The Effects of Increasing the Eligibility Age for Public Pension on Individual Labor Supply: Evidence from Japan*

Nobuhiko Nakazawa †

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Abstract

This paper investigates the effects of increasing the eligibility age for public pension on workers' retirement decisions, focusing on recent Japanese public pension reforms. In Japan, the pensionable age for Employees' Pension Insurance benefits gradually increased from 60 to 65 for males over the course of a decade. Using individual-level restricted-use data and a regression discontinuity design, I find that raising the pensionable age for flat-rate benefits by one year increases male employment at the critical ages by about 7-8 percentage points. Individual labor supply responses are heterogeneous across closeness to the implementation date due to anticipatory responses. I also find some evidence of spillovers from an affected husband to his wife and children and effects on other labor market outcomes.

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†Department of Economics, University of California, San Diego. Email: nnakazaw@ucsd.edu

1 Introduction

Does changing the eligibility age for public pension affect individual labor supply? As populations age, social security programs impose an increasing financial burden and pose potential threats to fiscal sustainability. Thus, public pension reforms are an increasingly debated topic among policymakers. Any reforms could have large impacts on the economy through changes in individual retirement decisions and changes in other dimensions, such as savings and earnings. Comprehensively investigating and quantifying the effects of reforms are crucial to optimal design of public pension programs.

Among countries, Japan has the highest ratio of elderly people in the world (United Nations (2017)).[1] To address the increased cost associated with the aging population, the Japanese government decided to raise the eligibility age for receiving public pension benefits. This reform was implemented in 2001 and gradually raised the male pension eligibility age for Employees' Pension Insurance (EPI) flat-rate benefits by one year every three years, starting from age 60 and ending at age 65. The reform was later extended to women, and the female eligibility age was raised from 60 to 65 in the same way, but five years after the male reform. In this paper, I mainly study the period when male workers were directly affected.

To estimate the causal effects of increasing the eligibility age on behaviors and outcomes, I employ a regression discontinuity design (RDD). Since this reform affects specific birth cohorts by age and gender, I can identify causal effects by locally comparing neighboring birth cohorts as it is phased in. To study a broad array of behaviors, I compile a unique dataset from restricted-use government data, spanning 30 years from 1986 through 2015. The data are uniquely suitable to analyze many generations over long periods and several previously under-analyzed margins.

I find that raising the public pensionable age by one year increases male employment at the critical ages by 7-8 percentage points. I also find that raising the pensionable age increases earnings and savings relative to non-affected cohorts.

[1] Specifically, life expectancy of the Japanese population is 84 years (WHO (2017)), and the ratio of the population aged 60 or over to the total population is 33% (United Nations (2017)), both of which are the highest in the world.

The labor supply and savings responses to raising the eligibility age are heterogeneous across cohorts depending on the number of years between the announcement and implementation date; older cohorts respond more, even though all affected cohorts face the same one-year loss of flat-rate benefits relative to the control cohorts. This suggests younger affected cohorts, who had more time to anticipate, are better able to smooth. The labor supply response is also stronger for affected males who live with dependent family members, such as parents, children, or grandchildren, and larger for males with less savings. I also find some evidence for spillovers from an affected husband to other family members, in terms of labor supply; the wife and children of an affected husband also increase their labor supply, responding to the delay of the husband's eligibility.

This paper builds on a large literature that investigates the relationship between social security incentives and individual labor supply. Much of the literature finds evidence that workers are responsive to financial incentives by exploiting different variations and empirical strategies (e.g., Krueger and Pischke (1992), Blau (1994), Samwick (1998), Coile and Gruber (2000), Mastrobuoni (2009), Staubli and Zweimüller (2013), and Manoli and Weber (2016)).[2] I am contributing to this literature with evidence that workers are responsive to financial incentives from a new empirical setting with a sharper loss in benefits.

This paper also contributes to the growing discussion of the role of *heterogeneity*. Recent papers examine the heterogeneity of behavioral responses to social security reforms across different groups that share common characteristics.[3]I find that individuals differentially respond to the same one-year loss of benefits across cohorts depending on the number of years between announcement and implementation; empirical evidence on decreasing treatment effects depending on the scope for anticipation is new to the literature.[4] The second contribution of the analysis is thus I find that individuals respond

[2]Descriptive evidence across developed countries is also summarized in Gruber and Wise (2000), Coile and Gruber (2004), and Coile et al. (2018).

[3]For example, Behaghel and Blau (2012) show labor supply responses are larger for individuals with higher cognitive skills. Similarly, Hanel and Riphahn (2012) emphasize the importance of educational background in terms of the magnitudes of workers' responsiveness. Staubli and Zweimüller (2013) find a heterogeneous response across individual health status and wage level.

[4]My empirical result is consistent with Mastrobuoni (2006), which theoretically shows that early-

differentially in a manner that is consistent with an important role for anticipation effects.

My study also contributes to the literature on *spillover* effects within families. Changing financial incentives for an old worker could also affect the retirement behavior of the spouse. Though most of the existing literature investigates spillover effects within couples,[5] I investigate spillovers from the affected head husband to his children as well as his wife. I find some evidence that the children and wife of an affected husband increase their labor supply, suggesting there exist some coordination and network effects within households.

Finally, beyond investigating labor supply, my analysis encompasses important *margins* including savings, consumption, private pensions, and measures of both physical and mental health. Previous studies generally have access to a more limited number of outcome variables.[6] In contrast, I am able to observe individual working statues, earnings, savings, consumption, private pensions, and measures of both physical and mental health from government restricted-use data. The empirical evidence on savings is most novel, and I provide evidence that raising the pensionable age increases savings after the announcement. I also find that the saving response after the announcement is larger for younger cohorts that had more time to save, which also decreases the labor supply response at the implementation than older cohorts.

More broadly, understanding the effects of the Japanese social security reforms is of general interest.[7] Other countries will inevitably face similar problems to Japan under the global trend of aging populations. Furthermore, since the loss of Japanese public pension

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informed workers of a reform are less likely to postpone retirement because of their longer time frame for smoothing behavior.

[5]For example, see Lalive and Parrotta (2017), Stancanelli (2017), Queiroz and Souza (2017), Johnsen and Vaage (2015), Schirle (2008), Coile (2004), and Gustman and Steinmeier (2004).

[6]As for outcome variables other than labor supply, Attanasio and Rohwedder (2003), Attanasio and Brugiavini (2003), and Lachowska and Myck (2018) investigate the effect of pension benefits on savings. Müller and Shaikh (2018), Gorry et al. (2018), Eibich (2015), and Rohwedder and Willis (2010) discuss the relationship between retirement decisions and individual health statuses.

[7]As for related Japanese studies, Oshio and Oishi (2004) quantitatively estimate the effect of changes in social security incentive measures (accrual, option value, and peak value) on retirement behavior using survey data in 1996. Ishii and Kurosawa (2009) analyze the effect of the change in benefits on labor supply using two-periods survey data in 2000 and 2004 and logit models. A recent paper by Oshio et al. (2018) provides descriptive evidence and examines the long-run relationship between social security incentives and employment for older workers.

benefits from this policy change is one year of benefits around the cutoff, the treatment is larger than in the recent U.S. reform.[8] Appendix Table B1 summarizes full retirement ages and the public pension reforms across several developed countries, and shows the magnitude of the change is the largest. This large reform enable me to not only estimate causal effects on labor supply using RDD but to study other margins for which responses might be more difficult to detect. Exploiting a social security reform in Japan is difficult because of the availability of micro data. I overcome this problem using restricted-use data; access to the original data was restricted to government-affiliated personnel.

The remainder of the paper is as follows: The next section presents the institutional background. Section 3 describes the identification strategy, and Section 4 lays out the data and descriptive statistics. Section 5 presents the baseline empirical results. Sections 6 and 7 discuss heterogeneity and spillovers, respectively. Section 8 explores validity and robustness. Section 9 summarizes the main points to conclude.

2 Institutional Background

2.1 Employees' Pension Insurance (EPI)

Employees' Pension Insurance (EPI) is a public pension and covers private and public employees in Japan.[9] Enrollment for workers is mandatory, and the contribution rate is 18.3% of employees' earnings (9.15% respectively by employers and employees), which is higher than that in the U.S (6.2% by each). A qualifying condition was at least 25 years of participation, and recently shortened to 10 years in August 2017, which is the same length as that in the U.S.[10]

[8] In the 2004 U.S. public pension reform, the discontinuity in the loss of public pension benefits is two months around the cutoff.

[9] The persons who are not covered by EPI are covered by National Pension (NP), which is the other public pension in Japan. NP covers persons such as self-employed persons, those who do not have a job, and dependents of insured persons by EPI. The share of persons covered by EPI is 65% and that by NP is 35%, as of the end of 2017.

[10] One might predict that individuals are incentivized to retire later to satisfy the eligibility requirement for EPI benefits. However, since most Japanese persons start to work at their late 10s or early 20s, most insured persons already satisfy this minimum years of requirement before they reach the eligibility age.

The benefits of EPI consist of two parts: flat-rate benefits and earnings-related benefits. The flat-rate benefit solely depends on the number of months of participation and does not depend on past earnings, whereas the earnings-related benefit is proportional to past working income. The formulas used to calculate EPI benefits at the pension reform are summarized as follows:[11]

- The annual EPI flat-rate benefit $\approx$ \$17 * the number of months enrolled in EPI (up to a maximum of 480 months)

- The annual EPI earnings-related benefit $\approx$ career-average monthly earnings * 0.7125% * the number of months enrolled in EPI

The eligibility age for EPI had been 60 for both flat-rate benefits and earnings-related benefits. Upon reaching the eligibility age 60, beneficiaries receive both these benefits. The net replacement rate (ratio of total annual public pension benefits to pre-retirement earnings) for typical full career workers is currently about 40.0% (OECD (2017)),[12] and the shares of each benefit are nearly equal if past earnings are close to the average.

Figure 1 graphically shows the total annual EPI benefits with respect to current earnings for typical workers before the policy change. Japanese beneficiaries are subject to the retirement earnings test if they continue to work at or after the eligibility age.[13] Insured persons do not need to retire to receive pension benefits; however, if they continue to work at or after the pensionable age, the total annual pension benefits are reduced by at least 20%. If an individual continues to work and earns more than \$0, total annual benefits are reduced by 20 percent. If current earnings are above the first threshold ($\approx$\$26,400), the social security office additionally withheld \$1 of benefits for every \$2 of earnings above the threshold. If earnings are above the second threshold ($\approx$\$40,800), the

[11] Actual received benefits are indexed with inflation and adjusted every year.

