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# Graying and staying on the job: The welfare implications of employment protection for older workers*

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**Abstract**

We study the welfare implications of employment protection for older workers, exploiting recent bans on mandatory retirement across Canadian provinces. Using linked employer-employee tax data, we show that the bans cause large and similar reductions in job separation rates and retirement hazards at age 65, with further reductions at higher ages. The effects vary substantially across industries and firms, and around two-fifths of the adjustments occur between ban announcement and implementation dates. We find no evidence that the demand for older workers falls, but the welfare effects are mediated by spillovers on savings behavior, workplace injuries, and spousal retirement timing.

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

**JEL Classification:** J26, J78, H55

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# 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. There is a clear opportunity for such interventions; for instance, the US has the weakest employment protection laws among OECD countries,[1] while many older Europeans are left without any form of employment protection due to the prevalence of mandatory retirement provisions.[2]

However, existing studies examining the overall employment effects of historical mandatory retirement policies in North America found minimal effects at best (Burkhauser and Quinn, 1983; Neumark and Stock, 1999; Shannon and Grierson, 2004).[3] In contrast, studies focusing on university professors found much larger effects (Ashenfelter and Card, 2002; Clark and Ghent, 2008; Warman and Worswick, 2010). Both sets of findings come with significant caveats. The former set of studies were hindered by limited policy variation and important data limitations, such as small sample sizes and imprecise information on age, while the latter were limited in scope, focusing on a specific and relatively rare occupation.

We revisit the effects of mandatory retirement policies in a contemporary North American setting. Between 2005 and 2009, five of Canada's ten provinces announced and implemented a ban on mandatory retirement, which had previously been possible at age 65. These bans applied across nearly all industries and brought the laws in these provinces, accounting for 60%

[1] Source: `https://stats.oecd.org/Index.aspx?DataSetCode=EPL_OV`.

[2] Countries with active mandatory retirement provisions include Germany, France, Ireland, Finland, Sweden, the Netherlands, Norway, Portugal, Iceland and Japan. These laws allow employers to fire workers above a given age (typically no younger than 65).

[3] Burkhauser and Quinn (1983) examine the retirement decisions of men around age 65 in the US prior to the nationwide increase in the mandatory retirement age from 65 to 70 in 1978, projecting that labor force participation would have been just 5% higher at age 65 without mandatory retirement. Neumark and Stock (1999) utilize variation in mandatory retirement ages across the US stemming from the 1978 increase, along with pre-existing state policies, finding no evidence of any employment effects at affected ages. Shannon and Grierson (2004) study the first mandatory retirement bans in Canada, likewise finding no employment effects.

of Canada's population, into line with other provinces. Our analysis leverages linked employer-employee tax records encompassing the universe of Canadian tax filers and firms from 2001 to 2019. During this period, mandatory retirement laws varied by age, year and province, enabling us to produce robust estimates of the policy effects using stacked difference-in-difference (DiD) and triple-difference (DDD) approaches.

Importantly, our linked data enables a comprehensive assessment of the effects of the bans. For instance, we evaluate the impact not only on overall employment and earnings but also on specific factors like job separations and hiring. Additionally, we investigate heterogeneity across industries and both between and within firms, exploring potential differences in industry norms, managerial decision making, and incentives to enforce mandatory retirement (Lazear, 1979). Finally, we examine an extensive range of spillovers on individuals and households — including unemployment insurance claims, transitions to self-employment, saving responses, health impacts, and changes in spousal retirement timing — all of which may mediate the welfare effects of the bans.

We thus provide the first comprehensive analysis of policies that completely ban mandatory retirement, building on recent studies utilizing administrative data that emphasize the importance of employers in the retirement decision (Frimmel et al., 2018; Rabaté, 2019; Rabaté et al., forthcoming; Deshpande et al., 2021). Particularly relevant is Rabaté (2019), which examines *increases* in mandatory retirement ages from 60 to 65 for specific industries in France using social security records for a 5% sample of private-sector workers. Rabaté (2019) estimates sizable employment effects but has to contend with a simultaneous pension reform, and the nature of the reform/data preclude analysis of general equilibrium impacts, heterogeneity across industries and within firms, and spillovers on other individual and household decisions.[4] The Canadian setting is novel and more compelling: provinces implemented mandatory retirement bans at different times; these bans were not associated with other changes to retirement policy; the bans affected nearly all industries and targeted workers at higher and more policy-relevant ages; and our linked employer-employee tax data is universal and comprehensive.

We start our analysis by exploring the effects of mandatory retirement bans on job sepa-

[4] Kondo and Shigeoka (2017) study a similar policy affecting all industries in Japan, documenting positive employment effects, but, like Rabaté (2019), face identification challenges due to concurrent reforms. Rabaté et al. (forthcoming) investigate a Dutch policy that increased a mandatory retirement age threshold, finding large employment effects, but they cannot separate the effects of this change from a delay in public-pension eligibility.

ration rates and retirement hazards. A simple graphical analysis uncovers clear policy impacts. Prior to the bans, treated provinces exhibited similar separation rates and retirement hazards at ages 61–64 to control provinces (those with existing bans), but significantly higher rates at age 65. These differences at age 65 vanish within two 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 led to a reduction in separation rates and retirement hazards at age 65 by six percentage points.

We also discover further negative effects at ages 66–68 — which increase the policy impacts by 50% — as well as an announcement effect. Each ban was announced between 6 and 26 months prior to implementation, and our estimates suggest that 40% of the decline in separation rates at age 65 occurs during these months. This observation aligns with weaker mandatory retirement enforcement following the ban announcements, a phenomenon consistent with an "expressive value of the law" (Sunstein, 1996; Posner, 2002; Benabou and Tirole, 2011; Acemoglu and Jackson, 2017; Moore and Morris, forthcoming).

We document considerable heterogeneity in the effects of the bans across industries and firms. Many industries show no change in separation rates at age 65, while others show reductions greater than 10 percentage points. The most affected industries are manufacturing; public administration; health care and social assistance; 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 higher rates of employer-pension coverage, higher wages, larger firms, higher rates of unionization and more workplace injuries. These associations are very strong and persist when we construct subgroups based on firm and worker characteristics. However, when we compare workers within the same firms, individual characteristics like earnings and previous workplace injuries do not prove to be important, suggesting that firms did not use mandatory retirement to systematically dismiss their least productive or most expensive older workers.

Our results indicate that the mandatory retirement bans caused sharp reductions in separation rates and retirement hazards at age 65. Thus, on first inspection, the increase in employment protection appears to significantly benefit older workers. However, this ostensibly positive outcome warrants further scrutiny: employment protection measures often have unintended consequences on the group that they are designed to protect (e.g., see Acemoglu and Angrist, 2001; Autor et al., 2006). In our context, firms may become more reluctant to hire older workers, or offer them lower wages, in response to the bans.[5] While similar concerns have been examined in the context of age-discrimination laws, with mixed findings (Neumark and Stock, 1999; Adams, 2004; Lahey, 2008), our data enables a more comprehensive analysis of general equilibrium effects on hiring. At the province level, 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 mandatory retirement was common. We also find no evidence of a decline in employment or earnings below age 65. For those above the age of 65, we estimate significant positive impacts. Specifically, employment rises by 14%, while earnings see an increase of 19% to 30% for those aged 66 to 69. This translates to an average worker approaching age 65 working 3.6 months longer and earning $20,000 more. These retirement delays resemble our own optimistic projections of a substantial labor supply intervention: raising Canada's public pension ages from 65 to 67. Moreover, the policies have different "compliers", which suggests that demand-side interventions can reach workers who are less responsive to pension ages.

The one-for-one link between separations and retirement is crucial in explaining the large employment effects of the policy. This link might arise from terminated older workers' lack of job search and/or from firms' reluctance to hire them (Neumark et al., 2019). Isolating these mechanisms is challenging, but we can gain some insight on job search intensity using information on unemployment insurance (UI) claims, which requires 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 likely holds some truth, as hiring rates for workers at age 65 do not change after the bans.

We examine spillovers of the postponed retirements on a variety of saving behaviors, two objective health measures, and the timing of spouses' retirements. Although there is a growing literature on the spillover effects of retirement,[6] most existing studies focus on only one of these

[5]The policy change may also improve perceptions about the capability of older workers, which could in turn reduce levels of age discrimination in hiring (Neumark et al., 2019).

[6]Examples include a handful of recent studies examining the spillover effects of pension reforms on saving

spillovers. This narrow focus limits our understanding of the overall welfare impacts. Moreover, nearly all studies exploit reforms aimed at the labor supply side (voluntary retirement) rather than the demand side (involuntary retirement).[7]

We uncover significant active and passive saving responses (Chetty et al., 2014). Before retirement, individuals reduce their disposable income — partly through passive contributions to private and public pension schemes and partly by actively contributing to individual retirement accounts. In retirement, they increase their income by claiming these pensions and drawing down their retirement accounts. These responses partially insure individuals against the income losses resulting from involuntary retirement; we estimate that at age 66, for every \$1 change in earnings, disposable income changes by just \$0.54. Saving adjustments account for three-quarters of this imperfect pass-through. Our findings imply that the welfare losses arising from involuntary retirement will be larger for passive savers and for workers not eligible for public or private pensions. Losses will also be larger in settings when involuntary retirement is unexpected.

To examine the effects on health, we estimate effects on workplace injuries (using income from workers' compensation) and all-cause mortality. We find a large increase in workplace injuries over age 65 but no short-term mortality effects. Our estimates imply an annual workplace injury risk of 3.7% for the affected workers, which means that the overall increase in employment due to the bans (207,000 person-years) has caused 7,600 more injuries. To our knowledge, these are the first estimates of a retirement policy on workplace injuries. Our estimates suggest that (i) workplace injuries may be a key mechanism for the short- and long-term health effects of retirement and (ii) heterogeneous injury risks across workers could explain some of the considerable variation in health effects estimated from different retirement policies.

Finally, we examine spillover effects on the retirement transitions of younger (not yet affected) spouses. Given that we exploit variation in *involuntary* retirements, our setting provides a unique opportunity to test the relative importance of two opposing sources of spousal spillovers: joint retirement preferences (Lalive and Parrotta, 2017; García-Miralles and Leganza,

(Lindeboom and Montizaan, 2020; Etgeton et al., 2023; García-Miralles and Leganza, forthcoming; 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, 2021; Lalive et al., forthcoming; Johnsen et al., 2022).

[7]Notable exceptions are Bíró et al. (2022) and Messacar and Kocourek (2019), who both focus on health outcomes (healthcare utilization and self-reported health, respectively). Using the same reforms as our study, Messacar and Kocourek (2019) construct an instrumental variable for involuntary retirement (based on cohort-by-province exposure) via a cross-sectional survey and find no significant effects on self-reported health measures.

2021; Johnsen et al., 2022) and *added worker effects* (Lundberg, 1985; Stephens, 2002; Autor et al., 2019; Fadlon and Nielsen, 2021). Our results reveal that preferences for joint retirement dominate. Among dual-earning couples, the spillovers amplify the direct effects of the bans by at least 20%. This proportion is at least as large as those found in studies utilizing pension reforms, which implies that the added worker effects are likely weak in our context. Such a phenomenon might be attributable to the substantial self-insurance facilitated by the saving responses.

# 2 Policy background

## 2.1 Mandatory retirement laws across Canada

In Canada, mandatory retirement referred to a sharp reduction in employment protection for workers reaching the age of 65. Under this policy, firms could terminate workers over this age without providing a cause or incurring termination costs. However, it remained legal for those workers to find other jobs or enter into new contracts with the same firm. Similar 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, some of which have been the focus of related studies (Rabaté, 2019; Kondo and Shigeoka, 2017; Rabaté et al., forthcoming), the Canadian labor market for older workers is closer to that in the US, with low levels of employment protection relative to other OECD countries and middling employment rates at age 55–64.[8]

Our administrative data starts in 2001, allowing us to focus on mandatory retirement bans enacted after this date. These bans were implemented in five out of Canada's ten provinces: Ontario, British Columbia, Saskatchewan, Nova Scotia, and Newfoundland and Labrador. Collectively, these five "treated" provinces make up 60% of Canada's population. The bans were legislated between December 2005 and May 2007 (Table 1). In each instance, there was a gap between the date the law was signed ("announcement date") and the date when mandatory retirement was prohibited ("implementation date"). This period ranged from 6 to 26 months.

The other provinces had already banned mandatory retirement by 2001. The earliest of

[8] In 2019, Canada ranked second lowest in the OECD (with the US the lowest) for the stringency of employment protection for individual and collective dismissals (https://stats.oecd.org/Index.aspx?DataSetCode=EPL_OV [Accessed August 11, 2023]). In 2022, both countries had the same employment rate at age 55–64 of 63.5%, which is also similar to the OECD average of 63.6% (https://data.oecd.org/emp/employment-rate-by-age-group.htm [Accessed August 11, 2023]).

these bans were enacted in New Brunswick in 1973, Manitoba in 1982, Quebec in 1983 and Prince Edward Island (PEI) in 1988.[9] More recently, mandatory retirement was abolished in Alberta in 2000. These five provinces, together with Canada's three territories (Nunavut, Yukon and the Northwest Territories), where mandatory retirement was never allowed, form our control group. Overall, we thus have five treated provinces and eight control provinces/territories.

A small fraction of workers in the treated provinces were not affected by the bans. Mandatory retirement was banned for workers in the federal public service (<2% of employment) in 1986. Similarly, about 6% of workers in very specific industries are covered by federal regulation, where mandatory retirement was abolished in December 2012.[10] Since these two groups of workers were not affected by provincial legislation, we do not consider them in our analysis.

### 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) firm-specific 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.[11] GIS is a supplement aimed at assisting lower-income retirees who receive OAS.[12]

The CPP/QPP functions as a defined benefit system similar to the Social Security system in the United States, 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

[9] In some cases (New Brunswick and PEI), these bans were relatively weak and contained numerous exceptions (see Messacar and Kocourek, 2019, for a discussion). In 2010, the bans on PEI were strengthened following a successful court challenge (Messacar and Kocourek, 2019). Although this could have led to a decrease in job separation rates at age 65, we observe no evidence of such a change relative to other control provinces, or when 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 exclude them from our sample.

[10] These industries include, for example, part of air, rail, road and maritime transportation, postal services, banks and telecommunications (see Appendix A.1 for a full list).

[11] 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).

[12] 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.

employee and employer.[13] 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 in Canada consists of firm-specific pension plans. Around 37% of Canadians are covered by an employer-pension plan, with the majority being defined benefit plans (Baldwin, 2015). Most of these plans have a stipulated retirement age of 65, while early retirement can be opted for at age 55. Unlike OAS and the CPP/QPP, benefits can only be accessed upon leaving the job.

The third and final pillar of the Canadian system consists of tax-favored retirement savings accounts. The most prominent among these is the Registered Retirement Savings Plan (RRSPs), which is similar to the Individual Retirement Account in the US. 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).

