The welfare effects of protecting older workers*

Todd Morris† Stefan Staubli‡ Benoit Dostie§

November 21, 2025

Abstract

We study provincial mandatory retirement bans in Canada using linked employer-employee tax data. The bans sharply reduce job separations and retirements at age 65, with sizable announcement effects and considerable heterogeneity across industries and employers. Employment rates and average earnings are unchanged below age 65 but rise substantially thereafter. These welfare gains for older workers are mediated by several spillovers, including higher savings and spousal retirement delays. Highly exposed firms maintain or increase worker productivity and profitability but hire fewer middle-aged workers, decreasing their overall payroll costs. Government revenue also rises, indicating that protecting older workers was a Pareto improvement.

Keywords: employment protection; retirement; welfare; active and passive savings responses; health; spousal spillovers; firms

JEL Classification: J26, J78, H55

*For helpful comments, we thank Agar Brugiavini, Esteban Garcia-Miralles, Maxime Gravoueille, Pierre-Carl Michaud, Maria Ptashkina and Han Ye, as well as seminar and conference participants at the Australian Workshop on Public Finance, Ca' Foscari University of Venice, the Canadian Economic Association conference, the CEPAR colloquium, e61 Institute, HEC Montréal, the Monash HELP conference, Monash University, the Natural Experiments workshop, the Society of Labor Economists conference, the University of Cagliari, the University of Melbourne, the University of Sydney and the University of Queensland. The analysis presented in this paper was conducted at the Quebec Interuniversity Centre for Social Statistics (QICSS) which is part of the Canadian Research Data Centre Network (CRDCN). The services and activities provided by the QICSS are made possible by the financial or in-kind support of the Social Sciences and Humanities Research Council (SSHRC), the Canadian Institutes of Health Research (CIHR), the Canada Foundation for Innovation (CFI), Statistics Canada, the Fonds de recherche du Québec and the Quebec universities. The views expressed in this paper are those of the authors, and not necessarily those of the CRDCN, the QICSS or their partners.

†University of Queensland, IZA, Life Course Centre, CEPAR & Netspar. todd.morris@uq.edu.au

‡University of Calgary, CEPR, CESifo, IZA & NBER. sstaubli@ucalgary.ca

§HEC Montréal, IZA. benoit.dostie@hec.ca

1 Introduction

Reducing barriers towards old-age employment is a pressing priority amid global population aging. Although retirement is often thought of primarily as a labor supply decision, in many countries, at least one-third of retirees report being forced into retirement by their employer (Dorn and Sousa-Poza, 2010; Steiber and Kohli, 2017). In the United States, Johnson and Gosselin (2018) document a substantial increase in the share of retirees reporting that they were completely or partly forced to retire, rising from 33% to 55% between 1998 and 2014. These findings suggest that demand-side interventions that boost employment protection for older workers may significantly increase old-age employment.

However, the welfare effects of protecting older workers is unclear. Employment protections often have unintended consequences on workers in equilibrium, even on groups they are designed to protect (Acemoglu and Angrist, 2001; Autor et al., 2006). These adverse consequences can arise if firms reduce their demand for older workers, resulting in lower hiring rates or wages. Furthermore, even if older workers benefit from stronger employment protections, the policies may reduce opportunities for younger workers and impose costs on firms' productivity and profitability (Lazear, 1979; Saez et al., 2024).

In this paper, we provide a comprehensive welfare analysis of extending standard employment protections to older workers, in the context of several wide-reaching bans on mandatory retirement (MR). MR refers to the practice whereby employment protections cease at a specified age.[1] We study a series of MR bans in a contemporary North American setting: between 2005 and 2009, five of Canada's ten provinces announced and implemented a ban on MR, which had previously been possible at age 65. These bans applied across nearly all industries. The bans aligned MR laws in these provinces, which account for 60% of Canada's population, with other provinces and most of the Anglosphere, including the US. Yet many other countries retain active MR provisions for large parts of the workforce (e.g., Germany, France, Ireland, Finland, Sweden, Slovenia, the Netherlands, Norway, Portugal, Iceland and Japan).[2] Thus, the effects of the Canadian bans have both historical and contemporary relevance.

[1] At this point, employers are legally permitted to terminate employment based solely on age, without cause or severance obligations. Workers may subsequently seek employment elsewhere or, in many cases, negotiate a new contract with their current employer.

[2] Additional countries have MR provisions specifically for civil servants (China, Spain, Belgium, Ireland, Portugal, Latvia and Czechia).

Our analysis leverages annual linked employer-employee tax records encompassing the universe of Canadian tax filers and employers from 2001 to 2019. During this period, MR laws vary by province, year and workers' age, providing an ideal natural experiment. We estimate equilibrium impacts of the MR bans at the province level on workers, firms and public finances using stacked difference-in-difference (DiD) and triple-difference (DDD) approaches.

We start our analysis by exploring the effects of MR bans on older workers' job separation rates and retirement hazards. A simple graphical analysis uncovers clear policy impacts. Prior to the bans, workers in treated provinces exhibit similar separation rates and retirement hazards at ages 61–64 to workers in control provinces (those with existing bans), but significantly higher rates at age 65. These differences at age 65 vanish within 2–3 years of the announcement of the bans, with the elevated rates in treated provinces declining to match the rates in control provinces, while there is no change in trends at ages 61–64. Our DiD and DDD regressions corroborate these results and yield similar estimates: the bans reduced separation rates and retirement hazards at age 65 by 6 percentage points. We also discover further reductions at ages 66–68 and a strong announcement effect. Each ban was announced between 6 and 26 months prior to implementation, allowing employers time to adjust to the policy. Interestingly, there is no evidence that employers rush to fire older workers over this period to reduce their exposure. Rather, separation rates at age 65 decline over the announcement period, a phenomenon consistent with an "expressive value of the law" (Sunstein, 1996; Posner, 2002; Benabou and Tirole, forthcoming; Acemoglu and Jackson, 2017; Moore and Morris, 2024).

We document considerable heterogeneity in the effects of the bans. Many industries show no change in workers' separation rates at age 65, while others show reductions greater than 10 percentage points. The most affected industries are health care and social assistance; manufacturing; public administration; educational services; information and cultural industries; utilities; and mining, quarrying, and oil and gas extraction. Together, these industries comprise 42% of older workers but account for 77% of the response. These industries are characterized by high levels of unionization, employer-pension coverage, wages, workforce size, and workplace injuries.

Our results indicate that the MR bans caused sharp reductions in separation rates and retirement hazards at age 65. However, these ostensibly positive developments for older workers may be just one side of the story; it is possible that firms respond by hiring fewer older workers, or offering them lower wages, in response to the bans. While similar concerns have been exam-

ined in the context of age-discrimination laws, with mixed findings (Neumark and Stock, 1999; Adams, 2004; Lahey, 2008), our administrative data enables a more comprehensive analysis. In treated provinces, we find no significant change in the hiring rates of workers aged 61–64 and can rule out reductions in hiring larger than 13% with 95% confidence. Moreover, there is little difference in the estimates for industries where MR was most common. We also find no evidence of a decline in employment or earnings below age 65, both for workers who are close to age 65 and younger workers who may be exposed to spillovers. Above age 65, we estimate significant positive impacts: employment rises by 14% at ages 66–69, while earnings increase even more (19–34%) due to the relatively high incomes of the affected workers. These estimates are considerably larger than previous studies examining the employment effects of historical MR policies in North America (Burkhauser and Quinn, 1983; Neumark and Stock, 1999; Shannon and Grierson, 2004), which relied on more limited data and policy variation.[3] Our estimated employment and earnings effects mean that an average worker approaching age 65 works 3.6 months longer and earns $20,000 more due to the bans. These retirement delays match optimistic projections of a substantial labor supply intervention — raising Canada's public pension ages from 65 to 67. Moreover, the MR bans have a different set of compliers, who are motivated rather than compelled to work beyond age 65.

The one-for-one link between separations and retirement is crucial in explaining the large employment effects of the policy. This link might reflect terminated older workers' lack of job search and/or from firms' reluctance to hire them (Neumark et al., 2019). Distinguishing these mechanisms is challenging, but we can gain some insight on job search intensity using information on UI claims, which require individuals to actively seek work, and transitions into self-employment. Our estimates imply that out of every 100 workers terminated at age 65, just 16 file UI claims, and merely 6 become self-employed. These results suggest low levels of job search by terminated older workers, which is consistent with older workers valuing workplace continuity and perceiving a lack of demand from other firms. This perception may be accurate, as hiring

[3]Burkhauser and Quinn (1983) use a longitudinal survey to examine the labor market transitions of men around age 65 in the US prior to the nationwide increase in the MR age from 65 to 70 in 1978; they projected that labor force participation would be just 5% higher at age 65 without MR. Neumark and Stock (1999) utilize 1% samples of US decennial census data sets to study an increase in the MR age from 65 to 70, with variation coming from the 1978 nationwide increase and pre-existing state policies; they find no evidence of any employment effects above age 60. Shannon and Grierson (2004) study the first provincial MR bans in Canada, relying on data with imprecise information on age (five-year groups), likewise finding no employment effects. Another set of studies focused on university professors (Ashenfelter and Card, 2002; Clark and Ghent, 2008; Warman and Worswick, 2010), finding much larger employment effects.

rates for workers at age 65 do not change after the bans, despite the decline in separations.

We examine spillovers of workers' postponed retirements on a variety of saving behaviors, two objective health measures, and the timing of spouses' retirements.[4] These effects are not only informative about the overall effects of the bans but also on the mechanisms through which they impact older workers' welfare and public finances. We uncover significant active and passive saving responses (Chetty et al., 2014), whereby workers who delay retirement make additional contributions to private and public pension schemes and tax-favored individual retirement accounts, and also delay claiming and withdrawals. We estimate that each $1 change in earnings leads to only a $0.54 change in disposable income, with savings adjustments explaining two-thirds of this imperfect pass-through. Thus, saving responses provide an important form of self-insurance for older workers forced to retire. In terms of health impacts, we find that the bans significantly increased workplace injuries after age 65, as measured by new workers' compensation payments, but had no observable short-term effect on mortality. Combined, this evidence is consistent with the elevated injury risk among workers in affected industries but does not reveal a general deterioration in health. The latter result aligns with Messacar and Kocourek (2019), who study the same policies and find no effects on self-reported health. Finally, we find that younger spouses delay their own retirements, indicating that preferences for joint retirement dominate added worker effects (Lundberg, 1985) in our setting. These spillovers on spouses amplify the direct effect of the bans by around 20% in dual-earning couples.

To estimate the effects on firms that are exposed to the bans, we construct a balanced panel of firms in treated and control provinces over an 11-year period around the policy announcement date. To maximize statistical power, we restrict our sample to firms in the most affected industries and focus on highly exposed firms: those with a high share of workers covered by a union or employer pension plan. We use a DDD approach, which controls for time-specific factors affecting these firms, using similar firms in other provinces, and province-year trends using moderately exposed firms as an additional control group. The latter set of controls is

[4]We relate to a growing literature on the spillover effects of retirement on savings behavior (Lindeboom and Montizaan, 2020; Etgeton et al., 2023; García-Miralles and Leganza, 2024*b*; Nakazawa, 2022), an extensive literature exploring health outcomes and behaviors (see Garrouste and Perdrix, 2022, for a literature review); and papers examining spillovers on spouses' retirement decisions (Lalive and Parrotta, 2017; Selin, 2017; García-Miralles and Leganza, 2024*a*; Lalive et al., 2023; Johnsen et al., 2022). Most studies of retirement spillovers focus on a single type of spillovers, preventing a broad understanding of welfare impacts, and nearly all exploit reforms aimed at the labor supply side (voluntary retirement) rather than the demand side (involuntary retirement).

important to account for pre-trends in firms' outcomes. Importantly, our DDD estimates show a clear first stage — exposed firms in treated provinces employ significantly more workers aged 66–69 after the bans — and the absence of pre-trends in firms' financial outcomes.

We find no evidence that highly exposed firms become less productive or profitable due to the bans. Our point estimates for both outcomes are positive, and the positive estimates on profitability are statistically significant in per-worker terms and for the most highly exposed firms. Highly exposed firms appear to substitute away from younger groups of workers, with a significant decline in overall payroll costs. This decline is driven mainly by a decrease in middle-aged (35–49) workers, who are less likely to be hired by these firms.[5] In terms of firms' other financial outcomes, we estimate no change in total revenue, total value added, intermediate inputs, total assets, and sales, while gross profits increase roughly one-for-one with the reduction in payroll costs. Overall, our results suggest that the MR bans encourage firms to retain valuable and motivated older workers, lessening their need to hire replacement workers (Mercan and Schoefer, 2020) and incur turnover costs (Bertheau et al., 2022).

Overall, our results suggest that eliminating the discontinuity in employment protection at age 65 is a Pareto improvement. We estimate a positive or insignificant effect on firms' profits, and we uncover a rich set of consequences for workers and public finances. Most importantly, workers experience an increase in earnings above age 65, with no observable negative labor market consequences at younger ages or changes in overall health, and no change in pension benefits. Thus, workers' revealed preference to respond so strongly by continuing to work beyond age 65 indicates that the policy significantly improves their welfare. For public finances, the bans generate large fiscal externalities, mostly via increased income tax receipts from workers and firms, with little direct cost. Thus, we conclude that the Canadian MR bans belong to a special case of welfare-improving policies with a Marginal Value of Public Funds $= \infty$ that "pay for themselves" (Hendren and Sprung-Keyser, 2020).

Our paper builds on an emerging literature that emphasizes the importance of employers in the retirement decision (Kondo and Shigeoka, 2017; Frimmel et al., 2018; Rabaté, 2019; Rabaté et al., 2024; Deshpande et al., 2021; Saez et al., 2024; Chrostek et al., 2024). Particularly relevant

[5]This analysis relates to an emerging literature that explores firms' personnel responses to policies that increase the retention of older workers, such as increases in the retirement age (Bianchi et al., 2022; Hut, forthcoming; Ferrari et al., 2023; Boeri et al., 2021; Carta et al., 2021). As discussed above, we do not find any adverse aggregate effects on the employment or earnings of workers below age 65, which is what matters for welfare analysis.

are studies exploiting *increases* in MR ages, either for specific industries (Rabaté, 2019) or nationally (Kondo and Shigeoka, 2017; Rabaté et al., 2024; Saez et al., 2024). These foundational studies, which focus on countries with more stringent employment protections in Europe (France, Sweden, and the Netherlands) or Asia (Japan), show that extending employment protections to older workers can significantly boost old-age employment. However, these studies do not consider the impacts on firms' performance or personnel decisions,[6] younger workers' labor market outcomes, the non-labor market outcomes for older workers or overall welfare. Moreover, causal identification in most of these studies is complicated by simultaneous pension reforms. By studying a complete ban on MR in a North American setting, with provincial variation in ban timing and otherwise relatively stable retirement policies, we have a novel and compelling setting for our comprehensive analysis.

2 Policy background

2.1 Mandatory retirement laws across Canada

Similar to the US, MR emerged in Canada with the establishment of public pensions, as these pensions set a default expectation that workers would retire at age 65 when they became eligible for full retirement benefits. Specifically, this default was set by the US Social Security Act (1935) and later by Canada's Old Age Security (1952) and Canada Pension Plan (1966). In both countries, MR referred to the absence of employment protections for workers aged 65 and older. Employers could terminate workers over this age without providing a cause or incurring termination costs. However, it remained legal for those workers to find jobs with other employers and, in most cases, workers were not prohibited from signing new contracts with the same employer (Gunderson, 1987). Given the relatively low firing frictions in both countries even up to age 64, the option to enforce MR may have been largely redundant for many employers. Enforcement is likely to be more common among unionized employers, as collective agreements commonly contained robust "just cause" provisions and seniority rights that strongly protected workers up to age 64, creating a sharp discontinuity in contractual protections at age 65.

[6] Chino (2024) examine a MR ban in Japan on the stock prices and financial outcomes of publicly listed companies. He finds a negative impact on the stock prices of firms with an older workforce in the week after the policy announcement date. Longer run estimates of the effects on firm performance, which are likely contaminated by a simultaneous increase in the pension age that would disproportionately affect firms with older workers, show no effect on worker productivity and an increase in payroll costs.

Although MR is no longer present in North America (except in a very limited set of occupations), MR regulations are still in effect in several European countries — including Germany, France, Ireland, Finland, Sweden, the Netherlands, Norway, Portugal, and Iceland — and Japan. Relative to these countries, the Canadian labor market for older workers is closer to that in the US, with low levels of statutory employment protection and similar employment rates at age 55–64. However, collective bargaining coverage in Canada remains strong — at 30.2% in 2019 — nearly triple that of the US (11.6%), but close to the OECD average (32.1%).

Unlike in the US, where MR was banned nationally in 1986, MR remained legal in much of Canada in 2001, when our universal administrative data starts. Specifically, MR remained possible in five Canadian provinces: Ontario, British Columbia, Saskatchewan, Nova Scotia, and Newfoundland and Labrador. Collectively, these five "treated" provinces make up 60% of Canada's population. In the rest of Canada, which comprises five provinces and three territories ("control provinces"), MR was already banned.[7] Our paper exploits MR bans legislated in our five treated provinces between December 2005 and May 2007 (Table 1), which covered around 92% of the workforce.[8] In each instance, there was a gap between the date the law was signed ("announcement date") and the date MR was prohibited ("implementation date"). This period ranged from 6 to 26 months and was designed to allow employers time to adjust their HR practices and modify collective agreements, where necessary.

Table 1: Post-2001 mandatory retirement bans in Canada

Province	Announcement date	Implementation date
Ontario	December 12, 2005	December 12, 2006
Newfoundland	May 26, 2006	May 26, 2007
Nova Scotia	April 13, 2007	July 1, 2009
British Columbia	April 25, 2007	Jan 1, 2008
Saskatchewan	May 17, 2007	November 17, 2007

[7]MR bans were implemented in New Brunswick (1973), Manitoba (1982), Quebec (1983), Prince Edward Island (1988) and Alberta (2000). It was never allowed in Nunavut, Yukon or the Northwest Territories. In some cases (New Brunswick and PEI), the MR bans were relatively weak and contained many exceptions (Messacar and Kocourek, 2019). In 2010, the bans on PEI were strengthened following a successful court challenge. While this could have reduced job separation rates at age 65, we observe no evidence of such an effect relative to other control provinces at age 65 or compared to separation rates at ages 61–64. This, combined with the small population on PEI (0.5% of Canada), implies that our estimates are almost identical even if we drop them from our sample.

[8]Workers in the federal public service (<2% of workers) were not affected, as MR was banned there in 1986. Another 6% of workers were not affected because they are covered by federal regulation, where MR was banned in December 2012. Federal regulation covers workers in specific industries including parts of air, rail, road and maritime transportation, postal services, banks and telecommunications (Appendix A.1).

2.2 Other retirement programs in Canada

As we are interested in labor market outcomes around the age of 65, it is important to consider the broader retirement system in Canada. This broader system is built on three pillars: (i) public pensions, (ii) employer pensions, and (iii) tax-favored retirement savings accounts.

The first pillar consists of two distinct parts: (i) Old Age Security (OAS) plus the Guaranteed Income Supplement (GIS) and (ii) the Canada/Quebec Pension Plan (CPP/QPP). OAS is a (relatively) flat-rate pension funded from general taxation and typically received from the age of 65.[9] GIS is a supplement aimed at assisting lower-income retirees who receive OAS.[10]

The CPP/QPP functions as a defined benefit system similar to the US Social Security system, designed to replace 25% of average lifetime earnings in retirement up to a maximum amount. Contributions are compulsory for all working Canadians aged 18–70. The contribution amount depends on annual earnings, with contributions required by both the employee and employer.[11] Upon retirement, the annual CPP/QPP benefits a retiree receives depends on the age they choose to start claiming. Although claims can be initiated as early as age 60 or deferred until age 70, the standard age for claiming is 65. Opting to claim benefits prior to this age incurs penalties, whereas deferring claims yields bonuses.

The second pillar of the retirement system consists of employer-pension plans. Around 37% of Canadians are covered by a plan, with the majority defined benefits (Baldwin, 2015). Most of these plans stipulate a normal retirement age of 65, with an option to claim early retirement from age 55. Contrary to public pension benefits, claiming requires leaving the job.

The final pillar of the Canadian system consists of tax-favored retirement savings accounts. The most prominent is the Registered Retirement Savings Plan (RRSP), which is similar to US Individual Retirement Accounts. RRSP contributions are tax deductible up to a contribution limit, while earnings accrue tax free. Withdrawals are taxed at the person's marginal tax rate and can occur at any age (but are mandatory by the age of 71).

[9] In 2023, the maximum amount was $691 per month ($8,292 per annum). There is a 15% clawback rate for individuals whose income is greater than $86,912. Since 2013, OAS claiming can be deferred (up until age 70), for a 0.6% higher rate per month of deferral, but around 96% claim at age 65 (ESDC Canada, 2019).

[10] Eligibility is contingent on household income, with a maximum amount of $1,032.10 per month ($12,385.20 per annum), and a 50% clawback rate for individuals earning more than $5,000 per annum.

[11] In 2023, the employee and employer contribution rates are 6.4% in Quebec and 5.95% in the rest of Canada. Contributions must be made up to the maximum pensionable earnings amount (approximately $66,000 in 2023).

Given the incentives in the first and second pillars, many Canadians may voluntarily retire at age 65 — coinciding with the age that MR becomes possible. Fortunately, variation in MR laws across provinces and over time allows us to discern the effects of these bans. Yet, a careful approach is necessary to prevent mistaking the effects of the bans with other policies that vary across provinces. In particular, there are three policies that varied between Quebec and the rest of Canada after 2011.[12] Importantly, all three policies were instituted several years after the last MR ban was announced in 2007 and likely had a minor impact on retirement timing. We control for the impact of these policies in our regressions and verify that the results remain stable if Quebec is excluded. The consistency in our estimates before and after 2011 further underscores the minimal relevance of these policies.

3 Labor market consequences for workers

3.1 Data

We use longitudinal linked employer-employee tax data from the Canadian Employer-Employee Dynamic Database (CEEDD), an annual data set covering the universe of tax filers and employers in Canada from 2001–19.[13] For individual tax filers, the data contains a unique ID, year of birth (and death, if applicable) and comprehensive measures of income, including income statements from each employer.[14] A family file allows us to link the records of spouses, while unique employer IDs allow us to link workers to employers. For employers, the data provide detailed financial information on (incorporated and unincorporated) firms' total revenue, value added, intermediate input costs, gross profits, total assets, total cost of sales, and payroll expenses.

We start our analysis by examining the effects of the MR bans on older workers' job separations rates and retirement hazards. This allows us to infer how much — and where — MR was enforced. We anticipate that the bans will directly affect job separations at age 65, and we are interested in the extent to which changes in separation rates affect decisions to retire. To study

[12]The first policy increased the size of the penalty/bonus for claiming the CPP/QPP before/after age 65 (Glenzer et al., 2025). These increases started in 2011 for the CPP and 2013 for the QPP. Second, the work cessation test for the CPP was dropped in 2012 (2014 for the QPP). Until these dates, individuals younger than 65 had to demonstrate that they had reduced their earnings for at least two months before they were able to claim the CPP/QPP. Third, in 2012 the Quebec government introduced an earned income tax credit for workers over the age of 65. The size of the credit was initially modest (with a maximum value of $450) but has increased over time (to a maximum value of $1,650) and been progressively expanded to include those aged 60–64.

[13]We find no evidence that tax-filing varies with age or MR laws.

[14]Like most administrative data sets, we do not have information on education, occupation, or hours worked.

these outcomes, we restrict our analysis sample to individuals aged 61–68 years old (at the end of year t) who are still working (defined as receiving employment income from a job of at least \$1,000).[15] We define a separation in year t if a worker is no longer working for their employer in year $t+1$.[16] We define a retirement in year t if the worker has no employer in year $t+1$.

In addition to analyzing job separations and retirement, we also measure the effects of the bans on the hiring rates of older people. Since both workers and non-workers may be hired, we include all tax filers aged 61–68. For this sample, we classify an individual as hired in year t if they start at least one new job that year.

The final labor market outcomes we consider for older workers are employment and total earnings (both measured in year $t+1$).[17] Our preferred estimates are based on a sample of individuals with positive earnings in the year they turn 61 (who are then tracked through to age 68). This restriction increases the power of our estimates by excluding people who are unlikely to be employed near age 65. Nonetheless, our estimates imply similar *percentage* changes in employment and total earnings (but are less precise) if we include all tax filers.

