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Abstract

Using population-level administrative data, we study labor market externalities stemming from age-specific employment protection legislation (EPL) targeted towards older workers. Our results show no economically meaningful overall effects of the EPL on employment or earnings of either men or women approaching eligibility. Considering separately incumbent workers and non-employees we find small positive and small negative employment effects for the former and the latter groups, respectively.

JEL-Codes: J630, J210, J230.

Keywords: employment protection, older workers, externalities, labor demand.

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1 Introduction

The last three decades saw sharp declines in fertility and increases in the size of older population in many developed countries. Average total fertility rate in OECD countries declined from 1.98 in 1990 to 1.61 in 2019, while fraction of population aged 65 plus increased from 11.4 to 17.1% over the same time frame. This led to stark increases in old age dependency ratios which are projected to be above 3.6 by the end of the 2020s, meaning that there will be on average 3.6 individuals aged 65 and over per 10 people of working age - at ages 20 to 64 (see panels a and b of Online Appendix Figure A.1).

Given these trends, there is a growing policy interest concerning incentives and regulations aimed at extending working lives and increasing productivity of older workers (Eyster et al. 2008; Deelen and Jongen 2009; OECD 2019; Abraham and Houseman 2020). Although labor force participation of near-retirement men and women has increased in the last three decades, it is still well below the rates of prime age population and has been growing at a sluggish pace since the 1990s (panels c and d in Online Appendix Figure A.1). Job retention rates among these workers tend to be low (Diebold et al. 1997; Hardy et al. 2018) and on average their job search lasts longer before they find a suitable job offer (Faberman and Kudlyak 2019).

On one hand, improved working conditions, changes in the nature of work, and better health status of recently aging cohorts should improve their labor market opportunities. On the other, these factors have not eliminated concerns about the costs of employment of this group of workers from the firm's perspective. The primary risks on the demand side include age-related reductions in productivity, high fixed costs of training, challenges in adjustment to new technologies, and inability to adjust the wages or hours of such workers downwards in settings with strict employment regulations (see e.g., Lazear (1990), Abowd and Kramarz (2003), Daniel and Heywood (2007), Perek-Białas and Turek (2012), Behaghel et al. (2014)).

Faced with insufficient demand for older workers, many countries implement policies which make it hard or costly to lay such individuals off (OECD 2019). While such policies benefit the covered groups, the natural concerns relate to potential negative labor market externalities on those who remain unprotected. In particular, age-specific protections might backfire by incentivizing firms to fire workers right before the coverage, and the related increased costs, kick in. In this paper, we explore this question by studying a unique policy: a strict age-specific employment protection legislation (EPL) in Poland, a country facing similar demographic challenges as many other OECD nations (Online Appendix Figure A.1). Since in our application the regulation in question is one of the most consequential from employer's perspective, we view our analysis as providing an upper bound on the potential effects stemming from more moderate policies protecting older (pre-retirement age) workers.

We ask the following research questions relevant to understanding of the externalities from age-specific employment protection: Are there negative effects of pre-retirement employment protection on employment and earnings of individuals nearing the eligibility threshold? Do these effects differ for men and women who - in our setting - face different retirement ages and labor force participation rates? And finally, do these effects differ by pre-policy employment status, across the earnings distribution and employer types?

We answer these questions by leveraging unique high-frequency administrative data from Poland and a tripple-difference research design generated by a quasi-random change in the EPL eligibility triggered by a retirement age reform. The exogenous change in the eligibility cutoff is generated by a reform passed in November 2016 and implemented in October 2017 which unexpectedly (and unintentionally) granted the EPL to 24 and 27 monthly birth cohorts of men (around age 61) and women (around age 56), respectively. Empirically, we take advantage of individual-level data from joint social security and tax registers which allow us to track, at monthly-level, a near-universe of individuals (both employed and not employed) who approach the protection cutoff between January 2015 and June 2018.

We find no economically meaningful effects of the age-specific EPL on employment and earnings of workers nearing the eligibility threshold. Our pooled estimates on a sample of employees and non-employees suggest statistically insignificant, at conventional levels, average employment effects of -0.04 and -0.20 percentage points (pp) for men and women, respectively. Using 95% confidence intervals we can thus rule out negative effect sizes larger than -0.5% and -0.6% for these two groups, respectively. Dynamics of earnings, for those employed, is likewise unaffected. Considering incumbent workers and those non-employed prior to the reform separately, we find small positive employment effects (up to 0.67 pp) for the former and small negative effects (up to -0.91 pp) for the latter population. Delving further into the heterogeneity analysis, we find that these positive effects are concentrated among workers in the most precarious conditions, those employed in high turnover companies and with lowest earnings.

This paper makes contributions to several strands of the literature. First, we extend the limited set of papers examining labor market consequences of age-specific EPL. Behaghel et al. (2008) show that age-specific exemptions from the French 'Delalande tax', which imposed an additional financial cost on employers for terminating contracts of those aged 50+, increased transitions from unemployment to employment for the affected workers. This is consistent with increased willingness to hire workers who might be laid off without the extra cost. In a recent paper, Saez et al. (2023) using Swedish data show that removing of EPL at the age of 67, among individuals who are already eligible for retirement pensions, leads to increased job separations, though the effects are modest. These findings are complementary to our work as we consider the implications of age-related EPL several years prior to statutory pension age. Except for these two studies there is an important knowledge gap when it comes to the consequences of age specific EPL recently highlighted in OECD (2019). Furthermore, to the best of our knowledge, no prior work examined the potential externalities from EPL coverage for those nearing eligibility - a group of workers which is most likely to be negatively affected.

Second, we add to the broader EPL literature where empirical findings are mixed and appear context- and methods-specific. Cross-country studies show that stringent EPL reduces demand for labor (Lazear 1990; Kahn 2007), although this association is sensitive to the business-cycle (Messina and Vallanti 2007; Duval et al. 2020). There is also evidence that restrictive labor legislation lowers productivity growth of industries (Bassanini et al. 2009) and increases investments in technology (Griffith and Macartney 2014). On the other hand, Bassanini and Garnero (2020) find no increase in separation rates but rather reduced rates of within-industry transitions while Kahn (2010) finds no employment effects of more lenient EPL. Studies using within-country data, focusing on specific regulations and reforms, find more consistently adverse effects of the EPL. Autor et al. (2007), Kugler and Pica (2008) and Kan and Lin (2011) show that stricter EPL reduces employment flows and job turnover. Hijzen et al. (2017) show that Italian firms facing stricter EPL increase their hiring of workers on temporary contracts (uncovered by the EPL). This is confirmed by Daruich et al. (2023) who additionally show that loosening EPL for temporary contracts increases firm profits but does not necessarily lead to increases in employment. Boeri and Garibaldi (2007), Sestito and Viviano (2018) and Yoo and Kang (2012) document negative relationship between EPL and employment that can, however, be transitory. Conversely, Ichino and Riphahn (2005), Martins (2009), Jacob (2013), and Bjuggren (2018) provide evidence that lowering the the level of protection could increase labor productivity and firm performance. When it comes to wages the results are more mixed with van der Wiel (2010) finding increases in wages while Leonardi and Pica (2013) finding decreases as a result of stricter EPL. Closest to this paper, Cahuc et al. (2019) show that stricter EPL leads to increased separation rates for workers nearing the additional coverage, highlighting the importance of considering potential negative externalities for uncovered workers. This finding could be particularly relevant for the population of older workers who already have low employment rates and experience labor market discrimination. Furthermore, given the aging society, they are of particular importance for the policy makers.

Third, we contribute to research on labor demand for older workers, especially those nearing retirement age. Experimental work documents robust discrimination in hiring of older workers (Bendick et al. 1997, 1999) with more recent studies highlighting the particular disadvantage of older women (Lahey 2008; Neumark et al. 2019). Furthermore, Boockmann et al. (2012) and Huttunen et al. (2013) document limited employment effects of wage subsidies and lower payroll taxes for older workers, respectively. On the other hand, Albanese and Cockx (2019) show that such wage subsidies increase retention rates of older workers. Although much of the retirement age policies focus on the supply side (e.g. Krueger and Pischke (1992), Staublii and Zwimüller (2013) or Laun and Palme (2023)), firm's demand for older workers could likewise be affected by such legislation. For example, Hakola and Uusitalo (2005) show that sharing early retirement expenses with employers reduces early labor market exits of older workers in Finland; Frimmel et al. (2018) show that Austrian firms play an active role in the determination of their workers' retirement age effectively pushing out those that are more costly; Rabaté (2019) reaches similar conclusion using the progressive ban on mandatory retirement in France; and Morris and Dostie (2023) show that banning mandatory retirement in Canada reduces separation rates for older workers. Additionally, Hairault et al. (2010), Ilmakunnas and Ilmakunnas (2015) and Bertoni and Brunello (2021) study how retirement age policies affect workers prior to the retirement eligibility. They all suggest that workers near the eligibility cutoff suffer in terms of their employment prospects - a result highlighting the need to understand any potential negative spillovers of the EPL for those approaching eligibility. Finally, although this perception is not necessarily supported by the data, one of the main concerns when it comes to the demand side is declining health and productivity of older workers. Mahlberg et al. (2013) document that productivity of Austrian firms is not related to the share of older workers they employ while Börsch-Supan and Weiss (2016) show that individual worker productivity in an assembly plant is stable until at least age 60.

Finally, we contribute to research on societal aging and its consequences. The aforementioned increases in old age dependency ratios put strain on the solvency of the social security systems (see Jimeno et al. (2008) for overview of this research) and will likely lead to lower economic growth (Kotschy and Bloom 2023). Furthermore, restricted employment opportunities of older workers could increase inequality and poverty rates among the elderly (Deaton and Paxson 1998). These factors could be one reason behind increasing voluntary retirement age in the US (Brown et al. 2022). Finally, recent work suggests that rapid population aging will soon become an important policy consideration beyond the developed world including in Africa (Duhon et al. 2023). One solution to underemployment of older workers could be an age-specific EPL which mechanically keeps them attached to the labor market. Such a policy would, however, only be effective to the extent that it does not generate negative externalities by incentivizing firms to lay off those who are about to be covered. Should such effects be present, the burden on the social security systems could actually increase.