Given the incentives in the first and second pillars, it is evident that a significant number of Canadians may chose to retire voluntarily at age 65 — coinciding with the age that mandatory retirement was possible. Fortunately, variation in mandatory retirement 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.[14] Importantly, all three policies were instituted years after the last mandatory retirement ban was announced in 2007 and likely exerted a marginal effect on retirement timing in our sample frame. We account for the impact of these policies in our regressions and verify that the results remain stable even if Quebec is excluded. The consistency in our estimates both before and after 2011 further underscores the minimal relevance of these policy differences.

[13]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).

[14]The first policy increased the size of the penalty/bonus for claiming the CPP/QPP before/after age 65 (Glenzer et al., 2023). 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.

# 3 Data and empirical strategy

## 3.1 Data, sample restrictions and outcome variables

We use longitudinal linked employer-employee tax data from the Canadian Employer-Employee Dynamic Database (CEEDD), a comprehensive data set covering the universe of tax filers in Canada from 2001–19.[15] Important information includes an individual's year of birth (and death, if applicable), income from a variety of sources — including spouses' incomes — and income statements from each employer an individual had over the year.[16]

We start our analysis by examining effects on job separations and retirement. We anticipate that mandatory retirement laws will directly affect job separations, and we are interested in the extent to which changes in separation rates affect decisions to retire. To study 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).[17] We define a separation in year $t$ for worker $i$ if they are no longer working for their employer in year $t+1$.[18] We define a retirement in year $t$ for worker $i$ if they have 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 during that year.

The final labor market outcomes we consider are employment and total earnings (both measured in year $t+1$).[19] 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.

[15] We found no evidence that tax-filing itself varies with age or mandatory retirement laws.

[16] Like most data from administrative sources, we do not have information on education, occupation or the intensity of work.

[17] We exclude individuals who are employed in the aforementioned industries not covered by provincial regulation (see Appendix A.1). We also exclude individuals who passed away in year $t$ or $t+1$ or are not observed in the data in both years for other reasons (e.g., migration).

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

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

We consider various spillovers at the individual level by linking information from their tax returns. Utilizing the same sample as before, 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. Furthermore, we also 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/active decisions to stop making contributions for one's retirement. Finally, we study two health outcomes: new workers' compensation payments (which provides a proxy for workplace injuries) and all-cause mortality. For these last outcomes, our sample is broadened to include (about 1% of) workers in year $t$ who die in years $t$ or $t+1$.[20]

Having assessed the effects of the bans on individuals, we next turn our attention to the retirement decisions of couples. Our focus is on couples in which at least one partner is part of our primary sample. We then narrow down the sample to include only those cases where the other partner is no older than 64. This criterion ensures that the spouse has not (yet) been affected by mandatory retirement laws, enabling a clean estimation of any potential spillover effects on their retirement transitions.

### 3.2 Graphical evidence

Figure 1 graphically displays the effects of mandatory retirement 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

[20]When someone dies, their legal representative must file a final income tax return for the deceased and report their year of death.

comparisons suggest that the mandatory retirement bans have reduced separation rates and transitions into retirement for workers aged 65 and older. The most substantial change is evident at age 65, the initial age when employers could dismiss workers. The magnitude of the effects are similar for both outcomes, hinting that most workers who are dismissed under mandatory retirement rules retire instead of finding another employment opportunity.

In Panel (b), we provide additional evidence showing how these outcomes have evolved over time by province and age. We plot the average separation rates and retirement hazards 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 reform is announced in a specific province. We then average the rates at each event year across workers in all provinces, which gives more weight to the trends in more populous provinces. 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.[21] 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 higher. However, the downward trends are similar between treated and control provinces. 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 around two years, which is to be expected given that (i) the announcement period for these reforms varied from 6 to 26 months and (ii) the full effects of the mid-year bans will not show up in our annual data 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 mandatory retirement 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

[21]The treated province determines the reform year, and then we take the average separation rates at a given event year across all observations in the control group (including duplicates).

DDD models combine these three age groups and use the effects of the mandatory retirement 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 mandatory retirement 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. 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). To circumvent this challenge, we adopt the stacking approach employed by Cengiz et al. (2019). This approach restricts identification to comparisons between treated and control provinces and is compatible with both binary and continuous treatments.

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 our outcome variable (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 (for the primary job) and $t$ year. The sample is stacked with five identical observations for individuals in control provinces. This explains the data set index $d$, which is a categorical variable that links the control observations to a single treated province.[22]

Within each data set, we include fixed effects for each age-province ($\theta_{dap}$) and age-year ($\gamma_{dat}$) cell. These account respectively for any differences across provinces in the age patterns at which workers retire and any Canada-wide trends in retirement behavior that may affect workers of a particular age. The variable $X_{pat}$ includes explanatory variables accounting for any other time-varying policy differences between Quebec and the rest of Canada that may differentially affect retirement hazards by age and province.[23] $\epsilon_{diapt}$ is the error term.

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

[23] $X_{pat}$ 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 mandatory retirement ban (see footnote 9). Overall, the estimates are very similar with and without these controls.

The key variables of interest are $\text{Ban}_{pt}$ and $\text{Ann}_{pt}$. $\text{Ban}_{pt}$ is defined as the proportion of year $t$ during which mandatory retirement is banned in province $p$, while $\text{Ann}_{pt}$ is the proportion of the year that a reform banning mandatory retirement has been announced but not yet taken effect. The inclusion of $\text{Ann}_{pt}$ allows for an "expressive value of the law", where the law change itself may affect social norms or preferences about the enforcement of mandatory retirement. The coefficients of interest are $\beta$, which identifies the effect of the bans after implementation, and $\delta$, 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 mandatory retirement 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 mandatory retirement 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, since 65 is the first age that firms could enforce mandatory retirement. We also anticipate a negative effect for ages 66–68, because (i) firms may value the option to enforce mandatory retirement at a later date (e.g., in response to diminishing worker productivity or worsening economic outlook) and (ii) historical data suggests that many collective agreements with mandatory retirement provisions stipulated retirement ages higher than 65 (Gunderson, 1987).[24]

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., 2019).[25,26] Considering the small number of clusters, this method

[24] Gunderson (1987) used data drawn from major collective agreements in Ontario in 1979. In these agreements, compulsory retirement with the option of being retained on a new contract was much more common than automatic retirement with no possibility of renewal. This suggests a third reason that separation rates may be higher at ages 66–68 when mandatory retirement is allowed: lower worker satisfaction with (renegotiated) contracts.

[25] We implement this approach with the `boottest` command in Stata. We also jackknife the bootstrap data-generating process as suggested by MacKinnon et al. (2023).

[26] Our main regressions are estimated at the individual level using the `reghdfe` Stata command (Correia, 2019). However, this is currently incompatible with `boottest` when multiple sets of fixed effects are absorbed. To solve this issue, we collapse the data to the dataset-age-province-year level and estimate our models using weighted least squares, absorbing only the dataset-province-age fixed effects and estimating the dataset-province-

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 equation (3.1). The key distinction from equation (3.1) is the addition of interaction terms for the $\text{Ban}_{pt}$ and $\text{Ann}_{pt}$ terms with an age-65 dummy variable and an additional dummy variable for individuals aged 66–68 years old. Their coefficients are interpreted as the differential effects of mandatory retirement 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 mandatory retirement bans and province-year factors affecting retirement behavior (e.g., differences in exposure to the Great Recession across provinces). Second, the DDD estimates tend to be more precise.

# 4 Effects on labor market outcomes

## 4.1 Main results

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 p.p.) and 66–68 (-0.8 p.p.). 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 p.p. and -1.0 p.p.) but more precise, with the effect at ages 66–68 becoming statistically significant at the 5% level.

year fixed effects. This yields identical point estimates and allows us to run the `boottest` command to compute the bootstrapped confidence intervals and $p$-values.

Since the estimated effect at age 65 is much larger than at 66–68, our estimates suggest that the mandatory retirement bans mainly reduce job separations at the earliest possible age. However, the estimate at ages 66–68 suggests that the delayed effects increase the effects on separations by around 50% ($0.5 \approx 3 \times 1.0/5.9$).

Interestingly, the DDD estimates of the announcement effects are also negative at ages 65 and 66–68, which suggests that part of the response occurs between the ban's announcement and implementation dates. At age 65, the estimate is -2.5 percentage points ($p = 0.049$), or 40% of the post-implementation effect. This is consistent with a change in social norms regarding the enforcement of mandatory retirement following the announcement of the bans.

In columns 5–8, we present the corresponding estimates for retirement hazards. The sign and magnitude of these estimates mirror those for separation rates. This indicates that nearly all of the workers who are fired at age 65 under mandatory retirement rules retire from paid work 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 average 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 mandatory retirement provisions in explaining the amount 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.

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 $\beta_k$ 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.[27]

[27] Figure A1 plots the estimates of the $\delta_k$ 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 mandatory retirement enforcement.

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

$$y_{diapt} = \sum_{\substack{k=-6 \\ k\neq -1}}^{13} \mathbf{1}(\text{Tr\_event\_year}_{pt} = k) \left\{\beta_k + \beta_{k,65}\mathbf{1}(\text{Age}_a = 65) + \beta_{k,66to68}\mathbf{1}(\text{Age}_a \in [66, 68])\right\} + \theta_{dap} + \gamma_{dat} + X_{pat} + \epsilon_{diapt} \tag{4.1}$$

where $\mathbf{1}(\text{Tr\_event\_year}_{pt} = k)$ is a set of event-time dummies for the treatment group, equal to one if it is $k$ years since a law banning mandatory retirement was announced. The baseline $\beta_k$ coefficients show 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 contains all treated provinces). There is no evidence of pre-trends and a clear and persistent decline in separation rates and retirement hazards at age 65 after mandatory retirement is banned in the treated provinces. It takes around two years for the full effects to be observed.[28]

The event-time estimates in Figure 2b 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). However, the estimates 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 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.

## 4.2 Additional robustness checks

We verify the robustness of our estimates in other 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

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

provinces like Ontario (39% of Canada's population) and British Columbia (13%) than other treated provinces (7% combined).[29] This is a deliberate choice, as we wish to understand the average effects of the mandatory retirement bans over our sample period on affected workers.[30] 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).[31] 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.

### 4.3 Effect heterogeneity

**Industry of employment.** We assess heterogeneity in the post-implementation effects on separation rates at age 65 across industries by estimating our DDD model for each two-digit industry (Figure 3a). We observe notable disparities across industries, with the largest effects in the following seven industries: 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 industries comprise 42% of the older workforce but account for 77% of the overall effect. The estimated average treatment effect for these workers (-10.7 p.p.) is five-times larger than for workers in the remaining 13 industries.

In Figure A6, we show how the size of the estimates correlates with various industry characteristics. The treatment effects are significantly larger in industries with higher rates of pension coverage (Figure A6a); larger organizations (Figure A6b); higher wages (Figure A6c); higher rates of unionization (Figure A6d); longer job tenures (Figure A6e); and higher injury risks for older workers (Figure A6f). However, there is little evidence of any correlation between the size of the effects and underlying job separation rates at age 65 (Figure A6g), nor the percentage

[29]Stacked regressions also give more weight to the effects in provinces where there is more variation in laws (i.e., when bans occur closer to the middle of the panel), but in our case the variation in ban-timing is modest.

[30]Nonetheless, we show how the estimates differ with equal weighting (Figure A4). In most cases, the estimates are slightly larger (due to larger estimates in smaller provinces).

[31]Specifically, we drop treated provinces and assign 3 out of 8 of the control provinces/territories as 'treated'. For each 'treated' province, we randomly assign treatment timing (ban announcement and implementation dates) by matching it to one of the 3 largest treated provinces (Ontario, British Columbia and Saskatchewan) without replacement. Then we create a stacked sample and estimate our triple-difference model (equation 3.2). We do this for the 56 possible combinations of the 3 'treated' provinces, saving the point estimates and t-statistics for each regression (MacKinnon and Webb, 2020).

difference between the average earnings of older and younger workers (Figure A6h).

These results suggest that institutional norms and structures are important determinants of whether firms enforce mandatory retirement on older workers. First, the correlation between the treatment effects and employer-pension coverage is particularly strong, which suggests that mandatory retirement policies and pension provision are jointly determined (Burkhauser and Quinn, 1983); it is also consistent with some pension plans having rules that require workers to leave the firm at age 65 (Gunderson, 2004). Second, the positive correlation with unionization is consistent with historical data that showed the presence of mandatory retirement provisions in many collective agreements (Gunderson, 1987). This correlation suggests that unions may be willing to accept mandatory retirement in exchange for higher wages and pension coverage.

While our results emphasize the importance of institutional norms and structures, it is somewhat inconclusive to what extent they support Lazear's (1979) model of mandatory retirement as a tool that facilitates long-term contracts with deferred compensation profiles. We observe a modest positive correlation between the treatment effects and average job tenure, and there is no correlation with the age-wage gradient. However, the latter analysis does not account for deferred compensation through pensions. Moreover, the age-productivity gradient is unlikely to be identical across industries, so we cannot rule out larger wage-productivity gaps at older ages in the more affected industries. There is a strong positive association between the treatment effects and the workplace injury risks of older workers, which suggests that workers in these industries have more physical jobs. It is plausible that employers observe or perceive an age-productivity gradient that is negative (or less positive) in such jobs near retirement.

**Observable worker and firm characteristics.** To assess the importance of observable worker and firm characteristics on separation rates, we estimate our DDD model for various subgroups and focus on the post-implementation effect at age 65. In terms of firm characteristics, we observe larger effects for firms with a relatively low share of workers over the age of 50 (Figure 3b). The composition of these firms may indicate the presence of less favorable views about the productivity or economic value of older workers, so it is not surprising that they are more likely to enforce mandatory retirement. In terms of workplace safety, we observe considerably higher impacts in firms with above-median rates of workplace injuries (based on workers' compensation payments). This reiterates the physical nature of affected workers' jobs.

In terms of firm size and pension coverage, the results confirm the correlations at the industry level; the effects are clearly increasing in firm size and for firms that cover more of their workers with employer-pension plans. Interestingly, we observe no difference in the effects for firms that are expanding or contracting, suggesting that firms do not use mandatory retirement provisions to lay off workers when times are tough. Finally, we observe no correlation between the average earnings gap for older and younger workers at the firm, suggesting that, to the extent that mandatory retirement laws allow firms to write deferred compensation contracts (Lazear, 1979), firms do so via pensions rather than salaries.

In terms of worker characteristics, we find a strong positive association between the size of the effects and individual employer-pension coverage (Figure 3c). The estimated effect on separation rates at age 65 is -16.3 p.p. for the 24% of workers that are currently covered by an employer-pension plan compared to -3.4 p.p. for the other 76%. Higher earners are also more strongly affected (e.g., the estimate is -10.0 p.p. for the top 25% of earners compared to -0.4 p.p. for the bottom 25%). There is suggestive evidence that the effects are larger for native-born Canadians than immigrants, while the effects do not differ significantly by gender.

While Figure 3c shows considerable differences in the post-implementation effects of the mandatory retirement bans based on worker characteristics, it is unclear to what extent this heterogeneity stems from differential worker sorting across firms versus systematic decisions by firms to enforce mandatory retirement for only certain types of older workers (e.g., the least productive or most expensive). If it is the latter, the bans may be particularly costly for firms, since they may be forced to retain less efficient workers (Gibbons and Katz, 1991) and the increase in employment protection may encourage shirking (Ichino and Riphahn, 2005).