[12] The net replacement rate is defined as the individual net pension entitlement divided by net pre-retirement earnings, taking account of personal income taxes and social security contributions paid by workers and pensioners.(OECD (2017))

[13] Retirement earnings tests generally mean public pension benefits are withheld if current earnings exceed specific thresholds. As for the analysis of the effect of earnings tests, see Gelber et al. (2017), Hernæs et al. (2016), and Song and Manchester (2007), for example.

social security office additionally withholds \$1 of benefits for every \$1 of earnings above the threshold.

Japanese persons may claim benefits at earlier or later ages. In these cases, the adjustments to benefits are designed to be actuarially fair for the average mortality rate. Thus, claiming benefits earlier or later has little effect on total social security wealth. As a result, most beneficiaries receive benefits at the full eligibility ages.[14] The retirement earnings test also applies to early and late claiming.

2.2 Raising the Eligibility Age for EPI

Japan has been the most aged country in the world; life expectancy has been the highest, while the birth rate has been historically low. Due to increasing longevity and low fertility, the proportion of older people has been expanding.[15] Because of these trends, social security benefit payouts have increased significantly, while social security contributions from younger people have not kept pace, thus posing a potential threat to the sustainability of the social security system.

As a response, the Japanese government decided to increase the eligibility age for EPI in November 1994. Figure 2 lays out the reform schedule, which is the variation I use. The male eligibility age for EPI's flat-rate benefits was gradually increased by one year every three years from 2001 to 2013, starting from age 60 and ending at age 65. This policy change affected specific birth cohorts at specific critical ages as it is phased in. Specifically, the pensionable age for males born after April 1941 was raised from 60 to 61; the pensionable age for males born after April 1943 was raised from 61 to 62; the pensionable age for males born after April 1945 was raised from 62 to 63; the pensionable age for males born after April 1947 was raised from 63 to 64; the pensionable age for males born after April 1949 was raised from 64 to 65.

Theoretically, these delays in eligibility ages lead to negative income shocks for cohorts

[14] For example, the total number of EPI beneficiaries was 22.33 million whereas beneficiaries claiming EPI flat-rate benefits earlier was 610 thousand and beneficiaries claiming later was 70 thousand, as of the end of March in 2005.

[15] Specifically, Japan's ratio of the population aged 60 or over to the total population is projected to rise from 33% to 42%, which is the highest in the world (United Nations (2017)).

born just after the cutoff dates at the critical ages. For example, for the first cutoff (April 1941), 60-year-old males born just before the cutoff date were eligible to receive both the EPI flat-rate and earnings-related pension benefits when they turned age 60 because they were not affected by the pension reform. However, 60-year-old males born just after the cutoff date could no longer receive the EPI flat-rate pension benefit when they reached 60. Instead, these affected cohorts could not receive this component of pension benefits until they turned 61. Because of this 1-year loss of benefits at the critical age of 60, the treatment cohort should be more likely to work and to delay retirement at age 60, as long as leisure is a normal good. Similarly, all the other cohorts at critical ages experience a 1-year loss of flat-rate benefits relative to the neighboring cohorts.[16]

It is also important to note here the related policy changes that occurred after raising the male eligibility age for EPI flat-rate benefits. First, the pension reform was later extended to women, and the female eligibility age was raised from 60 to 65 in the same way from 2006 to 2018, five years after the start of the male reform. Second, after the reform for the EPI flat-rate benefit, the male pensionable age for the EPI earnings-proportional benefit is also gradually being raised from 60 to 65 from 2013 to 2025, and the female pensionable age for the EPI earnings-proportional benefit is being raised from 60 to 65 from 2018 to 2030. In this paper, I mainly study the period when male workers were directly affected by the change in EPI flat-rate benefits, because my dataset spans 30 years from 1986 to 2015, before the end of these other changes. I do also present results for partial implementation of the above related reforms as supplementary analyses.

In addition, in 2005, the uniform 20% reduction in retirement earnings test was abolished, and workers could receive full benefits if the current earnings are below the first threshold. In the following year, Japanese companies were also required to raise the mandatory retirement age from age 60 to at least age 63 or to introduce a continued reemployment system that creates flexible positions for older workers to continue at the same company. Since individuals in cohorts surrounding the date-of-birth cutoffs at crit-

[16]The changes in the budget constraint induced by the policy at the critical ages are complicated for individuals with high earnings. These persons are more likely to face smaller negative income shocks and less marginal tax rates after the policy change. Both the smaller negative income effect and the positive price effect theoretically increase labor supply.

ical ages are always subject to the same reduction and mandatory retirement setting, I can extract the effects of raising eligibility ages for EPI benefits by *locally* comparing treatment and control groups around thresholds given critical ages. In other words, my research design isolates the specific policy change of raising eligibility ages at the critical ages and captures the causal effects, conditional on the broader policy environment.

One might also predict that this 1-year loss of EPI flat-rate benefits will increase enrollment in other social assistance programs and offset the financial incentives of the affected cohorts, mitigating the impact on retirement decisions. For example, Staubli and Zweimüller (2013) find spillover effects of raising the early retirement age on increases in enrollment in other social insurance programs in the context of Austria. However, public livelihood assistance benefits cannot be accessed by employees and beneficiaries of EPI in Japan.[17] Unemployment insurance also cannot be accessed by both the affected and non-affected cohorts at the critical ages in this setting, because the elderly cannot receive unemployment insurance in Japan if they receive public pension benefits, and they are still eligible for EPI earnings-related benefits at the critical ages. Similarly, medical insurance also does not have confounding effects, since the critical ages for public medical insurance (70 and 75) and long-term care (65) are different from the critical ages (60-64) of eligibility for EPI flat-rate benefits. Affected individuals also do not have incentives to move from EPI to the other public pension program (NP), because EPI is more attractive in the sense that the eligibility age for the NP has been 65 since 1961 and EPI benefits are higher than NP benefits.[18] Thus, other government transfer programs should not affect the behavioral responses observed in this setting.

[17] EPI benefits are generally higher than public livelihood assistance.

[18] In addition, the Japanese labor market is not liquid, and the number of individuals who change their career from employees (EPI) to self-employees (NP) is very limited. For example, the ratio of employees who changed their jobs into different job categories to total employees is only about 1.4% during the period from October 1st, 2011 to October 1st, 2012 (The Japanese Ministry of Health, Labor and Welfare (2014)).

3 Identification Strategy

To identify the causal effect of raising the eligibility age for EPI benefits on individual labor supply, I locally compare the probabilities of employment for the neighboring birth cohorts born just before (control group) and after (treatment) the cutoff date given critical ages and gender. Specifically, I implement the following regression discontinuity design (RDD) specification:

$$P(Employment|Age, Male)_i \quad = \quad \alpha + \beta\, 1(MOB_i > cutoff\, date) + f(MOB_i) + \epsilon_i \quad (1)$$

where the dependent variable is an employment status dummy that takes 1 if an individual i works given a critical age and gender and 0 otherwise; $1(MOB_i > cutoff\, date)$ is a dummy variable that takes 1 if the month of birth is above a cutoff date and 0 otherwise. $f(MOB_i)$ are flexible polynomials at the left and right sides of the cutoff. The cutoff dates and corresponding critical ages are April 1941 for males aged 60; April 1943 for males aged 61; April 1945 for males aged 62; April 1947 for males aged 63; April 1949 for male aged 64. Thus, individuals born just before the cutoff date are eligible for EPI flat-rate benefits given the critical age, whereas those born just after the cutoff date are not. Then β captures the causal effect of raising the pensionable age for male EPI flat-rate benefits by one year on male employment at the critical ages.

As for the implementation of the above estimations, I first run the pooled RDD by the normalized cutoff for the above five different months of birth. Then I also run separate RDD for each cutoff date and critical age to compare the magnitudes of the responses. For the baseline estimations, I use a local linear functional form, a triangular kernel, and the optimal bandwidth chosen by minimizing the mean squared error. As robustness checks, I also use a quadratic functional form, other lengths of bandwidths, control variables, and an uniform kernel. I use heteroskedasticity-robust standard errors.[19]

The underlying assumption of RDD is that there is no manipulation or differential

[19]Kolesár and Rothe (2018) recommend using heteroskedasticity-robust standard errors rather than clustered standard errors in this context.

attrition around the cutoff. To check this condition, I implement the validity test based on McCrary (2008) and check for smoothness of pre-determined covariates. The tests for the density and pre-determined covariates suggest that my research design is internally valid.

There is also a potential empirical concern for estimating equation (1). It is well known that seasonality affects the employment rate within a year, and my estimates may capture the seasonal effect in birth rather than the causal effect of the pension reform. To address this concern, I implement placebo tests for the same birth cutoff (April) but using placebo samples such as individuals who are not covered EPI and responses before the announcement. All the placebo tests suggest that there should not be concern for seasonality.

4 Data

I create an annual individual-level dataset spanning the years 1986 to 2015 from restricted-use data sources. All data are taken from the *Comprehensive Survey of Living Conditions*, a large household-level survey administered by the Japanese ministry of Health, Labour and Welfare. This survey was introduced in 1986 to understand the living conditions of people in Japan and has been conducted every year thereafter. The aggregate data were open to the public, but access to the original data was restricted to government-affiliated personnel.

There are several advantages of using the restricted-use data to investigate the public pension reform. First, the Japanese public pension reform has not been investigated using comprehensive individual-level data, because of the lack of public-use micro data that cover many generations over long periods before and after the entire reform. This data enables me to analyze the dynamics of individual behavioral responses over 30 years. Second, since the original data are household-level, it is also possible to study spillover effects within family members. Finally, these data contain very detailed information on people's lives in different areas: household demographics, income, health, long-term care, and savings. This comprehensive information on individual lives enables me to estimate not only the average effect of pension reform on older workers' labor supply, but also heterogeneous effects and effects on previously under-analyzed margins. One downside is that the survey is a repeated annual cross-section and does not follow the same individuals over years. Unfortunately, there are no administrative comprehensive panel data in Japan that span years before and after the public pension reform.

Table 1 presents summary statistics for the sample. For the main analysis, I exclude individuals not related to EPI, such as self-employed persons, housewives, and students, since the public pension reform in 2001 raised the pensionable age for EPI benefits only. I also exclude observations for the 1st stage who report the implausibly low values ($\leq$\$100) given 25 years of the minimum enrollment periods and benefit formula.

5 Empirical Results

5.1 Main Results for Raising the Eligibility Age

5.1.1 Effects on the Total Public Pension Benefit

Figure 3 shows graphical evidence of the effect of the pooled RD equation (1) of raising the eligibility age for male EPI flat-rate benefits by 1 year on total public pension benefits. The figure plots the average annual total public pension benefits which males receive at the critical ages. The cutoffs are normalized at zero as explained in the identification. The sample on the left side shows the annual total public pension benefits of non-affected males who were eligible for the EPI flat-rate benefit at the critical ages (control group). The sample on the right side shows the annual total public pension benefits of affected males who were not eligible for the EPI flat-rate benefit upon reaching the critical ages (treatment group). As expected, there is a noticeable discontinuity in the amount of public pension benefits around the cutoff, suggesting the affected cohorts received less public pension benefits than the non-affected cohorts at the old critical ages. Appendix Figure A1 also graphically shows the quadratic fitted values.

