

# **Pre-retirement employment protection: no harm when times are good\***

**Preliminary and incomplete**

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Employees in Poland with less than four years before they reach statutory retirement age are covered by strict labor protection. The legislation restricts employers' ability to lay them off, to lower their wages and to adjust their responsibilities. Such regulations may have unintended negative implications for employment among those who are nearing the age when the legislation becomes binding for their employers. We examine this issue using population-level administrative data and identify the role of employment protection with reference to a reform which lowered statutory retirement age to 60 (women) and 65 (men). An unintentional consequence of this reform was extension of employment protection to younger cohorts of employees. Preliminary results, show no economically or statistically significant negative effects of the legislation on employment of either men or women nearing the age of eligibility for protection. This suggests either that these effects are absent or any potential concerns of employers related to prospective employment protection were counteracted by strong labour demand at the time.

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# 1 Introduction

The workforce in most industrialized countries is aging and low levels of fertility imply that this trend is likely to continue in the coming decades. Thus effective incentives and regulations aimed at extending working lives and increasing productivity of older workers will be critical for sustained economic growth. Improved working conditions, changes in the nature of work and better health levels of subsequent cohorts contribute towards opportunities for older workers to continue their active and productive involvement on the labor market. However, these factors have not eliminated concerns about the costs of continued employment of older workers from the point of view of the employers. The risk of reduced productivity as workers age combined with the inability to adjust their wages downwards can affect firms' employment strategies, resulting in the replacement of older employees with younger ones, or adjustment of the overall capital-labor ratio. This would be particularly relevant if fixed costs of training are high and if adjustment to new technologies is more challenging for older workers (e.g., [Abowd and Kramarz \(2003\)](#); [Behaghel et al. \(2014\)](#); [Daniel and Heywood \(2007\)](#); [Lazear \(1990\)](#); [Perek-Białas and Turek \(2012\)](#)).

Concerns about the costs of employing older workers continue to be voiced in the public debate despite the fact that the literature is inconclusive with regard to the age-productivity profiles (e.g., [de Hek and van Vuuren \(2011\)](#); [Mahlberg et al. \(2013\)](#); [Romeu Gordo and Skirbekk \(2013\)](#); [Börsch-Supan and Weiss \(2016\)](#)) and that the degree of wage flexibility for older workers may be higher than it is commonly believed. While one would expect these arguments to be taken into account in firms' employment decisions, many others which are important from the social point of view might not be. These include differences in risk attitudes between employees and employers as well as broader social implications of employment decisions from the point of view of the ageing workforce such as lower mobility of older workers or lower degree of re-employability. In such a case firms' preference for younger workers might be socially sub-optimal and justify regulations aimed at protecting employment ([Bertola 2004](#)), and in particular employment of older workers - an approach adopted in several developed countries.

How effective different forms of such regulations are is of course an empirical question. The key concerns are, on the one hand, about the degree of protection extended to those covered, and on the other, about the potential negative consequences for other groups and in particular for those who are likely to become eligible. The latter is naturally of particular interest with regard to age-specific policies. There is extensive literature on effects of general employment protection on labor demand and labor market equilibrium, though the results are sensitive to labor market institutions and econometric approaches. Evidence from cross-country studies generally suggests that more stringent employment protection legislation (EPL) reduces demand for labor ([Lazear 1990](#); [Kahn 2007](#)) and that this association is sensitive to business-cycle ([Messina and](#)

Vallanti 2007; Duval et al. 2020). On the other hand, Bassanini and Garnero (2020) find no separation effects but rather reduced rates of within-industry transitions. Studies focused on specific regulations find both positive and negative consequences of EPL (Autor 2003; Autor et al. 2007; Blanchard and Landier 2002; Kugler and Pica 2008). For example, EPL has been found to enhance employment, in particular in high skill occupations (MacLeod and Nakavachara 2007). On the other hand, estimation of consequences of the Dutch protection scheme for older workers (Deelen et al. (2009)), suggests that reducing employment protection could have significant positive effects on employment for the age group 55-64. Similarly, the evaluation of the French ‘Delalande tax’, which imposed an additional financial cost on employers for terminating of contracts of those aged 50+ (in force 1987-2008), suggests that it had significant negative implications for hiring of older workers (Behaghel et al. 2008). However, besides these two papers we know very little about the consequences of age specific EPL legislation (see e.g., OECD (2014b,a, 2015)).

The focus of this paper is on quantifying the consequences of EPL targeted at older workers in Poland where such legislation has been part of the labor code since the 1970s. Originally the regulations protected employees from being fired in the period of two years prior to reaching retirement age, and were a symbolic legacy of the socialist centrally planned regime. However, protection was expanded to 4 years by a reform implemented in 2008 partly in return for significant limitations of options for early retirement. This new 4-year threshold has been present ever since. Employment protection with age specific regulations is not very common, which makes the Polish case rather unique and interesting from the policy perspective. To estimate the consequences of age-specific EPL we use detailed individual level administrative data on employment from a joint tax and social security database paired with a reform to statutory retirement age which came into force in late 2016. Since January 2013 retirement age in Poland was gradually increased from a sex-differentiate age of 60 (women) and 65 (men) with the aim to eventually reach 67 years for all. This became a focal point of the presidential and parliamentary election campaigns in 2015, and retirement age was eventually reduced back to 60/65. An unintended consequence of this reduction has been its implications for the age when workers become eligible to the EPL in the pre-retirement period.