We view our results as having two key policy implications. First, we show that age-specific employment protection targeted at older workers does not have major negative externalities with regard to employment and earnings for the soon-to-be-covered individuals. Since the protected workers are guaranteed employment until the retirement age, unless they decide to quit themselves, without the negative externalities the policy actually increases aggregate employment rates of older workers. Thus, our findings question the common policy concern – stemming from the labor demand model – that pre-retirement EPL, by increasing costs to employers, leads to a trade-off between benefiting those already protected and hurting those employees who are nearing eligibility. Second, our results highlight the importance of considering incumbent and not employed workers separately. We show that the null overall effect stems from slight positive employment effects for the incumbents and slight negative employment effects for those who are not employed. The implications for the latter group, despite relatively small effect sizes of the estimates, could be of a concern for policy. The positive effects for incumbent workers are further exacerbated among those working in more precarious jobs - those lower paid and employed in firms with high turnover. We speculate that one reason for such findings could be the fact that EPL induces increases in effort of incumbent workers who are trying to avoid being laid off in the final months without the coverage.

2 Institutional background

2.1 Pre-retirement employment protection legislation

Pre-retirement employment protection legislation (EPL) in Poland is regulated by the Labor Code, a set of laws which is separate from the Civil Code and focuses solely on labor relations. Article 39 of the Code specifies that an employer cannot terminate a contract if an employee has at most 4 years left until the retirement age and is eligible to retire at that time. This regulation has been present in the Polish Labor Code since its inception in June 1974, though it was extended from two to four years in 2008.[1] It applies to both open-ended and fixed-term labor contracts but excludes those who are eligible for disability pensions. If the company breaks the law and dismisses an employee who is eligible for employment protection it can be sued in the court on the basis of both the Labor Code and the Criminal Code. Importantly, the regulations encompass not only inability to terminate a contract but also lowering of wages, changing of hours of work, or moving to a more burdensome role or position. Exceptions include mass layoffs, dismissal due to disciplinary reasons, or restructuring of the wage scale for the whole company, however, these are not easily gameable by employers, often take time to implement, and are monitored closely by the authorities. Overall the Polish pre-retirement EPL is inflexible, offers little leeway to employers and is costly from their perspective. Anecdotal evidence suggest that the law is broadly obeyed while the courts tend to side with plaintiffs when it comes to its violations. Although

[1] The extension of the protection period from 2 to 4 years in 2008 was introduced as part of an agreement between the government and social partners in a package limiting access to early retirement. In 2023, an additional regulation was implemented further strengthening protection by forcing the employers to cover employees' wages during the potential litigation related to unlawful dismissal.

aggregate statistics related specifically to violations of the pre-retirement EPL are not available, between 2015 and 2018 the district labor courts in Poland issued 31,301 verdicts related to employment contract termination (with prior notice) lawsuits, which include EPL cases specifically covered by the Labor Code (those in pre-retirement age and other groups, e.g. pregnant women or members of trade unions). Among those cases only 6846, or 22%, were dismissed. The remaining cases resulted in either ruling in plaintiff's favor or out of court settlement, both of which are financially costly for employers. During the same time period, criminal courts issued 700 convictions on the basis of Article 218 of the Criminal Code concerning violations of worker rights; which constituted 54% of all criminal convictions related to employment regulations.

Employees who are not covered by specific types of additional EPL have little protection against dismissal, irrespective if they are on an open-ended or a fixed-term labor contract. According to the Labor Code (Article 30 Paragraph 4) employers can dismiss an employee as long as the reason for a lay off is (i) justified, (ii) specific, and (iii) true, so that an employee is aware why they are being dismissed. If the lay off is not due to disciplinary reason and if the worker was employed for at least three years, then the firm needs to provide a 3-month compensation. Thus, unlike in other European countries, Polish employers appear to be much less constrained when it comes to their hiring and firing decisions of workers uncovered by the EPL. This also means that a cost of dismissing worker changes discontinuously at the time when the age-specific EPL kicks in.

2.2 Retirement age legislation

Retirement regulations prior to January 1st, 2013 stipulated a retirement age of 65 and 60 years for men and women, respectively. This was changed by the 2012 reform which legislated a gradual increase in the statutory retirement age so that it becomes equalized at age 67 for both genders. Specifically, the retirement age was to grow gradually beginning in January 1st 2013, with increases of one month per each calendar quarter. Thus, men would have reached their target age of 67 in October 2020 while women in October 2040, due to the initial five year gap between the two groups.

The 2012 reform, which had been unannounced in the electoral campaign of 2011, proved unpopular and the return to the pre-reform retirement age became one of the key pledges of the opposition candidate, Andrzej Duda, in the 2015 presidential race. The promise played an important role in Duda's electoral victory and the theme of returning to a lower retirement age was again featured in the parliamentary elections in October 2015 (see Google search statistics presented in Figure 1). The incumbent coalition parties ended up losing both the presidential and the parliamentary elections and before the end of the year the new president presented the legislation to the parliament, which stipulated reverting to retirement age regulations from before 2013. This was done despite rapid population aging and against the economic and policy analyses provided both by independent entities and by the government itself, which was far from enthusiastic to embrace the change.

Following a freeze on the parliamentary discussion on this issue and a series of alternative options which were considered by the government, in the end the presidential proposal returned to parliament a year later. After a very brief debate in the parliament it was swiftly passed in November 2016 and signed into law on December 19th 2016 (Google search statistics presented in Figure 1 confirm increased interest in the issue of retirement age at the time). The new regulations came into effect on October 1st 2017, after less than a year of a hold-up period. By the time the new law came into force, multiple cohorts saw their retirement age had grown as a result of the gradual increases implemented since 2013. A month earlier, in September 2017, retirement eligibility was granted to men born in July 1951 (i.e. aged 66 and 2 months) and to women born in July 1956 (i.e. aged 61 and 2 months). The reversal meant that all cohorts who were 65 (men) or 60 (women) on October 1st 2017 were granted retirement rights on that day. Although they did not have to retire, the reform attracted a lot of attention and thousands of individuals took advantage of the opportunity to retire in accordance with the new regulations. In the last quarter of 2017 and first quarter of 2018, Polish Social Security Institution (ZUS) registered over 357 thousand new pension claims, while in 2015 and 2016 the same two quarters saw just over 100 thousand new claims.

2.3 Using the 2016 reform to estimate the effects of pre-retirement employment protection on workers approaching eligibility

Since the EPL was not defined with respect to a specific age, but rather with respect to the retirement age, cohorts whose retirement age grew after the 2012 reform became eligible for the benefit at a later age. Conversely, the policy reversal automatically (and unexpectedly) made multiple younger cohorts eligible for the EPL. Importantly, discussions surrounding the 2016 reform clearly drew attention primarily to the statutory retirement age and not the implied extension of the EPL (see Google search statistics in Figure 1). And yet, the gradual increase in retirement age for subsequent cohorts, meant that there were 24 monthly birth cohorts of men and 27 cohorts of women, who on October 1st, 2017 became covered by the additional age-related employment protection benefits.[2] Critically from the perspective of our research design, the workers who were about to gain the EPL coverage on October 1st 2017 were nonetheless unprotected during

[2] The difference in the number of eligible cohorts of men and women results from the fact that the youngest cohorts of men covered by the additional protection in October 2017 would have retired after October 2020 having reached the statutory retirement age of 67.

the policy hold up period, i.e. the time from the date it was signed into law (i.e., December 19th 2016) to when it was implemented (i.e., October 1st, 2017). The hold-up period thus generates a quasi-experiment allowing us to estimate the externalities in question. Furthermore, it is a time during which the employers and employees could adjust their labor demand and supply with the new rules in mind. In particular, to the extent that the additional protection is costly for firms, we expect most of the terminations to happen during this time period before they become illegal on October 1st. At the same time, we might expect positive labor supply responses of workers who might exert additional effort in order to signal their value to the firm and avoid termination.

Details on how the reforms affected specific cohorts are illustrated in Figure 2 which maps the two retirement age regimes in two dimensions: the month of birth and the calendar month (separately for men - Figure 2a, and women - Figure 2b). The Figure shows the dynamics over time (horizontal axis) of the 2012 and 2016 retirement age reforms and the corresponding coverage of EPL eligibility by monthly birth cohorts (vertical axis). The gray area delineates the statutory retirement age and reflects eligibility for retirement pension benefits according to the 2012 regulations and the growing retirement age. Retirement age of 67 would have been reached by October 2020 for men and October 2040 for women (not shown on the Figure). Corresponding to the gray area is the red area which reflects the time when according to the 2012 law specific monthly birth cohorts are eligible for additional age-related employment protection, starting four years prior to reaching their retirement age. The navy-blue shaded area (which overlaps part of the red area) shows additional months of retirement eligibility gained as a result of the 2016 reduction in retirement age which came into force on October 1st 2017. In October 2017 cohorts of men born between August 1951 and October 1952, as well as women born between August 1956 and October 1957 became eligible to claim retirement pensions, in addition to those born in July 1951/56 who would have been the only cohorts gaining retirement pension eligibility absent the reform. As it is clear from Figure 2 the retirement eligibility gains were higher for later born cohorts, whose retirement age was higher under the 2012 regulations. The orange shaded areas in turn show the additional months of EPL coverage for the younger cohorts for whom the 2016 reform changed the statutory retirement age.

All cohorts of men born after November 1st 1954 and women born after August 1st 1959 would at some point become eligible for the additional employment protection, which, as a result of reverting to the retirement ages of 65 and 60, would now cover them as they reach the age of 61 and 56, respectively. In this paper though, we restrict our attention to the 24 monthly cohorts of men and 27 monthly cohorts of women who became eligible for additional EPL on October 1st 2017 (orange shaded areas in Figure 2). These include men born between November 1st 1954 and October 31st 1956 as well as women born between August 1st 1959 and October 31st 1961. Among those cohorts we select individuals that gained the largest number of months of additional protection, and - in theory at least - would have been of particular concern for their employers given that their expected wage bill would be the highest should they be employed past their EPL eligibility date. Thus, in our main analysis we define the treatment group as ten monthly cohorts of men and women, born respectively January-October 1956 and January-October 1961. In Section 6 we show, however, that our results remain broadly unchanged when we alter the number of months considered when defining treatment.

To define the control groups, for reasons outlined below, we use cohorts who are two years younger. Although they will eventually also become eligible for the EPL at an earlier age, we assume that in October 2017 their eligibility is far enough into the future that it would not be considered as a factor in their employers' demand decisions. Specifically, we use January-October 1958 cohorts for men and January-October 1963 cohorts for women as controls. We use the same set of months to account for any season-of-birth effects. It is worth noting though, that to the extent that individuals from the control groups are potentially also negatively affected, our estimates should be treated as a lower bound. Additional samples used for robustness analyses are described in more details in Section 6 while details of the samples are presented in Tables A.1 and A.2 in the Appendix.