Table 2 reports the RDD estimate for the 1st stage. The RDD estimate is negative and statistically significant at 1% level. The magnitude of the estimate is 631.4 thousand Yen per year, which is almost consistent with the theoretical value of the one-year EPI flat-rate benefit. The decrease in the total public pension benefit is about 50 percent compared to that prior to the reform and about 20 percent compared to earnings. In sum, Figure 3 and Table 2 show that the raising eligibility ages for EPI flat-rate benefits causes sharp negative income shock for elderly people.

5.1.2 Effects on Individual Labor Supply

Figure 4 shows the graphical illustration of the pooled RDD regression equation (1) of raising the male pensionable age for EPI flat-rate benefits by one year on male labor supply. As in the first stage, the cutoffs are normalized at zero. The sample on the left side was eligible for EPI flat-rate benefits at the critical ages (control group), whereas the sample on the right side was not eligible for EPI flat-rate benefits even upon reaching the critical ages (treatment group). There is a noticeable jump around the normalized cutoff, suggesting individuals increase their labor supply at the critical ages. Appendix Figure A2 also shows graphically the quadratic fitted values.

Table 3 reports the RDD estimates for the 2nd stage. The odd-numbered columns report the local linear RDD estimates, and the even-numbered columns report the local quadratic RDD estimates. Columns (1) and (2) are estimated using a triangular kernel, and columns (3) and (4) are estimated using a uniform kernel. As one can see, the RDD estimates are positive and statistically significant at the 1% level across different functional forms and kernels. The magnitude of the difference in the male employment is about 7-8 percentage points across specifications, indicating that raising the male EPI flat-rate pensionable age by one year increases the male employment by 7-8 percentage points. Since the mean of the dependent variable is about 60 percent, the impact of the policy change is about 12-13 percent.

5.1.3 Effects on Other Outcomes

Table 4 through Table 8 show the RDD estimates for the different outcome variables. Table 4 presents the RDD estimates for intensive margin. The effect is all positive, suggesting raising eligibility age for public pension also increases intensive margin. The RDD estimates for earnings and working hours per week are statistically significant, whereas the RDD estimates for working days per week and working hours per day are statistically insignificant. Though the empirical results suggest that raising the eligibility age increased both extensive and intensive margins, the workers' main behavioral responses may be thorough the extensive margin rather than intensive margin, compared to the magnitudes of the responses in Table 3.

Table 5 reports the RDD estimate for savings. Raising the eligibility age for the flat-rate EPI benefit also increases savings for affected cohorts more than non-affected cohorts, with an impact of about 9.6 percent. This result suggests that the affected cohorts, who would not be able to receive the public pension benefit at the critical ages, prepared for the reform by accumulating savings.

Table 6 reports the RDD estimates for individual health. The relationship between raising the eligibility age and individual health looks ambiguous; the coefficients for physical health and mental health are all statistically insignificant. Thus, delayed retirement associated with raising the eligibility age does not significantly affect individual health.

Table 7 reports the RDD estimate for consumption. The estimate of the coefficient for consumption is not statistically significant. The result suggests that raising the eligibility age does not affect the consumption on the implementation date.

Table 8 shows the impact on private pension enrollments. The public pension reform could also affect the enrollment in private pensions, so that affected cohorts could depend on private pension benefits in the absence of flat-rate public pension benefits. However, the coefficient for the participation in private pension system is positive but not statistically significant, ruling out the possibility.

5.2 Impacts of Related Reforms

5.2.1 Raising the Eligibility Age for the Female EPI Flat-rate Benefit

Table 9 shows the result for the female labor supply response to raising the pensionable age for the EPI flat-rate benefit by one year. As explained in the institutional background section, female reform started five years after the male reform. Hence the experiment is the same but the timing is different. The result is similar to the male case; the magnitude of the increased female labor supply response is about 6-9 percentage points across specifications, which is almost the same as the magnitude for the male labor supply response. This result suggests that the difference of labor supply responses across genders is small.[20]

5.2.2 Raising the Eligibility Age for the Earnings-related Benefit

Appendix Table B2 shows the RDD estimates for the male labor supply response to raising the male pensionable age for EPI earnings-related benefits by one year. This result is consistent with the result of the EPI flat-rate benefit; raising the male pensionable age for the EPI earning-related benefit by one year significantly increase male employment by 5 percentage points. Though raising the eligibility age for the earnings-related benefit started in 2013 and is still ongoing, the results provide evidence for the increase in labor supply in response to the negative income shock.

6 Heterogeneity

So far, I find empirical evidence that raising the public pensionable ages increase labor supply. However, is the magnitude of the labor supply response same across groups that share common characteristics? To answer the extended question, I explore heterogeneity in this section.

[20] The comparable responses for women and men occur even though the women have five more years available after the announcement to change behavior prior to the implementation date. This suggests that women could have responded more to the loss in benefits, everything else equal. I'll detail anticipatory responses in the heterogeneity section.

6.1 Heterogeneity across Closeness to the Implementation

Table 10 shows the comparison of male labor supply responses to raising the eligibility age for EPI flat-rate benefits by one year at each cutoff. For example, the first column provides the RDD effect of raising the pensionable age from 60 to 61 on male employment at age 60; the second column provides the RDD effect of raising the pensionable age from 61 to 62 on male employment at age 61. As one can see, the labor supply response is larger for older affected cohorts (older policy changes) than for younger affected cohorts (newer policy changes), even though the magnitude of the lost pension benefits is the same (1-year loss of flat-rate benefits) for all affected cohorts relative to the neighboring non-affected cohorts. What is the underlying mechanism for this decreasing treatment effect?

One likely mechanism is due to anticipatory responses. Though the magnitude of the negative income shock is the same across cohorts, there is a variation in the number of years between the announcement and implementation date. As Appendix Table B3 and Appendix Table B4 show, the oldest cohorts had 6 years between the announcement and implementation, whereas the youngest cohorts had 18 years between the announcement and implementation. Evidently, the younger affected cohorts had more time to smooth their labor supply during years between the announcement and implementation date. Appendix Table B4 shows the RD estimates for labor supply responses for the oldest affected cohort (1941.April-) and the youngest affected cohort (1949.April-) relative to each control cohort in the periods between the announcement and implementation. As one can see, the youngest affected cohort better smooth their labor supply, leading to smaller labor supply response at the implementation. Table 11 also shows the estimate from a regression with an interaction term, allowing the treatment coefficient for pension eligibility to depend on the number of years between enactment and implementation. The coefficient for the interaction term is negative and statistically significant at 1% level, suggesting cohorts with more anticipatory periods respond less to the raising eligibility age at the implementation. The magnitude of the interaction term is about -0.7, suggesting the additional one year of anticipation decreases the labor response at the implementation by about 0.7 percentage points.[21][22]

Liquidity constraints would be another underlying mechanism. Since the length of periods between the announcement and implementation date is longer for younger affected cohorts than for older affected cohorts, the younger affected cohorts had more time to accumulate their savings to prepare for the negative income shock in the future. Appendix Table B5 reports the RD estimates for savings for the youngest affected cohort and the oldest affected cohort relative to each neighboring control cohort during the periods between the announcement and implementation. As one can see, the sum of the estimates is higher for the youngest affected cohort than the oldest affected cohort, suggesting that the younger cohort accumulated more savings than the older cohort after its announcement. Hence, following the implementation date, the youngest cohort become less dependent on social security by the increased savings, leading to smaller labor supply response on the implementation date. Appendix Table B6 also shows the estimates from a regression with an interaction term, allowing the treatment coefficient to depend on years between the enactment and implementation. The coefficient for the interaction term is positive, suggesting the cohort with more anticipation periods accumulated more savings at the implementation.

Table 12 also reports the result of heterogeneity by savings. Here, I run RD regression equation (1) including the treatment cohort dummy interacted with savings. The estimated coefficient for the interaction term is negative and statistically significant at 1% level, suggesting the labor supply response decreases as the amount of savings increases. The increase of savings by 1,000,000 Yen ($\approx$ 10,000 USD) decreases the employment by

[21] It is important to note that there may be direct effects according to age of treatment. Delaying one year from age 60 to 61 may be different than delaying one year from age 64 to 65. However, the baseline employment rates do not change very much across critical ages. Table 10 also shows the effect in terms of percent (rather than percentage points) and still treatment effects are decreasing, ruling out this possibility.

[22] Some researchers also argue that social norms could affect older workers' retirement behavior. For example, Brown and Laschever (2012) argue that peer effects and social norm could affect individual retirement decisions, whereas Asch et al. (2005) do not find evidence. However, this social norm story does not seem to apply to Japan. Social norms would imply a smaller effect of the reform on labor supply of the older cohorts, since they face more continuing social pressure to retire at age 60. Younger cohorts, in contrast, can more easily continue working. This is the opposite pattern to what I see in the data, discounting this social norm story.

0.5 percentage points. One potential concern with using actual savings is that savings decisions and retirement decisions could be made together; those with higher savings could represent those who plan on retiring earlier, making this variable endogenous. To address this concern, I also instrument for savings with the average saving rates in pre-announcement periods given area. The intuition of this instrument is that preference over the saving is different across geography, and this area-specific preference only affected the amount of saving in the post-treatment but did not affect the other factors that potentially influence working decision. As in Appendix Table B7, the coefficient for the interaction term with saving is also negative and statistically significant by the IV regression, suggesting individual labor supply decreases with the amount of savings.

6.2 Other Heterogeneity

Heterogeneity by Family Structure

Table 13 reports the comparison of the RDD estimates of labor supply between single males, males living with their spouses, males living with their parents, males living with their children, and males living with their grandchildren. As one can see, the labor supply response is higher for males living with their dependents; the estimate for single males are lower and statistically insignificant, whereas the estimates for males living with their dependents are higher and statistically significant. In other words, males living with dependent family members responded to the negative income shock more than single males. The estimated result is consistent with the economic theory; living with dependents should make consumption less elastic, explaining the greater response to negative income shocks.

The magnitude is the highest for males living with their grandchildren, suggesting the incentives to invest on their descendants. The second highest case is males living with their parents, suggesting that the older workers living with their parents at the age of around 90 would need fixed expenses for long-term care, which would increase their labor supply when they faced the negative income shock. In sum, the table provides evidence for the case that, when older persons live with dependent family members, they responded more to the negative income shock by increasing their labor supply.