3.2 Graphical evidence of effects for older workers

Figure 1 shows clear impacts of the MR bans on older workers' job separation rates and retirement hazards. In Panel (a), we show average rates and hazards by single year of age, time period (pre/post reforms), and treatment status. We divide workers into treated and control provinces (see Table 1) and show the rates and hazards for two distinct time periods: 2001–05 (pre-bans) and 2010–18 (post-bans). In both treated and control provinces, we observe a downward change over time in separation rates and retirement hazards at each age. Both rates increase with age and peak at 65, similar to age patterns in the US (Deshpande et al., 2021). Prior to the bans, both outcomes are similar between treated and control provinces at ages 55–64, but we see a notable spike for those aged 65 and over in treated provinces. After the bans, the outcomes are indiscernible across all ages for treated and control provinces. These comparisons suggest that the MR bans have reduced separation rates and transitions into retirement for workers aged 65

[15] We exclude about 8% of workers who either work in the federal public service or are employed in the aforementioned industries not covered by provincial regulation (see Appendix A.1). We also exclude individuals who die in year t or $t+1$ or are not observed in the data in both years for other reasons (e.g., international migration).

[16] To avoid multiple observations per individual, we focus on the main job (highest-earning job in year t).

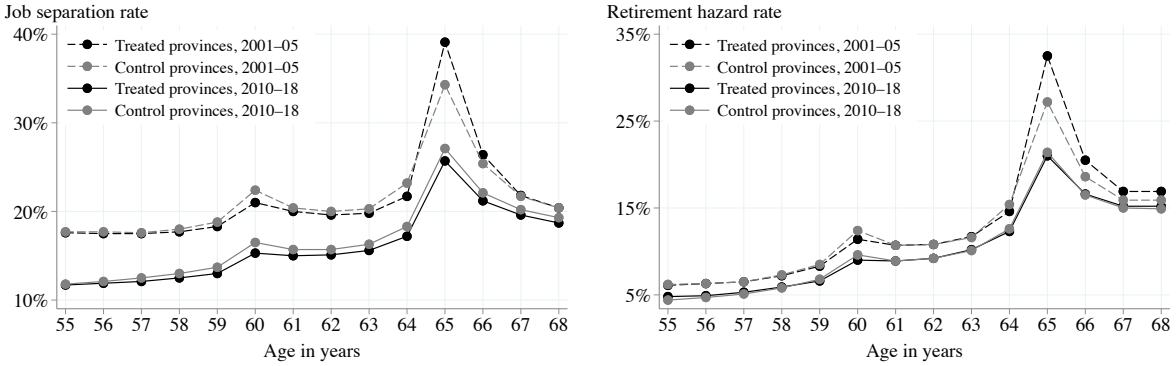
[17] Earnings are inflation-adjusted to their 2019 values based on the annual CPI index for each province.

and older. The most substantial change is evident at age 65, the initial age when employers could dismiss workers. The magnitude of the effects is similar for both outcomes, hinting that most workers who are dismissed under MR retire instead of finding another employment opportunity.

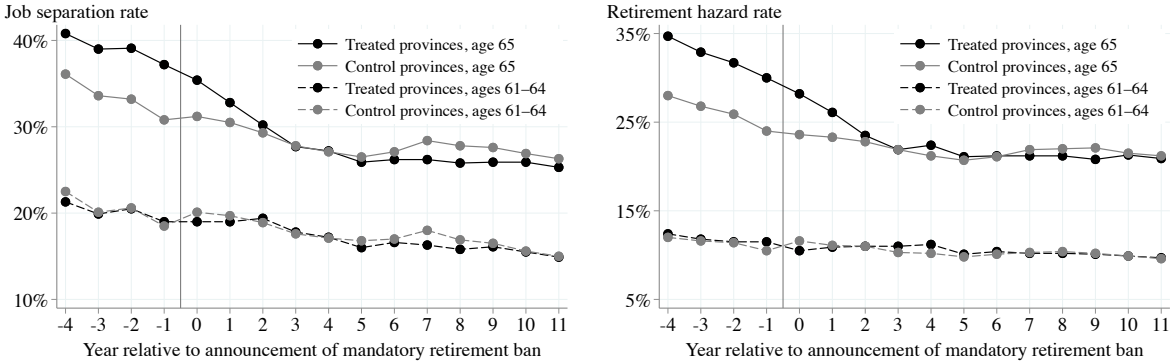
Figure 1: Trends in separation rates and retirement hazards by treatment status, age and time

Job separation rates | Retirement hazards

(a) Mean rates by age, treatment status and time period (pre/post reforms)



(b) Mean rates in event time by age group and treatment status



In Panel (b), we show how these outcomes have evolved in event time by treatment status and age group (61–64 or 65). For treated provinces, event time on the x-axis is the number of years until the ban is announced in that province. For the control provinces and territories, there are no reforms. Thus, we create a stacked data set where each control province observation is duplicated four times and matched to a single treated province (allowing us to assign an 'announcement year'). At ages 61–64, we observe a slightly downward trend in separation rates and retirement hazards over time, and the trends are indistinguishable in treated and control provinces before and after the bans. At age 65, there is a clear gap in separation

rates and retirement hazards prior to the bans, with rates in treated provinces roughly six percentage points (pp) higher. However, the downward trends are very similar. After the bans are announced, separation rates and retirement hazards fall in treated provinces and converge to the level observed in the control provinces. This convergence takes 2–3 years, which is expected given that (i) the announcement period for these reforms varied from 6 to 26 months and (ii) the full effects of bans implemented mid-year will not show up until the subsequent year.

3.3 Empirical strategy

We estimate stacked difference-in-difference (DiD) and triple-difference (DDD) models in line with the comparisons presented in Figure 1b. Our DiD models estimate the effects of the MR bans for specific age groups (61–64, 65 and 66–68). These models use the trends in job separation rates and retirement hazards among workers of the same age in control provinces to provide the counterfactual for the trends in the treated provinces. Our DDD models combine these three age groups and use the effects of the MR bans at age 61–64 as the reference category. That is, we allow for differential effects on separation rates and retirement hazards at ages 65 and 66–68.

We aim to estimate the average effect of the MR bans across all Canadian workers in the affected provinces over our sample period. Since the bans are staggered, utilizing standard two-way fixed effect models may lead to biased estimates if the treatment effects are heterogeneous over time or across provinces.[18] We thus adopt a stacking approach similar to Cengiz et al. (2019), which restricts identification to comparisons between treated and control provinces and is compatible with both binary and continuous treatments (fractional, in our case).

We start by estimating the following stacked DiD models:

$$y_{diapt} = \beta \text{Ban}_{pt} + \delta \text{Ann}_{pt} + \theta_{dap} + \gamma_{dat} + X_{pat} + \epsilon_{diapt} \tag{3.1}$$

where y is the outcome (e.g., a job separation), d indexes data set, i individual, a age in years (at the end of year t), p province of employment and t year. The sample is stacked with five identical observations for individuals in control provinces. This explains the index d, which is a categorical variable that links the control observations to a single treated province.[19]

[18] This potential bias results from previously treated units forming part of the control group for newly treated units (see Roth et al., 2023, for a review of this literature).

[19] That is, we effectively pool five data sets that each contain one treated province and all control provinces.

Within each data set, we include fixed effects for each age-province (θ_{dap}) and age-year (γ_{dat}) cell. These account respectively for any differences across provinces in the age patterns at which workers retire and any national age-specific trends in retirement behavior. The variable X_{pat} accounts for other time-varying policy differences between Quebec and the rest of Canada that may differentially affect retirement hazards by age and province.[20] ϵ_{diapt} is the error term.

The key variables of interest are Ban_{pt} and Ann_{pt}. Ban_{pt} is defined as the fraction of year t that MR is banned in province p, while Ann_{pt} is the fraction of the year that a ban has been announced but not yet taken effect. The inclusion of Ann_{pt} allows for an "expressive value of the law", where the law change itself may affect social norms or preferences about the enforcement of MR. The coefficients of interest are β, which identifies the effect of the bans after implementation, and δ, which identifies the effect between the announcement and implementation dates.

We estimate separate treatment effects for ages 61–64, 65 and 66–68. Given that MR could only be enforced from age 65, we do not expect any effect for ages 61–64. The DiD estimates for this age bracket serve as a test of any association between variations in MR bans and underlying retirement trends across provinces. The absence of an association would raise our confidence that the DiD estimates at ages 65 and 66–68 are likely to capture the causal effects of the bans on job separation rates and retirement hazards. At age 65, we expect the effect to be negative and larger than at ages 66–68, as 65 is the first age that MR is enforceable. We also anticipate a negative effect for ages 66–68, because (i) firms may value the option to enforce MR beyond age 65 (e.g., if workers' productivity declines or economic conditions worsen) and (ii) historically, collective agreements often stipulated MR ages higher than 65 (Gunderson, 1987). Given our reliance on provincial policy changes, we cluster standard errors by province (Bertrand et al., 2004). However, we have a small number of clusters overall (13) and even fewer treated clusters (5). This can lead to a significant over-rejection of hypothesis tests using the $t(K-1)$ distribution, where K is the number of clusters (MacKinnon and Webb, 2018). Therefore, our preferred approach for hypothesis testing employs the wild cluster-bootstrap method (Roodman et al.,

[20] X_{pat} includes controls for the slightly different policies in Quebec and the rest of Canada (ROC) since 2011. Specifically, we take into account (i) the maximum tax credit available to workers at age a in province p and year t under the Quebec tax credit for career extension; (ii) a dummy variable for the 2012–13 years interacted with an ROC dummy and age dummies, allowing for age-specific effects of the earlier removal of the work cessation test in the ROC; and (iii) the annual percentage increase in CPP/QPP benefits from delaying claiming by an additional year at age a in province p and year t. We also include a post-2010 dummy for Prince Edward Island and its interaction with a dummy for people over 65 to account for an increase in the stringency of its MR ban. Overall, the estimates are very similar with and without these controls.

2019), and we jackknife the bootstrap data-generating process as suggested by MacKinnon et al. (2023). Considering the small number of clusters, this method emerges as both more reliable and more conservative than the conventional approach of clustering by province and using the $t(K-1)$ distribution (MacKinnon and Webb, 2018).

After estimating the DiD models for various age groups around the age of 65, we pool all observations for individuals aged between 61–68. We estimate the following DDD models:

$$\begin{aligned} y_{diapt} =& \text{Ban}_{pt}\{\beta + \mathbf{1}(\text{age}_a = 65)\beta_{65} + \mathbf{1}(\text{age}_a \in [66,68])\beta_{66to68}\} + \\ & \text{Ann}_{pt}\{\delta + \mathbf{1}(\text{age}_a = 65)\delta_{65} + \mathbf{1}(\text{age}_a \in [66,68])\delta_{66to68}\} + \theta_{dap} + \gamma_{dat} + X_{pat} + \epsilon_{diapt} \end{aligned} \tag{3.2}$$

where the indexing, fixed effects, and control variables are the same as in DiD model (3.1). The key distinction from the DiD model is the inclusion of interaction terms for Ban_{pt} and Ann_{pt} with an age-65 dummy and an additional dummy for individuals aged 66–68 years old. Their coefficients are interpreted as the differential effects of MR bans during the post-implementation and announcement periods for the two age groups relative to the effects for 61–64-year-olds.

The DDD model has two potential advantages over its DiD counterpart. First, the estimates of the policy effects at ages 65 and 66–68 may be more robust, especially if there is an underlying association between the MR bans and province-year factors affecting retirement behavior. Second, the DDD estimates tend to be more precise.

3.4 Effects on separation rates and retirement hazards near age 65

Table 2 shows the estimated effects on job separation rates from our DiD models in columns 1–3 and DDD models in column 4. As expected, the DiD results show no effect of the bans on separation rates at ages 61–64. There are negative effects at ages 65 (-5.8 pp) and 66–68 (-0.8 pp). The former estimate is statistically significant at the 1% level based on both the cluster-robust and wild-bootstrap p-values. Our preferred DDD estimates are almost identical in magnitude (-5.9 pp and -1.0 pp) but more precise, with the effect at ages 66–68 becoming statistically significant at the 5% level. Overall, the MR bans mainly reduce job separations at age 65, although the delayed effects at age 66–68 augment the age-65 effect considerably.

Interestingly, the DDD estimates of the announcement effects are also negative at ages 65 and 66–68. At age 65, the estimate is -2.5 pp ($p = 0.049$), or 40% of the post-implementation effect. This may reflect a change in social norms regarding the enforcement of MR following the

Table 2: Effects of the mandatory retirement bans on job separation rates and retirement hazards

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Job separation rates				Retirement hazards			
	Differences-in-differences				Differences-in-differences			
	61–64	65	66–68	DDD	61–64	65	66–68	DDD
Mandatory retirement ban	-0.001	-0.058***	-0.008	0.000	-0.004	-0.060***	-0.014**	-0.003
	(0.005)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.004)	(0.006)
Cluster-robust p-value	0.880	0.000	0.069	0.954	0.560	0.000	0.004	0.666
Wild bootstrap p-value	0.948	0.000	0.239	0.976	0.705	0.009	0.046	0.744
Ban announced, not yet implemented	0.011	-0.015	0.006	0.011	-0.006	-0.019	-0.009	-0.006
	(0.011)	(0.010)	(0.006)	(0.011)	(0.007)	(0.010)	(0.006)	(0.007)
Cluster-robust p-value	0.364	0.169	0.334	0.368	0.409	0.086	0.150	0.403
Wild bootstrap p-value	0.445	0.264	0.507	0.446	0.558	0.139	0.296	0.546
MR Ban × age 65				-0.059***				-0.058***
				(0.004)				(0.006)
Cluster-robust p-value				0.000				0.000
Wild bootstrap p-value				0.000				0.000
Announcement × age 65				-0.025**				-0.013*
				(0.007)				(0.004)
Cluster-robust p-value				0.004				0.010
Wild bootstrap p-value				0.049				0.065
MR Ban × ages 66–68				-0.010**				-0.013**
				(0.002)				(0.003)
Cluster-robust p-value				0.000				0.000
Wild bootstrap p-value				0.030				0.038
Announcement × ages 66–68				-0.005				-0.003
				(0.007)				(0.002)
Cluster-robust p-value				0.464				0.227
Wild bootstrap p-value				0.588				0.417
Treated mean, post bans	0.161	0.262	0.202		0.102	0.213	0.158	
R-squared	0.005	0.007	0.003	0.013	0.003	0.006	0.002	0.018
Observations	25,679,960	4,223,050	7,439,200	37,342,210	25,679,960	4,223,050	7,439,200	37,342,210

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes: This table shows estimates of the effects of the mandatory retirement bans and ban announcements on the job separation rates (columns 1–4) and retirement hazards (columns 5–8) of older workers. Columns 1–3 and 5–7 show difference-in-difference estimates from equation (3.1) at different ages. Columns 4 and 8 show triple-difference estimates from equation (3.2) that combine the three age groups.

announcement of the bans and/or proactive changes to collective agreements.

In columns 5–8, we present the corresponding estimates for retirement hazards. The sign and magnitude of these estimates mirror those for separations. This indicates that essentially all of the workers who are dismissed under MR retire rather than finding a new job.

The estimated effects are substantial. Our estimates imply that the bans reduce the excess retirement hazard at age 65 (relative to the mean at ages 61–64) by 34%. Notably, this rate is nearly triple the estimate reported for France (Rabaté, 2019). This finding underscores the importance of MR as a cause of "bunching" at statutory retirement ages. If these bans had not occurred, there would be 52% more bunching at age 65 in treated provinces today.

3.5 Robustness checks

To assess the robustness of these results, we extend our DDD model to produce regression-adjusted versions of Figures 1a and 1b. To estimate the treatment effects by age, we modify equation (3.2) by replacing the $\mathbf{1}(\text{age}_a = 65)$ and $\mathbf{1}(\text{age}_a \in [66, 68])$ terms with indicators for each year of age from 61 to 68 (using 64 as the reference category). The β_k coefficients are shown in Figure 2a with 95% confidence intervals. There is no evidence of any differential effects of the bans at ages 61, 62 and 63 (relative to the effect at age 64). At age 65, the estimate is negative and highly statistically significant (and similar in magnitude to our estimates in Table 2). While there are negative effects at ages 66–68, these are weaker than at 65 and diminish with age.[21]

To estimate the treatment effects in event time, we use the following regressions:

$$y_{diapt} = \sum_{\substack{k=-6 \\ k\neq -1}}^{13} \mathbf{1}(\text{Tr}_{pt} = k) \left\{\beta_k + \beta_{k,65}\mathbf{1}(\text{age}_a = 65) + \beta_{k,66to68}\mathbf{1}(\text{age}_a \in [66, 68])\right\} + \theta_{dap} + \gamma_{dat} + X_{pat} + \epsilon_{diapt} \quad (3.3)$$

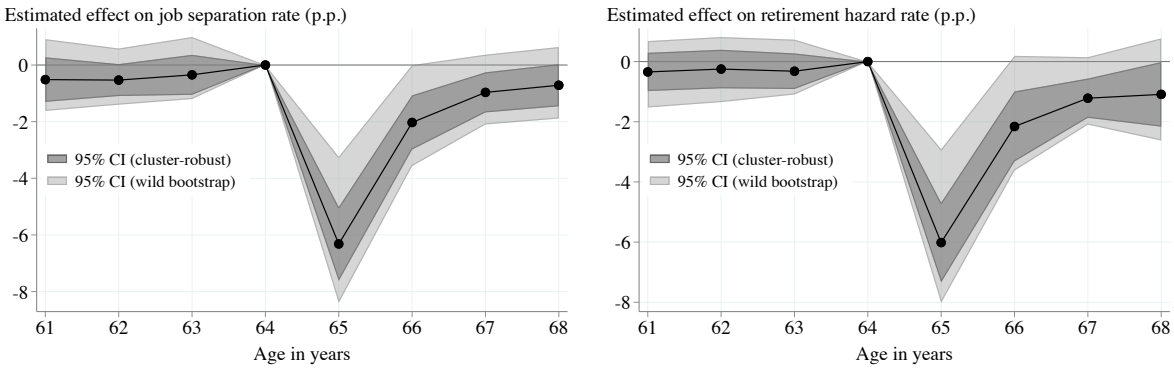
where $\mathbf{1}(\text{Tr}_{pt} = k)$ is a set of event-time dummies for the treatment group, equal to one if it is k years since a MR ban was announced. The baseline β_k coefficients estimate the effects in event time at ages 61–64 and the $\beta_{k,65}$ and $\beta_{k,66to68}$ allow for differential effects at ages 65 and 66–68. Figure 2b shows the estimated $\beta_{k,65}$ coefficients along with 95% confidence intervals for event-years -4 to 11 (the years that the sample is balanced across provinces). There is no evidence of

[21] Figure A1 plots the estimates of the δ_k coefficients, which show the effects at each age between the ban's announcement and implementation dates. The estimates show that there is no evidence of an announcement effect at ages 61–63 (relative to the effect at age 64), but there is a statistically significant reduction in separation rates at age 65. As mentioned before, the fact that the decline in separation rates occurs exclusively at age 65 is consistent with a change in social norms regarding MR enforcement.

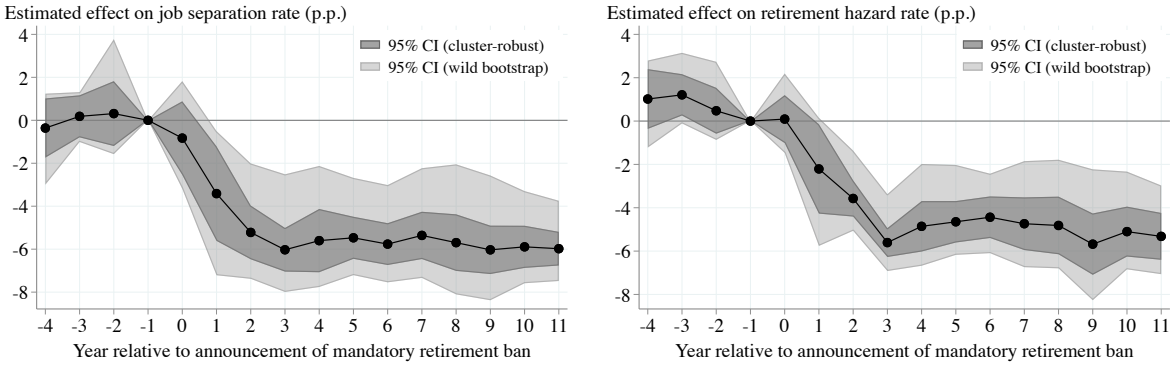
Figure 2: Estimated effects of mandatory retirement bans by age and in event time

Job separation rates | Retirement hazards

(a) Estimated effects by age (relative to effect at age 64)



(b) Estimated effects at age 65 in event time (relative to year before ban announced)



pre-trends and a clear and persistent decline in separation rates and retirement hazards at age 65 after MR is banned. It takes 2–3 years for the full effects to be observed.[22]

These estimates resolve two possible concerns with our identification strategy. The first is that our negative estimate of the ban announcement effect may simply reflect a pre-existing trend (i.e., if separation rates in treated and control provinces were converging prior to the policy). The event-time estimates resolve this concern as they show no evidence of any differential trend in separation rates for treated provinces prior to the announcement year. A second concern relates to the modest differences in retirement policies between Quebec and the rest of Canada from 2011 (although our regressions include controls for these policies, and these controls have little impact on the estimates). If these differences were important, we would expect the magnitude

[22]We observe a similar pattern in the estimates at ages 66–68, with smaller changes post bans (Figure A2).

of the estimates to vary between event-years 3 (2008–10 in treated provinces) and 11 (2016–18). However, the treatment effects remain remarkably consistent across these years.

We verify the robustness of our estimates in additional ways. First, we show that the estimates and confidence intervals are similar if we exclude provinces/territories from the sample one at a time (Figure A3). Second, we show that the estimates of our main effects are relatively similar for each treated province individually (Figure A4). Our stacked regressions produce a variance-weighted average of these effects (Dube et al., 2023), which gives more weight to the effects in populous provinces like Ontario (39% of Canada's population) and British Columbia (13%) than other treated provinces (7% combined). This is a deliberate choice, as we wish to understand the average effects of the MR bans over our sample period on affected workers. Third we use a randomization inference procedure among the control group to show that our estimates are larger than what we would expect to obtain by chance (Figure A5). Relative to the distribution of placebo effects, the post-implementation effect at age 65 is extreme, with a larger coefficient and t-statistic than any of the placebo effects. The post-implementation effect at age 66–68 and the announcement effect at age 65 are also relatively extreme, with either the point estimates or t-statistics implying p-values less than 0.05 for each outcome.

3.6 Effect heterogeneity

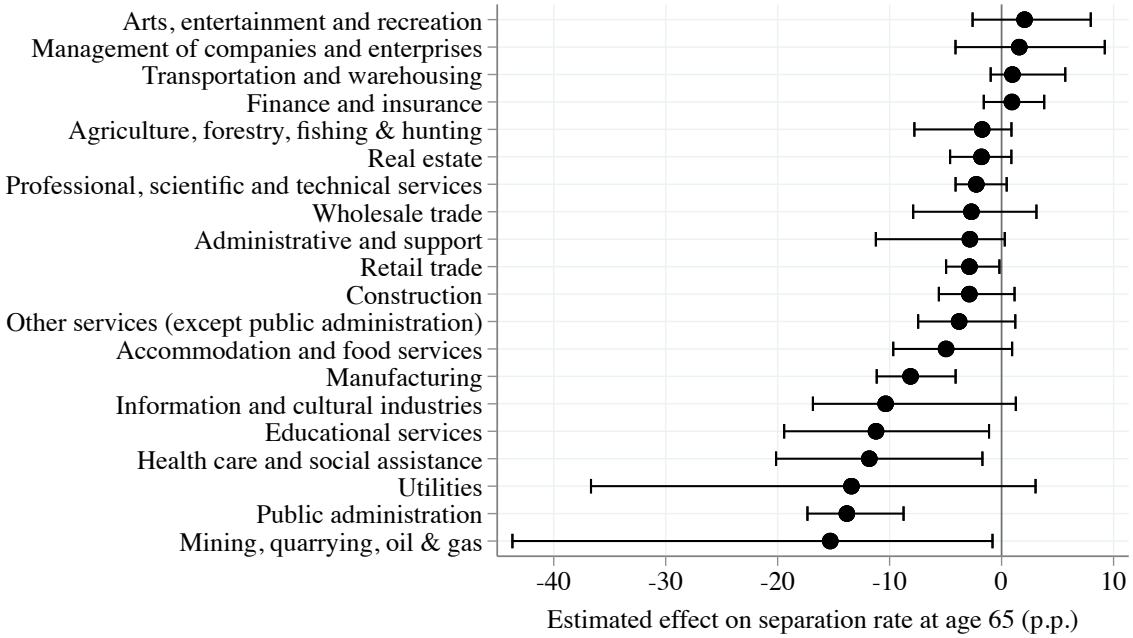
We assess heterogeneity in the post-implementation effects on separation rates at age 65 across industries, employers and based on worker characteristics.

Industry of employment. Figure 3a shows considerable effect heterogeneity across industries, with the largest effects in the manufacturing, public administration, health care and social assistance, educational services, information and cultural industries, utilities, and mining, quarrying, and oil and gas extraction. Workers in these seven "strongly affected industries" comprise 42% of the older workforce but account for 77% of the effects. The estimated average treatment effect for these workers (-10.7 pp) is five times larger than for workers in other industries. The estimates correlate strongly with industry-specific levels of unionization, employer-pension coverage, workplace size, median wages, job tenures, and injury risks (Figure A6).