To shed some light on this, we construct subgroups based on selected individual worker characteristics (earnings and prior workplace injuries) that account for the average characteristics of older workers at the firm.[32] Figure A7 shows no evidence that the treatment effects vary between these subgroups, which suggests that firms were not systematically firing their least productive or most expensive older workers.

[32]Specifically, we divide older workers into earnings quartiles based on the within-firm earnings distribution of all older workers at the firm (over the entire sample period). Similarly, we compare workers who were receiving workers' compensation payments last year to workers who were not, dropping workers in firms that never had at least one older worker that received workers' compensation.

### 4.4 General equilibrium effects on hiring

Several studies suggest that, in general equilibrium, employment protection measures can have unintended consequences on the group that they are designed to protect (Acemoglu and Angrist, 2001; Autor et al., 2006; Sestito and Viviano, 2018; Bamieh et al., forthcoming). In our context, firms may become more reluctant to hire older workers in response to the bans on mandatory retirement. Leveraging policy variation across provinces, our DiD regressions allow us to assess whether mandatory retirement laws affect the 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.

The results in Table 3 show no evidence of any effect on hiring rates at any age with either model.[33] 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 percentage point (14%). 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.

### 4.5 Net effects on 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 using equations (3.1) and (3.2) for individuals who were employed at age 61. The estimates indicate that the mandatory retirement bans have increased employment at ages 66–69 by 14% and earnings by 19–34% (Table 4).[34] There are no effects at ages 61–64.

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 an additional 3.6 months (0.30 years) because of the mandatory retirement bans.[35] 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 $).

[33] We continue to estimate null effects even if we only count hires into the seven industries that are most affected by the mandatory retirement bans or restrict the sample to people who were working at age 61.

[34] 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.

[35] This is a lower bound, since it assumes that there is no persistence in the employment effects beyond age 69.

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 (forthcoming) 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.[36]

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 mandatory retirement 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 public pension claiming ages.

# 5 Spillovers on self-insurance behaviors, health and spouses

In this section, we examine various spillover effects of the bans on individuals and households. These effects are not only informative about the overall effects of the bans but also on the mechanisms through which they impact welfare. Section 5.1 examines various self-insurance behaviors by older workers, including unemployment insurance claims, transitions to self-employment and saving responses. Section 5.2 sheds light on health effects. Section 5.3 investigates the effects on spousal retirement timing, which is informative about spousal-insurance mechanisms and preferences for joint retirement. Appendix C contains additional results that validate the main findings: event-time estimates from equation (4.1) and estimates with alternative data sources.

## 5.1 Self-insurance mechanisms

**Unemployment insurance claims.** Our results suggest that banning mandatory retirement caused almost identical reductions in separation rates and retirement hazards. Essentially, this means that none of the workers who are fired at age 65 find a new job. This result could re-

[36] These estimates are likely an upper bound since they do not account for other causes of bunching at age 65. In particular, employer-pension plans typically have a normal retirement age of 65, and workers covered by such plans display considerably more bunching (Appendix Figure B2). Retirement may also exhibit stickiness at age 65 when pension ages are increased (Deshpande et al., 2021).

flect either a lack of job search by 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 mandatory retirement bans on new unemployment insurance (UI) claims, which requires individuals to actively look for work.

We estimate equation (3.2) for our main sample and focus on the post-implementation effect on new claims at age 65. We estimate that the bans decrease new UI claims among individuals who are still working in the year they turn 65 by 0.9 percentage points (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 percentage points ($p = 0.008$).[37] Overall, these estimates suggest that approximately 16% of workers who are terminated at age 65 actively search for a new job. These workers appear to have little success, given the absence of any effect on hiring rates at age 65.[38]

Our results indicate an apparent contradiction in older workers' labor supply decisions: mandatory retirement bans have strong effects on job separation rates at age 65, indicating that older workers are keen to extend their careers 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 norms about the "appropriate" time to retire. The role of norms in retirement decisions is underscored by several studies showing bunching in retirement and pension claiming decisions even when there is little economic incentive to do so (Behaghel and Blau, 2012; Cribb et al., 2016; Seibold, 2021; Deshpande et al., 2021; Glenzer et al., 2023).

**Transitions into self employment.** For older workers faced with termination, those who do not wish to retire may either look for a new job or start a business. However, unlike UI

[37]This near-doubling of the estimates is proportional to the change in the estimates for separation rates when we move from the full sample (-5.9 p.p.) to the "strongly affected industries" (-10.7 p.p.). 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.

[38]Throughout Section 5, we focus our discussion on the estimates of our main DDD model (equation 3.2) for all workers and workers in strongly affected industries. Appendix C.1 presents estimates of our event-time model for workers in strongly affected industries (equation 4.1). These estimates show little evidence of pre-trends and post-ban dynamics that are consistent with the main estimates.

claims, which normally occur soon after job loss, starting a business may take more time. Thus, we study the effects of the mandatory retirement 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 p.p.), 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). To sum up, our estimates suggest that around 6% of terminated workers transition into self-employment within one year.

**Pension claiming and savings behavior.** Despite the importance of retirement decisions in a life-course perspective, we have surprisingly little evidence about the effects of changes in retirement timing on pension claiming and savings decisions. A handful of recent studies offer exceptions (Lindeboom and Montizaan, 2020; Etgeton et al., 2023; García-Miralles and Leganza, forthcoming; Nakazawa, 2022), with most of these studies relying on survey data and all focusing on pension reforms affecting labor supply. In contrast, we use administrative tax data to study a policy influencing labor demand and investigate a wide range of active and passive savings decisions (Chetty et al., 2014).

Contributions to an employer-pension plan at age 65 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.[39] In contrast, the decision to claim CPP/QPP benefits is a more active choice, since claiming is possible from age 60 to 70, and there is considerable heterogeneity in when individuals claim (Glenzer et al., 2023). 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 mandatory retirement bans reduce the chance of making a final contribution to an employer-pension plan by 4.9 percentage points ($p < 0.01$) and reduce the chance of claiming benefits by 5.3 percentage points ($p < 0.001$). These estimates, being 85–90%

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

as large as the impact on retirement, provide further evidence that the effects of the bans are concentrated among the minority of older workers who are covered by an employer-pension plan.

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 percentage points, 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 p.p. ($p = 0.144$) among all workers, and an even more negative and statistically significant effect for workers in strongly affected industries (-3.4 p.p., $p = 0.045$). These results suggest that around 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 p.p. ($p = 0.010$), while new withdrawals decrease by 0.7 percentage points in the same year ($p = 0.018$) and 1.1 percentage points in the following year ($p < 0.001$). These effects intensify for those 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, it is notable that the magnitude of these responses is only one-third as large as the effects on retirement hazards. This suggests that most of the affected workers are "passive savers", as has been found in other contexts (Chetty et al., 2014).

While our identification comes from changes in involuntary retirement, the heterogeneity in the effects across industries and firms suggest that these retirements may be anticipated by older workers, who could respond by making pre-emptive savings adjustments (Milligan, 2003; Lindeboom and Montizaan, 2020; Etgeton et al., 2023), particularly on margins of adjustment that are more flexible (e.g., CPP/QPP claiming, RRSP contributions). We examine potential anticipatory saving responses at ages 61–64 using our DiD strategy (Table A2) and find no evidence that workers start saving less at ages 61–64 after the bans. 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, forthcoming), showing that individuals mainly respond to a pre-announced delay in pension eligibility by saving more at ages where employment is affected.

In sum, we find evidence of both active and passive savings responses in response to changes in involuntary retirement at age 65. Individuals reduce their disposable income available for consumption while working — through passive private and public pension contributions and active contributions to RRSPs — and increase their disposable income in retirement by claiming such pensions and making RRSP withdrawals. This suggests that pensions and tax-favored savings vehicles help cushion the welfare losses associated with involuntary retirements and highlights a key benefit of flexible pension systems that allow individuals to start claiming/dis-saving at a variety of ages (e.g., CPP/QPP and Social Security in the US).

**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 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.

### 5.2 Health impacts

Our data allows us to consider two objective measures of workers' health that may be affected by the mandatory retirement bans. The first is workplace injuries, which is discernible through workers' compensation payments recorded on tax forms. The second is (all-cause) mortality, which must be reported on the final income tax form of the deceased by their representative.

**Workplace injuries.** Our results in Section 4.3 showed stronger effects of mandatory retirement bans on workers in industries and firms with higher injury rates. Thus, it is plausible that those bans may cause a substantial increase in the number of workplace accidents and workers' compensation claims. Such effects are an unexplored but potentially important mechanism for explaining the effects of retirement on health.

In Canada, workers' compensation systems 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).[40] We focus on new payments in the year ahead so that we can estimate the effects in the first full year when the individual is aged 65.[41]

Estimates from our DDD model show that the mandatory retirement bans increase new workers' compensation payments at age 66 by 0.26 percentage points ($p < 0.001$) 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 p.p., $p < 0.001$).

Overall, our estimates imply an annual workplace injury rate of 3.7% for workers affected by the mandatory retirement 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

[40] `https://awcbc.org/en/statistics/national-work-injurydisease-statistic-program-definitions/`

[41] 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 Appendix C.2, we show that we estimate a similar increase in claim rates among individuals aged 65 to 68 if we use aggregate administrative data on the number of awarded claims at the age-province-year level.

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.[42] In 2019, the average system cost of a claim across Canada was $36,818, which includes benefit payouts (around 80% of the total cost) and administrative costs (approximately 20%).[43]

**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.[44] 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.

### 5.3 Retirement transitions of spouses

Finally, we exploit the information in the tax forms that links spouses to test for intra-household spillovers in terms of retirement transitions. The theoretical predictions are ambiguous and depend on the relative importance of joint retirement preferences and added worker effects. We focus on spouses who are younger than age 65, effectively ruling out any direct effects of the mandatory retirement bans on the spouse. Thus, spouses in our sample are typically younger than the reference individual and 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. Accordingly, we restrict the sample of couples to those where the spouse is also working in year $t$ (around two-thirds of couples).[45] We then

[42] 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.

[43] 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]).

[44] 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.

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

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 p.p., $p < 0.001$) and slightly larger at ages 66–68 (-1.5 p.p., $p = 0.009$). We calculate a weighted average of these effects (-2.96 p.p., $p < 0.001$) 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 p.p., $p = 0.204$) and 66–68 (-0.77 p.p., $p = 0.044$). The weighted average of these effects is -0.66 p.p. ($p = 0.019$). This implies an average 'scaled spillover effect' (García-Miralles and Leganza, 2021) of 22%.

We also examine how the spillovers vary with the spouse's age and the industry of the reference individual (Table 6). In general, the point estimates are larger for both partners if the reference individual is employed in a strongly affected industry (although the scaled spillover effect of 15% is slightly smaller). We 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. The estimated scaled spillover effects are 39% for spouses aged 60–64 and 8% for spillovers on spouses under 60. These patterns are consistent with García-Miralles and Leganza's (2021) analysis of joint retirement in Denmark, which highlight the importance of small age differences and the spouse's own eligibility for retirement benefits.[46]

Remarkably, our estimates of the scaled spillover effects are at least as large as other estimates in the literature based on administrative data exploiting variation in incentives to retire *voluntarily*. For example, Lalive and Parrotta (2017) estimate a scaled spillover effect of 8% for female spouses around their partner's full retirement age, but observe no spillover on male spouses; similarly, Selin (2017) and Lalive et al. (forthcoming) both find no evidence of spillovers on male spouses from respective reforms in Sweden and Switzerland that reduced the retirement probability of women; García-Miralles and Leganza (2021) estimate a scaled spillover effect of

[46] We also examined dynamic effects (i.e., effects on spouses' probability of retiring in the following year). Again, the estimates are negative (Table A5), 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 mandatory retirement bans have spillover effects on spouses that increase the overall employment effects.

8% in Denmark around the early retirement age; and Johnsen et al.'s (2022) estimates imply a scaled spillover effect of 17% in response to a reduction in the retirement age in Norway.

The large effects in our setting are surprising given that our estimates are identified from changes in *involuntary* retirement. Such retirements might be less predictable for couples. However, the fact that the effects typically occur at the age of 65 and are concentrated within certain firms/industries suggest that they can be anticipated. Furthermore, the ability of individuals to mitigate their income losses upon retirement via saving and pension claiming responses potentially explains why *added worker effects* may be small in our setting. Two additional 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 the retirement timing of their partner.[47] Second, our estimates may detect a larger fraction of retirement spillovers than estimates that are 'local' around a partner's statutory retirement age (Lalive and Parrotta, 2017; García-Miralles and Leganza, 2021), since partners' retirement transitions are likely to be interdependent even if they are not simultaneous.

As noted by García-Miralles and Leganza (2021), there are five main reasons discussed in the literature for joint retirement behaviour: (i) correlated preferences for leisure, (ii) common shocks, (iii) the shared household budget constraint, (iv) financial incentives, such as spousal pension benefits, and (v) leisure complementarities. Typically, it has been difficult to isolate specific reasons, since existing studies have relied on variation in incentives to retire voluntarily and many studies could only estimate spillovers on (less responsive) male spouses.

Our setting allows us to estimate spillovers on younger (mostly female) spouses and exploit exogenous variation in *involuntary* retirements across provinces and time. This method effectively rules out the influence of correlated preferences for leisure and common shocks. Furthermore, the shared household budget constraint works against joint retirement in our setting because involuntary retirement imposes a negative income shock on the couple. This leaves financial incentives and leisure complementarities as the remaining possible explanations. In terms of financial incentives, most retirement benefits in Canada are linked to the individual, although GIS is means-tested based on household income and thus may provide an incentive

[47] 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 A6).

for spouses to stop working following the job loss of their partner.[48] However, our estimates in Section 5.1 showed no evidence of any effects on GIS receipt. Thus, leisure complementarities are likely to be the primary driver of the spillover effects on spousal retirement timing.

# 6 Conclusion

This paper provides the first comprehensive analysis of the welfare implications of employment protection for older workers. We exploit mandatory retirement bans in five Canadian provinces between 2005 and 2009 that affected workers in nearly all industries. Using linked employer-employee tax data, we find that the bans had large effects on job separation rates and retirement hazards for workers aged 65 and above, but no apparent effects on the hiring rates of older workers. Accordingly, we find no effects on employment or earnings prior to age 65 and large positive effects at ages 66–69. Our estimates imply that the average worker approaching 65 works 3.6 months longer and earns $20,000 more. These employment effects are similar to what we would anticipate from a substantial pension reform, such as raising Canada's two public pension ages from 65 to 67, and they target a different group of older workers.

To shed light on the broader welfare consequences for older workers, we examine changes in a variety of saving behaviors, objective health measures and spousal retirement timing. We find that both active and passive saving responses provide insurance against involuntary retirement, with only 54% of the effects on earnings flowing through to changes in disposable income. Consistent with this high level of self-insurance provided by saving responses, we find no evidence of added worker effects among younger spouses. Rather, spouses respond to the bans by delaying retirement themselves, consistent with preferences for joint retirement due to leisure complementarities. These responses magnify the direct effects on retirement hazards by at least 20% among dual-earning couples. In terms of health impacts, we find evidence of a significant increase in workplace injuries among older workers, consistent with the elevated injury risks for workers in the most affected industries, but no evidence of mortality effects.

