban, and Jonathan M Leganza. 2024*a*. "Joint retirement of couples: Evidence from discontinuities in Denmark." *Journal of Public Economics*, 230: 105036.

García-Miralles, Esteban, and Jonathan M Leganza. 2024*b*. "Public pensions and private savings." *American Economic Journal: Economic Policy*, 16(2): 366–405.

Geyer, Johannes, and Clara Welteke. 2021. "Closing routes to retirement for women: How do they respond?" *Journal of Human Resources*, 56(1): 311–341.

Geyer, Johannes, Peter Haan, Anna Hammerschmid, and Michael Peters. 2020. "Labor market and distributional effects of an increase in the retirement age." *Labour Economics*, 65: 101817.

Gruber, Jonathan, and David A. Wise, ed. 2004. *Social Security Programs and Retirement around the World: Micro-Estimation.* University of Chicago Press.

Gruber, Jonathan, Ohto Kanninen, and Terhi Ravaska. 2022. "Relabeling, retirement and regret." *Journal of Public Economics*, 211: 104677.

Gustman, Alan L, and Thomas L Steinmeier. 2005. "The social security early entitlement age in a structural model of retirement and wealth." *Journal of Public Economics*, 89(2-3): 441–463.

Han, Jeehoon, and Wei Qian. 2025. "Digging Beneath the Hood of Multidimensional Pension Rules."

Kim, Bongseop. 2024. "The Effects of Increasing the Pensionable Age on Labor Supply: Evidence from Korea."

Krueger, Alan B, and Bruce D Meyer. 2002. "Labor supply effects of social insurance." *Handbook of Public Economics*, 4: 2327–2392.

Lachowska, Marta, and Michał Myck. 2018. "The effect of public pension wealth on saving and expenditure." *American Economic Journal: Economic Policy*, 10(3): 284–308.

Lalive, Rafael, Arvind Magesan, and Stefan Staubli. 2023. "How social security reform affects retirement and pension claiming." *American Economic Journal: Economic Policy*, 15(3): 115–150.

Manoli, Dayanand S, and Andrea Weber. 2016. "The effects of the early retirement age on retirement decisions." National Bureau of Economic Research.

Mastrobuoni, Giovanni. 2009. "Labor supply effects of the recent social security benefit cuts: Empirical estimates using cohort discontinuities." *Journal of Public Economics*, 93(11-12): 1224–1233.

McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics*, 142(2): 698–714.

Morris, Todd. 2022. "The unequal burden of retirement reform: Evidence from Australia." *Economic Inquiry*, 60(2): 592–619.

Munnell, Alicia H. 1974. "The impact of social security on personal savings." *National Tax Journal*, 27(4): 553–567.

Nakazawa, Nobuhiko. 2025. "The effects of increasing the eligibility age for public pension on individual labor supply: Evidence from Japan." *Journal of Human Resources*, 60(1): 102–128.

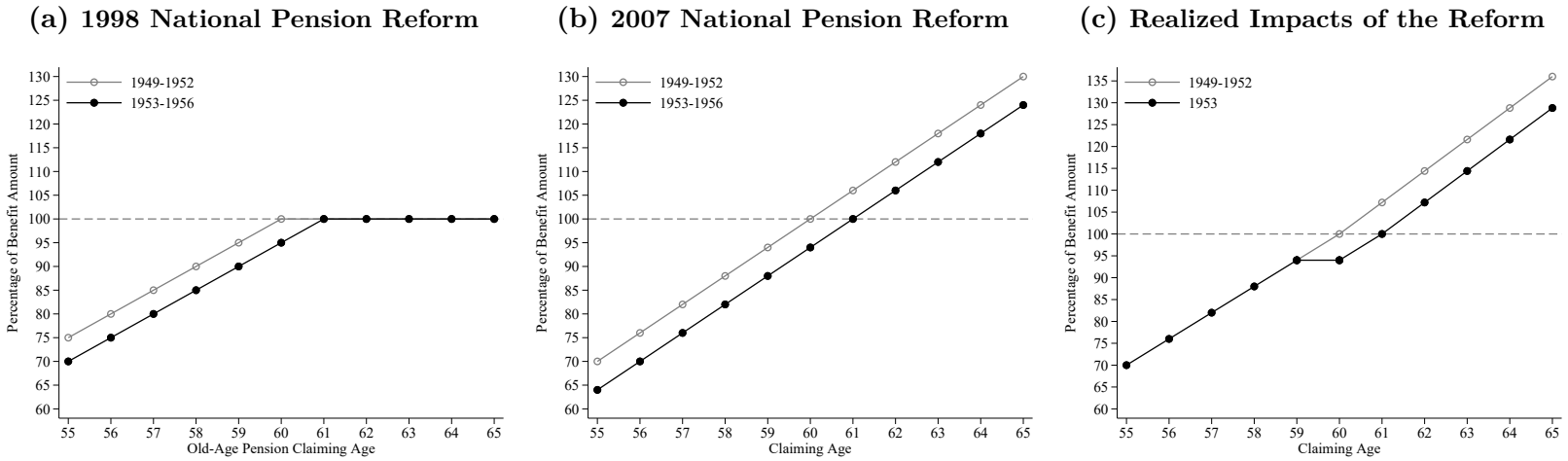
OECD. 2023. "Pensions at a Glance 2023: OECD and G20 Indicators." `https://doi.org/10.1787/678055dd-en`.