Unlike previous studies focused on older workers we do not find any negative effects on employment for either male or female soon-to-be-covered workers. The point estimates are small and never statistically significant, and based on 95% confidence intervals we can rule out effect sizes exceeding 1 percentage point. One reason for the null finding could be that the reform was implemented during an economic boom where demand for labor was very strong. In such a context firms might have been willing to incur the risk of the additional costs of inflexibility with

regard to the status of their older employees.<sup>1</sup>

## 2 Institutional background

### 2.1 Employment protection legislation

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 regulations and 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. The only major substantive change in how it operates has been the extension of the protection period from 2 to 4 years in 2008 as part of an agreement between the government and social partners on a package limiting access to early retirement. The regulations encompass not only inability to terminate a contract but also lowering of wages, changes in hours, or moving to a more burdensome role or position. In that, they are fairly inelastic from the employer's perspective and leave little leeway in employment practices. Major exemptions 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 and are monitored closely by the authorities. The EPL is limited to those employed on open-ended and fixed-term labor contracts and excludes those who are eligible for disability pensions. Importantly, from the perspective of the changes to retirement age, employers are allowed to hand in a leave notice to their employees right before the start of the period in which protection begins. 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 Labor Code and Criminal Code.<sup>2</sup>

Overall the Polish pre-retirement EPL is inflexible and thus could be costly from an employer's perspective. The aggregate statistics suggest that the law is broadly obeyed or at least the courts tend to side with plaintiffs when it comes to its violations. Employers therefore in a way face a double risk. On the one hand, they are unable to lay off or reduce the pay for employees whose performance might drop over time as they age. On the other hand, they face the risk that employees who are covered by the EPL and realise their improved bargaining position, could take advantage of it making reduced productivity a self-fulfilling prophecy.

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<sup>1</sup>In subsequent analyses we hope to explore this angle further by investigating sectoral, regional and individual level heterogeneity.

<sup>2</sup>Criminal Code, in addition to the labor code, regulates financial penalties for illicit contract terminations irrespective of employees age and employment type.

## 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. These rules were set right after the World War II and were in place throughout the communist era. Already then there were numerous exceptions to the 60/65 threshold for specific occupations, such as miners, police force, teachers, and many others. Early retirement then became a common way to escape unemployment in the transition phase after 1989 and low employment of older workers became a significant challenge for Polish governments in the late 1990s and early 2000s. While various early retirement rights were slowly phased out, it was only in 2008 that a set of laws was passed substantially limiting their scope. In return, the government agreed to extend employment protection legislation for older workers from two to four years prior to reaching retirement age.

The first change which affected the overall statutory retirement age that sets the stage for this paper took place when the Donald Tusk government introduced retirement age extension in June 2012. This legislation paved the way to gradual increases in the statutory retirement age aimed at reaching 67 for both men and women. Retirement age was to grow gradually beginning with January 1st 2013, with increases in retirement age of one month in each subsequent quarter. In this way men would reach their target age of 67 in 2020 while women in 2040, due to the initial five year gap between the genders.

The retirement age reform, which had been unannounced in the electoral campaign of 2011, proved unpopular and became one of the key reference points in the presidential and parliamentary elections in May and October 2015, respectively. Importantly, although the pensions and labor legislation is out of the hands of the Polish President, the then opposition candidate, Andrzej Duda, made an electoral pledge of return to the pre-2013 statutory retirement ages. The incumbent President, who signed the retirement age reform into law, and the government coalition who had introduced it, lost both elections to the right-wing opponents. Andrzej Duda became President, and the government was formed by the Law and Justice (PiS) party. While the presidential pledge proved popular among the electorate, its realisation was initially considered very unlikely given the rapid population ageing in Poland and the heavy long-term costs of such a policy to the public purse. Andrzej Duda, however, kept his promise and a few months after becoming President presented the relevant legislation to the Parliament. The government was far more hesitant about its implementation and claimed to prepare its own versions of changes to the pensions system. In the end though, it gave its approval and the final go in the autumn of 2016. Once in Parliament it passed swiftly through both chambers in November 2016 and Duda signed it into law on December 19th, with regulations coming into force on October 1st 2017, after less than a year of a hold-up period.

By the time the policy reversal came into force in October 2017, a number of cohorts of both men and women saw their retirement age grow as a result of the 2013 reform. In fact a month earlier, in September 2017, retirement eligibility (under the 2012 legislation) 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 effect of the reversal was that all cohorts who were 60/65 or older on October 1st 2017, were granted retirement rights on that day, and numerous individuals took advantage of this opportunity. In the last quarter of 2017 and the first quarter of 2018 the ZUS registered 357.3 thousand new retirement pension claimants. A year earlier as well as two years earlier the numbers for corresponding two quarters were equal to 106 thousand. This massive outflow of retirement-eligible workers happened despite increasing employment rates for older workers in Poland (see Figure 1) and lack of mandatory retirement.

### **2.3 Using the retirement age reform to estimate the effects of employment protection**

Since employment protection legislation was not defined with respect to a specific age, but rather with respect to retirement age, subsequent cohorts whose retirement age grew after the 2012 reform, correspondingly became eligible for employment protection at a later age. In the same way, the legislation which lowered statutory retirement age automatically made younger cohorts of men and women eligible for employment protection. Because cohorts who gained employment protection in October 2017 were born later relative to those who could then retire, and thus faced further extensions to their retirement age, the number of monthly cohorts additionally covered by employment protection was higher than the number of cohorts of the newly retired. Respectively, there were 24 and 27 monthly cohorts of men and women who on October 1 2017 gained the right to pre-retirement employment protection.