In sum, our results imply that employment protection raised the average welfare of older workers, mostly through large positive effects on employment and earnings. Additionally, our results show that both savings responses and health impacts are important mediators of the

[48]Spouses aged 60–64 can also receive a GIS allowance if their partner qualifies for GIS and the couples' combined annual income is less than a maximum threshold.

overall welfare effects of involuntary retirement. Specifically, the welfare losses of involuntary retirement are likely to be largest among passive savers, in settings where involuntary retirement is unpredictable, among populations that are not covered by public or private pensions, and among workers with lower risks of workplace injuries.

While our findings show welfare benefits for workers, the effects on firms warrant further research. In theory, the bans may be harmful for firms, since they raise firing costs. However, several of our results suggest that the welfare costs on firms are unlikely to be large. First, we find little evidence that the bans meaningfully reduce firms' demand for older workers who are just below age 65, even in industries where mandatory retirement was common. The lack of an effect on the demand for older workers would be difficult to reconcile with the bans causing large negative effects on firm profits. Second, our results indicate that mandatory retirement enforcement was extremely heterogeneous across industries and firms. This heterogeneity cannot be explained by age-wage gradients or underlying job separation rates. There is also little evidence of heterogeneity within firms based on worker characteristics. These results suggest the primacy of industry and firm norms — rather than economic incentives — in explaining enforcement. Third, firms do not rush to dismiss older workers after the bans are announced; instead, they reduce mandatory retirement enforcement over the announcement period, consistent with the announcement of the law itself affecting social norms.

Our research holds immediate relevance to the numerous countries with active mandatory retirement provisions (e.g. Germany, France, Ireland, Finland, Sweden, the Netherlands, Norway, Portugal, Iceland and Japan). Our findings suggest that policies in these countries that limit or ban mandatory retirement could have large positive effects on old-age employment. Other developed countries like the US and Canada have banned mandatory retirement but still have relatively weak employment protection laws. Older workers in these countries are vulnerable to being laid off and may benefit from more employment protection. Such measures may reduce the share of retirees who are completely or partially forced to retire, which is on the rise in the US and now represents the majority of new retirees (Johnson and Gosselin, 2018). Our results show a one-for-one link between involuntary job separations and retirement hazards, which means that initiatives that boost the retention of older workers by firms (e.g., through employment protection, training or workplace accommodations) can significantly bolster the employment and welfare of older workers.

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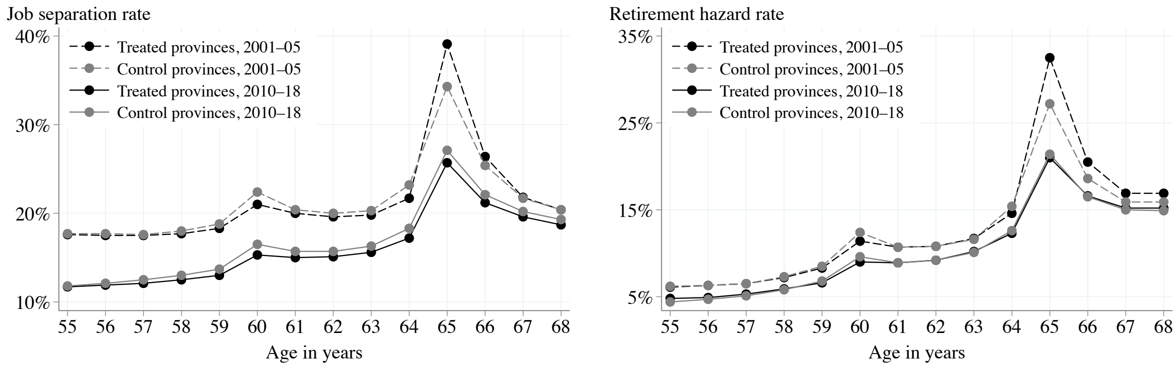
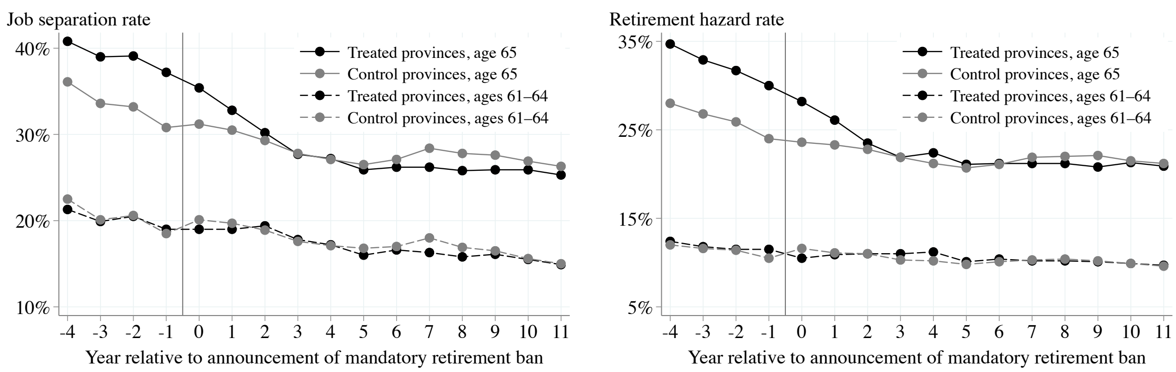
Figure 1: Trends in separation rates and retirement hazards by treatment status, age and time

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

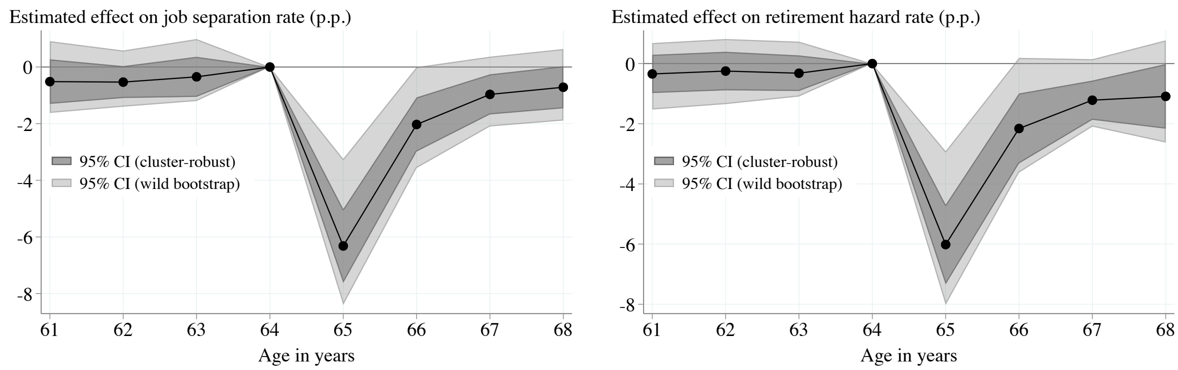



Notes: These figures show average job separation rates and retirement hazards of older workers by province (treatment/control), age and time. Panel (a) displays these rates by single year of age in two distinct time periods: 2001–05 (before all the recent mandatory retirement bans occurred in treated provinces) and 2010–18 (after all the bans occurred). Panel (b) shows the rates for 61–64-year-olds and 65-year-olds in event time. For treated provinces, event-time is relative to the ban announcement year. For control provinces, we create stacked sample that duplicates observations and links each control observation to a single treated province (which gives an 'announcement year'). Then we take the average rates across control observations in each 'event year'.

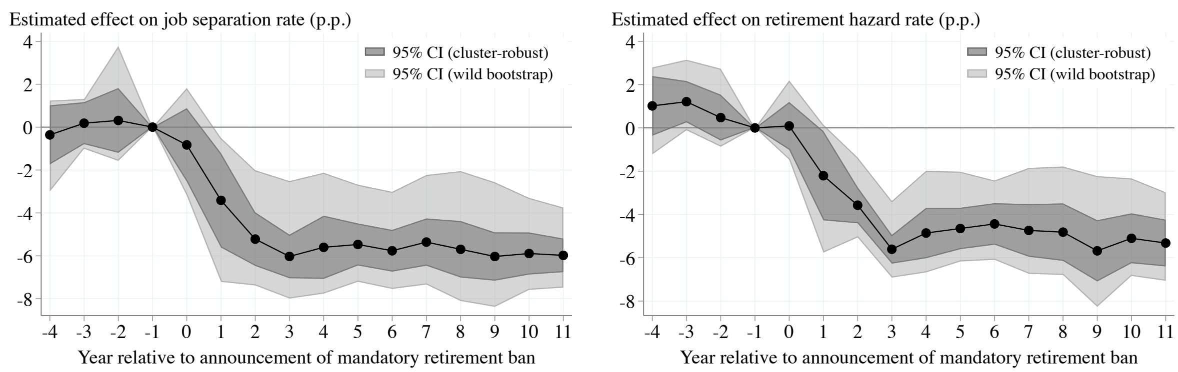
Figure 2: Estimated effects of mandatory retirement bans by age and in event time

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)



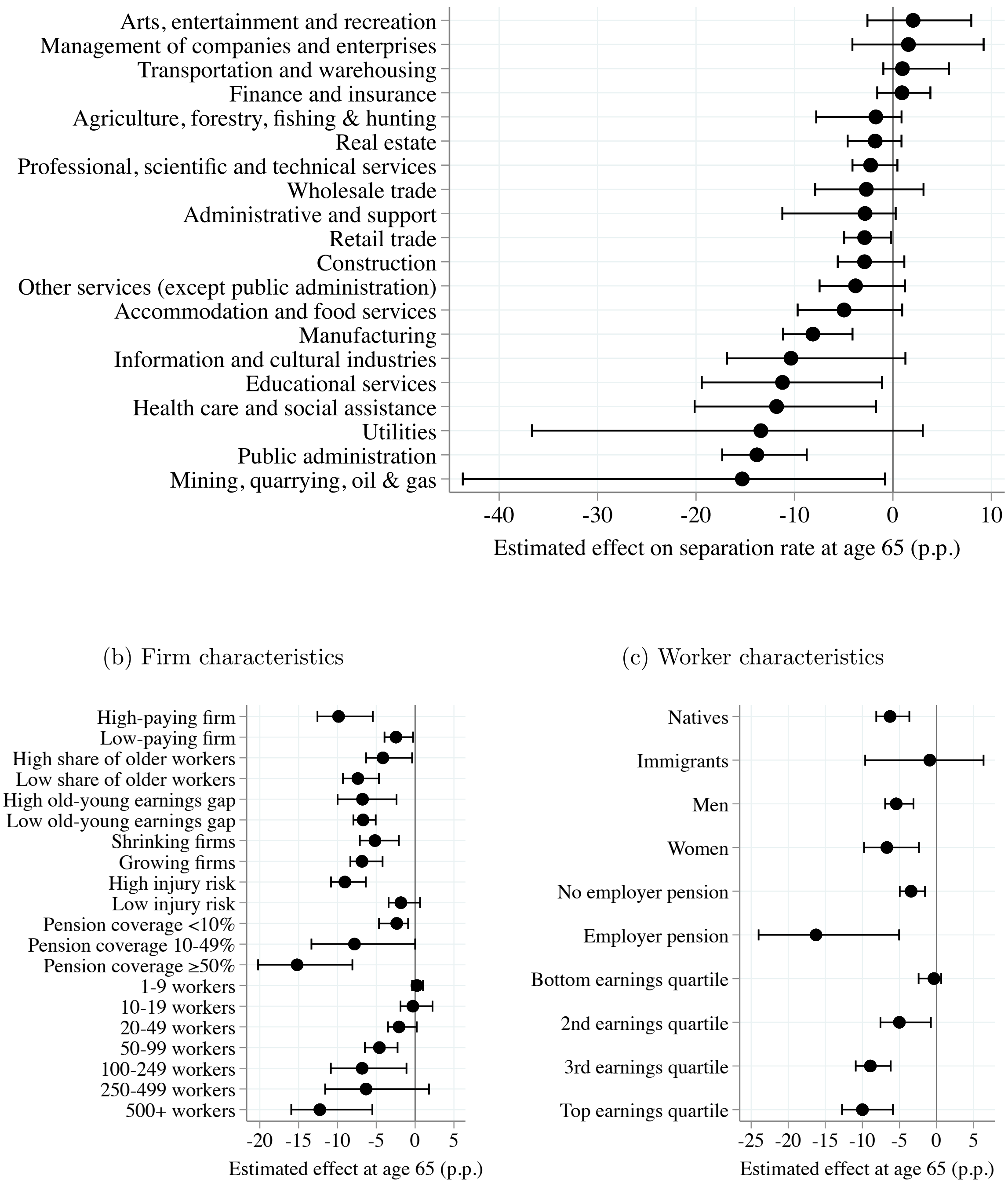

Notes: These figures show the estimated effects of the mandatory retirement bans by age in panel (a) and in event time in panel (b) with 95% confidence intervals. Panel (a) shows estimates of the post-implementation effects from a modified version of equation (3.2) that allows for different effects at each age from 61 to 68 (relative to the effect at age 64). Panel (b) shows the estimated effects at age 65 in event time from equation 4.1 (relative to the effect in the year before the ban was announced).

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.

Table 1: Dates that mandatory retirement was banned in Canadian provinces

| | Date of ban | | |
|---|---|---|---|
| Province | Announcement | Implementation | 2019 population (millions) |
| *Provinces/territories with early bans (control)* | | | |
| New Brunswick | | 1973 | 0.777 |
| Manitoba | | 1982 | 1.369 |
| Quebec | | 1983 | 8.485 |
| Prince Edward Island | | 1988 | 0.157 |
| Alberta | | 2000 | 4.371 |
| Nunavut | | Never allowed | 0.039 |
| Yukon | | Never allowed | 0.041 |
| Northwest Territories | | Never allowed | 0.045 |
| *Provinces with recent bans (treated)* | | | |
| Ontario | 12 Dec 2005 | 12 Dec 2006 | 14.570 |
| Newfoundland | 26 May 2006 | 26 May 2007 | 0.522 |
| Nova Scotia | 13 Apr 2007 | 01 Jul 2009 | 0.971 |
| British Columbia | 25 Apr 2007 | 01 Jan 2008 | 5.071 |
| Saskatchewan | 17 May 2007 | 17 Nov 2007 | 1.174 |

Notes: This table shows the date that mandatory retirement became illegal in Canadian provinces ("implementation date"). We also include the dates that recent bans were legislated ("announcement date") and the population of each province/territory in 2019.

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.

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

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Hired this year | | | |
| | 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 allowed for in the regression and are not statistically significant.

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 that 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 $t+1$).

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 | | | |
| 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 seven most strongly affected industries (42% of the full sample). 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. The direct effects on job separation rates and retirement hazards are 1.8 times larger in this sample than the full sample.

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 | |
| | | | Panel B: 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 C: 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: 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). In columns 4–6, we restrict the sample to workers in the seven most strongly affected industries (see the notes below Table 5 for a full list).

# Web Appendix for "Graying and staying on the job: The welfare implications of employment protection for older workers"

Todd Morris Benoit Dostie

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# 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.