Seibold, Arthur. 2021. "Reference points for retirement behavior: Evidence from german pension discontinuities." *American Economic Review*, 111(4): 1126–1165.

Slavov, Sita, Devon Gorry, Aspen Gorry, and Frank N Caliendo. 2019. "Social security and saving: An update." *Public Finance Review*, 47(2): 312–348.

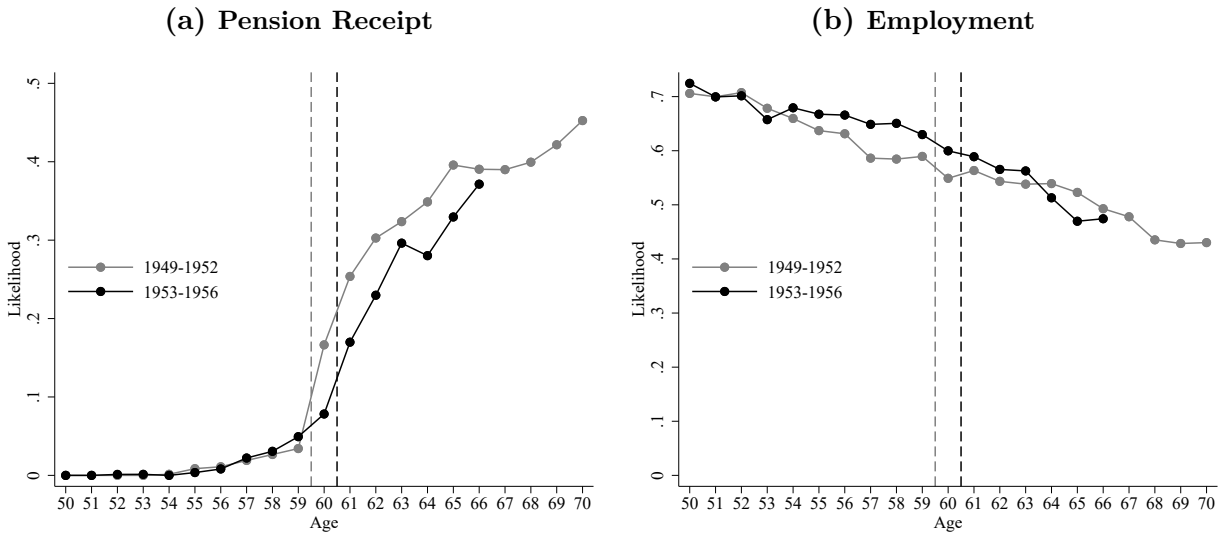
Staubli, Stefan, and Josef Zweimüller. 2013. "Does raising the early retirement age increase employment of older workers?" *Journal of Public Economics*, 108: 17–32.

Figure 1: Public Pension Benefit Schedules for Affected and Unaffected Cohorts



Notes: This figure illustrates how the reforms impacted pension benefit schedules. Each panel plots pension benefit schedules by cohort and age, and each line represents different birth cohorts. Panel (a) shows that the 1998 reform delayed the full pension eligibility age based on individuals' birthdates. Panel (b) shows that the 2007 reform increased the penalties for early old-age pension claims and introduced incentives for delaying benefit claims. Panel (c) shows the effects of these reforms for the 1953 cohort compared to the 1949-1952 cohorts in practice. Each panel assumes no additional earnings upon reaching the full retirement age.

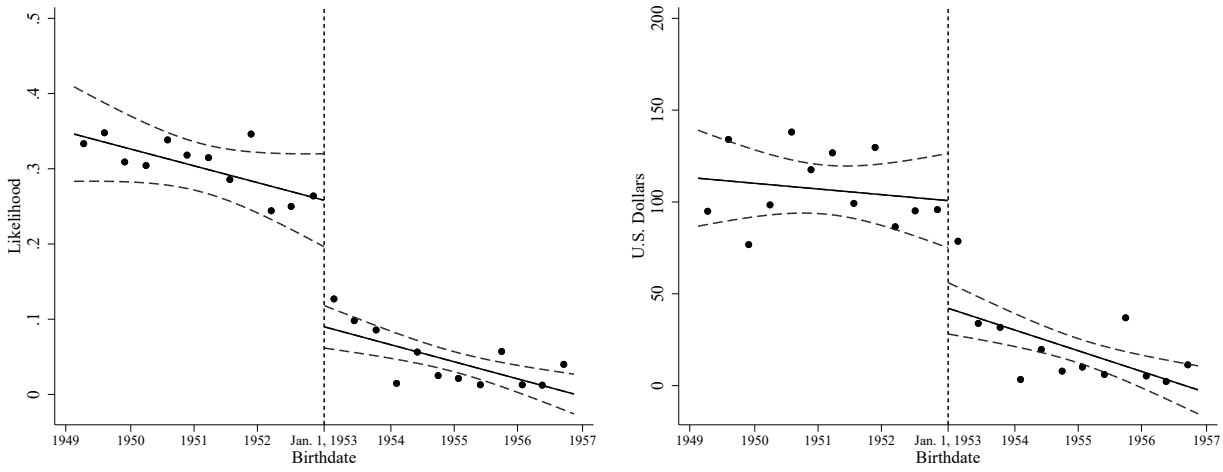
Figure 2: Trends in Pension Benefit Receipt and Employment by Birth Cohort



Notes: This figure presents age trends in pension benefit claiming behavior and employment across cohorts subject to different full pension eligibility ages. The gray lines are for the 1949 to 1952 cohorts, who faced a full pension eligibility age of 60. The black lines are for the 1953 to 1956 cohorts, who faced a full eligibility age of 61. Panel (a) plots the fraction of individuals receiving pension benefits at each age. Panel (b) plots the fraction of individuals who are employed at each age.

