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Essays in Public and Labor Economics

A dissertation submitted in partial satisfaction of the
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by

Nobuhiko Nakazawa

Committee in charge:

Professor Julie Cullen, Chair
Professor Itzik Fadlon
Professor Roger Gordon
Professor Seth Hill
Professor Ruixue Jia

2020

The dissertation of Nobuhiko Nakazawa is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California San Diego

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DEDICATION

To my wife Yukina and my son Alex.

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ABSTRACT OF THE DISSERTATION

Essays in Public and Labor Economics

by

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Doctor of Philosophy in Economics

University of California San Diego, 2020

Professor Julie Cullen, Chair

This dissertation consists of three papers in public and labor economics. My work investigates how individuals, municipalities, and firms respond to incentives created by public policies and provides empirical evidence on their efficacy.

Chapter 1 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 at the critical ages are heterogeneous across closeness to the implementation date

due to anticipatory responses.

Chapter 2 studies the effect of allocating central administrators on local government units. During the 2000s, Japanese central administrators were actively transferred from the central government to mentor and monitor local governments. Exploiting the timing of hosting transfers and rich administrative data, I find that municipalities with transferred central administrators in fact persistently improved fiscal discipline by shrinking expenditure and lowering debt. Heterogeneity analyses reveal, though, that transferred administrators temporarily increase local expenditure and categorical grants in fields closely related to their respective departments.

Chapter 3 investigates the effect of raising the mandatory retirement age and introducing a continued reemployment system on older workers and young job-seekers. In 2006, Japanese companies were required to raise the mandatory retirement age from age 60 to at least age 63 or to introduce a continued employment system that creates flexible positions for older workers to continue at the same company. Relying on quasi-experimental variation in exposure to the policy change according to pre-reform norms by industry, geography, and firm size, I find that the reform was effective in terms of decreasing the job separation rate of older workers. It also decreased the job finding rate of young people for firms that were more affected by the policy change.

Chapter 1

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

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 at the critical ages 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 adult children and effects on other labor market outcomes such as savings.

1.1 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, because the reform for females is ongoing and my dataset does not cover the full periods for the female reform.

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 novel 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

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

and savings relative to non-affected cohorts.

The labor supply responses to raising the eligibility age are heterogeneous at the critical ages across cohorts depending on the number of years between the announcement and implementation date; older cohorts respond more at the critical ages, 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. 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), Coile and Gruber (2000), Mastrobuoni (2009), Behaghel and Blau (2012), Staubli and Zweimüller (2013), and Manoli and Weber (2016)).[2] I contribute to this literature with evidence that workers respond to financial incentives from a new empirical setting with a sharper loss in benefits.

Beyond the general contribution to the literature on labor supply responsiveness, my paper makes three advances. First, I find that individuals respond differentially in a manner that is consistent with an important role for anticipation effects. Recent papers examine the *heterogeneity* of behavioral responses to social security reforms across different groups that share common characteristics (e.g., Behaghel and Blau (2012), Hanel and Riphahn (2012), and Staubli and Zweimüller (2013)).[3] My paper provides empirical evidence that individuals differentially respond to the same one-year loss of benefits across cohorts depending on the number of years between announcement and implementation. My empirical result is consistent with Mastrobuoni (2006),

[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. Hanel and Riphahn (2012) find a heterogeneous labor supply response across educational level. Staubli and Zweimüller (2013) find a heterogeneous response across individual health status and wage level.

which theoretically shows that early-informed workers of a reform are less likely to postpone retirement because of their longer time frame for smoothing behavior. Empirical evidence on decreasing treatment effects at the critical ages depending on the scope for anticipation is new to the literature.

Second, 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 other family members. Though most of the existing literature investigates spillover effects within couples,[4] I investigate spillovers from the affected head husband to his children as well as his wife. Empirical evidence on this type of intergenerational spillover effects is very rare. One of the few exceptions is Dahl and Gielen (2018), which empirically show that children whose parents are kicked off of a disability insurance program in the Netherlands are more likely to increase their labor supply. My paper finds that the resident adult children and wife of an affected husband increase their labor supply, suggesting there exist some coordination and network effects within households to offset the negative income shock.

Finally, beyond investigating labor supply, my analysis encompasses a variety of other important *margins*. Though previous studies generally have access to a more limited number of outcome variables, 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, because empirical evidence on the relationship between public pensions and private savings is relatively rare.[5] I provide evidence that the decrease in generosity in public pension increases savings, suggesting the substitution between household savings and public pension benefits. My paper is consistent with a recent paper by Lachowska and Myck (2018), which find substitution between public pensions and private savings in Poland. My paper suggests that the result of the substitution between private savings and social

[4]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).

[5]My paper also provides evidence of the effect of the pension reform on health, but there are recent active literature on the relationship between retirement and health (e.g., Müller and Shaikh (2018), Gorry et al. (2018), Fitzpatrick and Moore (2018), Eibich (2015), and Rohwedder and Willis (2010).

security wealth could be more likely to be generalized to other countries.[6]

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 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 in Japan is the largest. This large reform enables me to not only estimate causal effects on labor supply but to study other margins for which responses might be more difficult to detect. Though exploiting a social security reform in Japan is difficult because of the lack of availability of micro data, I overcome this problem using restricted-use data, the access to which 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 summarizes the main points and concludes.

1.2 Institutional Background

1.2.1 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

[6] Attanasio and Rohwedder (2003) and Attanasio and Brugiavini (2003) also provide evidence on the substitutability in the U.K. and Italy, respectively, though they are not recent evidence.

[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.

[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

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]

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 roughly 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,

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.

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

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 (≈\$26,400), the social security office additionally withholds \$1 of benefits for every \$2 of earnings above the threshold. If earnings are above the second threshold (≈\$40,800), the 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.

1.2.2 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

[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)).

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 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

[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.

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 critical 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 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 to individuals in the sense that the eligibility age for the NP has been 65 since 1961.[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)).

1.3 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:

$$P(Employment|Age, Male)_i = \alpha + \beta 1(MOB_i > cutoff\,date) + f(MOB_i) + \varepsilon_i \quad (1.1)$$

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 attrition around the cutoff. To check this condition, I implement the validity test based on McCrary (2008)

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

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. I also implement some robustness checks to show the results are quantitatively robust. All the results of the validity, placebo, and robustness tests are presented in Appendix.

1.4 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 comprehensive 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.

1.5 Empirical Results

1.5.1 Main Results for Raising the Eligibility Age

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 about 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 the pre-reform benefits and about 20 percent compared to the pre-retirement 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 the affected elderly people.

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.

Effects on Other Outcomes

Table 4 shows the RDD estimates for the different outcome variables. Panel A presents the RDD estimates for intensive margins. The estimates are all positive, suggesting raising eligibility age for public pension also increases intensive margin. Specifically, the RDD estimates for working hours per week and earnings are statistically significant, suggesting increase in working hours leads to the increase in earnings. In contrast, the increases in working days per week and working hours per day are tiny, suggesting that the daily adjustment of working hours and the adjustment of working days are not main behavioral response of the affected cohorts. The percent changes in intensive margins are smaller than those in the extensive margin in Table 3, suggesting the workers' main behavioral responses are more likely to be thorough the extensive margin rather than the intensive margin. One possible mechanism of this large effect on extensive margin is that the negative income shock is large in this public pension reform. Since the affected cohorts lost 20% of pre-retirement earnings, they would not be able to cover lost income only by increasing working hours. Thus, the main behavioral response should be more likely to increase employment rather than working hours.

Panel B reports the RDD estimates for individual health. The columns from (1) through (4) reports the RDD estimates for the measures of individual physical health, and the column (5) reports the effect on individual mental health. The relationship between raising the eligibility age and individual health looks ambiguous; the RDD estimates for physical health and mental health are all statistically insignificant and close to zero, suggesting that delayed retirement associated with raising the eligibility age does not significantly affect individual physical health and mental health. Recent literature provide mixed results on the effect of retirement on health.[20] My paper provides empirical evidence that the delayed benefits and retirement in the specific context in Japan had little effect on health measures.

[20] For example, Fitzpatrick and Moore (2018) find a discontinuous increase in aggregate mortality rate in age 62, the early retirement age in the United States. Müller and Shaikh (2018) find that own retirement positively affects subjective health and leads to a increase in alcoholic consumption. Gorry et al. (2018) provide evidence that retirement improves reported health, mental health, and life satisfaction by using the Health and Retirement Study data.

Panel C reports the RDD estimate for other outcomes. Column (1) reports the RDD estimate on the amount of savings. Raising the eligibility age for the flat-rate EPI benefit 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. Column (2) reports the RDD estimate for consumption. The estimate of the coefficient for consumption is not statistically significant, suggesting that affected cohorts were more likely to increase their labor supply rather than decreasing consumption, in order to cover the lost benefits. One possible mechanism of the insignificant effect on consumption is that the negative income shock is large in this reform. Since affected cohorts lost 20% of pre-retirement earnings, they would not be able to cover lost income only by reducing consumption. Thus, the main behavioral response should be more likely to increase the labor supply, particularly extensive margin, rather than reducing consumption. Column (3) reports the RDD estimate for private pension enrollments. The delayed eligibility of public pension could also increase the enrollment in private pensions for the affected cohorts, because affected cohorts could be more likely to depend on private pensions rather than public pensions. However, the coefficient for the participation in private pension system is positive but not statistically significant, discounting the possibility of this type of substitution.

1.5.2 Impacts of Related Reforms

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

Table 5 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, the female reform started five years after the start of the male reform. Since the data do not span the full time period for females and the female sample is smaller than the male sample, my paper focuses on males but also provides complementary analysis for females.

The estimates for females is similar to the male result; 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.[21]

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.

1.6 Heterogeneity and Mechanism

So far, I find empirical evidence that raising the public pensionable ages increase labor supply at the critical ages. The paper will now test whether the average response to the policy change is different across groups with different characteristics.

Table 6 shows the comparison of RD 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 effect of raising the pensionable age from 60 to 61 on male employment at age 60; the second column provides the 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 (to older policy changes) than for younger affected cohorts (to newer policy changes), even though the magnitude of the lost pension benefits is the same across 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

[21]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 more on anticipatory responses in the heterogeneity section.

negative income shock is the same across cohorts, there is variation in the number of years between the announcement and implementation date. As Appendix Table B3 and 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 the shock of the impact of the policy change during longer years between announcement and implementation. Table 7 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.[22][23]

Liquidity constraint seems to play an important role in this anticipatory response. Since the length of periods between announcement and implementation 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. *Anticipatory Response* in Table 8 reports the pooled RD estimate for outcomes across all of those ages between announcement and implementation. As one can see, the cumulative effect of anticipatory responses on savings 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, as in column (1).

[22] It is important to note that there may be direct effects according to age of treatment. Delaying retirement by one year from age 60 to 61 may be different from delaying from age 64 to 65. However, the baseline employment rates do not change very much across critical ages for critical cohorts. Table 6 also shows the effect in terms of percent (rather than percentage points) and the treatment effects are still decreasing, ruling out this possibility.

[23] 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.

1.7 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 sign and magnitude of spillovers depend on both income effects and complementarity of leisure between couples. As Table 9 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, suggesting there exist some coordination benefits within couples to offset the negative income shocks. 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.[24]

Furthermore, Table 9 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 of themselves.[25] 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 social security system and fiscal imbalances. As a result, well-informed children would be more likely to increase their labor supply.[26]

[24] 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.

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

[26] The 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.

1.8 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, 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.

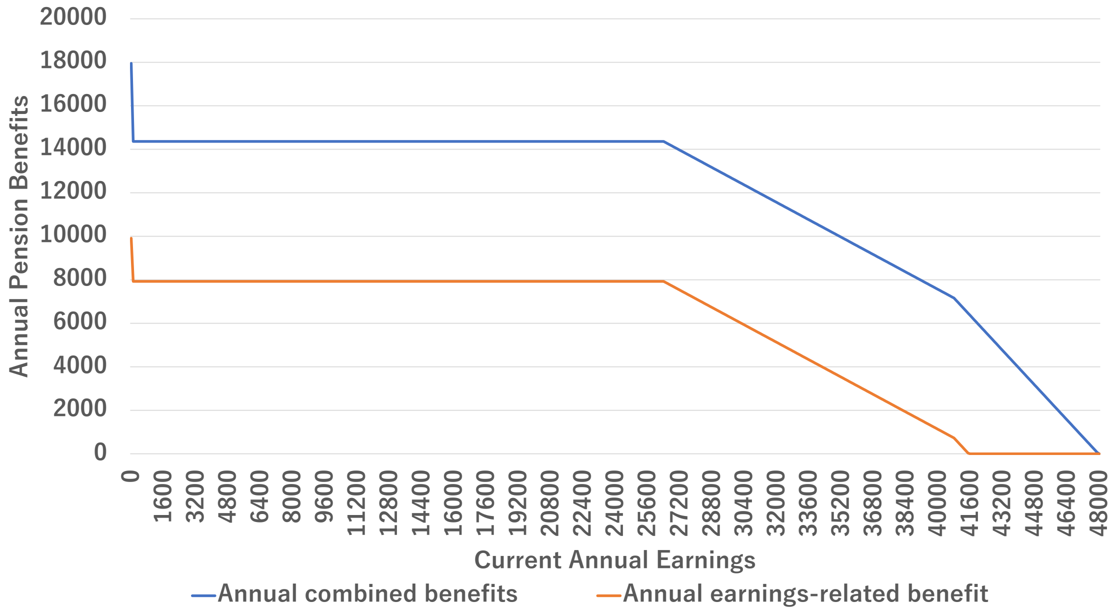
My paper includes two novel contributions. First, I find that individuals respond differentially to the same one-year loss of benefits across cohorts depending on the number of years between announcement and implementation. The fact that treatment effects are decreasing along with the scope for adjustment 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 in many countries with the rapidly aging populations. My empirical results suggest that public pension reforms differentially and comprehensively affect individual behaviors, and policy makers should not design them by only looking at the average effect on older workers' labor supply. 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 design of public pension reforms.

1.9 Acknowledgments

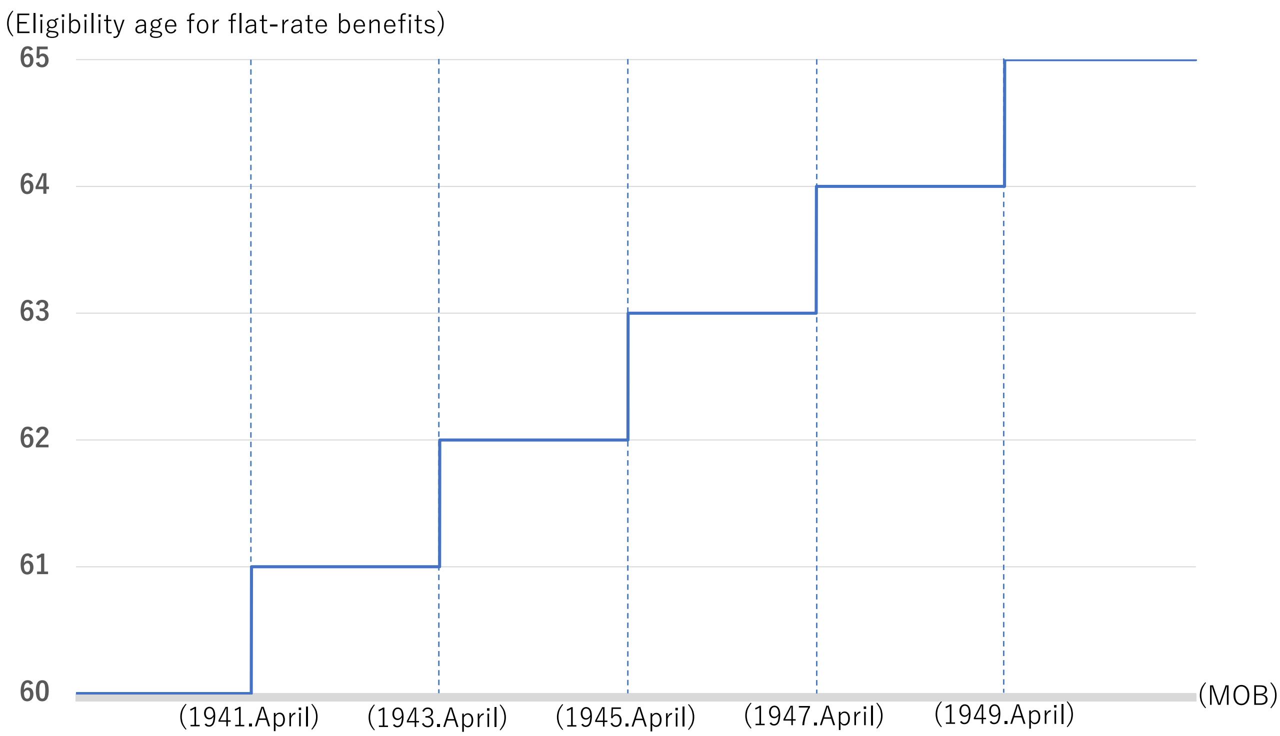
Chapter 1, in full, is currently being prepared for submission for publication of the material. Nobuhiko Nakazawa. “The Effects of Increasing the Eligibility Age for Public Pension on Individual Labor Supply: Evidence from Japan”. The dissertation author was the primary investigator and author of this material.

Figures and tables



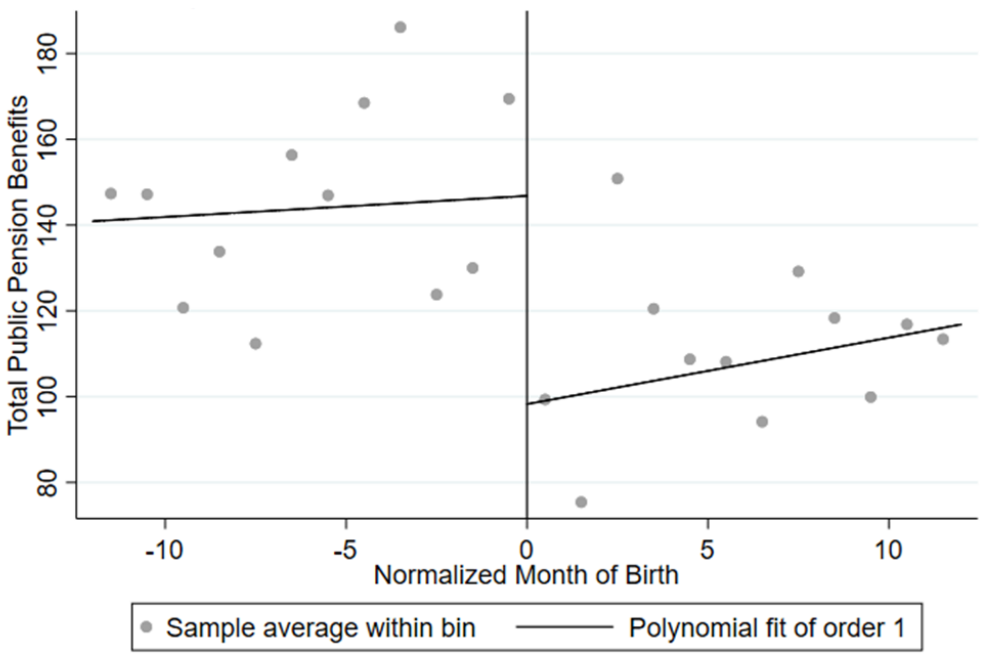
Notes: The figure shows annual public pension benefits at the eligibility age for typical workers before the pension reform. The blue line shows the combined earnings-related and flat-rate benefits; the orange line shows just the earnings-related benefit; the gap between two lines show the flat-rate 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 1.1: Japanese annual public pension benefits



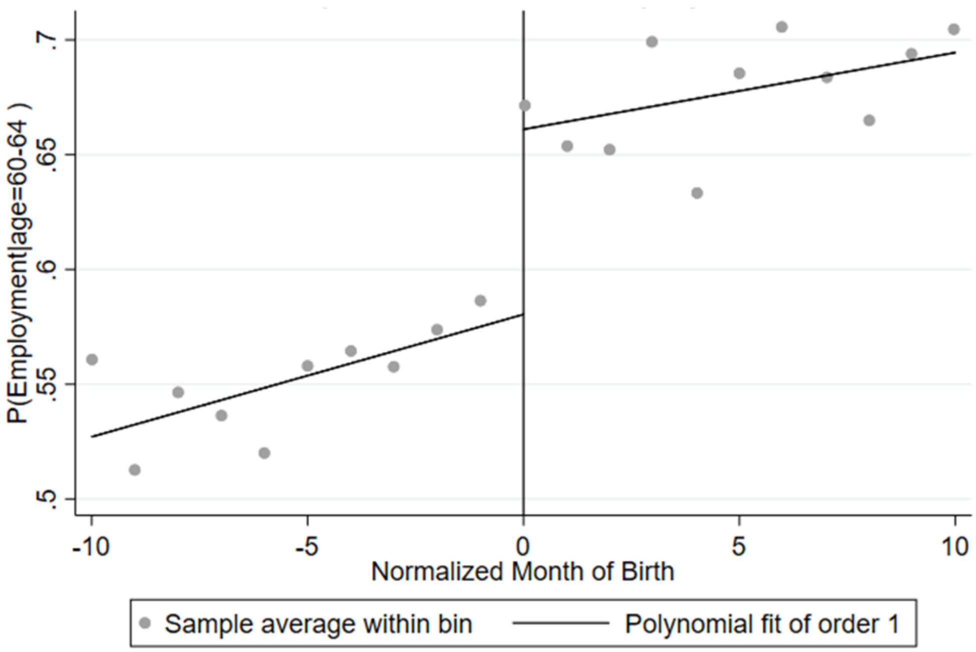
Notes: The figure plots the male eligibility age for EPI flat-rate benefits by month of birth.