Details of how the reform affected specific cohorts of men and women are illustrated in Figure 2. The figure maps the two retirement age laws at monthly level in two dimensions - the month of birth and the calendar month, separately for men (Figure 2A) and women (2B). The gray areas represent months in which specific monthly cohorts were expected to gain eligibility to retirement pensions under the 2012 legislation. The navy areas represent combinations of months and monthly cohorts of additional eligibility following the reform reversal which came into force on October 1st 2017. The red and orange areas represent the corresponding periods of eligibility for employment protection under the extended and the reduced retirement thresholds, respectively (the navy areas overlap some of the red areas). Because of the phased-in increases in retirement eligibility, as time went on the age at which individuals could retire grew and, correspondingly, grew the age of those who were four years younger who became eligible to

employment protection. It is precisely this variation in the EPL age eligibility across cohorts combined with the reversal of retirement age increases that allows us to examine the influence of EPL on employment, and in particular to focus on the potentially negative consequences of protection for individuals just before they become eligible.

Thus we know that on October 1st 2017 24 monthly cohorts of men and 27 cohorts of women gained additional coverage due to reform. These are men born between November 1st 1954 and October 31st 1956 and women born between November 1st 1959 and October 31st 1961 (see Figure 2). Therefore, they could be considered as treated by the reform. An illustration of the treated vs non-treated monthly cohorts of men taken from the perspective of 01 October 2017 is presented in the first row of Figure 3 (similarly Figure 3 shows the distinction for women). Cohorts marked in green are those unaffected by the reform as of 01 October 2017: the oldest ones because they would already have been covered by the EPL even without the reform, and those born in November 1956 or later, who in October 2017 were still not covered by employment protection. The cohorts in between - marked in red - are those who gained labor protection coverage on 1 October 2017. The treatment varies by cohort in two important dimensions. First, all cohorts of men younger than that born in November 1956 are also treated by the reform, since eventually they will also be eligible for the earlier EPL. Their EPL coverage starts later than October 2017 but they will also be affected by the changed rules. For example, while in Figure 3 those born in November 1956 are identified as non-treated, the reform implies that their eligibility for the EPL would start in November 2017, only a month after the reform came in. Second, there are cohorts who gain eligibility on 1st October 2017 that would - without the reform - be in any case eligible for protection only a month or a few months later. For example while the cohort born in November 1954 is identified as treated - since they gain eligibility to the EPL in October 2017 - this group of men would be covered by the EPL only a month later under the old rules.

To identify the implications of the EPL we thus have to be careful in selecting the treatment and control groups, taking into consideration the EPL implications for employers, employees, and the overall labor market dynamics, including among other factors the seasonality of flows into retirement. Because of different retirement ages naturally the treatment and control groups are defined separately for men and women. Based on these considerations, we specify our treatment and control groups in the following way. First, we identify two groups of monthly cohorts from among the treated groups who - from the perspective of 01 October 2017 - are the most severely affected by the reform, i.e. those who gain a substantial number of months of protection. One is defined more leniently around the policy cutoff while the other is more narrow. The two approaches are presented as samples (a) and (b) in rows 2 and 3 of Figures 3 and 4 for men and women respectively. Specifically, in the first case (sample (a)) we consider as treated those men who were born between January and October 1956, while in the latter case we consider as treated

those born between May and October 1956 (sample (b)). The difference stems from the fact that January to April workers could be somewhat different than workers closer to the policy cutoff. Second, we match these groups with a sample of control cohorts who are born exactly two years later. Although these individuals will eventually also be covered by the earlier EPL, our assumption is that at the time of the reform their eligibility is far enough into the future that it would not be considered as a factor in their employers' demand decisions. Thus, the corresponding control samples for men are: those born between January and October 1958 (sample (a)) and those born between May and October 1958 (sample (b)). Comparing workers born in the same months further allows us to control for any season-of-birth effects.

In Figure 4 we present similar considerations for women. Since retirement age of women was 5 years lower than that for men, they experienced similar employment protection conditions that were just shifted towards younger cohorts. In that, our lenient comparison contrasts women born between January and October 1961 with similar women born in the same months but in 1963. When we consider the more conservative approach the treatment group is defined as May to October 1961 while the control group is women born between May and October 1963.

Irrespective of the gender component, a complicating factor in difference-in-differences strategy outlined above is the fact that it ignores any potential age-related differences in employment.<sup>3</sup> In other words, even if parallel trends hold in the pre-treatment period, there may be factors related specifically to age, and not the overall economics conditions, which affect the treated cohort in the post-period, and which do not affect the younger control group. The most obvious reason in the specific case of employment of older people is the correlation between age and health. Thus, even if overall economic conditions affect treatment (older) and control (younger) people in the same way, the former group could suffer from differential age-specific effects. Therefore, in addition to parallel trends assumption identifying employment effects of a specific cohort (first difference) and time (second difference), we need additional assumptions for specific age groups at a given time (third difference). This logic motivates our use of an alternative identification strategy – a triple-difference approach – where we lag the cohorts by one year. For men, we thus assume that differential effects of age would be the same between the 1956 and 1955 cohorts relative to the difference between the 1958 and 1957 cohorts. For women, these correspond to identical age effects for 1961 and 1960 cohorts in comparison to 1963 and 1962 cohorts. In the next section we outline how these considerations translate into estimating equations.

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<sup>3</sup>This is similar to canonical problem in labor economics when using panel data related to separation of time, cohort, and age effects.