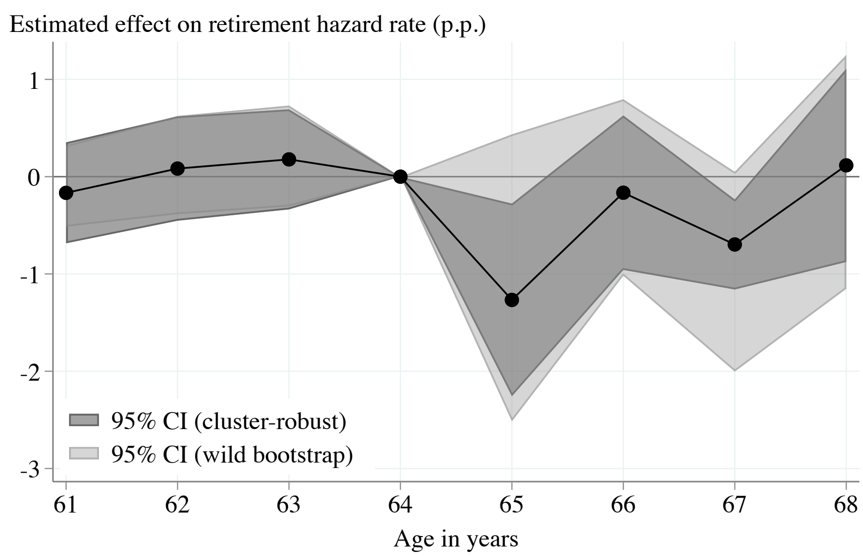
[A1] https://www150.statcan.gc.ca/n1/daily-quotidien/161130/dq161130g-eng.htm

## A.2 Main appendix figures and tables

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

(a) Job separation rates

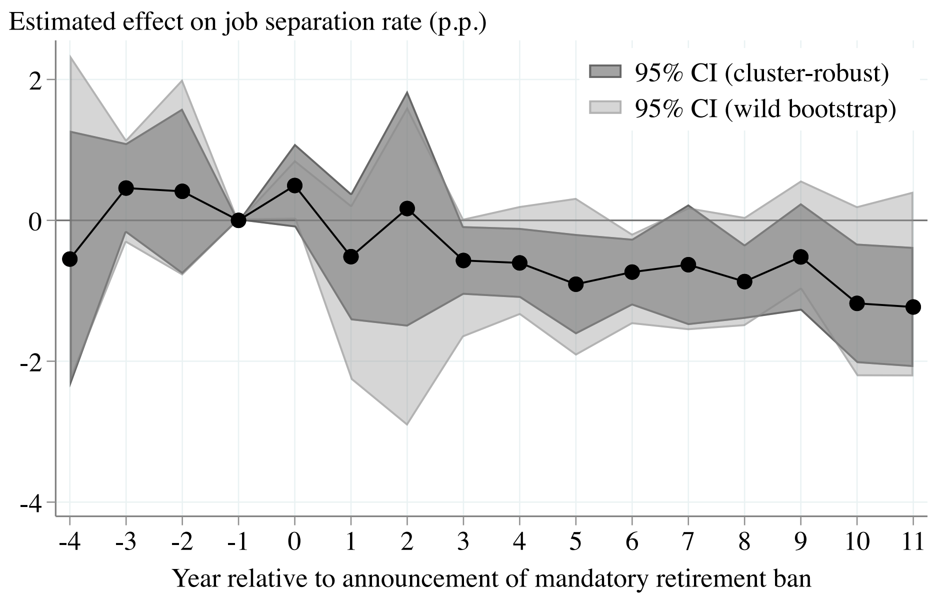

(b) Retirement hazards



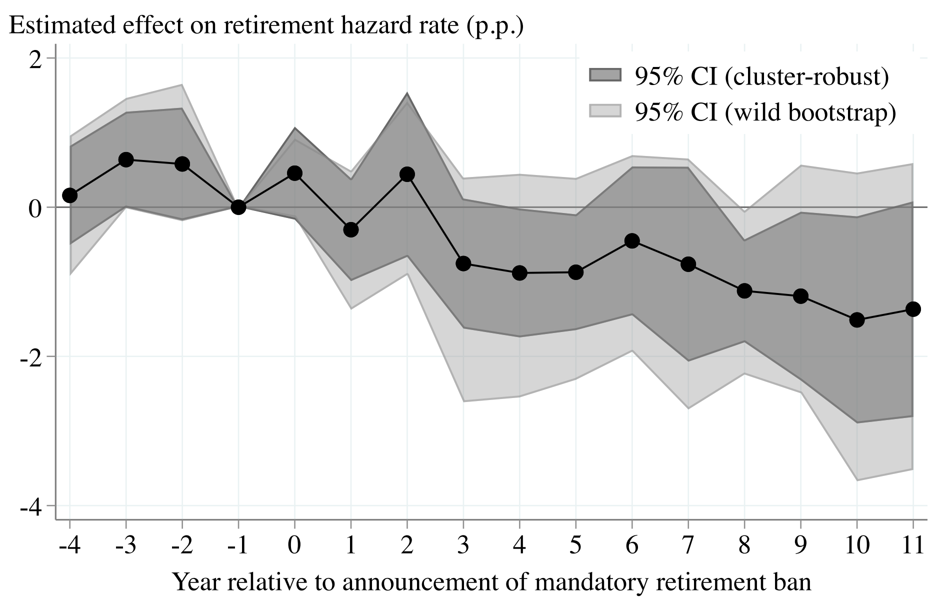

Notes: These figures show estimates of the announcement effects of the mandatory retirement bans by age on job separation rates in panel (a) and retirement hazards in panel (b) with 95% confidence intervals. The estimates come from a modified version of equation (3.2) that allows for different effects 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

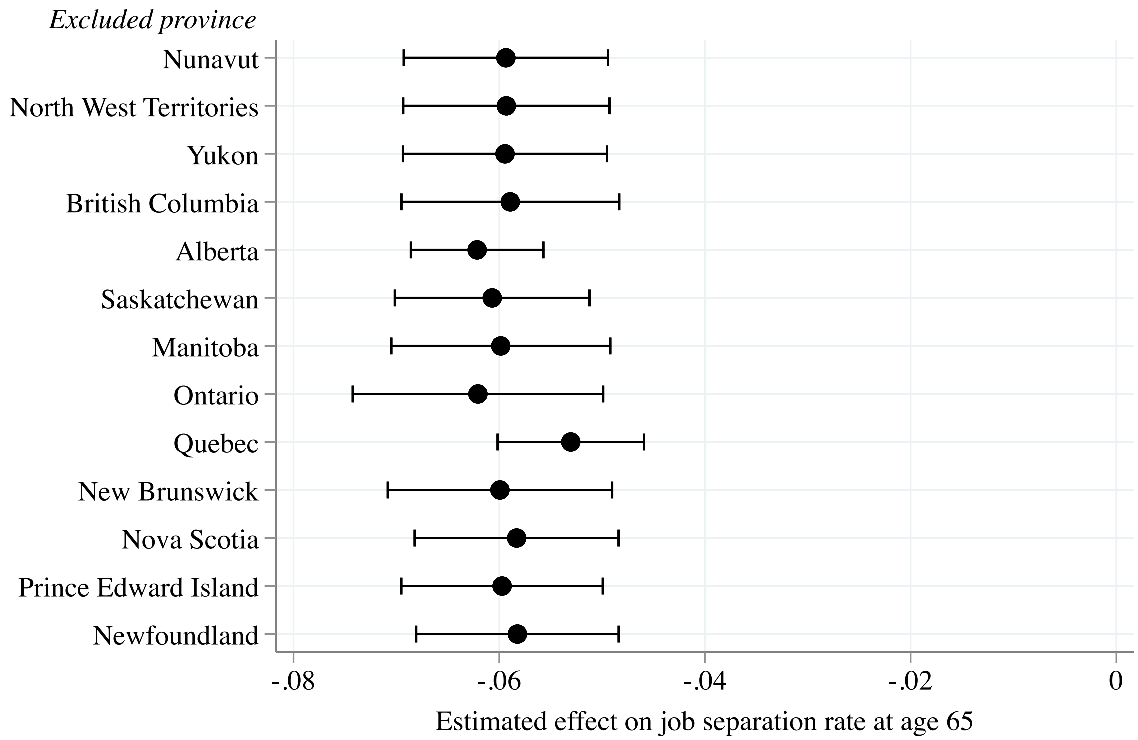



(b) Retirement hazards




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 4.1 (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

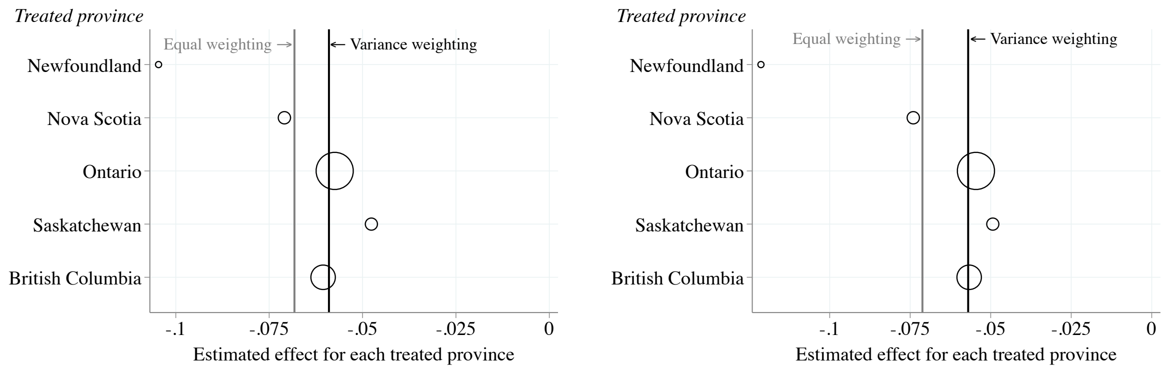



Notes: This figure shows the robustness of the estimated post-implementation effects of the mandatory retirement bans on job-separation rates at age 65 to the exclusion of individual provinces from the sample. We show the point estimates from equation (3.2) with 95% confidence intervals (based on our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

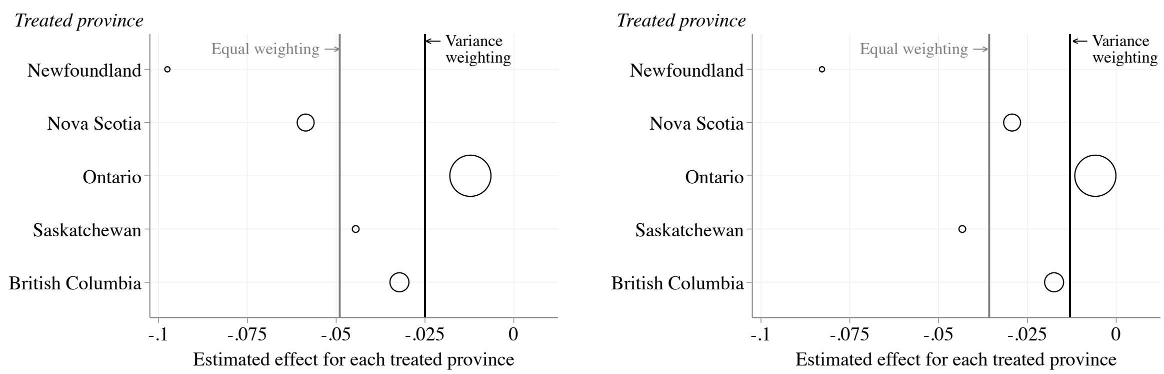
Figure A4: Estimates for each treated province individually

Job separation rates | Retirement hazards

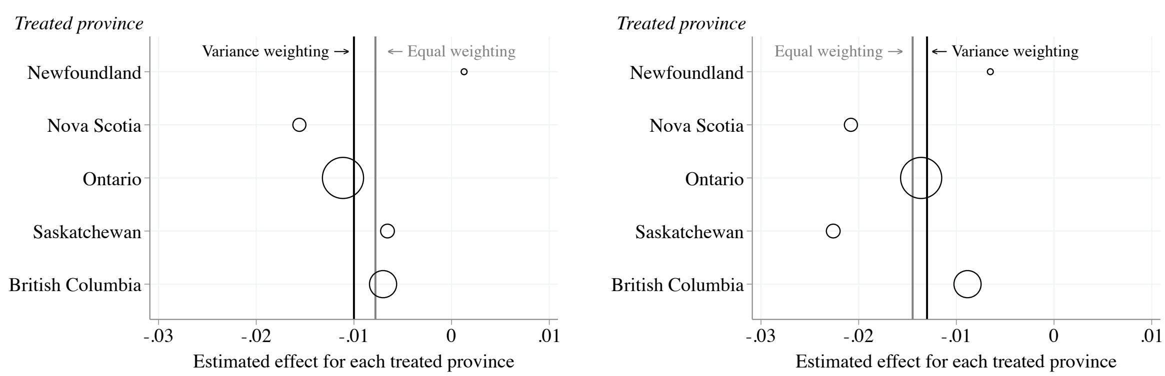
(a) Post-implementation effects at age 65




(b) Announcement effects at age 65




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

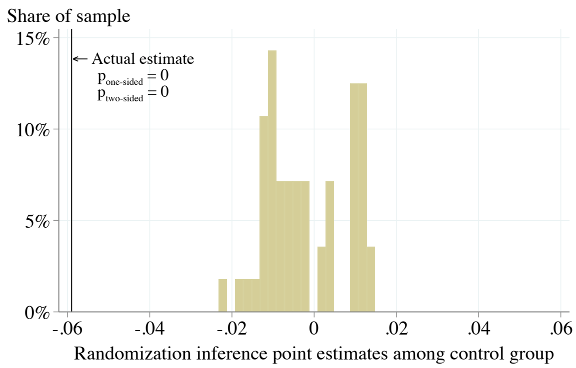


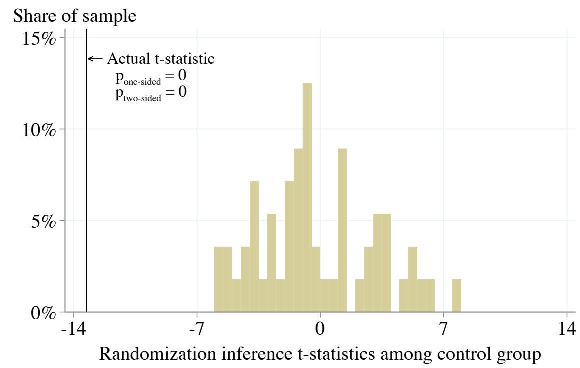

Notes: These figures show the point estimates from equation (3.2) when we restrict the sample to a single treated province and the set of control provinces. We show the estimates of the post-implementation effects at age 65 in panel (a), the announcement effects at age 65 in panel (b), and the post-implementation 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