Figure 1.2: Public Pension Reform Schedule for Male EPI Flat-rate Benefits



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 ($\approx$ 100 USD) per year.

Figure 1.3: RDD Estimates of the Total Public Pension Benefits (1st 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.

Figure 1.4: RDD Estimates of the Male Employment (2nd Stage)

Table 1.1: 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 1.2: Effects on the Total Public Pension Benefit

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 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 ($\approx$ 100 USD) per year. The number of observations reports effective number of observations. 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 1.3: Effects on Male Labor Supply

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 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 1.4: Effects on Other Outcomes

Panel A: Intensive Margin	(1)Working Hours per Week	(2)Working days per Week	(3)Working Hours per Day	(4)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	
Dependent mean	37.94	4.79	7.69	1.00	
Obs.	6,083	6,189	6,065	11,356	
Panel B: Effects on Health	(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
Panel C: Other Outcomes	(1)Savings	(2)Consumption	(3)Private Pension		
RDD	95.27*	12.60	0.03		
	[54.93]	[13.77]	[0.03]		
Functional form	Linear	Linear	Linear		
Kernel	Triangular	Triangular	Triangular		
Dependent mean	856.42	331.81	0.15		
Obs.	3,947	12,928	1,761		

Notes: The parameters are results from separate local linear RDD of equation (1) for various outcome variables indicated in the column header at the critical ages. As for Panel B, each dependent variable is a dummy that takes 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 your daily life? (5) Do you have a worry or stress? As for column (3) in Panel C, the dependent variable takes 1 if and individual is enrolled in a private pension and 0 otherwise. For other details, see the notes to Table 3.

Table 1.5: 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 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 1.6: 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 1.7: 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 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 1.8: RDD Estimate by Age by Cohort

	(1)Employment	(2)Consumption	(3)Aggregate Savings
Panel A: Older Affected Cohort			
Anticipatory Response	0.00	8.95	15.71
	[0.01]	[9.72]	[43.23]
Implementation (critical age)	0.09***	41.02*	35.49
	[0.03]	[21.17]	[131.96]
Delayed Response (critical age +1)	-0.01	-30.59	75.86
	[0.05]	[37.77]	[174.42]
Panel B: Younger Affected Cohort			
Anticipatory Response	0.00	-10.06	92.20*
	[0.01]	[11.48]	[55.11]
Implementation (critical age)	-0.04	21.09	9.86
	[0.03]	[19.98]	[155.94]
Delayed Response (critical age +1)	-0.01	-141.49*	N/A
	[0.06]	[75.60]	

Notes: The table shows RDD estimates of equation (1) for different outcomes indicated in the column header at different ages by cohort. Panel A shows the result for the older affected cohort, and Panel B shows the result for the younger affected cohort. *Anticipatory Response* reports the pooled RDD estimate across all of the ages between announcement and implementation. *Implementation* reports the RDD estimate at critical age at policy implementation. *Delayed Response* reports the RDD estimate at age one year after policy implementation. There is no data for delayed response for savings for the youngest cohort. Reported in brackets are standard errors. Statistical significance is indicated by * at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 1.9: 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.

1.10 Appendix

Validity, Placebo and Robustness

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.

Validity Tests

Manipulation

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.

Smoothness 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 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.

Placebo Tests

Individuals 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 B7, 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.

Response 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 B8 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 B9 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 B10 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 B11. 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.

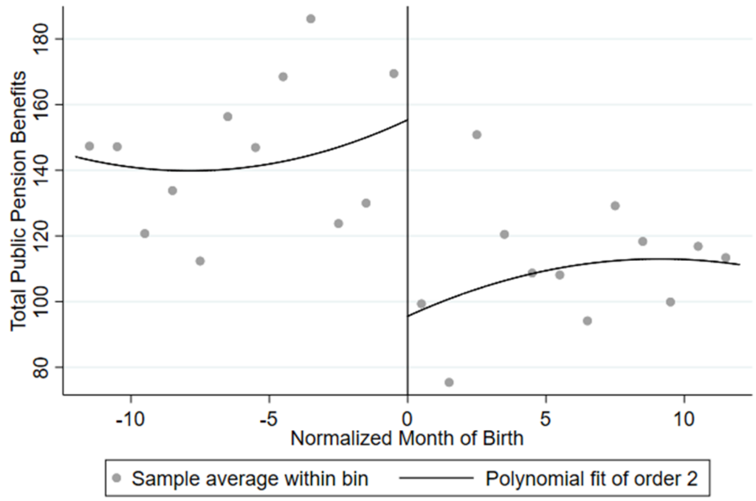
Robustness

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 B12 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.

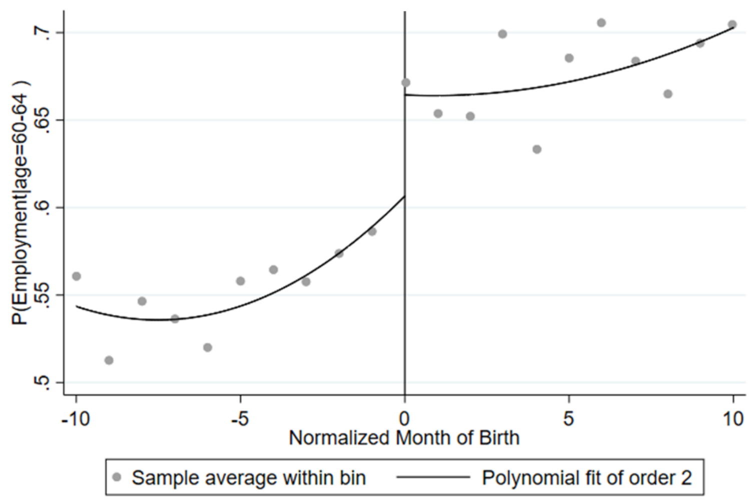
Appendix Table B13 shows the RDD estimates with additional predetermined covariates. The inclusion of covariates should not affect the estimated discontinuity under the non-manipulation assumption. The estimates of the covariates adjustment (area and spousal age) in the RDD equation (1) show the consistent results with the baseline.

Appendix Figures



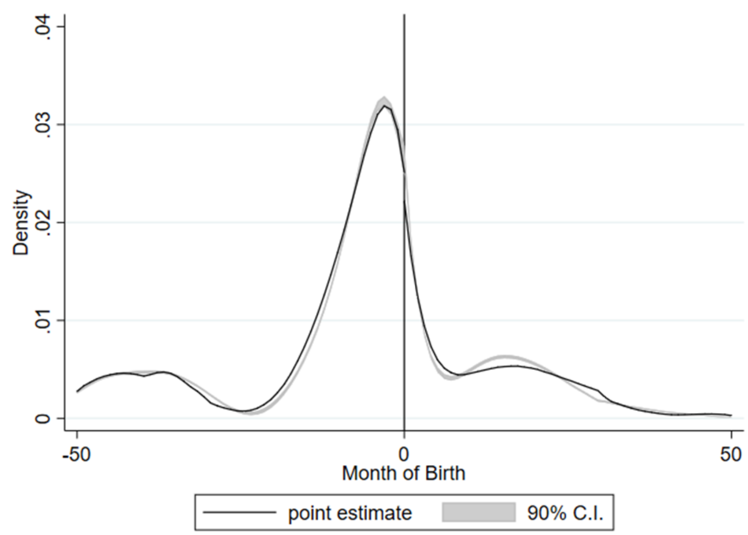
Notes: The figure plots the male total annual EPI benefits at the critical ages by month of birth. The solid lines on the panel correspond to quadratic fitted values. For other details, see the notes to Figure 3.

Figure 1.A1: RDD Estimates of the Total Public Pension Benefits (quadratic function)



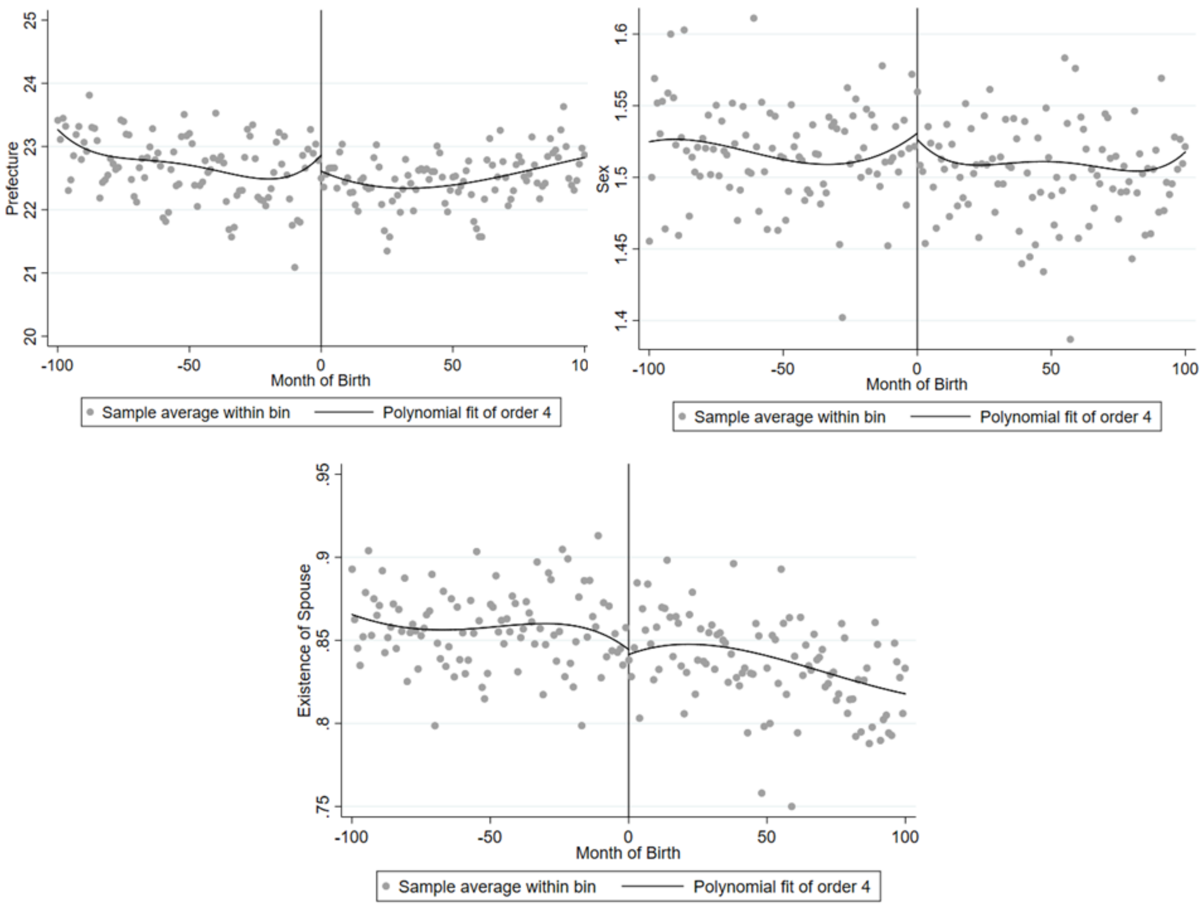
Notes: The figure plots the probability of employment for males at the critical ages by month of birth. The solid lines on the panel correspond to quadratic fitted values. For other details, see the notes to Figure 3.

Figure 1.A2: RDD Estimates of the Male Employment (quadratic function)



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

Figure 1.A3: Density of Month of Birth



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.

Figure 1.A4: Pre-determined Covariates

Appendix Tables

Table 1.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 1.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 1.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 by birth cohort.

Table 1.B4: Years between Announcement and Implementation for Oldest and Youngest Cohorts

Cohort	Age 46	Age 54	Age 60	Age 64
Oldest Affected (1941.April-)		Announcement	Implementation	
Youngest Affected (1949.April-)	Announcement			Implementation

Notes: The table shows the years between announcement and implementation for the oldest and youngest affected cohorts.

Heterogeneity by Family Structure

Appendix Table B5 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 is 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; consumption is less elastic for individuals with dependents, leading to higher labor supply responses for those groups. In sum, the table provides evidence of the heterogeneous labor supply across family structure.

Heterogeneity by Education

Appendix Table B6 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 negative and not statistically significant, suggesting there is little differential behavioral response across educational levels using this natural experiment.

Table 1.B5: 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 1.B6: 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 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 1.B7: Placebo Test: Individuals Not Enrolled in EPI

Dependent variable:	(1)	(2)	(3)	(4)
Employment for 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 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 1.B8: 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 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 1.B9: 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 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 1.B10: 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 1.B11: 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 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 that takes 1 if an individual is a regular employee and 0 otherwise. The dependent variable for the third column is the number of employees in a firm an individual worked for. For other details, see the notes to Table 3.

Table 1.B12: 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 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 1.B13: 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 local linear RDD 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.

Chapter 2

Do mentoring and oversight matter? The effects of allocating central administrators to local government units: Evidence from Japan

Abstract

During the 2000s, Japanese central administrators were actively transferred from the central government to mentor and monitor local governments in the hopes of mitigating deficit bias. Exploiting the timing of hosting transfers and rich administrative data, I find that municipalities with transferred central administrators in fact persistently improved fiscal discipline by shrinking expenditure and lowering debt. Voters seem to reward the incumbent mayor in the local election for better administration and fiscal conditions. Heterogeneity analyses reveal, though, that transferred administrators temporarily increase local expenditure and categorical grants in fields closely related to their respective departments.

2.1 Introduction

Deficit bias is a general concern for local governments. There is a tendency for local governments to run fiscal deficits, due to electoral incentives, vertical fiscal imbalances, and implicit bailout by the central government. To address deficit bias, central governments in many countries implement fiscal rules fixed in laws that impose numerical constraints on local government debt and deficits.

Instead of setting a common fiscal rule, a novel policy was implemented in Japan to circumvent deficit bias: promoting the allocation of central administrators to local government units. Compared to the standard regulatory policies for deficit bias, this allocation policy is top-down, but more flexible and less explicit. This paper quantifies the effects of this novel policy on local governments.

Japanese local governments' fiscal conditions worsened in the 1990s, especially in terms of debt accumulation and continuing fiscal deficit. In response, the central government first legislated a common limit on local governments' fiscal deficit in 1997. However, this limit was soon withdrawn by the central government, because of the economic downturn resulting from the Asian Crisis. Thus, in the following year, the central government decided to promote personnel exchanges between the central government and local governments to encourage administrative reform and develop human resources. The allocation of central administrators to local governments could improve outcomes via both mentoring local administrators and monitoring discipline in local governments.

The empirical analysis is based on a rich city-level panel dataset constructed from Japanese administrative data. The dataset consists of detailed fiscal information and the full history of assigned administrators for more than 1,700 municipalities over 15 years. This rich panel dataset allows in-depth analysis of the effects of transferred central officials on local municipalities.

The key underlying assumption for the identification is that the timing of hosting a transfer is idiosyncratic conditional on observables, and there is no unobservable factors correlated with the assignment that could affect the evolutions in municipal fiscal conditions. Though this assumption cannot be directly tested, I provide empirical evidence to support the internal validity of my research design. Specifically, I run placebo tests by estimating the effect of the assignment on the outcome variables one year prior to the actual arrival of the central administrators. The results of the placebo tests suggest that unobservable characteristics potentially correlated with the timing of hosting a transfer cannot explain the improvement of fiscal discipline. I also implement a specification test and show that the timing of departure and the length of the stay cannot be anticipated by the past local fiscal conditions.

The empirical results are broken down into two parts. In the first part, I analyze the average effects of the allocation of central administrators on fiscal conditions of local governments. Estimated results show that municipalities with transferred central bureaucrats improve fiscal discipline by shrinking expenditure by 8.6% and lowering debt by 8.9%. The average effects of improving fiscal conditions are persistent and continuing in the years after the central bureaucrat leaves, suggesting that central administrators' mentoring effects take root in local governments even after they leave. I also find that the probability of the sitting mayor winning in the next local election increases, suggesting that local residents seem to reward the incumbent as a result of better administration and fiscal conditions.

In the second part, I explore heterogeneity in impacts on specific categories of expenditures and grants in local governments, according to the transferred administrators' home departments. Central administrators bring with them preferences, knowledge, and personal connections that may influence success in obtaining grants as well as which spending areas are protected. I find some evidence that discretionary grants provided by home departments and expenditures in related areas increase during the stay. Hence, the mentorship and oversight by allocated administrators help localities manage deficit bias; but, the newly formed political connections might lead to new misallocations. Interestingly, these heterogeneous effects are transitory and disappear with the bureaucrat. This is consistent with the mechanism that a central administrator's specific preference and personal connections of local governments are strengthened only during the stay.

The existing literature primarily investigates the effects of formal and strict fiscal rules. In contrast, this paper contributes to understanding the effect of a more flexible fiscal rule, in the sense that the rule is top-down but does not impose a common or explicit response on local governments. Though the efficacy of formal fiscal rules is controversial in the literature,[1] I find a more flexible fiscal rule without numerical targeting has an impact on local governments.

My paper also contributes to the political business cycle literature documenting increases in spending and reductions in taxes in election years.[2] My results suggest that the allocation of central administrators decreases the incentive for mayors to overspend. This corroborates the expectation that appointed officials are insulated.[3] The result is consistent with Rose (2006), which discusses the relationship between political cycles and fiscal rules, whereby the strict fiscal rules have been found to moderate the cycle but at the risk of too much inflexibility when shocks occur. My paper also finds that the allocation policy and resulting better administration and fiscal condition increase the probability of reelection for the incumbent mayor, resulting in a win-win situation for both players.

More generally, my paper also contributes to the literature on bureaucratic transfers and human resources. Bureaucratic transfers are common in many countries. Previous papers have mainly focused on the career perspectives of the bureaucrat as part of the public sector carrier path, and have found pervasive corruption channels. For example, in China, bureaucratic rotations and promotions across jurisdictions are a common practice, and Shi et al. (2018) find that these transfers affect investment flow, especially in real estate and construction industries. Bureaucratic rotations are also common in Japan, and Yunoue (2005) finds a positive correlation between a small part of

[1] Some previous studies find that fiscal rules are effective for constraining expenditure. For example, Grembi et al. (2016) and Christofzik and Kessing (2018) show that fiscal rules enforced by a central government are effective in reducing accumulated debt of local governments, and Clemens and Miran (2012) show that fiscal rules significantly affect state government spending and long run budget constraints. Other studies, such as Wyplosz (2012) and Alesina et al. (1999), argue that fiscal rules may be ineffective and vulnerable due to enforcement problems and unpredictable events.

[2] For example, Shi and Svensson (2006) and Enkelmann and Leibrecht (2013) find increases of public spending and fiscal deficit in election years, and Fatás and Mihov (2003) show prudent fiscal policy is mainly explained by political variables. Foremny and Riedel (2014) find a political cycle in local business tax rates, and Labonne (2016) find a cycle in the level of employment.

[3] In the context of comparing elected politicians and appointed public officials, Whalley (2013) demonstrates that having an appointed, rather than an elected, city treasurer decreases borrowing costs in cities by about 20-30 percent. Enikolopov (2014) also shows that elected public officials are more likely to engage in targeted redistribution as opposed to appointed bureaucrats. Hessami (2018) find that elected mayors attract more grants in electoral years whereas appointed mayors do not.

block grants (6%) and the presence of transferred administrators in local governments.[4] Compared to these, this paper focuses on the impact of the bureaucrat on the unit of the transfer and shows who is assigned to a municipality matters. I find that even short political connections lead to a distortion, by increasing grants provided by home departments and expenditures in related areas, during the stay.

The remainder of the paper is as follows; the next section discusses the institutional background; Section 3 describes the identification strategy; Section 4 lays out data; Section 5 presents empirical results; Section 6 discusses the underlying mechanism; Section 7 concludes.

2.2 Institutional Background

2.2.1 Municipal Government System in Japan

Japanese municipal governments provide a variety of public services to local residents and play an important role in general government spending.[5] There are more than 1,700 municipalities such as cities, towns, and villages in Japan. Mayors in municipalities are elected by residential vote every four years, and all other public staff members are locally hired in the same municipality. That is, there is one elected mayor but multiple locally hired public staff members in a typical municipality.[6] The simple average of the number of public officials per municipality is about 523 as of 2016.[7][8]

[4] For other related papers, Brierley (2017) studies Ghanaian bureaucrats, who typically work at a number of local governments over their careers, and shows they engage in corruption because politicians have discretionary control over their careers. Iyer and Mani (2012) study the Indian case, where politicians typically use reassignments across posts to control bureaucrats, and show that the most important posts are not necessarily given to the most competent bureaucrats. Bessho (2010) and Hayashi and Kaneto (2010) investigate Japanese bureaucratic rotations, and they estimate the average effects of political connections on local governments at 47 prefectural levels.