### 3 Empirical Specification and Identification

We use the retirement age law changed passed in November 2016 in Poland as a source of exogenous variation in employment protection and estimate its effects on labor demand. Specifically, we exploit the fact that depending on date of birth, the reform exogenously assigned workers to new employment protection regime thus inducing some employees to be eligible at a much earlier age than without the change. As discussed above, Figures 2, 3, and 4 delineate cohorts that were affected by the reform forming our difference-in-differences estimator. Furthermore, to account for potential age-specific effects of common shocks we also propose a tripple-difference estimator where we include one cohort lags. For now, we present our results in an event study format but we intend to include aggregate treatment effects in the next iteration of the paper. The estimating equation for our double-difference event study estimator is the following:

$$Y_{ict} = \alpha + \sum_{j=Jan2015}^{Oct2016} \beta_j^1 T_{ic} \mathbb{1}[j = t] + \sum_{j=Dec2016}^{Dec2017} \beta_j^2 T_{ic} \mathbb{1}[j = t] + \gamma_t + \delta_c + \epsilon_{ict} \quad (1)$$

where  $Y_{ict}$  equals to one if an individual  $i$  from birth cohort  $c$  is employed in a given time period (month-year)  $t$ ,  $\sum_{t=Jan2015}^{Oct2016} \beta_j^1$  are pre-trend coefficients while  $\sum_{t=Dec2016}^{Dec2017} \beta_j^2$  represent post-reform effects, and we include time ( $\gamma$ ) and cohort ( $\delta$ ) fixed effects. The latter being dummies taking a value of one for those men born in specific months of 1956 and 1958 (either January to October or May to October). For women, these dummies take a value of one if they were born in these same specific months but for cohorts 1961 and 1963. Variable,  $T_{ic}$ , takes values of one for each specific month-year period  $t$  for individuals who are born between January and October (or May to October) 1956 for men or 1961 for women (cohort  $c$ ). Standard errors,  $\epsilon_{ict}$ , are clustered at individual level since we have repeated observations on individuals. Reference period in this equation is October 2016, the last month prior to the reform affecting our treated group. If employment protection legislation reduces demand for labor of the covered group, we expect coefficients  $\beta_j^1$  to oscillate around zero and coefficients  $\beta_j^2$  to be negative.

Since we are concerned that men born in 1956 and women born in 1961, who are older, could respond differently to the common shocks compared to men born in 1958 and women born in 1963, who are younger, we also propose a tripple-difference estimator of the following form:

$$Y_{iact} = \alpha + \sum_{j \neq Nov2016} \beta_j^1 T_{ic} \times A_{ia} \times \mathbb{1}[j = t] + \beta^2 T_{ic} \times A_{ia} + \sum_{j \neq Nov2016} \beta_j^3 T_{ic} \times \mathbb{1}[j = t] + \sum_{j \neq Nov2016} \beta_j^4 A_{ia} \times \mathbb{1}[j = t] + \gamma_t + \delta_c + \kappa_a + \epsilon_{iact} \quad (2)$$

where to conserve space we do not separate the sums into pre- and post-effects but rather denote them as a single term omitting the reference period of October 2016. Here we can decompose the coefficients of interest  $\beta_j^1$  into pre-trend estimates  $\sum_{j=Jan2016}^{Oct2016} \beta_j^1$  and post-treatment effects of interest  $\sum_{j=Dec2016}^{Dec2017} \beta_j^1$ . The dependent variable,  $Y_{iact,t}$ , now equals to one if an individual  $i$  from birth cohort  $c$  and age group  $a$  is employed in a given time period (month-year)  $t$ . Since this is a tripple-difference estimator we also include all double-difference interactions in this equation – represented by coefficients  $\beta^2$ ,  $\sum_{j \neq Nov2016} \beta_j^3$ , and  $\sum_{j \neq Nov2016} \beta_j^4$  – as well as fixed effects for age group ( $\kappa$ ), cohort ( $\delta$ ), and time period ( $\gamma$ ). Here cohorts and time periods are defined in the same way except we need to shorten the time frame by 12 months so that we can add the third age difference while age fixed effects are dummies for these 12 lagged monthly birth cohorts. Thus,  $a$  represents age in month  $t$  and treatment is now defined across both cohort (with respect to calendar month  $t$ ) and age. The identification of parameters of interest  $\beta_j^1$  relies on different treatment effects with respect to age for different cohorts.

## 4 Data

We use individual-level administrative data that match tax, health insurance, and social security information via a unique identifier. The data are compiled by the Ministry of Finance and we are one of the first researchers to be able to use it for scientific purposes. The data cover the period between January 1st 2015 and December 31st 2017. Therefore, we have employment information for 31 months prior to when the reform which reduced retirement age became effective and two months after. Importantly, since the reform was legislated in the autumn of 2016 and was signed into law in December of that year, we have a period of a legislative hold-up of about 12 months before the new cohorts became covered by the EPL. This period, given the anticipated theoretical consequences of the legislation, can be considered as a treatment period from the point of view of the cohorts who were to become protected from October 2017 onward. In fact, we would expect most dismissals to occur exactly during the hold up period once the employers learn the future protected status of their employees but before these employees become formally covered by the employment protection law.