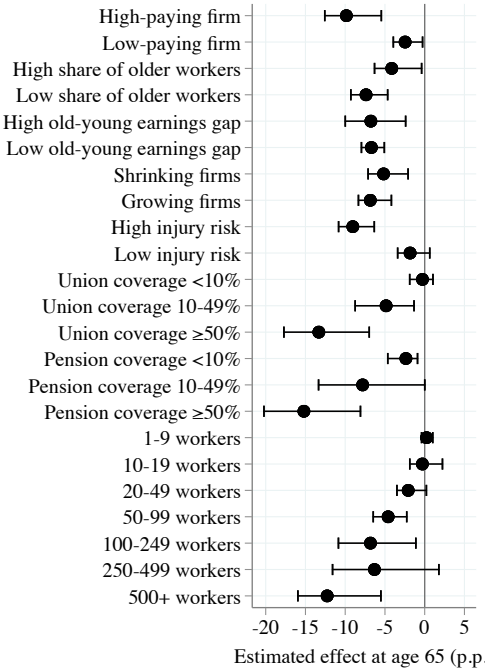
Employer characteristics. Figure 3b shows how the effects vary based on the characteristics of the employer. We observe larger effects among employers with a younger workforce, in

Figure 3: Heterogeneity in the effects on job separation rates at age 65

(a) Industry of employment



(b) Firm characteristics



(c) Worker characteristics



Notes: These figures show the estimated post-implementation effects of the mandatory retirement bans at age 65 for different subgroups based on industry of employment and firm/worker characteristics. We show our triple-difference estimates (equation 3.2) and 95% confidence intervals based on wild cluster-bootstrap inference.

terms of the share of workers above age 50. These employers may hold less favorable views about the productivity or cost of older workers, so it is perhaps not surprising that they are more likely to enforce MR. In terms of workplace safety, we observe much larger impacts among employers with above-median rates of workplace injuries (based on workers' compensation payments). This reiterates the physical nature of affected workers' jobs. For workforce size, unionization, and pension coverage, the results align with the correlations at the industry level; the effects are clearly increasing in workforce size and for employers that have high union and employer-pension coverage. Interestingly, we observe little difference in the effects for employers that are expanding or contracting, suggesting that firms do not use MR provisions to lay off workers when times are tough. Finally, we observe no correlation between the average earnings gap for older and younger workers, suggesting that, to the extent that MR laws allow employers to write deferred compensation contracts (Lazear, 1979), employers do so via pensions rather than salaries.

Worker characteristics. Figure 3c confirms that, at the worker level, we find strong positive associations between the size of the treatment effects and individual coverage by unions and employer-pension plans. The estimated effect on age-65 separation rates at age 65 is -14.8 pp for workers covered by a union (-16.3 pp if covered by an employer-pension plan) compared to -2.0 pp (-3.4 pp) for those who are not covered. Higher earners are also more strongly affected (e.g., the estimate is -10.0 pp for the top 25% of earners compared to -0.4 pp for the bottom 25%). There is suggestive evidence that the effects are larger for native-born Canadians than immigrants, while the effects do not appear to differ by gender.

3.7 General equilibrium effects on the hiring of older workers

By strengthening employment protections for older workers, firms may become more reluctant to hire them, potentially undermining the employment gains of the MR bans. Leveraging policy variation across provinces, our DiD regressions allow us to assess whether MR bans affect the annual hiring rates of older tax filers at particular ages (61–64, 65, and 66–68), while the DDD model tests for a differential effect at ages 65 and 66–68. Table 3 shows no evidence of any effect on hiring rates at any age with either model. The 95% confidence intervals for our DiD estimates at ages 61–64 allow us to rule out reductions in hiring rates larger than 1.1 pp

Table 3: Effect of the mandatory retirement bans on the hiring rates of older tax filers

	(1)	(2)	(3)	(4)
	Differences-in-differences			
	61-64	65	66–68	DDD
Mandatory retirement ban	-0.001	0.002	0.002	-0.002
	(0.004)	(0.002)	(0.002)	(0.004)
Cluster-robust p-value	0.818	0.506	0.364	0.701
Wild bootstrap p-value	0.901	0.639	0.514	0.855
MR Ban × age 65				0.002
				(0.003)
Cluster-robust p-value				0.483
Wild bootstrap p-value				0.923
MR Ban × ages 66–68				0.004
				(0.005)
Cluster-robust p-value				0.349
Wild bootstrap p-value				0.828
Treated mean, post bans	0.066	0.046	0.032	
R-squared	0.006	0.005	0.005	0.011
Observations	69,266,960	15,740,690	43,309,090	128,316,730

Notes: This table shows estimates of the effects of the mandatory retirement bans on the hiring rates of older tax filers. Columns 1–3 show difference-in-difference estimates at different ages from equations (3.1). Column 4 shows triple-difference estimates that pool the three age groups from equation (3.2). Standard errors in parentheses are clustered by province. Announcement effects (not shown) are not statistically significant.

(14%).[23] A one-sided test even rules out reductions in hiring larger than 13% (11%) with 95% (90%) confidence. Thus, there is little evidence of any meaningful drop in firms' demand for older workers after the bans.

3.8 Net effects on old-age employment and earnings

Given our findings of lower retirement hazards and unchanged hiring practices, the bans likely had positive effects on older workers' employment rates and earnings. We test this conjecture by estimating equations (3.1) and (3.2) for individuals who were employed at age 61. The estimates indicate that the MR bans have increased employment at ages 66–69 by 14% and earnings by 19–34% (Table 4).[24] There are no effects at ages 61–64.

[23]We continue to estimate null effects even if we only count hires from employers in the strongly affected industries or restrict the sample to people who were working at age 61.

[24]Our triple-difference estimates imply similar percentage changes in employment and earnings (11% and 22%) if we include tax filers who are not working at age 61 in our sample (Table A1). This is not surprising as these individuals are unlikely to work at ages 66–69 regardless of treatment status.

Table 4: Effect of the mandatory retirement bans on employment and earnings

	(1)	(2)	(3)	(4)
	Differences-in-differences			
	61-64	65	66–68	DDD
Panel A: Employed next year				
Mandatory retirement ban	0.007	0.052**	0.053**	0.005
	(0.008)	(0.010)	(0.009)	(0.008)
Cluster-robust p-value	0.381	0.000	0.000	0.521
Wild bootstrap p-value	0.576	0.040	0.049	0.676
MR Ban × age 65				0.045***
				(0.006)
Cluster-robust p-value				0.000
Wild bootstrap p-value				0.008
MR Ban × ages 66–68				0.051***
				(0.007)
Cluster-robust p-value				0.000
Wild bootstrap p-value				0.000
Treated mean, post bans	0.782	0.513	0.377	
R-squared	0.051	0.011	0.018	0.173
Observations	26,950,710	5,174,530	12,289,280	44,414,520
Panel B: Earnings next year (in 2019 $)				
Mandatory retirement ban	-1,253	3,489**	2,745*	-1,372
	(1,130)	(718)	(485)	(1,074)
Cluster-robust p-value	0.289	0.000	0.000	0.226
Wild bootstrap p-value	0.435	0.035	0.051	0.349
MR Ban × age 65				5,056***
				(577)
Cluster-robust p-value				0.000
Wild bootstrap p-value				0.000
MR Ban × ages 66–68				4,536**
				(711)
Cluster-robust p-value				0.000
Wild bootstrap p-value				0.026
Treated mean, post bans	41,353	24,195	16,658	
R-squared	0.051	0.025	0.021	0.096
Observations	26,950,710	5,174,530	12,289,280	44,414,520

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes: This table shows estimates of the effects of the mandatory retirement bans on the employment rates and total earnings of older people who had some labor market attachment at age 61. Columns 1–3 show difference-in-difference estimates at different ages from equations (3.1). Column 4 shows triple-difference estimates that pool the three age groups from equation (3.2). Only the post-implementation effects are shown (announcement effects are allowed for). As earnings are measured in year $t + 1$, we control for the effect of the bans on 64 year-olds (who are above 65 for part of $t + 1$).

Our estimates thus imply substantial increases in employment and earnings at ages 66–69. A 14% increase in employment means that the average worker (marginally below age 65) works 3.6 months (0.30 years) longer because of the MR bans. For earnings, the relative effects are even more pronounced, which is consistent with the larger treatment effects for higher earners (Figure 3c). Our more conservative DiD estimates suggest the same worker earns approximately $20,000 more overall due to the bans (in 2019 $).

How large are these effects in a broader context? One useful reference point for these estimates is to compare them to the impact of raising public pension ages on employment. In Appendix B, we project an 'upper bound' of the effects of a one-year increase in the claiming ages for OAS/GIS and full CPP/QPP benefits (from 65 to 66). We draw on Rabaté et al.'s (2024) insight that the amount of bunching in retirement hazards at a pension age strongly predicts the employment effects of raising that age. Using this approach, we estimate that the average worker approaching 65 would delay retirement by 1.7 months or 0.14 years.

Thus, an increase in the retirement age of 3.6 months would require an increase in pension ages of 2.1 years ($2.1 = 3.6/1.7$). These projections show that the estimated retirement delays we find are comparable to large policies targeting labor supply at the same ages. Moreover, the MR bans raise employment for different workers (since these workers choose to work beyond the pension age). This suggests that demand-side interventions can boost employment for older workers that are less responsive to increases in public pension claiming ages. Such interventions also provide an alternative way to boost aggregate old-age employment when, as in Canada's case, attempts to raise public pension ages have proved unsuccessful.[25]

3.9 Labor market spillovers on younger workers

From an overall welfare perspective, it is important to understand whether the increase in old-age employment had spillover effects on the labor market outcomes of younger age groups. While there is a growing understanding that negative spillovers on younger workers may arise within specific organizations (Ferrari et al., 2023; Bianchi et al., 2022; Boeri et al., 2021; Hernæs et al., 2023; Hut, forthcoming), generalizing these findings to aggregate labor market impacts would risk suffering from the *lump of labor* fallacy (i.e., the idea that an economy holds a fixed number

[25] In 2012, the Canadian government legislated an increase in the eligibility age for OAS/GIS, with the age set to rise from 65 to 67 between 2023 and 2029. The policy was repealed in 2016 following a change in government.

of jobs). To examine the aggregate impacts at the province level, we apply our DiD models from equation (3.1) to assess the average effects of the MR bans on four outcomes — annual employment, earnings, hiring rates and separation rates — for tax filers aged 20–34, 35–49 and 50–64. For each age group, we find no evidence that any of the four labor market outcomes are affected (Appendix Table A8).[26] These null effects are consistent with the majority of studies examining the equilibrium consequences of old-age employment on younger workers (Gruber et al., 2009; Munnell and Wu, 2013; Börsch-Supan and Schnabel, 2010; Mohnen, forthcoming).

4 Spillovers on self-insurance behaviors, health and spouses

In this section, we examine various spillover effects of the bans on older workers and their households. These effects are not only informative about the overall effects of the bans but also on the mechanisms through which they impact older workers' welfare and public finances. Section 4.1 examines various self-insurance behaviors by older workers, including unemployment insurance claims, transitions to self-employment, and saving responses.[27] Section 4.2 sheds light on health effects.[28] Section 4.3 investigates the effects on spousal retirement timing, which is informative about spousal-insurance mechanisms and preferences for joint retirement Throughout Section 4, we focus our discussion on the estimates of the post-implementation effects from our main DDD model (equation 3.2) for the full sample and for workers in strongly affected industries.[29]

4.1 Self-insurance mechanisms

Unemployment insurance claims. Our results suggest the MR bans caused similar reductions in separation rates and retirement hazards, implying that none of the workers who are fired at age 65 find a new job. This result could reflect either a lack of job search by

[26]The findings are qualitatively similar if we look at employment or earnings within the strongly affected industries where MR was most common.

[27]Utilizing the same sample as our main analysis of job separations and retirement hazards (people working in year t aged 61–68), we define a series of binary outcomes. These outcomes measure whether an individual has income from a new source (i.e., in year t but not in $t-1$). Specifically, we consider income from unemployment insurance, self-employment, employer pensions, OAS, GIS, CPP/QPP and RRSP withdrawals. We also measure the net income from each source in year $t+1$ in 2019 $. Furthermore, we define two binary measures of 'final' contributions to employer-pension plans and RRSPs (contribution in year t but not $t+1$). These outcomes measure passive or active decisions to stop making contributions for one's retirement.

[28]We study two health outcomes: new workers' compensation payments (which provide a proxy for workplace injuries) and all-cause mortality. For both outcomes, our sample is broadened to include (about 1% of) workers in year t who die in years t or $t+1$.

[29]Appendix C.1 presents estimates of our event-time model (equation 3.3). These estimates show no evidence of pre-trends and post-ban dynamics that are consistent with the main estimates.

terminated workers or low demand for older workers (Neumark et al., 2019). To shed light on the relative importance of these mechanisms, we estimate the effects of the MR bans on new unemployment insurance (UI) claims, which require individuals to actively look for work.

For the full sample, we estimate that the bans decrease new UI claims among individuals who are still working in the year they turn 65 by 0.9 pp (Table 5, $p = 0.010$). Narrowing the sample to workers in industries with the largest effects on separation rates ("strongly affected industries"), the estimated effect increases to 1.7 pp ($p = 0.008$). This near-doubling of the effects is proportional to the change in the estimates for separation rates when we move from the full sample (-5.9 pp) to "strongly affected industries" (-10.7 pp). Overall, the estimates suggest that around 16% of workers who are fired at age 65 claim UI. These workers appear to have little success finding new jobs, given the absence of any effect on hiring rates at age 65.

Our results indicate an apparent contradiction in older workers' labor supply decisions: MR bans have strong effects on job separation rates at age 65, indicating that older workers are keen to work beyond age 65, but much smaller effects on the number of workers looking for or finding new jobs. This set of results suggests that older workers place a high value on workplace continuity (e.g., due to friendships at work or firm-specific human capital) and/or perceive a lack of demand from other firms. Other potentially important factors that may explain these results include the ability of individuals to self-insure via pension claiming and saving responses as well as policy-induced norms about the "appropriate" age to retire (Behaghel and Blau, 2012; Cribb et al., 2016; Seibold, 2021; Deshpande et al., 2021; Glenzer et al., 2025).

Transitions into self employment. For older workers who lose their job, those who do not wish to retire may elect to start a business. However, unlike UI claims, which normally occur soon after job loss, business creation may take more time. Thus, we study the effects of the MR bans at age 65 on both contemporaneous transitions into self-employment (i.e., in the same year) and transitions in the following year (i.e., in the year the individual turns 66).

Our DDD results imply that individuals who are still working in the year they turn 65 are less likely to become self-employed because of the bans (Table 5). The estimates imply similar reductions at ages 65 and 66 (-0.17 and -0.16 pp), but the estimate at age 66 is more precise ($p = 0.043$). Again, we observe larger estimates if we restrict the sample to workers in strongly affected industries (with statistical significance at 5% and 1% levels). Overall, our estimates

Table 5: Spillover effects of the bans at age 65 on other outcomes

	(1)	(2)	(3)
	Regression estimates		
	Full sample	Strongly affected industries	Mean at 65 (full sample)
Dependent variable			
Separates from job	-0.059***	-0.107***	0.262
	[0.000]	[0.000]	
Retires from paid work	-0.058***	-0.104***	
	[0.000]	[0.000]	0.213
New UI claim	-0.009***	-0.017***	0.043
	[0.010]	[0.008]	
Becomes self-employed	-0.0017	-0.0033**	0.016
	[0.247]	[0.028]	
Becomes self-employed $(t+1)$	-0.0016**	-0.0045***	0.017
	[0.043]	[0.004]	
Final employer-pension contribution	-0.049***	-0.097***	0.078
	[0.002]	[0.008]	
New employer-pension income	-0.053***	-0.098***	0.156
	[0.000]	[0.000]	
New OAS claim	-0.021	-0.028	0.733
	[0.579]	[0.507]	
New GIS claim	0.017	0.011	0.057
	[0.314]	[0.438]	
New CPP/QPP claim	-0.026	-0.034**	0.344
	[0.144]	[0.045]	
Final RRSP contribution	-0.019***	-0.034**	0.110
	[0.010]	[0.010]	
New RRSP withdrawal	-0.007**	-0.011**	0.060
	[0.018]	[0.033]	
New RRSP withdrawal $(t+1)$	-0.011***	-0.016***	0.067
	[0.000]	[0.000]	
New workers' compensation payment $(t+1)$	0.0026***	0.0034***	0.007
	[0.000]	[0.000]	
All-cause mortality	-0.00031	-0.00001	0.004
	[0.178]	[0.951]	
All-cause mortality $(t+1)$	-0.00017	0.00035	0.005
	[0.772]	[0.653]	

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values, which are shown in square brackets.

Notes: This table shows triple-difference estimates of the spillover effects of the bans at age 65 on various outcomes. We focus on the post-implementation effects. Each estimate is from a separate regression. The number of observations ranges from 35.7–37.0 million in column 1 and 15.1–15.6 million in column 2, depending on whether the outcome is measured in year t or $t+1$. For outcomes measured in year $t+1$, we control for the effect of the bans on 64 year-olds (who are above 65 for part of year $t+1$). Column 2 shows the estimates when we restrict the sample to workers in the "strongly affected industries" .

suggest that 6% of terminated workers transition into self-employment within one year.

Pension claiming and savings behavior. Despite the importance of retirement timing in life-cycle models, there is scant evidence about the effects of changes in retirement timing on pension claiming and savings decisions. We study a novel policy that exogenously shifted retirement timing without changing incentives for pension claiming or retirement savings.[30] This allows us to study how retirement influences a wide range of active and passive savings decisions (Chetty et al., 2014). Contributions to an employer-pension plan and the decision to start claiming benefits are largely passive savings decisions, tied directly to employment status. Other one-off decisions like claiming OAS/GIS are somewhat automatic as they are contingent upon reaching age 65 and individual/household income.[31] In contrast, the decision to claim CPP/QPP benefits is an active choice, since claiming is possible from age 60 to 70, and there is considerable heterogeneity in when individuals claim (Glenzer et al., 2025). Finally, the most active choices we consider are RRSP contributions and withdrawals, which are annual decisions.

We find strong effects on passive savings decisions that are strongly connected to employment (Table 5). At age 65, the MR bans reduce the chance of making a final contribution to an employer-pension plan by 4.9 pp ($p < 0.01$) and reduce the chance of claiming benefits by 5.3 pp ($p < 0.001$). These large effects provide further evidence that the effects of the bans are concentrated among the minority of workers who are covered by employer-pension plans.

There are no clear effects on OAS or GIS receipt. Our point estimates indicate that the bans reduce OAS claiming at age 65 by 2.1 pp, which is consistent with some high-income earners becoming ineligible due to the bans. However, the effect is imprecisely estimated. For GIS, there is little evidence of any effects at age 65. The absence of a significant change is consistent with the larger impacts of the bans on high-income workers with employer-pension plans. These workers are unlikely to satisfy the means test for GIS benefits in retirement.

For CPP/QPP benefits, we estimate a negative effect on claiming at age 65 of -2.6 pp ($p = 0.144$) among all workers, and an even more negative and statistically significant effect for workers in strongly affected industries (-3.4 pp, $p = 0.045$). These results suggest that around

[30] Other recent studies, which mostly use survey data, rely on pension reforms for identification (Lindeboom and Montizaan, 2020; Etgeton et al., 2023; García-Miralles and Leganza, 2024*b*; Nakazawa, 2022).

[31] OAS claiming could not be deferred until 2013, several years after the last MR ban. Thus, our estimates on OAS claiming likely reflect changes in eligibility from the income test rather than active decisions.

45% of individuals in our sample wait to start claiming CPP/QPP benefits until they retire.

Our estimates indicate that individuals save into and dis-save from RRSPs in the expected way. At age 65, the bans decrease the likelihood that workers make a final RRSP contribution by 1.9 pp ($p = 0.010$), while new withdrawals decrease by 0.7 pp in the same year ($p = 0.018$) and 1.1 pp in the following year ($p < 0.001$). These effects intensify for workers in strongly affected industries. Overall, these results suggest that individuals stop contributing to their RRSPs when they are forced to retire — as their disposable income and marginal tax rates drop — and begin withdrawals. While this behavior is consistent with optimal savings behavior, the magnitude of these responses is only one-third as large as the effects on retirement hazards, which suggests that most of the affected workers are "passive savers"(Chetty et al., 2014).[32]

Overall effects on disposable income. The role of the savings adjustments described above as a form of insurance against involuntary retirement can be highlighted by quantifying and contrasting the effects of the bans on earnings and disposable income. Disposable income includes earnings, public and private pension income, UI benefits and net RRSP withdrawals (withdrawals minus contributions) and subtracts income taxes and contributions to public and private pensions. Our estimates imply that, for individuals who are still working in the year they turn 65, disposable income at age 66 increases by $3,503 (Table A3), representing only 54% of the increase in their earnings. This demonstrates that individuals are able to significantly cushion the transition into retirement, ensuring a softer decline in income.

We can also decompose the change in disposable income into different sources. Table A3 shows positive effects on disposable income from earnings ($6,459 per annum) and negative effects on disposable income due to reductions in UI benefits (-$184), net income from employer-pension plans (-$959), net CPP/QPP benefits (-$467), net RRSP withdrawals (-$799) and higher income taxes ($1,056). Crucially, private pension income and active saving responses via RRSPs are particularly important in mitigating the negative effects of involuntary retirements on disposable income. These results suggest that the welfare losses stemming from involuntary retirement

[32] We examine potential anticipatory saving responses at ages 61–64 using our DiD strategy (Table A2) and find no evidence that treated workers start saving less at ages 61–64. If anything, the estimates suggest that workers are less likely to stop contributing to employer-pension plans and RRSPs and less likely to make withdrawals, although none of the estimates are statistically significant at the 5% level. There is also no evidence of any effect on CPP claiming. Collectively, these results suggest that anticipatory saving responses are small relative to the savings adjustments at ages where employment is affected. This result is consistent with findings based on Danish register data (García-Miralles and Leganza, 2024*b*), showing that individuals mainly respond to a pre-announced delay in pension eligibility by saving more at ages where employment is affected.

are likely to be considerably larger for passive savers, and in settings that affect workers who are either not covered by an employer-pension plan or are below pension-eligibility ages.

4.2 Health impacts

Workplace injuries. Our results in Section 3.6 showed stronger effects of the MR bans on workers with high employer-specific injury risks. Thus, it is likely that the increased the number of workplace accidents and workers' compensation claims.[33] Such effects are an unexplored but potentially important mechanism for explaining the effects of retirement on health and public finances. We focus on new workers' compensation payments in the year ahead so that we can estimate the effects in the first full year when the individual has reached age 65.[34]

Estimates from our DDD model show that the MR bans increase new workers' compensation payments at age 66 by 0.26 pp ($p < 0.001$) for those who were employed in the previous year (Table 5). This effect is 22% of the mean rate for workers aged 65 (1.2%). This increase is considerably larger than the relative increase in employment, consistent with the higher underlying injury risks for affected workers (Figure 3c). Once again, the estimates are larger for workers in strongly affected industries (0.34 pp, $p < 0.001$).

Overall, our estimates imply an annual workplace injury rate of 3.7% for workers affected by the MR bans, surpassing both the average rate of all workers in Canada (1.68% in 2019) and all workers aged 65 (1.2%). Given that our results imply that the bans have caused a total increase in employment at ages 66–69 of 207,000 person-years over our sample period, a 3.7% injury rate translates to an additional 7,600 workplace injuries.[35] In 2019, the average system cost of a claim across Canada was $36,818, which includes benefit payouts (around 80% of the

[33]Workers' compensation schemes are managed by provincial boards. While coverage varies slightly across provinces, the overwhelming majority of workers are covered in all provinces for a loss of wages due to a work-related injury (or exposure to a noxious substance).

[34]This definition means that our measure will not detect any new claims following the first claim in the same year or new claims following a claim in the previous year. In an appendix of an earlier version of this paper (Morris and Dostie, 2023), we found a similar increase in claim rates among individuals aged 65–68 with aggregate administrative data on the number of awarded claims at the age-province-year level.

[35]Our tax data lacks specific details about the type or source of the injury. Aggregate administrative data on awarded claims in 2019 indicates that 86% of claims relate to 'traumatic injuries and disorders', with 'systemic diseases and disorders' the next highest category (7%). The most common injuries are in the back/spine (21% of claims). In 2019, there were 0.3 accepted fatalities for every 100 claims among the entire population but 4.8 per 100 for individuals aged 65 and older. The much higher fatality-claim ratio at older ages partly reflects the lag between claims and fatalities (e.g., the most common cause of workplace-related death in Canada is asbestos exposure) but suggests that workplace fatality risk may be non-negligible for injured older workers.

total cost) and administrative costs (approximately 20%).[36]

Mortality. We use our DDD model to consider contemporaneous effects of the bans on all-cause mortality (i.e., in the year a worker reaches age 65) and effects in the following year.[37] Our point estimates are close to zero and far from conventional significance thresholds in both years (as detailed in Table 5). We find a similar absence of impact if we restrict the sample to workers in strongly affected industries. Moreover, our 90% (95%) confidence intervals rule out large short-term effects on mortality: changes more extreme than -17% to 2% (-18% to 7%) in the same year and -28% to 19% (-31% to 22%) in the following year.