Heterogeneity by Education

Appendix Table B8 reports the treatment coefficient interacted with the educational levels. Some papers (e.g., Hanel and Riphahn (2012) and Mastrobuoni (2009)) argue that educational background differentially affects the magnitude of labor supply responses. However, the coefficient of the interaction term is not statistically significant, suggesting there is little differential behavioral response across educational levels.

7 Spillovers

Changing the pensionable age could also affect the labor supply of other family members. For example, the wife of the affected husband could respond to the delay of the husband's benefits. The total effect on spousal labor supply is theoretically ambiguous, since the direction and magnitude of spillovers depend on both income effects and complementarity of leisure between couples. As Table 14 shows, the labor supply response of the wife of the affected husband is higher than that of the wife of the non-affected husband around the cutoff. The increase of the spousal labor supply with respect to the partner's eligibility is consistent with Lalive and Parrotta (2017), which find that couple labor supply decreases as the partner reaches the full retirement age.[23]

Furthermore, Table 14 also suggests that the children of the affected husband increase more labor supply relative to the children of the non-affected husband. One likely mechanism of the effect on children's labor supply is a scarring effect suggested by Dahl and Gielen (2018), the idea of which is that children whose parents are kicked off of government assistance programs infer they cannot rely on the government, making children work more. Seeing the father be unable to access public pension benefit, the children of the affected father would be more likely to lose reliability of public pension and take care

[23]Stancanelli (2017) also finds that the husband's probability of retirement decreases if the wife experiences a delayed eligibility, whereas the wife's probability does not change immediately if the husband experienced the delay.

of themselves.[24] The other possible interpretation would be learning and information transmission from the parents to children. Being ineligible for public pension benefit, the affected parent would give children information on the reform schedule and fiscal imbalances of social security systems. As a result, well informed children would be more likely to increase their labor supply from learning. Labor supply response of the children of the affected husband is mainly intensive margin rather than extensive margin. Since the employment rate for younger persons is high (close to 90%), there is little room for the extensive margin to increase.

8 Validity and Robustness

8.1 Validity

For the internal validity of a RDD, I first implement the validity tests to see if there is a manipulation or differential attrition around the cutoff. I also implement several placebo tests to further explore the validity of my estimates.

8.1.1 Validity Tests

***M*anipulation**

The underlying assumption of a RDD is that the running variable is continuous and individuals cannot manipulate the running variable. This condition is tested based on the methods in McCrary (2008). Appendix Figure A3 graphically shows the density of the running variable (months of birth) for males, and there is no spike around the cutoff. The p-value of the manipulation test by McCrary (2008) is 0.27, indicating no statistical evidence of systematic manipulation of the running variable.

***S*moothness of Predetermined Covariates**

I also check for smoothness of predetermined covariates around the cutoff. Since the predetermined variables are determined before the public pension reform, eligibility for

[24] Okumura and Usui (2014) also show that younger people have more pessimistic view about future public pension and benefit than older people.

the public pension benefit should not affect them. Appendix Figure A4 plots the predetermined covariates (area, gender, and spouse) along the running variable, and there is no discontinuity around the cutoff. The p-values of the null hypothesis that the variable is continuous are 0.60, 0.71, 0.50, respectively, providing the evidence of the smoothness of the predetermined covariates.

8.1.2 Placebo Tests

***I*ndividuals not participating in EPI**

There could be confounding policy changes or factors that only influence cohorts affected by the pension reform. Many possible factors, such as macroeconomic conditions, private pensions, and time trends, could have differentially affected the employment status for two birth cohorts around the cutoff. My underlying assumption is that these factors would have affected the employment status less in my running variable (month of birth) as opposed to the sharp discontinuity via the negative income shock experienced by the cohorts born after the cutoff relative to the cohorts born before the cutoff. To check this condition, I run RD with the same birth cutoff but those who were not enrolled in EPI. Since the public pension reform only affected people who were enrolled in EPI, this test works as a placebo test. As in Appendix Table B9, the affected cohorts did not respond any more to raising the pensionable ages than non-affected cohorts. Thus, other policy changes and factors should not confound my identification.

***R*esponse before the Announcement**

I also check the individual labor supply response prior to the announcement of the public pension reform. Since individuals could not anticipate the policy change before the announcement, a differential response between affected cohorts and non-affected cohorts before the announcement would violate my identification strategy. Appendix Table B10 shows the behavioral response in labor supply for both treatment and control cohorts prior to the announcement. As one can see, the RD estimates before the announcement are not statistically significant, suggesting affected individuals did not respond any more than non-affected cohorts prior to the announcement.

Placebo Cutoffs and Cohorts

I also implement a placebo test for the same critical age but for different placebo cutoffs. Since this public pension reform only affected specific cohorts separated by the true birth cutoff, there should not be a jump for the placebo cutoffs. Appendix Table B11 shows the result of the placebo tests, and the RD estimates are all statistically insignificant, suggesting no discontinuous effect on the placebo cohorts.

Placebo Tests for Other Outcome Variables

I also implement the above placebo tests for other outcome variables in addition to the labor supply. Even if the labor supply passes the above placebo tests, a discontinuity for other labor market outcomes might suggest a systematic difference between the treatment and control cohorts; however, Appendix Table B12 rules out this possibility. Specifically, earnings, savings, consumption, and health status also pass the above placebo tests, lending credibility to my research design.

Placebo Tests for Labor Demand Side

It is also possible that the change in firms' labor demand could affect the quantity of labor supplied by individuals. However, firms characteristics such as occupations, the ratio of regular employees, and firm size do not change significantly around the cutoff of the eligibility for public pension, as in Appendix Table B13. The results of the placebo tests suggest that the effect of the labor demand side is limited.

In 2006, the government changed the mandatory retirement rule and required companies to raise the mandatory retirement age or introduce a continued re-employment system up to age 63. However, the cutoff of this mandatory retirement reform doesn't coincide with the date separating male treatment and control in the pension reform. Since RDD captures the local average treatment effect around the cutoff, this policy change had a little effect on my local treatment estimates. Appendix Table B14 also separates the effect of raising pensionable age from the effect of the mandatory retirement age, by allowing the treatment coefficient to depend on whether that cohort is treated after the end of mandatory retirement age 60. As expected, the effect of raising pensionable age is still statistically significant and large.

8.2 Robustness

In the empirical results section, I already show that my RDD estimates are robust to chosen polynomial, kernel, and optimal bandwidth. In this section, I also present the following further additional robustness tests to show my estimated results are quantitatively robust under different conditions.

Appendix Table B15 shows the sensitivity analysis by length of bandwidth. The first row presents the RDD estimates within 10 months, 15months, and 20 months. All the estimates are similar in magnitude and statistically significant at 1% level across a range of bandwidth, providing consistent results with the estimates.

Appendix Table B16 shows the RDD estimates with additional predetermined covariates. The inclusion of covariates should not affect the estimated discontinuity under the non-manipulation assumption. The estimates of the covariates adjustment (area and spousal age) in the RDD equation (1) show the consistent results with the baseline.

Appendix Table B17 shows RDD estimates for the log approximation for the dependent variables. Since it is well known that several labor market variables have high standard errors, I take inverse hyperbolic sines for these variables to reduce noise. The estimates of the log approximation of the RDD equation (1) are robust to my main baseline.

9 Conclusion

In this paper, I investigate how workers' retirement decisions are affected by recent Japanese public pension reforms to ages of eligibility. In Japan, the pensionable age for Employees' Pension Insurance benefits gradually increased from 60 to 65 for males over the course of a decade in order to reduce fiscal imbalances in the system. Using individual-level restricted-use data spanning three decades and a regression discontinuity design based on date of birth, I find that raising the pensionable age for flat-rate benefits by one year increases male employment at the critical ages by about 7-8 percentage points. I also find that raising eligibility age also affects other outcomes such as savings and earnings.

My paper includes two novel contributions. First, I find that individuals respond differentially to the same one-year loss of benefits across cohorts depending on the number of years between announcement and implementation. The fact that treatment effects are decreasing along with the scope for adjustment is strong evidence of anticipatory responses. Second, I document spillovers to family members. The wife and the children of an affected husband increase their labor supply, suggesting there exist some coordination benefits within households that offset negative income effects. These original findings highlight that factors such as timing and family circumstances must be considered for the optimal design of public pension reforms.

My paper provides policy implications and prescriptions for public pension reforms. Public pension reform becomes an increasingly debated topic among policymakers with the rapidly aging populations. My empirical results suggest that the effects of pension reforms are not simple, and policy makers should not design them by only looking at the average effect on older workers' labor supply. Pension reforms affect many labor market outcomes, and individual behavioral responses are heterogeneous across groups in several dimensions. Specifically, policymakers should care about the periods between the policy announcement date and implementation date for each cohort, because the length of anticipatory periods differentially affects the behavior for each birth cohort after the announcement, in terms of labor supply and savings. Ideally, policy makers should take plenty of years after the announcement so that the impact on the labor market on the implementation should be mitigated. In addition, policymakers should also pay much attention to individuals living with their dependents, because the impact is larger for those living with their dependents. My analysis on spillovers also reveals that a policy targeted to a husband changes the behavior of the wife and children; implementing a reform without taking other family members into account would miss important effects in labor markets. Finally, policy makers should also consider possible incidental effects on other outcomes associated with delayed retirement. Thus, many factors must be considered for the optimal policy design for public pension reforms.

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Figures and tables

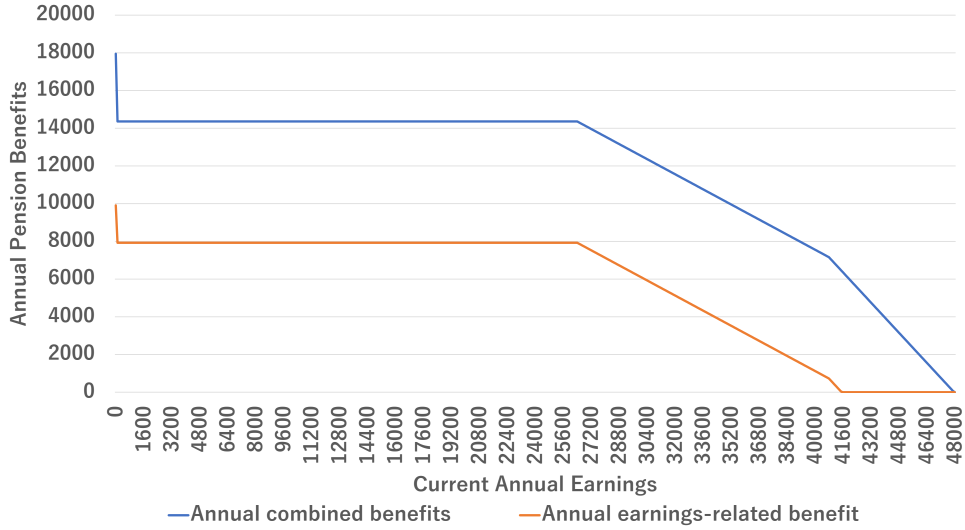

Figure 1: Japanese annual public pension benefits

Notes: The figure shows annual public pension benefits at the eligibility age for typical workers before the pension reform. The blue line shows the combined earnings-related and flat-rate benefits; the orange line shows just the earnings-related benefit; the gap between two lines show the flat-rate benefit. Benefits are calculated for a typical single worker who worked for 40 years at average earnings before the pension reform. The unit is dollar/year, and one dollar roughly equals 100 Yen.