The data we use is derived from three separate administrative sources: the Social Insurance Institution (ZUS), our main database which collects information at monthly level on all income sources from which social security contributions are deducted, the Personal Identification Number database for information on gender, the date of birth and information if a person is alive at the end of the studied period, and finally the PIT database which is annual but provides additional information used for sample selection with respect to the receipt of retirement pension. The ZUS data has a nearly universal coverage with respect to the working population. The main

exceptions are farmers who have a separate insurance scheme, students' temporary jobs and result-based contracts. Overall the ZUS data covers (1) workers on employment contracts, (2) uniformed services, (3) judge and public prosecutors, (4) workers on civil contracts, (5) individuals conducting business and (6) members of supervisory boards. For the purpose of our analysis we exclude categories (2) and (3) as these are jobs regulated by very specific types of contracts. Moreover, because employment protection covers only those on employment contracts, for the purpose of the analysis we treat those on civil contracts and the self-employed as not employed, since they are not subject to the EPL.<sup>4</sup> Additionally the data match with health insurance contributions facilitates the match of information on contributions towards health insurance paid by the labor office, which allows us to identify those registered as unemployed. This means that the non-employed group in the studied samples are: those who were registered as paying contributions at some point and who are missing from the data at other periods (when they are considered as non-employed), those who are registered as paying contributions on civil contracts and self employment (since they are not covered by the EPL), and those who at least once over the studied period were officially registered with the government as unemployed (they are then considered non-employed throughout - unless of course they appear in the data as working on employment contract).<sup>5</sup> The employee-employer match which is possible in the ZUS data for most registered contributions (with the exception of the self-employed) is used to assign an industry code to each working person which, in turn, allows us to exclude industries with industry-specific retirement regulations and employees in the public sector.<sup>6</sup>

In order to address the issue of eligibility for sector specific early retirement regulations among those who are not selected out of the sample, we exclude from the sample those who by the end of 2017 are recorded as receiving such pensions. Similarly, to limit the influence of age effects on disability, we also exclude those on disability pensions. These data form the basis for our main estimation sample (sample 1). To examine the possible endogeneity of disability pensions - as those laid off could in consequence seek to receive disability pensions - our alternative sample includes those identified as ever receiving disability pensions (sample 2). As noted earlier, for each of these two samples we specify two different treatment and control samples (a) and (b) as specified in Figures 3 and 4. This gives rise to four samples used for the analysis presented below: 1a, 1b, 2a and 2b. The respective overall numbers of men in each of them are approximately: 132

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<sup>4</sup>Our results are not sensitive to how we classify this group of workers and to the extent that the reform induced changes in the type of employment we should be able to observe these changes in the data.

<sup>5</sup>In principle, a worker can have several records in each month, for example, if they change jobs or work at more than one institution; in such cases we select the observation within a month with the highest earnings.

<sup>6</sup>In particular, 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 e.g., United Nations (U).

thousand, 76 thousand, 125 thousand, and 73 thousand individuals. For women, these numbers are: 126 thousand, 73 thousand, 121 thousand, and 70 thousand individuals, respectively. In each case the panel samples we work with are balanced as we only include individuals who were alive at the end of 2017. Thus every individual is in the sample for 36 consecutive months observed as either employed or non-employed. Specific details, including decomposition into treatment and control groups, on these samples are presented in Tables 1 and 2.

## 5 Results

Figures 5 and 6 present our main results for men and women, respectively. Panels A and B present results based on our difference-in-differences approach defined in Equation 1 while Panels C and D present results based on the tripple-difference estimator defined in Equation 2. Column 1 of each Figure uses the sample where we define treatment and control groups based on January to October while column 2 uses more limited set of months from May to October. Finally, in each graph, the black lines represent estimates excluding those on disability pensions while grey lines include these individuals in the sample. The dashed vertical line defines November 2016 when the reform was voted into law while the solid vertical line represents October 2017 when the legislation become binding and workers gained legal protections against unjust dismissal. Thus, the period between November 2016 and October 2017 is our hold-up period where we expect most of potential worker firing to happen.

Contrary to the theoretical predictions and unlike in two earlier studies ((Behaghel et al. 2008) and Deelen et al. (2009)), irrespective of the exact sample, treatment definition, estimation method, or gender we do not find any statistically significant effects of stricter employment protection legislation on employment of the covered workers. In difference-in-differences specifications the sample of men where we include individuals on disability pensions exhibits small pre-trends which is consistent with the idea that age could play a role in differential responses when it comes to common shocks and be correlated with the ability to claim disability pension. When we account for this possibility in our tripple-difference estimator, this age-specific pre-trend is eliminated in either sample. We do not observe any statistically significant pre-trends for women in double difference estimators but we nonetheless present the tripple difference specifications for comparability. In the months following the introduction of the legislation in late 2016, i.e. the time when employers could have reacted to the fact that the younger cohorts of their employees would become affected by the EPL, we do not find any changes in the estimates. Coefficients in the sample excluding disability pensioners are more negative compared with the sample where we include them, but this pattern is only present for men, and both sets of estimates for both genders are never statistically significant.

Focusing more on the magnitudes of the estimates, even the most negative coefficients are quantitatively small and appear economically insignificant. For example, for men, the most negative lower bound of the 95% confidence interval in panel D implies an effect of approximately -0.0075 while the mean of the employment for males in this group is about 65%. This thus implies an employment effect size of at most about negative 1%. For women, the point estimates are even smaller.

## 6 Conclusions

There is much controversy when it comes to employment protection legislation. On the one hand, such regulation increases costs of laying off potentially lower productivity workers and in fact could induce lower productivity if employees take advantage of increased protection. On the other hand, added job security could actually increase worker motivation and output. Furthermore, any effect could depend on the business cycle and the tightness of the labor market. In particular, at times of high overall labor demand the additional expected burden might not discourage firms from hiring and/or from keeping the employees who are about to become eligible. Policy-wise the debate on age specific employment protection could become critical in the coming years given rapid aging of the population in many developed countries.