This choice of cohorts forms the basis of our quasi-experimental design. The first difference is the before-after 2016 reform while the second difference is the 1956(61) vs. 1958(63) birth cohorts for men (women). This yields a standard difference-in-differences design. A complicating factor in this approach, however, is that it ignores any potential age-related differences in labor market outcomes between our treatment and control groups; especially given a two-year gap between those groups. In other words, even if parallel trends hold in the pre-treatment period, there may be factors related specifically to age which would affect the treated (older) cohorts in the post-period but that do not affect (to the same extent) the control (younger) groups. The most obvious reason in the context of employment of older people is the correlation between age and health: If age-specific effects exist, they could affect the treated and control groups differentially and could invalidate our parallel trends assumption. Thus, to account for the potential age-specific effects we define a third difference where we use cohorts lagged by one year. In essence therefore, we assume that these age-related confounders would play the same role in the labor market dynamics of cohorts 1956 and 58 (for men) in the year of our treatment, as they would in the case of cohorts 1955 and 57 a year earlier when the age of these earlier cohorts was the same (the corresponding birth years in the sample of women are 1961/63 and 1960/62). In Section 4 we outline how these considerations translate into the estimating equations.

2.4 Firm and worker strategies in light of age-related EPL

The 2016 reform unexpectedly granting employment protection to a specific group of workers plausibly changed the incentives when it comes to both the labor demand as well as the labor supply. Importantly, it involved an almost year-long hold-up period which allowed for adjustments in both of these dimensions.

Employers' decisions need to weigh the benefits of continued employment of workers, some of whom may have been with the company for a long time and possess valuable know-how, with the expected costs that go beyond remuneration and need to consider other risks of limited workforce adjustments forced by additional employment protection. Some of these uncertainties include possible divergence of wages and productivity as employees grow older or additional sick leave. Given the legislation these expected costs extend for the period of up to four years, over which employers cannot terminate potentially "deadwood labor". Furthermore, the workers who gained the most months of protection - our treatment group - are those who would generate the highest burden. To the extent that firms incorporate these risks in their production decisions, it may be optimal to terminate employment of some of the workers right before they become covered by the EPL i.e., in our empirical setting towards the end of the hold-up period. Furthermore, firms might reduce hiring of workers in this specific age-range for the same reasons.

Employees' decisions are more straightforward given that they can always quit their jobs regardless of the EPL coverage. At the same time, once they reach a specific age, the additional 4-year protection period is a clear benefit. Those workers who are not concerned about their productivity and employment prospects are unlikely to change their labor supply as a result of the reform. On the other hand, those employees who are concerned about being laid off would be less likely to quit and more likely to increase their effort in order to signal their commitment to the employers and to ensure that they are not perceived as the types who are likely to turn into "deadwood labor". This is similar to a signalling problem (Spence 1973) where incumbent workers try to reveal their type and reduce information asymmetry through effort. Since for non-employed workers it is much harder (if not impossible) to communicate their true type, we do not expect meaningful supply responses in this group.

Bringing the theoretical predictions stemming from the reform to the data is not straightforward, however, given that our administrative data only contain information on who is employed and what their remuneration is. Thus, as in most prior research, we cannot separate quits and lay offs and the employment effects we observe will reflect a combination of both demand and supply responses. Nonetheless, we can divide workers into incumbent and non-employed to shed some light on the plausible mechanisms. In particular, for non-employed individuals we expect the demand channel to dominate while for incumbent workers the observed overall outcome will be a combination of both the demand and the supply channels.

Another complicating factor is that Polish law requires employers to present employees with contract termination notices prior to formal separation. Although the notice date is binding when it comes to the EPL regulation, and thus employers may hand them in right up to the moment the employees reach the age of protection, employees might still be observed on the payroll for a few more months. This is due to the lag between the notice and the actual work separation which for long term contracts may last up to three months.[3] For this reason we first present our results in the form of event studies, showing the evolution of the analysed outcomes prior to the legislation, during the hold-up period and then several months after the reform came into force. Similarly when estimating average treatment effects we consider several months around the time of the implementation of the reform. It is also worth noting that prior to the 2016 reform the age of protection kept growing for subsequent cohorts together with the increasing statutory retirement age. This, combined with the fact that job separations may not be immediate means that any discrete jumps in separations at the eligibility cut off would be difficult to identify prior to the reform. We thus cannot document bunching at the eligibility cutoff and reliably use regression kink or discontinuity designs. These constraints motivate our triple-difference design outlined in Section 4.

3 Data

We use a dataset of combined individual-level information from several administrative sources matching monthly labor market information from the social and health insurance register (ZUS), annual income tax data, and basic individual characteristics (gender, date of birth and death) from the PESEL registry. The data has been compiled at the Polish Ministry of Finance and the matched information is available from January 1st 2015. Labor market data used for our analysis end by June 31st 2018, but we also use additional data up to October 2020 to specify our sample selection criteria. We therefore observe labor market outcomes for up to 22 months prior to the November 2016 reform, 11 months of the hold up period and 9 months after the legislation came into effect.

The ZUS data include monthly information on all income sources for which the social security and health insurance contributions are paid and have a near-universal coverage of the working population.[4] The dataset also facilitates a match between employers and employees as well as

[3] The lag depends on, among others, type of contract, tenure, and prolongation strategies that employees might engage in such as sick leave or personal time off. Furthermore, the process of handing in notices can itself be staggered over time and it's unclear if all workers would receive the notice on the last possible day before the protection kicks in.

[4] The data do not include farmers, students' with only temporary jobs, and individuals with only result-based contracts. Additionally, we exclude uniformed services as well as judges and public prosecutors from the data because

includes employer industry codes. We can thus derive such firm-level characteristics as total employment, turnover and age composition of the workforce at the firm level, all measured prior to the policy change, which we take advantage of in the heterogeneity analysis. On the basis of industry codes we further exclude individuals eligible for industry-specific retirement regulations and employees in the public sector.[5] We do not observe individual level information on occupations.

Additionally, using data on pension claims from the ZUS registry we drop men/women who by the age of 62/57 claimed either early retirement (so-called "bridge pension") or disability pensions.[6] The 'bridge pensions" should be thought of as exogenous given that the eligibility for this specific pension, which varies at birth cohort-by-age-by-occupation/industry level, was determined long prior to the reform and applies only to people who for at least 15 years of their career (including time before 1999) worked in "special conditions".[7] We drop these individuals since they could confound the reform variation which is partially determined at cohort-by-age level. In robustness analysis we adjust these selection criteria to remove only those claiming "bridge pensions" (i.e. including those who end up claiming disability pensions, in the case of which there is a higher risk of potential endogeneity with respect to the EPL eligibility) as well as broadening them to cover claims of any form of pension. We also experiment with the pension qualification cut-off age changing it to 61.5/56.5.[8]

The dataset includes all individuals who at any point between 2015-2020 have paid any social or health insurance contributions (including contributions paid by the government's labor office for the registered unemployed) and/or filed any income tax.[9] Overall, the starting sample for our main analysis among cohorts considered as treated (i.e. men/women born in January-October,

these jobs are regulated separately. A worker can have several records each month (e.g., if they change jobs or work at more than one institution) and in such cases we select the observation within a month with the highest earnings.

[5]Specifically, we exclude the following sectors: primary sector e.g., hunting (A), mining (B), water and sewerage management (E), scientific contracting and research (M), public administration and military (O), education and teaching (P), household production for household use purposes (T), and international organizations (U).

[6]For this purpose we use data from the registry all the way up to October 2020, which is the month in which the youngest control cohorts (men born in October 1958, women born in October 1963) turn 62/57.

[7]The list of these conditions includes, for example, jobs performed underground, heavily demanding physical jobs and those requiring special psycho-physical agility.

[8]Because information on pension receipt (including the type of pension) in the matched administrative dataset is available only from 2016, when we impose the pension claim criterion at 61.5/56.5 we have to limit the monthly birth cohorts we examine to July-October (rather than January-October as in the main specification). This is because the July cohort is the first for which we have pensions observation at the point when the oldest cohorts turn 61.5/56.5 (men born in July 1955, women born in July 1960). Ideally, we would observe pension receipts at ages 61/56 but, alas, this data is not available. Our results are robust to using either 62/57 or 61.5/56.5 cutoffs so we are less concerned about this limitation.

[9]Although registering as unemployed with the labor office is voluntary, it is tied to health insurance coverage and thus most of the unemployed register to receive (temporary) unemployment benefits and (permanent) health insurance coverage.

1956/61) corresponds to 155,087 men and 150,368 women. After applying the pensions and industry/sector selection criteria we end up with 54,396 men and 60,374 women in the treated sample. Corresponding numbers for the control sample as well as for samples used in the triple difference estimation under various assumptions are given in Online Appendix Tables A.1 and A.2 for men and women, respectively. The preferred triple difference estimation is based on a sample of over 233 thousand men and 246 thousand women. Considering employment as an outcome we use balanced panels while earnings analyses are based on unbalanced panels of earnings observations.

The two outcomes of interest are an indicator for being an employee and labor earnings. The former variable takes value of one for everyone who is employed on a labor contract ("umowa o pracę"), i.e. a contract that is subject to coverage of the EPL, and zero for all other individuals. This means that the non-employed group in our sample includes individuals who are: currently not working, employed on civil contracts, or self-employed.[10] Labor earnings are monthly earnings on labor contracts as recorded in the social insurance database.

4 Empirical Specifications and Identification

Mapping the institutional setting to the data, recall first that we have monthly level information on labor market outcomes between January 2015 and June 2018. We index these observations by t which runs from 1 (January 2015) to 42 (June 2018). We also have monthly birth cohorts c which will be differentially affected by the reform changing the eligibility for the EPL. We consider men born in 1956 (1961 for women) as a treatment group while those born in 1958 (1963 for women) as a control group. We run all the analyses separately by gender. Since the reform was passed in the parliament in November 2016, we treat this month as the first treated time period (month 23 in our time index t) i.e., $\mathtt{Post}_{it} = \mathbb{1}[t \geqslant 23]$. These two differences constitute our baseline difference-in-differences setting: we compare the 1956 (1961) monthly birth cohorts to the 1958 (1963) monthly birth cohorts, before vs. after November 2016. The event study for estimating these effects is given by:

$$Y_{ict} = \alpha + \sum_{j=-22}^{-2} \beta_j^1 T_{ic} \mathbb{1}[t - Nov2016 = j] + \sum_{j=0}^{19} \beta_j^2 T_{ic} \mathbb{1}[t - Nov2016 = j] + \gamma_t + \delta_c + \varepsilon_{ict} \quad (1)$$

where Y_{ict} is an indicator equal to one if individual i born in monthly cohort group c ob-

[10]The latter two groups are not eligible for the EPL coverage. We include these individuals in the main specification because laid off individuals could move to these forms of activity and because firms could change the form of employment to avoid the potential costs of employing individuals covered by the EPL.

served in calendar month-year t is employed on the EPL eligible contract, and zero otherwise. Additionally, for those employed we use monthly labor earnings as another outcome variable. T_{ic} is an indicator variable taking value one for men born in January to October 1956 and women born in 1961 (versus zero for men born in January to October 1958 and women born in 1963) while $\mathbb{1}[t - Nov2016 = j]$ are event time dummies running from -22 (t month 1 i.e., January 2015 minus t month 23 i.e., November 2016) to 19 (t month 42 i.e., June 2018 minus t month 23 i.e., November 2016) and we omit October 2016 (indexed as $j = -1$ at t month 22) as the reference period. We also include two sets of fixed effects defining the comparisons: γ_t for calendar month and δ_c for monthly birth cohort. Parameters of interest in Equation 1 are β_j, where β_j^1 represent pre-treatment period estimates allowing examination of the parallel trends assumption and β_j^2 represent post-reform dynamic treatment effects.