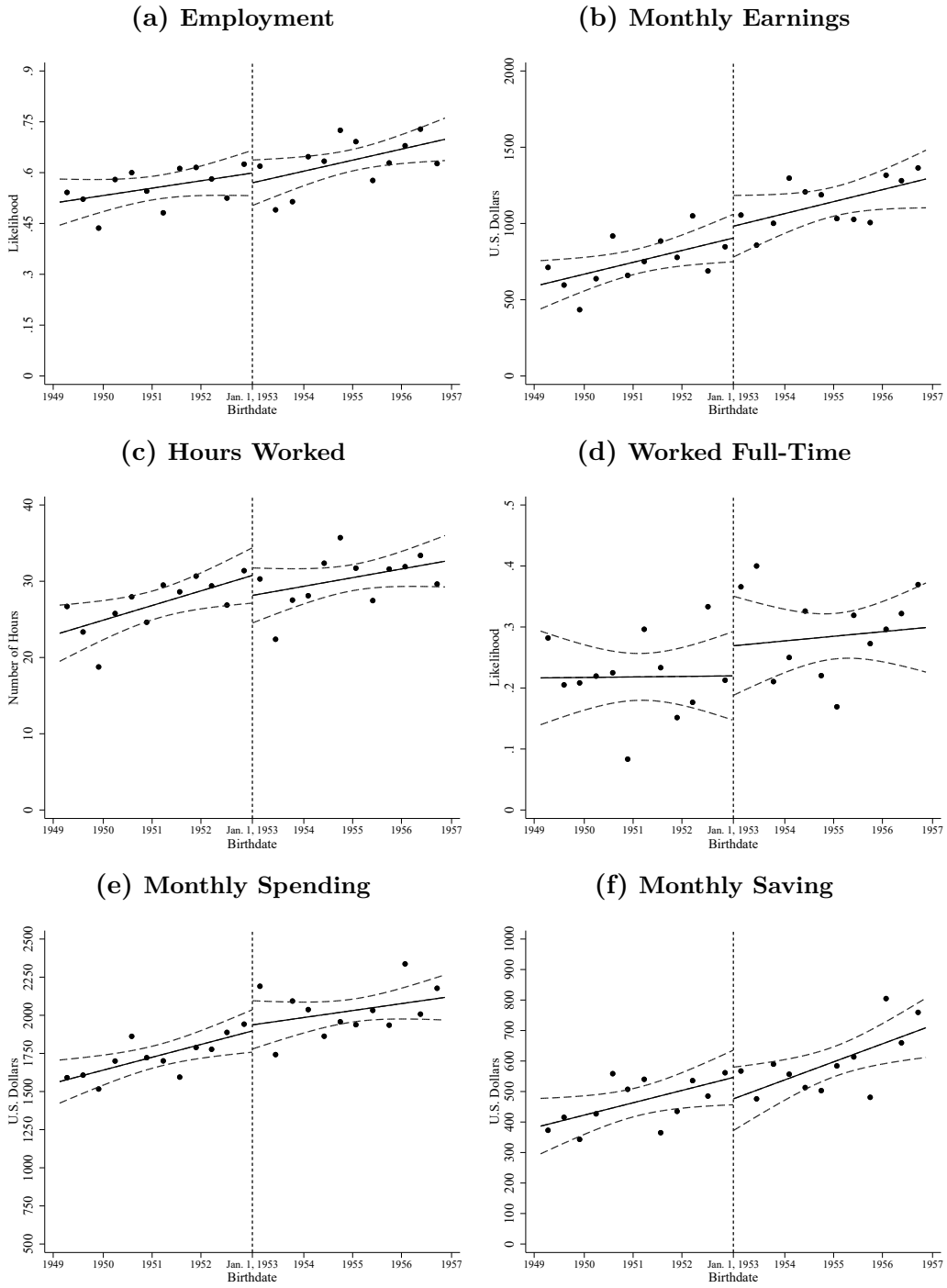
Figure 3: Effect of the Reform on Pension Benefits at Age 60