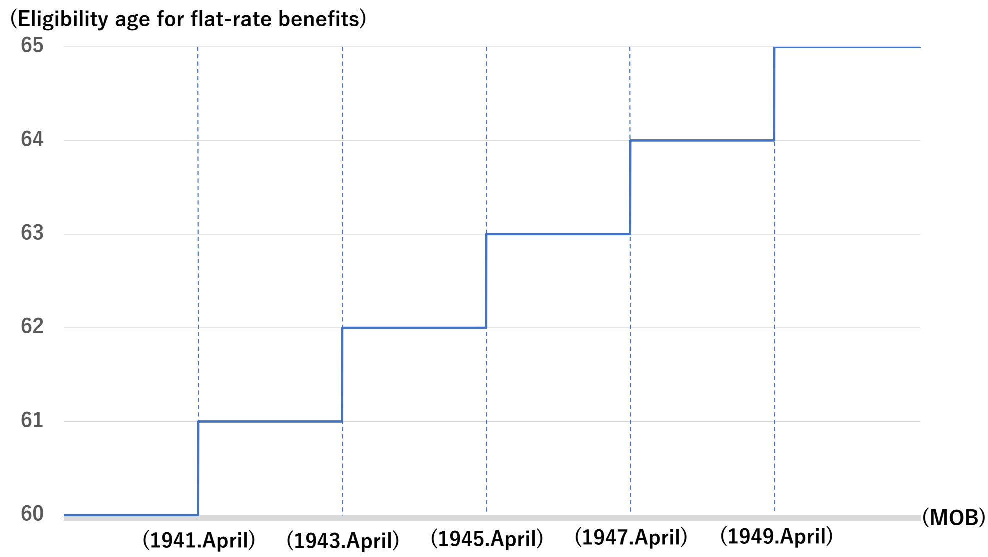

Figure 2: Public Pension Reform Schedule for Male EPI Flat-rate Benefits

Notes: The figure plots the male eligibility age for EPI flat-rate benefits by month of birth.

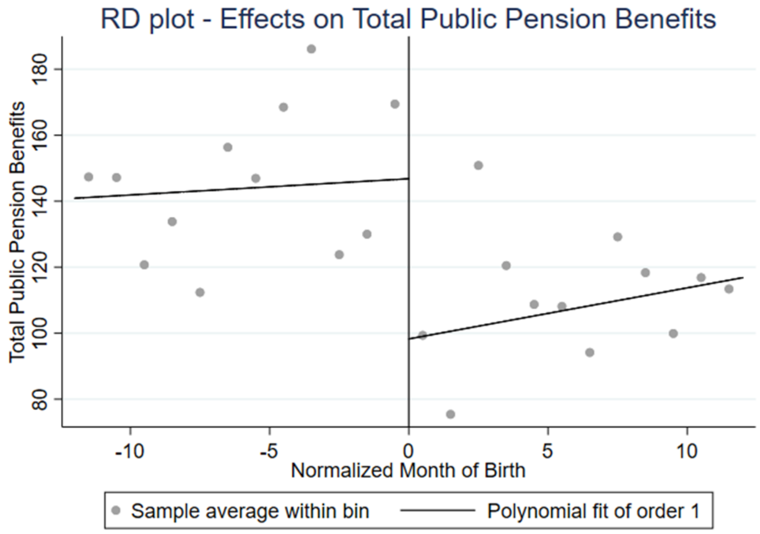

Figure 3: RDD Estimates of the Total Public Pension Benefits (1st stage)

Notes: The figure plots the male total annual EPI benefits at the critical ages by month of birth. The solid lines on the panel correspond to linear fitted values. The sample on the left side is eligible for the EPI flat-rate benefit at the critical ages, whereas the sample on the right side is not eligible for the flat-rate benefit at the critical ages. The cutoff at point zero is normalized and shows five different dates: 1941.April, 1943.April, 1945.April, 1947.April, and 1949.April. The sample restrictions are described in the text. The unit of observations is 10,000 Yen.

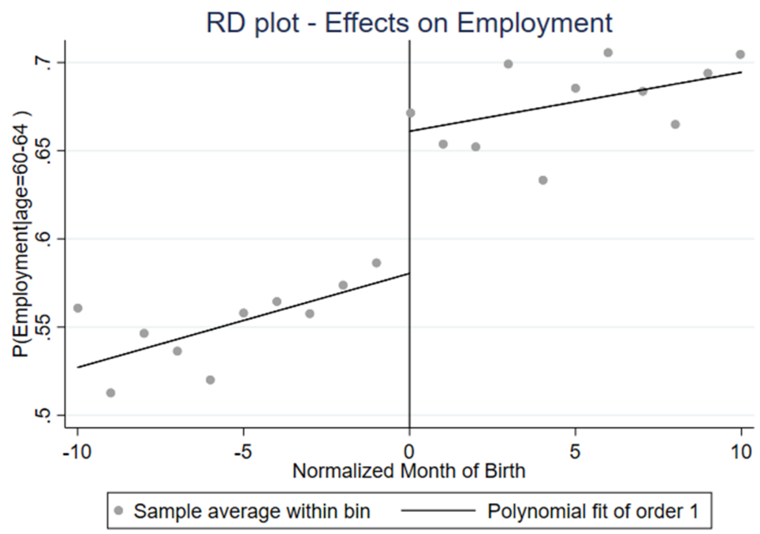

Figure 4: RDD Estimates of the Male Employment (2nd Stage)

Notes: The figure plots the probability of employment for males at the critical ages by month of birth. For other details, see the notes to Figure 3.

Table 1: Descriptive Statistics of Main Variables

Variable	Mean (Standard Deviation)
Outcome Variables	
Probability of working	0.73(0.44)
Public pension benefit (10,000 Yen)	131.14(97.75)
Earnings (10,000 Yen)	376.52(328.37)
Savings (10,000 Yen)	640.49(767.56)
Consumption (10,000 Yen)	345.32(432.81)
Hospitalized (1:Yes, 0:No)	0.02(0.12)
Subjective symptom (1:Yes, 0:No)	0.31(0.46)
Went to a hospital within one month (1:Yes, 0:No)	0.38(0.49)
Health problem influencing daily life (1:Yes, 0:No)	0.11(0.31)
Worry or stress (1:Yes, 0:No)	0.48(0.50)
Other Characteristics	
Male	0.49(0.50)
Age	42.32(22.68)
Married	0.55(0.50)
Number of households	3.65(3.63)
Obs	8,040,105

Notes: The table reports the means and standard deviations (in parentheses) of the main variables in the entire sample. The data comes from the Japanese Ministry of Health, Labor and Welfare. The ideas underlying the dataset are described in the text.

Table 2: Effects on the Total Public Pension Benefit (1st stage)

Dependent variable: Public pension benefit	
RDD	-63.14***
	[18.16]
Functional form	Linear
Kernel	Triangular
Bandwidth	12.47
Mean of the dependent variable (10,000 Yen)	128.22
Mean of the individual earnings (10,000 Yen)	338.53
Obs.	1,876

Notes: The parameter is the result from a local linear RDD regression of equation (1) for male individual annual total EPI benefits, where the running variable is month of birth. The coefficient reports the local linear RDD estimate with a triangular kernel. The cutoff is normalized and represents five different dates: 1941.4.1, 1943.4.1, 1945.4.1, 1947.4.1, and 1949.4.1. The unit of observations is 10,000 Yen (≈ 100 USD) per year. The sample restriction is described in the text. Reported in brackets are heteroskedasticity-robust standard errors. Statistical significance is indicated by * at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 3: Effects on Male Labor Supply (2nd stage)

Dependent variable: Male employment at the critical ages	(1)	(2)	(3)	(4)
RDD	0.069***	0.075***	0.071***	0.079***
	[0.022]	[0.027]	[0.025]	[0.030]
Functional form	Linear	Quadratic	Linear	Quadratic
Kernel	Uniform	Uniform	Triangular	Triangular
Bandwidth	4.41	7.65	5.64	7.73
Mean of the dependent variable	0.60	0.60	0.60	0.60
Obs.	8,238	12,427	9,633	12,427

Notes: The parameters are results from separate local RDD regressions of equation (1) for male employment at the critical ages, where the running variable is month of birth. Odd-numbered columns report the local linear RDD estimates, and even-numbered columns report the local quadratic RDD estimates. Columns (1) and (2) are estimated with an uniform kernel, and columns (3) and (4) are estimated with a triangular kernel. For other details, see the notes to Table 3.

Table 4: Effects on Intensive Margin

Dependent variable	Working Hours per Week	Working days per Week	Working Hours per Day	IHS of Earnings
RDD	1.32*	0.09	0.11	0.19*
	[0.74]	[0.06]	[0.12]	[0.11]
Functional form	Linear	Linear	Linear	Linear
Kernel	Triangular	Triangular	Triangular	Triangular
Bandwidth	11.75	11.75	11.75	11.75
Dependent mean	37.94	4.79	7.69	1.00
Obs.	6,083	6,189	6,065	11,356

Notes: The parameters are results from separate local linear RDD regressions of equation (1) for intensive margins. Earnings take the inverse hyperbolic sine of earnings, and the magnitudes of the bandwidths of the other columns are fixed for comparison. For other details, see the notes to Table 3.

Table 5: Effects on Savings

Dependent variable	Amount of Savings
RDD	95.27*
	[54.93]
Functional form	Linear
Kernel	Triangular
Mean of the dependent variable (10,000 Yen)	856.42
Obs.	3,947

Notes: The parameter is the result from a local linear RDD regression of equation (1) for the amount of savings. The unit is 10,000 Yen (≈ 100 USD). For other details, see the notes to Table 3.