[5] Local governments in Japan account for more than 70 percent of general government spending when excluding social security expenditure in fiscal year 2015.

[6] As for vice mayors, the incumbent mayor appoints them and requires approval from the congress. Vice mayors are typically chosen from the local public officials as a promotion.

[7] According to the Japanese Ministry of Internal Affairs and Communications (2017), there are 899,936 staff members in 1,721 municipalities as of April 1, 2016.

[8] As an upper tier of local governments, 47 prefectures provide public services on a larger scale than municipalities. This paper focuses on municipalities and does not analyze prefectural variables.

Figure 1 shows the municipal relative expenditure and revenue shares for the various components spanning the years from 2000 to 2014. Japanese municipal expenditure is mainly financed by local taxes, borrowing, grants, and other miscellaneous revenue. More than 20% of municipal revenues came from local taxes. Local taxes are categorized into three categories based on the degree of local discretion: fixed tax rate, standard tax rate, and arbitrary tax rate. Fixed tax rates, like the tobacco tax rate, are constant throughout Japan. On the other hand, arbitrary tax rates, like the city-planning tax rate, can be freely changed by municipalities. Regarding standard tax rates, like the residential tax rate, the central government stipulates "standard" tax rates that municipalities should set; however, local governments are able to set higher-than-standard tax rates if they have special circumstances. Furthermore, municipalities can introduce new local taxes not listed in local tax law as long as they obtain approval from the central government.

As for borrowing, when municipalities want to issue local government bonds, they must consult with upper level government (i.e., the central government or prefecture) in principle. However, municipalities with good fiscal performance do not need to consult first and instead notify the upper-level government about the issuance of bonds beforehand. In contrast, local governments with poor fiscal conditions do in fact have to get approval in advance to issue bonds.[9]

About 40% of local governments' revenue came from grants from the central governments. There are two types of grants in Japan: categorical grants and block grants. Japanese categorical grants, which are called national government disbursements, can only be used for specific areas of expenditure. To obtain categorical grants, a local government first needs to apply for a specific categorical grant from department-related ministries. Then, each ministry decides whether or not to give the specific grant to the specific locality. Since each ministry focuses on its own field, and other ministries are not involved in the process, the Japanese bureaucratic system is called a "vertically divided administrative system," which is often criticized as an inefficient form of bureaucracy. In contrast, Japanese block grants, which are called local allocation taxes, can be used for any type of expenditure. Unlike discretionary categorical grants, the allocation of Japanese block grants

[9]Specifically, approval is required if the ratio of debt payment to the financial scale is 18 percent or higher.

is mechanically determined by a formula based on differences in local governments' needs and revenue.[10] The formula for block grant is given by standard basic needs subtracted by standard basic revenue, where standard basic needs represents the expenditure necessary to provide basic local public goods, and standard basic revenue represents 75% of tax revenue and other revenue sources.[11] The main idea of the disbursement of block grants is that resource-poor local governments receive large block grants, while rich local governments receive small, or no amounts.[12] Since the main function of Japanese block grants is to serve as a financial equalization tool, they adjust imbalances in financial capacity among municipalities. The main criticism about block grants is moral hazard; since the allocation of block grants is directly tied with to tax revenue estimates, debt payment, and investment expenses for capital formation of each local government, having more fiscal capacity pushes down the amount of block grants.[13] Thus, municipal governments have less incentive to implement fiscal reform due to the grant provided by the central government.

2.2.2 Fiscal Reform and Transfer of Central Administrators

During the 1990s, Japanese local governments' fiscal conditions worsened: the Japanese economy became stagnant after the burst of the asset price bubble in 1990, and a large number of local government bonds were issued to stimulate regional communities. Consequently, outstanding local government bonds accumulated sharply (Figure 2), and several municipalities declared themselves to be in a state of fiscal crisis.

To address this problem, the central government aimed to improve the fiscal conditions

[10]To be precise, 94% of block grants are based on an explicit formula, where 6% are not based on a formula. Yunoue (2005) shows the latter to be correlated with political factors.

[11]Standard basic needs include public debt payments and investment expenses for capital as well as other expenses calculated by objective components such as population and area. Standard basic revenue includes not only local taxes, but also local transfer tax, which is collected as a national tax and transferred to local governments afterwards. For details on the allocation of Japanese block grants, please see Doi and Ihori (2009), Ihori (2009), Hirota and Yunoue (2017b), and the Japanese Ministry of Internal Affairs and Communications (2017).

[12]In fiscal year 2015, only 59 out of a total of 1,718 cities, towns, and villages did not receive block grants, but the other 1,659 municipalities received block grants.

[13]For example, Tajika and Yui (2004) argues that Japanese block grants lose local governments' willingness to pay efforts to increase their own revenue, and Hirota and Yunoue (2017b) describe the Japanese block grant system as "atypical" from a global perspective.

of local governments. In the Fiscal Structural Reform of 1997, the central government decided upon policies to shrink local governments' expenditure and borrowing, setting the numerical goal of reducing the fiscal deficit of the central and local governments to GDP to below 3 percent by 2003. However, this standard regulatory fiscal rule was withdrawn immediately after the occurrence of the Asian Financial Crisis and domestic economic downturn. Hence, in the following year, to help local governments improve in autonomy and become fiscally independent from the central government, the Cabinet decided in the Decentralization Promotion Plan on the more generic goals for local governments to streamline administration, review expenditure efficiency, and increase local tax revenue. In this plan, promoting allocation of central bureaucrats to local governments was documented for the purpose of reforming local administration and developing human resources. Thus, the promotion of allocating central administrators by the central government can be considered a device of informal mentoring and monitoring of local governments. Accordingly, the ratio of municipalities to transfered administrators evolved sharply (Figure 3). The plan also documented a short-term rotation and municipal interaction to avoid corruptions rising from long-term close relationship between municipality and a higher-ranked central administrator.

These transferred central administrators typically work as vice mayors or general managers, which are typically the second-highest and third-highest positions in local governments. Their actual activities that central administrators were tasked with cover a board range: supervising staffs, residing general affairs, giving advice to the municipal mayor, conducting policies in place of the municipal mayor in cases the mayor delegated authority, and so forth. Hence, they were endowed with not only a simple advisory role, but also a policy-making role. The number of vice mayors depends on each municipality; some municipalities appoint plural vice mayors, while other municipalities appoint only one vice mayor or even no vice mayors. In contrast, the number of general managers is typically only one per municipality. The transfers displace the incumbents, but in some cases additional positions were created in the local governments for these transferred administrators. The term of transfer for a centrally appointed bureaucrat is short (typically 2-3 years), to avoid political corruptions between the locality and the transfered person. After completion of the term, the transfered person returns to the central government, and in some cases, another bureaucrat from the central government is transferred to that same local government.[14]

2.3 Identification Strategy

To identify the causal effect of hosting a central administrator on local government units, I estimate the model following:[15]

$$y_{i,t} = \alpha + \beta Bureaucrat_{i,t} + \gamma Lag_{i,t} + \delta Lead_{i,t} + \zeta X_{i,t} + \lambda_i + T_t + \varepsilon_{i,t} \tag{2.1}$$

where $y_{i,t}$ is the outcome variable for municipality i in year t; $Bureaucrat_{i,t}$ is a dummy that equals one if the municipality hosts a transfer in year t; $Lag_{i,t}$ and $Lead_{i,t}$ are dummies to estimate persistent effects and test for any pre-arrival effects. $X_{i,t}$ is a vector of control variables, such as demographic variables and political variables. λ_i is a municipality-fixed effect; T_t is a year-fixed effect. The central parameter of interest is given by β, which measures the concurrent effect of the allocation of central administrators on municipal fiscal conditions. Standard errors are clustered at the prefecture level, to account for unobservable shared shocks.

One key underlying assumption for the above identification is that there are no unobservable factors correlated with the timing of the arrival that could affect fiscal conditions. Though this assumption cannot be directly tested, I provide evidence to support the internal validity of my research design. First, central administrators were dispersed throughout Japan (Appendix Figure A1). Though they are more likely to be located in municipalities with higher shares of working-aged individuals and that had local elections one year before, hosting a transfer does not depend on

[14] It is also important to understand whether there are contemporaneous policies that might confound the analysis of the bureaucrat allocation. Since the Japanese central government does not directly purchase local government bonds, I can rule out one that might apply in other settings like the EU countries. As another possibility of potential confoundness, new fiscal rules were established in 2008 after the bankruptcy of city Yubari; the central government imposed four new fiscal indices on municipalities (Hirota and Yunoue (2017a)). I also did the robustness by limiting the sample to years prior to this policy change.

[15] For further discussion on the theoretical background, see de Chaisemartin and d'Haultfoeuille (2019). For an empirical application, see Enikolopov et al. (2011).

the existing fiscal discipline (Appendix Table B1). I also estimate the effect of the assignment on the outcome variables one year prior to the actual assignment by including a lead indicator ($Lead_{i,t}$), which takes 1 if the timing is one year prior to the actual arrival of a central administrator. Table 1 presents the results of this placebo test for many fiscal variables, and the coefficients are all statistically insignificant for all the variables. The results of the placebo tests suggest that unobservable characteristics potentially correlated with the timing of hosting cannot explain the improvement of fiscal discipline.

The other important assumption for identification is that the timing of the exit is exogenous to past local fiscal outcomes. Since equation (1) decomposes the periods after the arrival into the years during the stay and the years after the exit, the coefficient on the indicator for the years during the stay (β) picks up the causal impacts concurrent to the stay as stay length is not endogenous to fiscal outcomes. For example, if central bureaucrats stay until fiscal conditions improve, concurrent effects of the stay versus any persistent effects after exit would be confounded by endogeous stay length.[16] To disentangle the effect of hosting central administrators from potential confounders, I also implement the following specification test:

$$P(Leave\,in\,the\,next\,year|Stay\,in\,this\,year)_{i,t+1} = \alpha + \beta fiscal\,condition_{i,t} + \gamma X_{i,t} + \lambda_i + T_t + \varepsilon_{i,t} \tag{2.2}$$

where the dependent variable is the likelihood of a bureaucrat leaving in a given year, given that the bureaucrat was there in the prior year. I allow that to depend on past fiscal conditions, in a model that also includes municipality and year fixed effects. As shown in Appendix Table B2, the coefficients for the past fiscal outcomes are all statistically insignificant, suggesting that years of hosting a transfer cannot be anticipated by the fiscal performance of local governments in their previous years. Thus, the decision to withdraw a central administrator does not convey information per se about the future fiscal performance of the community.

[16] As explained in the institutional background section, the term of office of hosting a transfer is typically around 2-3 years to avoid corruptions. After the term, either a successor from the central government is transferred to the same municipality or the transfer simply ends. The decision to extend a transfer could be endogenous.

In addition to estimate the effect of the stay, I also investigate the persistent effect of hosting a transfer by including lag indicators ($Lag_{i,t}$) to the estimation equation. Specifically, I estimate the short-run persistent effect and long-run persistent effect by including two kinds of lag indicators to the equation (1). The short-run persistent indicator is equal to one if the timing is one year after a bureaucrat leaves a municipality and zero otherwise. The long-run persistent indicator takes one if the timing is more than one year after a bureaucrat leaves a municipality and zero otherwise. If the estimates for these parameter are sizable and statistically significant, then the allocation of transferred bureaucrats has persistent effects even after bureaucrats leave the localities. Otherwise, the policy effects are transitory.

In the second part of the empirical result section, I explore heterogeneity in impacts on specific expenditure and grants in local governments, according to transferred administrators' home departments. Central administrators bring with them preferences, knowledge, and personal connections that could influence success in obtaining grants as well as the changes in specific spending areas. To capture the heterogeneity across transferred central administrators, I estimate the following model with an interaction term:

$$y_{i,t} = \alpha + \beta Bureaucrat_{i,t} + \gamma Bureaucrat_{i,t} \times RelatedBureaucrat_{i,t} + \delta X_{i,t} + \lambda_i + T_t + \varepsilon_{i,t} \quad (2.3)$$

where $RelatedBureaucrat_{i,t}$ is a bureaucrat dummy that equals one if local government i hosts a central administrator from a home ministry related to the outcome variable in year t. I define *related* ministry, which administers the specific expenditure and grants, as shown in Appendix Table B3. The central parameter of interest is γ, which measures how the allocation effects depend on the previous experience of central administrators in the central government. In other words, the slopes of the regression lines between dependent variables and bureaucrats are different for the various categories of ministries, and γ indicates how different those slopes are. I also investigate the pre-arrival and persistent effects of related central administrators by including the lead- and lag-indicators of related central administrators in the same way as the main specification (1). As shown in Appendix Table B4, the choice of department a bureaucrat came from was not correlated with the changes of the related expenditure and revenue of municipalities before the arrival.

2.4 Data and Descriptive Statistics

I create a large municipality-level panel spanning the years 2000 to 2014, from different administrative data sources. Within this period, all municipalities are included in my sample. My main outcome variables of local governments' fiscal data are taken from the *Situation of Local Government Finance*, which is administered by the Statistics Bureau of the Ministry of Internal Affairs and Communications. In this data, fiscal variables are itemized for many detailed spending and revenue categories, by municipality and year. All the outcome variables, except for the weighted average interest rate of local government bonds, are divided by the municipal population to be converted into per capita variables.

Another key important variable is the bureaucratic rotations. Information on the assignment of bureaucrats is from the *Situation of Personnel Exchange between Central Government and Local Government* by the Japanese Cabinet Bureau of Personnel Affairs. This personnel dataset provides the full history of bureaucratic central government positions and transfers to local governments. The full data are not publicly available, and independent contact with Japanese Cabinet Bureau of Personnel Affairs is needed for data access.

My dataset also includes political data from *Electoral Data*, which is administered by the Ministry of Internal Affairs and Communications. Political data are used for both the outcome variable and control variables: the probability of the incumbent mayor winning in the next local election, and a local election dummy by year and municipality. I also use population data from the *Population Census* by the Statistics Bureau of the Ministry of Internal Affairs and Communications to control for demographic factors.

Table 2 presents descriptive statistics for the main variables in my sample. A total of 28,820 observations from 2000 to 2014 is in my data set. Figure 4 also presents the distribution of years of exposure to transferred central administrators in hosting municipalities. The number of years of exposure to transferred administrators in local governments is reasonably low, reflecting the short-term rotation policy to avoid corruptions.

2.5 Empirical results

2.5.1 Main Results for Fiscal Discipline

Results for the Expenditure Side

Table 3 presents the estimate of β by equation (1), where the outcome variable is total spending per capita in local governments. The coefficient for the bureaucrat dummy is statistically significant and negative across specifications with and without municipality-by-year fixed effects, suggesting that cities with a central bureaucrat spend less and improve fiscal discipline. The magnitude of the current effect of allocating central administrators on decreases in total expenditure per capita is about 52.36 thousand Yen in the main specification (3) with municipality-by-year fixed effects and demographic and political controls. Since the average of the dependent variable is about 608.83 thousand Yen, the impact of the policy change on municipal expenditure is about 8.6 percent. In sum, Table 3 provides evidence that cities with transferred central administrators improve fiscal discipline by shrinking expenditure. In contrast, the coefficient for the pre-arrival effect in Panel A in Table 1 is not statistically significant, suggesting that unobservable factors correlated with the timing of the assignment of central administrators did not affect the change in expenditure.

The persistent effect is summarized in Table 7. The coefficient for the short-run persistent effect for total expenditure in Panel A is negative and statistically significant, suggesting that local governments continue to hold down expenditures even after the transferred bureaucrats leave the local governments. This result indicates that the effect of fiscal improvement takes root in municipalities after they leave the localities. Furthermore, the persistent effect continues for long years as in the coefficient for the long-run persistent effect, and the magnitudes of the persistent effects are larger than that of the current effect. This result suggests that cutting expenditures can require long periods of time before they take effect in some fields. In sum, reducing expenditures has a lasting effect even after the transfer ends.

Table 4 reports the regression results of expenditure decomposed into several components: social welfare, general administration, agriculture, public works, debt service, and others. The magnitudes of the effects on current expenditure are different by each spending category. Specifically, expenditures were largely decreased in the fields of general administration, agriculture, debt service, and other miscellaneous expenditures. One possibility of larger effects in these fields would be that transferred bureaucrats could have viewed expenditure in the fields as less important in terms of the welfare and economic growth. For example, expenditures on general administration, miscellaneous expenditure, and debt service are less likely to immediately increase the utility of the local residents. Similarly, the contribution of the agricultural sector to the economic growth is smaller than those of manufacturing and service industries in Japan. Hence, this result could reflect the bureaucratic view of each field. In contrast, Panel A in Table 7 shows that expenditures on social welfare and public works were not affected immediately, but affected with lags. As for social security expenses, some social expenses, such as cost of caring, also include mechanical spending, which would make the expenditure persistent. Furthermore, since social security expenses are closely associated with many interest groups, local residents, and firms, transferred administrators could not have reformed expenditure immediately. In addition, since Japan is the most aged country in the world, elected mayors in localities could be afraid of rapidly decreasing expenditures in social welfare due to the electoral incentives.[17]. As for expenditure on public works, existing contracts could prevent central administrators from reducing expenditures immediately. Since public works are mainly implemented based on contracts, and construction periods typically span over long years, this specific circumstance in infrastructure will be more likely to reduce the expenditure with lags. In sum, the insignificant current effect and significant persistent effect on public works and social welfare suggest that it is hard for central administrators to reform these fields immediately.

[17] Specifically, the life expectancy of Japanese people is 84 years, and the proportion of the population aged 65 and over to the total population is over 25 percent, both of which are the highest in the world.

Results for the Revenue Side

Table 5 reports the results for the revenue-side outcomes. The table shows that local governments achieve fiscal reform by increasing tax and other revenues while decreasing local government loans. Effects on the revenue side also are also persistent as shown in Panel B in Table 7. In particular, local government loans improved years after the transfer, suggesting the effect of fiscal improvement remains even after they leave the local government. The table also shows that the effect on the borrowing cost is statistically significant in the long run. One likely mechanism is that the fiscal improvement affected the borrowing cost of local governments, though it takes time for regional banks and security companies to reflect this improvement in fiscal discipline of municipalities. Another interpretation of this lagged effect on the borrowing cost could be due to characteristics of the data. Since the interest rate used in the analysis is the weighted average of outstanding local government bonds, it would take time for the current improvements in fiscal conditions to affect the interest rate of accumulated outstanding local government bonds.[18][19]

The effect of transferred central administrators on grants is negative and statistically significant. This result might seem inconsistent with the fiscal improvement of local governments at first glance. To understand the underlying mechanism behind the decline of the grants, I decomposed grants into two types: categorical grants and block grants; Appendix Table B5 presents the regression results for categorical grants and block grants. The table shows that the current allocation effect of central administrators is positive for categorical grants, but negative for block grants. As explained in the institutional background section, the size of categorical grants is discretionary and mainly determined by the application form and negotiation between local governments and the central government. The size of block grants, however, is mechanically determined by a formula, where the amount of block grants is directly tied to debt payment, other expenditures

[18] In this sense, using interest rates of local government bonds in the secondary market could reflect the fiscal conditions of local governments immediately. However, the data for the interest rate of municipal bonds in the secondary market are not available for most of the Japanese municipal governments.

[19] The improvement in the cost of borrowing also has implication for welfare and efficiency. The cost of borrowing could work as a measure of efficiency with respect to the management of finance. Hence, we could say the efficiency of local governments becomes better off in terms of the management of finance at the very least. For example, Whalley (2013) uses borrowing cost as a measure of a local government's performance.

such as investment payment to capital, and estimates of tax revenue. Thus, local governments use transferred administrators' personal connections with the central government to increase the size of discretionary grants, while the size of block grants automatically decreases in response to improvements in the fiscal conditions.[20]

Effect on Local Deficits

Column (1) in Table 6 shows the effect of allocating central administrators on municipal deficits. The coefficient is negative and statistically significant at 1% level, suggesting that local governments decrease fiscal deficits and improve fiscal discipline. The coefficient for the persistent effect in Panel C in Table 7 is also negative and statistically significant, suggesting that the effect on fiscal discipline is persistent, even after central administrators leave the municipalities. In contrast, the coefficient for the pre-arrival effect in Table 1 is not statistically significant, suggesting that unobservable factors correlated with the timing of hosting did not drive the changes in local fiscal deficits.

Effect on Local Elections

Column (2) in Table 6 shows the effect of allocating central administrators on mayors' elections. The allocation of central administrators could potentially affect political outcomes in municipalities since this allocation policy improved administration and fiscal conditions of localities. For example, local residents hosting a transfer could reward the sitting mayor for improvements in fiscal discipline and better administration.[21] To see this effect on political outcomes, I regressed the probability of the incumbent mayor winning the next local election in the next 3 years on

[20] Another possibility for the decrease in block grants could be that transferred bureaucrats intentionally tried to decrease the size of block grants to reduce local governments' financial dependence on the central government. As explained in the institutional background section, the central government wanted local governments to become financially independent and achieve the fiscal reform. Several papers argue that the allocation of block grants is not necessarily objective and formula-based, but instead more politically oriented in the sense that bureaucrats can modify items and coefficients of the formula used to determine the allocation of block grants (e.g., DeWit (2002)). Thus, transferred central administrators could have tried to achieve both fiscal improvement and financial independence from the central government by reducing block grants.