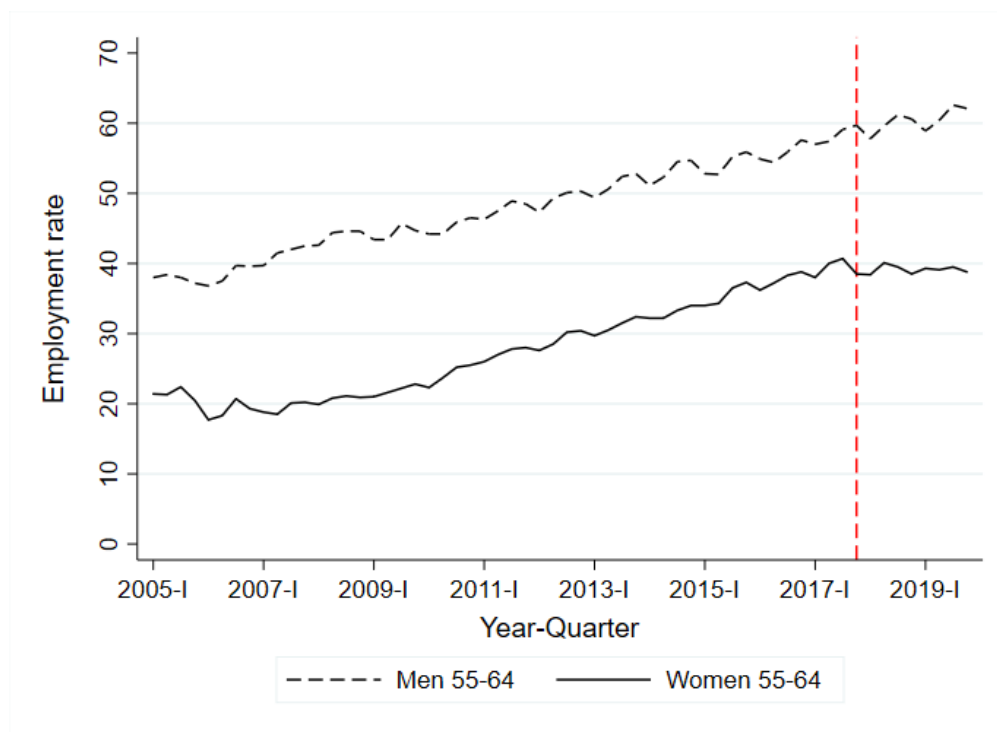
In this paper we document the effects of covering pre-retirement age workers with strict employment protection legislation. Using administrative data from Poland and a reform to retirement age which had a side effect of extending employment protection to younger cohorts, we show that protection granted to workers for up to 4 years prior to their official retirement age does not have any negative employment effects on those who are about to become eligible. This is true for both men and women and we generally do not see any meaningful gender differences in the effects of the employment protection. In the next iterations of this work we intend to study heterogeneous effects by local labor market and sector and investigate additional outcomes including wages.

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Figure 1: Employment in Poland 2005-2019: men and women aged 55-64

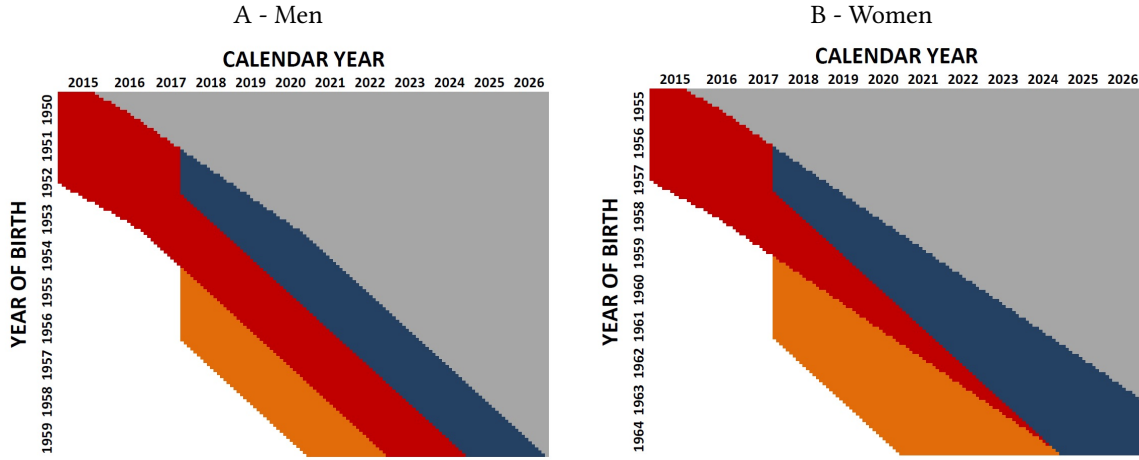


Source: Polish CSO based on the Labor Force Survey.

Notes: Vertical line marks the implementation of reduced retirement age in October 2017.



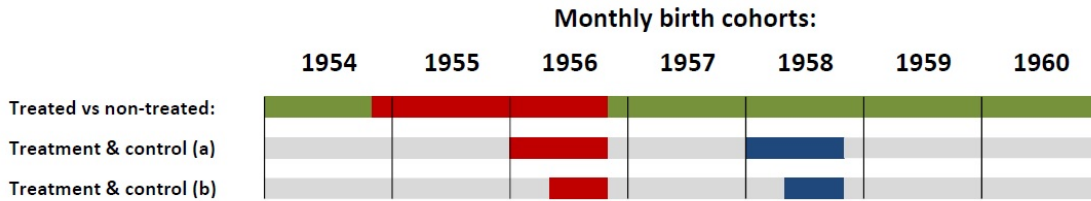
Figure 2: Retirement age and employment protection by cohort



Source: Authors' compilation.