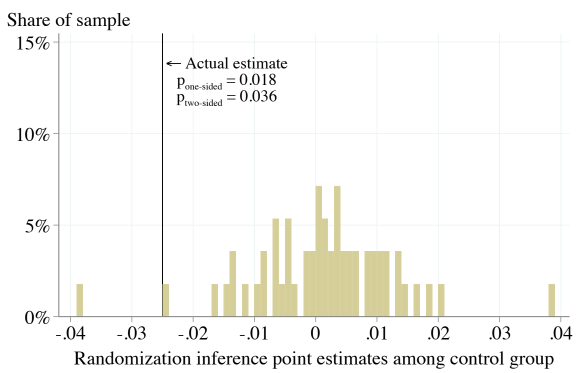
Point estimates | t-statistics

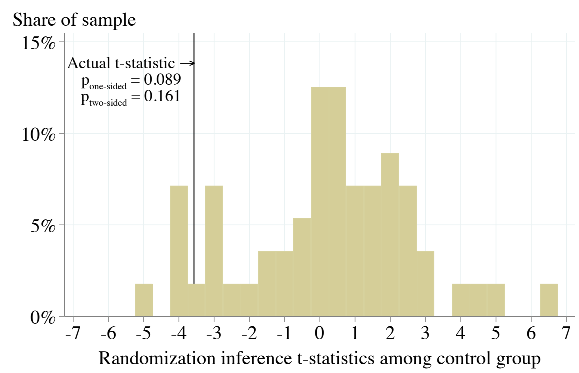
(a) Post-implementation effect at age 65



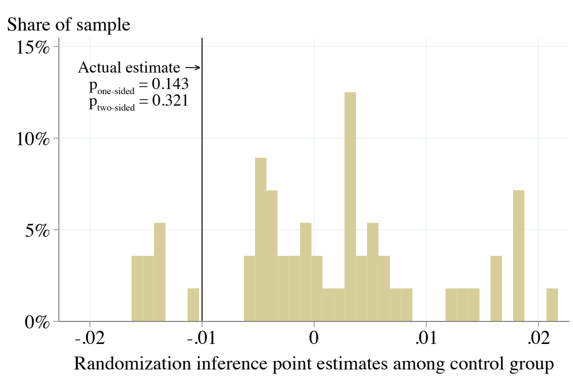



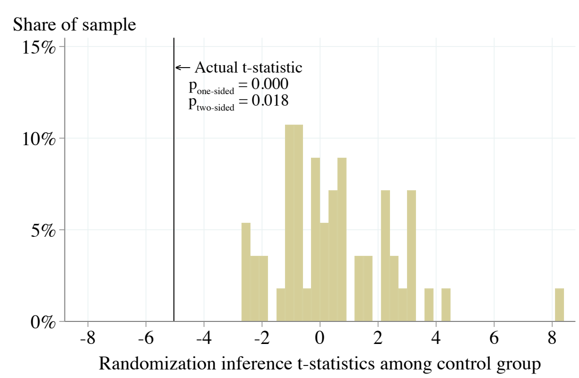

(b) Announcement effect at age 65




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



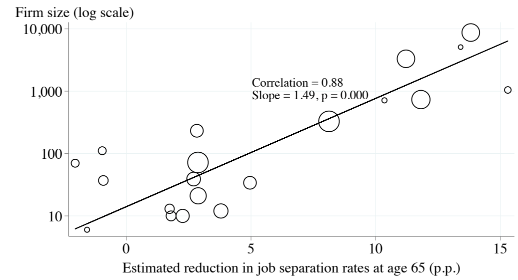




Notes: These figures show the distributions of point estimates and t-statistics from equation (3.2) when we randomly assign treatment to 3 out of 8 of the control provinces. For each possible set of 3 'treated' provinces, we randomly assign these provinces to the ban timing of Ontario, British Columbia and Saskatchewan without replacement. Job separation rates in the 'treated' provinces are then compared to the other 5 control provinces. The vertical red 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.

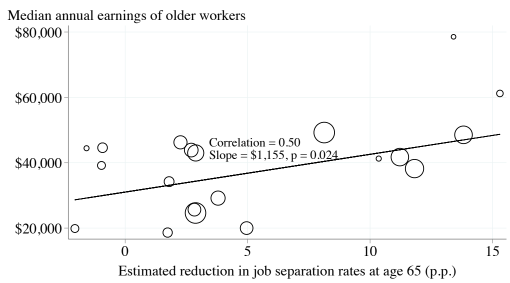
Figure A6: Correlation between effect heterogeneity by industry and industry characteristics

(a) Pension coverage of older workers

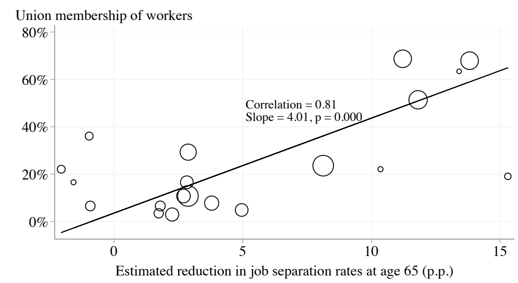

(b) Firm/organization size



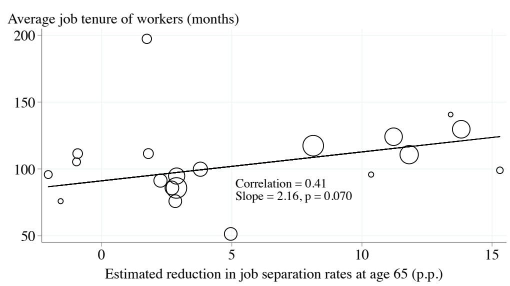

(c) Average earnings of older workers



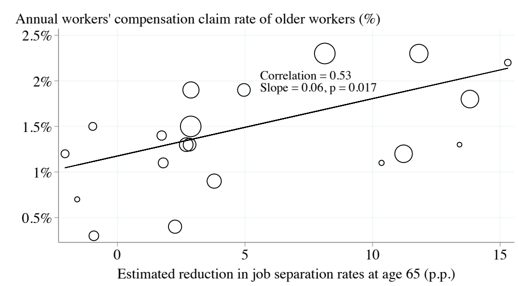

(d) Unionization of workers



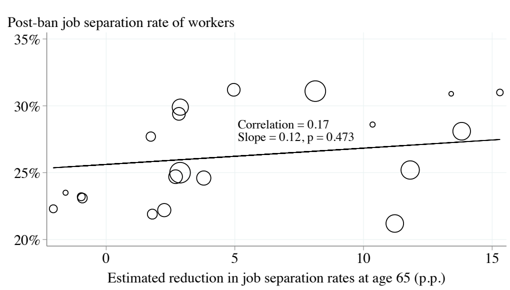

(e) Average job tenure



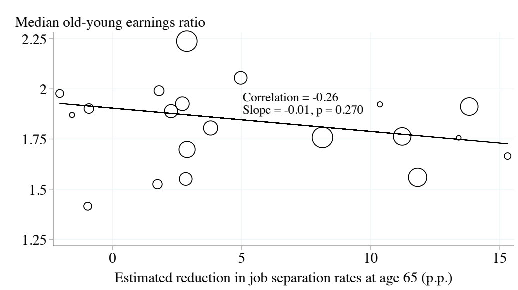

(f) Workplace injury risks of older workers




(g) Post-ban separation rates at age 65

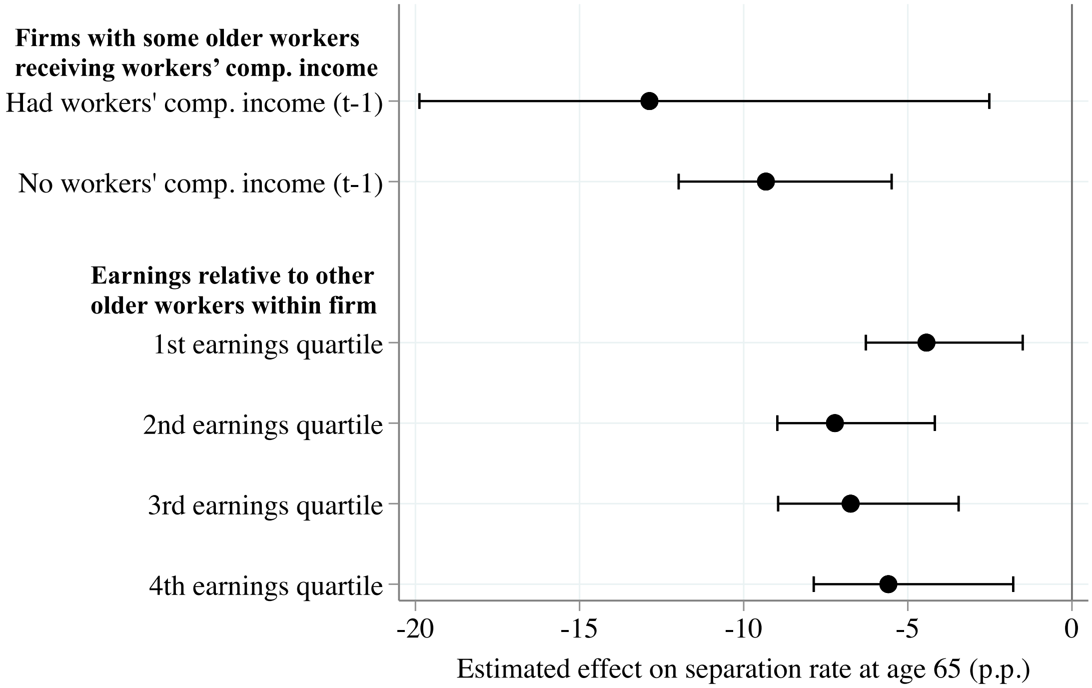



(h) Earnings gap between old and young




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: 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 A6: 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).

# 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 (forthcoming) 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 percentage points higher (21.0%), and the hazard at age 66 is 4.3 percentage points 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.

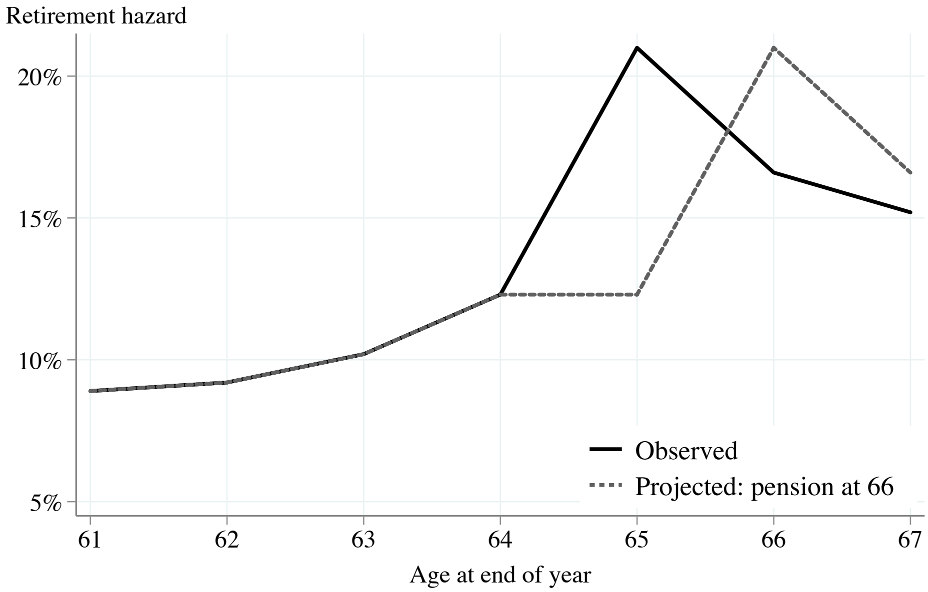
[B2]Rates are remarkably similar at each age in control provinces during this period (Figure 1a).

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

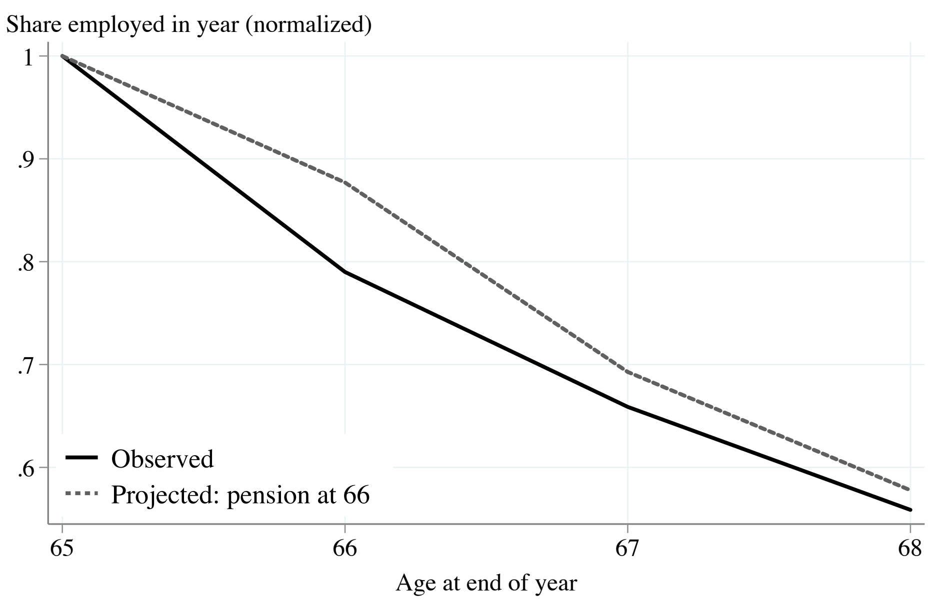
[B4]It is also possible that our approach could miss employment responses below age 65 (e.g., if someone delays

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

(a) Retirement hazards

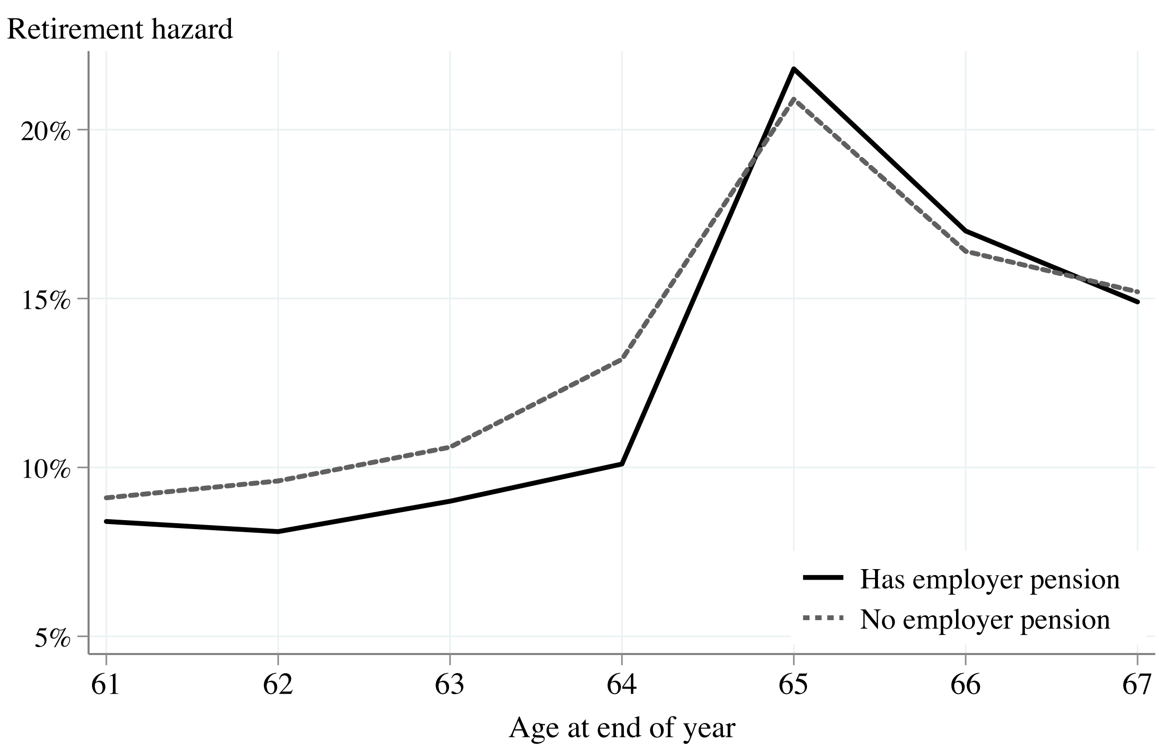



(b) Employment effects



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




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

# C Additional robustness checks for spillover analysis

## C.1 Assessing the parallel-trends assumption

This section shows event-time plots for the results in Section 5. 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 (4.1). 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 percentage points, similar to the estimate in column 2 of Table 5 (-1.7 percentage points).