(a) Pension Receipt **(b) Monthly Pension Benefits**



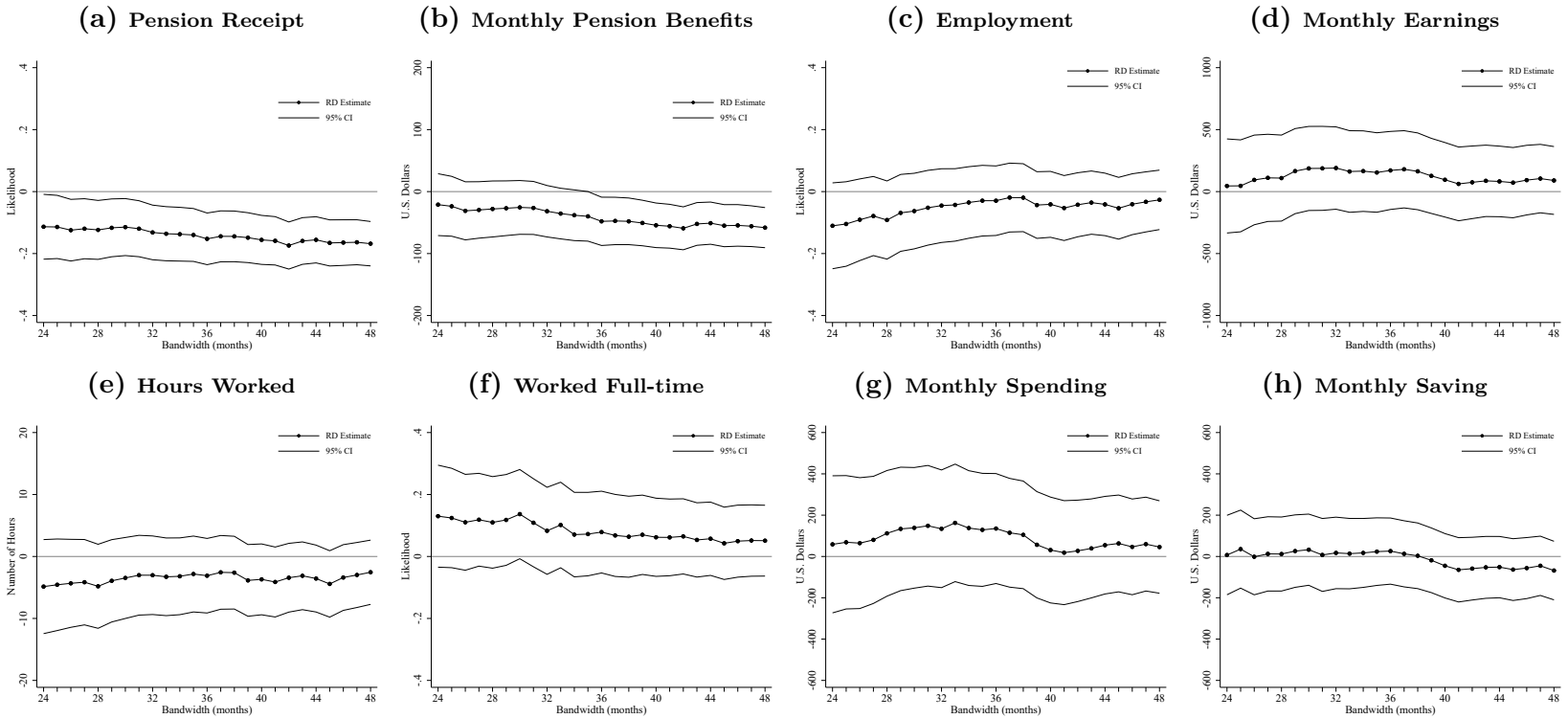
Notes: This figure illustrates the effect of the increase in the full pension eligibility age on the receipt of pension benefits in 2013. Panel (a) is for an indicator for pension benefit receipt. Panel (b) is for monthly pension benefits. Each panel is constructed as follows. The running variable along the horizontal axis is birthdate. The cutoff is January 1, 1953 and is denoted by the dashed vertical line. The dots are average outcomes in 4-month bins. The superimposed regression lines and 95-percent confidence intervals are based on the underlying, unbinned data.