[21] Conversely, fiscal conditions could affect the election results. For example, Brender and Drazen (2008) find that fiscal deficits in election years punish the politicians at the elections in developed countries.

bureaucrat dummies.[22] The coefficient for the bureaucrat dummy in the current effect is positive and statistically significant at 1% level, suggesting that the allocation policy also increases the probability of an incumbent mayor in a municipality hosting a transfer winning in the next local election. Since the magnitude of the coefficient divided by the dependent mean is about 0.1, the allocation of central administrators increases the probability of the incumbent winning by more than 10 percent. Thus, voters seem to reward the incumbent mayor for better administration and fiscal conditions. In contrast, the coefficient for the pre-arrival effect in Table 1 is not statistically significant, suggesting that unobservable factors correlated with the timing of hosting a transfer cannot explain the election results.

The result of the political outcomes also suggests the relationship between a transferred central bureaucrat and the sitting mayor. My empirical results suggest that the mayor was less likely to reject the reform plan initiated by the monitor and mentor. Additionally, the resulting improved fiscal discipline increased the probability of reelection for the sitting mayor. Hence, the relationship between them would be a mediation (reconciliation) rather than a conflict (mandate), and the allocation policy results in a win-win situation for both players. The central government could improve the governance of local governments, while the head of local government could increase the probability of reelection for the next term of office.

2.5.2 Heterogeneity of Bureaucrats—Is "Who Came" Important?

So far, we have seen the average effects of the allocation of central administrators on local governments. Specifically, municipalities with transferred central administrators persistently improve fiscal discipline by shrinking expenditure and lowering debt. Thus, the allocation of central administrators is effective in reducing deficit bias for local governments.

However, is the allocation effect uniform regardless of distinctive characteristics between bureaucrats? The effects that allocating central administrators have on local governments' expenditure and revenue could depend on *who* came to the local government. Central administrators

[22] As described in the institutional background section, Japanese local elections occur every four years.

bring with them preferences, knowledge, and personal connections that may influence success in obtaining grants as well as which spending areas are protected. For example, transferred bureaucrats from the Ministry of Infrastructure may want to "protect" against the decrease of expenditure in public works. They could also receive a larger amount of grants in public works through personal connections with the home department in the central government. Hence, local governments' specific expenditure and revenue could also be affected by each central administrator's respective home ministry, and bureaucrats' heterogeneities could be transmitted to local governments. Thus, in this subsection, I investigate the heterogeneous effects of the allocation of bureaucrats as an extension of the research.[23]

Heterogeneity by Home Departments

First I explore heterogeneity in impacts on specific expenditure and grants in local governments, according to transferred administrators' home departments. The estimated results by equation (2) for the expenditure side are shown in Table 8. The average effects (coefficients for *Bureaucrat*) are overall negative, but the heterogeneous effects (coefficients for $Related$) are overall positive. This results suggest that average transferred administrators improved fiscal discipline by decreasing general expenditure while protecting specific department-related expenditure. For example, in column 1, cities with bureaucrats decreased the expenditure on social welfare on average by 4.42 thousand Yen per capita, but cities with bureaucrats from the Ministry of Health, Labor, and Welfare significantly protected against the decrease in social expenses by 13.90 thousand Yen. This result suggests that the transfered central administrators from the Ministry of Health, Labour, and Welfare are more conscious of social security expenses than other bureaucrats. Since the dependent variable mean is 170.32 thousand Yen, this heterogeneous effect of increasing related expenditure reaches 8.2 percent. Similarly, in column 6, the average effect of transferred central administrators

[23] As another variation of the heterogeneous effects, I also explore the heterogeneous effect by the initial fiscal condition of municipalities. As shown in Appendix Table B6, the effects of central administrators on fiscal discipline are slightly larger for municipalities with a higher borrowing ratio in the initial year than other municipalities with a lower borrowing ratio in the initial year. This result makes sense because municipalities with bad initial fiscal conditions have the potential to improve administration and fiscal discipline.

on education expenses is close to 0. However, bureaucrats from the Ministry of Education, Culture, Sports, Science and Technology significantly increase expenditure on education by 10.77 thousand Yen (15.6%). These results provide some evidence that transferred central bureaucrats increase specific expenditures in related areas during the stay, suggesting the heterogeneity by their home departments has an influence on local governments' expenditure. In contrast, the coefficients for the pre-arrival effect for the overall bureaucrat in Panel A in Appendix Table B4 are statistically insignificant, ruling out the possibility that unobservable factors correlated with transfered bureaucrats affected the evolution of outcome variables. Furthermore, the pre-arrival effect for the related bureaucrats in Panel A in Appendix Table B4 are all statistically insignificant, suggesting that the choice of department is also idiosyncratic conditional on observables.

The persistent effect of heterogeneous analysis is summarized in Table 10. The coefficients for the average effects (Bureaucrat) on expenditure side in Panel A are overall statistically significant, while the coefficients for the heterogeneous effects (Related) are all statistically insignificant. In other words, the average effects of improving general fiscal conditions are overall persistent, whereas the heterogeneous effects of increasing related expenditure are overall transitory. This result suggests that central administrators' mentoring effects relating to the maintenance of fiscal discipline take root in local governments even after they leave, whereas local governments no longer have incentives to protect specific expenditures once they leave.

Table 9 presents the results for the heterogeneous effects on the revenue side (categorical grants). The heterogeneous effects (coefficients for the related term) are all positive, suggesting that cities with transferred central administrators are more likely to receive categorical grants related to their respective departments. For example, in column 1, the average allocation effect of receiving categorical grants for ordinary construction is -3.95 thousand Yen per capita, but municipalities with transferred administrators from the Ministry of Land, Infrastructure, Transport and Tourism receive a significantly larger amount of categorical grants by 3.48 thousand Yen. Similarly, in column 2, the average effects of bureaucrats on grants for other construction are negative and significant, but cities with central administrators from the Ministry of Land, Infrastructure, Transport and

Tourism received a larger amount of categorical grants. In column 3, the average effects of central administrators on welfare are positive, but the effect is bigger for municipalities with transferred administrators from the Ministry of Health, Labour, and Welfare. In column 4, cities with bureaucrats overall decreased the amount of grants for electricity, but cities with bureaucrats from the Ministry of Economy, Trade, and Industry and from the Ministry of Education, Culture, Sports, Science and Technology have a positive and insignificant heterogeneous effect on acquisitions of the grant. In column 5, whereas the overall allocation effects of central administrators on grants for medical expenses and children are insignificant, the effect is positive and statistically significant for localities with central administrators from the Ministry of Health, Labour, and Welfare. In sum, the current effect in the table shows that discretionary grants provided by home departments increase during the stay. In contrast, the coefficients for the pre-arrival effect in Appendix Table B4 are all statistically insignificant for both overall transferred bureaucrats and bureaucrats from specific departments, ruling out the possibility that unobservable factors correlated with the timing of hosting overall bureaucrats and specific bureaucrats affected the evolution of categorical grants.

As for the heterogeneous persistent effect for revenue side in Panel B in Table 10, the coefficients for the heterogeneous effects are all statistically insignificant. This insignificant heterogeneous persistent effect indicates weaker negotiation power and personal connections in obtaining discretionary grants after they leave the locality. Municipalities could no longer have incentive to receive more discretionary grants in the related area to the transferred administrators after they leave. In sum, the results indicate that local governments with transferred bureaucrats are more likely to get discretionary grants in the related areas in the current period, but the effect disappears once they go back to the central government.

Heterogeneity by Positions

To capture another type of heterogeneity, I also categorize transferred central administrators into two subgroups depending on their positions in municipalities: a vice mayor or general manager. The effect of hosting central bureaucrats in local governments could vary with the bureaucrat's position in local governments. For example, vice mayors (higher position) could be more likely to acquire national grants from the central government because bureaucrats working as vice mayors are typically older and have higher titles, suggesting that they have more negotiation power than general managers. Alternatively, bureaucrats working as general managers (lower position) could be more likely to achieve better performance in other fields due to the closeness to other local public staff. To estimate the effect by each position, I run regressions with the following dummies. $Vice\ Mayor_{i,t}$ is a bureaucrat dummy that is equal to one if a transferred central administrator works as a vice mayor in local government i in year t. $General\ Manager_{i,t}$ is a bureaucrat dummy that is equal to one if the transferred central administrator works as a general manager in local government i in year t. I also include the lead- and lag- indicators for these variables in the same way as before.

Appendix Table B7 reports the result of the allocation of central administrators by their positions. This result shows that the bureaucrats' effect on local government is slightly different depending on the position, and the coefficients for the current effects are comparatively more significant and higher for general managers than vice mayors. One possible interpretation is that younger general managers are more passionate and productive in implementing reforms in localities than older vice mayors. Another interpretation could be that the coefficients on vice mayors may be noisier because the number of vice mayors changes by each ministry, whereas the number of general managers is typically limited to one.[24]

2.5.3 Robustness

Alternative Sample Size

I implement a robustness test by using two alternative samples to account for two breaks. First, in 2008, the new fiscal rules were implemented to all municipalities, as described in the institutional background section. Second, in 2011, Japan experienced the Great East Japan Earthquake. This magnitude 9 earthquake caused a tsunami and the subsequent meltdown of nuclear power

[24]Unfortunately, there is no available administrative data on the number of vice mayors by municipality by year.

plants in the Fukushima region.[25] This catastrophe could have caused financial deteriorations in specific regions.

To address these issues, I run the following additional regressions with different samples. First, I limit samples to regions outside the Fukushima region, as it suffered from severe nuclear damage. Second, I limit the sample periods to ones before 2008, which was during the introduction of the new fiscal rules and before the disaster. The results are presented in Appendix Table B8 and Appendix Table B9, respectively. These results are consistent with the results by the original estimated results, suggesting my estimations are robust to alternative samples.

Alternative Specification

I also implement another specification, by estimating average impacts in the years immediately following arrival regardless of how long the bureaucrat remains. Specifically, I run the following event-study type specification;[26]

$$Y_{i,t} = \lambda_i + \sum_k \beta \times T(k) + \gamma X_{i,t} + \varepsilon_{i,t} \tag{2.4}$$

where $Y_{i,t}$ is the outcome variable for local government i in year t; λ_i is a municipality-fixed effect; $X_{i,t}$ is a vector of control variables, such as demographic variables and political variables. $T(k)$ is a time dummy that equal one if it takes k years after the arrival of a central administrator. For example, T(+1) takes 1 if the year is one year after the arrival of a transferred bureaucrat, and 0 otherwise. The results are presented in Appendix Figure B2 and Appendix Figure B3. The municipalities' tax revenue sharply increased after the arrival of bureaucrats, and this fiscal improvement decreased the borrowing cost with the lag of two years. These results provide consistent results with the main specification.

[25] The damage of the disaster was severe; the central government confirmed more than 15,000 deaths and 120,000 collapsed buildings from the disaster, and local residents close to nuclear power plants were obliged to evacuate.

[26] The downside of the event-study approach is that this specification cannot capture the effect of the duration of the hosting a transfer. Hence, I use the two-way fixed effect model in my main analysis.

2.6 Mechanism and Discussion

I find that the allocation of central administrators contributes to the improved fiscal discipline of local governments. Furthermore, these allocation effects are also heterogeneous by the transferred central administrators' home departments. Specifically,

- Fiscal Discipline (Average Effects)
  - Municipalities with transferred central administrators improved fiscal discipline by shrinking expenditure and lowering debt.
  - The average effects on general fiscal conditions are overall persistent.
  - Voters seem to reward the incumbent mayor in the local election for better administration and fiscal conditions.
- Specific Expenditure and Grants (Heterogeneous Effects)
  - Transferred central administrators increase local expenditure and categorical grants in fields closely related to their respective departments.
  - The heterogeneous effects are transitory once administrators leave the localities.

What mechanisms can explain these average and heterogeneous effects of allocating central administrators on municipalities? Table 11 summarizes the three channels causing average and heterogeneous effects: monitoring, mentoring, and past experience and personal connections in the central government. Monitoring and mentoring affect the general fiscal conditions (average effects), whereas personal connections and past experience affect specific expenditure and revenue (heterogeneous effect).

2.6.1 Monitoring

Under the large debt of local governments during 1990s, the central government became highly conscious of improving fiscal discipline. However, local governments had little incentive to reform their fiscal conditions because of the moral hazard that arose from the expectation of bailouts, financially equalizing block grants from the central government, and mayors' electoral incentives. Thus, allocated central administrators could work as monitors by observing closely to decrease the aforementioned incentives to implement fiscal reform.[27]

Theoretically, monitoring improves general fiscal conditions of a municipality in the current period by decreasing the moral hazard incentive, but not in the future periods after the allocated administrators leave the municipality. For example, a municipality would neither overspend nor expect to be saved by the central government if a monitor closely watched its actions. However, once monitors return to the central government, a municipality would have more moral hazard incentives again.

Empirically, I find that the allocation of central administrators significantly improves general fiscal conditions both in the current and future periods. The empirical result is consistent with the theoretical prediction in the current period, suggesting transfered central administrators worked as monitors in the current period. However, monitoring cannot fully explain my empirical results, because my results also suggest that the fiscal discipline improved even after the monitor leaves the locality.

2.6.2 Mentoring

Mentoring is more consistent with the long-run effects of my main results. Allocated central administrators could also work as mentors by transmitting information and guiding local public officials. Local governments typically do not know the decision process over the budget by the central government, and there is an asymmetric information between the central and local governments. Hence, transferred central administrators could give local officials useful information such as how to submit an appealing application for receiving a discretionary grant from the central government. They could also convey the importance of fiscal discipline and consequently help municipalities reduce overspending.

[27] For example, Avis et al. (2018) shows government audits significantly decrease municipal corruptions.

Furthermore, there is also a significant difference in productivity between transferred central administrators and local public officials. The transferred administrators typically hold a bachelor's or master's degree from the highest-ranking university in Japan,[28] whereas less than half of locally hired officials in cities, town, and villages had a bachelor's degree as of 2010. Hence, transferred central administrators with better management skills work as mentors by developing better human resources and increasing the productivity of local public officials.

Theoretically, mentoring effects would be significant for both current and future periods. Transmitted information and developed human capital would accumulate in the municipality, even after the central administrator leaves the local government.

Empirically, I find that general fiscal conditions significantly improved both in the current and future periods. My empirical results are more consistent with the theoretical prediction of mentoring, both for the current and future periods. Thus, my empirical results strongly support the role of mentors, and mentoring can explain and complement for the significant persistent effect that monitoring effects cannot explain.

2.6.3 Past Experience and Personal Connections

A central administrator's past experience and personal connections as a central bureaucrat could also affect municipalities. A central administrator from a particular department is more likely to have stronger preference over the specific related field. In Japan, the labor market in the central government is not liquid, and central administrators typically work for the same ministry until the retirement. Since transferred central bureaucrats have worked for the same department for long periods prior to the assignment, they are more likely to believe that their departments provide the greatest value to the country. Furthermore, central administrators had strong interest in related policies even before they even enter their respective ministries. In the Japanese recruitment system of central administrators, undergraduate and graduate students can choose the ministry with which they wish to interview, after passing a general written examination. Hence, bureaucrats are more

[28] Central bureaucrats also have the opportunity to study abroad in M.A. or Ph.D. programs in foreign universities.

likely to have strong interest in the specific fields before they enter the ministry, and after working in the same field for a long period of time, they are more likely to have stronger preferences over the related policies, leading to increase spending related to the home department when assigned to a locality.

A central administrator's personal connections as a central bureaucrat would also lead to the heterogeneous effect. Individual connections with the central ministry strengthen the negotiation power of the municipality and increase the likelihood of receiving a categorical grant related to the administrator's field. As mentioned in the institutional background section, the Japanese bureaucratic system is often described as a vertically divided administrative system where each ministry solely focuses on its own field; ministries are not to interfere with other ministries in irrelevant fields. In terms of discretionary grants, the persons in charge in each central ministry judge whether or not to give specific grants to specific municipalities. Under this vertically divided decision process, there is potential for transferred central administrators to influence the process of determining categorical grants using negotiation power arising from strong personal connections with the ministry they were in. Furthermore, these transferred bureaucrats are relatively elderly than the persons in charge in the home ministry, which also helps them influence the decision process of allocating grants. In the Japanese bureaucracy system, workers' promotions and wages are mainly based on seniority rather than productivity, and there is a hierarchical relationship between supervisors and subordinates in the central government. Hence, the personal connections would also contribute to transferred central administrators' influence on the process of determining grants from the central government.

Theoretically, the effect of the past experience and personal connections would be statistically significant in the current period, but not significant in future periods after transferred central administrators leave the localities. After the central administrators leave, the municipalities are less likely to protect the specific expenditure.

Empirically, I find that the effect of increasing spending and discretionary grants related to their respective departments is statistically significant only in the the current period, which is consistent with the theoretical prediction. Thus, my empirical evidence supports the channel of individual preference over the municipalities. Central administrators' specific preference and personal connections work in the current period but disappear after they leave the localities.

2.7 Conclusion

In this paper, I investigate whether or not the allocation of transferred central administrators affects local governments' fiscal conditions. Using a two-way fixed effect model and rich administrative data from Japan, I find that municipalities with transferred central bureaucrats improve fiscal discipline by shrinking expenditure by 8.6% and lowering debt by 8.9%, suggesting transferred administrators mitigated deficit bias. I also find that the probability of the sitting mayor winning in the next local election increases, suggesting that local residents seem to reward the incumbent as a result of better administration and fiscal conditions. The average effects of improving fiscal conditions are overall persistent, continuing in the years after the central bureaucrat leaves. This is consistent with the mechanism that not only monitoring effects reduce local governments' moral hazard incentives in the current period, but also mentoring effects take root in local governments even after they leave.

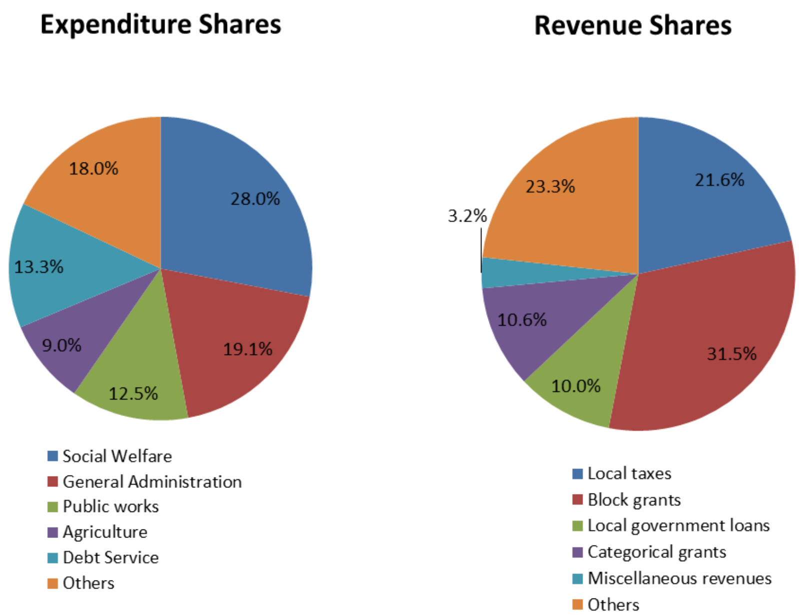
Furthermore, I explore heterogeneity in impacts on specific categories of expenditures and grants in local governments, according to the transferred administrators' home departments. Central administrators bring with them preferences, knowledge, and personal connections that may influence success in obtaining grants as well as which spending areas are protected. I find some evidence that discretionary grants provided by home departments and expenditures in related areas increase during the stay. Hence, the mentorship and oversight by allocated administrators help localities manage deficit bias; but, the newly formed political connections might lead to new misallocations. Interestingly, these effects are transitory and disappears with the bureaucrat. This is consistent with the mechanism that central administrators' personal preference and negotiation power of local governments are strengthened only during the stay.

My empirical results have strong policy implications. Compared to the findings in standard regulatory policies for deficit bias, this paper newly proves that a more flexible fiscal rule without numerical targeting has an impact on municipal governments and improve fiscal discipline. The finding provides prescriptions for deficit bias, robust to enforcement problems and unpredictable events which standard regulatory policies have difficulty addressing. I also find that even short political connections between a central administrator and municipal government lead to a distortion, by increasing grants provided by home departments and expenditures in related areas, during the stay. Thus, in policy implementation, policymakers also need to consider the heterogeneity of central administrators.