Table 6: Effects on Health

Dependent variable	(1)Hospitalization	(2)Subjective symptom	(3)Hospital visits	(4)Influence on life	(5)Worry or stress
RDD	-0.004	0.011	0.001	-0.009	0.005
	[0.004]	[0.017]	[0.018]	[0.007]	[0.018]
Functional form	Linear	Linear	Linear	Linear	Linear
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Dependent mean	0.02	0.36	0.54	0.18	0.41
	(1:yes, 0:no)	(1:yes, 0:no)	(1:yes, 0:no)	(1:yes, 0:no)	(1:yes, 0:no)
Obs.	14,147	13,741	13,644	12,688	8,422

Notes: The parameters are results from separate local linear RDD regressions of equation (1) for health variables indicated in the column header at the critical ages. Each dependent variable is a dummy taking 1 if an individual says yes for each question described below and 0 otherwise. Questions are as follows; (1) Are you hospitalized? (2) Do you have any subjective symptom such as disease or injury in a couple of days? (3) Do you go to a hospital? (4) Do you have any health problem that affects daily life? (5) Do you have a worry or stress? For other details, see the notes to Table 3.

Table 7: Effects on Consumption

Dependent variable	Amount of Consumption
RDD	12.60
	[13.77]
Functional form	Linear
Kernel	Triangular
Bandwidth	8.69
Mean of the dependent variable (10,000 Yen)	331.81
Obs.	12,928

Notes: The parameter is the result from a local linear RDD regression of equation (1) for the amount of annual consumption. The unit is 10,000 Yen ($\approx$ 100 USD) per year. For other details, see the notes to Table 3.

Table 8: Effects on Private Pension

Dependent variable	Enrollment in Private Pension
RDD	0.03
	[0.03]
Functional form	Linear
Kernel	Triangular
Bandwidth	8.49
Mean of the dependent variable	0.15
Obs.	1,761

Notes: The parameter is the result from a local linear RDD regression of equation (1) for the status of the enrollment in private pension. For other details, see the notes to Table 3.

Table 9: Effects on Female Labor Supply

Dependent variable: Female employment rate at the critical ages	(1)	(2)	(3)	(4)
RDD	0.079*	0.060	0.076*	0.088
	[0.041]	[0.046]	[0.046]	[0.056]
Functional form	Linear	Quadratic	Linear	Quadratic
Kernel	Uniform	Uniform	Triangular	Triangular
Bandwidth	5.37	10.21	5.76	9.14
Magnitude of the difference of the RDD estimates between males and females	0.010	-0.015	0.005	0.009
Obs.	2,596	5,777	2,596	5,129

Notes: The parameters are results from separate RDD regressions of equation (1) for the female employment at the critical ages. Odd-numbered columns report the local linear RDD estimates, and even-numbered columns report the local quadratic RDD estimates. Columns (1) and (2) are estimated with an uniform kernel, and columns (3) and (4) are estimated with a triangular kernel. The cutoff is normalized and shows four different dates: 1946.4.1, 1948.4.1, 1950.4.1, and 1952.4.1. For other details, see the notes to Table 4.

Table 10: Heterogeneity: Comparison of the Separate RDD Estimates of Male Labor Supply

Policy change	(1)60→61	(2)61→62	(3)62→63	(4)63→64	(5)64→65
Dependent variable	Emp at 60	Emp at 61	Emp at 62	Emp at 63	Emp at 64
RDD	0.09***	0.06*	0.05*	0.04	-0.04
	[0.03]	[0.03]	[0.03]	[0.04]	[0.03]
Functional form	Linear	Linear	Linear	Linear	Linear
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Bandwidth	8.66	16.94	29.05	4.77	11.53
Dependent var mean	0.62	0.56	0.61	0.56	0.57
Impact of policy change	14.5%	10.7%	8.1%	7.1%	7.0%
Year of implementation	2001	2004	2007	2010	2013
Obs.	2,687	3,636	5,419	1,899	3,224

Notes: The table shows the comparison of the local linear RDD estimates of male labor supply in response to raising the male EPI flat-rate eligibility age by one year. The first column reports the effect of raising the pensionable age for the EPI flat-rate benefit from 60 to 61 on male employment at the age of 60. The second column reports the effect of raising the pensionable age for the EPI flat-rate benefit from 61 to 62 on male employment at the age of 61. The third column reports the effect of raising the pensionable age for the EPI flat-rate benefit from 62 to 63 on male employment at the age of 62. The forth column reports the effect of raising the pensionable age for the EPI flat-rate benefit from 63 to 64 on male employment at the age of 63. The fifth column reports the effect of raising the pensionable age for the EPI flat-rate benefit from 64 to 65 on male employment at the age of 64. The cutoff of the running variable is April 1941 for the first column, April 1943 for the second column, April 1945 for the third column, April 1947 for the fourth column, and April 1949 for the fifth column. For other details, see the notes to Table 3.

Table 11: Heterogeneity: RDD Interacted with Anticipatory Periods

Dependent variable	Male employment at the critical ages
RDD	0.14***
	[0.01]
RDD*Length of Periods between Announcement and Implementation	-0.007***
	[0.001]
Functional form	Linear
Kernel	Triangular
Bandwidth	24.00
Pre-treatment mean	0.60
Obs.	19,455

Notes: The parameter is the result from a local linear RDD regression of equation (1) for the male employment at the critical ages, where the treatment dummy is interacted with the periods between the announcement and implementation. For other details, see the notes to Table 3.

Table 12: Heterogeneity: RDD Interacted with Savings

Dependent variable	Male labor supply at their critical ages
RDD	0.14***
	[0.05]
RDD*Savings	-0.005*
	[0.003]
Mean of the dependent variable	0.60
Mean of the savings (1,000,000 Yen)	8.091
Bandwidth	10.00
Obs.	1,441

Notes: The parameters are results from a local linear RDD regression of equation (1) for the male employment at the critical ages, where the treatment dummy is interacted with the amount of savings. The unit of the interacted savings term is 1,000,000 Yen ($\approx$ 10,000 USD) per year. For other details, see the notes to Table 3.

Table 13: Heterogeneity by Family Structure

Subsample	Single males	Married males with spouses	Males with parents	Married males with children	Married males with grandchildren
RDD	0.00	0.05***	0.07***	0.07***	0.09**
	[0.04]	[0.01]	[0.03]	[0.02]	[0.04]
Function	Linear	Linear	Linear	Linear	Linear
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Bandwidth	5.90	9.80	9.51	8.23	10.17
Obs.	1,809	24,572	4,401	12,506	1,928

Notes: The table shows the comparison of the local linear RDD estimates from separate regressions of equation (1) for male labor supply by different subsample. The subsample consists of single males for the first column, married males living with their spouses for the second column, males living with their parents for the third column, males living with their children for the fourth column, and males living with their grandchildren for the fifth column. For other details, see the notes to Table 3.

Table 14: Spillovers within Family Members

Dependent variable	(1)Wife's employment	(2)Wife's earnings	(3)Child's employment	(4)Child's earnings
RDD	0.05*	2.27	0.01	63.25*
	[0.03]	[39.60]	[0.03]	[36.03]
Functional form	Linear	Linear	Linear	Linear
Kernel	Triangular	Triangular	Triangular	Triangular
Bandwidth	28.63	38.57	24.51	29.81
Mean of the dependent variable	0.42	202.00	0.88	302.44
Mean of Age	57.34	56.87	28.81	29.36
Obs.	3,782	1,097	10,189	1,180

Notes: The table shows the local linear RDD estimates of separate regressions (1) for different dependent variables indicated in the column heading, where the running variable is the head husband's month of birth. The unit of annual earnings is 10,000 Yen ($\approx$ 100 USD). For other details, see the notes to Table 3.

Online Appendix for "The Effects of Increasing the Eligibility Age for Public Pension on Individual Labor Supply: Evidence from Japan"

by Nobuhiko Nakazawa

Sections:

A. Appendix Figures

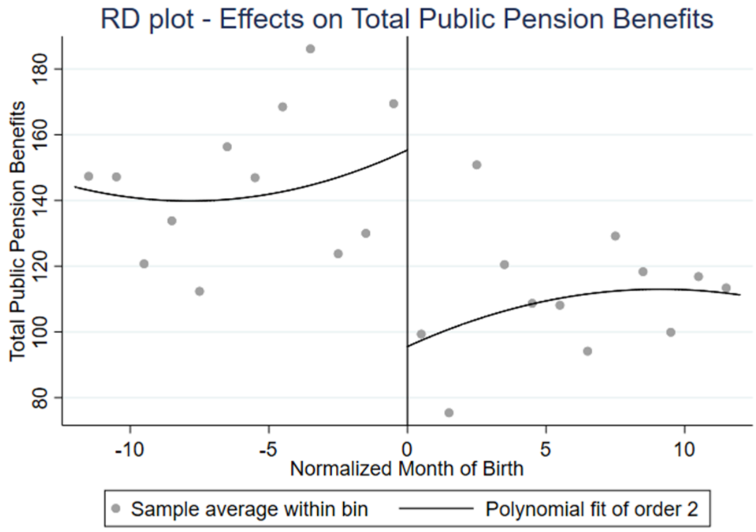

Figure A1: RDD Estimates of the Total Public Pension Benefits (1st stage, quadratic function)
Notes: The figure plots the male total annual EPI benefits at the critical ages by month of birth. The solid lines on the panel correspond to quadratic fitted values. For other details, see the notes to Figure 3.

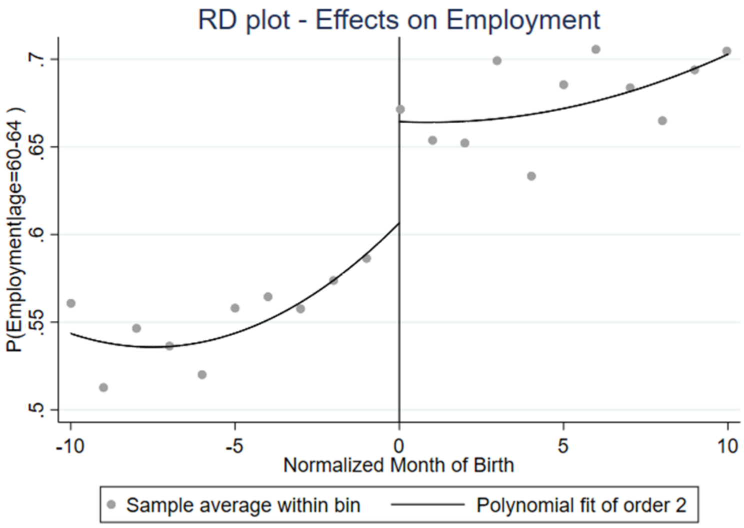

Figure A2: RDD Estimates of the Male Employment (2nd Stage, quadratic function)

Notes: The figure plots the probability of employment for males at the critical ages by month of birth. The solid lines on the panel correspond to quadratic fitted values. For other details, see the notes to Figure 3.