Figure 4: Effects of the Reform on Labor Supply, Spending, and Saving at Age 60



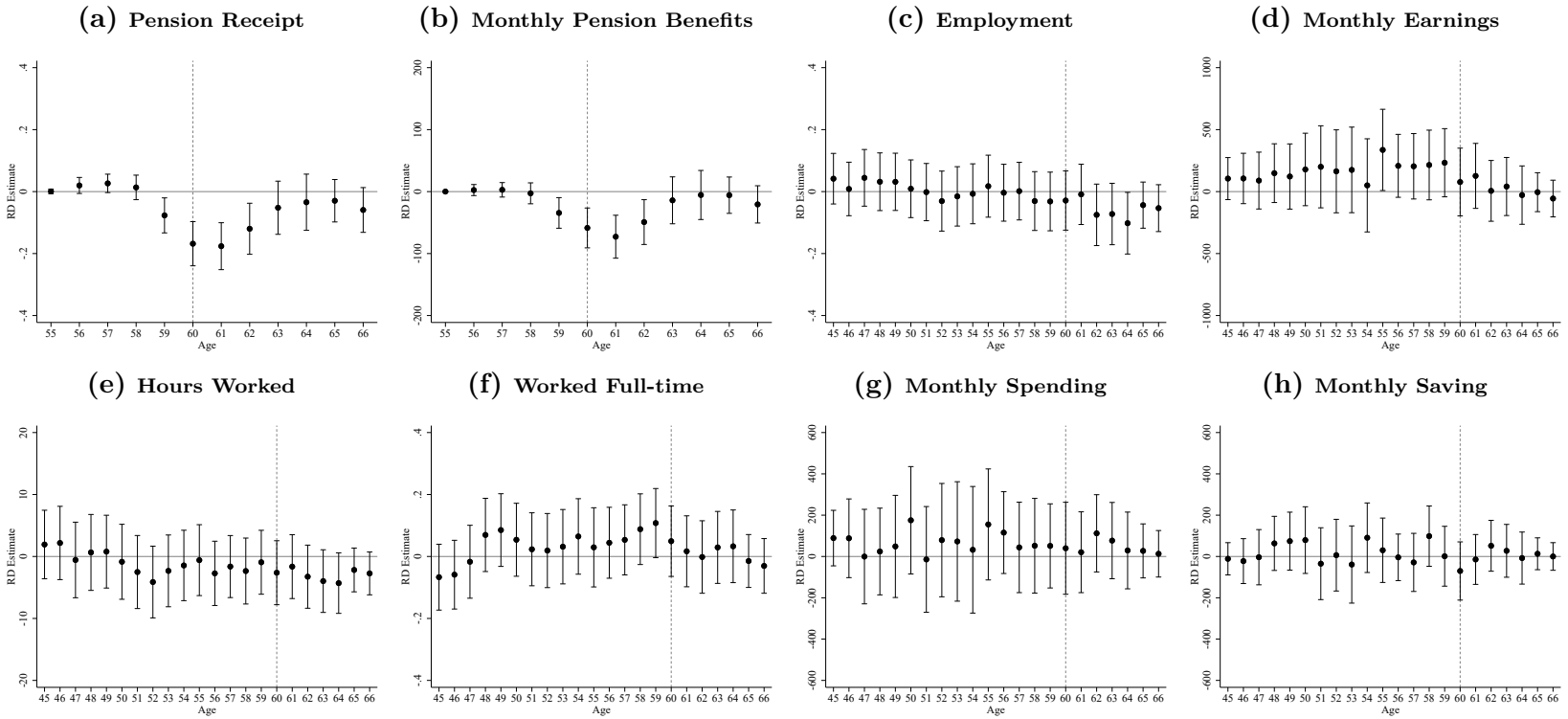
Notes: This figure illustrates the effects of the reform on labor supply, spending, and saving in 2013. Each panel corresponds to a different outcomes. Panel (a) is for employment, panel (b) is for monthly earnings, panel (c) is for hours worked, panel (d) is for full-time work status, panel (e) is for monthly spending, and panel (f) is for monthly saving. See the notes of Figure 3 for more details on how each panel is constructed.

Figure 5: Robustness of Main Estimates to Bandwidth Selection



Notes: This figure illustrates the sensitivity of our estimates to choice of bandwidth. Each panel corresponds to a different outcome variable and plots the RD estimates and 95-percent confidence intervals from using different bandwidths ranging from 24 to 48 months. Panel (a) is for an indicator for pension benefit receipt. Panel (b) is for monthly pension benefits. Panel (c) is for an employment indicator. Panel (d) is for monthly earnings. Panel (e) is for hours worked. Panel (f) is for full-time work status. Panel (g) is for monthly spending. Panel (h) is for monthly saving.

Figure 6: Effects of the Reform at Different Ages



Notes: This figure illustrates the effects of the reform on outcome variables at different ages. Each panel corresponds to a different outcome variable and plots RD estimates and 95-percent confidence intervals from estimating separate regressions for different years. The x-axis indicates the age of a person born just after the cutoff in that year. The vertical dashed line denotes the estimates for 2013, which is the critical year that people born after the cutoff turn 60. Panel (a) is for an indicator for pension benefit receipt. Panel (b) is for monthly pension benefits. Panel (c) is for an employment indicator. Panel (d) is for monthly earnings. Panel (e) is for hours worked. Panel (f) is for full-time work status. Panel (g) is for monthly spending. Panel (h) is for monthly saving.

Table 1: Summary Statistics

	Mean (1)	SD (2)	Individuals (3)
Panel A. Demographics			
Age	59.3	2.3	1,686
Male	0.46	0.50	1,686
Married	0.82	0.38	1,686
College	0.13	0.34	1,686
Panel B. Outcomes			
Employment	0.61	0.49	1,686
Monthly Earnings	1,004	1,374	1,685
Weekly Hours Worked	30.0	30.0	1,686
Worked Full-Time	0.28	0.45	1,057
Monthly Spending	1,873	1,055	1,640
Monthly Saving	534	733	1,639

Notes: This table presents summary statistics. The underlying sample consists of individuals born between 1949 and 1956 observed in 2012, one year before the critical year that people born at the cutoff turn 60. Panel A presents statistics on demographis. Panel B presents statistics on labor market, spending, and saving outcomes.

Table 2: Regression Discontinuity Estimates at Age 60

	Pension Receipt (1)	Monthly Pension Benefits (2)	Employment (3)	Monthly Earnings (4)	Weekly Hours Worked (5)	Worked Full-time (6)	Monthly Spending (7)	Monthly Saving (8)
RD Estimate	-0.168***	-58.6***	-0.029	77.4	-2.6	0.049	39.8	-70.3
	(0.036)	(16.4)	(0.049)	(139.2)	(2.6)	(0.058)	(113.5)	(71.8)
Mean	0.302	106.8	0.556	753.1	27.0	0.218	1,733.0	467.2
Clusters (Households)	1,334	1,334	1,334	1,333	1,334	896	1,311	1,311
Observations (Individuals)	1,668	1,668	1,668	1,667	1,668	1,017	1,640	1,640

Notes: This table presents regression discontinuity (RD) estimates for the effects of the reform on outcomes in 2013, which is when people born at the cutoff turn 60. The RD estimates come from estimating equation (1). Standard errors clustered at the household level are in parentheses. The mean refers to the average value of the outcome for individuals born between 1949 and 1952.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