2.8 Acknowledgments

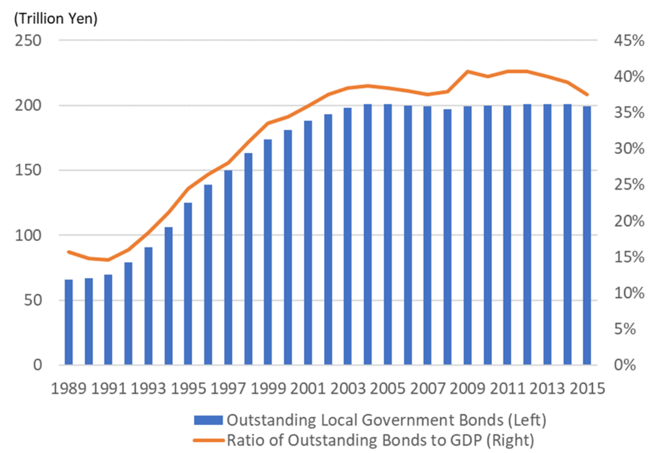
Chapter 2, in full, is currently being prepared for submission for publication of the material. Nobuhiko Nakazawa. "Do mentoring and oversight matter? The effects of allocating central administrators to local government units: Evidence from Japan". The dissertation author was the primary investigator and author of this material.

Figures and Tables



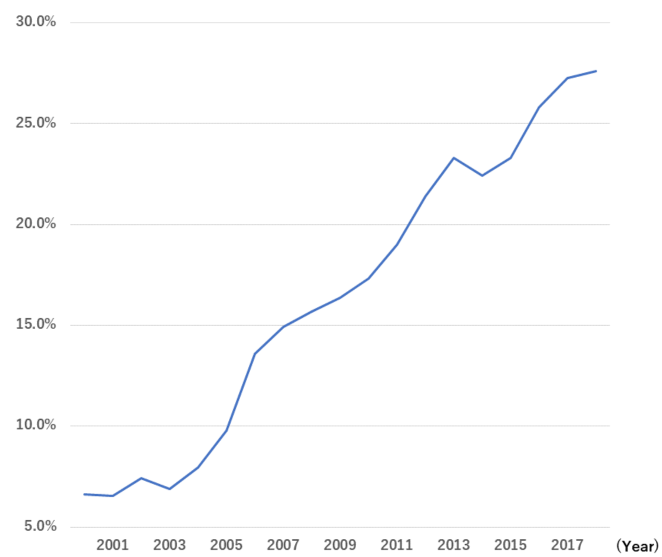
Notes: The figure shows the composition of expenditure and revenue of local governments in my sample from 2000 to 2014.

Figure 2.1: Expenditure and Revenue Shares for the Various Components (2000-2014)



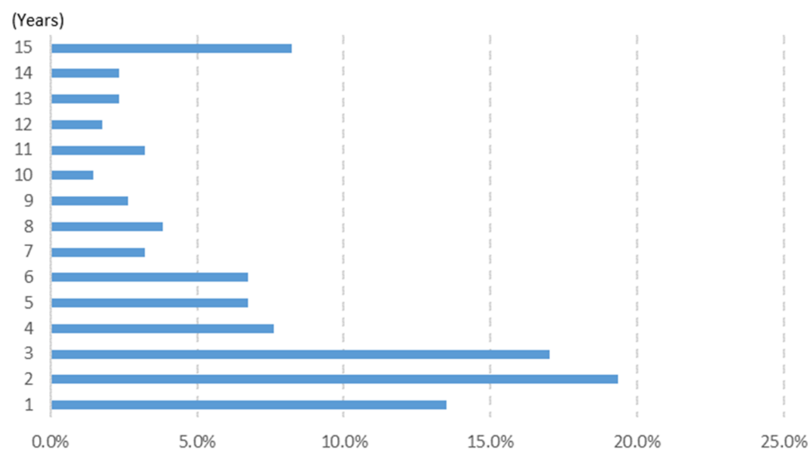
Notes: The figure plots outstanding local government bonds (trillion Yen) and its ratio to GDP (%).

Figure 2.2: Japanese Outstanding Local Government Bonds



Notes: The figure plots the percentage of transferred central bureaucrats over municipalities by year. Central bureaucrats worked as a manager or higher in a municipality.

Figure 2.3: Evolution of Allocating Central Administrators



Notes: The figure plots the distribution of years of exposure to transferred central administrators in hosting municipalities during the years of my sample.

Figure 2.4: Years of Exposure to Transferred Central Administrators

Table 2.1: Placebo Experiment

Panel A: Expenditure Side	Total expenditure	Social welfare	General administration	Agriculture	Public works	Debt service	Other
Pre-arrival Effect	-18.17	-3.67	3.64	-4.98	-2.09	-5.39	-1.84
	[25.94]	[2.39]	[15.06]	[3.51]	[2.86]	[3.20]	[5.43]
Municipality and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R squared	0.16	0.20	0.09	0.14	0.04	0.17	0.10
Dependent var	608.83	170.32	116.39	55.07	76.40	81.27	109.39
Obs.	26,580	26,580	26,580	26,580	26,580	26,580	26,580
Panel B: Revenue Side	Local taxes	Grants	Miscellaneous revenues	Local gov loans	Interest rate of loans		
Pre-arrival Effect	2.73	-5.42	1.26	-2.91	0.02		
	[2.04]	[18.73]	[1.01]	[1.96]	[0.013]		
Municipality and year FE	Yes	Yes	Yes	Yes	Yes		
R squared	0.03	0.21	0.02	0.14	0.82		
Dependent var	131.33	255.81	19.26	60.63	2.19		
Obs.	26,580	26,580	26,580	26,580	26,580		
Panel C: Other Variables	Local fiscal deficits	P (incumbent mayor winning)					
Pre-arrival Effect	-16.13	-0.069					
	[10.01]	[0.048]					
Municipality and year FE	Yes	Yes					
R squared	0.30	0.03					
Dependent var	227.61	0.59					
Obs.	26,580	15,215					

Notes: The table shows the results of the placebo tests for the fiscal outcomes. *Pre-arrival Effect* reports the estimates of the assignment on the outcome variables one year prior to the actual assignment by including a lead indicator, which takes 1 if the timing is one year prior to the actual arrival of a central administrator. Panel A shows the result for the expenditure side. Panel B shows the result for the revenue side. Panel C shows the result for the other outcome variables. All the specifications include municipality- and year- fixed effects and demographic and political controls. Standard errors robust to clustering by prefecture are reported in brackets. Statistical significance is indicated by * at the 10% level, ** at the 5% level, and *** at the 1% level. For other details, see the notes to Table 3.

Table 2.2: Descriptive Statistics: Means (Standard Deviations) of Main Variables

Variables	Mean (Standard Deviation)
Expenditure	
Social Welfare	170.32 (85.96)
General Administration	116.39 (292.92)
Public works	76.40 (134.73)
Agriculture	55.07 (101.04)
Debt	81.27 (82.58)
Revenue	
Local taxes	131.33 (77.19)
Grants	255.81 (375.94)
Local government loans	60.63 (66.40)
Interest rate of loans	2.19(0.66)
Political variables	
Probability of incumbent mayor winning in a local election	0.59(0.49)
Ratio of municipalities having local elections in a year	0.22(0.42)
Demographic variables	
Ratio of people aged 65 or over (total=100)	26.22(7.31)
Ratio of people aged 15 or younger (total=100)	13.29(2.31)
Ratio of males to females (female=100)	94.10(8.86)
Classification of municipalities defined by the Japanese MIC (From 1 to 9)	4.67(1.03)
Obs.	28,820

Notes: The table presents descriptive statistics in my sample. The sample is all Japanese municipalities over the period from 2000 to 2014. The numbers show means and standard deviations (in parentheses). The unit of observation for fiscal variables except for interest rates is 1,000 Yen per capita, and one dollar roughly equals 100 Yen.

Table 2.3: Regression Results for Expenditure

Dependent variable:	Total spending per capita		
	(1)	(2)	(3)
Bureaucrat	-108.09***	-72.36**	-52.36**
	[11.40]	[29.94]	[24.54]
Year fixed effect	No	No	Yes
Municipality fixed effect	No	Yes	Yes
Demographic and political controls	Yes	Yes	Yes
R squared	0.16	0.12	0.16
Dependent-variable mean (1,000 Yen per capita)	608.83	608.83	608.83
Obs.	26,580	26,580	26,580

Notes: The parameters are results from separate regressions of equation (1) for total expenditure expressed as 1,000 Yen per capita. *Bureaucrat* is a dummy that equals one if the local government hosts a transfer in the same year, and the coefficient reports the effect during the years the administrator visits. The specification in the first column includes demographic and political controls but does not include municipality-by-year fixed effects. The specification in the second column includes control variables and municipality fixed effect but does not include year fixed effect. The specification in the third column includes both municipality-by-year fixed effects and control variables. The demographic and political controls are shown in Table 2. The specifications also include lead and lag indicators for hosting a transfer. The sample is all Japanese municipalities over the period 2000 to 2014. Standard errors are reported in brackets, and standard errors in column (2) and (3) are robust to clustering by prefecture. Statistical significance is indicated by * at the 10% level, ** at the 5% level, and *** at the 1% level. One dollar roughly equals 100 Yen. Specification (3) is the main specification, here and what follows.

Table 2.4: Decomposition of Expenditure

Dependent variable:	Social welfare	General Administration	Agriculture	Public works	Debt service	Other expenditure
Bureaucrat	-3.78	-24.32***	-10.06***	7.74	-6.93*	-7.22
	[3.08]	[6.92]	[3.39]	[5.21]	[3.45]	[5.59]
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R squared	0.20	0.09	0.14	0.04	0.17	0.10
Dependent-var mean	170.32	116.39	55.07	76.40	81.27	109.39
Obs.	26,580	26,580	26,580	26,580	26,580	26,580

Notes: Each column shows results from a separate regression of equation (1) for the expenditure-side outcome variables indicated in the column heading. All of the dependent variables are expressed as 1,000 Yen per capita. For other details, see the notes to Table 3.

Table 2.5: Regression Results for the Revenue Side

Dependent variable:	Local taxes	Grants	Miscellaneous revenues	Local gov loans	Interest rate of loans
Bureaucrat	8.83***	-38.97***	5.30***	-2.61	0.01
	[3.25]	[14.36]	[1.65]	[2.29]	[0.014]
Fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
R squared	0.03	0.21	0.02	0.14	0.82
Dependent-variable mean	131.33	255.81	19.26	60.63	2.19
Obs.	26,580	26,580	26,580	26,580	26,580

Notes: Each column shows results from a separate regression of equation (1) for the revenue-side outcome variables indicated in the column heading. All of the dependent variables except for the interest rate are expressed as 1,000 Yen per capita, and the interest rate is defined as percentage points. For other details, see the notes to Table 3.

Table 2.6: Effects on Local Deficits and Probability of Incumbent Mayors Winning

Dependent variable:	(1) Local Fiscal Deficits	(2) Probability of Incumbent Mayors Winning
Bureaucrat	-37.82***	0.06***
	[11.69]	[0.02]
Municipality and year fixed effects	Yes	Yes
Demographic and political controls	Yes	Yes
R squared	0.30	0.03
Dependent variable mean	227.61	0.59
Obs.	26,580	15,215

Notes: The parameter is from a regression equation (1) for the municipal fiscal deficits and the probability of the incumbent mayor winning in the local election in the next three years. Fiscal deficits are the net of financially equalizing transfers. The dependent variable in the first column is expressed as 1,000 Yen per capita. The unit observation of the second column is a municipality-by-electoral year. For other details, see the notes to Table 3.

Table 2.7: Persistent Effects

Panel A: Expenditure Side	Total expenditure	Social welfare	General administration	Agriculture	Public works	Debt service	Other
Short-run Persistent Effect	-87.19***	-7.35**	-29.09**	-13.99***	-7.05*	-11.47***	-15.35***
	[22.13]	[3.10]	[6.29]	[4.44]	[3.80]	[3.46]	[3.90]
Long-run Persistent Effect	-56.68***	-3.65	-21.20***	-7.13*	-5.60*	-6.24*	-10.96***
	[20.95]	[2.90]	[6.20]	[4.13]	[3.32]	[3.71]	[3.64]
R squared	0.16	0.20	0.09	0.14	0.04	0.17	0.10
Dependent var	608.83	170.32	116.39	55.07	76.40	81.27	109.39
Obs.	26,580	26,580	26,580	26,580	26,580	26,580	26,580
Panel B: Revenue Side	Local taxes	Grants	Miscellaneous revenues	Local gov loans	Interest rate of loans		
Short-run Persistent Effect	0.03	-52.91***	1.43	-5.40***	0.00		
	[2.43]	[12.31]	[1.29]	[2.36]	[0.02]		
Long-run Persistent Effect	-2.87	-35.77***	1.08	-1.77***	-0.03*		
	[2.56]	[10.81]	[1.54]	[2.28]	[0.01]		
R squared	0.03	0.21	0.02	0.14	0.82		
Dependent var	131.33	255.81	19.26	60.63	2.19		
Obs.	26,580	26,580	26,580	26,580	26,580		
Panel C: Other Variables	Local fiscal deficits	P(Incumbent mayor winning)					
Short-run Persistent Effect	-40.72***	-0.002					
	[12.28]	[0.053]					
Long-run Persistent Effect	-30.85**	-0.040					
	[13.58]	[0.027]					
R squared	0.30	0.03					
Dependent var	227.61	0.59					
Obs.	26,580	15,215					

Notes: The table reports the persistent effects for lag indicators for overall bureaucrats and specific bureaucrats. *Short-run Persistent Effect* reports the persistent effect one year after a bureaucrat left the municipality. *Long-run Persistent Effect* reports the persistent effect more than one year after a bureaucrat left the municipality. All the specifications include municipality- and year- fixed effects and demographic and political controls. For other details, see the notes to Table 3.

Table 2.8: Heterogeneous Effects on Expenditure by Home Department of the Administrators

Dependent variable:	Social welfare	General administration	Agriculture	Public works	Debt service	Education
Bureaucrat	-4.42	-22.50**	-9.73***	5.90	-6.88*	0.09
	[3.06]	[8.94]	[3.60]	[9.60]	[3.48]	[1.39]
Related	13.90*	-4.58	-3.63	3.09	1.84	10.77***
	[7.43]	[8.85]	[6.51]	[8.54]	[11.52]	[3.86]
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Share of bureaucrats	3.7%	28.7%	5.1%	42.1%	0.6%	3.8%
R squared	0.20	0.09	0.14	0.04	0.17	0.10
Dependent-var mean	170.32	116.39	55.07	76.40	81.27	64.02
Obs.	26,420	26,468	26,432	26,511	26,415	26,421

Notes: The parameters are results from separate regressions of equation (3) for the expenditure-side outcome variables indicated in the column heading. *Related* is a dummy variable that equals one if municipality hosts a bureaucrat from a ministry related to the outcome variable in the year. Specifically, *Related* bureaucrats are from the Ministry of Health, Labour and Welfare when the dependent variable is expenditure on social welfare; bureaucrats from the Ministry of Internal Affairs and Communications when the dependent variable is expenditure on general administration; bureaucrats from the Ministry of Agriculture and Forestry when the dependent variable is expenditure on agriculture; bureaucrats from the Ministry of Land, Infrastructure, Transport and Tourism when the dependent variable is expenditure on public works; bureaucrats from the Ministry of Finance when the dependent variable is expenditure on debt service; and bureaucrats from the Ministry of Education, Culture, Sports, Science and Technology when the dependent variable is expenditure on education. Share of bureaucrats shows the ratio of related transferred bureaucrats to total transferred bureaucrats. For other details, see the notes to Table 3.

Table 2.9: Heterogeneous Effects on Categorical Grants by Home Department of the Administrators

Dependent var:	Grants for ordinary construction	Grants for social structure	Grants for welfare	Grants for electricity	Grants for medical expenses and children	Grants for East Japan Earthquake
Bureaucrat	-3.95***	-0.87*	3.65***	-0.42	0.12	18.50
	[1.21]	[0.48]	[0.99]	[0.54]	[0.11]	[15.91]
Related	3.48***	2.64***	2.00	3.49	0.88**	
	[1.26]	[0.61]	[2.48]	[2.84]	[0.35]	
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Share of bureaucrats	42.1%	42.1%	3.7%	13.1%	3.7%	
R squared	0.02	0.05	0.30	0.00	0.62	0.01
Dependent-var mean	16.42	6.02	4.07	1.67	15.39	13.35
Obs.	26,511	15,328	12,819	26,435	11,961	7,097

Notes: The parameters are results from separate regressions of equation (3) for the categorical grants indicated in the column heading. *Related* is a dummy variable that equals one if municipality hosts a bureaucrat from a ministry related to the outcome variable in the year. Specifically, *Related* shows bureaucrats from the Ministry of Land, Infrastructure, Transport and Tourism when the dependent variables are grants for ordinary construction and social structure; bureaucrats from the Ministry of Health, Labour and Welfare when the dependent variable are grants for welfare, medical expenses, and children; and bureaucrats from the Ministry of Economy, Trade, and International and from the Ministry of Education, Culture, Sports, Science and Technology when the dependent variables are grants for electricity. When the dependent variables are grants for the East Japan Earthquake, coefficients for *Related* are kept blank in the table since bureaucrats were not transferred from the Reconstruction Agency. For other details, see the notes to Table 8.

Table 2.10: Persistent Effects for Heterogeneous Analysis

Panel A: Expenditure Side	Social welfare	General administration	Agriculture	Public works	Debt service	Education
Short-run Persistent Effect	-7.75**	-25.92**	-13.92***	-5.95	-11.27***	-3.80**
of Bureaucrat	[3.03]	[6.55]	[4.72]	[4.42]	[3.50]	[1.51]
Short-run Persistent Effect	8.83	-11.76	3.19	-3.09	-5.20	11.73
of Related Bureaucrat	[8.84]	[7.98]	[6.70]	[4.33]	[6.70]	[8.67]
Long-run Persistent Effect	-5.16*	-19.71***	-8.14*	-4.43	-6.43*	-3.06*
of Bureaucrat	[2.72]	[5.95]	[4.33]	[4.40]	[3.76]	[1.22]
Long-run Persistent Effect	13.00**	-8.21	11.80**	-1.01	-12.28*	6.92
of Related Bureaucrat	[4.91]	[7.37]	[4.83]	[3.13]	[6.47]	[4.91]
R squared	0.20	0.09	0.14	0.04	0.17	0.10
Dependent var	170.32	116.39	55.07	76.40	81.27	64.02
Obs.	26,420	26,468	26,432	26,511	26,415	26,421
Panel B: Categorical Grants	Grants for construction	Grants for social structure	Grants for welfare	Grants for electricity	Grants for medical and children	Grants for Earthquake
Short-run Persistent Effect	-4.08*	-0.19	3.07***	-0.71	0.18	-16.19
of Bureaucrat	[2.05]	[0.52]	[0.80]	[0.52]	[0.26]	[12.41]
Short-run Persistent Effect	-0.45	-0.12	-0.63	3.82	0.42	
of Related Bureaucrat	[2.41]	[0.65]	[1.93]	[4.09]	[0.51]	
Long-run Persistent Effect	-2.69**	-0.88**	3.39***	-0.60	-0.05	-1.33
of Bureaucrat	[1.29]	[0.51]	[1.25]	[0.47]	[0.13]	[9.51]
Long-run Persistent Effect	0.25	0.09	0.64	0.07	-0.14	
of Related Bureaucrat	[1.11]	[0.35]	[1.50]	[0.35]	[0.19]	
R squared	0.02	0.05	0.30	0.00	0.62	0.01
Dependent var	16.42	6.02	4.07	1.67	15.39	13.35
Obs.	26,511	15,328	12,819	26,435	11,961	7,097

Notes: Short-run Persistent Effect of Related Bureaucrat reports the persistent effect one year after a bureaucrat from a ministry related to the outcome variable left. *Long-run Persistent Effect of Related Bureaucrat* reports the persistent effect more than one year after a bureaucrat from a ministry related to the outcome variable left. All the specifications include municipality- and year- fixed effects and demographic and political controls. For other details, see the notes to Table 9.