As outlined in Section 2.3, one issue with the difference-in-differences approach is that it does not account for differential effects of ageing between the treated and control cohorts. In that, our treatment group is always older, and thus, through health and family shocks, could have adverse labor market outcomes irrespective of the stricter EPL (potentially downward biasing our results). Note that this problem is independent of the parallel trends assumption, which needs to hold in the post-reform period absent the treatment, and it still could be that the post-reform effects are exacerbated due to the fact that the treated cohorts are relatively older compared to the control cohorts. To mitigate this, assuming that the aging effects are the same for cohorts born a year earlier, we introduce a triple difference event study design of the following form:

$$
\begin{aligned}
Y_{ict} = \alpha + \sum_{\substack{j=-10 \\ j\neq -1}}^{19} \zeta_j D_{ic} \mathbb{1}[t - Nov2016 = j] + \sum_{\substack{j=-10 \\ j\neq -1}}^{19} \phi_j A_{ic} \mathbb{1}[t - Nov2015 = j] \\
+ \sum_{j=-10}^{-2} \theta_j^1 (D_{ic} \mathbb{1}[t - Nov2016 = j])(A_{ic} \mathbb{1}[t - Nov2015 = j]) \\
+ \sum_{j=0}^{19} \theta_j^2 (D_{ic} \mathbb{1}[t - Nov2016 = j])(A_{ic} \mathbb{1}[t - Nov2015 = j]) \\
+ \gamma_t + \delta_c + \varepsilon_{ict}
\end{aligned}
\tag{2}
$$

where Y_{ict}, γ_t, and δ_c are defined as in Equation 1 while we shift the treatment and control groups by 12 months to generate the third difference proxying for the aging effect. This is reflected by the fact that in our "true reform" sample the first treatment month remains November 2016 while in our "placebo reform" sample the first treatment month is November 2015. Since our data do not extend before January 2015, due to taking the 12-months lag, we need to trim the left hand side of the event study and hence in this specification $-10 \leqslant j \leqslant 19$. We also need to modify the definition of treatment and control groups (T_{ic} in Equation 1) and hence we rewrite it as D_{ic} which takes value one for individuals born January to October 1956 (T_{ic} in Equation 1) or January to October 1955 (placebo treatment cohorts) for men and in 1961 or 1960 for women. An indicator variable, A_{ic}, then takes value one for men born January to October 1956 or January to October 1958 (1961 or 1963 for women) reflecting the "true reform" sample cohorts. Parameters of interest in Equation 2 are θ_j, where θ_j^1 represent pre-treatment period estimates allowing examination of the parallel trends assumption and θ_j^2 represent post-reform dynamic treatment effects. In Section 5 we present results from Equations 1 and 2 in parallel on the same set of graphs.

Finally, we present average treatment effects by estimating double and tripple difference equations. Recall that the reform was passed by the parliament in November 2016, signed into law in December 2016, but became binding only on October 1st 2017. Thus, employers could react to the reform by adjusting their employment all the way up to September 2017 and we might expect the spillover effects of the additional protection for our treated cohorts to be most evident close to and beyond the time when the retirement age reform comes into force. Note, however, that as time passes eventually the control cohorts in our design also begin to get closer to their own age-specific protection. In this case, if external effects of approaching protection are homogeneous across cohorts, the difference between our treated and control cohorts could be attenuated over time. To account for this, when considering pooled regressions, the estimation is based on data for six pre- and six post-treatment calendar months. Equation 3 summarizes our approach for the triple difference estimation:

$$
\begin{aligned}
Y_{ict} = \alpha + \theta \mathrm{Post}_{it} \times D_{ic} \times A_{ic} + \zeta \mathrm{Post}_{it} \times D_{ic} \\
+ \phi \mathrm{Post}_{it} \times A_{ic} + \gamma_t + \delta_c + \varepsilon_{ict}.
\end{aligned}
\tag{3}
$$

Pre-treatment months include May-October (2016 for "true reform" and 2015 for "placebo reform"), while post-treatment months, defining Post_{it} in Equation 3, cover September-February (2017-2018 for "true reform" and 2016-2017 for "placebo reform").[11] After the policy was legislated in November 2016 employers could decide until September 2017 whether to keep workers from the cohorts which would gain additional protection from October. As most employment contracts have a leave period of up to three months, this means that the full effects of the policy would materialize over the following months. Y_{ict}, D_{ic}, A_{ic}, γ_t, and δ_c in Equation 3 are de-

[11] We limit the post-treatment months to February as for the later months we could not exclude potential non-zero implications of the new legislation for our "placebo reform" cohorts during the hold-up period.

fined as in Equation 2. The parameter of interest in Equation 3 is θ and it describes the average treatment effect of stricter EPL on labor market outcomes of workers approaching the eligibility threshold (older) compared to workers farther away from eligibility (younger), net of the aging effect accounted for by the third difference. Since we have repeated observations on the same individuals, in all estimating equations we cluster the error term, ε_{ict}, at the individual level.

5 Results

Figure 3 presents our main results for men (left column) and women (right column) in the form of event studies as specified in Equations 1 and 2. The dashed vertical line defines October 2016, the last month before the reform was legislated in the parliament, while the solid vertical line represents October 2017, when the legislation become binding and workers in the treatment group became covered by additional age-related employment protection. Thus, the period between November 2016 and October 2017 is our hold-up period where we expect most of the adjustment to the forthcoming EPL eligibility - both on the employer and the employee side - to happen. The top row (Figures 3a and 3b) shows overall estimates for employment, while the middle row (Figures 3c and 3d) the estimates for earnings conditional on employment. The gray plots represent the difference-in-differences event studies (Equation 1) and the black plots show the triple difference estimates (Equation 2). Considering the former design we see no pre-trends when it comes to employment but a declining trend in the pre-reform period for earnings. The latter reflects our concerns related to the fact that our treatment group is two-years older than our control group, and may therefore experience different labor market outcomes regardless of any labor market legislation. While there is no evident pre-trend leading up to November 2016 in employment, this does not guarantee that the age-specific differences would not show up later in the life of the two cohorts (post-treatment parallel trends), and thus be confused with the implications of the treatment. We address this problem using the triple difference design which accounts for the differential effects of age. As we can see in Figures 3c and 3d differencing out the age effects addresses the pre-trends in earnings. While the pre-trends in the employment event study (Figures 3a and 3b) are not affected, the estimates suggest that the small negative effects which are assigned to the additional employment protection in difference-in-differences coefficients, disappear in the triple difference specification.

In the full sample irrespective of the outcome, employment or earnings, we do not see any statistically significant or economically meaningful effects of the forthcoming additional employment protection for the treated cohorts, either for men or for women. Table 1 summarizes these results based on Equation 3. Our preferred triple difference employment estimates suggest statistically insignificant treatment effects of -0.0004 and -0.0022 (i.e.: -0.04 and -0.20 percentage points) for men and women, respectively. Given the standard errors we can rule out negative effects as small as 0.5% and 0.6% for the two groups, respectively. Note that similarity of the effect sizes across genders is partially driven by the differences in the baseline employment probabilities, with women having about 10 percentage points higher employment rate. The difference is, on the one hand, due to the fact that women in our samples are five years younger then men, and on the other hand, that those out of the labour market are more likely to be completely outside of the administrative sample. It is worth noting that the higher employment level of these younger cohorts of women compared to the older cohorts of men is consistent with data from Polish Labor Force Survey. For earnings the preferred point estimates imply statistically insignificant reductions of 0.1% for men and statistically insignificant increases of 0.3% for women. Given the standard errors we can rule out negative effect sizes as small as -0.9% and -0.3% for the two groups, respectively. Here, unlike for employment, we find that average earnings of women are about 13% lower than those of men. Overall, we view even the lower bounds of these effects as economically very small.

Importantly, note that the double difference estimates reported in Table 1 for completeness suggest negative, larger, and statistically significant at conventional level treatment effects for both employment and earnings.[12] Figure 3 clearly illustrates why this occurs. For earnings we see a negative pre-trend that continues into the post-treatment period, while for employment there is no pre-trend but the post-reform treatment effects are larger compared to the triple difference design. Both facts are consistent with potential negative labor market effects of ageing which is corrected by taking the third difference using adjacent cohorts. It is worth noting though, that even these downward biased estimates are relatively small and imply effect sizes that do not exceed 0.3pp in the case of employment and 1% in the case of earnings.

Given the hypotheses outlined in Section 2.4, in the next step, we split the sample based on pre-reform employment status into those with "stable employment" history and those "out of employment". We define individuals as in "stable employment" if they are continuously employment between July and September while those who in at least two of the three months were unemployed are considered as not employed. The same conditions are applied for the treatment and the control samples both in the "true reform" and "placebo reform" samples for the triple difference estimation. The triple difference event studies are shown in Figures 3e and 3f for men and women, respectively, and the results are summarized in Table 1.