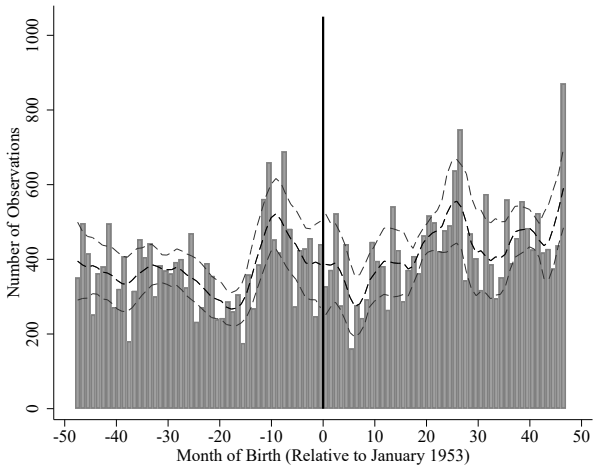
Table 3: Regression Discontinuity Estimates for Overall Responses Before Age 60

	Fraction of Years Employed (1)	Average Monthly Earnings (2)	Average Monthly Spending (3)	Average Monthly Saving (4)
RD Estimate	-0.010	168.2	38.6	-14.6
	(0.037)	(113.8)	(91.3)	(44.9)
Mean	0.628	863.1	1,821.0	446.9
Clusters (Households)	1,330	1,329	1,330	1,330
Observations (Individuals)	1,663	1,662	1,663	1,663

Notes: This table reports regression discontinuity (RD) estimates for the effects of the reform on outcomes that capture overall responses before age 60. The outcomes are constructed as within-person averages between 1998 (the first year of our data) and 2012 (the year people born at the cutoff turn 59). The RD estimates come from estimating equation (1). Standard errors clustered at the household level are in parentheses. The mean refers to the average value for individuals born between 1949 and 1952.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

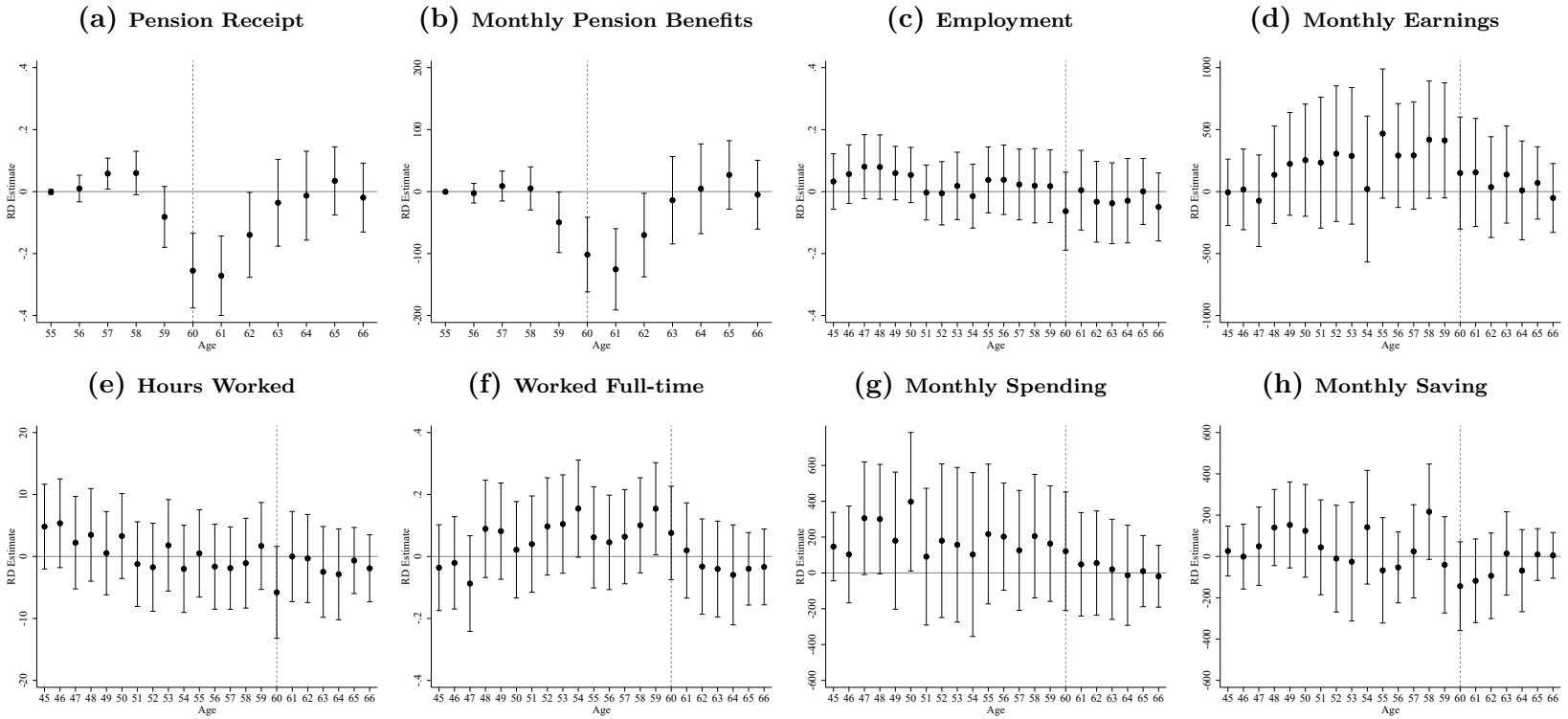
Appendix A Additional Figures and Tables