Notes: Different colours represent retirement and employment protection eligibility for different birth cohorts (vertical axis) at different points in time (horizontal axis) in the pre- and post-reform systems: grey - pre-reform retirement eligibility; blue - post-reform retirement eligibility; red - pre-reform EPL eligibility (overlapping with red); orange - post-reform EPL eligibility.

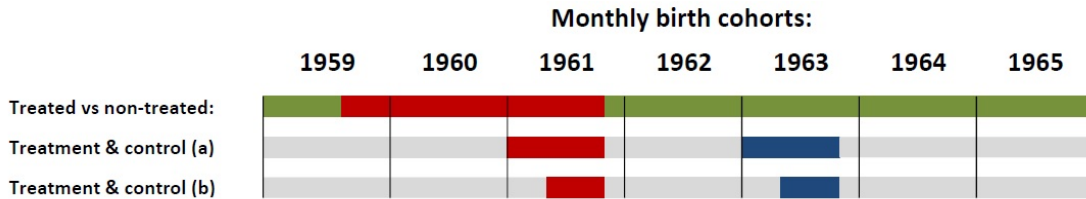
Figure 3: Treatment and control samples for different specifications: men



Source: Authors' compilation.

Notes: Treatment and control cohorts with reference to treatment in October 2017.

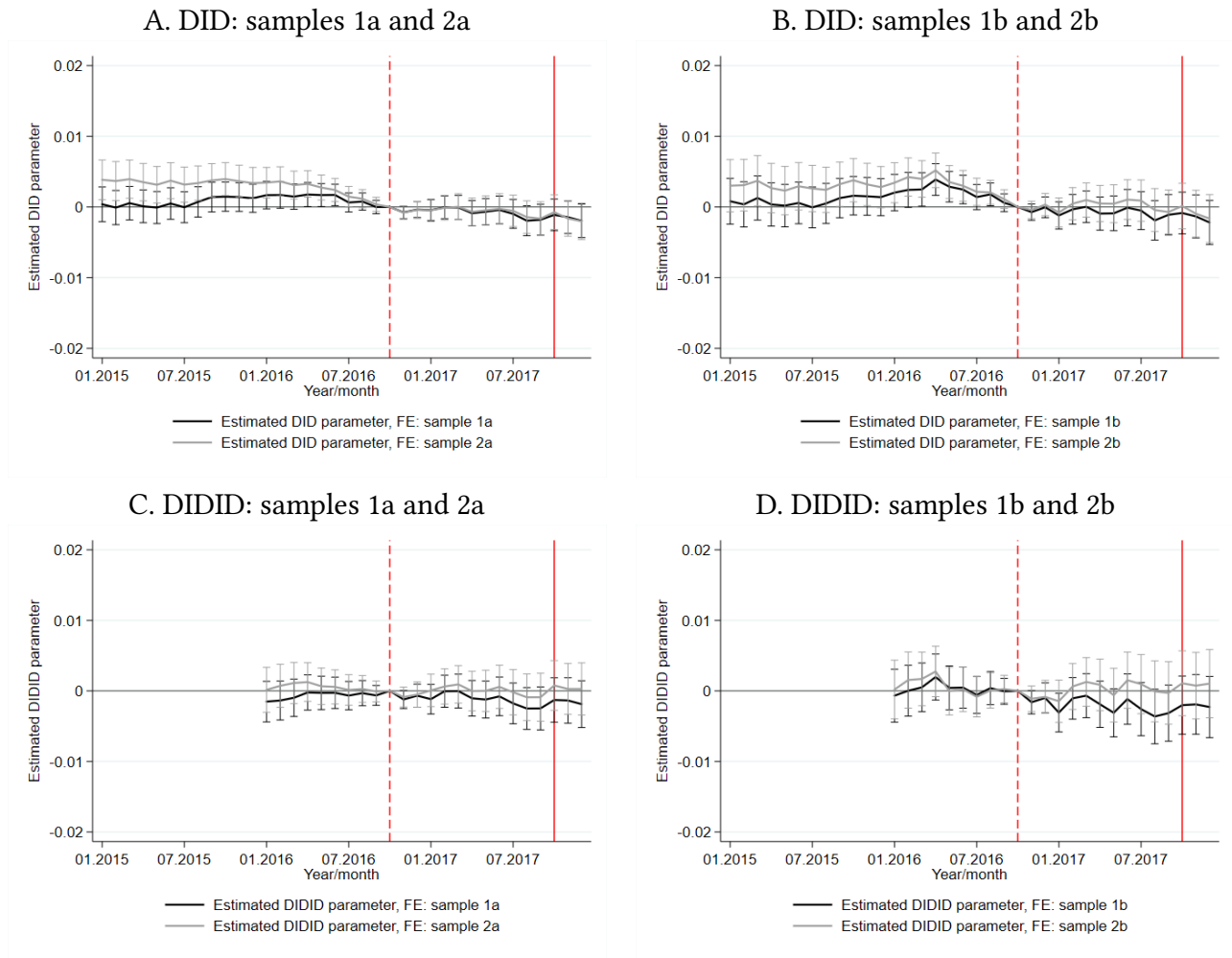
Figure 4: Treatment and control samples for different specifications: women



Source: Authors' compilation.

Notes: Treatment and control cohorts with reference to treatment in October 2017.