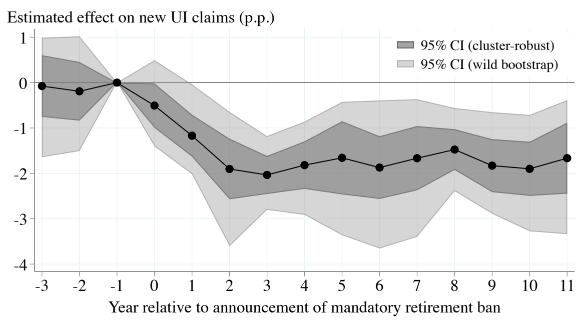
Figure C1b shows the estimates for transitions to self-employment. The estimates in years -3 and -2 are positive but marginally insignificant at the 5% level, as is the estimate in year 0. The estimates in subsequent years are smaller and mostly negative. These results are consistent with at most a modest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

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 percentage points, similar to the estimates in column 2 of Table 5.

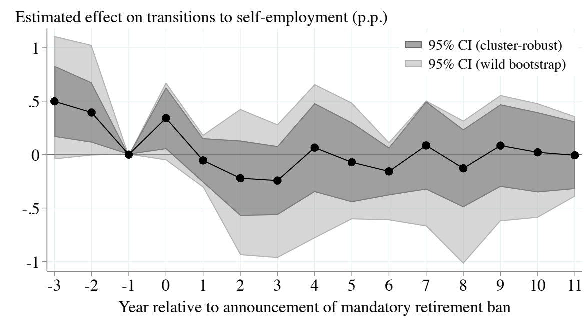
[C1] These industries include manufacturing; public administration; health care and social assistance; educational services; information and cultural industries; utilities; and mining, quarrying, and oil and gas extraction.

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

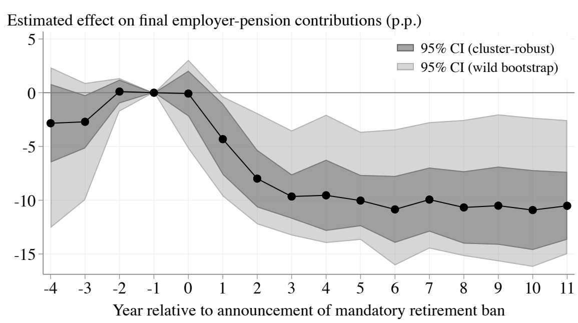
(a) New UI claim



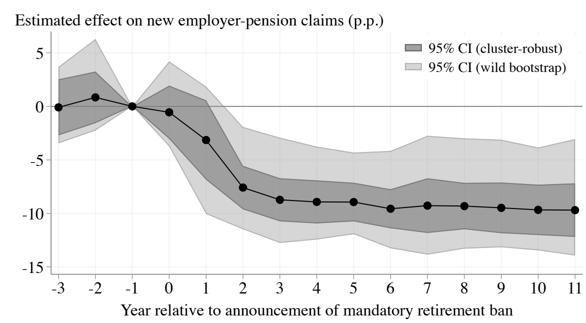

(b) Becomes self-employed



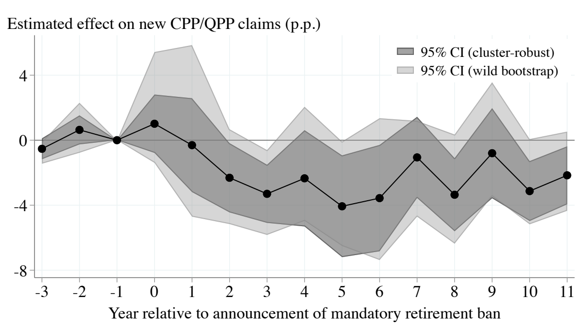

(c) Final employer-pension contribution



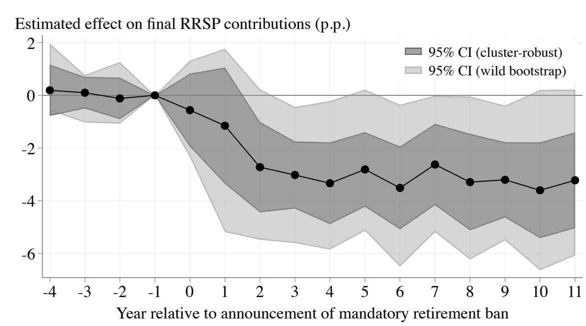

(d) New employer-pension income



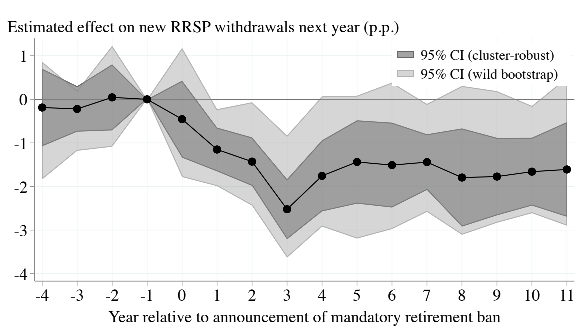

(e) New CPP/QPP claim




(f) Final RRSP contribution




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




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

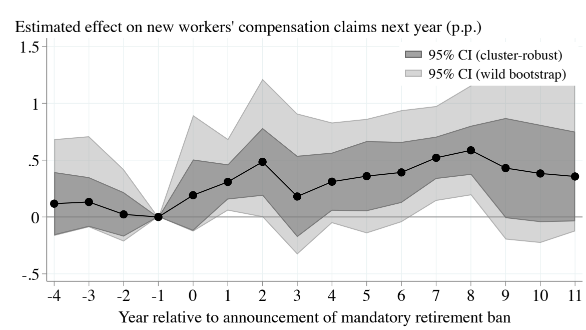


Figure C1e shows the estimates for new CPP/QPP claims. Again, the estimates display a similar pattern to those for retirement hazards. There is no evidence of pre-trends before a clear and sustained decrease after the mandatory retirement bans are announced. Similar patterns are observed for RRSP contributions 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 percentage points, again similar to the estimate in Table 5 (0.34 percentage points).

**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.

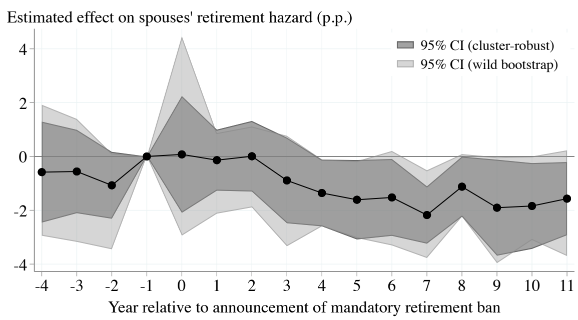
## C.2 Using administrative workers' compensation claims data

We assess the robustness of our estimates of the effects of the mandatory retirement bans on workers' compensation claims using administrative claims data. We obtained aggregate data on the annual number of awarded lost-time claims by province and single year of age for 1996–2021 from the National Work Injury/Disease Statistics Program.[C2] A lost-time claim occurs when "a worker is compensated by a Board/Commission for a loss of wages following a work-related

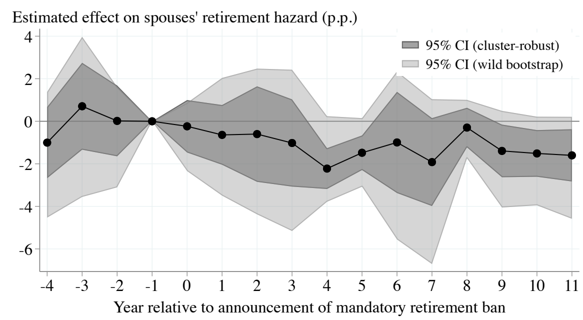
[C2] In Canada, each province and territory has its own workers' compensation board, with the exception of the Northwest Territories and Nunavut, which share a board. These twelve provincial boards manage workers' compensation claims and report data to the Association of Workers' Compensation Boards of Canada.

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




injury (or exposure to a noxious substance), or receives compensation for a permanent disability with or without any time lost in his or her employment (for example, if a worker is compensated for a loss of hearing resulting from excessive noise in the work place)".[C3] Unfortunately, our data thus do not contain information on rejected claims.

We have information on the exact number of awarded lost-time claims for each province-year-age cell if it is zero or at least four, but claims of one, two or three are listed as "X" for confidentiality reasons. This is common in the territories and Prince Edward Island for individuals aged in their 60s. We thus focus on the other nine, more populous provinces. For these provinces, only 4.4% of cells are listed as "X". In these cases, we impute a value of 2, the midpoint of the possible 1–3 range. This seems reasonable given that there is a similar number of cells with 0 and 4 claims.[C4]

Our annual data starts in 1996 and ends in 2021, but we restrict the sample to the period from 2001 to 2015. This allows us to construct claim rates per capita for each province-age-year cell (using population data from Statistics Canada,[C5] which starts in 2001), and follow claim rates for 8 years after the last reform was announced.[C6] Figure C3 shows how claim rates have

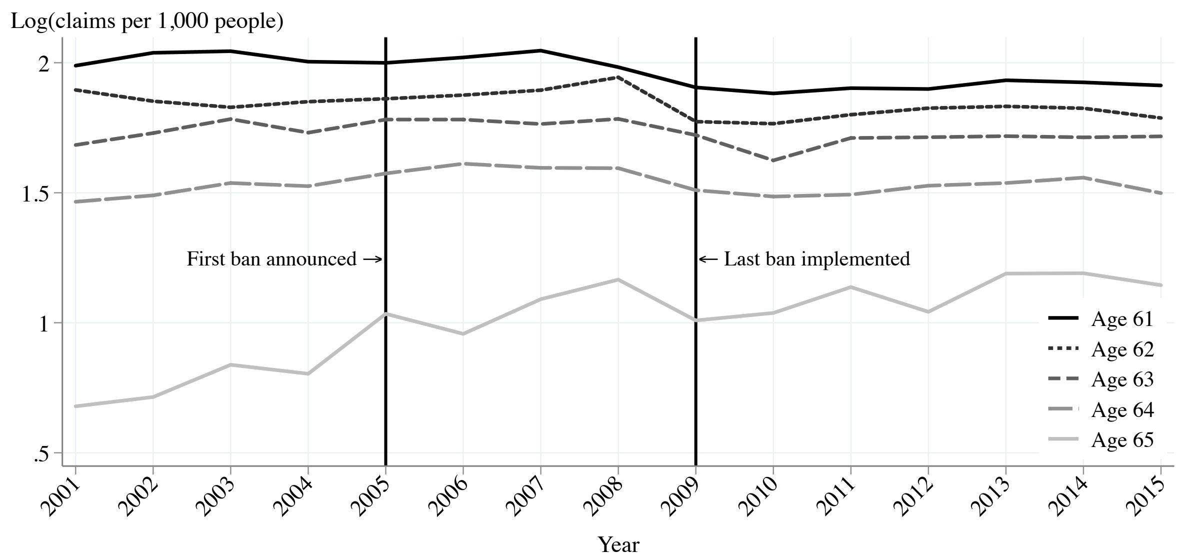
[C3]`https://awcbc.org/en/statistics/national-work-injurydisease-statistic-program-definitions/`. There are some differences in the definition across provinces. For example, Ontario and Newfoundland do not include claims that receive compensation for a permanent disability without any time lost.

[C4]This censoring occurs mainly in the smaller Atlantic provinces (New Brunswick, Newfoundland and Nova Scotia). Our regressions weight cells by population size, which reduces the importance of censored observations. Reassuringly, we find similar point estimates if we omit these provinces from the sample.

[C5]`https://www150.statcan.gc.ca/t1/tbl1/en/tv.action?pid=1710013701`

[C6]Our results are similar if we use data through to 2019. However, we observe a large increase in claim rates at changed over time across Canada close to the age of 65. Since 2001, claim rates have been relatively stable for individuals aged 61–64 years old. In contrast, claim rates have increased considerably for 65 year olds. This increase has closed three-quarters of the gap between claim rates at ages 64 and 65: in 2001, claim rates at age 64 were 120% higher than at age 65, but the gap in 2015 fell to 31%. Most of this convergence occurred after the first reform was announced in Ontario in December 2005, which suggests that the mandatory retirement bans may have contributed to a higher number of workplace accidents for individuals aged 65 and older. However, there is some evidence of convergence even during the 2001–05 period, which suggests that other factors may play a role.

Figure C3: Trends in awarded workers' compensation claims around age 65 across Canada




To assess the effects of the mandatory retirement bans, we assess the trends separately over time and by age for treated and control provinces. Figure C4a shows the percent change in claim rates by age in treated and control provinces between two distinct time periods: 2001–05 (before all the bans had occurred) and 2010–15 (after the bans). At ages 61–64, there has been minimal change in claim rates between these two time periods for both treated and control provinces, and the net changes were about 10 percentage points lower at each age in treated provinces. However, there was a clear increase in claim rates at ages 65 and above, and the net changes were 10–30 percentage points *higher* in treated provinces. This provides preliminary

all ages over the 2015–19 period, so we choose to focus on the earlier years when claim rates were more stable.

evidence that the mandatory retirement bans have increased workers' compensation claims in treated provinces for individuals over the age of 65.

We investigate the dynamics of these changes in claim rates further in Figure C4b, which is analogous to Figure 1b. Figure C4b shows the trends in event time in claim rates for individuals aged 61–64 and 65–68 for treated and control provinces. Among 65–68 year olds, we observe a clear gap of approximately 45% between the claim rates of individuals in treated and control provinces prior to the reforms. While claim rates were also lower in treated provinces among 61–64 year olds, the gap was considerably smaller (around 10%). After the reforms, we see that the claim rates at ages 65–68 have converged, with the gap closing to around 15%. This convergence is largest around 4–6 years after the mandatory retirement bans are announced: by this time, few individuals aged 68 would have been subject to mandatory retirement laws when they were aged 65. In contrast, there is no evidence of any sustained convergence at ages 61–64.