Figure A.1: Histogram of the Running Variable



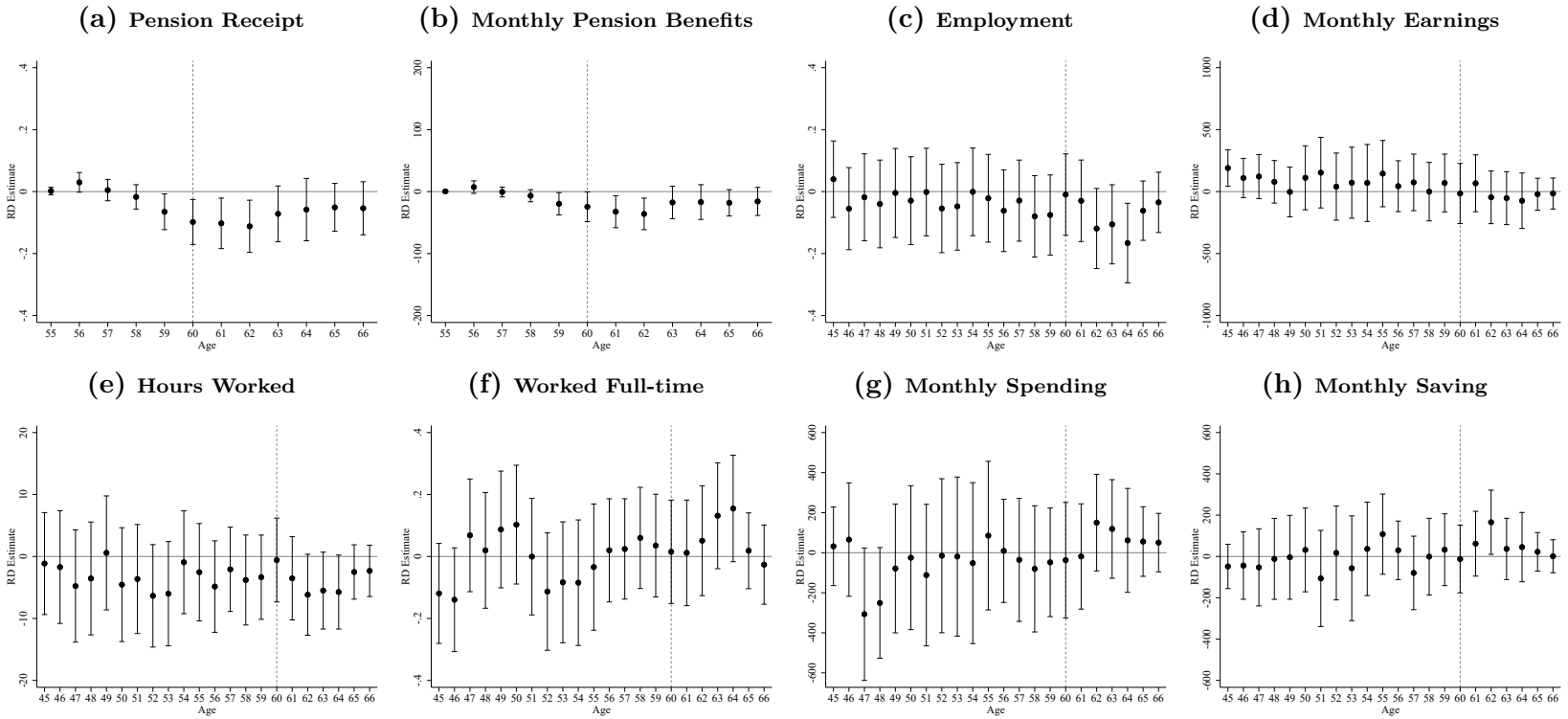
Notes: This figure presents a histogram of the running variable, birthdate, measured in months of birth and defined relative to the cutoff, January 1953. Superimposed on the histogram are smoothed values and confidence intervals from kernel-weighted local polynomial regressions of the number of observations of individuals born on month of birth, run separately on each side of the cutoff

Figure A.2: Effects of the Reform at Different Ages for Men



Notes: This figure illustrates the effects of the reform on outcome variables at different ages for men. Each panel corresponds to a different outcome variable and plots RD estimates and 95-percent confidence intervals from estimating separate regressions for different years when the sample is limited to men. The x-axis indicates the age of a person born just after the cutoff in that year. The vertical dashed line denotes the estimates for 2013, which is the critical year that people born after the cutoff turn 60. Panel (a) is for an indicator for pension benefit receipt. Panel (b) is for monthly pension benefits. Panel (c) is for an employment indicator. Panel (d) is for monthly earnings. Panel (e) is for hours worked. Panel (f) is for full-time work status. Panel (g) is for monthly spending. Panel (h) is for monthly saving.