We see small, negative but statistically significant at conventional levels results for the non-employed samples both in the case of men and women, with the estimates of -0.57 and -0.91

[12]The difference-in-differences estimating equation is $Y_{ict} = \alpha + \theta \mathrm{Post}_{it} \times T_{ic} + \gamma_t + \delta_c + \varepsilon_{ict}$ where Post_{it} takes value of one for September-February 2017-2018 and zero for May-October 2016 while T_{ic}, γ_t, and δ_c are defined as in Equation 1.

percentage points (pp) respectively, and these negative effects clearly evolve over the hold-up period between the time of passing of the retirement legislation in the parliament and the time it took effect in October 2017. This makes sense if during the hold up period employers have particularly strong incentives to refrain from hiring outside workers who soon will be covered by the EPL and about whose productivity they have limited knowledge. The effects then reach a maximum and stabilize in subsequent periods when the treatment group enters the age of employment protection. In contrast, the reform effects for the employees' samples are modestly positive. As shown in Table 1 employment among men grows by 0.67 pp and among women by 0.19 pp. As highlighted in Section 2.4, for those already employed, where asymmetry of information between the firm and the worker is smaller (compared to outside hires), the effects combine two plausibly opposite forces: (i) firm's incentives to lay off potential "deadwood labor" (negative employment effect) and (ii) worker's greater efforts to remain employed across the protection threshold and to signal that they would not become "deadwood labor" (positive employment effect). Our estimates capture the sum of those opposing forces. We would expect disemployment effects if the former dominates and positive employment effects if the latter dominates. For both men and women our results are consistent with a positive supply side reaction outweighing the negative demand effects stipulated in the extant EPL literature.

The results for the full sample, and in particular those for the sample of employees stand in contrast to what we could expect having in mind a simple labor demand model and given the results of the two aforementioned studies focusing on older workers. While Behaghel et al. (2008) does not provide effects on earnings, their effects on transitions from unemployment to employment for older people are very large at up to 53%. Saez et al. (2023) is the paper closest to ours as they examine both employment and earnings (conditional on employment effects). They find separations for 8.4% of jobs once the EPL protections end at the age of 67 while for those who remain employed the earnings decline by about 8%. These findings are, likewise, an order of magnitude larger than the externalities which we document in this work.

We propose three plausible reasons for why the effects might differ. First, individuals in our sample are not yet covered by the EPL while those in Sweden are. In that, for our external effects employers need to make a judgement before the age-related EPL coverage starts and try to identify the individuals who are at the highest risk of turning into "deadwood labor" during the next 4 years. This might not be an obvious task and it might be additionally complicated by the countervailing positive labor supply efforts by the employees. In contrast, in Sweden employers have much better information on who the "deadwood labor" is in their company, as these workers have already been covered by the EPL for at least 6 months (but often multiple years), enough time to reveal their "type". Note also that when we consider non-employed individuals, where the risk aversion and asymmetry of information is arguably higher on the demand side, we find statistically significant negative effects of up to 0.91 pp. Second, our workers are much younger, at 61 for men and 56 for women, compared to the age cutoff at 67 (for both men and women) considered in Sweden. To the extent that mismatch between wages and productivity, and thus potential costs to employers, increase in age this could explain the difference. Furthermore, in Poland the workers we consider are (mostly) not eligible for retirement benefits while all workers considered in Sweden have an outside option to retire. Lastly, it could be that the results diverge due to macroeconomic factors. For the years we consider the economic growth in Poland appears much stronger than in 2019 in Sweden. For example, the economic growth in Poland was 5.1% in 2017 vs. 2.0% in 2019 in Sweden while unemployment rate was 4.9% in 2017 in Poland vs. 6.8% in 2019 in Sweden. If higher economic growth makes the demand for labor less elastic this could be a plausible explanation for the diverging results.

6 Robustness analysis

Our main results presented in Section 5 are based on samples conditional on a number of selection criteria. These include removing recipients of an early ("bridge pension") retirement or a disability pension in the month in which individuals turn 62 (men) or 57 (women). Furthermore, the treatment sample used for our main results consists of ten monthly birth cohorts, which in October 2017 gained the most in terms of additional employment protection out of the 24 and 27 cohorts of men and women respectively who in that month gained EPL protection as a result of the reform. For clarity we refer to these main samples as A1 (samples used for analysis of employment) and A2 (samples used for analysis of earnings). In the robustness analysis we show results based on samples constructed using alternative criteria (see Online Appendix Tables A.1 and A.2 for details). In particular:

- we extend the samples by excluding only those who at a specific age receive the "bridge" pensions (samples B1/B2, E1/E2 and H1/H2);
- we remove all individuals who at a specific age receive any social security pension (samples C1/C2, F1/F2 and I1/I2);
- we focus on the four youngest monthly cohorts among the treated, i.e. those born from July to October (and correspondingly in all other samples for the triple difference analysis, samples D1/D2, E1/E2 and F1/F2);
- we remove recipients of a specific type of pension not at the age of 62/57 (men/women) as in the main samples (A1/A2) but rather at 61.5/56.5 (men/women, samples G1/G2, H1/H2, I1/I2).

First, although disability pensions might be endogenous with respect to the EPL legislation we want to ensure that our results are insensitive to including this outside option in the data. Second, we want to verify that results are similar for the "core workforce" that isn't eligible for any special pension benefits. Here our concern is that workers with such outside options might be more likely to be fired as well as less likely to exert on the job effort. Third, we further limit the sample to focus only on four, rather than ten, monthly cohorts with the highest expected implications of the reform for employers (i.e., those workers who gain the longest potential protection). Finally, we look at the four monthly cohorts using different age cut-offs for the receipt of different types of pensions: 61.5 for men and 56.5 for women. Since in our data we can only identify pension types from January 2016, for the oldest cohorts included in the analyses (men born in 1955 and women born in 1960, i.e. the "placebo reform" treated samples in the triple difference analyses) this lower age criterion allows us to observe their pension receipt only for those born in the second half of the calendar year. Hence for this sample we need to focus on individuals born from July to October. There are two important reasons why conditioning on pension receipt at a younger age might play a role. First of all, those who lose their jobs might make an extra effort to secure incomes form pensions, thus making pension receipt endogenous with respect to treatment. Lowering the age considered thus limits this source of potential bias. Second, as we noted in the data section, when we condition on pension receipt at the age of 62/57, in the case of the youngest cohorts (men born in 1959 and women born in 1964, i.e. the "true reform" control samples) we draw on the pension information from as late as October 2020. Lowering the conditioning age by six months means that the latest month of pension data we use for these samples is April 2020, which makes is less likely to be affected by the COVID-19 pandemic to the same extent.

Results of our robustness analyses are presented in Table 2, where we only show regressions based on the triple differences design. To ease the comparisons, the first panel shows results using the main sample criteria: including ten monthly birth cohorts and removing "bridge" and disability pension recipients at the age of 62/57. We then observe that either focusing on a narrower set of pensions (i.e., including those on disability pensions) or excluding all pension eligible individuals does not change out results. On the other hand, focusing the analysis on the youngest treated monthly cohorts, those born from July to October, makes a difference for employment of women but not men. These are the workers who gain the longest protection period and are potentially most costly from the firms' perspective should they become "deadwood labor". Irrespective if we retain the same age cutoff for pensions (age 62/57) or lower the threshold to age 61.5/56.5 we observe about twice as large and statistically significant negative employment effects for women. Even thought these effect sizes are still relatively small at -0.48 to -0.55 pp, they suggest that the extent of additional protection for women could matter more than for men. This result may be a reflection of labor market discrimination that older women may face (Neumark et al. 2019), for example, due to potential concerns among employers with regard to family care duties (of spouse or grandchildren) or more rapidly declining health. When it comes to earnings the results are qualitatively similar irrespective of the exact sample permutation we use and they are never statistically significant at conventional levels. Quantitatively, using 95% confidence intervals, we can rule out the estimates ranging from -2.0% to -1.0% for men and from -1.0% to -0.3% for women; all of which are much smaller compared to the negative estimates reported by (Saez et al. 2023).

7 Heterogeneity

In Section 5 we showed that while the overall effects on employment and earnings are statistically insignificant and of economically very low magnitude, the split by the pre-reform labor market status showed that these results are a net effect of positive and negative implications of approaching additional employment protection eligibility on, respectively, the employed and the non-employed individuals. Here we further explore the heterogeneity for those employed prior to the reform passage among different types of firms as well as by the level of individual earnings. Recall that our hypothesis is that the positive findings are driven by positive supply side effects (e.g., via increased effort) dominating the negative demand side effects (e.g., via expected productivity-wage mismatch). Given this logic, we propose that positive effects should be most pronounced for lower earnings workers, workers in high turnover firms, workers in firms with relatively young workforce, and workers in smaller firms. This is because, on one hand, these workers might have stronger incentives to exert the extra effort – e.g., because of having lower incomes or working in firms that monitor productivity more closely – to sway their employers to retain them past the employment protection cutoff. On the other hand, for some firms these employees may constitute a small proportion of the wage bill, which would mean that they could bear the potential costs of retaining such workers past the cutoff should the older worker underperform.

Our individual level administrative data contains firm-level identifiers which allows us to define such firm-level characteristics as firm size (by employment), firm employment turnover and firm-level age composition of the workforce. In each case we conduct the analysis separately for each tertial of individuals defined on the basis of these firm characteristics specified for firms with at least 10 employees. We measure the characteristics prior to the reform. Additionally, we divide all employees into tertials by their individual pre-reform level of earnings.

Results of our heterogeneity analysis, shown in Table 3, seem to confirm our hypothesis of the mechanisms which underline the (net) positive employment protection effects among the sample of employees. In particular, we find that among men the largest positive effects are found for individuals employed in firms with the highest turnover (positive employment effect of 1.28pp in the top teritial) and for those in the lowest teritial of earnings (1.12pp). For women, as in the main analysis, the effects are more muted but it is still the case that women in the lowest earnings tertial are most positively affected (0.73pp). We also find somewhat larger estimates for smaller firms and firms with younger work-force although these results are not always monotonic. We do not find much heterogeneity in the effects for earnings, but recall that we did not detect any meaningful changes in earnings on average either (Table 1).

The fact that the effects are larger in high turnover firms and for individuals with lower earnings is consistent with the hypothesis that employed workers who approach the threshold age of additional EPL eligibility might exert extra effort in order to signal their value to the firm and keep their job to ensure that they maintain employment up to the protected age range. In this type of firms and for these workers the extra effort would matter the most in terms of keeping their jobs for the next four years. This supply reaction on behalf of the employees, of course, does not exclude the fact that at the same time employers are also laying off others, having in mind the approaching limitations concerning their continued employment. Nevertheless, the net effects for the incumbent employees, while relatively low, are positive and question the basic intuition of negative external implications of the additional employment protection.

8 Conclusions

There is continued controversy regarding the implications of regulations which offer different forms of protection for employees vis-à-vis their employers. On the one hand, since labor market protection usually operates through increasing costs of lay-offs, it limits employers' scope for adjustment of their employment strategies and production technology. Moreover higher levels of protection might encourage employees to reduce effort and hence lower their productivity. On the other hand, added job security could actually increase worker motivation and output through higher expected pay-offs from their investments in human capital via on- or off-the-job training. Greater job security could also have important social implications if different types of workers have varying degrees of risk aversion and/or mobility. These arguments seem particularly important with regard to older workers who, on the one hand, may be at greater risk of job loss due to deteriorating health and lower productivity, and on the other hand, face difficulties in finding new jobs as a result of being less mobile and because of age discrimination. Policy-wise the debate on age specific employment protection is likely to gain in relevance in the coming years given rapid aging of the population in most developed countries and increasing efforts of many governments to extend working lives.