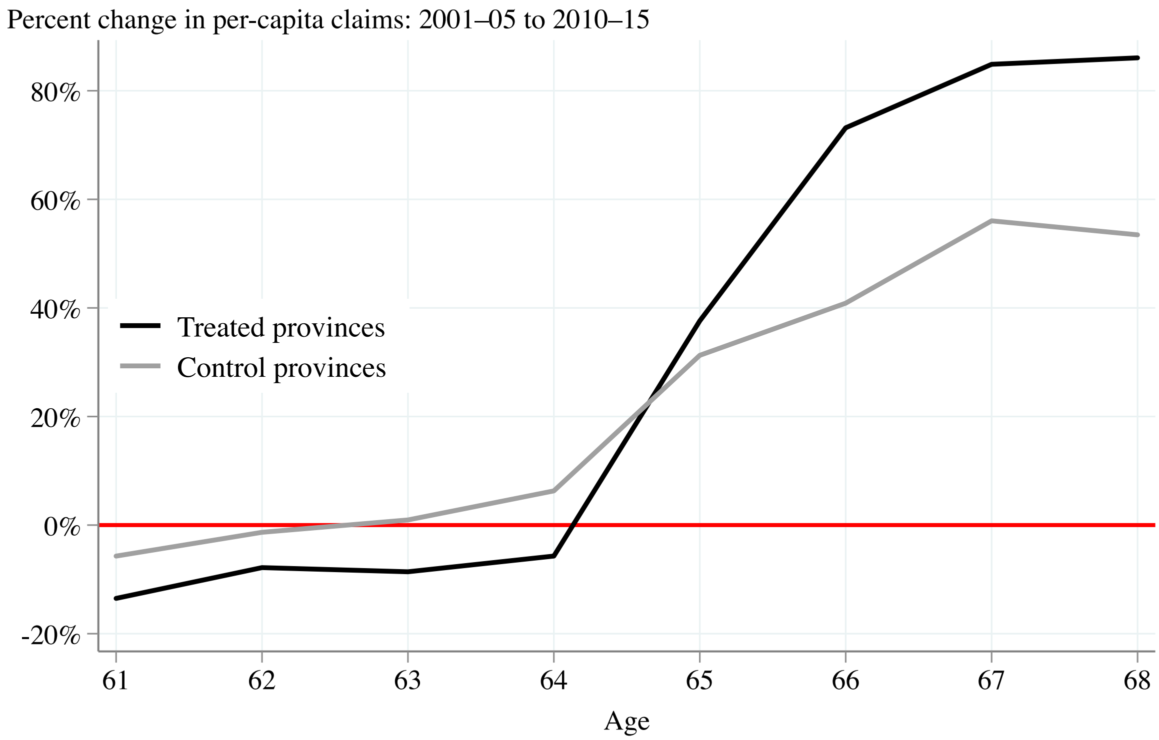
To further assess whether this convergence is linked to the change in retirement behavior, which is heterogeneous across industries, we obtained additional data on the number of lost-time claims in province-year-age-industry cells. Specifically, for each province-year cell, we obtained the total number of lost-time claims for 60–64 year olds and the 65+ population for the seven two-digit industries that have the largest enforcement of mandatory retirement rules at age 65 (based on our DDD estimates).[C7] We then use our counts at the province-year-age level to construct the counts for the other 13 industries. In Figure C5, we show how claim rates change in event time by age (60–64 vs 65+), treatment status (treated/control provinces) and industry (strongly vs weakly affected). We observe clear evidence that the convergence in claim rates above age 65 between treated and control provinces is entirely explained by the industries that consistently enforce mandatory retirement.

We formally estimate the effects of mandatory retirement laws using aggregate DDD models. We create a stacked dataset, with observations at the dataset-province-year-age level, and cells weighted by population size. Our outcome variable is the natural log of the per capita claim rate $\ln\left(\frac{\text{claims}+1}{\text{pop}_{dpat}}\right)$. Since we have zero counts in 0.5% of cells, we add one to claims before dividing by population size. Our results are similar if we use the inverse-hyperbolic-sine transformation or do not add one (dropping zero counts).

[C7] 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 C4: Trends in awarded workers' compensation claims by age and treatment status

(a) Percent changes in claim rates by treatment status from 2001–05 to 2010–15




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

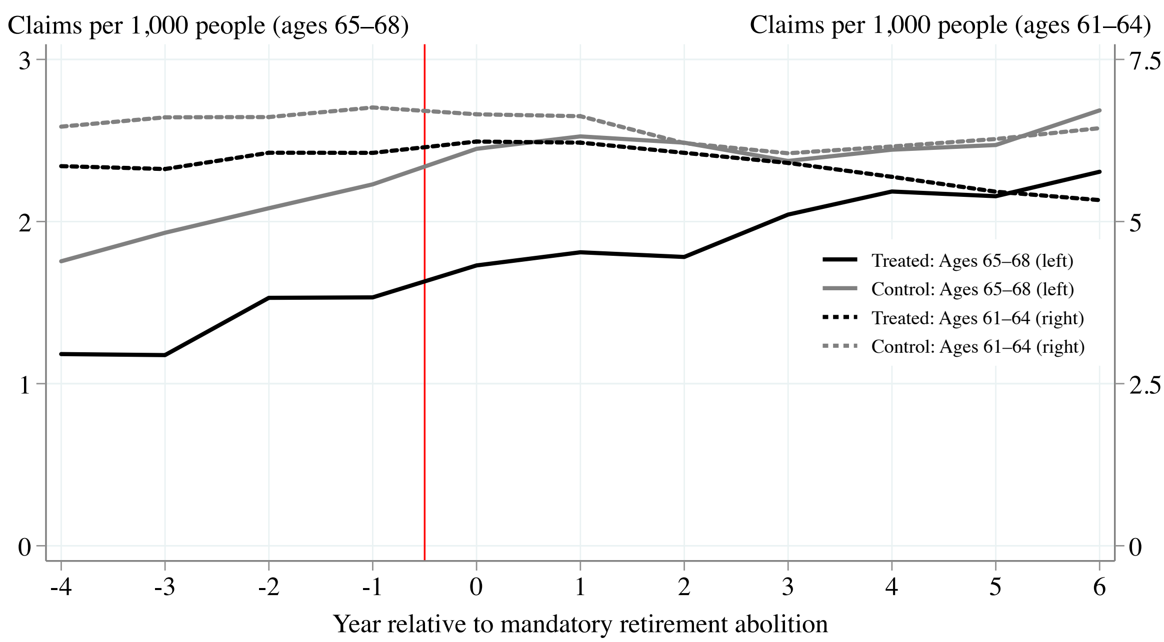
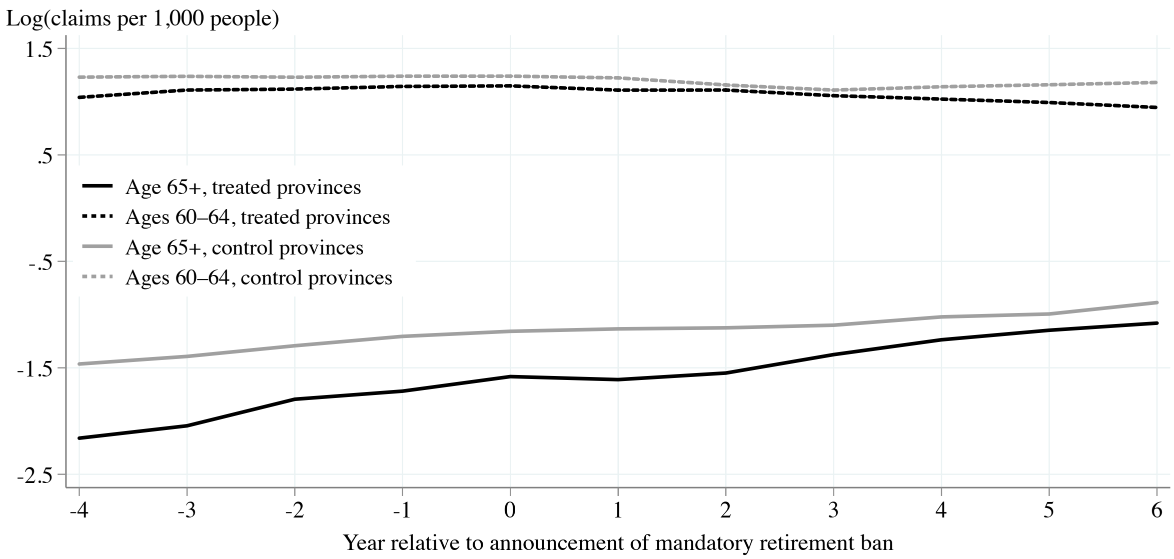
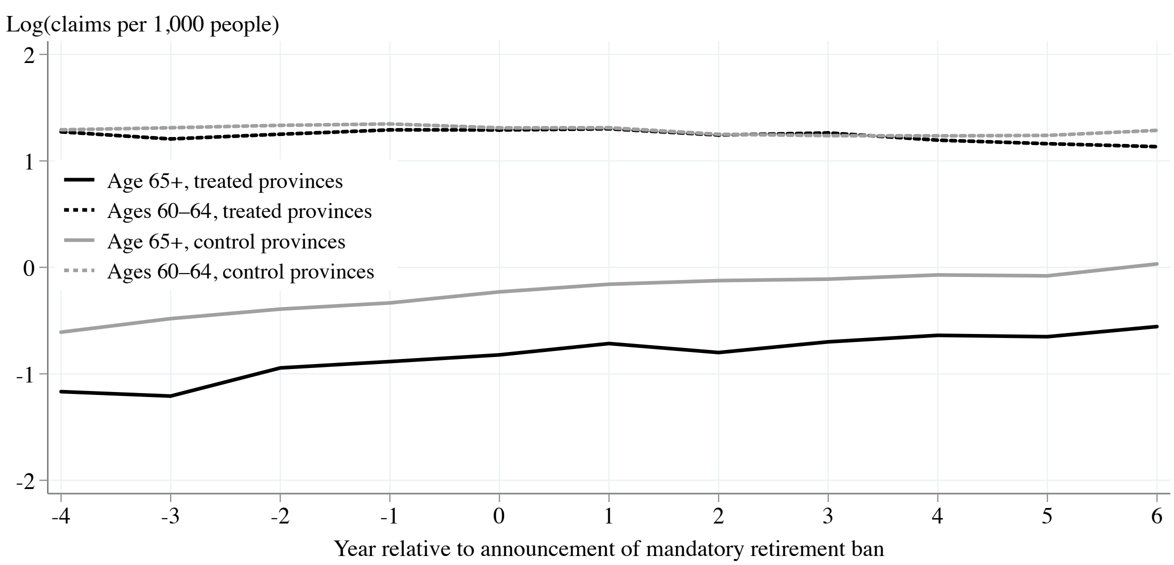


Figure C5: Trends in awarded workers' compensation claims by age and treatment status

(a) Strongly affected industries (large effects on job separation rates)




(b) Less affected industries (small/zero effects on job separation rates)



Specifically, we estimate the following regressions among 61–68 year olds:

$$\ln\left(\frac{\text{claims}_{dcapt}+1}{\text{pop}_{dcpat}}\right) = \text{Ban}_{cp}\{\alpha + \beta \mathbf{1}(Age_a \geq 65)\} + \theta_{dap} + \gamma_{dat} + \epsilon_{diapt} \tag{C1}$$

where the indexation is the same as in equation (3.2), except $c$ represents birth cohort (instead of $i$ for individual). The key variables of interest are $\text{Ban}_{cp}$, which is a measure of exposure to mandatory retirement bans for cohort $c$ in province $p$, and its interaction with a dummy variable for the cohort being at least 65 in year $t$. We define $\text{Ban}_{cp}$ as the fraction of time that birth cohort $c$ in province $p$ is exposed to a mandatory retirement ban at ages 65–68, with 1 representing full exposure. The coefficient $\alpha$ estimates the average effect of full exposure at ages 61–64, which is expected to be zero. The coefficient $\beta$ estimates the differential effect at ages 65–68. This is the DDD coefficient of interest.

We present the estimates in Table C1. In column 1, we show the estimates without any further sample restrictions. The estimated effect at ages 61–64 is small and statistically indistinguishable from 0, while the differential effect at ages 65–68 is positive and statistically significant at the 1% level. The point estimate of 0.231 implies that full exposure to mandatory retirement bans at ages 65–68 results in a 26.0% increase in the number of awarded workers' compensation claims per capita.[C8] The size of this effect is approximately double the implied effects on employment (14%), consistent with the high injury rates for affected workers implied by the estimates in Section 5.2. In column 2, we restrict the sample to cohorts that are observed at least once before and after the age of 65 (1937–50). The estimated DDD effect at ages 65–68 remains significant at the 1% level and increases to 32%. The advantage of this sample is that we can check whether other time-invariant cohort-by-province factors may drive the results. Specifically, we include province-cohort fixed effects in column 3, and find that the estimated effect remains at 32%. Thus, the estimates suggest that the mandatory retirement bans increased workers' compensation claims at ages 65–68 by 26–32%.

Despite different data sources, variable definitions and regressions, our estimates in this section imply similar increases in workers' compensation claims at age 65–68 to our estimates in Section 5.2 for the effect at age 66. The 26–32% increase in workers' compensation claims implies an increase in per-capita claim rates of 0.004–0.005 percentage points. Since only around

[C8] $26.0 = 100 \times (\exp\{0.231\} - 1)$.

Table C1: Percent effect of mandatory retirement bans on workers' compensation awards

| | (1) | (2) | (3) |
|---|---|---|---|
| | Log(awarded claims per capita) | | |
| | All cohorts | 1937-50 | + controls |
| Ban exposure | -0.074 | -0.074 | |
| | (0.087) | (0.087) | |
| Cluster-robust p-value | 0.421 | 0.421 | |
| Wild bootstrap p-value | 0.634 | 0.623 | |
| Implied percent effect | -7.1% | -7.1% | |
| Ban exposure $\times$ $\mathbf{1}(\text{age} \geq 65)$ | 0.231*** | 0.276** | 0.276** |
| | (0.053) | (0.087) | (0.107) |
| Cluster-robust p-value | 0.002 | 0.013 | 0.032 |
| Wild bootstrap p-value | 0.005 | 0.015 | 0.045 |
| Implied percent effect | 26.0% | 31.8% | 31.8% |
| Province-cohort fixed effects | | | ✓ |
| Observations | 2,750 | 2,500 | 2,500 |

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

Notes: This table presents weighted least squares estimates of equation (C1) at the dataset-province-year-age level. Column 1 includes everyone individuals aged 61–68, while columns 2 and 3 restrict the sample to individuals born between 1937 and 1950. Column 3 adds province-cohort fixed effects.

one-quarter of tax filers are employed at ages 65–68, we need to quadruple this estimate (0.16–0.20 percentage points) to make it comparable to our estimate in Section 5.2 of the effects at age 66 among individuals who are employed in the year prior (0.26 percentage points).