Figure A.3: Effects of the Reform at Different Ages for Women



Notes: This figure illustrates the effects of the reform on outcome variables at different ages for women. Each panel corresponds to a different outcome variable and plots RD estimates and 95-percent confidence intervals from estimating separate regressions for different years when the sample is limited to wommen. The x-axis indicates the age of a person born just after the cutoff in that year. The vertical dashed line denotes the estimates for 2013, which is the critical year that people born after the cutoff turn 60. Panel (a) is for an indicator for pension benefit receipt. Panel (b) is for monthly pension benefits. Panel (c) is for an employment indicator. Panel (d) is for monthly earnings. Panel (e) is for hours worked. Panel (f) is for full-time work status. Panel (g) is for monthly spending. Panel (h) is for monthly saving.

Table A.1: Summary Statistics by Gender

	Men			Women		
	Mean (1)	SD (2)	Individuals (3)	Mean (4)	SD (5)	Individuals (6)
Panel A. Demographics						
Age	59.2	2.3	780	59.4	2.3	906
Married	0.87	0.33	780	0.78	0.41	906
College	0.19	0.39	780	0.08	0.27	906
Panel B. Outcomes						
Employment	0.79	0.40	780	0.46	0.50	906
Monthly Earnings	1,671	1,579	780	429	809	905
Weekly Hours Worked	39.1	23.8	780	22.2	26.0	906
Worked Full-Time	0.31	0.46	627	0.23	0.42	430
Monthly Spending	2,087	1,094	759	1,688	984	881
Monthly Saving	636	802	759	447	655	880

Notes: This table presents summary statistics by gender. The underlying sample consists of individuals born between 1949 and 1956 observed in 2012, one year before the critical year that people born at the cutoff turn 60. Panel A presents statistics on demographis. Panel B presents statistics on labor market, spending, and saving outcomes.

Table A.2: Regression Discontinuity Estimates At Age 60 Using Covariates as Outcomes

	Male	Married	High Education
	(1)	(2)	(3)
RD Estimate	0.016	0.007	-0.032
	(0.049)	(0.039)	(0.034)
Mean	0.437	0.805	0.118
Clusters (Households)	1,334	1,334	1,334
Observations (Individuals)	1,668	1,668	1,668

Notes: This table presents regression discontinuity (RD) estimates for 2013, which is when people born at the cutoff turn 60, when we use covariates as outcome variables. The RD estimates come from estimating equation (1). Standard errors clustered at the household level are in parentheses. The mean refers to the average value for individuals born between 1949 and 1952.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.3: Robustness of Main Estimates to Specification Checks

	Pension Receipt (1)	Monthly Pension Benefits (2)	Employment (3)	Monthly Earnings (4)	Weekly Hours Worked (5)	Worked Full-time (6)	Monthly Spending (7)	Monthly Saving (8)
A. Baseline	-0.168***	-58.6***	-0.029	77.4	-2.6	0.049	39.8	-70.3
	(0.036)	(16.4)	(0.049)	(139.2)	(2.6)	(0.058)	(113.5)	(71.8)
B. Add Controls	-0.172***	-59.7***	-0.036	82.5	-3.1	0.055	60.6	-65.9
	(0.035)	(15.8)	(0.046)	(127.4)	(2.5)	(0.057)	(101.9)	(70.1)
C. Use Triangular Weights	-0.143***	-41.7**	-0.046	106.2	-3.4	0.081	75.3	-23.6
	(0.041)	(19.3)	(0.054)	(151.5)	(2.9)	(0.064)	(130.7)	(77.3)
D. Use Survey Weights	-0.141***	-52.7***	-0.033	48.1	-2.6	0.041	12.6	-108.0
	(0.041)	(18.7)	(0.056)	(177.9)	(2.9)	(0.071)	(139.3)	(84.7)

Notes: This table reports the robustness of the regression discontinuity (RD) estimates to the inclusion of controls and the use of different weights. Each row corresponds to a different regression specification. Row A reproduces the baseline estimates. Row B adds control variables to the regression. Row C uses triangular weights. Row D uses survey weights. For each specification, standard errors are clustered at the household level and are in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.4: Regression Discontinuity Estimates At Age 60 by Gender

	Pension Receipt (1)	Monthly Pension Benefits (2)	Employment (3)	Monthly Earnings (4)	Weekly Hours Worked (5)	Worked Full-time (6)	Monthly Spending (7)	Monthly Saving (8)
Panel A. Men								
RD Estimate	-0.255***	-101.7***	-0.063	149.8	-5.8	0.076	120.9	-143.9
	(0.061)	(30.6)	(0.064)	(230.2)	(3.8)	(0.077)	(168.5)	(109.6)
Mean	0.490	191.3	0.716	1,261.6	35.9	0.238	1,958.7	618.2
Observations (Individuals)	771	771	771	771	771	597	758	758
Panel B. Women								
RD Estimate	-0.098***	-24.6**	-0.010	-15.5	-0.6	0.015	-37.3	-12.4
	(0.037)	(12.2)	(0.067)	(123.7)	(3.4)	(0.085)	(146.9)	(83.5)
Mean	0.155	41.1	0.432	358.0	20.1	0.193	1,558.0	350.1
Observations (Individuals)	897	897	897	896	897	420	882	882

Notes: This table presents regression discontinuity (RD) estimates for the effects of the reform on outcomes in 2013, which is when people born at the cutoff turn 60, by gender. Panel A is for men. Panel B is for women. The RD estimates come from estimating equation (1). Standard errors clustered at the household level are in parentheses. The mean refers to the average value of the outcome for individuals born between 1949 and 1952.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$