In this paper we use administrative data from Poland to document the external effects of age-specific employment protection legislation for individuals who approach the eligibility threshold. The regulations offer strict protection with regard to employment and earnings and cover employees with less than four years prior to reaching retirement age. Our identification strategy relies on a reform which took effect in October 2017 and reduced retirement age. As an unexpected and likely unintended side effect, it extended employment protection to younger cohorts of individuals. Since it was legislated almost a year earlier, both employers and employees had plenty of time to adjust their strategies to the approaching change in regulations.

Overall we find no economically meaningful effects of additional protection either with respect to employment or earnings among those who are expected to gain additional months of labor market protection. These total effects, however, hide diverging patterns among the subsamples of incumbent employees and those in the relevant age group who are non-employed. For the first group we find small, statistically significant positive effects on employment, and among the latter small, statistically significant negative effects. The positive effects are stronger for men, for whom we find an average effect of about 0.67 percentage point, which is more than three times higher than the average effect for women (0.19pp). We propose that one reason behind the positive findings could be that incumbent workers increase their effort to signal their value to firms, and this labor supply effect dominates any negative demand effects steaming from potentially increased cost of employment. Additional heterogeneity analysis we conducted seems to support the hypothesis that the positive effects among incumbent employees might derive from increased efforts among those approaching eligibility compared to the control cohorts. Namely, we find that the effects are particularly strong among those in more precarious jobs - employees in the lowest tertial of earnings and employed in firms with highest employee turnover. The negative implications with regard to employment among the non-employed suggest that women (-0.91pp) suffer more as a result of the regulations in comparison to men (-0.57pp). While these effects are still relatively small, they might not be negligible from the point of view of the policy makers.

Our results seem to suggest that the additional employment protection extended to older workers in Poland, on average, does not have negative employment consequences for those approaching eligibility. Combined with (mechanical) positive employment effects for the covered workers implied by the additional protection, our results suggest that age-specific employment protection could help in keeping elderly workers attached to the labor market. This finding has three important economic and societal implications. First, employed older workers keep contributing to their pensions rather than relying on welfare or early retirement benefits. This reduces strain on the social security system and in the medium run increases the level of pension payouts that workers could expect post-retirement. Second, there is evidence (see e.g., (Atalay et al. 2019) or (Filomena and Picchio 2023)) that employment could be beneficial for workers nearing retirement when it comes to their cognition and health, which in turn reduces potential burden for the healthcare and social assistance systems. Taken together these two factors could contribute to healthier and wealthier elderly population which relies less on the state. Finally, expanded exposure to productive, older workers could help overcome the previously documented ageism when it comes to the demand for labor.

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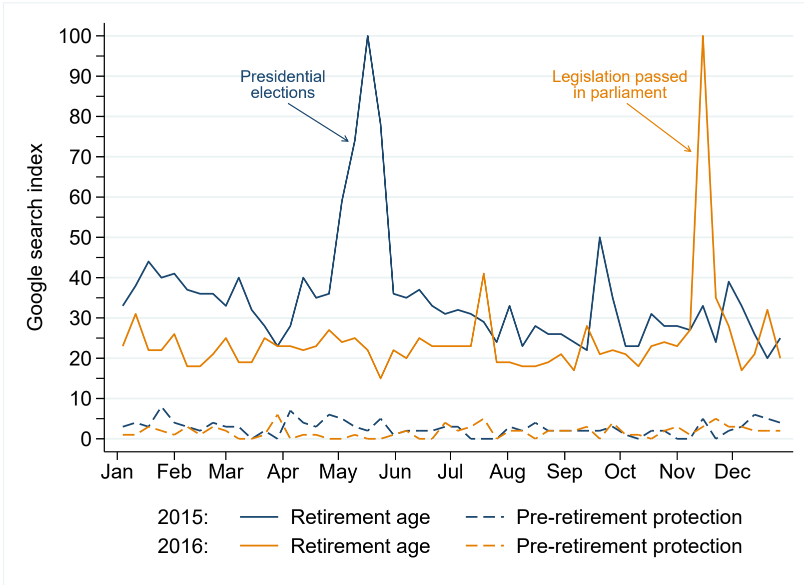
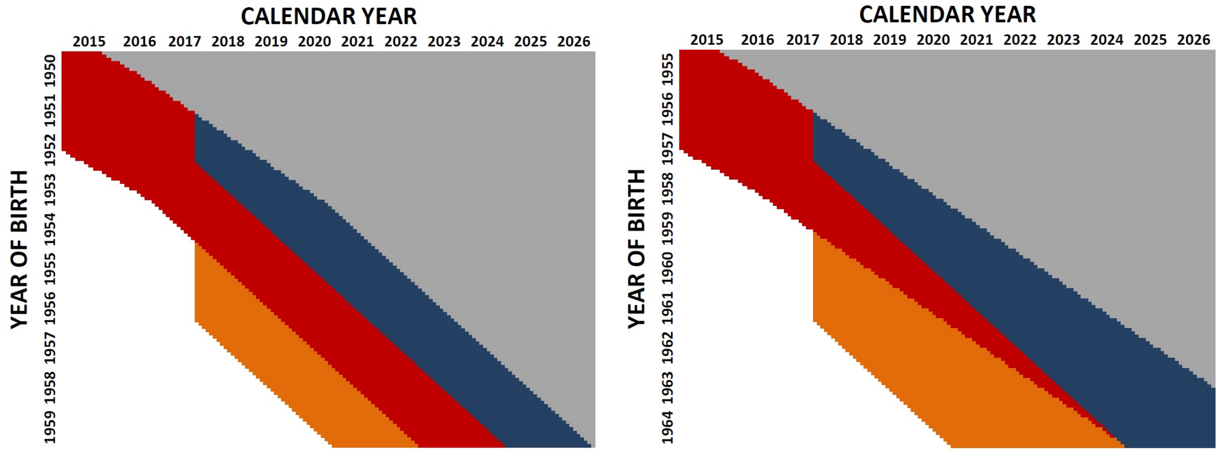
Figure 1: Social reactions to retirement age reform initiatives: “retirement age” and "pre-retirement protection" in online searches in 2015 and 2016



Note: Google search statistics for “wiek emerytalny" (EN: “retirement age”) and “ochrona przedemerytalna” (EN: “pre-retirement protection”) based on Google Trends.
The two rounds of the presidential election took place in Poland on May 10th and 24th 2015 with the pledge of the opposition candidate, Andrzej Duda, to return to lower retirement age of 60 and 65 for women and men, respectively. Parliamentary elections followed on October 25th 2015 and were won by the Law and Justice (PiS) party who supported Andrzej Duda in the presidential vote. The first reading of the presidential legislative initiative took place December 2015, the second reading nearly a year later on November 15th 2016, with the legislation passed on the following day. See main text for more details.
Source: Google Trends.

Figure 2: Retirement age and employment protection by cohort and time

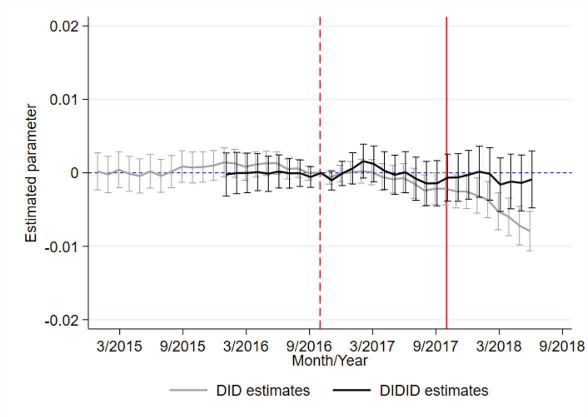


(a) Men

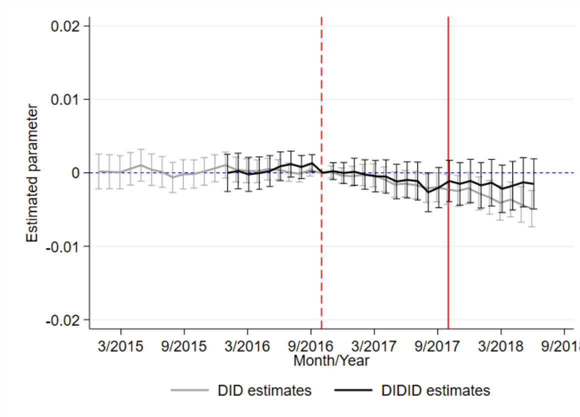
(b) Women

Note: The grey area reflects cohort-specific retirement age eligibility for public pensions in accordance with the 2012 reform which gradually increased retirement age from 60/65 (women/men) to 67. The corresponding red area reflects the months of eligibility to age-related employment protection. The navy-blue area (overlapping partly with the red area) shows the additional earlier eligibility for public pensions following the 2016 reform which reduced retirement age back to 60/65. The orange area is the additional age-related EPL eligibility gained as a result of the retirement age reform which came into effect in October 2017.
Source: Authors' illustration on the basis of the corresponding legislation.