Table 2.11: Mechanisms

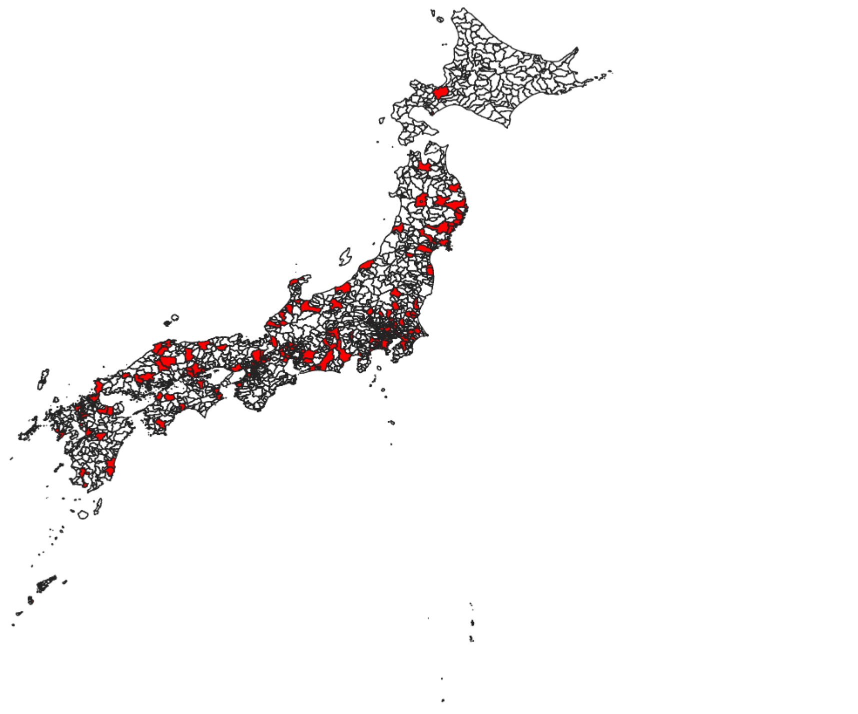
	Monitoring	Mentoring	Past Experience and Personal Connections
Affected Outcomes	General fiscal conditions	General fiscal conditions	Specific expenditure and revenue
Effects	Decrease moral hazard	Transmit information	Strengthen preference over specific public goods
		Develop human capital	Increase negotiation power
Theoretical Predictions			
Current effect	Significant	Significant	Significant
Persistent effect	Not significant	Significant	Not significant
Empirical Results			
Current effect	Significant	Significant	Significant
Persistent effect	Significant	Significant	Not significant

Notes: The table summarizes the theoretical predictions and my empirical results about three effects underling the allocation of central administrators: monitoring, mentoring, and past experience and personal connections in the central government. Monitoring improves fiscal discipline by decreasing municipalities' moral hazard incentives. Mentoring transmits useful information and develops human capital of local public officials. A central administrator's past experience and personal connections strengthen the preference over specific expenditure and negotiation power to receive a categorical grant in the area related to the administrator's home department.

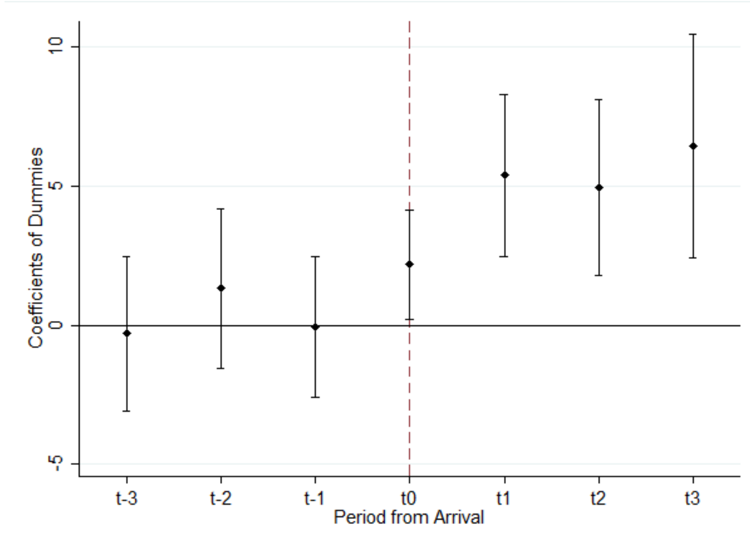
Theoretically, monitoring effects are statistically significant in the current period, but not statistically significant after the monitor returns to the central government. Mentoring effects are statistically significant for both current and future periods, because transmitted information and developed human capital will accumulate in the municipality. Effects for central administrators' specific preference and personal connections are statistically significant only in the current period and not in the future periods after they leave the localities.

Empirically, I find that the current effects are statistically significant for both general fiscal conditions and specific fiscal variables, and the persistent effects are statistically significant for overall fiscal conditions but not for specific discretionary grants and expenditure. Thus, the empirical findings are more consistent with the theoretical prediction of the mentoring and past experience and personal connections, though monitoring could also work in the current period.

2.9 Appendix

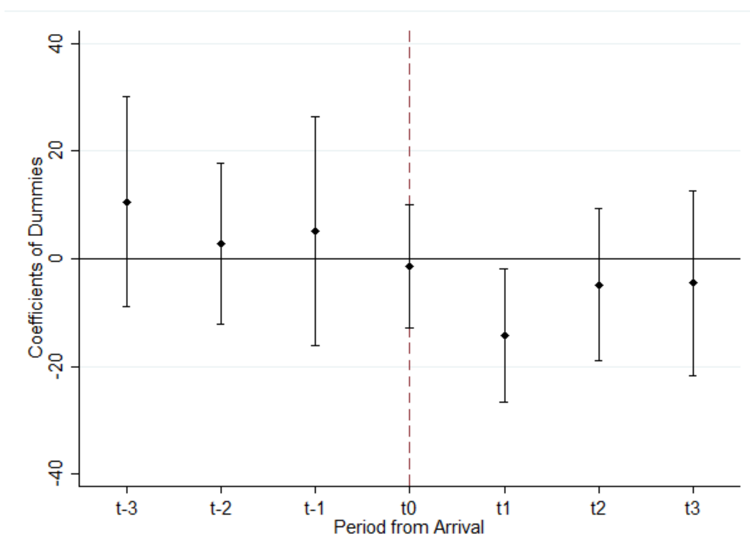


Notes: The figure shows the Japanese municipalities that hosted a transfer in my sample.
Figure 2.A1: Municipalities Hosting a Transfer



Notes: The figure shows the estimated result from the event-study specification (4) for the local tax revenue. Plotted on the x axis is the year relative to the arrival of a central administrator; the point estimates and their 90% confidence intervals for the coefficients are plotted on the y axis. The omitted period corresponds to t-4.

Figure 2.A2: Robustness: Event Study Approach



Notes: The figure shows the estimated result from the event-study specification (4) for the outstanding of local government bonds. Plotted on the x axis is the year relative to the arrival of a central administrator; the point estimates and their 90% confidence intervals for the coefficients are plotted on the y axis. The omitted period corresponds to t-4.

Figure 2.A3: Robustness: Event Study Approach

Table 2.B1: Correlates of the Municipalities Hosting Central Administrators

Variables	Probability of Central Administrators Arriving in Year t
Total expenditure in year t-1	0.004
	[0.005]
Total revenue in year t-1	-0.006
	[0.005]
Outstanding of local government loans in year t-1	-0.001
	[0.006]
Borrowing cost in year t-1	1.73
	[1.37]
Ratio of municipalities having local elections in year t-1	0.005**
	[0.002]
Ratio of people aged 65 or over in year t-1	-0.013***
	[0.003]
Ratio of people aged 15 or younger in year t-1	-0.010***
	[0.001]
Ratio of males to females in year t-1	0.0003
	[0.0002]
Classification of municipalities in year t-1	0.014
	[0.012]
Constant	-0.067
	[0.074]
Municipality fixed effect	Yes
R squared.	0.16
F statistic	10.70
Prob > F	0.00***
Obs.	24,905

Notes: The table reports the municipal-fixed-effect estimates for the arrivals of central administrators. The reported parameters except for the borrowing cost are expressed as 100,000 Yen per capita, and the borrowing cost is defined as percentage points. Standard errors, robust to clustering by prefecture, are reported in brackets. Statistical significance is indicated by * at the 10% level, ** at the 5% level, and *** at the 1% level. For other details, see the notes to Table 3.

Table 2.B2: Specification Test for the Timing of Leaving

Independent variables:	Expenditure	Local taxes	Grants	Miscellaneous revenues	Local gov loans .
Lag (-1)	0.021	-0.059	-0.033	-0.140	-0.058
	[0.032]	[0.071]	[0.039]	[0.118]	[0.071]
Lag (-2)	0.024	-0.116	-0.016	0.090	0.070
	[0.014]	[0.114]	[0.038]	[0.133]	[0.076]
Lag (-3)	-0.026	0.044	0.012	0.007	0.019
	[0.020]	[0.093]	[0.025]	[0.008]	[0.059]
Municipality and year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Joint F-statistic	5.16	5.16	5.16	5.16	5.16
Prob>F	0.00	0.00	0.00	0.00	0.00
R squared	0.01	0.01	0.01	0.01	0.01
Dependent-var mean	0.10	0.10	0.10	0.10	0.10
Obs.	1,579	1,579	1,579	1,579	1,579

Notes: The parameters are results from a two-way fixed effect equation (2) for the likelihood of a bureaucrat leaving in a given year, given that the bureaucrat was there in the prior year. Independent variables are described in the column header. Lag (-1) shows the fiscal variables in the column header one year before the exit. Lag (-2) shows the fiscal variables in the column header two years before the exit. Lag (-3) shows the fiscal variables in the column header three years before the exit. The specification includes lagged fiscal variables in years other than just the prior year to see if the actual withdrawal decision could be based on several years of outcomes. The specification also includes municipality-by-year fixed effects and demographic and political controls. The dependent variable is expressed as percentage points. Standard errors, robust to clustering by prefecture, are reported in brackets. Statistical significance is indicated by * at the 10% level, ** at the 5% level, and *** at the 1% level. For other details, see the notes to Table 3.

Table 2.B3: Related Expenditures and Grants

Items	Ministries
Expenditures	
Social Welfare	Ministry of Health, Labor and Welfare
General Administration	Ministry of Internal Affairs and Communication
Agriculture	Ministry of Agriculture and Forestry
Public Works	Ministry of Land, Infrastructure, Transport and Tourism
Debt Service	Ministry of Finance
Education	Ministry of Education, Culture, Sports, Science and Technology
Grants	
Ordinary Construction	Ministry of Land, Infrastructure, Transport and Tourism
Social Structure	Ministry of Land, Infrastructure, Transport and Tourism
Medical Expenses and Children	Ministry of Health, Labor and Welfare
Other Social Welfare	Ministry of Health, Labor and Welfare
Electricity	Ministry of Economy, Trade, and International
	Ministry of Education, Culture, Sports, Science
East Japan Earthquake	Reconstruction Agency

Notes: The table shows expenditure and revenue categories and the departments in the central government that administer them.

Table 2.B4: Placebo Regressions for Heterogeneous Analysis

Panel A: Expenditure Side	Social welfare	General administration	Agriculture	Public works	Debt service	Education
Pre-arrival Effect of	-3.63	-11.88	-5.06	-3.88	-5.17	-0.87
Bureaucrats	[2.40]	[21.75]	[3.64]	[3.70]	[3.20]	[1.49]
Pre-arrival Effect of	-0.20	-27.99	3.76	4.62	-4.62	1.15
Related Bureaucrats	[10.25]	[24.61]	[12.39]	[3.58]	[15.94]	[2.63]
Municipality and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R squared	0.20	0.09	0.14	0.04	0.17	0.10
Dependent var	170.32	116.39	55.07	76.40	81.27	64.02
Obs.	26,420	26,468	26,432	26,511	26,415	26,421
Panel B: Categorical Grants	Grants for construction	Grants for social structure	Grants for welfare	Grants for electricity	Grants for medical and children	Grants for Earthquake
Pre-arrival Effect of	-2.19	-0.34	1.33	-0.49	0.17	49.94
Bureaucrats	[1.44]	[0.73]	[0.81]	[0.45]	[0.25]	[33.47]
Pre-arrival Effect of	0.49	1.25	1.74	3.15	-0.14	
Related Bureaucrats	[1.45]	[0.92]	[1.86]	[2.98]	[0.39]	
Municipality and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R squared	0.02	0.05	0.30	0.00	0.62	0.01
Dependent var	16.42	6.02	4.07	1.67	15.39	13.35
Obs.	26,511	15,328	12,819	26,435	11,961	7,097

Notes: The table reports the placebo tests for lead indicators for overall bureaucrats and related bureaucrats. *Pre-arrival Effect of Bureaucrats* reports the allocation effect of a central administrator on a municipality one year before he or she arrives. The lead indicator equals one if the timing is one year before a bureaucrat arrives at the municipality. *Pre-arrival Effect of Related Bureaucrats* reports the allocation effect of a related central administrator on a municipality one year before she arrives. The lead indicator for related bureaucrats equals one if the timing is one year before he or she arrives at a municipality. For other details, see the notes to Table 8 and Table 9.

Table 2.B5: Effects on Grants

Dependent variable:	Categorical Grants	Block Grants
Bureaucrat	16.85*	-55.68***
	[10.00]	[9.56]
Municipality and year fixed effects	Yes	Yes
Demographic and political controls	Yes	Yes
R squared	0.03	0.29
Dependent-variable mean	64.24	191.57
Obs.	27,180	27,180

Notes: The parameters are results from a separate regression of equation (1) for the different outcome variables. All of the dependent variables are expresses as 1,000 Yen per capita. For other details, see the notes to Table 3.

Table 2.B6: Heterogeneous Effects by Initial Fiscal Condition

Dependent var:	Expenditure	Local taxes	Grants	Miscellaneous revenues	Local gov loans
Panel A: Good fiscal condition (Low borrowing ratio) in initial year					
Bureaucrat	-43.49**	3.25	-29.71***	5.59***	-0.71
	[19.31]	[3.91]	[10.72]	[1.87]	[2.39]
Fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
R squared	0.14	0.02	0.20	0.03	0.12
Dependent var	593.50	137.80	227.18	17.97	55.78
Obs.	13,281	13,281	13,281	13,281	13,281
Panel B: Bad fiscal condition (High borrowing ratio) in initial year					
Bureaucrat	-61.24	15.16***	-48.57**	5.40**	-5.49*
	[40.23]	[4.20]	[23.87]	[2.51]	[2.97]
Fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
R squared	0.17	0.04	0.21	0.02	0.16
Dependent var	624.94	124.53	263.80	20.62	65.73
Obs.	13,299	13,299	13,299	13,299	13,299

Notes: The parameters are results from a separate regression of equation (1) for the different outcome variables. Panel A shows the result for municipalities with a lower borrowing ratio in the initial year, and panel B shows the result for municipalities with a higher borrowing ratio in the initial year of my sample. For other details, see the notes to Table 3.

Table 2.B7: Heterogeneous Effects by Position of Transferred Central Administrators in Municipalities

Dependent var:	Expenditure	Grants	Local taxes	Miscellaneous revenues	Local gov loans
Vice mayor	-17.40	-6.00	8.70***	3.12	-3.51
	[38.40]	[23.74]	[2.45]	[1.93]	[2.37]
General manager	-39.94*	-26.58***	8.60**	6.34***	1.24
	[20.11]	[12.56]	[4.19]	[1.72]	[2.64]
Fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
R squared	0.16	0.21	0.03	0.02	0.14
Dependent-var mean	608.83	131.33	245.04	19.26	60.63
Obs.	26,433	26,433	26,433	26,433	26,433

Notes: The parameters are results from a separate regression of equation (1) for the different outcome variables. *Vice mayor* is a bureaucrat dummy that is equal to one if a bureaucrat works as a vice mayor in local government i in year t; *General manager* is a bureaucrat dummy that is equal to one if a bureaucrat works as a general manager in local government i in year t. Lead and lag terms for these variables are also included. For other details, see the notes to Table 3.

Table 2.B8: Robustness: Samples Excluding the Fukushima Region

Dependent var:	Expenditure	Local taxes	Grants	Miscellaneous revenues	Local gov loans
Bureaucrat	-45.22**	9.99***	-36.19***	5.48***	-1.65
	[20.36]	[3.08]	[12.72]	[1.54]	[1.99]
Fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
R squared	0.14	0.03	0.19	0.01	0.14
Dependent var	602.14	130.56	243.44	19.35	60.58
Obs.	26,300	26,300	26,300	26,300	26,300

Notes: The parameters are results from a separate regression of equation (1) for the different outcome variables, where the sample is limited to local governments outside the Fukushima region. For other details, see the notes to Table 3.

Table 2.B9: Robustness: Sample Period Before 2008

Dependent var:	Expenditure	Local taxes	Grants	Miscellaneous revenues	Local gov loans
Bureaucrat	-50.65**	13.69***	-39.75***	5.93***	-5.35*
	[23.10]	[3.55]	[10.84]	[1.86]	[2.99]
Fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
R squared	0.13	0.03	0.20	0.01	0.14
Dependent var	571.96	128.20	220.17	19.48	59.88
Obs.	16,103	16,103	16,103	16,103	16,103

Notes: The parameters are results from a separate regression of equation (1) for the different outcome variables, where the sample is limited to periods before 2008. For other details, see the notes to Table 3.

Chapter 3

The Effects of Changing Mandatory Retirement Systems on Labor Supply: Individual- and Macro- Level Responses

Abstract

This paper investigates the effect of raising the mandatory retirement age and introducing a continued reemployment system on older workers and young job-seekers. In 2006, Japanese companies were required to raise the mandatory retirement age from age 60 to at least age 63 or to introduce a continued employment system that creates flexible positions for older workers to continue at the same company. Relying on quasi-experimental variation in exposure to the policy change according to pre-reform norms by industry, geography, and firm size, I find that the reform was effective in terms of decreasing the job separation rate of older workers. It also decreased the job finding rate of young people for firms that were more affected by the policy change, suggesting that older workers crowd out opportunities for young job seekers in the labor market.

3.1 Introduction

How does changing mandatory retirement systems of firms affect labor markets? Under rapidly aging populations, many countries are currently urged to reform social security systems, and researchers have investigated effects of reforms on individual retirement. However, relatively fewer papers investigate reforms to firms' policies designed to address population aging. In this paper, I investigate the effect of changing mandatory retirement systems on labor markets, focusing on a recent Japanese reform. In 2006, Japanese firms were required to raise the mandatory retirement age from age 60 to at least age 63 or to introduce a continued employment system that creates flexible positions for older workers to continue to work at the same company.

The reform to mandatory retirement affects labor markets in two ways. First, the policy change directly affects labor supply for older workers (i.e., partial equilibrium effect). Since the policy increases firms' labor demand for older workers, this shift of the labor demand curve should increase the quantity of labor supplied by older workers. Second, the policy change could also indirectly affect young jobseekers (i.e., general equilibrium effect). If more older workers continue to work in the same firm after the reforms, then the number of young hires could decrease if younger workers and older workers are substitutes. The presence of intergenerational spillovers would be important for the optimal design of public policies.

Since this mandatory retirement policy change affects firms' labor demand for older workers, one simple and direct identification strategy should be to use firm-level panel data and exploit variation in the number of employees by age. However, there are no available firm-level panel data in Japan that include the full age distribution of employees. To overcome this challenge, I construct a novel rich dataset from government restricted-use data sources spanning 10 years and including worker and firm surveys.

To estimate the causal effect of raising the mandatory age on older workers, I employ a regression discontinuity design (RDD). There is a sharp discontinuity in firms' policies about mandatory retirement ages and continued re-employment systems around the policy change date. Furthermore, there is variation in exposure to the policy change by industry, area, and firm size.

Using this variation, I compare job separation rates of older workers who worked for the more affected firms and older workers who worked for the less affected firms. As for the effect on the young, I employ event-study approaches to compare job offers across firms that were more or less affected by the policy change.

My results show that raising the mandatory retirement age and expanding continued re-employment significantly decreases the job separation rate of older workers at the critical ages by about 6 percentage points. Furthermore, the reform also decreases the job finding rate for young people for treated firms that were more affected by the policy change, suggesting the reform crowded out opportunities for young job seekers. The degree of crowd out is larger for young persons with high skills or in their early 20s.

There are two novel contributions of my paper. First, only a limited number of papers investigate the causal effects of changing mandatory retirement ages on the labor market, in part because mandatory retirement is generally unlawful in many countries including the U.S.[1] Some existing papers find empirical evidence that elimination or mitigation of compulsory retirement decreases retirement rates (e.g., Ashenfelter and Card (2002), Von Wachter (2002), and Kondo and Shigeoka (2017)), but other empirical research suggests that mandatory retirement is not an important determinant of retirement age (e.g., Neumark and Stock (1999) and Shannon and Grierson (2004)).[2] My results corroborate the findings in the former set of studies, as the Japanese reform has significantly decreased the job separation rate of older workers.

Second, empirical evidence of this type of intergenerational substitution between older workers and younger jobseekers is rare. Gruber and Wise (2010) conclude that there is no strong link between increasing the retirement age and higher unemployment in most countries.[3] Rather,

[1]However, this does not necessarily mean the absence of external validity. For example, the U.K. introduced a mandatory retirement age of 65 in 2006, though that was abolished in 2009. In the U.S., about 40 percent of male employees were covered by mandatory retirement rules at age 65 in the 1970s, though the rules were abolished later. In Canada, though two provinces have banned mandatory retirement, the other provinces generally accept mandatory retirement. Australia generally does not have a mandatory retirement age, but the Australian Defence Force strictly enforces a mandatory retirement age. Thus, many countries have, or had, mandatory retirement systems at least for certain industries, occupations, or geographic areas.

[2]For a theoretical framework of mandatory retirement, see Lazear (1979).