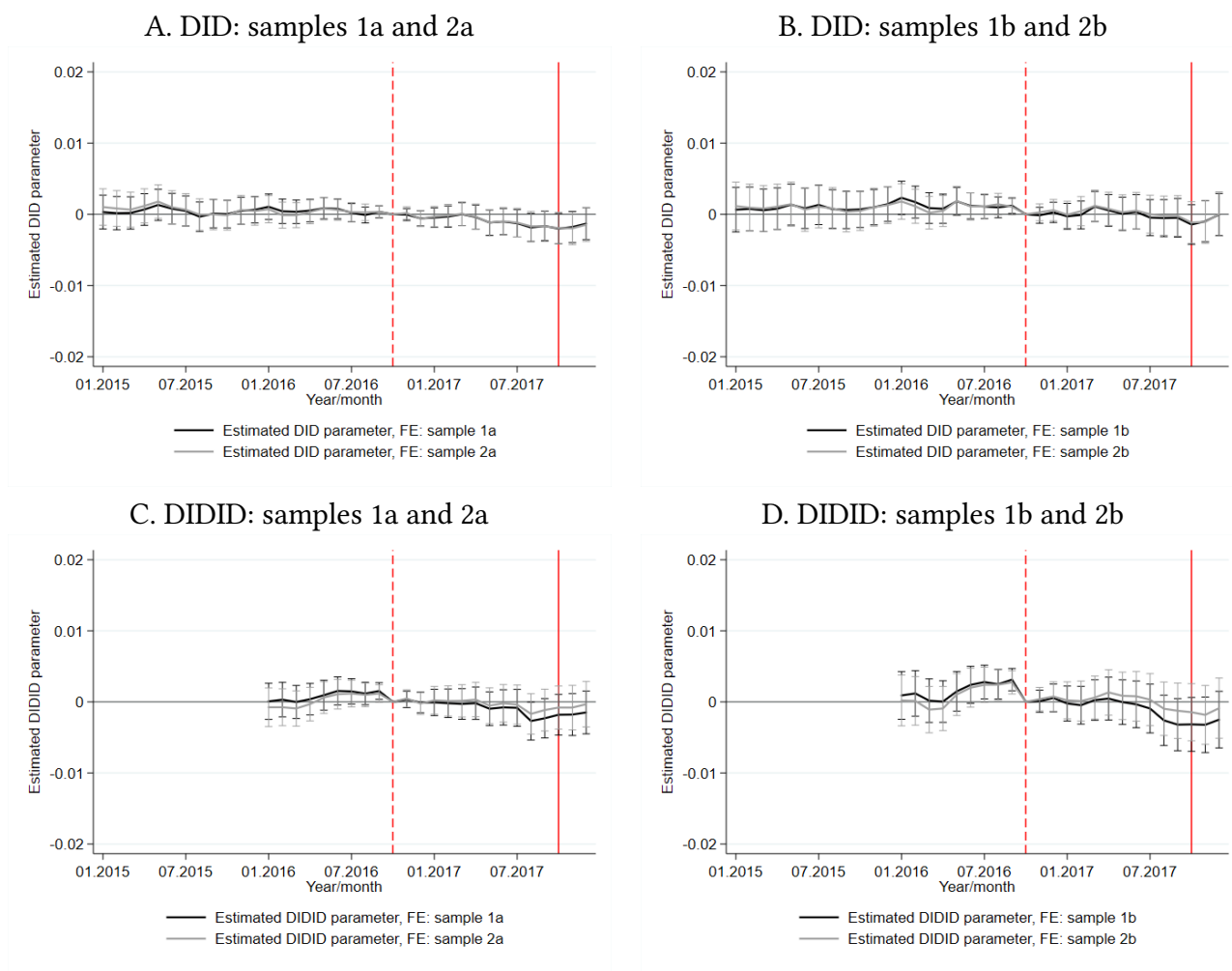
Figure 5: Pre-retirement employment protection: DID and DIDID estimates for men



Source: own calculations on MF administrative database.

Notes: Treatment and control cohorts as defined in Figure 3, samples 1a and 2a; vertical lines mark October 2017 (solid line) - the time when lower retirement age took effect, and October 2016 (dashed line) when the final shape of the reform was adopted by the ruling party. The latter considered as the date of treatment.

Figure 6: Pre-retirement employment protection: DID and DIDID estimates for women



Source: own calculations on MF administrative database.

Notes: Treatment and control cohorts as defined in Figure 4, samples 1a and 2a; vertical lines mark October 2017 (solid line) - the time when lower retirement age took effect, and October 2016 (dashed line) when the final shape of the reform was adopted by the ruling party. The latter considered as the date of treatment.

Table 1: Employment protection legislation: sample sizes for the estimation: men

	Total sample:	Treatment:	Control:
Sample 1a			
Individuals	132100	56625	75475
Observations	4755600	2038500	2717100
Sample 1b			
Individuals	76492	33128	43364
Observations	2753712	1192608	1561104
Sample 2a			
Individuals	125464	52993	72471
Observations	4516704	1907748	2608956
Sample 2b			
Individuals	72695	31097	41598
Observations	2617020	1119492	1497528

Source: Ministry of Finance administrative data set.

Table 2: Employment protection legislation: sample sizes for the estimation: women

	Total sample:	Treatment:	Control:
Sample 1a			
Individuals	125866	61427	64439
Observations	4531176	2211372	2319804
Sample 1b			
Individuals	72880	35696	37184
Observations	2623680	1285056	1338624
Sample 2a			
Individuals	120671	58515	62156
Observations	4344156	2106540	2237616
Sample 2b			
Individuals	69859	33987	35872
Observations	2514924	1223532	1291392

Source: Ministry of Finance administrative data set.