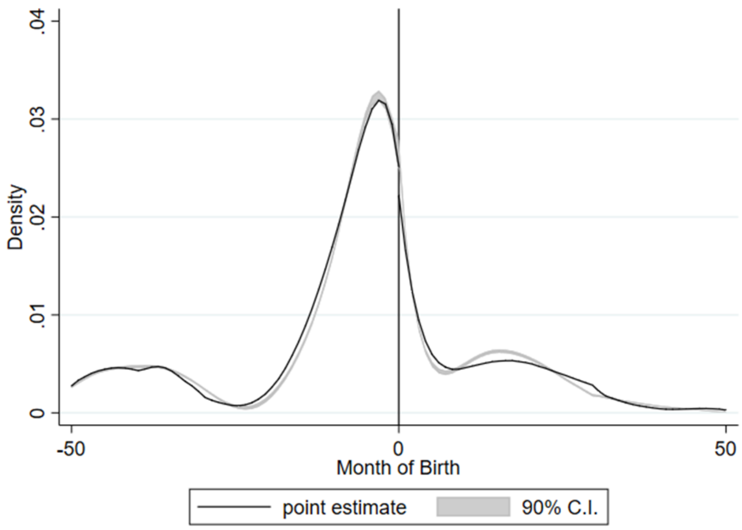

Figure A3: Density of Month of Birth

Notes: The figure plots the density of the running variable. The p-value of the manipulation test by McCrary (2008) is 0.27.

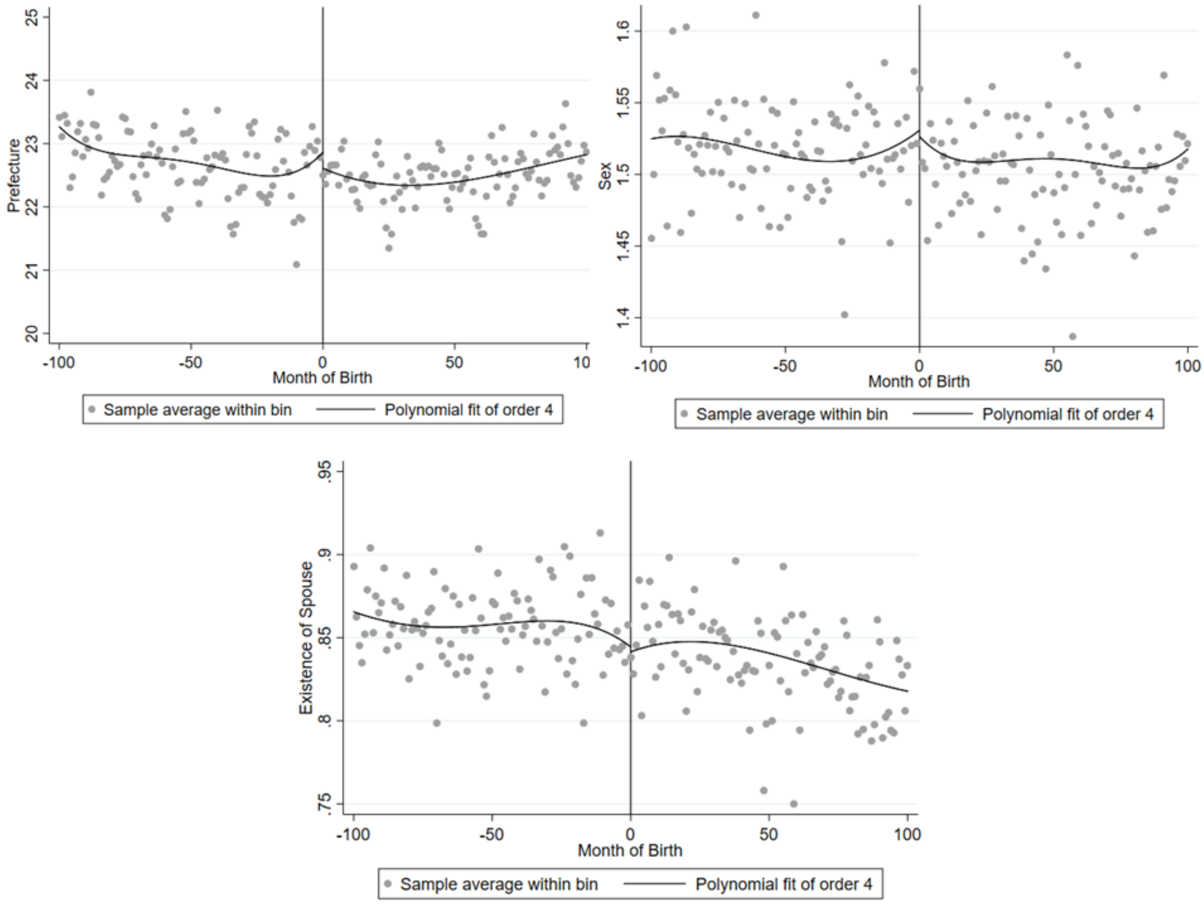

Figure A4: Pre-determined Covariates

Notes: The figures plot the means of the pre-determined covariates along the running variable at the age 60. The upper left figure plots the means of the 47 prefectures where individuals live. The upper right figure plots the means of the gender (1:male, 2: female) of individuals. The bottom figure plots the means of the probability of having a spouse. The p-values of the null hypothesis that the variable is continuous around the cutoff are 0.60, 0.71, 0.50, respectively.

B. Appendix Tables

Table B1: Comparison of Public Pension Reforms in Developed Countries

Country	Eligibility age	Start year	End year	Discontinuity in eligibility age around the cutoff
Japan	60 → 65	2001	2013	1 year (every three years from 2001 to 2013)
US	65 → 67	2003	2027	2 months (every year from 2003 to 2009) 2 months (every year from 2021 to 2027)
Germany	65 → 67	2012	2029	1 month (every year from 2012 to 2023) 2 months (every year from 2023 to 2029)
UK	65 → 67	2018	2027	1-4 months (from Dec 2018 to Oct 2020) 1 month (every month from Apr 2026 to Mar 2027)
Italy	66 → 67	2012	2019	3 months (in 2012) 4 months (in 2016) 5 months (in 2019)
France	65 → 67	2016	2022	The age of the full-rate pension is gradually increasing from 65 to 67 between 2016 and 2022.
Canada	65 (→ 67)	2012	2029	The federal government reversed the reform in 2015.

Notes: The figure shows the comparison of ongoing public pension reforms and full retirement ages for males in the G7 countries.

Table B2: Effects of Raising the Eligibility Age for EPI Earnings-related Benefits

Dependent variable	Employment for 60-year-old males
RDD	0.05*
	[0.03]
Bandwidth	7.85
Dependent mean	0.66
Obs.	2,568

Notes: The parameter is the result from a local linear RDD regression of equation (1) for male labor supply at age 60, where the birth cutoff is April 1953. Since the reform for EPI earnings-related benefits is still ongoing, the cutoff is not normalized. For other details, see the notes to Table 3.

Table B3: Years between the Announcement and Implementation by Cohort

Birth Cohorts	Years between Announcement and Implementation	Eligibility Age for EPI Flat-Rate Benefits
Male		
Before 1941.April		60
1941.April-	6	61
1942.April-	7	61
1943.April -	9	62
1944.April-	10	62
1945.April-	12	63
1946.April-	13	63
1947.April-	15	64
1948.April-	16	64
1949.April-	18	65

Notes: The table shows the eligibility ages for EPI flat-rate pension benefits and years between the announcement and implementation date by birth cohort.

Table B4: Heterogeneity: Labor Supply Responses by Cohorts in the Anticipatory Periods

Cohort	Pension Eligibility	At Age 46	At Age 54	At Age 59	At Age 63
Oldest Affected	Eligible at 61		Year 1995	Year 2000	
Cohort(1941.April-)	(60 $\rightarrow$ 61)		(Announcement)	(Just before Implementation)	
				Mean of RDD estimates for 6 years : 0.018	
Youngest Affected	Eligible at 65	Year 1995			Year 2012
Cohort(1949.April-)	(64 $\rightarrow$ 65)	(Announcement)			(Just before Implementation)
				Mean of RDD estimates for 18 years: 0.004	

Notes: The table compares the RDD estimates of male labor supply responses for the oldest and youngest affected cohorts in the periods between the announcement and implementation, relative to the neighboring control cohorts. The oldest affected cohorts consist of males are born after 1941.April, and the youngest affected cohorts are born after 1949.April. The coefficients report the mean of the significant RDD estimates in the periods between the announcement and implementation by cohort. For other details, see the notes to Table 3.

Table B5: Heterogeneity: Saving Responses by Cohorts in the Anticipatory Periods

Cohort	Pension Eligibility	At Age 46	At Age 54	At Age 59	At Age 63
Oldest Affected	Eligible at 61		Year 1995	Year 2000	
Cohort(1941.April-)	(60 $\rightarrow$ 61)		(Announcement)	(Just before Implementation)	
				Sum of significant RDD estimates for 6 years: 0.00	
Young Affected	Eligible at 65	Year 1995			Year 2012
Cohort(1949.April-)	(64 $\rightarrow$ 65)	(Announcement)			(Just before Implementation)
				Sum of significant RDD estimates for 18 years: 270.20	

Notes: The table compares the RDD estimates of savings for the oldest and youngest affected cohorts in the periods between the announcement and implementation, relative to the neighboring control cohorts. The oldest affected cohorts consist of males born after 1941.April, and the youngest affected cohorts consist of males born after 1949.April. The coefficients report the sum of the significant RDD estimates in the periods between the announcement and implementation by cohort. For other details, see the notes to Table 3.

Table B6: Heterogeneous Saving Responses: RDD Interacted with the Anticipatory Periods

Dependent variable	Male saving at critical ages
RDD*Years between Announcement and Implementation	22.46
	[16.67]
Bandwidth	24.00
Mean of the dependent variable (10,000 Yen)	810.54
Obs.	1,147

Notes: The parameter is the result from a local linear RDD regression of equation (1) for male savings at the critical ages, where the treatment dummy is interacted with the periods between the announcement and implementation. For other details, see the notes to Table 3.

Table B7: Heterogeneity by Savings Instrumented with IV

Dependent variable	Male employment at their critical ages
RDD*Saving	-0.002*
	[0.001]
1st-stage F-Statistic (Prob > F)	3.88(0.05)
Wu-Hausman (P-value)	21.44(0.00)
Mean of the dependent variable	0.60
Mean of the savings (1,000,000 Yen)	8.287
R squared	0.40
Obs.	8,584

Notes: The parameters are results from a local linear RDD regression of equation (1) for the male employment at the critical ages, where the treatment dummy is interacted with the amount of savings, and savings are instrumented with the average saving rates in the pre-announcement period given area. The unit of the interacted saving term is 1,000,000 Yen ($\approx$ 10,000 USD). For other details, see the notes to Table 3.