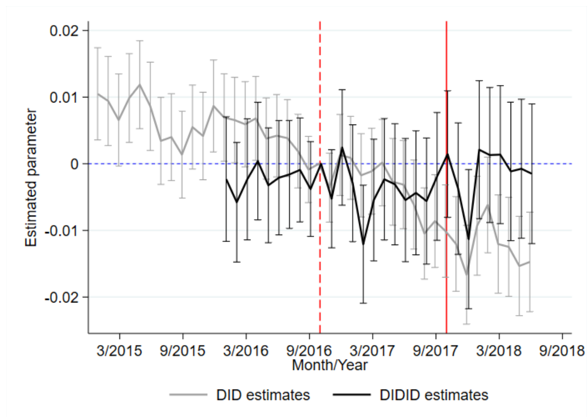
Figure 3: Effects of pre-retirement employment protection on employment and earnings of cohorts approaching eligibility: Event studies



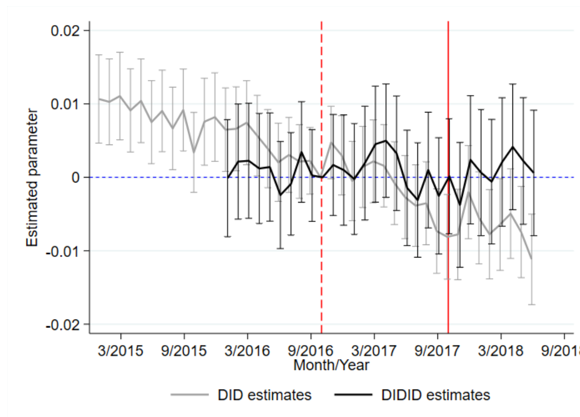
(a) Employment: Men



(b) Employment: Women

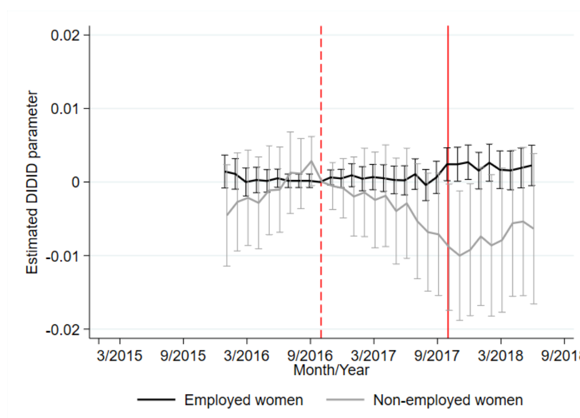


(c) Labor earnings (logs): Men



(d) Labor earnings (logs): Women

(e) Employment, men: employees & non-employed



(f) Employment, women: employees & non-employed

Notes: DID - difference-in-differences estimates; DIDID - triple difference estimates. Figures shifted marginally to avoid overlap for better visual clarity. The dashed vertical lines mark October 2016 (last month before legislation of reduced retirement age), while the solid vertical lines mark October 2017 (when legislation becomes binding). Samples of employees and non-employed defined on the basis of labor market status in the months July-September prior to treatment (and in corresponding months for respective samples in the triple difference estimation). See text for details. Standard errors clustered at an individual level.
Source: Authors' calculations using Ministry of Finance administrative database.

Table 1: Effects of pre-retirement employment protection on employment and earnings of cohorts approaching eligibility: Average treatment effects

	Men	Women
Employment:		
Full sample mean#:	0.6656	0.7641
Full sample estimates, DID:	-0.0033***	-0.0027***
	(0.0011)	(0.0010)
Full sample estimates, DIDID:	-0.0004	-0.0022
	(0.0015)	(0.0014)
Employees sample estimates, DIDID	0.0067***	0.0019*
	(0.0014)	(0.0011)
Non-employed sample estimates, DIDID	-0.0057*	-0.0091**
	(0.0034)	(0.0043)
Earnings (logs):		
Earnings sample mean#:	8.071	7.939
Earnings sample estimates, DID:	-0.0099***	-0.0081***
	(0.0029)	(0.0024)
Earnings sample estimates, DIDID	-0.0009	0.0030
	(0.0041)	(0.0033)

Notes: DID - difference-in-differences estimates, DIDID - triple difference estimates (in accordance with Equation 3). Samples of employees and non-employed defined on the basis of labor market status in the months July-September prior to treatment (and in corresponding months for respective samples in the triple difference estimation). Based on samples A1 (employment) and A2 (earnings), see main text for definition of samples and Tables A.1 and A.2 for details. # - mean values calculated for the treated cohorts for months considered as pre-treatment (May-October 2016). Standard errors clustered at an individual level. ***, **, and * mark statistical significance at 1, 5, and 10% level.
Source: Authors' calculations using Ministry of Finance administrative database.

Table 2: Effects of pre-retirement employment protection on employment and earnings of cohorts approaching eligibility: Robustness analysis

DIDID estimates	Employment:		Earnings:	
	Men	Women	Men	Women
Main sample criteria (months 1-10)				
Sample 62/57, samples A1/A2	-0.0004	-0.0022	-0.0009	0.0030
	(0.0015)	(0.0014)	(0.0041)	(0.0033)
Sample 62/57, samples B1/B2	0.0001	-0.0014	0.0017	0.0045
	(0.0018)	(0.0015)	(0.0042)	(0.0034)
Sample 62/57, samples C1/C2	-0.0009	-0.0020	-0.0037	0.0030
	(0.0015)	(0.0014)	(0.0040)	(0.0033)
Robustness estimates: (months 7-10)				
Sample 62/57, samples D1/D2	0.0003	-0.0055**	-0.0064	0.0052
	(0.0025)	(0.0023)	(0.0066)	(0.0052)
Sample 62/57, samples E1/E2	0.0010	-0.0051**	-0.0042	0.0079
	(0.0028)	(0.0025)	(0.0068)	(0.0054)
Sample 62/57, samples F1/F2	-0.0013	-0.0048**	-0.0064	0.0001
	(0.0024)	(0.0023)	(0.0066)	(0.0052)
Robustness estimates: (months 7-10)				
Sample 61.5/56.5, samples G1/G2	0.0006	-0.0051**	-0.0061	0.0045
	(0.0025)	(0.0023)	(0.0066)	(0.0052)
Sample 61.5/56.5, samples H1/H2	0.0012	-0.0050**	-0.0040	0.0077
	(0.0028)	(0.0025)	(0.0068)	(0.0054)
Sample 61.5/56.5, samples I1/I2	-0.0015	-0.0048**	-0.0067	0.0001
	(0.0024)	(0.0023)	(0.0066)	(0.0052)

Notes: All values show triple difference estimates (DIDID) of the average treatment effect as specified in Equation 3. First row shows the main specification results from Table 1 for comparison. See main text for the definitions of the analysis samples and Tables A.1 and A.2 for details of sample sizes. Standard errors clustered at an individual level. ***, **, and * mark statistical significance at 1, 5, and 10% level.
Source: Authors' calculations using Ministry of Finance administrative database.

Table 3: Effects of pre-retirement employment protection on employment and earnings of cohorts approaching eligibility: Heterogeneity analysis

DIDID estimates	Men:				Women:			
	Full (sub-) sample	Tertial 1	Tertial 2	Tertial 3	Full (sub-) sample	Tertial 1	Tertial 2	Tertial 3
Employment:								
By firm size:	0.0062***	0.0062**	0.0085***	0.0038*	0.0010	0.0029	-0.0014	0.0016
	(0.0013)	(0.0025)	(0.0022)	(0.0020)	(0.0010)	(0.0019)	(0.0016)	(0.0016)
By firm turnover:	0.0062***	0.0011	0.0033*	0.0128***	0.0010	0.0006	0.0008	0.0016
	(0.0013)	(0.0014)	(0.0020)	(0.0030)	(0.0010)	(0.0011)	(0.0015)	(0.0023)
By firm age composition:	0.0062***	0.0082***	0.0034*	0.0067***	0.0010	0.0019	0.0014	-0.0003
	(0.0013)	(0.0025)	(0.0020)	(0.0022)	(0.0010)	(0.0020)	(0.0015)	(0.0017)
By earnings:	0.0067***	0.0112***	0.0050***	0.0013	0.0019*	0.0073***	0.0001	-0.0023*
	(0.0014)	(0.0033)	(0.0018)	(0.0018)	(0.0011)	(0.0026)	(0.0014)	(0.0012)
Earnings:								
By firm size:	-0.0057	-0.0091	-0.0046	-0.0032	0.0019	0.0012	-0.0007	0.0055
	(0.0040)	(0.0058)	(0.0069)	(0.0079)	(0.0033)	(0.0049)	(0.0054)	(0.0065)
By firm turnover:	-0.0057	-0.0101	0.0019	-0.0025	0.0019	-0.0001	-0.0004	0.0063
	(0.0040)	(0.0066)	(0.0065)	(0.0074)	(0.0033)	(0.0053)	(0.0054)	(0.0061)
By firm age composition:	-0.0057	-0.0105	0.0035	-0.0107*	0.0019	-0.0034	0.0070	-0.0020
	(0.0040)	(0.0081)	(0.0066)	(0.0061)	(0.0033)	(0.0067)	(0.0053)	(0.0048)
By earnings:	-0.0053	0.0053	-0.0024	0.0008	-0.0008	-0.0089	0.0018	0.0075
	(0.0037)	(0.0067)	(0.0052)	(0.0062)	(0.0031)	(0.0054)	(0.0047)	(0.0054)

Notes: All values show triple difference estimates (DIDID) of the average treatment effect as specified in Equation 3. Results for the "Full (sub-)sample" are identical by firm characteristics as they are run on the same samples. See main text for the definitions of the analysis samples and Tables A.1 and A.2 for details. Standard errors clustered at an individual level. ***, **, and * mark statistical significance at 1, 5, and 10% level.
Source: Authors' calculations using Ministry of Finance administrative database.

Appendix:

Appendix Tables

Table A.1: Sample selection and analysis samples, men
Main, heterogeneity and robustness samples, numbers of individuals

	Reform sample:		Placebo sample:		Total sample:
	Treatment	Control	Treatment	Control	
Birth months:	01-10.1956	01-10.1958	01-10.1955	01-10.1957	
Full sample (individuals):	155,087	163,536	149,688	160,755	629,066
Sample selection for main specifications:					
- sample after step 1: sectors of employment	79,781	84,988	76,541	82,988	324,298
- sample after step 2: disability/early retirement at age 62	54,396	63,476	56,164	59,031	233,067
(employment estimation sample, A1)					
Heterogeneity analysis:					
Estimation samples, employment:					
- terciles by firm size, turnover and age composition	29,394	35,164	28,168	31,924	124,650
- terciles by individual earnings (employees)	34,683	41,573	33,363	37,906	147,525
- non-employees	18,785	20,851	21,845	20,217	81,698
Estimation samples, earnings:					
earnings estimation sample (sample A2)	38,052	45,492	37,053	41,280	161,877
- terciles by firm size, turnover & age composition	29,394	35,163	28,168	31,924	124,649
- terciles by individual earnings	34,683	41,573	33,363	37,906	147,525
Alternative samples for robustness analysis					
adjusted sample selection criteria in step 1:					
Birth months:	01-10.1956	01-10.1958	01-10.1955	01-10.1957	
Estimation sample, employment:					
B1, step 2: only early retirement pension at age 62	61,625	72,001	59,576	66,972	260,174
C1, step 2: any pension at age 62	49,688	60,838	43,738	55,658	209,922
Estimation sample, earnings:					
B2, step 2: only early retirement pension at age 62	44,596	53,616	41,531	49,152	188,895
C2, step 2: any pension at age 62	36,285	45,290	31,243	41,268	154,086
Birth months:	07-10.1956	07-10.1958	07-10.1955	07-10.1957	
Estimation sample, employment:					
D1, step 2: disability/early retirement at age 62	20,998	23,624	21,616	22,474	88,712
E1, step 2: only early retirement at age 62	23,789	26,845	23,034	25,588	99,256
F1, step 2: any pension at age 62	19,366	22,613	17,139	21,282	80,400
G1, step 2: disability/early retirement at age 61.5	21,399	23,954	22,179	22,830	90,362
H1, step 2: only early retirement at age 61.5	24,033	26,992	23,352	25,753	100,130
I1, step 2: any pension at age 61.5	19,373	22,932	17,139	21,616	81,060
Estimation sample, earnings:					
D2, step 2: disability/early retirement at age 62	14,716	16,986	14,273	15,805	61,780
E2, step 2: only early retirement at age 62	17,238	20,053	16,051	18,878	72,220
F2, step 2: any pension at age 62	14,150	16,874	12,245	15,839	59,109
G2, step 2: disability/early retirement at age 61.5	15,045	17,275	14,707	16,095	63,122
H2, step 2: only early retirement at age 61.5	17,441	20,183	16,300	19,015	72,939
I2, step 2: any pension at age 61.5	14,154	17,157	12,245	16,119	59,675

Notes: See notes in text presented below Table A.2.
Source: Authors' calculations using Ministry of Finance administrative database.