4.3 Retirement transitions of spouses

Finally, we test for intra-household spillovers in terms of spouses' retirement transitions. The theoretical predictions are ambiguous as they depend on the relative importance of joint retirement preferences and added worker effects. We build a sample of married individuals from our primary sample ("reference individuals"), and then measure retirement outcomes for themselves and their spouses. We only include individuals if the spouse is younger than age 65, effectively ruling out any direct effects of the MR bans on the spouse. Thus, spouses are typically younger than reference individuals and, as such, around three-quarters are female.

Transitions into retirement are much more common than transitions out of retirement and thus are likely to be the main adjustment margin for spouses. Accordingly, we restrict the sample of couples to those where the spouse is also working in year t (around two-thirds of couples).[38] We then estimate the effects on the retirement hazards of partnered individuals and their spouses using our DDD model, adding spouse age-by-sex fixed effects.

Table 6 shows the estimates. We present the post-implementation effects of the bans at ages 65 and 66–68. The estimates for the reference individual closely align with those in Table 2: they are slightly smaller at age 65 (-5.0 pp, $p < 0.001$) and slightly larger at ages 66–68 (-1.5 pp,

[36]Source: Association of Workers' Compensation Boards of Canada, *Detailed Key Statistical Measures* for 2019 (`https://awcbc.org/en/statistics/ksm-annual-report/` [Accessed August 14, 2023]).

[37]Our estimates relate to a growing literature on the mortality effects of pension income and claiming ages (Snyder and Evans, 2006; Hernaes et al., 2013; Hallberg et al., 2015; Bloemen et al., 2017; Fitzpatrick and Moore, 2018; Hagen, 2018; Nielsen, 2019; Kuhn et al., 2020; Bozio et al., 2021; Bellés-Obrero et al., 2022). These studies have yielded a variety of findings across policies and subgroups.

[38]We do not find any economically or statistically significant effect on exit rates from retirement among non-working spouses (Table A4).

$p = 0.009$). We calculate a weighted average of these effects (-2.96 pp, $p < 0.001$) to improve precision and enable comparisons of the size of the direct and indirect effects. For spouses, the estimates are also negative, both when the reference worker is aged 65 (-0.51 pp, $p = 0.204$) and 66–68 (-0.77 pp, $p = 0.044$). The weighted average of these effects is -0.66 pp ($p = 0.019$). This implies an average 'scaled spillover effect' (García-Miralles and Leganza, 2024*a*) of 22%.

Table 6: Spillover effects of the bans on the retirement hazards of spouses

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Strongly affected industries		
	Age 65 effect	Age 66–68 effect	Weighted average	Age 65 effect	Age 66–68 effect	Weighted average
Panel A: All spouses aged < 65						
Reference individual	-0.0499***	-0.0146***	-0.0296***	-0.0984***	-0.0390***	-0.0658***
	(0.0045)	(0.0014)	(0.0022)	(0.0078)	(0.0038)	(0.0046)
Cluster-robust p-value	0.000	0.000	0.000	0.000	0.000	0.000
Wild bootstrap p-value	0.000	0.009	0.000	0.000	0.005	0.000
Spouse	-0.0051	-0.0078**	-0.0066**	-0.0092	-0.0106	-0.0100*
	(0.0023)	(0.0012)	(0.0010)	(0.0040)	(0.0033)	(0.0028)
Cluster-robust p-value	0.048	0.000	0.000	0.040	0.007	0.004
Wild bootstrap p-value	0.204	0.044	0.019	0.151	0.113	0.058
Observations		12,305,540			4,960,640	

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes: This table shows triple-difference estimates of the post-implementation effects of the bans at ages 65 and 66–68 on the retirement hazards of older workers and their spouse (from a version of equation 3.2 that adds spousal age-by-sex fixed effects). Estimates for reference individuals and spouses come from separate regressions. To increase power and facilitate comparisons of the direct and indirect effects, we calculate a weighted average of the estimates at ages 65 and 66–68 (with weights based on the share of reference individuals aged 65 and 66–68 in the sample). In columns 4–6, we restrict the sample to workers in the seven *strongly affected industries*.

We also examine how the spillovers vary with the spouse's age and the industry of the reference individual. In general, the point estimates are larger for both partners if the reference individual is employed in a strongly affected industry. In Table A5, we also estimate the effects separately for spouses aged 60–64, who are eligible for some form of public retirement benefits (from the CPP/QPP), and those who are younger than 60 (who are not yet eligible). While the estimated effects on reference individuals are similar in both samples, the spillover effects are driven by spouses who are aged 60–64, with scaled spillover effects of 39% for spouses aged 60–64 and 8% for spillovers on spouses under 60. These patterns are consistent with García-Miralles and Leganza's (2024*a*) analysis of joint retirement in Denmark, which highlight the importance

of small age differences and the spouse's own eligibility for retirement benefits.[39]

Remarkably, our estimates of the scaled spillover effects are at least as large as other estimates exploiting variation in incentives to retire *voluntarily*. The large spillover effects are surprising given that our estimates are identified from changes in *involuntary* retirement, which are likely less predictable and desirable. However, the fact that the effects typically occur at age 65 and are concentrated within certain firms suggest that they can be anticipated. Furthermore, the ability of individuals to mitigate their income losses upon retirement via saving and pension claiming responses may explain why *added worker effects* may be small in our setting.

Three other factors help explain the large spillover effects on spouses. First, most spouses in our sample are female, and the literature suggests that female spouses are more responsive to their partner's retirement timing.[40] Second, our estimates may detect a larger fraction of retirement spillovers than estimates that are 'local' around a partner's pension-eligibility age (Lalive and Parrotta, 2017; García-Miralles and Leganza, 2024*a*), since partners' retirement transitions are likely to be interdependent even if they are not simultaneous. Third, the MR bans affect a higher-income set of workers than the "compliers" in other studies who retire at pension-eligibility ages; high-income couples are less likely to be liquidity-constrained and hence may be more able to co-ordinate their retirements.

5 Effects on firm outcomes

We now assess the effects of the bans on firms' financial outcomes and employment stocks and flows (hiring and separations) for workers of different age groups (20–34, 35–49, 50–64, and 66–69). The financial outcomes include gross profits, revenue, sales from goods and services, value added, intermediate inputs, payroll expenses, total assets, and the total cost of sales.[41] We use value added per worker to measure productivity (Daruich et al., 2023; Risch, 2024).

We construct a balanced panel of firms in treated and control provinces that have at least five workers in each year and non-missing financial information over an 11-year period around the

[39] We also examined dynamic effects (i.e., effects on spouses' probability of retiring in the following year). Again, the estimates are negative (Table A6), and largest when the reference individual is in a strongly affected industry and the spouse is eligible for retirement benefits (age 60–64 in year $t+1$). These estimates provide further evidence that the MR bans have spillover effects on spouses that increase the overall employment effects.

[40] Our results are consistent with this empirical pattern: we estimate a scaled spillover effect of 26% for female spouses and 6% for male spouses (Table A7).

[41] We winsorize all outcomes at the 99$^{\text{th}}$ percentile and those with negative values at the 1$^{\text{st}}$ percentile, too.

bans: from event-years -4 to +6 relative to the policy announcement year. Importantly, we do not find any evidence that the bans affected firm survival (Appendix D.2). To maximize statistical power (i.e., the strength of the first-stage relationship between the bans and employment at ages 66–69), we restrict our sample to firms in the strongly affected industries. We also restrict our sample to firms that operate in a single province, which ensures that we can assign firms to either a treated or control province. The focus on financial outcomes covered by the tax data means that we cannot quantify the performance of public sector organizations or non-profits.

We examine the effects on treated firms that are highly exposed to the policy using a triple-differences (DDD) model, where moderately exposed firms in the same province provide an additional control group. We define highly exposed firms as those that are above the 80^{th} percentile in the sample for either total employer-pension plan contributions or total union contributions in the year before the ban is announced. Moderately exposed firms are defined as those that are below the 80^{th} percentile for both but have at least one worker covered by a union and at least one covered by an employer-pension plan. This approach — comparing highly and moderately exposed firms — is similar to Saez et al.'s (2019) DiD analysis of the firm-level consequences of a Swedish payroll tax cut, although we also leverage variation in ban timing across provinces. We assign exposure using firm-level information on unionization and pension coverage because it strongly predicts the effects of the bans on separations at age 65 and leads to a compelling 'first-stage' impact on firms' employment of workers aged 66–69.

Like Saez et al. (2019), we also estimate an alternative DDD model with a more stringent definition of highly exposed firms ("very highly exposed"), whom we compare to the moderately exposed. Very highly exposed firms are a subset of the highly exposed who are above the 90^{th} percentile for employer-pension plan contributions or union contributions. This approach should yield a stronger signal, but we expect a tradeoff in terms of precision.

We summarize our firm-level variables for highly exposed and moderately exposed firms in treated and control provinces in Table D1. In total, we have approximately 105,000 unique firm-year observations on nearly 9,000 firms. The majority of firms in the sample come from the two largest industries in Canada in terms of total employment: (i) health care and social assistance and (ii) manufacturing. Approximately one-quarter of firms are highly exposed, with the remainder classified as moderately exposed. Highly exposed firms (38 workers) are slightly larger than moderately exposed firms (26 workers). We observe only modest differences between

firms in treated and control provinces: firms in treated provinces are slightly larger and more profitable, a pattern that is present for both the 'highly exposed' and 'moderately exposed'. Workers aged 66–69 represent approximately 1% of total employment for firms in our sample.

To assess the effects of the bans on highly exposed (and very highly exposed) firms in treated provinces, we estimate the following stacked DDD models:

$$y_{dfept} = \beta \text{ Ban}_{pt} \cdot \mathbf{1}(\text{H}_{dfe} = 1) + \delta \text{ Ann}_{pt} \cdot \mathbf{1}(\text{H}_{dfe} = 1) + \theta_{dep} + \gamma_{det} + \pi_{dpt} + X_{df} + \epsilon_{dfept} \tag{5.1}$$

where y_{dfept} represents an outcome for firm f operating in province p in year t, e stands for exposure (differentiating firms with high or moderate exposure) with associated dummy variable $\mathbf{1}(\text{H}_{dfe} = 1)$ for firms that are highly exposed, and d denotes dataset (linking firms in control regions to a single treated province). Within each stacked dataset d, we control for province by exposure fixed effects, exposure by year fixed effects and province by year fixed effects. We also control via X_{df} for the number of workers at the firm in event-time -1, which helps improve precision. Like in equation (3.1), the key regressors are Ban_{pt} and Ann_{pt}, which respectively denote the share of year t that MR is banned in year t and the share a ban has been announced but not yet implemented. Here, these terms are interacted with $\mathbf{1}(\text{H}_{dfe} = 1)$. Thus, our main coefficient of interest, β, estimates the difference in the effects of MR bans during the post-implementation period for highly and moderately exposed firms.[42] We cluster standard errors by firm, which is the level at which policy exposure varies.

To assess the validity of our identification strategy and dynamics, we also estimate treatment effects in event time using the following regressions:

$$y_{dfept} = \sum_{\substack{k=-6 \\ k\neq -1}}^{8} \beta_k \ \mathbf{1}(\text{Tr_event_year}_{pt} = k) \cdot \mathbf{1}(\text{H}_{dfe} = 1) + \theta_{dep} + \gamma_{det} + \pi_{dpt} + X_{df} + \epsilon_{dfept} \tag{5.2}$$

where $\mathbf{1}(\text{Tr_event_year}_{pt} = k)$ is a set of event-time dummies for firms in treated provinces, equal to one if it is k years since a law banning MR was announced.

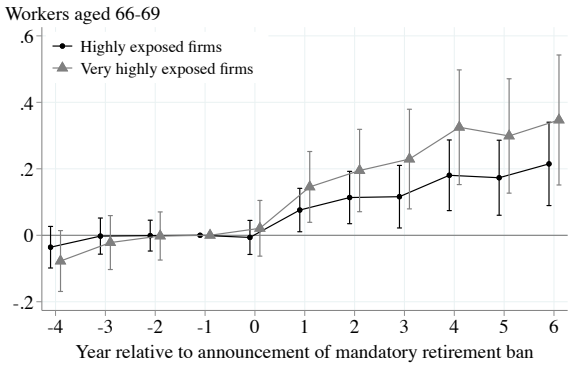
We present the estimates of β from equation (5.1) in Table 7 and the β_k estimates from equation (5.2) in Figures 4, D2 and D3. Both models show a strong relationship between the MR bans and employment at ages 66–69. The event-time estimates, which are consistently

[42]The baseline impact on moderately exposed firms is absorbed by the province by time fixed effects. We do not find any evidence of announcement effects for any of our firm-level outcomes, consistent with the gradual increase in employment at ages 66–69 within firms. Estimates are available upon request.

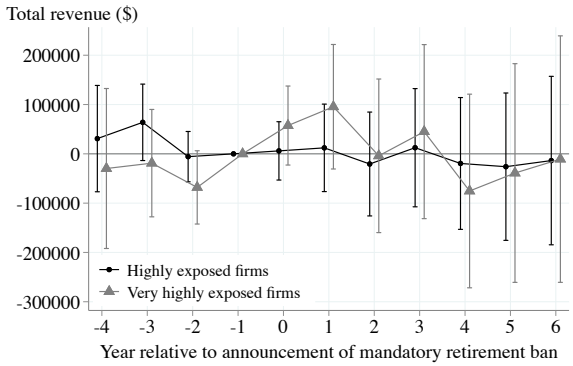
close to zero before the bans are announced (Figure 4a), gradually increase and then stabilize as older workers are increasingly retained beyond 65. As expected, this pattern is stronger for 'very highly exposed' firms. On average, our DDD estimates imply an increase in the number of 66–69-year-old workers by 0.17 for highly exposed firms and 0.29 for very highly exposed firms (both $p < 0.01$). These effects amount to a 27–33% increase in age 66–69 employment.

Figure 4: Event-time effects of mandatory retirement bans on outcomes for highly exposed firms

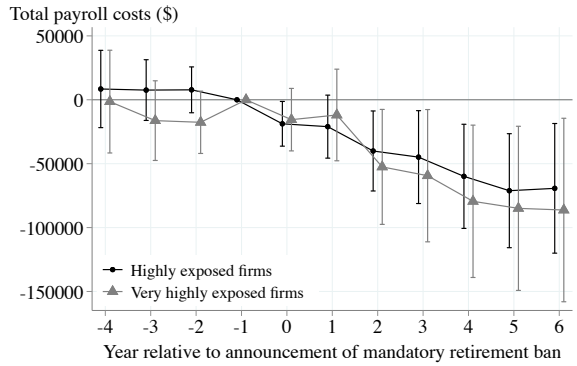
(a) No. of workers aged 66–69 (first stage)



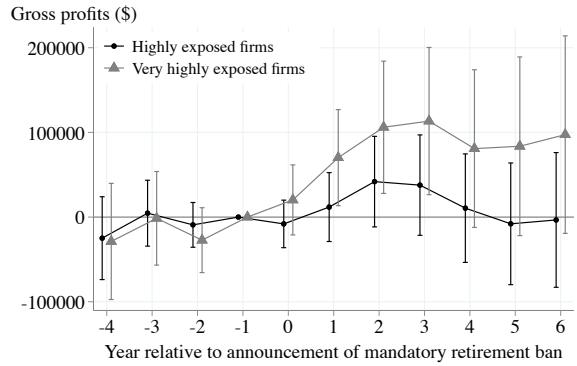
(b) Value added per worker **correct graph**



(c) Total payroll expenses



(d) Gross profits



Notes: These figures show the estimates and 95% confidence intervals from equation (5.2) for a limited set of firm outcomes. Equivalent estimates for the other outcomes are presented in Appendix Figures D2 and D3.

Regarding firms' financial outcomes, we observe little evidence of pre-trends for any of our outcomes, which supports our parallel-trends assumptions. After the bans are announced, we observe no evidence of a change in workforce productivity, in terms of value added per worker (Figure 4b), while a decline in total payroll expenses emerges for both highly exposed and very highly exposed firms (Figure 4c), equal to around $60,000 per annum or a 5–6% reduction. This

Table 7: Effects of mandatory retirement bans on highly exposed firms' financial outcomes

	(1) No. workers age 66–69	(2) Gross profits	(3) Total revenue	(4) Value added	(5) Intermediate inputs	(6) Payroll expenses	(7) Total assets	(8) Sales of goods & services	(9) Total cost of sales
Panel A: Highly exposed vs moderately exposed (DDD model 1)									
Ban × High	0.166***	23,305	-34,962	-65,533	30,571	-60,151***	16,590	21,558	4,049
	(0.041)	(31,279)	(69,333)	(41,946)	(41,198)	(20,936)	(52,565)	(67,704)	(49,252)
Mean	0.622	1,339,461	2,981,558	1,629,710	1,351,848	1,013,736	1,950,124	2,756,714	1,331,598
R-squared	0.099	0.292	0.374	0.362	0.286	0.485	0.277	0.356	0.293
Observations	247,335	247,335	247,335	247,335	247,335	247,335	247,335	247,335	247,335
Panel B: Very highly exposed vs moderately exposed (DDD model 2)									
Ban × Very High	0.294***	107,217**	4,966	-84,872	89,838	-62,086**	89,700	140,031	66,145
	(0.065)	(46,737)	(104,992)	(63,122)	(63,895)	(30,358)	(75,817)	(101,362)	(76,914)
Mean	0.859	1,536,476	3,744,172	2,019,755	1,724,417	1,251,018	2,363,845	3,464,258	1,831,882
R-squared	0.103	0.273	0.358	0.361	0.271	0.481	0.267	0.340	0.282
Observations	211,825	211,825	211,825	211,825	211,825	211,825	211,825	211,825	211,825

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$. Standard errors in parentheses are clustered by firm.

Notes: This table shows triple-differences estimates from equation (5.1) of the post-implementation effects of the provincial mandatory retirement bans on highly exposed firms in treated provinces, relative to similar firms in control provinces and moderately exposed firms. High and moderate exposure is defined based on firms' employer-pension plan and union contributions, which is strongly predictive of the effects of the bans on job retention at age 65. Panel B reports estimates from the same regressions when the sample of 'highly exposed' firms is restricted to those that are 'very highly exposed'. The sample includes a balanced panel of firms that operate in strongly affected industries and have non-missing financial information over an 11-year period around the ban announcements.

decrease in payroll costs and lack of productivity impacts lead to an increase in firms' profits for very highly exposed firms (Figure 4d). For these firms, we estimate an average increase in annual profits in the post-period of $107,000 or 7% ($p < 0.05$). The estimated effects on profits are also positive for 'highly exposed' firms, but the point estimate is considerably smaller and not statistically significant. Neither model shows any significant effects on firms' other financial outcomes (total revenue, value added, intermediate inputs, total assets, sales or cost of sales).

Combined, these results suggest that the bans led firms to retain valuable older workers, allowing them to substitute quite strongly away from younger workers without any observable effects on productivity or total revenue. We assess this explanation more directly by looking at the employment, hiring, and separations of workers in different age groups at the firm level (Figures D2–D3; Table D3). The estimates show a relatively large decrease in employment for middle-aged workers (age 35–49) of 0.9 workers in highly exposed firms ($p < 0.1$), which appears to be driven by a decrease in hiring of 0.6 workers per year ($p < 0.1$) rather than an increase in separations. We observe a similar pattern for the very highly exposed firms, with larger point estimates and a significant decline in hiring for middle-aged workers ($p < 0.05$). We do not observe any effects on these outcomes for the younger or older age group. Overall, these empirical patterns are consistent with several recent studies that show that policies that delay the retirements of older workers can reduce hiring among exposed firms (Ferrari et al., 2023; Hut, forthcoming; Boeri et al., 2021; Bianchi et al., 2022; Hernæs et al., 2023).

6 Overall welfare effects

We assess the overall welfare effects of the MR bans within the Marginal Value of Public Funds (MVPF) framework (Hendren and Sprung-Keyser, 2020). The MVPF captures society's willingness to pay (WTP) for the policy divided by its net fiscal cost, which is the sum of its direct fiscal cost and any fiscal externalities. To apply our regression estimates from previous sections, it is helpful to write society's WTP as $\text{WTP} = \text{WTP}_{\text{workers}} + \text{WTP}_{\text{firms}}$, the total WTP of workers and firms in the economy. In turn, we further decompose workers' and firms' WTP based on whether they are directly targeted by the policy or not. We assume that $\text{WTP}_{\text{workers}} = \text{WTP}_{\text{old workers}} + \text{WTP}_{\text{young workers}}$, where the first term is the WTP of old workers, who are exposed to the MR bans in the short term, and the second represents the WTP of young workers, who are potentially exposed to spillovers in the short term and the MR bans in

the long term. Similarly, we assume that $\text{WTP}_{\text{firms}} = \text{WTP}_{\text{exposed firms}} + \text{WTP}_{\text{other firms}}$, where the first term is the WTP of firms that are directly exposed to the bans and the second the WTP of firms that are not directly exposed. Thus, the relevant MVPF formula is as follows:

$$\text{MVPF} = \frac{\text{WTP}_{\text{old workers}} + \text{WTP}_{\text{young workers}} + \text{WTP}_{\text{exposed firms}} + \text{WTP}_{\text{other firms}}}{\text{Direct cost} + \text{Fiscal externality}} \tag{6.1}$$

Our empirical results are informative about the right-hand-side of (6.1). Starting with $\text{WTP}_{\text{old workers}}$, older workers appear to derive several benefits from the bans. First, they experience positive effects on earnings: the average worker who is on the cusp of reaching age 65 earns $20,000 more due to the policy. Second, these workers may benefit from the dignity of being able to choose when they retire and more freedom to co-ordinate their retirement date with their spouse. Finally, greater choice about retirement timing may offer consumption-smoothing benefits, although such benefits may be attenuated in our setting given workers' considerable access to public and private pensions at age 65. In terms of the costs of the policy, we observe no evidence of any adverse mortality impacts or changes in older workers' earnings below age 65, but older workers give up leisure time to work longer. Overall, older workers' revealed preference to respond so strongly by continuing to work beyond age 65 (with the same options to retire at age 65) implies that the benefits significantly outweigh the costs ($\text{WTP}_{\text{old workers}} >> 0$).

For younger workers, whilst we observe some evidence that their opportunities are crowded out at specific firms that are highly exposed to the policy, we do not estimate any effects on their overall employment rates, earnings, or hiring and separation rates at the province level. As such, we conclude that there is no evidence that younger workers are harmed by the policy in the short run. In the long run, these workers likely benefit from improvements in public finances and greater options to work beyond age 65. Hence, we assume that $\text{WTP}_{\text{young workers}} \geq 0$.

For highly exposed firms, we observe no change in worker productivity and a positive or insignificant effect on profits, depending on the specification used. Given these results for profits, we assume that $\text{WTP}_{\text{exposed firms}} \geq 0$.

For non-exposed firms, we lack estimates that are directly informative of the welfare effects (and province-specific time trends preclude us from reliably estimating these effects). However, there is little reason to think that non-exposed firms would be worse off due to the policy. These firms do not lose access to older workers — since they were not choosing to hire them at

age 65 before the bans anyway — and may gain access to a wider talent pool of middle-aged workers, who appear to lose some opportunities in highly exposed firms. Furthermore, the drop in consumption at retirement (Battistin et al., 2009) may lead to an increase in the demand for goods and services sold by non-exposed firms from older workers who now delay retirement. Thus, we assume that $\text{WTP}_{\text{other firms}} \geq 0$. Overall, our results suggest that society's WTP for the MR bans is positive, as all of its components are positive or nonnegative.

In terms of public finances, the direct cost of the policies is negligible, since the bans simply reflect a regulatory change rather than an increase in spending. However, the bans generate large positive fiscal externalities by raising income tax receipts on older workers (Table A3). Workers also continue making CPP/QPP contributions beyond age 65 and delay claiming, and claim fewer UI benefits, responses which are likely to further ease pressure on government expenditure. Corporate income tax revenue likely also increases, due to the increase in profits among the most exposed firms. Other sources of federal and provincial tax revenue, such as consumption taxes, may also be affected by general equilibrium changes in consumption. Thus, the total cost of the policy (the denominator of the MVPF) is negative.

Overall, our estimates suggest that the policies are welfare-improving for both workers and firms, and also beneficial for public finances. Thus, our analysis implies that the Canadian MR bans are a Pareto improvement. In terms of the MVPF, our results imply that the policy has a MVPF of ∞, which simply means that these are welfare-improving policies that "pay for themselves" (Hendren and Sprung-Keyser, 2020).