Table B8: Heterogeneity by Education

Dependent variable	Male employment at the critical ages
RDD*Education	-0.001
	[0.006]
Functional form	Linear
Kernel	Triangular
Pre-treatment mean	2.58
Obs.	9,192

Notes: The parameter is the result from a local linear RDD regression of equation (1) for male employment at the critical ages with the interaction term where the treatment status is interacted with the educational levels. Educational variable takes 1 if an individual is a junior high school graduate; takes 2 if an individual is a high school graduate; takes 3 if an individual is a vocational school graduate; takes 4 if an individual is a junior college graduate; takes 5 if an individual is a university graduate; takes 6 if an individual graduates a graduate school. For other details, see the notes to Table 3.

Table B9: Placebo Test: Individuals Not Enrolled in EPI

Dependent variable:	(1)	(2)	(3)	(4)
Employment for 60-year-old males not enrolled in EPI				
RDD	-0.030	-0.029	-0.003	-0.056
	[0.036]	[0.043]	[0.040]	[0.036]
Functional form	Linear	Quadratic	Linear	Quadratic
Kernel	Triangular	Triangular	Uniform	Uniform
Bandwidth	35.17	50.34	25.46	67.26
Dependent var mean	0.36	0.36	0.36	0.36
Obs.	3,340	5,288	1,826	6,458

Notes: The parameters are results from separate RDD regressions of equation (1) for male labor supply at the age 60, where the sample is restricted to males not enrolled in EPI. Odd-numbered columns report the local linear RDD estimates, and even-numbered columns report the local quadratic RDD estimates. Columns (1) and (2) are estimated with a triangular kernel, and columns (3) and (4) are estimated with an uniform kernel kernel. The cutoff for the running variable is 1941.4.1. For other details, see the notes to Table 4.

Table B10: Placebo Test: Responses before the Announcement

	(1)Two years before the announcement	(2)One year before the announcement
Dependent variable:	Male employment at the age before the announcement	
RDD	-0.020	-0.003
	[0.032]	[0.039]
Bandwidth	11.21	5.81
Dep var mean	0.93	0.93
Obs.	1,563	1,547

Notes: The parameters are results from separate RDD regressions of equation (1) for male labor supply at the ages prior to the announcement of raising eligibility ages for EPI benefits. Specifically, the first column reports the RDD estimate of male labor supply two years before the announcement, and the second column reports the RDD estimate of male labor supply one year before the announcement. For other details, see the notes to Table 3.

Table B11: Placebo Cohorts and Cutoffs

Birth cohorts	(1)Before 1939.4.1 vs After 1939.4.1	(2)Before 1937.4.1 vs After 1937.4.1
Dependent variable: Male employment at age 60		
RDD	0.051	-0.014
	[0.053]	[0.060]
Bandwidth	16.70	9.35
Dependent var mean	0.58	0.59
Obs.	3,108	2,443

Notes: The parameters are results from separate RDD regressions of equation (1) for male labor supply at the age 60 for placebo birth cohorts. Specifically, the first column compares birth cohorts born before and after 1939.4.1, and the second column compares birth cohorts born before and after 1937.4.1. For other details, see the notes to Table 3.

Table B12: Placebo Tests for Other Outcome Variables

	(1)Placebo Cohorts and Cutoff	(2)Response before the Announcement
Panel A: Earnings		
RDD	62.94	-45.62
	[55.89]	[63.89]
Bandwidth	36.66	35.87
Dependent var mean (10,000 Yen)	507.43	652.83
Obs.	940	1,333
Panel B: Consumption		
RDD	-54.62	30.84
	[34.39]	[44.15]
Bandwidth	34.06	20.24
Dependent var mean (10,000 Yen)	399.90	381.38
Obs.	10,222	5,892
Panel C: Savings		
RDD	128.52	317.23
	[146.57]	[252.66]
Bandwidth	63.97	37.61
Dependent var mean (10,000 Yen)	951.46	703.10
Obs.	1,301	1,014
Panel D: Health Status		
RDD	-0.10	0.00
	[0.11]	[0.05]
Bandwidth	30.82	24.00
Dependent var mean (1:there is a health problem)	0.13	0.08
Obs.	4,611	5,936

Notes: The table shows the RDD estimates of the placebo tests for different outcome variables. Panel A shows the result for earnings, panel B shows the result for consumption, panel C shows the result for savings, and Panel D shows the result for health status (existence of a health problem). The first column shows the estimated result for the placebo cutoff (1937.4.1), and the second column shows the estimated result for the response two years before the announcement. The unit for Panel A, B and C is 10,000 Yen ($\approx$ 100 USD). For other details, see the notes to Table 3.

Table B13: Placebo test: Labor Demand Side

Dependent variable:	Occupations	Regular Employees	Firm Size
RDD	-0.21	-0.21	15.89
	[0.18]	[0.19]	[63.40]
Bandwidth	24.00	9.50	8.08
Dependent var mean	5.88	0.76	567.72
	(12 categories)	(1:Yes, 2:No)	(Number of employees)
Obs.	7,655	10,509	7,490

Notes: The parameters are results from RDD regressions of equation (1) for labor demand side. The dependent variable for the first column is occupations, which are categorized into 12 job categories defined by the Ministry of Health, Labor, and Welfare. The dependent variable for the second column is the dummy variable for regular employees. The dependent variable for the second columns is the number of employees. For other details, see the notes to Table 3.

Table B14: Effects of Raising the Mandatory Retirement Age

Dependent variable	Employment for 60-year-old males
RDD	0.060***
	[0.017]
RDD*Mandatory Retirement	0.039*
	[0.021]
Bandwidth	72.00
Dependent var mean	0.66
Obs.	15,830

Notes: The parameters are results from a RDD regression of equation (1) for the male employment at the age 60, where the treatment dummy is interacted with the start of the mandatory retirement policy change. The coefficient in the first raw reports the average effect of raising pensionable age, and the coefficient in the second raw reports the heterogeneous effect by the mandatory retirement policy change. For other details, see the notes to Table 3.

Table B15: Robustness: Non-parametric Estimates by Bandwidth

Dependent variable	Male Employment at the critical ages		
	Bandwidth=10	Bandwidth=15	Bandwidth=20
RDD	0.059***	0.071***	0.086***
	[0.019]	[0.016]	[0.015]
Bandwidth	10.00	15.00	20.00
Dependent mean	0.60	0.60	0.60
Obs.	13,959	17,032	19,353

Notes: The parameters are results from separate RDD regressions of equation (1) for male labor supply with fixed bandwidths indicated in the column heading. For other details, see the notes to Table 3.

Table B16: Robustness: Inclusion of Covariates

Dependent variable	Male employment at the critical ages
RDD	0.076***
	[0.018]
Bandwidth	5.37
Dependent mean	0.60
Obs.	9,633

Notes: The parameter is the result from a RDD regression of equation (1) for male labor supply at the critical ages with the covariates (geographic area and spousal age). For other details, see the notes to Table 3.

Table B17: Robustness: Log approximation

Dependent variable	Log approximation at the critical ages	
	Savings	Consumption
RDD	0.32**	0.01
	[0.16]	[0.02]
Functional form	Logarithm	Logarithm
Dependent mean	6.23	6.29
Obs.	3,947	12,928

Notes: The parameters are the results from separate RDD regressions of equation (1) for different dependent variables. Each dependent variable takes the inverse hyperbolic sign. For other details, see the notes to Table 3.

C. Data Appendix

C.1 Data Access

I use data from *Comprehensive Survey of Living Conditions* from 1986 through 2015 to analyze the effect of raising the eligibility age for public pension on the labor market. As of the data application date,[25] the data from 1986 through 2015 were available. The data are administered by the Ministry of Health, Labor and Welfare. The aggregated data are publicly available,[26] but the use of the micro data (household-level data) was restricted to government-affiliated personnels by the Japanese Statistics Law, Article 33.[27] Due to this law, the data cannot be disclosed to third parties and cannot be used for other purposes. To obtain the data, individuals need to apply for data access to the Ministry officials in charge of the data. In the application process, applicants must submit a detailed research proposal, a list of variables of interest, and other application formats defined by the Statistics Bureau, the Japanese Ministry of Internal Affairs and Communications.[28] Upon request, I can provide the contact address and am willing to further assist persons interested in the data.

C.2 Creation of Dataset

I took three steps to analyze the effect of the public pension reform on the labor market using the data. First, the data are household-level, whereas my primary variable of interest is individual-level labor supply. To see the effect of the reform on individuals, I decomposed the original household-level data into individual-level data, keeping original unique household IDs.[29]

Second, the data consist of five questionnaires: household questionnaire, income questionnaire, health questionnaire, savings questionnaire, and long-term care questionnaire.

[25] The data application date was May 28th, 2017.

[26] The aggregated data are available at e-Stat, which is the portal site of the Japanese government Statistics (https://www.e-stat.go.jp/en).

[27] The Japanese Statistics Law, Article 33 is available through this link (in Japanese).

[28] The guideline of the Statistics Law, Article 33 is also available at the website of the Ministry.

[29] For the analysis of spillover effects on family members, I use the original household IDs.

Table C3: List of Variables by Questionnaire

Questionnaire	Variables	Questionnaire	Variables
Household	Residential areas	*Income*	Earnings
	Household ID		Pension benefits
	Family member ID		Private pensions
	Family structure	*Health*	Hospitalization
	Consumption		Hospital visits
	Relationship		Worry or stress
	Gender		Subjective symptoms
	Age		Other health problems
	Working statuses	*Savings*	Amount of Savings
	Regular employees		
	Job categories		
	Firm size		
	Hours (Days) worked		
	Pension statuses		
	Education		
	Year of birth		
	Month of birth		

Notes: The table shows the list of used variables by questionnaire.

The household questionnaire is the main questionnaire and provides individual characteristics for each household members. The other questionnaires provide additional information on incomes, health, savings, and long-term care. In my analysis, I did not use long-term care questionnaire since long-term care is out of the interest of this paper. All data files were provided by year by questionnaire, and I combined the files by questionnaire by year using unique household IDs, residential areas, and years and months of birth.

Finally, all the data by the Ministry are provided in text files. Unfortunately, the raw text files are not organized and cannot be imported into a statistical software as they are. To analyze the data using a statistical software, I created additional files by year and questionnaire that tell the software how to read the data.

Following these steps, I created a comprehensive annual individual-level dataset that cover many generations over long periods. Table 1 presents summary statistics of the main variables. There are several advantages of using this dataset. This dataset enables me to analyze the dynamics of individual behavioral responses before the announcement through after the implementation. Furthermore, since the original data are household-level, it is also possible to study spillover effects within family members. Finally, the comprehensive information on individual lives enables me to estimate not only the average effect of pension reform on older workers' labor supply, but also heterogeneous effects and effects on previously under-analyzed margins.

C.3 List of Variables

Table C3 presents the list of variables by questionnaire. Since the household questionnaire is the main questionnaire of the data, I used many variables in the questionnaire. The summary statistics for the main variables are described in the text.