Table A.2: Sample selection and analysis samples, women
Main, heterogeneity and robustness samples, numbers of individuals

	Reform sample:		Placebo sample:		Total sample:
	Treatment	Control	Treatment	Control	
Birth months:	01-10.1961	01-10.1963	01-10.1960	01-10.1962	
Full sample (individuals):	150,368	148,902	157,283	147,534	604,087
Sample selection for main specifications:					
- sample after step 1: sectors of employment	68,114	67,991	70,738	67,545	274,388
- sample after step 2: disability/early retirement at age 57	60,374	60,329	65,565	60,187	246,455
(employment estimation sample, A1)					
Heterogeneity analysis:					
Estimation samples, employment:					
- terciles by firm size, turnover and age composition	38,655	38,651	40,641	38,436	156,383
- terciles by individual earnings (employees)	44,576	44,617	46,958	44,497	180,648
- non-employees	14,802	14,772	17,522	14,726	61,822
Estimation samples, earnings:					
earnings estimation sample (sample A2)	48,004	48,102	51,053	48,134	195,293
- terciles by firm size, turnover & age composition	38,655	38,651	40,430	38,298	156,034
- terciles by individual earnings	44,576	44,617	46,615	44,248	180,056
Alternative samples for robustness analysis:					
adjusted sample selection criteria in step 1					
Birth months:	01-10.1961	01-10.1963	01-10.1960	01-10.1962	
Estimation sample, employment:					
B1, step 2: only early retirement pension at age 57	62,872	63,239	67,062	62,977	256,150
C1, step 2: any pension at age 57	56,814	58,107	56,995	57,526	229,442
Estimation sample, earnings:					
B2, step 2: only early retirement pension at age 57	50,913	51,662	53,512	51,357	207,444
C2, step 2: any pension at age 57	46,646	47,704	46,723	47,469	188,542
Birth months:	07-10.1961	07-10.1963	07-10.1960	07-10.1962	
Estimation sample, employment:					
D1, step 2: disability/early retirement at age 57	22,881	22,476	24,220	22,573	92,150
E1, step 2: only early retirement pension at age 57	23,845	23,585	24,834	23,627	95,891
F1, step 2: any pension at age 57	21,588	21,660	21,264	21,643	86,155
G1, step 2: disability/early retirement at age 56.5	23,036	22,630	24,446	22,736	92,848
H1, step 2: only early retirement pension at age 56.5	23,931	23,655	24,961	23,710	96,257
I1, step 2: any pension at age 56.5	21,593	21,810	21,264	21,800	86,467
Estimation sample, earnings:					
D2, step 2: disability/early retirement at age 57	18,183	17,968	18,934	18,107	73,192
E2, step 2: only early retirement pension at age 57	19,317	19,332	19,871	19,338	77,858
F2, step 2: any pension at age 57	17,718	17,819	17,455	17,920	70,912
G2, step 2: disability/early retirement at age 56.5	18,316	18,100	19,077	18,244	73,737
H2, step 2: only early retirement pension at age 56.5	19,391	19,393	19,976	19,409	78,169
I2, step 2: any pension at age 56.5	17,721	17,952	17,455	18,055	71,183

Notes: See notes in text presented below.
Source: Authors' calculations using Ministry of Finance administrative database.

Notes for Tables A.1 and A.2:

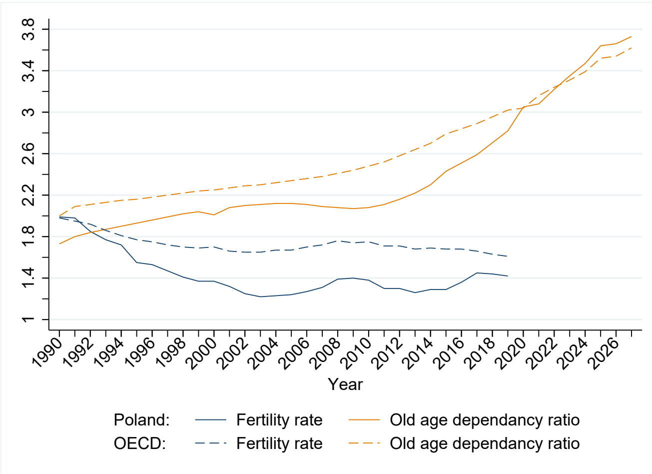
Sample sizes presented in the Tables reflect the number of individuals. 'Reform sample' – sample subjected to the retirement age reform in 2016-2017 in the analysis time frame; 'Placebo sample' – sample not subjected to the retirement age reform in 2016-2017 in the analysis time frame, used for 'placebo reform' in years 2015-2016 for triple difference specification; 'Full sample' – individuals born in specific months, registered as being alive at the end of 2018.

- Sample selection, step 1: excluding individuals who at any point between 01-2015 and 06-2018 are recorded as being employed in selected sectors (see # below) and/or worked in occupations that have a separate pension scheme (uniformed services, judges and public prosecutors).
- Sample selection, step 2: excluding individuals who at a specific age (62/57 in main sample for men/women, 61.5/56.5 in robustness analysis for men/women) are registered as claiming a specified type of social security pension.
- Samples for heterogeneity analysis by firm characteristics are additionally conditional on being employed in firms with more than 10 employees in the first half of 2016 ("reform sample") and first half of 2015 ("placebo sample"); firm-level turnover defined on the basis of turnover between first half of 2015 and 2016; age composition of employment defined by the proportion of people age 50+;
- Samples for analysis of earnings: individuals need to have at least one observation of positive earnings from an employment contract in the estimation time frame;
- Samples for earnings tertials: based on average earnings (from an employment contract) for the months January-June 2016 ("reform sample") or January-June 2015 ("placebo sample");

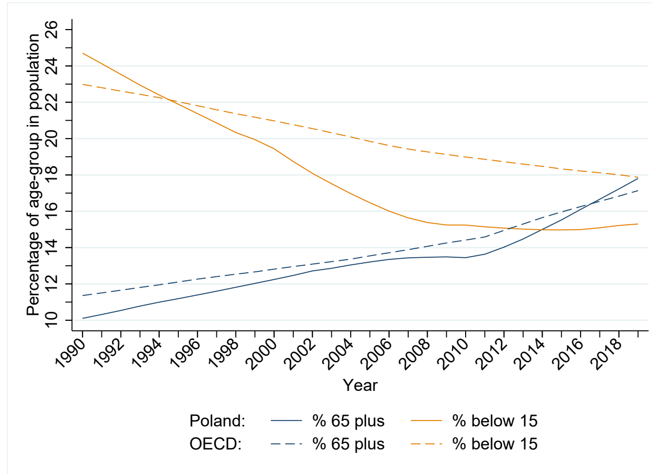
- Employees recorded as working in the following sectors are excluded from the analysis: agriculture, forestry, hunting and fishing (A); mining and quarrying (B); water supply, sewage and waste management and reclamation activities (E); professional, scientific and technical activities (M); public administration and national defence (O); education (P); households with employees, households producing goods and providing services for their own needs (T); extraterritorial organizations and teams (U).

Appendix Figures

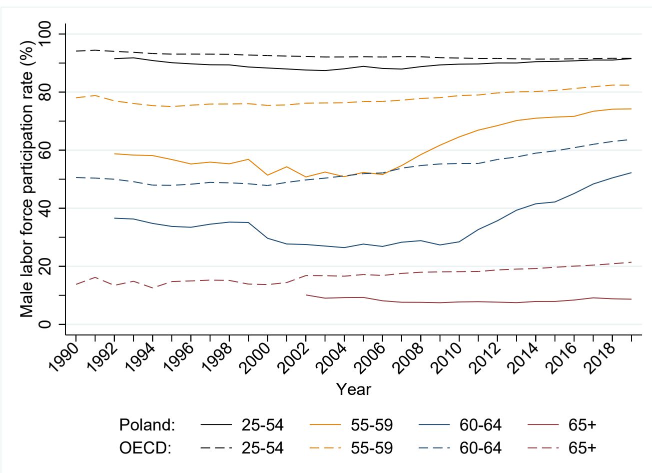
Figure A.1: Demographic trends and labor force participation: Poland and OECD



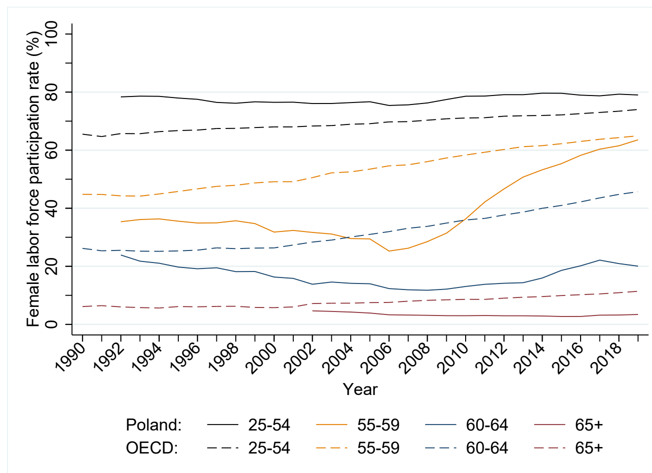
(a) Fertility rates and old age dependency ratios



(b) Percentage of individuals aged below 15 and over 65 years old in total population



(c) Male labor force participation rates



(d) Female labor force participation rates

Note: OECD values represent the average for all OECD countries.
Source: OECD database.