[3]They analyze the following countries: Belgium, Canada, Denmark, France, Germany, Italy, Japan, Netherlands, Spain, Sweden, the United Kingdom, and the United States.

they find that greater labor force participation by older persons is associated with greater youth employment, suggesting that older workers and younger workers are not substitutes, but instead, complements.[4] In contrast, several recent papers provide evidence of substitution in labor markets, as my analysis does. Mohnen (2019) shows that U.S. commuting zones with fewer retirements have worse outcomes for younger workers in terms of occupations and wages. Boeri et al. (2017) provide evidence of substitution by using data from Italian provinces and regions. Bovini and Paradisi (2019) show that older workers and younger co-workers are substitutes, by using matched employer-employee records for firms in Italy and firm-level measures of the shock to retirement of older workers. As for Japanese studies, Oshio et al. (2010) do not find strong evidence of trade-off between older and young workers in the labor force, by using labor supply side data and social security wealth as a measure of retirement of older workers. My findings about the intergenerational substitution between young workers and older workers in Japan could reconcile the existing results; substitution does not hold generically across countries or at a country level without variation across firms or space, but holds within countries across firms and space. Heterogeneity within young workers could be also important.

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. Section 6 explores validity and robustness. Section 7 summarizes the main points and concludes.

3.2 Institutional Background

Japan has taken the global lead in population aging. The proportion of elderly people has been highest among the world, which threatened the sustainability of the social security system.[5] In

[4] Several papers also investigate externalities or general equilibrium effects in the context of unemployment insurance benefits (e.g., Hagedorn et al. (2013), Lalive et al. (2015), Johnston and Mas (2018), Chodorow-Reich and Karabarbounis (2016), and Marinescu (2017)).

[5] According to United Nations (2019), Japan's old-age dependency ratio (65+/20-64) is 51% in 2019 and expected to increase to 81% in 2050.

response, the Japanese government phased in increases in the eligibility age for public pensions from age 60 to 65 starting in 2001 to reduce the fiscal imbalance.

A problem that arose was that the mandatory retirement age was 60 in most firms in the early 2000s.[6] Because of the gap between the mandatory retirement age and pension eligibility age, older workers who would like to continue working incur additional job search costs. Hence, to provide stable employment for people in their early 60s who would no longer be eligible for public pension benefits, the government passed the Employment Measures Law in 2004. This law was implemented in April 1, 2006 and required companies to ensure employment up to the pensionable age; thus, it obligates companies to raise the mandatory retirement age, introduce a continued employment system from age 60 to at least age 63, or abolish mandatory retirement completely.

Table 1 details the mandatory retirement policy change. Firms that had set the mandatory retirement age at 63 or above were legal before and after the policy change date (case 1 and 2). Similarly, if firms had set the mandatory retirement age at 60 but also had a continued reemployment system in which workers could continue to work after age 60 until at least age 63, then those firms were also legal before and after the policy change (case 3). However, if firms set the mandatory retirement age at 62 or below and did not have a reemployment system, then those firms were out of compliance after the policy implementation date and would have to introduce a continued reemployment system or raise the mandatory retirement age to at least 63 (case 4). Thus, firms with a mandatory retirement age below 63 and without a re-employment system up to at least age 63 are the treated companies, that were directly affected by the policy change.

Table 2 also suggests that most firms responded to the government intervention by having the same mandatory retirement age (60) and introducing a continued employment system from age 60 to at least age 63, rather than raising the mandatory retirement age itself. This is probably because firms can save on labor cost by having a continued reemployment system up until the new age rather than raising the mandatory retirement age. If employees continue to work for the same firms which raised mandatory retirement ages to at least 63, then their wages are more likely to be same as before.

[6]According to the General Survey on Working Conditions and Survey on Employment Management, more than 90% of firms with mandatory retirement systems set the mandatory retirement age at 60.

In contrast, in a continued reemployment system, affected employees typically earn a much lower wage rate while continuing to work for the same firm. For example, according to a 2014 survey by the Japan Institute for Labor Policy and Training, more than half of older workers experienced 21-50% wage decreases in continued reemployment after a mandatory retirement age. There was no clear restriction on wage reductions for firms in a continued reemployment.[7] However, if a wage reduction rate was more than 25%, a worker can claim benefits called ”continuous employment benefits for the elderly”, the amount of which corresponds to less than or equal to 15% of the prior wage.

Figure 1 shows the fraction of firms in compliance with the new regulations by year. The share of firms that did not have a continued re-employment system nor have a retirement age above 60 sharply dropped after the policy change. Consequently, the employment rate for ages 60-64 increased after the policy change in 2006, whereas the employment rate for ages 20-24 decreased, as shown in Figure 2. The gap in employment rates between the two age groups shrunk after the policy change, suggesting there could have been both micro- and macro- responses in the labor market.

3.3 Identification Strategy

Since this mandatory retirement policy change affects firms' labor demand for older workers, one simple and direct identification strategy would be to use firm-level panel data and exploit variation in the number of employees by age. However, there are no available firm-level panel data in Japan that include the full age distribution of employees. Furthermore, the names of Japanese firms are typically not available in firm-level data due to privacy reasons.

To overcome this challenge, I utilize two kinds of government data sources: firm-level data and individual-level data. Using firm-level data, I first observe firms' pre-reform mandatory

[7] For example, the Japanese supreme court passed judgment that about 20% decrease in wage after a mandatory retirement age was not illegal on June 1st, 2018. In contrast, 75% decrease in wage after a mandatory retirement age was illegal, according to the judgment by the Fukuoka High Court on September 7th, 2017.

retirement age and continued reemployment policies. I create a treatment indicator that takes the value 1 if a firm did not have a continued employment system nor a mandatory retirement age above 60, and 0 otherwise to create a measure of treatment intensity. I then average this treatment indicator across area, industry, and firm size. Figure 3 shows treatment intensity by geographic area, industry, and firm size as of one year before the policy implementation. As one can see, there is variation in firms' mandatory retirement policies across all three dimensions. For example, smaller firms were more likely to be affected by the policy change because they had lower mandatory retirement ages or did not have continued re-employment system.

Next I combine data sources to create an individual-level panel data including information on workers' firms. Then I run the following RDD where the sample is restricted to individuals who worked for companies that were more likely to raise the mandatory retirement age or introduce continued re-employment after April 1st, 2006 ($I_i > Mean(I_i)$);

$$P(Lose\, a\, job|Age = 60, had\, a\, job\, one\, year\, ago)_i = \alpha + \beta D_i + f(MOB_i) + \gamma X_i + \varepsilon_i \tag{3.1}$$

The dependent variable is the probability of job separation at the age of 60 given having a job one year ago. D_i equals 1 if the individual is born in or after April, 1946 and 0 otherwise, since affected cohorts are individuals born after the date.[8] $f(MOB_i)$ are flexible polynomials at the left and right sides of the cutoff. X_i are individual and family characteristics. I_i is the treatment intensity variable that shows exposure to the policy change according to pre-reform norms for firms with the same observable characteristics as a worker's firm one year ago. I'll explain the data later, but the dataset is basically a two-periods panel data (this year and one year ago (later)), and in this specification, $I_i > Mean(I_i)$ means an individual i worked for a firm with a higher treatment intensity in terms of three observable firms' characteristics one year ago. The base year for the calculation of the treatment intensity is one year prior to the policy implementation. My interest is in the coefficient β, which shows the causal effect of changing the mandatory retirement system on older

[8]When individuals born at or after April 1946 reached age 60, the policy was just implemented. In contrast, when individuals born before April 1946 reached age 60, the policy was not implemented.

persons' labor supply. The intuition for this specification is that affected individuals who reached age 60 after the policy implementation date would be less likely to lose a job at age 60, if they worked for companies more affected by the policy change ($I_i > Mean(I_i)$) before the government intervention. As for the implementation of RDD, I use a local linear functional form, a triangular kernel, and the optimal bandwidth by minimizing the mean squared error. I also use other functional forms, bandwidths, and kernels, for robustness. I report heteroskedasticity-robust standard errors following Kolesár and Rothe (2018).

To estimate impacts on young persons (i.e., intergenerational substitution effects), I implement the following event-study model for young persons, by estimating impacts immediately after the policy implementation;

$$P(Getting\, a\, job\, in\, a\, firm\, with\, higher\, intensity | Young, no\, job\, one\, year\, ago)_{i,t} \\ = \alpha + \sum_{k \neq 2005} \beta_k \times T(k) + \gamma X_{i,t} + \varepsilon_{i,t} \tag{3.2}$$

where the dependent variable is the job finding rate in a firm with a higher treatment intensity in terms of three observable characteristics of firms for young persons at ages below 25 conditional on no job one year ago; $X_{i,t}$ is a vector of individual and family characteristics. $T(k)$ are year dummies, and year 2005 is left out for a reference. My interest is in the coefficient β for year 2006, the timing of the policy implementation. The intuition of this specification is that I would expect younger workers to be less likely to work in the types of firms that were highly treated, according to the intensity.

The underlying assumption of RDD is that there is no manipulation or differential attrition around the cutoff. I check this condition by implementing a validity test based on McCrary (2008) and balance checks for pre-determined covariates. The underlying assumption of the event-study approach is that there was no pre-trend prior to the government intervention. I also implement several placebo tests to explore the further internal validity. All the validity tests and placebo tests suggest that the research design is internally valid.

3.4 Data

I create individual-level panel data spanning years from 2002 to 2011 that also includes affluent information on firms' characteristics. Information on firms' mandatory retirement statuses are taken from the *General Survey on Working Conditions* and *Survey on Employment Management* by the Japanese Ministry of Health, Labor, and Welfare. Both are restricted-use data, and *Survey on Employment Management* has not been utilized by anyone. These surveys are conducted to grasp the working conditions of private companies in Japan. Every year, about more than 6,000 nationwide establishments from all private establishments with 30 or more regular employees are selected by random sampling.

Data on individual labor supply are taken from the *Labor Force Survey* by the Japanese Ministry of Internal Affairs and Communications. This survey covers households residing in Japan and investigates the current employment statuses every month.[9] About 40,000 households in about 2,900 enumeration districts are surveyed every time. The survey is a rotating panel survey, and an individual is followed up with at four different times via home address: now, one month later, twelve months later, and thirteen months later. Surveys consist of basic questionnaire about individual characteristics and special questionnaire that details employment statuses. Table 3 shows the descriptive statistics in my dataset.

3.5 Empirical Results

3.5.1 Direct Effect on Old Workers

Figure 4 presents graphical evidence for older workers based on equation (1). The figure plots the average job separation rate for 60 year-old persons who had a job in treated firms one year ago with respect to month of birth. The sample on the left side shows the job separation rate for

[9] This data roughly corresponds to the Current Population Survey in the U.S.

the old who were not affected by the policy change (control group).[10] The sample on the right side shows the job separation rate for the old who were affected by the policy change (treatment group). The sample is restricted to old persons who worked for treated firms one year ago which were more likely to raise the mandatory retirement age or introduce continued re-employment system after April 2006. The figure reveals that there is a noticeable jump in the job separation rate for older people around the cutoff, suggesting the affected old persons who worked for the treated firms which raised the mandatory retirement ages or introduced continued re-employment system after April 2006 decreased job separation rate than the non-affected cohorts at age 60 who also worked for the treated firms but were not affected by the policy change.

Table 4 reports the RDD estimates of raising the mandatory retirement age and introducing continued employment system on older workers. The first column reports the RDD estimate of of β in equation (1) with full sample. The coefficient is negative but not statistically significant, as expected. But if I limit the sample into individuals who worked for the treated firms which were more likely to raise the mandatory retirement age or introduce continued re-employment system and compare cohorts who were affected and not affected by the policy change, then the effect is larger and statistically significant, as in column (2). The result is robust to including the variables controlling individual and family characteristics, as in column (3). In sum, the table provides empirical evidence that raising mandatory retirement age and introducing reemployment system decreases the job separation rate of older employees who were affected by the policy change in the treated firms with higher intensity. Specification (3) is the main specification, here and in what follows.

The magnitude of the decrease in the job separation rate is about 6 percentage points in the main specification, meaning that raising the mandatory retirement age and introducing a continued reemployment system decrease the job losing rate for the affected cohorts who worked for treated companies by 6 percentage points. Since the mean of the conditional job losing rate was about 20 percentage, the impact of raising the mandatory retirement age and introducing a continued

[10] When they reached the age 60, the government had not implemented the policy yet for the cohorts in the left hand side.

employment system is close to 30 percent.

3.5.2 Indirect Effect on Young Persons

Table 5 shows the effect of the retirement policy change on young persons by the event-study regression equation (2). As in the first column, the estimate is negative and statistically significant, suggesting the increase in employment of older workers also leads to the decrease in the job finding rate for young persons. The magnitude of the decrease in the job separation rate is about 0.5 percentage points, indicating that raising the mandatory retirement age and introducing a continued reemployment system decrease the job finding rate for the young persons from treated companies by 0.4 percentage points. The impact of the policy change to young persons is close to 5.0 percent, suggesting the magnitude of the crowding-out is not so large as the increase in old employment.

Table 5 also presents the estimates for different age subgroups in column (2) and column (3). The effect is most striking for young people in the early 20s but the effect is statistically insignificant for young persons in the late 10s. The result suggests that older workers were more likely to be substitute for young people in the early 20s, but not for young people in the late 10s. This heterogeneous effect could be explained by educational levels, because ages are correlated with educational levels for younger persons. In fact, if I categorize young people into two subgroups depending on the educational levels as shown in Table 6, then the effect is statistically significant for high-skilled persons but not statistically significant for low-skilled persons. The result suggests that older workers' rich working experience could be substitutes for higher education for the young. The table provides empirical evidence that there is heterogeneous intergenerational effect by education in labor markets, and older workers are substitutes for high-skilled workers but not for low-skilled workers.

Figure 5 also graphically presents the estimates by the event-study approach by regression equation (2). The figure plots the estimates of the year dummies before and after the policy change. The job finding rate for the young in firms in a higher treatment intensity sharply decreased immediately after the policy implementation, suggesting the mandatory retirement reform also had an impact on the young.

3.6 Validity, Placebo and Robustness

The internal validity of RDD is that there is no manipulation or differential attrition at the cutoff. To analyze this underlying assumption, I first show the results of a manipulation test and balance checks. I also show the several placebo estimates to augment the further validity of my research design. Finally I also show the results of the robustness tests to see if the estimates are quantitatively robust to alternative specifications.

3.6.1 Validity

A key underlying assumption of RDD is that the running variable cannot be strategically manipulated at the cutoff. This condition is tested based on the methods in McCrary (2008). Appendix Figure A1 graphically shows the density of the running variable for 60-year old people at the cutoff. The p-value of the manipulation test is 0.16, and I do not find a statistical evidence of a systematic manipulation of the running variable.

I also implement balance checks to see if there is a jump for other covariates at the cutoff. Since predetermined variables such as individual and family characteristics were determined prior to the retirement reform, they should not change around the critical cutoff. Appendix Figure A2 presents the predetermined covariates with respect to the running variable, and there is no noticeable discontinuity at the cutoff. The p-values of the null hypothesis that the variable is continuous are 0.70, 0.50, 0.14, respectively, augmenting the further validity of the research design.

3.6.2 Placebo Tests

Appendix Table B1 shows the RDD estimates at a placebo cutoff instead of the true cutoff. The RDD estimate is statistically insignificant at 1 year prior to the true cutoff, increasing the credibility of my research design.

Appendix Table B2 shows the RDD estimates at placebo ages 58 and 59, instead of the critical age 60. As one can see, the estimates are not statistically significant, increasing the further validity of my research design.

Appendix Table B3 presents the RDD estimates where the sample is restricted to the individuals who did not work for treatment firms ($I_i < Mean(I_i)$) before the government intervention. As one can see, the effect of the retirement reform on less affected individuals who worked for non-treated firms is statistically insignificant, providing the consistent results to my main results.

3.6.3 Robustness

Appendix Table B4 shows the sensitivity analysis across different lengths of bandwidth. All the estimates are negative and statistically significant, suggesting the main estimates are robust to the alternative lengths of bandwidth.

Appendix Table B5 shows the estimates with other functional form (quadratic function) and kernel (uniform kernel). The estimates are all negative and provide the consistent results with the main result.

3.7 Conclusion

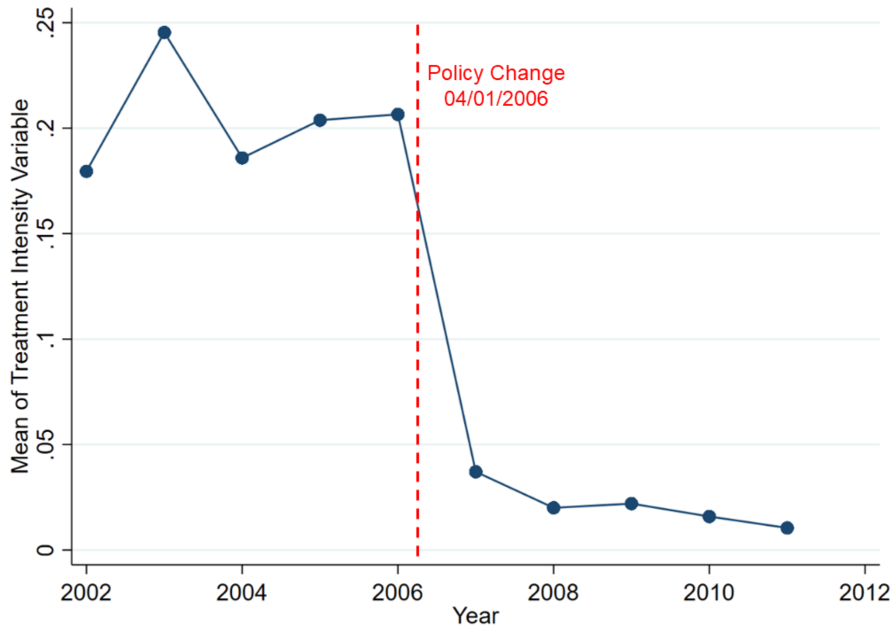
This paper investigates the effect of raising the mandatory retirement age and introducing a continued reemployment system on older workers and young job-seekers. In Japan, the retirement reform in 2006 obligated companies to raise the mandatory retirement age from age 60 to at least age 63 or introduce a continued employment system up until at least age 63. Using RDD and restricted-use data sources, I find that raising the mandatory retirement age and introducing a continued reemployment system decreases the job separation rate of older workers. Furthermore, I also find that the policy change also decreased the job finding rate of young people for the firms that were more affected by the policy change, suggesting older workers crowded out young job seekers in the labor market. The impact of the crowding out is relatively modest compared to the direct impact on the old persons and higher for aged young people with higher education.

Reform of labor markets is an urgent issue with the population aging in the world. This paper provides empirical evidence that reform of mandatory retirement age and related retirement systems play an important role in labor markets. Raising the mandatory retirement age significantly increases firms' labor demand and quantities of labor supplied by old individuals. My empirical results also suggest that the retirement reform has negative externalities in labor markets; more older persons in the labor force lead to less job opportunities for the young. Thus, my findings highlight that policymakers should consider negative externalities to the young when designing policies targeted to the old.

3.8 Acknowledgments

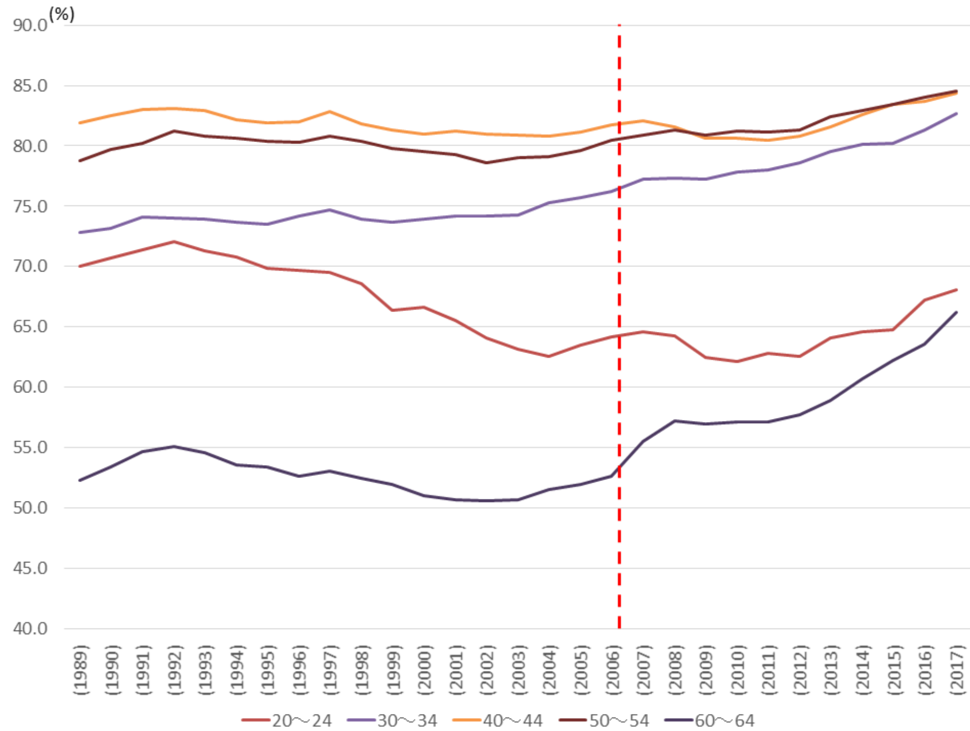
Chapter 3, in full, is currently being prepared for submission for publication of the material. Nobuhiko Nakazawa. "The Effects of Changing Mandatory Retirement Systems on Labor Supply: Individual- and Macro- Level Responses". The dissertation author was the primary investigator and author of this material.