7 Conclusions and discussion

Using comprehensive linked employer-employee data, we examine the welfare effects of abolishing mandatory retirement (MR). We exploit five provincial MR bans in Canada between 2005 and 2009, with these five provinces joining the rest of North America in outlawing MR. We provide the first comprehensive evidence on how these laws affected the welfare of workers, firms, and public finances. The bans generated large and persistent increases in employment and earnings among workers aged 65 and older, with no discernible adverse effects on younger workers, firm productivity, or profitability. Several mechanisms mediated the welfare gains for older workers, including an increase in savings and spousal retirement delays. The fiscal impacts were strongly

positive: higher tax revenues and lower public pension outlays more than offset any additional expenditures associated with longer working lives. Taken together, the evidence implies that the MR bans constituted a Pareto improvement, raising workers' welfare without imposing costs on firms, and alleviating pressure on government budgets.

MR provisions became embedded in Canadian labor markets more than four decades before their abolition. It is plausible that these arrangements were once privately efficient (Lazear, 1979) in an era characterized by shorter life expectancies, physically demanding work, and less comprehensive social insurance. Since then, both the economic and policy context have changed profoundly. Life expectancy at age 65 has risen by more than four years, female labor supply has surged, and public pensions have become substantially more generous, reducing the need for individuals with declining physical capacity to remain employed. Moreover, the task content of jobs has evolved alongside the economy's shift from manufacturing toward services. Technological change has reduced the importance of routine manual and cognitive tasks, while expanding roles that rely on non-routine problem-solving, communication, and analytical skills (Autor et al., 2003; Acemoglu and Autor, 2011). These shifts have likely increased the value of experience and firm-specific knowledge while diminishing the relevance of physical decline.

Why, then, did firms continue to enforce MR for so long? One possibility is that these policies offered employers institutional cover to engage in (inefficient) taste-based or statistical age discrimination (Neumark et al., 2019). Another is that they served as a commitment device within deferred-compensation contracts, in which workers are paid less than their marginal product when young and more when old (Lazear, 1979). However, our results, which show no adverse productivity effects on firms, do not appear to align with either explanation. The bans' effects are concentrated among unionized employers with high rates of pension coverage — patterns difficult to reconcile with generalized animus. Nor are the effects correlated with firms' wage–age gradients, as we would expect if late-career labor costs were the main concern. Moreover, firms adjust during the announcement period by increasingly retaining, rather than shedding, older workers — behavior inconsistent with either discrimination or deferred-compensation motives.

The empirical evidence points instead to an institutional and political interpretation. The enforcement of MR appears to have reflected internal labor-market norms shaped by unionized workforces. Many unions actively opposed the bans, as their median member had little interest in extending work beyond age 65 and viewed mandatory retirement as a mechanism to protect

pension entitlements and the advancement prospects of younger colleagues (Klassen and Forgione, 2005).[43] Contemporaneous media coverage reflected these concerns.[44] The persistence of MR, despite fundamental shifts in longevity, technology, and workforce composition, likely reflects the path-dependent nature of economic and political institutions (North, 1990; Pierson, 2000; Acemoglu et al., 2001). These institutional dynamics remain relevant not only in Canada, where collective bargaining is much more common than the US, but also in many European countries that continue to maintain formal MR systems (Germany, France, Ireland, Finland, Sweden, the Netherlands, Norway, Portugal, Iceland).

Our research holds direct relevance to these countries. Our findings suggest that policies in that limit or ban mandatory retirement can have large positive effects on old-age employment and workers' welfare. Other countries like the US and Canada may have already banned MR but have weak statutory employment protections. Older workers in these countries are thus vulnerable to being laid off and may benefit from more employment protection, especially as they often face discrimination in the hiring process (Neumark et al., 2019). Such measures may reduce the share of retirees who are completely or partially forced to retire, which appears to be on the rise in the US (Johnson and Gosselin, 2018). Our results show a one-for-one link between involuntary job separations and retirement hazards at age 65, which means that initiatives that boost the retention of older workers by firms (e.g., through employment protection, training or workplace accommodations) can bolster the employment and welfare of older workers. Importantly, our results indicate that these interventions do not necessarily harm firms or younger workers.

[43] This reasoning aligns with the Black's (1948) median voter theorem, which predicts that policies in democratic institutions — such as unions — reflect the preferences of the median member (Freeman and Medoff, 1984).

[44] On the day the first MR ban was announced in Ontario, the president of the Ontario Federation of Labour — an affiliation of 54 unions with more than one million workers — warned that the government might use the ban as a first step toward raising the pension-eligibility age: "So what's really going on here is they're paving the way for people to work longer, mainly because of the fact we're all living longer". Meanwhile, the head of the Canadian Autoworkers Union expressed concerns about spillover effects on younger workers: "It does undermine our policies in how we generate opportunities for young people". Source: `https://www.cbc.ca/news/canada/ottawa/ontario-bans-mandatory-retirement-1.614411` [Accessed October 23, 2025.]

References

Acemoglu, Daron, and David Autor. 2011. "Skills, tasks and technologies: Implications for employment and earnings." In *Handbook of labor economics*. Vol. 4, 1043–1171. Elsevier.

Acemoglu, Daron, and Joshua D Angrist. 2001. "Consequences of employment protection? The case of the Americans with Disabilities Act." *Journal of Political Economy*, 109(5): 915–957.

Acemoglu, Daron, and Matthew O Jackson. 2017. "Social norms and the enforcement of laws." *Journal of the European Economic Association*, 15(2): 245–295.

Acemoglu, Daron, Simon Johnson, and James A Robinson. 2001. "The colonial origins of comparative development: An empirical investigation." *American Economic Review*, 91(5): 1369–1401.

Adams, Scott J. 2004. "Age discrimination legislation and the employment of older workers." *Labour Economics*, 11(2): 219–241.

Ashenfelter, Orley, and David Card. 2002. "Did the elimination of mandatory retirement affect faculty retirement?" *American Economic Review*, 92(4): 957–980.

Autor, David H, Frank Levy, and Richard J Murnane. 2003. "The skill content of recent technological change: An empirical exploration." *Quarterly journal of economics*, 118(4): 1279–1333.

Autor, David H, John J Donohue III, and Stewart J Schwab. 2006. "The costs of wrongful-discharge laws." *Review of Economics and Statistics*, 88(2): 211–231.

Baldwin, Bob. 2015. "The economic impact on plan members of the shift from Defined Benefit to Defined Contribution in workplace pension plans." *Canadian Labour & Employment Law Journal*, 19: 23.

Battistin, Erich, Agar Brugiavini, Enrico Rettore, and Guglielmo Weber. 2009. "The retirement consumption puzzle: Evidence from a regression discontinuity approach." *American Economic Review*, 99(5): 2209–26.

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

Bellés-Obrero, Cristina, Sergi Jiménez-Martín, and Han Ye. 2022. "The effect of removing early retirement on mortality." *IZA DP No. 15577*.

Benabou, Roland, and Jean Tirole. forthcoming. "Laws and norms." *Journal of Political Economy*.

Bertheau, Antoine, Pierre Cahuc, Simon Jäger, and Rune Vejlin. 2022. "Turnover costs: Evidence from unexpected worker separations." *mimeo*.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How much should we trust differences-in-differences estimates?" *Quarterly Journal of Economics*, 119(1): 249–275.

Bianchi, Nicola, Giulia Bovini, Jin Li, Matteo Paradisi, and Michael L Powell. 2022. "Career spillovers in internal labor markets." *Review of Economic Studies*.

Black, Duncan. 1948. "The decisions of a committee using a special majority." *Econometrica*, 245–261.

Bloemen, Hans, Stefan Hochguertel, and Jochem Zweerink. 2017. "The causal effect of retirement on mortality: Evidence from targeted incentives to retire early." *Health Economics*, 26(12): e204–e218.

Boeri, Tito, Pietro Garibaldi, and Espen R. Moen. 2021. "In medio stat victus: Labor demand effects of an increase in the retirement age." *Journal of Population Economics*, 1–37.

Börsch-Supan, Axel, and Reinhold Schnabel. 2010. "Early retirement and employment of the young in Germany." *Social Security Programs and Retirement around the World: The Relationship to Youth Employment*, 147–166.

Bozio, Antoine, Clémentine Garrouste, and Elsa Perdrix. 2021. "Impact of later retirement on mortality: Evidence from France." *Health Economics*, 30(5): 1178–1199.

Burkhauser, Richard V, and Joseph F Quinn. 1983. "Is mandatory retirement overrated? Evidence from the 1970s." *Journal of Human Resources*, 337–358.

Carta, Francesca, Francesco D'Amuri, and Till Von Wachter. 2021. "Workforce aging, pension reforms, and firm outcomes." *NBER WP No. 28407*.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The effect of minimum wages on low-wage jobs." *Quarterly Journal of Economics*, 134(3): 1405–1454.

Chetty, Raj, John N Friedman, Søren Leth-Petersen, Torben Heien Nielsen, and Tore Olsen. 2014. "Active vs. passive decisions and crowd-out in retirement savings accounts: Evidence from Denmark." *Quarterly Journal of Economics*, 129(3): 1141–1219.

Chino, Atsushi. 2024. "The negative effects of delayed retirement: Employment protection of older workers and firm value." *SSRN Paper No. 4342509*.

Chrostek, Paweł, Krzysztof Karbownik, and Michał Myck. 2024. "Labor market externalities of pre-retirement employment protection." *NBER WP No. 32371*.

Clark, Robert L, and Linda S Ghent. 2008. "Mandatory retirement and faculty retirement decisions." *Industrial Relations: A Journal of Economy and Society*, 47(1): 153–163.

Cribb, Jonathan, Carl Emmerson, and Gemma Tetlow. 2016. "Signals matter? Large retirement responses to limited financial incentives." *Labour Economics*, 42: 203–212.

Daruich, Diego, Sabrina Di Addario, and Raffaele Saggio. 2023. "The effects of partial employment protection reforms: Evidence from Italy." *Review of Economic Studies*, 90(6): 2880–2942.

Deshpande, Manasi, Itzik Fadlon, and Colin Gray. 2021. "How sticky is retirement behavior in the US?" *Review of Economics and Statistics*, 1–55.

Dorn, David, and Alfonso Sousa-Poza. 2010. "'Voluntary' and 'involuntary' early retirement: An international analy-

sis." *Applied Economics*, 42(4): 427–438.
Dube, Arindrajit, Daniele Girardi, Oscar Jorda, and Alan M Taylor. 2023. "A local projections approach to difference-in-differences event studies." *NBER WP No. 31184*.
ESDC Canada. 2019. "Who defers the start of their OAS pension?" *Report*.
Etgeton, Stefan, Björn Fischer, and Han Ye. 2023. "The effect of increasing retirement age on households' savings and consumption expenditure." *Journal of Public Economics*, 221: 104845.
Ferrari, Irene, Jan Kabátek, Todd Morris, et al. 2023. "Longer careers: A barrier to hiring and coworker advancement?" *IZA DP No. 16098*.
Fitzpatrick, Maria D, and Timothy J Moore. 2018. "The mortality effects of retirement: Evidence from social security eligibility at age 62." *Journal of Public Economics*, 157: 121–137.
Freeman, Richard B., and James L. Medoff. 1984. *What Do Unions Do?* New York:Basic Books.
Frimmel, Wolfgang, Thomas Horvath, Mario Schnalzenberger, and Rudolf Winter-Ebmer. 2018. "Seniority wages and the role of firms in retirement." *Journal of Public Economics*, 164: 19–32.
García-Miralles, Esteban, and Jonathan M Leganza. 2024*a*. "Joint retirement of couples: Evidence from discontinuities in Denmark." *Journal of Public Economics*, 230: 105036.
García-Miralles, Esteban, and Jonathan M Leganza. 2024*b*. "Public pensions and private savings." *American Economic Journal: Economic Policy*, 16(2): 366–405.
Garrouste, Clémentine, and Elsa Perdrix. 2022. "Is there a consensus on the health consequences of retirement? A literature review." *Journal of Economic Surveys*, 36(4): 841–879.
Geyer, Johannes, and Clara Welteke. 2021. "Closing routes to retirement for women — How do they respond?" *Journal of Human Resources*, 56(1): 311–341.
Glenzer, Franca, Pierre-Carl Michaud, and Stefan Staubli. 2025. "Frames, incentives, and education: Effectiveness of interventions to delay public pension claiming." *Journal of Public Economics*, 248: 105419.
Gruber, Jonathan, Kevin Milligan, and David A Wise. 2009. "Social security programs and retirement around the world: The relationship to youth employment, introduction and summary." *NBER WP No. 14647*.
Gunderson, Morley. 1987. "Effect of banning industrial relations functions." *Report to the Ontario Task Force on Mandatory Retirement*.
Hagen, Johannes. 2018. "The effects of increasing the normal retirement age on health care utilization and mortality." *Journal of Population Economics*, 31(1): 193–234.
Hallberg, Daniel, Per Johansson, and Malin Josephson. 2015. "Is an early retirement offer good for your health? Quasi-experimental evidence from the army." *Journal of Health Economics*, 44: 274–285.
Hendren, Nathaniel, and Ben Sprung-Keyser. 2020. "A unified welfare analysis of government policies." *Quarterly Journal of Economics*, 135(3): 1209–1318.
Hernaes, Erik, Simen Markussen, John Piggott, and Ola L Vestad. 2013. "Does retirement age impact mortality?" *Journal of Health Economics*, 32(3): 586–598.
Hernæs, Erik, Tom Kornstad, Simen Markussen, and Knut Røed. 2023. "Ageing and labor productivity." *Labour Economics*, 102347.
Hut, Stefan. forthcoming. "Impact of raising the retirement age on firms." *Journal of Human Resources*.
Johnsen, Julian Vedeler, Kjell Vaage, and Alexander Willén. 2022. "Interactions in public policies: Spousal responses and program spillovers of welfare reforms." *Economic Journal*, 132(642): 834–864.
Johnson, Richard W, and Peter Gosselin. 2018. "How secure is employment at older ages." Urban Institute Research Report `https://shorturl.at/bxUX5`.
Klassen, Thomas R., and David Forgione. 2005. "Forced Retirement: Organized Labour's Predicament." In *Time's Up! Mandatory Retirement in Canada.* , ed. C. T. Gillin, David MacGregor and Thomas R. Klassen, 75–89. Toronto:James Lorimer & Company Ltd., Publishers.
Kondo, Ayako, and Hitoshi Shigeoka. 2017. "The effectiveness of demand-side government intervention to promote elderly employment: Evidence from Japan." *ILR Review*, 70(4): 1008–1036.
Kuhn, Andreas, Stefan Staubli, Jean-Philippe Wuellrich, and Josef Zweimüller. 2020. "Fatal attraction? Extended unemployment benefits, labor force exits, and mortality." *Journal of Public Economics*, 191: 104087.
Lahey, Joanna. 2008. "State age protection laws and the Age Discrimination in Employment Act." *Journal of Law and Economics*, 51(3): 433–460.
Lalive, Rafael, and Pierpaolo Parrotta. 2017. "How does pension eligibility affect labor supply in couples?" *Labour Economics*, 46: 177–188.
Lalive, Rafael, Arvind Magesan, and Stefan Staubli. 2023. "How social security reform affects retirement and pension claiming." *American Economic Journal: Economic Policy*, 15(3): 115–150.
Lazear, Edward P. 1979. "Why is there mandatory retirement?" *Journal of Political Economy*, 87(6): 1261–1284.
Lindeboom, Maarten, and Raymond Montizaan. 2020. "Disentangling retirement and savings responses." *Journal of Public Economics*, 192: 104297.
Lundberg, Shelly. 1985. "The added worker effect." *Journal of Labor Economics*, 3(1, Part 1): 11–37.
MacKinnon, James G, and Matthew D Webb. 2018. "The wild bootstrap for few (treated) clusters." *Econometrics Journal*, 21(2): 114–135.
MacKinnon, James G, Morten Ørregaard Nielsen, and Matthew D Webb. 2023. "Cluster-robust inference: A guide to empirical practice." *Journal of Econometrics*, 232(2): 272–299.
Mercan, Yusuf, and Benjamin Schoefer. 2020. "Jobs and matches: Quits, replacement hiring, and vacancy chains."

American Economic Review: Insights, 2(1): 101–24.

Messacar, Derek, and Petr Kocourek. 2019. "Pathways to retirement, well-being, and mandatory retirement rules: Evidence from Canadian reforms." *Journal of Labor Research*, 40: 249–275.

Mohnen, Paul. forthcoming. "The impact of the retirement slowdown on the US youth labor market." *Journal of Labor Economics*.

Moore, Timothy J, and Todd Morris. 2024. "Shaping the habits of teen drivers." *American Economic Journal: Economic Policy*, 16(3): 367–393.

Morris, Todd, and Benoit Dostie. 2023. "Graying and staying on the job: The welfare implications of employment protection for older workers." *IZA Discussion Paper No. 16430*.

Munnell, Alice H, and April Wu. 2013. "Do older workers squeeze out younger workers?" *SIEPR DP No. 13–011*.

Nakazawa, Nobuhiko. 2022. "The effects of increasing the eligibility age for public pension on individual labor supply: Evidence from Japan." *Journal of Human Resources*, 0421–11627R1.

Neumark, David, and Wendy A Stock. 1999. "Age discrimination laws and labor market efficiency." *Journal of Political Economy*, 107(5): 1081–1125.

Neumark, David, Ian Burn, and Patrick Button. 2019. "Is it harder for older workers to find jobs? New and improved evidence from a field experiment." *Journal of Political Economy*, 127(2): 922–970.

Nielsen, Nick Fabrin. 2019. "Sick of retirement?" *Journal of Health Economics*, 65: 133–152.

North, Douglass C. 1990. *Institutions, institutional change and economic performance.* Cambridge university press.

Pierson, Paul. 2000. "Increasing returns, path dependence, and the study of politics." *American Political Science Review*, 94(2): 251–267.

Posner, Richard A. 2002. *Behavioral law and economics: A critique.* American Institute for Economic Research.

Rabaté, Simon. 2019. "Can I stay or should I go? Mandatory retirement and the labor-force participation of older workers." *Journal of Public Economics*, 180: 104078.

Rabaté, Simon, Egbert Jongen, and Tilbe Atav. 2024. "Increasing the retirement age: policy effects and underlying mechanisms." *American Economic Journal: Economic Policy*, 16(1): 259–291.

Risch, Max. 2024. "Does taxing business owners affect employees? Evidence from a change in the top marginal tax rate." *Quarterly Journal of Economics*, 139(1): 637–692.

Roodman, David, Morten Ørregaard Nielsen, James G MacKinnon, and Matthew D Webb. 2019. "Fast and wild: Bootstrap inference in Stata using boottest." *Stata Journal*, 19(1): 4–60.

Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe. 2023. "What's trending in difference-in-differences? A synthesis of the recent econometrics literature." *Journal of Econometrics*, 235(2): 2018–2244.

Saez, Emmanuel, Benjamin Schoefer, and David G Seim. 2024. "Deadwood Labor? The Effects of Eliminating Employment Protection for Older Workers." *NBER WP No. 31797*.

Saez, Emmanuel, Benjamin Schoefer, and David Seim. 2019. "Payroll taxes, firm behavior, and rent sharing: Evidence from a young workers' tax cut in Sweden." *American Economic Review*, 109(5): 1717–1763.

Seibold, Arthur. 2021. "Reference points for retirement behavior: Evidence from German pension discontinuities." *American Economic Review*, 111(4): 1126–65.

Selin, Håkan. 2017. "What happens to the husband's retirement decision when the wife's retirement incentives change?" *International Tax and Public Finance*, 24: 432–458.

Shannon, Michael, and Diana Grierson. 2004. "Mandatory retirement and older worker employment." *Canadian Journal of Economics*, 37(3): 528–551.

Snyder, Stephen E, and William N Evans. 2006. "The effect of income on mortality: Evidence from the social security notch." *Review of Economics and Statistics*, 88(3): 482–495.

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

Steiber, Nadia, and Martin Kohli. 2017. "You can't always get what you want: Actual and preferred ages of retirement in Europe." *Ageing & Society*, 37(2): 352–385.

Sunstein, Cass R. 1996. "Social norms and social roles." *Columbia Law Review*, 96: 903.

Warman, Casey, and Christopher Worswick. 2010. "Mandatory retirement rules and the retirement decisions of university professors in Canada." *Labour Economics*, 17(6): 1022–1029.

Web Appendix for "The welfare effects of protecting older workers"

Todd Morris Stefan Staubli Benoit Dostie

Contents

A Data appendix **A2**

A.1 List of industries covered by federal regulation A2

A.2 Main appendix figures and tables A3

B Projected employment effects of higher pension ages **B1**

C Additional robustness checks for worker-level spillovers **C1**

C.1 Assessing the parallel-trends assumption C1

D Appendix for firm-level analysis **D1**

D.1 Descriptive statistics D1

D.2 Analysis of effects on firm survival D2

D.3 Additional firm-level estimates D3

List of Figures

A1 Effect of announcement of mandatory retirement bans by age A3

A2 Estimated effect of mandatory retirement bans at ages 66–68 in event time A4

A3 Robustness of estimated effects on job separations to exclusion of provinces A5

A4 Estimates for each treated province individually A6

A5 Randomization inference estimates and t-statistics for estimates on job separations A7

A6 Correlation between effect heterogeneity by industry and industry characteristics A8

A7 Heterogeneity in effects on job separations within firms A9

B1 Actual and projected changes in retirement hazards and employment B2

B2 Retirement hazards around age 65 by employer-pension coverage B3

C1 Event-study plots for individual-level spillovers at age 65 (workers in strongly affected industries) . C2

C2 Event-study plots for spillovers on spousal retirement hazard (reference workers in strongly affected industries) C4

D1 Estimated effects on firm shutdown probability in event-time D2

D2 Event-study plots for firm-level financial outcomes and employment D4

D3 Event-study plots for firm-level hiring and separations D5

List of Tables

A1 Comparison of estimates for employment and earnings using all tax filers compared to those working at age 61 . . . . . A10

A2 Saving responses to the mandatory retirement bans at ages 61–64 . . . . . A11

A3 Effects of the mandatory retirement bans on disposable income at age 66 and its components . . . A12

A4 Estimated spillover effects on retirement exit rates of non-working spouses A13

A5 Heterogeneity in spillover effects on retirement hazard rates of working spouses by age of spouse . A14
A6 Estimated spillover effects on retirement hazard rates of working spouses in the following year . . . A15
A7 Estimated spillover effects on retirement hazard rates of working spouses by gender of spouse . . . A16
A8 Estimated effects of mandatory retirement bans on annual employment outcomes — DiD estimates at the province level by age A17
D1 Summary statistics — Balanced panel of firms in strongly affected industries D1
D2 Effects on firm shutdown probability after ban implementation D2
D3 Effects of mandatory retirement bans on highly exposed firms' broader personnel decisions D6

A Data appendix

A.1 List of industries covered by federal regulation

Workers in some industries are covered by federal regulation and thus were not affected by the provincial mandatory retirement bans. In 2015, these industries accounted for around 6% of workers and included:[A1]

- Air transportation (NAICS codes: 481215, 488111, 488119, 488190, 561612, 611510, 621912)
- Rail transportation (NAICS codes: 482112, 482113, 482114)
- Road transportation (local) (NAICS codes: 484110, 484221, 484222, 484223, 484229)
- Road transportation (excluding local) (NAICS codes: 484121, 484122, 484210, 484231, 484232, 484233, 484239, 485110, 485210, 485510, 485990, 561613)
- Maritime transportation (NAICS codes: 483115, 483116, 483213, 483214, 487210, 488310, 488320, 488331, 488332, 488339, 488390)
- Postal services and pipelines (NAICS codes: 486110, 486210, 486910, 491110, 492110)
- Banks (NAICS codes: 521110, 522111, 522112)
- Feed, flour, seed and grain (NAICS codes: 311119, 311211, 311214, 311221, 411120, 418320, 419120, 493130)
- Telecommunications (NAICS codes: 515110, 515120, 515210, 517111, 517112, 517210, 517410, 517910)
- Other: Fisheries and oceans, nuclear industry and mines, extraction (oil and gas) (NAICS codes: 211113, 212291, 541380, 541620, 541690, 541710, 911290)

In addition, workers in the federal public service (NAICS code: 911) were not affected, since mandatory retirement was banned there in 1986.