Figures and tables



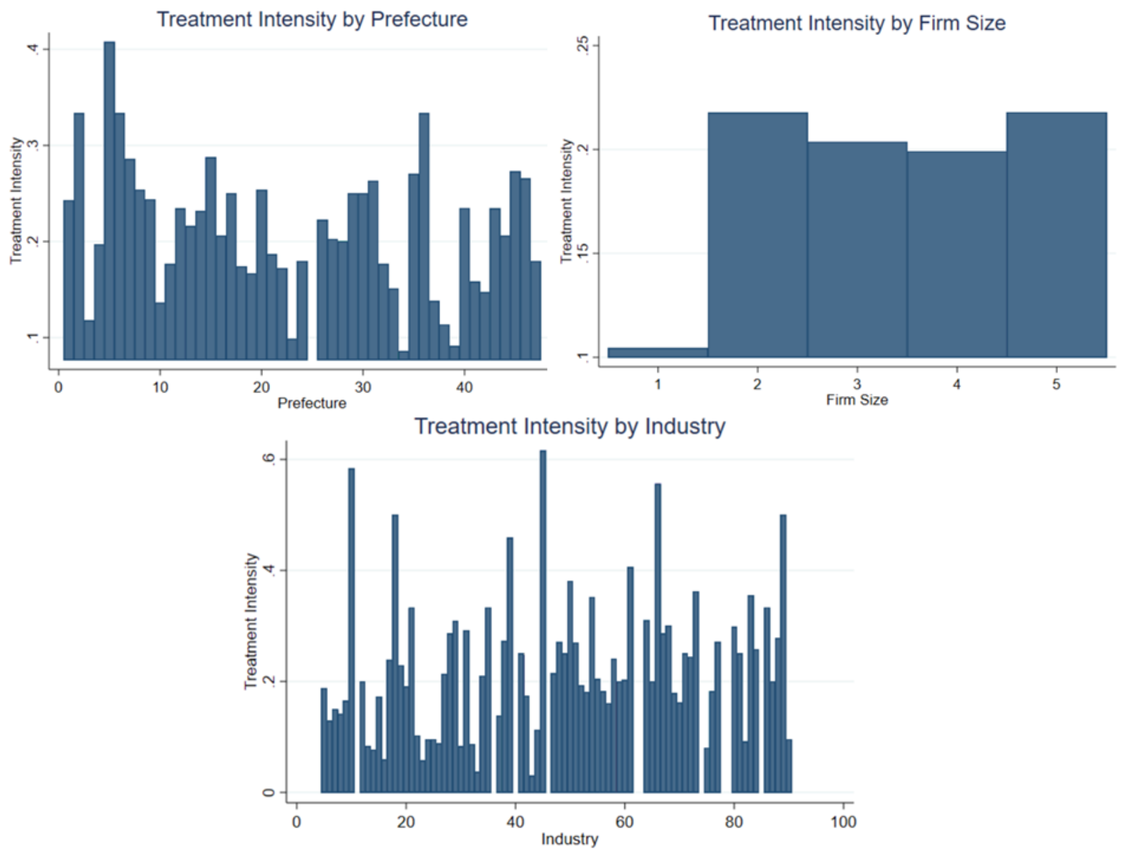
Notes: The figure plots the fraction of the number of treated firms to the total number of firms by year (i.e., the mean of the treatment intensity by year). Specifically, the figure plots the fraction of the number of firms without a continued re-employment system nor a mandatory retirement age above 60 to the total number of firms by year.
Sources: General Survey on Working Conditions and Survey on Employment Management

Figure 3.1: Fraction of Treated Firms by Year



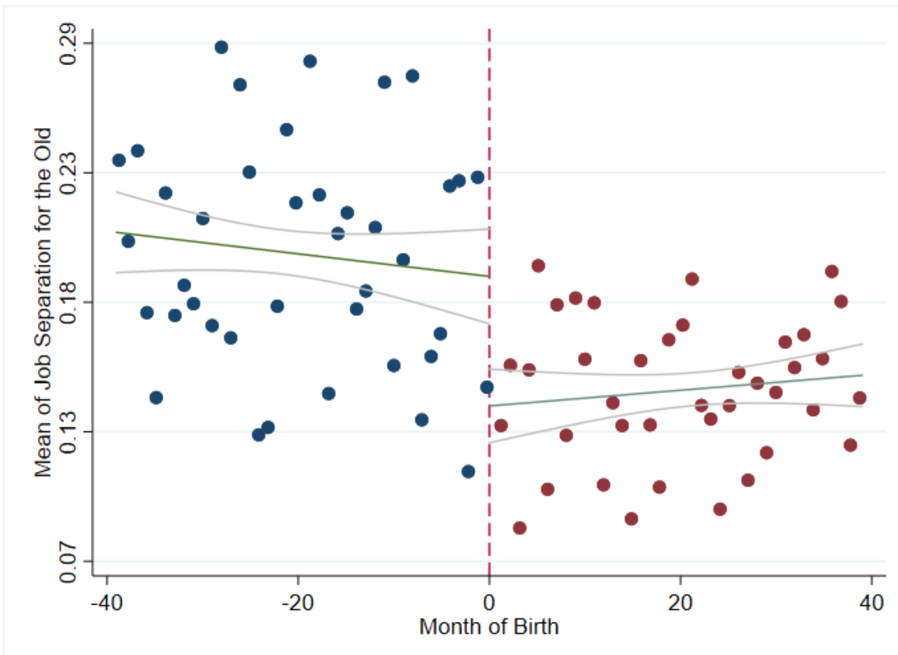
Notes: The figure plots the employment rate by age group. The reform of mandatory retirement systems was implemented in April 2006 on the vertical dotted line.
Sources: Labor Force Survey

Figure 3.2: Employment Rate by Age Group



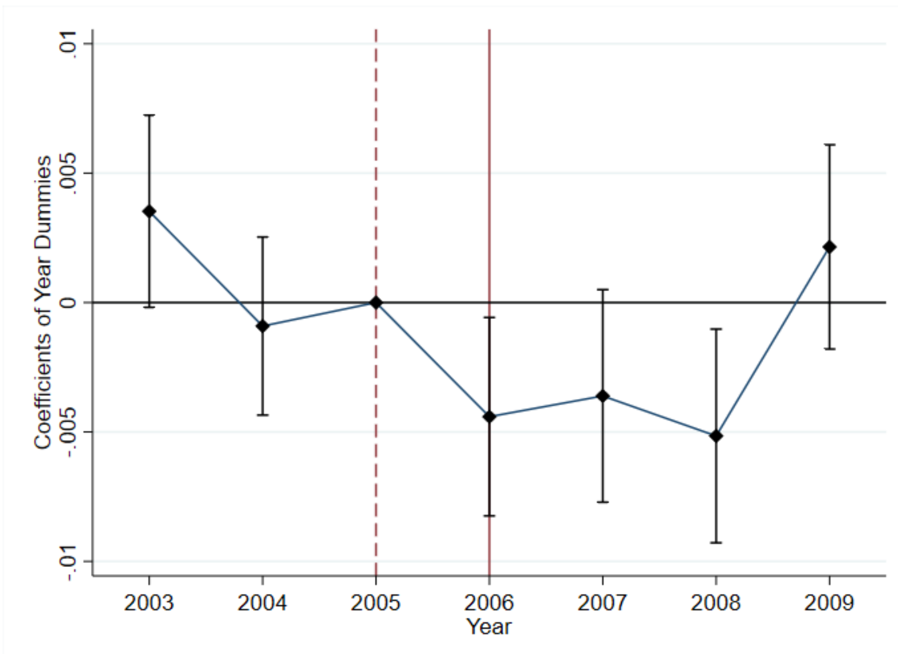
Notes: The figure plots the means of the treatment intensity variable one year prior to the policy implementation by area, firm size, and industry. There are 47 prefectures and more than 90 industrial categories in Japan. Each number of each prefecture on the x-axis is defined by Japanese Ministry of Internal Affairs and Communications, and smaller numbers correspond to northern areas whereas larger numbers correspond to southern areas. Each number of each industry on the x-axis is defined by Japanese Ministry of Internal Affairs and Communications. As for firm size, 1 on the x-axis shows the number of employees is 5,000 or more; 2 on the x-axis shows the number of employees is less than 5,000 and equal to or more than 1,000; 3 on the x-axis shows the number of employees is less than 1,000 and equal to or more than 300; 4 on the x-axis shows the number of employees is less than 300 and equal to or more than 100; 5 on the x-axis shows the number of employees is less than 100. The treatment intensity variable takes 1 if a firm did not set the mandatory retirement age above 60 and did not have continued reemployment system up until the age above 60.

Figure 3.3: Variation of the Treatment Intensity by Area, Firm Size, and Industry



Notes: The figure plots the job separation rate for 60-year-old persons conditional on having a job by month of birth. The vertical dotted line separates the sample into affected cohorts and non affected cohorts. The sample on the left side shows the job separation rate for the old who were not affected by the policy change whereas the sample on the right side shows the job separation rate for the old who were affected by the policy change. The solid straight lines on the panel correspond to linear fitted values, and the lines below and above the linear fitted values correspond to the 99% confidence interval. The sample restriction is described in the text.

Figure 3.4: RDD Estimates of the Effect on Old Persons



Notes: The figure shows the estimated coefficients for the year dummies from the event-study specification (3) for the job finding rate in a firm with a higher treatment intensity for young persons conditional on no job one year ago. The point estimates and their 90% confidence intervals for the coefficients are plotted on the panel. The vertical dotted line corresponds to the omitted year 2005, one year prior to the policy implementation. The vertical solid line corresponds to the policy implementation year 2006.

Figure 3.5: Event Study Approach for Young Persons

Table 3.1: Mandatory Retirement Policy Changes in 2006

	Retirement System of a Firm		Legal or Illegal	
	Mandatory Retirement Age 63 or above	Re-employment System until at least Age 63	Before 2006.3.31	After 2006.4.1
Case 1	Yes	Yes	Legal	Legal
Case 2	Yes	No	Legal	Legal
Case 3	No	Yes	Legal	Legal
Case 4	No	No	Legal	**Illegal**

Notes: The table shows the example of a firm's retirement system and legality. If a firm set the mandatory retirement age at 62 or below and did not introduce continued re-employment system that supports older workers to work until age 63 in the table as of April 1st, 2006, then that firm was illegal and had to introduce a continued employment system or raise the mandatory retirement age (case 4). In contrast, if a firm set the mandatory retirement age at 63 or above or introduced a re-employment system that supports workers to work up until the age at least 63, then that firm was not illegal after the policy implementation (case 1, 2, and 3).

Table 3.2: Mandatory Retirement Ages or Continued Reemployment System

Date	Mandatory Retirement Age	Continued-Reemployment System
01/01/2006	60.37	77.1%
01/01/2007	60.45	93.0%

Notes: The table shows the comparison of the means of mandatory retirement ages set by firms and the fractions of the number of firms having a continued-reemployment system to the total number of firms, before and after the policy change.
Sources: General Survey on Working Conditions

Table 3.3: Descriptive Statistics of Main Variables

Variable	Mean (Standard Deviation)
Outcome Variables	
Job losing rate conditional on having a job one year ago	0.08(0.27)
Job finding rate conditional on no job one year ago	0.08(0.27)
Firms' Retirement System	
Mandatory Retirement Age	60.39(1.39)
Existence of Continued Reemployment System until a certain age	0.82(0.38)
Individual Characteristics	
Ratio of Males	0.50(0.50)
Age	50.71(19.22)
Birth year	1955.01(19.25)
Birth month	6.28(3.53)
Prefecture (47 in total)	21.84(13.30)
Firm Size (1: large, 5: small)	2.54(1.61)
Industry (from 1 to 99; classification by the Ministry of Internal Affairs and Communications)	50.04(29.47)
Obs	8,120,103

Notes: The table reports the means and standard deviations (in parentheses) of the main variables in the entire sample. The sample periods are form 2002 to 2011. The data comes from the Japanese Ministry of Internal Affairs and Communications and the Ministry of Health, Labor and Welfare. The ideas underlying the dataset are described in the text.

Table 3.4: RDD Estimates of the effect of the Retirement Age Reform on Older Workers

Probability of job separation at age 60 conditional on having a job one year ago			
	(1)	(2)	(3)
D	-0.02	-0.05**	-0.06**
	[0.02]	[0.02]	[0.02]
Functional Form	Linear	Linear	Linear
Kernel	Triangular	Triangular	Triangular
Bandwidth	15.09	15.09	15.09
Sample	All	$I_i > Mean(I_i)$	$I_i > Mean(I_i)$
Controls	No	No	Yes
Pre-treatment mean	0.16	0.20	0.20
Obs.	12,061	5,407	5,399

Notes: The parameters are the result from local linear RDD of equation (1) for the probability of separating at the age of 60 conditional on having a job one year ago. The first column reports RDD estimates with the full sample. The second column reports the RDD estimate with the restricted sample with the higher treatment intensity (i.e., the treatment intensity is above the mean). The third column reports the RDD estimate with the restricted sample and with control variables of gender and spouse. The coefficient in the first column is estimated using minimizing square error optimal bandwidth, a linear functional form and triangular kernel. The lengths of the bandwidth of the second, third, and fourth columns are set same as that in the first column. Control variables include genders and existence of spouse. The sample is over the period year 2002 through year 2011. 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 3.5: Impacts on Young Persons

Probability of the job finding in a higher treatment intensity for the young conditional on no job one year ago	(1)	(2)	(3)
Year 2006	-0.004*	-0.025*	-0.003
	[0.002]	[0.014]	[0.002]
Sample	All (Age<25)	21<Age<25	Age<=21
Controls	Yes	Yes	Yes
Pre-treatment mean	0.08	0.21	0.06
R squared	0.01	0.02	0.01
Obs.	84,017	9,984	74,033

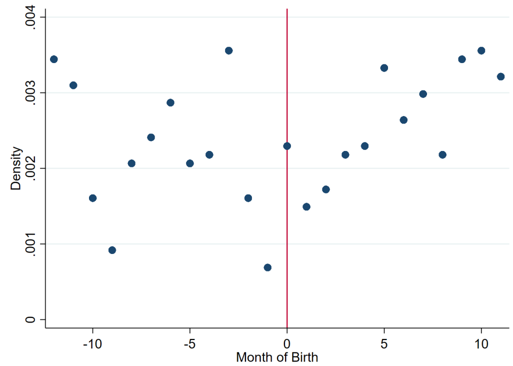
Notes: The parameters are from the event-study regression equation (2). The dependent variable is the probability of job-finding rate of young persons in a higher treatment intensity conditional on no job one year ago. The omitted period corresponds to year 2005, which is one year prior to the policy implementation. The first column shows the result for all young persons at ages at or above 15 and below 25. The second column shows the result for young persons aged above 21 and below 25. The third column shows the result for young persons aged 21 or below and at or above 15. For other details, see the notes to Table 4.

Table 3.6: Heterogeneity: Impacts on Young Person by Education

Probability of the job finding in a higher treatment intensity for the young conditional on no job one year ago	(1)Higher education	(2)Lower education
Year 2006	-0.011**	-0.001
	[0.005]	[0.002]
Sample	All (Age<25)	All (Age<25)
Education	High	Low
Controls	Yes	Yes
Pre-treatment mean	0.11	0.06
R squared	0.01	0.01
Obs.	31,484	52,481

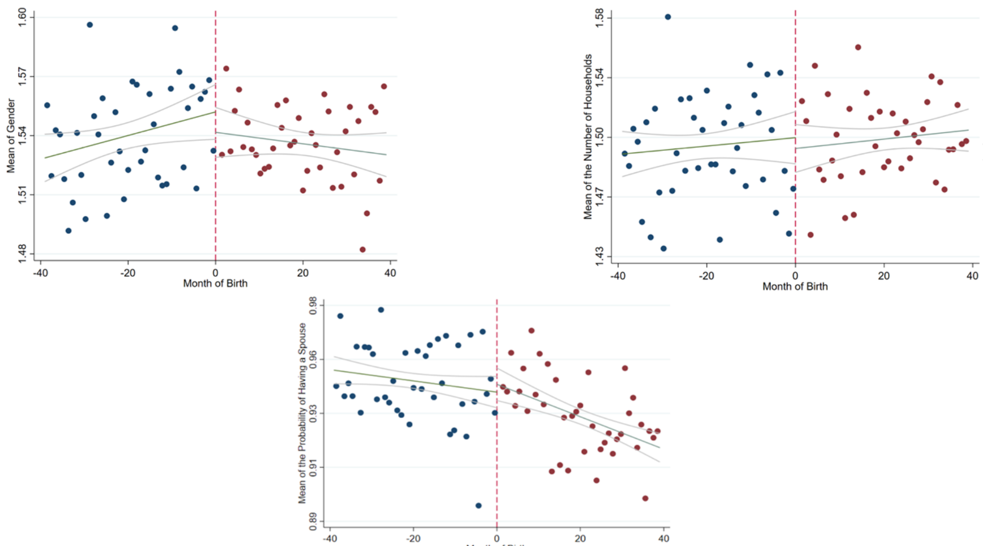
Notes: The parameters are from the event-study regression equation (2) for the young by education. The educational level for young persons in the first column is high school or below. The educational level for young persons in the second column is above high school. For further details, see the notes to Table 5.

Appendix Figures



Notes: The figure plots the density of the running variable around the boundary separating affected cohorts from non-affected cohorts with a window of one year. The p-value of the manipulation test by McCrary (2008) is 0.16.

Figure 3.A1: Density of the Running Variable



Notes: The figures plot the means of the pre-determined covariates along the running variable at the age 60. The solid straight lines on the panel correspond to linear fitted values, and the lines below and above the linear fitted lines correspond to the 99% confidence interval. The upper left figure plots the means of the gender (1:male, 2: female). The upper right figure plots the means of the number of households. 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 0.14, 0.50, 0.70, respectively. For other details, see the notes to Figure 4.

Figure 3.A2: Pre-determined Covariates

Appendix Tables

Table 3.B1: Placebo Cutoff

Probability of job separation at age 60 conditional on having a job one year ago	
	Placebo Cutoff (1 years ago)
D	0.056
	[0.036]
Functional Form	Linear
Kernel	Triangular
Bandwidth	10.53
Sample	$I_i > Mean(I_i)$
Pre-treatment mean	0.18
Obs.	3,149

Notes: Parameter are from separate regressions (1), where the cutoff is one year before the true cutoff. For further details, see the notes to Table 4.

Table 3.B2: Placebo Ages

Job separation rate at the placebo ages conditional on having a job one year ago		
	(1)Placebo Age (59)	(2)Placebo Age (58)
D	-0.026	0.037
	[0.027]	[0.024]
Functional Form	Linear	Linear
Kernel	Triangular	Triangular
Bandwidth	12.01	8.26
Sample	$I_i > Mean(I_i)$	$I_i > Mean(I_i)$
Pre-treatment mean	0.10	0.06
Obs.	3,816	2,879

Notes: Parameter are from separate regressions (1) for the job separation rate at age 58 and 59, instead of the critical age 60. For further details, see the notes to Table 4.

Table 3.B3: Placebo Sample

Probability of job separation at age 60 conditional on having a job one year ago	
	$I_i < Mean(I_i)$
D	0.001
	[0.024]
Functional Form	Linear
Kernel	Triangular
Bandwidth	14.88
Sample	$I_i < Mean(I_i)$
Pre-treatment mean	0.15
Obs.	6,058

Notes: The parameter is from a regression equation (1), where the sample is restricted to the one which was less affected by the policy change. For further details, see the notes to Table 4.

Table 3.B4: Robustness: Sensitivity Analysis

Probability of job separation at age 60 conditional on having a job one year ago		
	(1)	(2)
D	-0.073*	-0.065**
	[0.039]	[0.032]
Functional Form	Linear	Linear
Kernel	Triangular	Triangular
Bandwidth	15	20
Sample	$I_i > Mean(I_i)$	$I_i > Mean(I_i)$
Pre-treatment mean	0.20	0.20
Obs.	4,946	7,128

Notes: The parameters are from separate RDD regressions (1), where the lengths of the bandwidth are fixed at 15 and 20, respectively. For further details, see the notes to Table 5.

Table 3.B5: Robustness: Other Functional Forms and Kernels

Probability of job separation at age 60 conditional on having a job one year ago			
	(1)	(2)	(3)
D	-0.079*	-0.073**	-0.068
	[0.042]	[0.033]	[0.043]
Functional Form	Quadratic	Linear	Quadratic
Kernel	Triangular	Uniform	Uniform
Bandwidth	16.40	10.07	14.01
Sample	$I_i > Mean(I_i)$	$I_i > Mean(I_i)$	$I_i > Mean(I_i)$
Pre-treatment mean	0.20	0.20	0.20
Obs.	5,837	3,271	4,946

Notes: The parameters are from separate RDD regressions (1), where each functional form and each kernel are used as in the second and third rows. For further details, see the notes to Table 4.

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