[A1] `https://www150.statcan.gc.ca/n1/daily-quotidien/161130/dq161130g-eng.htm`

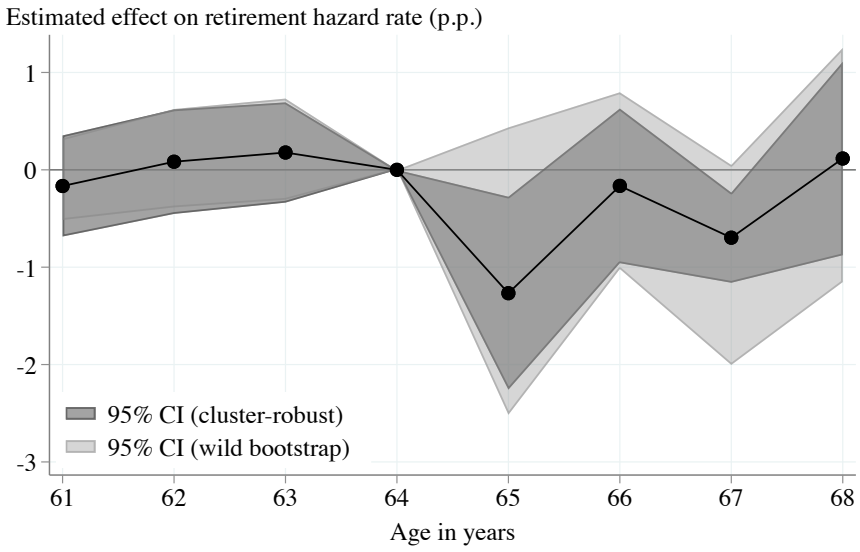
A.2 Main appendix figures and tables

Figure A1: Effect of announcement of mandatory retirement bans by age

(a) Job separation rates



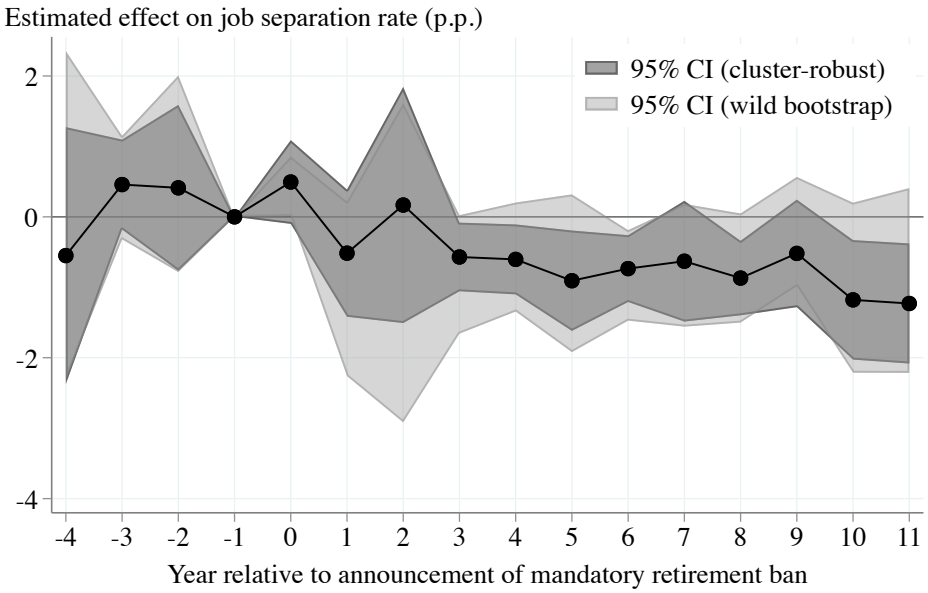
(b) Retirement hazards



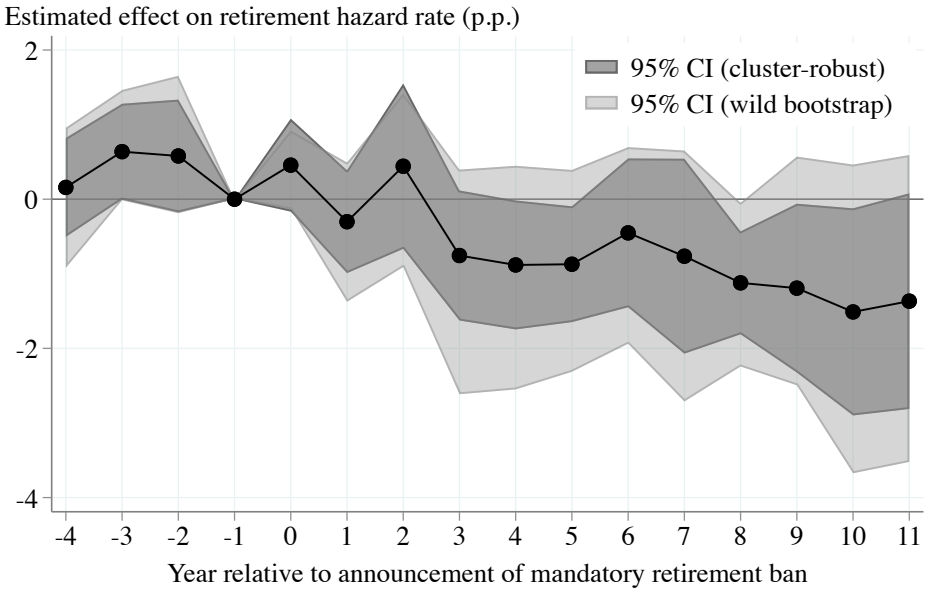
Notes: These figures show estimates of the announcement effects of the mandatory retirement bans by age on job separation rates in panel (a) and retirement hazards in panel (b) with 95% confidence intervals. The estimates come from a modified version of equation (3.2) that allows for different effects between the ban announcement and implementation dates at each age from 61 to 68 (relative to the effect at age 64). See Figure 2a for the estimates of the post-implementation effects by age, which come from the same regression.

Figure A2: Estimated effect of mandatory retirement bans at ages 66–68 in event time

(a) Job separation rates

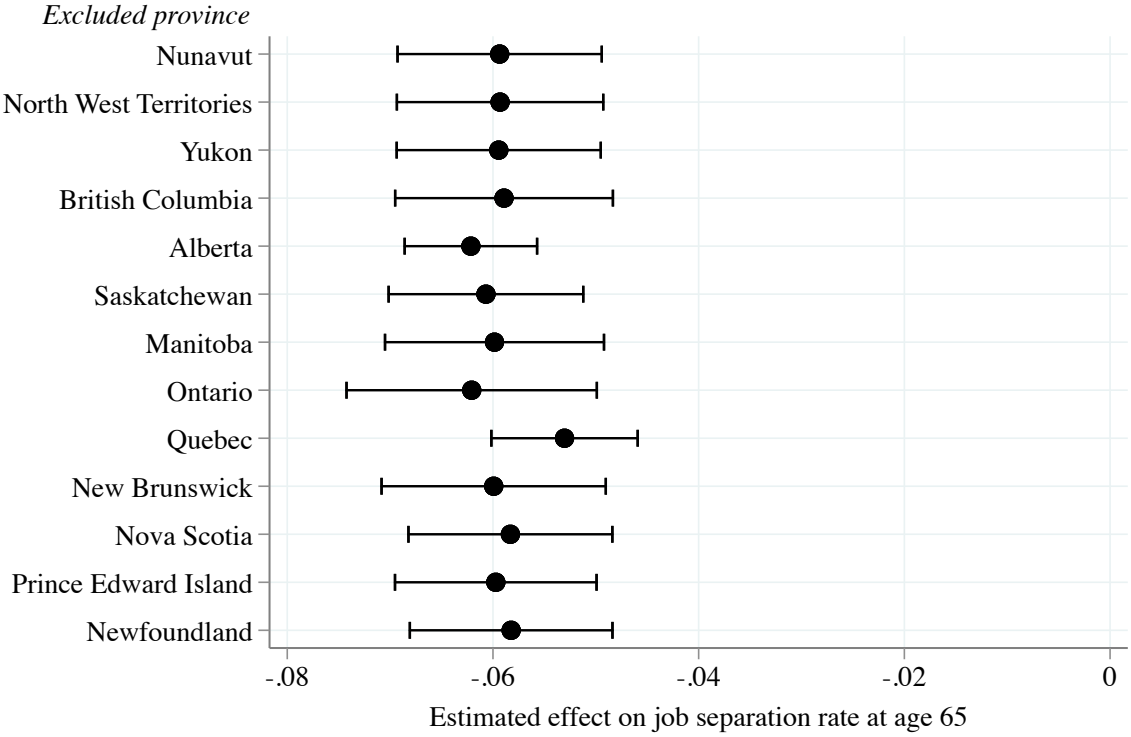


(b) Retirement hazards



Notes: These figures show the estimated effects of the mandatory retirement bans at ages 66–68 in event time with 95% confidence intervals. The figures show the estimated effects at ages 66–68 in event time from equation 3.3 (relative to the effect in the year before the ban was announced). See Figure 2b for the estimated effects at age 65, which come from the same regression.

Figure A3: Robustness of estimated effects on job separations to exclusion of provinces

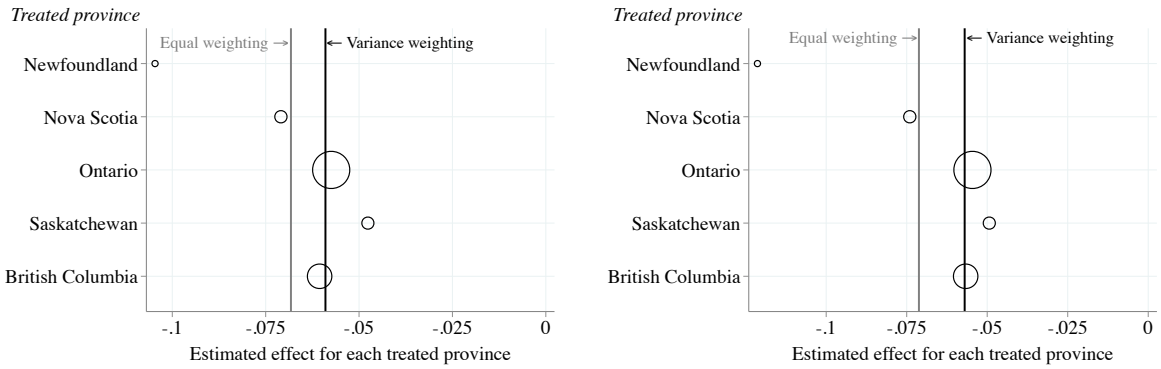


Notes: This figure shows the robustness of the estimated post-implementation effects of the mandatory retirement bans on job-separation rates at age 65 to the exclusion of individual provinces from the sample. We show the point estimates from equation (3.2) with 95% confidence intervals (based on our preferred wild-cluster bootstrap method) when different provinces are excluded from the sample.

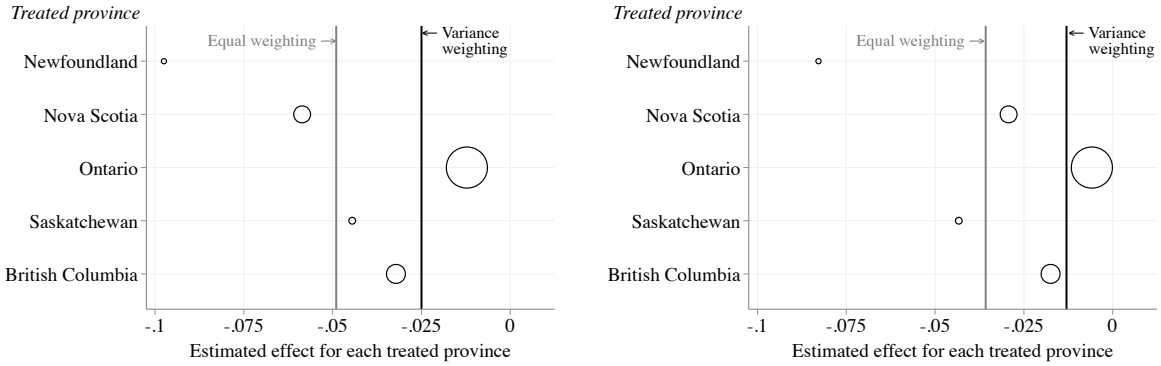
Figure A4: Estimates for each treated province individually

Job separation rates | Retirement hazards

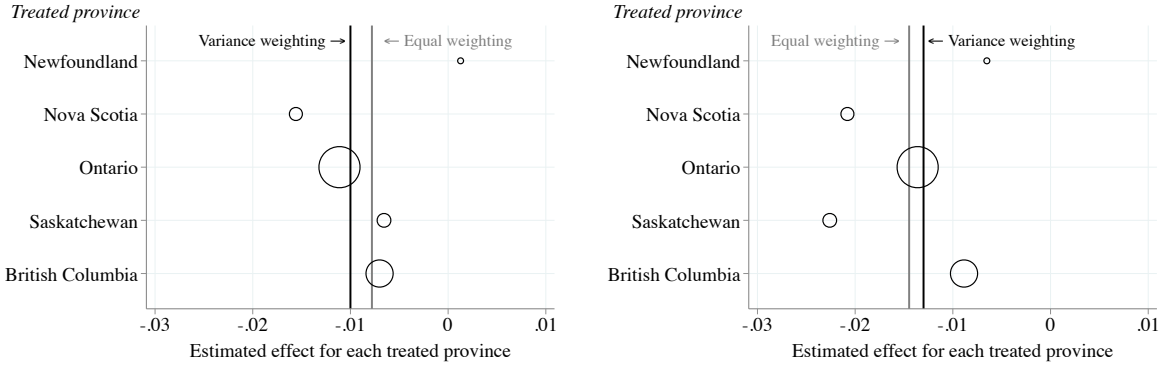
(a) Post-implementation effects at age 65



(b) Announcement effects at age 65

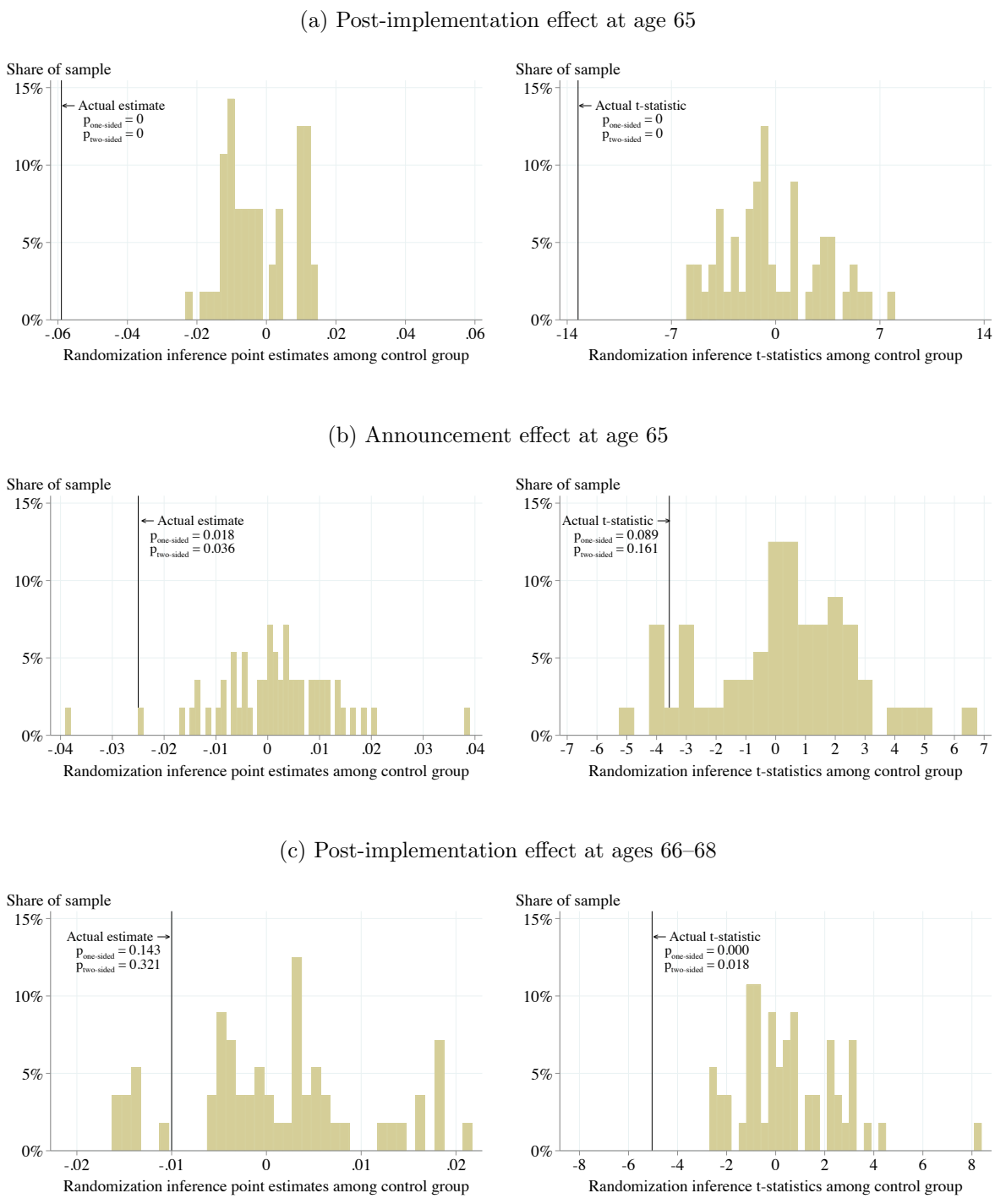


(c) Post-implementation effects at ages 66–68



Notes: These figures show the point estimates from equation (3.2) when we restrict the sample to a single treated province and the set of control provinces. We show the estimates of the post-implementation effects at age 65 in panel (a), the announcement effects at age 65 in panel (b), and the post-implementation effects at age 66–68 in panel (c). The size of the circles reflects the importance of each treated province in determining our variance-weighted estimates, which depends on population size and ban timing (or the duration of the announcement period in the case of panel b). We use vertical lines to compare our variance-weighted point estimates with the estimates implied by equally weighting the effects in the five provinces.

Figure A5: Randomization inference estimates and t-statistics for estimates on job separations



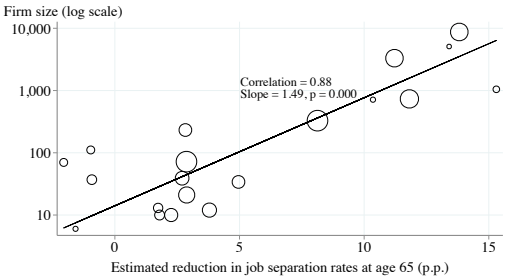
Notes: These figures show the distributions of point estimates and t-statistics from equation (3.2) when we randomly assign treatment to 3 out of 8 of the control provinces. For each possible set of 3 'treated' provinces, we randomly assign these provinces to the ban timing of Ontario, British Columbia and Saskatchewan without replacement. Job separation rates in the 'treated' provinces are then compared to the other 5 control provinces. The vertical line denotes the actual point estimates and t-statistics from our estimates in Table 2. One- and two-sided p-values show the fraction of estimates/t-statistics from our randomization inference method that are more extreme than these estimates.

Figure A6: Correlation between effect heterogeneity by industry and industry characteristics

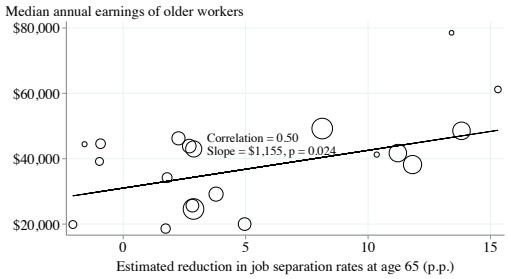
(a) Pension coverage of older workers



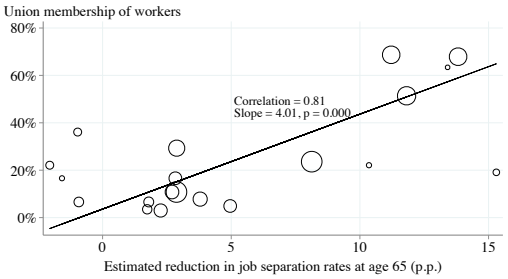
(b) Firm/organization size



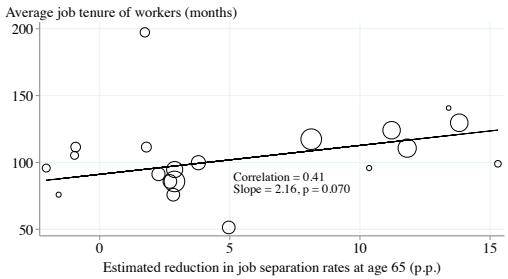
(c) Average earnings of older workers



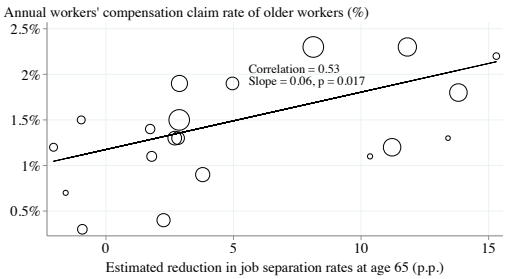
(d) Unionization of workers



(e) Average job tenure



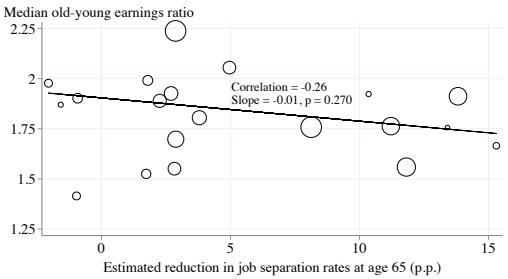
(f) Workplace injury risks of older workers



(g) Post-ban separation rates at age 65

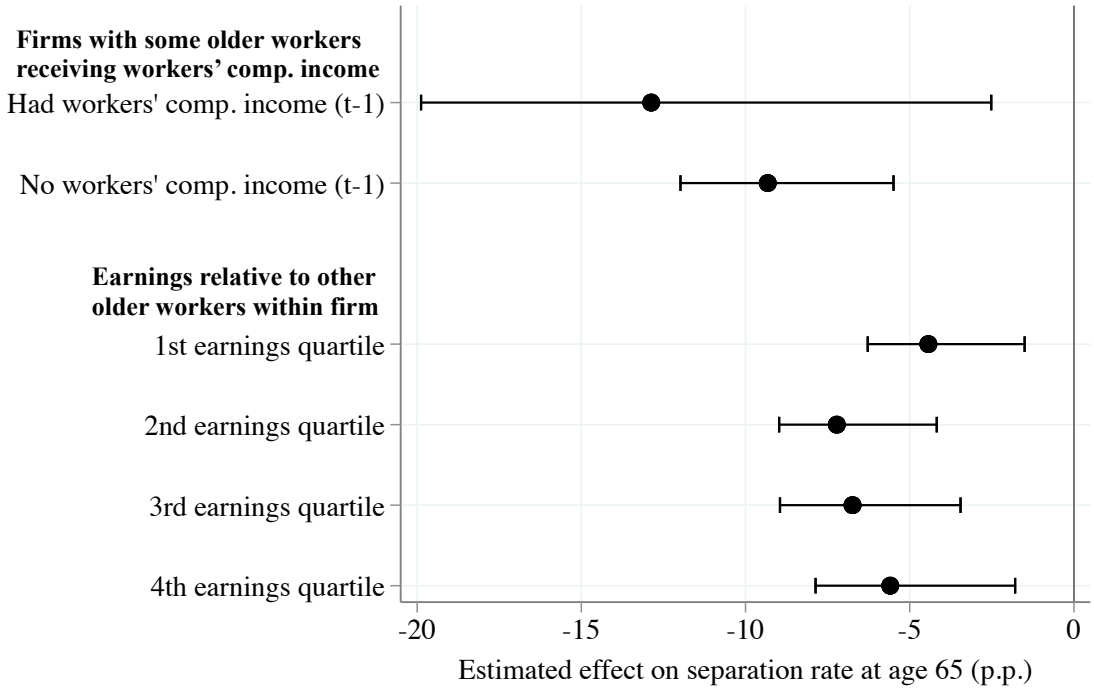


(h) Earnings gap between old and young



Notes: These figures show how the estimated effects on job separation rates at age 65 from equation (3.2) correlate with industry characteristics. The size of the circles denote the number of workers in a given industry. We show the correlation between the estimates and the particular industry characteristics and a regression of one on the other. Observations are weighted by the number of workers in each industry (with a line of best fit shown in red). Industry characteristics are based on our own calculations in panels (a)–(c) and (f)–(h) over our sample period and data from Statistics Canada for the 2019 year in panels d (`https://www150.statcan.gc.ca/t1/tbl1/en/tv.action?pid=1410013201`) and e (`https://www150.statcan.gc.ca/t1/tbl1/en/tv.action?pid=1410005501`).

Figure A7: Heterogeneity in effects on job separations within firms



Notes: This figure shows the estimated post-implementation effects of the mandatory retirement bans at age 65 for different subgroups based on the characteristics of the older workers relative to other older workers within the same firm. We show our triple-difference estimates (equation 3.2) and 95% confidence intervals based on wild cluster-bootstrap inference. The top part of the figure compares older workers who were receiving income from workers' compensation in the previous year to those who were not, with the sample restricted to firms that employed at least one older worker over the sample period who was receiving workers' compensation. The second part of the figure ranks older workers based on their earnings (in the previous year) relative to other older workers within the firm.

Table A1: Comparison of estimates for employment and earnings using all tax filers compared to those working at age 61

	(1)	(2)	(3)	(4)
	Employed next year		Earnings next year (2019 $)	
	Working at 61	All tax filers	Working at 61	All tax filers
Mandatory retirement ban	0.005	-0.012	-1,372	-1,138
	(0.008)	(0.006)	(1,074)	(1,213)
Cluster-robust p-value	0.521	0.058	0.226	0.366
Wild bootstrap p-value	0.676	0.383	0.349	0.581
MR Ban x age 65	0.045***	0.024	5,056***	2,033***
	(0.006)	(0.010)	(577)	(259)
Cluster-robust p-value	0.000	0.025	0.000	0.000
Wild bootstrap p-value	0.008	0.213	0.000	0.000
MR Ban x age 66–68	0.051***	0.022	4,536**	1,611*
	(0.007)	(0.012)	(711)	(335)
Cluster-robust p-value	0.000	0.087	0.000	0.000
Wild bootstrap p-value	0.000	0.473	0.026	0.063
Implied percent increase at 65–68	13.7%	11.4%	33.6%	22.1%
R-squared	0.173	0.080	0.096	0.065
Observations	44,414,520	122,778,040	44,414,520	122,778,040

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes: This table compares the estimated effects on employment and earnings based on our main sample, which restricts the sample to individuals who were working at age 61, and for all tax filers. The estimates come from equation (3.2). The point estimates imply similar percentage increases in employment and earnings at ages 66–69 for both samples, although the absolute size of the estimates is unsurprisingly much larger when the sample is restricted to individuals working at age 61.

Table A2: Saving responses to the mandatory retirement bans at ages 61–64

	(1) Full sample	(2) Strongly affected industries
Final employer-pension contribution	-0.0055	-0.0074
	[0.105]	[0.444]
New employer-pension income	-0.0053*	-0.0069
	[0.072]	[0.130]
New CPP/QPP claim	-0.0002	-0.0007
	[0.974]	[0.945]
Final RRSP contribution	-0.0071*	-0.0095
	[0.083]	[0.103]
New RRSP withdrawal	-0.0039	-0.0050*
	[0.107]	[0.074]

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values, which are shown in square brackets.

Notes: This table shows difference-in-difference estimates of the spillover effects of the bans at age 61–64 on savings behavior from equation (3.1). We focus on the post-implementation effects. Column 2 shows the estimates when we restrict the sample to workers in the seven most strongly affected industries (42% of the full sample). The direct effects on job separation rates and retirement hazards are 1.8 times larger in this sample than the full sample. The estimates show little evidence that older workers adjust their savings behavior prior to age 65.

Table A3: Effects of the mandatory retirement bans on disposable income at age 66 and its components

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Net benefits						
	Disposable income	Earnings	Employer pensions	CPP/QPP	RRSPs	OAS/GIS benefits	UI benefits	Other income	Income tax
Effect of ban	3,503***	6,459***	-959**	-467	-799***	-34	-184	543	1,056**
	(492)	(699)	(185)	(160)	(108)	(213)	(80)	(191)	(205)
Cluster-robust p-value	0.000	0.000	0.000	0.013	0.000	0.877	0.040	0.015	0.000
Wild bootstrap p-value	0.000	0.000	0.021	0.116	0.000	0.773	0.256	0.130	0.037
Mean at age 66	$54,384	$35,224	$7,619	$6,864	-$2,042	$6,622	$921	$10,461	$11,286
Observations	36,988,320								

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes: This table shows triple-difference estimates of the effects of the bans on workers' income in the following year when they are aged 65. We focus on the post-implementation effects. As outcomes are measured in year $t + 1$ (i.e., at age 66), we control for the effect of the bans on 64 year-olds (who are above 65 for part of $t + 1$). Components of disposable income in 2019 $.

Table A4: Estimated spillover effects on retirement exit rates of non-working spouses

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Strongly affected industries		
	Age 65 effect	Age 66–68 effect	Weighted average	Age 65 effect	Age 66–68 effect	Weighted average
Panel A: Effect on retirement hazard of reference individual						
Ban effect at age 65	-0.0470**	-0.0150**	-0.0285**	-0.0878**	-0.0391	-0.0611**
	(0.0074)	(0.0023)	(0.0035)	(0.0134)	(0.0125)	(0.0090)
	0.000	0.000	0.000	0.000	0.009	0.000
	0.018	0.024	0.011	0.021	0.154	0.011
Observations		5,388,900			2,337,380	
Panel B: Effect on retirement exit rate of spouse						
Ban effect at age 65	-0.0034	-0.0027	-0.0030	-0.0034	-0.0059	-0.0048
	(0.0015)	(0.0024)	(0.0015)	(0.0020)	(0.0031)	(0.0017)
	0.039	0.278	0.063	0.109	0.080	0.016
	0.164	0.460	0.312	0.330	0.254	0.159
Observations		5,388,900			2,337,380	
Panel C: Effect on retirement exit rate of spouse $(t+1)$						
Ban effect at age 65	0.0002	-0.0067	-0.0037	-0.0018	-0.0013	-0.0015
	(0.0024)	(0.0025)	(0.0023)	(0.0029)	(0.0049)	(0.0036)
	0.933	0.019	0.136	0.546	0.801	0.687
	0.974	0.134	0.369	0.715	0.858	0.780
Observations		4,126,870			1,786,750	

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes: This table shows triple-difference estimates of the post-implementation effects of the bans at ages 65 and 66–68 on the retirement hazards of older workers and the exit rates from retirement of their spouse (from a version of equation 3.2 with spouse age-by-sex fixed effects). The sample is restricted to couples where only one individual is working and the other is aged less than 65. Estimates for reference individuals and spouses come from separate regressions. For outcomes measured in year $t+1$, we control for the effect of the bans when the reference individual is 64 years old. To increase power and facilitate comparisons of the direct and indirect effects, we calculate a weighted average of the estimates at ages 65 and 66–68 (with weights based on the share of reference individuals aged 65 and 66–68 in the sample).

Table A5: Heterogeneity in spillover effects on retirement hazard rates of working spouses by age of spouse

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Strongly affected industries		
	Age 65 effect	Age 66–68 effect	Weighted average	Age 65 effect	Age 66–68 effect	Weighted average
Panel A: Spouses aged 60–64						
Reference individual	-0.0456***	-0.0109**	-0.0256***	-0.0924***	-0.0293***	-0.0579***
	(0.0046)	(0.0022)	(0.0026)	(0.0091)	(0.0046)	(0.0048)
Cluster-robust p-value	0.000	0.000	0.000	0.000	0.000	0.000
Wild bootstrap p-value	0.000	0.042	0.004	0.004	0.007	0.000
Spouse	-0.0063	-0.0126	-0.0099**	-0.0120	-0.0240***	-0.0186***
	(0.0027)	(0.0027)	(0.0015)	(0.0047)	(0.0034)	(0.0031)
Cluster-robust p-value	0.036	0.001	0.000	0.025	0.000	0.000
Wild bootstrap p-value	0.170	0.106	0.036	0.139	0.008	0.000
Observations		6,661,960			2,667,270	
Panel B: Spouses aged < 60						
Reference individual	-0.0564***	-0.0210***	-0.0361***	-0.1060***	-0.0525**	-0.0764***
	(0.0069)	(0.0027)	(0.0028)	(0.0111)	(0.0056)	(0.0061)
Cluster-robust p-value	0.000	0.000	0.000	0.000	0.000	0.000
Wild bootstrap p-value	0.007	0.010	0.000	0.005	0.012	0.005
Spouse	-0.0042	-0.0021	-0.0030	-0.0069	0.0063	0.0004
	(0.0026)	(0.0019)	(0.0018)	(0.0058)	(0.0042)	(0.0035)
Cluster-robust p-value	0.126	0.294	0.122	0.254	0.161	0.912
Wild bootstrap p-value	0.357	0.631	0.408	0.409	0.297	0.903
Observations		5,643,580			2,293,380	

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes:

Table A6: Estimated spillover effects on retirement hazard rates of working spouses in the following year

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Strongly affected industries		
	Age 65 effect	Age 66–68 effect	Weighted average	Age 65 effect	Age 66–68 effect	Weighted average
Panel A: All spouses aged < 64						
Spousal spillover $(t+1)$	-0.0070	-0.0055	-0.0061	-0.0138**	-0.0060	-0.0096*
	(0.0025)	(0.0026)	(0.0024)	(0.0024)	(0.0039)	(0.0026)
Cluster-robust p-value	0.015	0.054	0.026	0.000	0.150	0.003
Wild bootstrap p-value	0.155	0.340	0.244	0.015	0.407	0.081
Observations		9,932,540			4,000,520	
Panel B: Spouses aged 59–63						
Spousal spillover $(t+1)$	-0.0106	-0.0087	-0.0095	-0.0245**	-0.0127	-0.0182**
	(0.0037)	(0.0026)	(0.0029)	(0.0034)	(0.0048)	(0.0034)
Cluster-robust p-value	0.015	0.006	0.007	0.000	0.020	0.000
Wild bootstrap p-value	0.167	0.166	0.169	0.011	0.116	0.024
Observations		5,878,530			2,350,900	
Panel C: Spouses aged < 59						
Spousal spillover $(t+1)$	-0.0013	-0.0006	-0.0009	0.0024	0.0038	0.0032
	(0.0025)	(0.0039)	(0.0027)	(0.0035)	(0.0063)	(0.0035)
Cluster-robust p-value	0.622	0.874	0.745	0.514	0.553	0.379
Wild bootstrap p-value	0.720	0.961	0.927	0.672	0.665	0.491
Observations		4,054,010			1,649,620	

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes: This table shows triple-difference estimates of the post-implementation effects of the bans at ages 65 and 66–68 on the retirement hazards of older workers' spouse in the following year (from a version of equation 3.2 with spouse age-by-sex fixed effects). The sample is restricted to couples where both individuals are working and the 'spouse' is aged less than 64. As outcomes are measured in year $t+1$, we control for the effect of the bans on the reference individual when they are 64 years old. To increase power and facilitate comparisons of the direct and indirect effects, we calculate a weighted average of the estimates at ages 65 and 66–68 (with weights based on the share of reference individuals aged 65 and 66–68 in the sample).

Table A7: Estimated spillover effects on retirement hazard rates of working spouses by gender of spouse

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Strongly affected industries		
	Age 65 effect	Age 66–68 effect	Weighted average	Age 65 effect	Age 66–68 effect	Weighted average
Panel A: Female spouses						
Reference individual	-0.0485***	-0.0150**	-0.0288***	-0.0992***	-0.0414***	-0.0665***
	(0.0050)	(0.0023)	(0.0027)	(0.0087)	(0.0046)	(0.0051)
Cluster-robust p-value	0.000	0.000	0.000	0.000	0.000	0.000
Wild bootstrap p-value	0.003	0.016	0.000	0.004	0.003	0.000
Spouse	-0.0065*	-0.0080	-0.0074**	-0.0107*	-0.0107	-0.0107
	(0.0019)	(0.0019)	(0.0009)	(0.0038)	(0.0040)	(0.0032)
Cluster-robust p-value	0.005	0.001	0.000	0.016	0.021	0.006
Wild bootstrap p-value	0.087	0.120	0.028	0.085	0.206	0.114
Observations		8,930,240			3,396,110	
Panel B: Male spouses						
Reference individual	-0.0567***	-0.0176	-0.0374***	-0.0891***	-0.0272	-0.0599*
	(0.0047)	(0.0072)	(0.0033)	(0.0125)	(0.0131)	(0.0118)
Cluster-robust p-value	0.000	0.030	0.000	0.000	0.060	0.000
Wild bootstrap p-value	0.003	0.148	0.004	0.006	0.136	0.064
Spouse	0.0033	-0.0083	-0.0024	0.0010	-0.0130*	-0.0056
	(0.0033)	(0.0027)	(0.0023)	(0.0050)	(0.0039)	(0.0028)
Cluster-robust p-value	0.337	0.009	0.315	0.843	0.006	0.067
Wild bootstrap p-value	0.394	0.158	0.348	0.843	0.066	0.187
Observations		3,375,300			1,564,530	

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes: This table shows triple-difference estimates of the post-implementation effects of the bans at ages 65 and 66–68 on the retirement hazards of older workers and their spouse (from a version of equation 3.2 with spouse age-by-sex fixed effects). Estimates for reference individuals and spouses come from separate regressions. To increase power and facilitate comparisons of the direct and indirect effects, we calculate a weighted average of the estimates at ages 65 and 66–68 (with weights based on the share of reference individuals aged 65 and 66–68 in the sample).

Table A8: Estimated effects of mandatory retirement bans on annual employment outcomes — DiD estimates at the province level by age

	(1) Employed	(2) Earnings	(3) Hired	(4) Separated
Panel A: People aged 20–34				
Mandatory retirement ban	-0.005	-1,739	-0.006	0.006
	(0.007)	(1,760)	(0.009)	(0.005)
Cluster-robust p-value	0.521	0.343	0.514	0.260
Wild bootstrap p-value	0.717	0.424	0.751	0.470
Mean	0.812	28,049	0.330	0.267
Panel B: People aged 35–49				
Mandatory retirement ban	-0.004	-1,417	-0.002	-0.001
	(0.004)	(1,624)	(0.004)	(0.004)
Cluster-robust p-value	0.394	0.400	0.740	0.821
Wild bootstrap p-value	0.759	0.517	0.861	0.965
Mean	0.777	44,234	0.169	0.135
Panel C: People aged 50–64				
Mandatory retirement ban	-0.016	-1,737	-0.002	-0.001
	(0.006)	(1,901)	(0.006)	(0.003)
Cluster-robust p-value	0.026	0.379	0.764	0.864
Wild bootstrap p-value	0.235	0.506	0.966	0.895
Mean	0.623	34,635	0.100	0.137

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$ based on wild cluster-bootstrap p-values. Standard errors in parentheses are clustered by province.

Notes: This table shows stacked differences-in-differences estimates of the effects of the mandatory retirement bans on the employment outcomes of workers at different stages of the life cycle. Estimates come from a modified version of equation (3.1) adapted to younger groups of workers. **We include controls for X, Y and Z.**

B Projected employment effects of higher pension ages

In this section, we project the effects of raising the claiming ages for OAS/GIS and full CPP/QPP benefits, which are both set at 65.[B1] We will then compare this projection's upper bound to our previous estimate that the mandatory retirement bans caused the average worker just below age 65 (i.e., at age "64.99") to work 3.6 months longer (0.30 years).

Our strategy draws on Rabaté et al.'s (2024) insight — based on a review of many studies in the literature — that the amount of bunching in retirement hazards at a pension age strongly predicts the employment effects of raising that age. We focus on the average retirement hazards at ages 61–68 between 2010 and 2018 in treated provinces (Figure B1a; black line).[B2] There is strong evidence of bunching in the retirement hazard in the year people turn age 65 and some evidence that the hazard at age 66 is also elevated. Relative to the retirement hazard at age 64 of 12.3%, the hazard at age 65 is 8.7 pp higher (21.0%), and the hazard at age 66 is 4.3 pp higher (16.6%). Thus, we estimate that around 12.1% ($0.121 = 0.087 + (1 - 0.21) \times 0.043$) of individuals who are still working at age 64.99 elect to retire due to the public pension age.

We project the employment effects of raising the public pension ages from 65 to 66. To do so, we assume that the retirement hazard at age 65 becomes equal to the hazard at age 64 (12.3%), with 'bunchers' delaying retirement by one year. Figure B1a shows the observed and projected retirement hazards at each age. The excess mass in the retirement hazard at ages 65 and 66 is shifted to the right by one year. Figure B1b shows the employment rates at each age implied by these hazards, based on the assumption that retirement is an absorbing state. Employment rises at ages 66–68 and is assumed to be unaffected at higher ages. We estimate that the average person working at age 64.99 works 1.7 months longer (0.14 years).[B3] Thus, an increase in the retirement age of 3.6 months (0.30 years) would require an increase in the public pension age of 2.1 years ($2.1 = 3.6/1.7$).

Our approach here likely produces an upper bound of the employment effects of raising Canada's public pension ages.[B4] First, it assumes that all of the bunching in retirement hazards

[B1] CPP/QPP benefits can be claimed from age 60 for a lower monthly benefit, but the 'normal' age is 65.

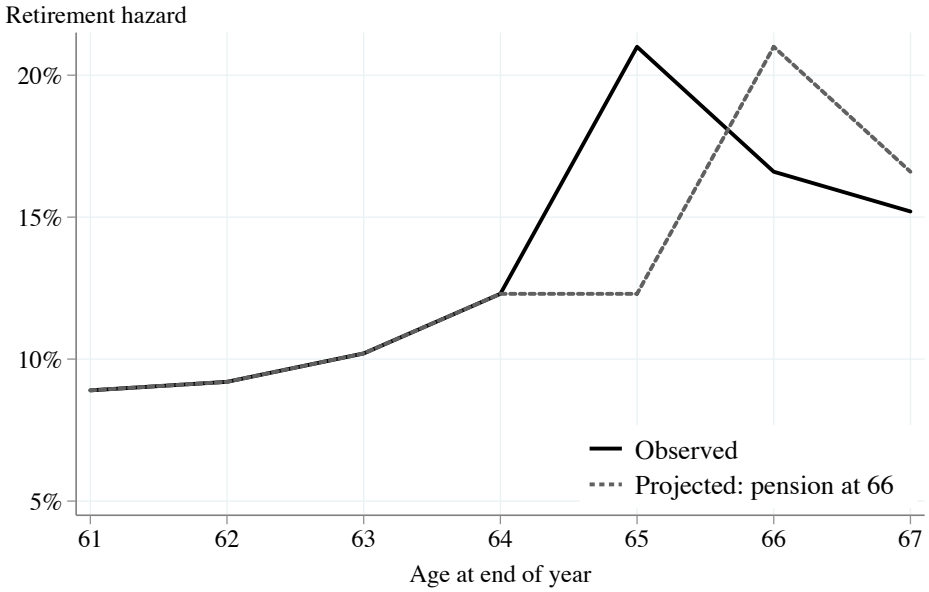
[B2] Rates are remarkably similar at each age in control provinces during this period (Figure 1a).

[B3] This can be calculated as the sum of the vertical distance between the two lines at each age.

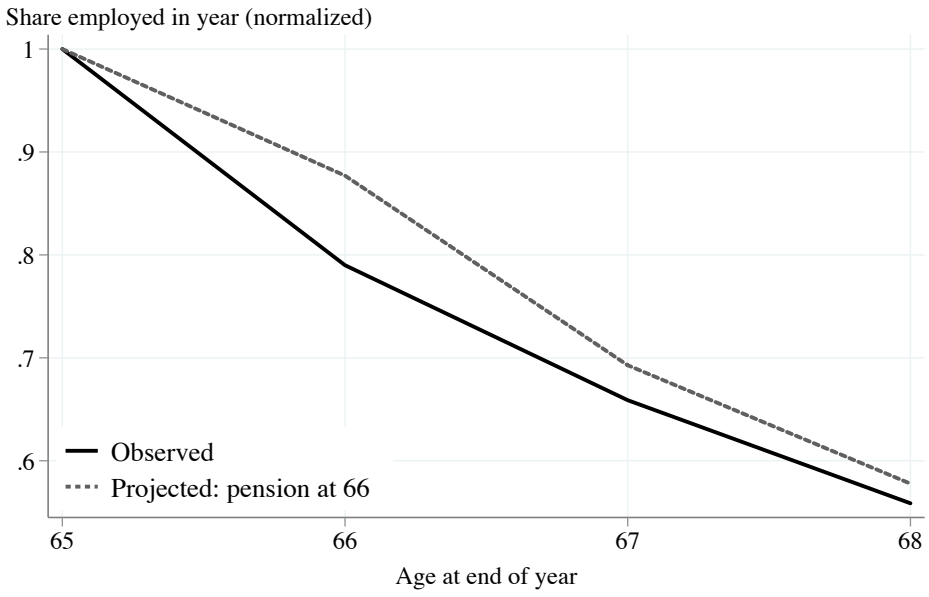
[B4] It is also possible that our approach could miss employment responses below age 65 (e.g., if someone delays retirement from 62 to 63) due to negative pension-wealth effects. However, most empirical studies of increases in pension-eligibility ages find little evidence of any employment effects below the existing age (e.g., Staubli and

Figure B1: Actual and projected changes in retirement hazards and employment

(a) Retirement hazards

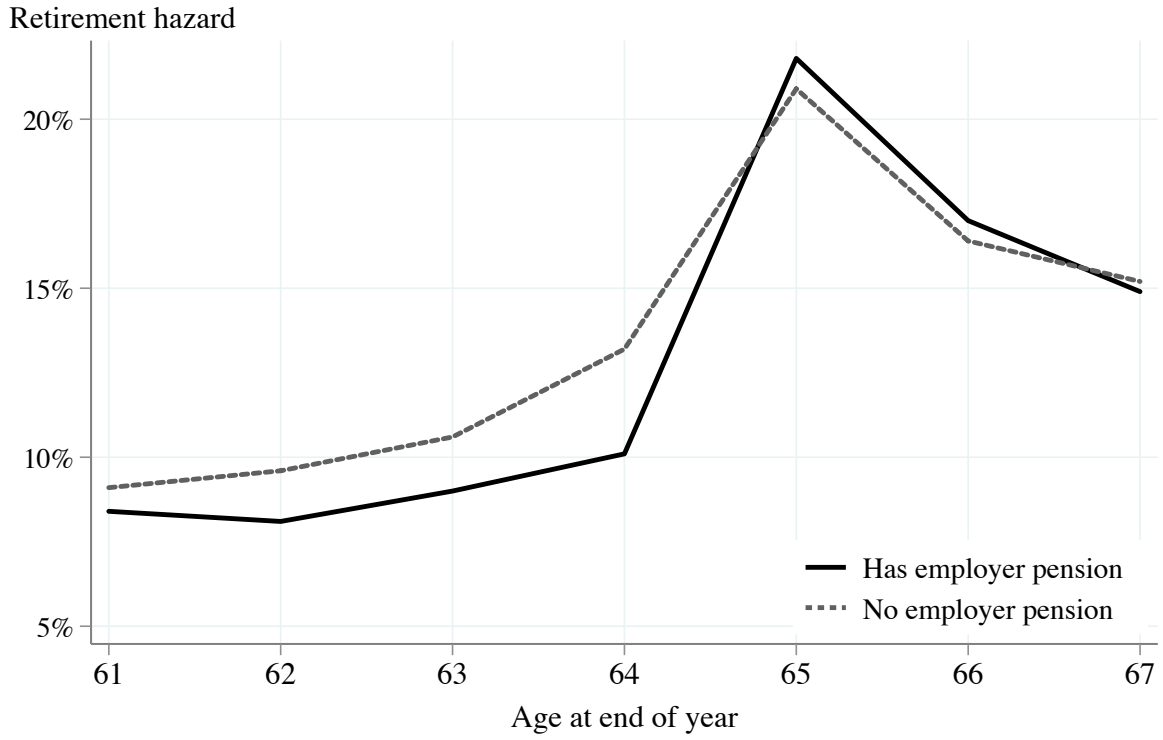


(b) Employment effects



at ages 65 and 66 is due to public pension ages. Other factors may be important, such as social norms and the fact that age 65 is the normal retirement age for most employer-pension plans. Indeed, workers who are not (currently) covered by an employer-pension plan show considerably less bunching at age 65 than those who are (Figure B2). Second, retirement hazards at age 65 may exhibit 'stickiness' even as public pension ages are increased (Deshpande et al., 2021). Third, retirement hazards typically increase with age, but we constrain the counterfactual hazard at age 65 to be the same as at age 64, and we do not allow for any 'catch up' in the hazard at ages 66 and 67. This means that our approach allows for a residual positive effect on employment *after* the new pension age, which is typically not found in empirical studies examining the consequences of higher pension ages.

Figure B2: Retirement hazards around age 65 by employer-pension coverage



Zweimüller, 2013; Cribb et al., 2016; Geyer and Welteke, 2021; Rabaté et al., 2024).

C Additional robustness checks for worker-level spillovers

C.1 Assessing the parallel-trends assumption

This section shows event-time plots for the results in Section 4. We focus on outcomes where we detect a statistically significant impact in Table 5 and restrict the sample to individuals in strongly affected industries to increase statistical power.[C1] We start by discussing the estimates for individual-level spillovers, before discussing the estimates for the spillovers on spouses.

Individual-level spillovers. Figure C1 shows the estimates of the effects at age 65 from equation (3.3). We note that, in contrast to the estimates for job separations and retirement hazards, many of our outcomes require information from both years t and $t-1$ (rather than t and $t+1$). This means that our sample effectively starts in 2002 for some outcomes (e.g., new pension claims), which is only three years before the first ban was announced in Ontario. Thus, for some outcomes we only present the event-study estimates for year -3 and later, as the earlier estimates are only identified by a subset of treated provinces and thus are difficult to interpret.

Figure C1a shows the estimates for new unemployment insurance (UI) claims. The estimates display a similar pattern to those for retirement hazards. There is no evidence of pre-trends before a clear and sustained decrease after the mandatory retirement bans are announced. The magnitude of the decrease is around 2 pp, similar to the estimate in column 2 of Table 5 (-1.7 pp).

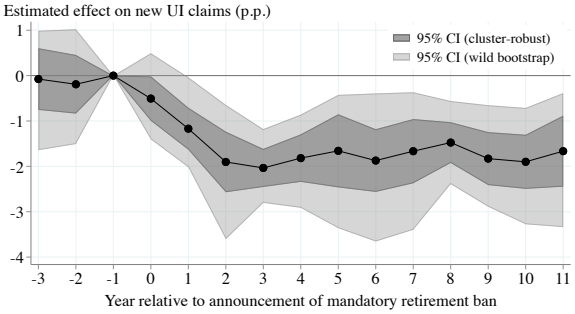
Figure C1b shows the estimates for transitions to self-employment. The estimates in years -3 and -2 are positive but marginally insignificant at the 5% level, as is the estimate in year 0. The estimates in subsequent years are smaller and mostly negative. These results are consistent with at most a modest negative effect on transitions into self-employment, as found in Table 5 (-0.33 pp).

Figure C1c and Figure C1d shows the estimates for employer-pension contributions and claiming. There is little evidence of pre-trends for either outcome and strong evidence of a large decline in final contributions and new claims following the bans. The magnitude of these effects is close to 10 pp, similar to the estimates in column 2 of Table 5.

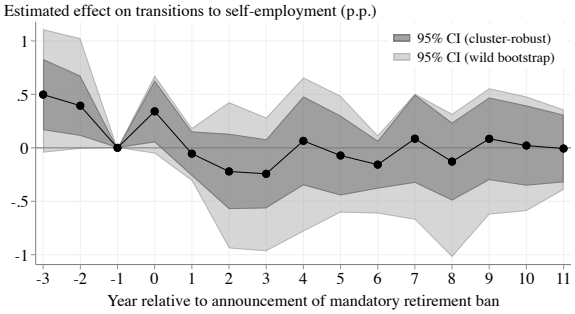
[C1] These industries include manufacturing; public administration; health care and social assistance; educational services; information and cultural industries; utilities; and mining, quarrying, and oil and gas extraction.

Figure C1: Event-study plots for individual-level spillovers at age 65 (workers in strongly affected industries)

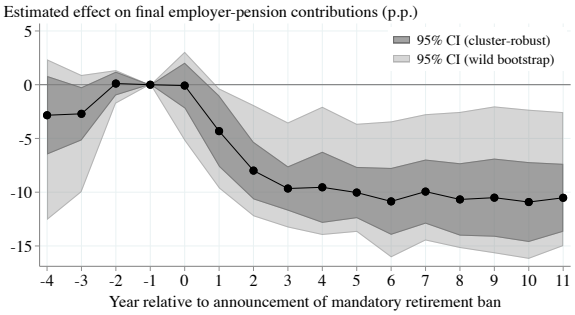
(a) New UI claim



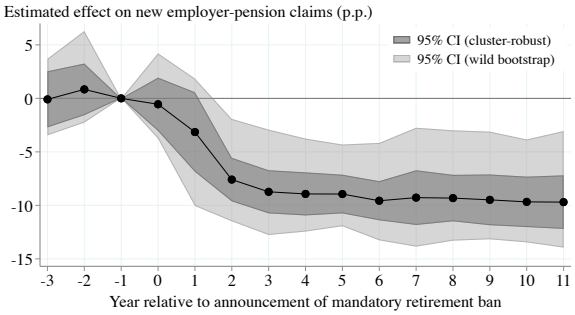
(b) Becomes self-employed



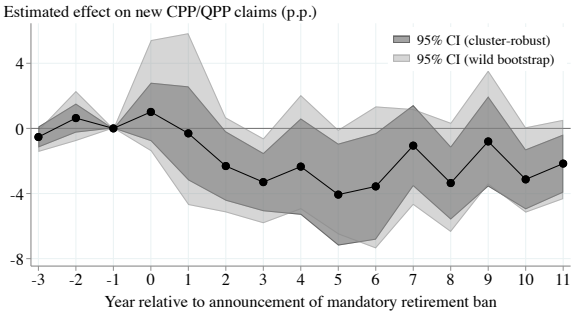
(c) Final employer-pension contribution



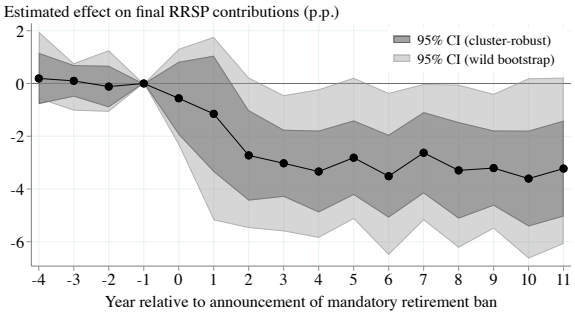
(d) New employer-pension income



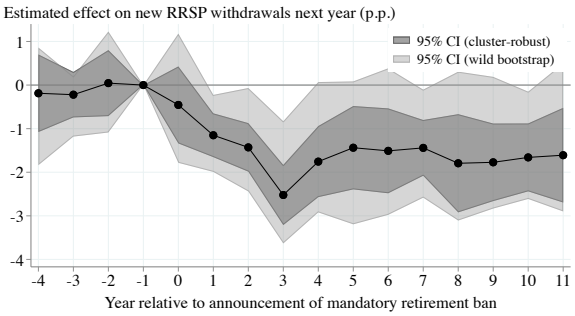
(e) New CPP/QPP claim



(f) Final RRSP contribution



(g) New RRSP withdrawal $(t+1)$



(h) New workers' compensation payment $(t+1)$

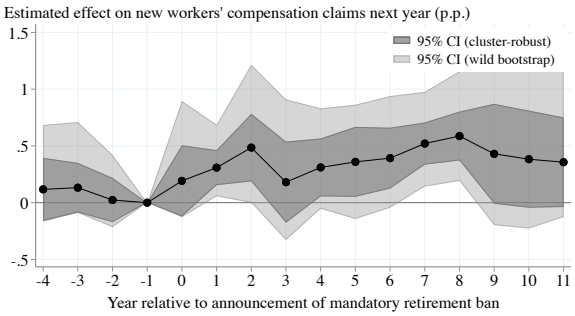


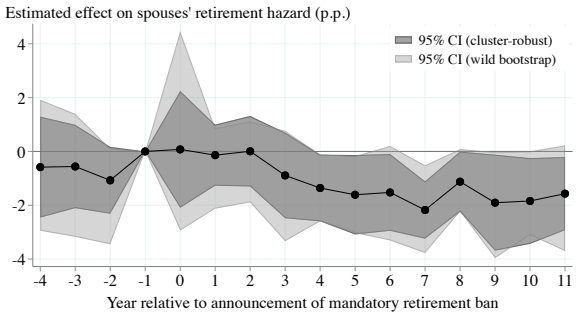
Figure C1e shows the estimates for new CPP/QPP claims. Again, the estimates display a similar pattern to those for retirement hazards. There is no evidence of pre-trends before a clear and sustained decrease after the mandatory retirement bans are announced. Similar patterns are observed for RRSP contributions and withdrawals (Figure C1f and Figure C1g). Final contributions to RRSPs at age 65 clearly fall after the mandatory retirement bans are announced, while new withdrawals also decline. The size of these effects is comparable to the estimates in Table 5.

Finally, Figure C1h shows the estimates for new workers' compensation claims (measured in year $t+1$). We observe stable pre-trends and positive estimates in the post-period, consistent with an increase in workplace injuries at age 66 as retirements are postponed. The size of the increase is approximately 0.4 pp, again similar to the estimate in Table 5 (0.34 pp).

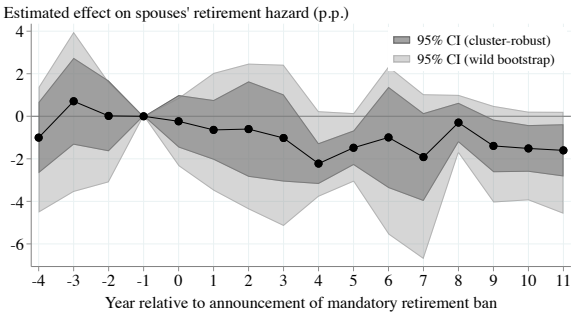
Spousal spillovers. Figure C2 shows the equivalent estimates for spouses' retirement hazards. We again restrict the sample to workers in strongly affected industries and then further restrict the sample to partnered individuals with a spouse that is younger than 65 and still working. Like in Section 5.3, we also include spousal age-by-sex fixed effects. We display the estimates of the spillover on spouses when the reference worker is aged 65 and 66–68, since Table 6 showed similar effects at both ages. Figure C2a shows that, at age 65, there is little evidence of pre-trends. In the post-period, the estimates decline and become statistically significant or marginally insignificant. We observe a similar pattern for the spillovers when the reference worker is aged 66–68. Overall, these estimates are consistent with the estimates in Table 6, which imply a decline in spouses' retirement hazards of around 1 percentage point at both ages.

Figure C2: Event-study plots for spillovers on spousal retirement hazard (reference workers in strongly affected industries)

(a) Reference individual aged 65



(b) Reference individual aged 66–68



D Appendix for firm-level analysis

D.1 Descriptive statistics

Table D1: Summary statistics — Balanced panel of firms in strongly affected industries

	Highly exposed firms		Moderately exposed firms	
	Treated provinces	Control provinces	Treated provinces	Control provinces
Firm characteristics and potential exposure				
Firm size	38.1	37.8	25.5	25.8
Workers aged 66–69	0.46	0.28	0.27	0.22
Workers' total union contributions	9,212	8,693	1,060	1,161
Workers' total employer-pension contributions	17,366	22,893	1,543	1,602
Annual hiring and separations				
Hires: Workers aged 20–34	10.4	12.3	6.0	6.5
Hires: Workers aged 35–49	7.1	6.8	3.9	4.0
Hires: Workers aged 50–64	3.5	3.0	1.8	1.9
Separations: Workers aged 20–34	5.0	5.3	3.1	3.1
Separations: Workers aged 35–49	4.2	3.7	2.4	2.4
Separations: Workers aged 50–64	2.4	1.8	1.2	1.3
Firms' financial outcomes				
Value added	1,512,054	1,341,992	1,145,897	1,054,351
Total revenue	2,788,977	2,329,059	2,478,410	2,192,864
Sales goods & services	2,600,088	2,017,759	2,406,594	2,120,609
Total cost of sales	1,281,868	1,037,646	1,431,420	1,283,864
Payroll costs	955,235	783,703	730,998	608,842
Total assets	1,743,922	1,482,387	1,452,075	1,444,705
Intermediate inputs	1,276,923	987,066	1,332,512	1,138,513
Gross profits	1,238,726	924,364	946,583	812,958
Industry shares				
Health care and social assistance	0.503	0.642	0.244	0.230
Manufacturing	0.367	0.280	0.637	0.655
Education	0.095	0.041	0.065	0.043
Information	0.014	0.017	0.034	0.035
Mining	0.009	0.011	0.018	0.034
Utilities	0.011	0.009	0.002	0.003
Unique observations	7,900	20,090	22,985	53,535
Unique firms	725	1,660	2,095	4,505

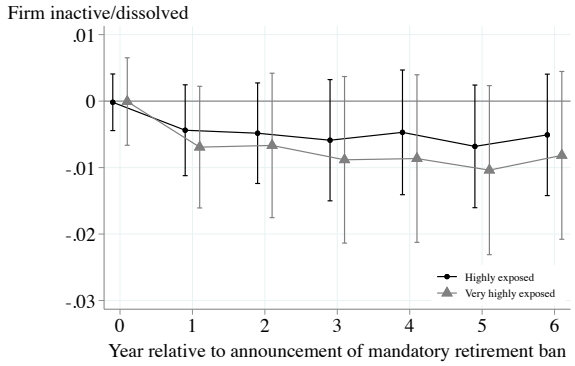
Notes: This table presents means of our annual firm-level financial outcome variables and industry shares for the firms in our analysis sample in Section 5, based on the firms' predicted exposure to the mandatory retirement bans (based on their total union/employer-pension plan contributions) and whether the firm is in a treated or control province.

D.2 Analysis of effects on firm survival

In this section, we assess the effects of the MR bans on firm survival. Specifically, we relax our sample restriction that firms must operate continuously with a minimum of five workers over event-years -4 to +6 around the ban announcement year, instead applying this restriction over event-years -4 to -1 only. Then, we estimate our two DDD models using two measures of firm shutdowns as a dependent variable: (i) firms that are 'inactive/dissolved' or (ii) have 'ceased operations'. The second outcome is more common, although both outcomes are rare, affecting less than 1% of firms. We find no evidence that shutdown probabilities differ for highly exposed (or very highly exposed) firms in treated provinces in the post-period relative to firms in control provinces and moderately exposed firms (Figure D1, Table D2). Figure D1 shows estimates of equation (5.2) with 95% confidence intervals, while Table D2 shows estimates of equation (5.1).

Figure D1: Estimated effects on firm shutdown probability in event-time

(a) Firm is inactive/dissolved



(b) Firm has ceased operation

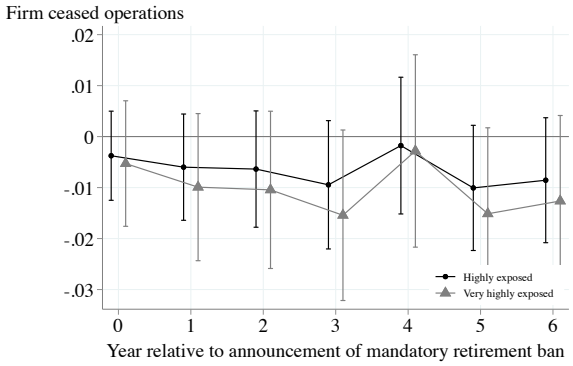


Table D2: Effects on firm shutdown probability after ban implementation

	(1) Inactive/ dissolved	(2) Ceased operation	(3) Inactive/ dissolved	(4) Ceased operation
Ban × High	-0.005	-0.007		
	(0.004)	(0.006)		
Ban × Very High			-0.008	-0.010
			(0.006)	(0.008)
Mean	0.002	0.004	0.002	0.004
R-squared	0.007	0.012	0.008	0.013
Observations	339,225	339,225	292,815	292,815

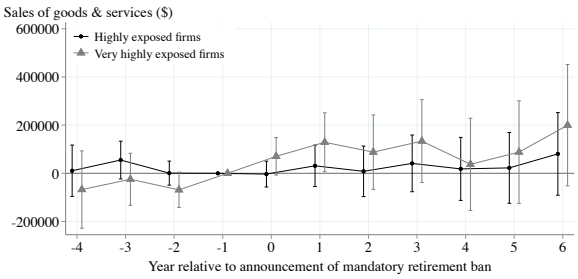
Notes: Standard errors in parentheses are clustered by firm.

D.3 Additional firm-level estimates

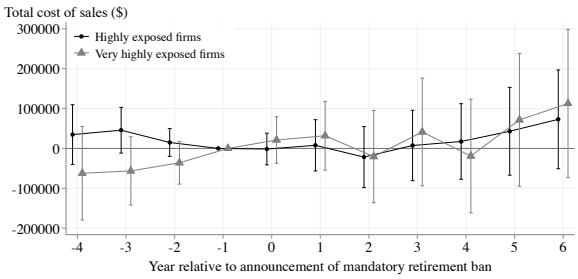
This section shows event-time triple-differences plots for the firm-level outcomes that are not shown in Figure 4. This allows us to assess the parallel-trends assumption and the dynamic responses to the bans from highly exposed firms. We also present triple-differences estimates from equation (5.1) for the age-specific employment variables (total employment and hires/separations).

Figure D2: Event-study plots for firm-level financial outcomes and employment

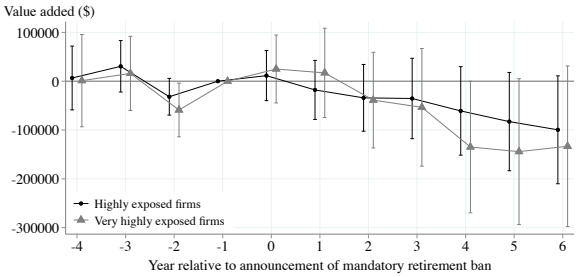
(a) Sales of goods & services



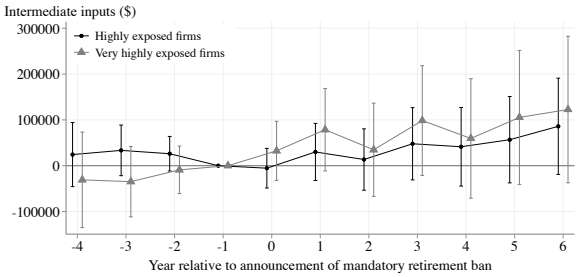
(b) Total cost of sales



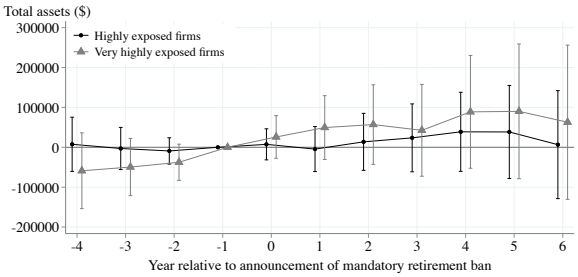
(c) Value added



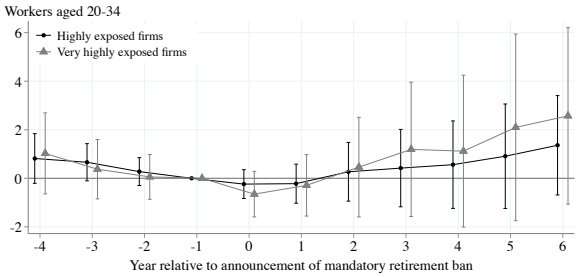
(d) Intermediate inputs



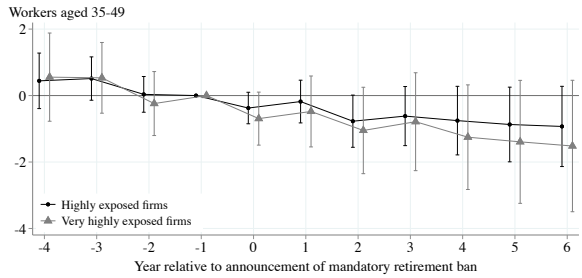
(e) Total assets



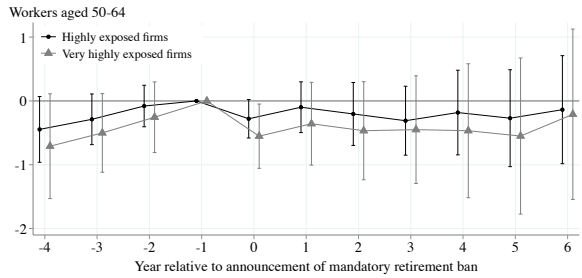
(f) Employment of young workers



(g) Employment of middle-aged workers



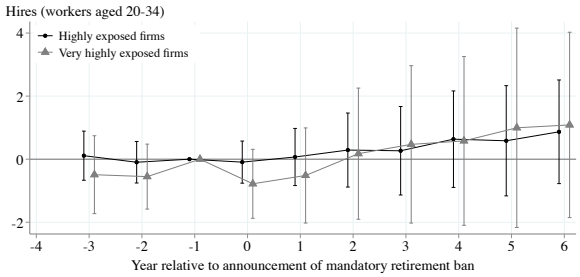
(h) Employment of older workers



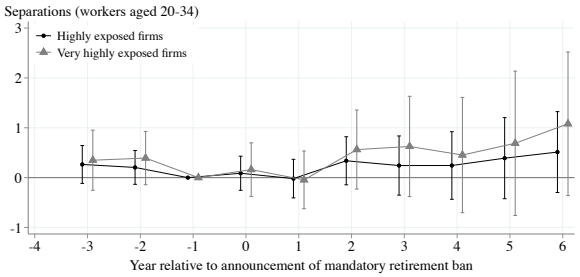
Notes: These figures show the estimates and 95% confidence intervals from equation (5.2). The estimates show the dynamic effects of the mandatory retirement bans on the outcomes of highly exposed firms in treated provinces relative to similar firms in control provinces and moderately exposed firms. The estimates shown in black use all highly exposed firms. The estimates shown in gray restrict the set of 'highly exposed' firms to those that are 'very highly exposed'.

Figure D3: Event-study plots for firm-level hiring and separations

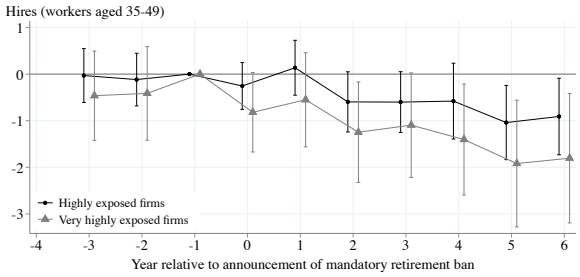
(a) Number of young workers hired



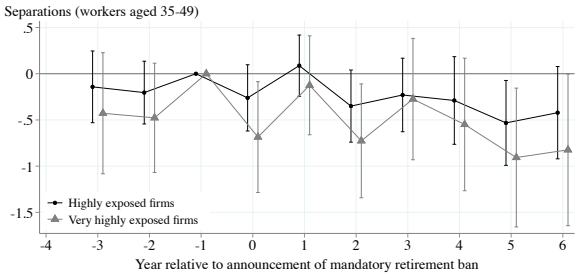
(b) Number of young workers separating



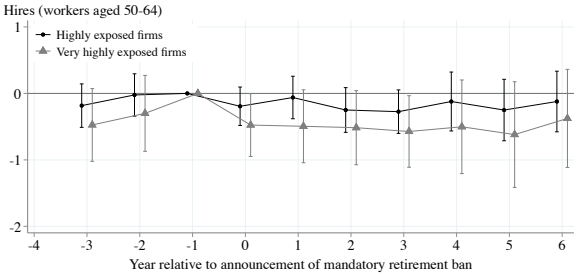
(c) Number of middle-aged workers hired



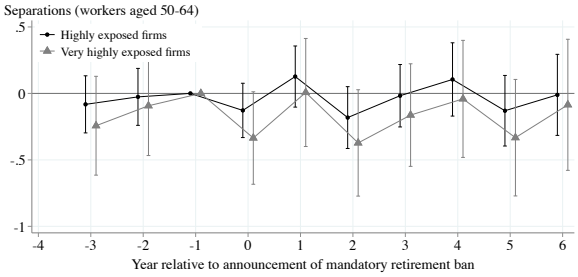
(d) Number of middle-aged workers separating



(e) Number of older workers hired



(f) Number of older workers separating



Notes: These figures show estimates and 95% confidence intervals from equation (5.2). The estimates show the dynamic effects of the mandatory retirement bans on the outcomes of highly exposed firms in treated provinces relative to similar firms in control provinces and moderately exposed firms. The estimates shown in black use all highly exposed firms. The estimates shown in gray restrict the set of 'highly exposed' firms to those that are 'very highly exposed'.

Table D3: Effects of mandatory retirement bans on highly exposed firms' broader personnel decisions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Number of workers			Number of hires			Number of separations		
	age <34	age 35–49	age 50–64	age <34	age 35–49	age 50–64	age <34	age 35–49	age 50–64
Panel A: Highly exposed vs moderately exposed (DDD model 1)									
Ban × High	0.25	-0.94*	-0.01	0.51	-0.61*	-0.11	0.14	-0.19	0.01
	(0.84)	(0.52)	(0.34)	(0.68)	(0.32)	(0.16)	(0.29)	(0.18)	(0.10)
Mean	13.68	13.59	9.85	10.00	6.57	3.64	5.01	4.04	2.49
R-squared	0.532	0.620	0.443	0.437	0.466	0.329	0.438	0.456	0.313
Observations	247,335	247,335	247,335	224,850	224,850	224,850	224,850	224,850	224,850
Panel B: Very highly exposed vs moderately exposed (DDD model 2)									
Ban × Very High	1.06	-1.34	-0.07	0.96	-1.14**	-0.26	0.36	-0.28	-0.05
	(1.48)	(0.87)	(0.54)	(1.22)	(0.55)	(0.28)	(0.52)	(0.30)	(0.17)
Mean	17.22	17.83	13.17	12.72	8.95	5.06	6.30	5.53	3.49
R-squared	0.522	0.628	0.454	0.429	0.477	0.338	0.427	0.459	0.315
Observations	211,825	211,825	211,825	192,570	192,570	192,570	192,570	192,570	192,570

*** denotes $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$. Standard errors in parentheses are clustered by firm.

Notes: This table shows triple-differences estimates from equation (5.1) of the post-implementation effects of the provincial mandatory retirement bans on highly exposed firms in treated provinces, relative to similar firms in control provinces and moderately exposed firms. High and moderate exposure is defined based on firms' employer-pension plan and union contributions, which is strongly predictive of the effects of the bans on job retention at age 65. Panel B reports estimates from the same regressions when the sample of 'highly exposed' firms is restricted to those that are 'very highly exposed'. The sample includes a balanced panel of firms that operate in strongly affected industries and have non-missing financial information over an 11-year period around the ban announcements.