THREE ESSAYS IN LABOR ECONOMICS

By

Marissa Dae Eckrote-Nordland

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ABSTRACT

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This dissertation is comprised of three chapters analyzing how establishments react to increases in pensionable age.

Chapter 1: Understanding the Impact of Postponed Retirements on the Hiring Decisions of Firms

The solvency of public pension systems in countries with pay-as-you-go pension schemes have led many of these countries to adopt changes in the age of eligibility for full-benefits. One such country is Germany who implemented a change in their pensionable age in a major reform enacted in 1992. There have been multiple studies that have looked at the effectiveness of this reform in terms of older workers delaying their retirements. However, less is known about how firms have reacted to these changes and if these changes in policy have caused firms to change their hiring behavior. Using administrative linked employer-employee data I exploit pre-policy variation in worker age distributions to serve as a source of identification for studying how employers reacted in-terms-of hiring behavior. I find that firms that had a higher share of older workers, and thus were impacted more by the change in pensionable age, decreased their hiring. For a one percentage-point increase in the share of workers who are predicted to have retired under the old pension system the share of workers that are new hires decreases by 0.324 percentage points. This is a 2.16% decrease at the mean. When smaller age bins are studied, I find that this negative impact is found for those aged under 25 and those age 25-34. In contrast there is a positive impact on individuals age 45-54, 55-64, and over 65. When looking at contract types there is an over 7% decrease in the hires of trainees and an over 10% increase in the hires of workers on partial retirement contracts.

Chapter 2: Effect of Postponed Retirements on Wage Growth of Younger Workers (with Peter Berg, Mary Hamman, Daniela Hochfellner, Matthew M. Piszczek and Christopher Ruhm)

This paper uses linked-employer-employee data to examine the effects of postponed retirements on the wage progression of younger workers within establishments. A German pension reform is the source of identification. We find no evidence of slower wage growth. Instead we find faster wage growth, especially among workers aged 41 to 57. We cannot rule out separations as a mechanism, but patterns in estimates by age and tenure are not consistent with layoffs. Instead, we find evidence of less frequent promotions and we interpret the wage findings as consistent with compensating wage differentials for postponed promotions.

Chapter 3: Pension Reforms and their Implications for Establishment Downsizing (with Peter Berg, Mary Hamman, Daniela Hochfellner, Matthew M. Piszczek and Christopher Ruhm)

While the empirical literature on the effects of pension reform on workers is broad, less is known about the impact on employers. Yet reforms that create incentives to postpone retirement may have extensive effects on employer labor demand and labor costs, especially in settings where there are strict legal protections against age discrimination in employment. Although public pension system reforms generally are structured to treat all workers within the same birth cohort similarly, the impact on employers may vary substantially due to differences in the age composition of their employees. Using this variation as a source of identification, we examine whether the differential impact of pension reform leads to differences in the incidence of workforce downsizing, a sign of possible financial distress. To ensure estimates are not biased due to attrition, we also model associations between the impact of pension reform and establishment closures and find no association. Results for downsizing consistently show establishments with a higher share of older workers are more likely to experience downsizing. When we segment workers within establishments by age, the absolute changes in downsizing probabilities are highest for younger workers. Preliminary results indicate works councils may increase the risk of downsizing for older workers and protect employment for young and prime workers.

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CHAPTER 1

UNDERSTANDING THE IMPACT OF POSTPONED RETIREMENTS ON THE HIRING DECISIONS OF FIRMS

1.1 Introduction

Individuals around the world are living longer; according to the latest available estimates from the World Bank those born in Germany in 2016 have a life expectancy of almost 81 years, compared to less than 70 for those born in 1960 (The World Bank, 2019). If no changes are made, this has the potential to threaten the solvency of pay-as-you-go pension systems. For this reason, many OECD countries with these systems have opted to increase the age of eligibility for full benefits (OECD, 2011). Some notable examples are the United States' increase in pensionable age from 65 to 67 and Germany's 1992 pension reform which increased the age for full pension benefits from 60 to 65. However, these increases in pensionable age have led to some concerns among younger workers. This can be seen in popular press articles with titles such as "Are older workers getting in the way of the young?" (Miller, 2012) and "Are older workers job hoarding, hurting economy? 44% of young employees say graying workforce is a problem. (Soergel, 2019)". Concerns such as these are based on beliefs that there are a fixed number of jobs in an economy. Economists have studied this question for years and have even gone as far as labeling it the "lump of labor fallacy" Walker (2007). However, this is based on research that has been done at a macro level, meaning that there does not appear to be a fixed number of jobs in the economy as a whole (Gruber and Wise, 2010; Brugiavini and Peracchi, 2003; Jousten et al., 2010; Munnell and Wu, 2012). Yet, at the establishment level there is much less known about how firms react to increases in pensionable age, and if there are a fixed (or relatively fixed) number of positions within establishments.

A large literature has studied the labor supply effects of changes in pensionable age and has found that individuals, who are directly impacted, do react to these changes in pensionable age by adjusting their retirement behavior (Stock and Wise, 1988; Krueger and Pischke, 1989; Börsch-

Supan, 2000; Duval, 2003; Coile, 2004; Barr and Diamond, 2006) . However, much less is known about how firms have reacted to these changes in retirement age. If the additional workers are imposing new unexpected constraints on firms' budgets and/or are impacting the firms' production processes, then the firms may be forced to make adjustments. A margin that has previously been studied in the context of this reform is that of wages. A recent working paper finds that there is little evidence of negative impacts on the wages of younger workers in firms with greater shares of older workers (Eckrote-Nordland et al., 2021). Therefore, if firms must adjust they may be adjusting on the hiring margin. This is especially likely in the German context due to the worker protections in place making layoffs difficult.

A previous macro level study of the 1992 German pension reform studied here by Gruber and Wise (2010) find a positive correlation between youth and senior employment, lending to the notion that the "lump of labor" is indeed a fallacy. However, this paper along with many others that study pension reforms do not allow for the study of individuals within the same employer to get at the question of how are establishments responding to these reforms. For this reason a new strand of literature has started to develop that uses linked micro-data to study this question in different contexts. In the context of Italy Bertoni and Brunello (2017) find than an increase in the labor supply of senior workers decreases employment of younger workers. Bianchi et al. (2019) also study Italy but look at within-firm spillovers of a 2011 pension reform they find that older workers that delay retirement there are reductions in hiring of new workers and an increase in layoffs of current employees. A third paper that studies Italy is Carta et al. (2020) actually finds the opposite effect that an increase in pensionable age leads to an increase in the employment of younger workers. In the context of the United States Mohnen (2019) looks at delays in retirements across commuting zones and finds that these delays lead to decreases in the shares of young workers working in high-skill jobs and increases in the share of young workers working in low-skill jobs. Crowd out of young workers in response to increases in pensionable age are also found in Japan (Nakazawa, 2020) and the Netherlands (Hut, 2019).

These adjustments in hiring are important to study because they are one way that firms may adjust when facing this increase in pensionable age. As shown in Bönke et al. (2015) the age earnings profile in Germany is quite steep and earnings of older individuals are large. For this reason, when the retirement age is increased, this can pose a large unexpected cost shock on firms who may have been planning on their older workers to retire in the near future rather than to stick around and work longer. However, this may not be the case. On the contrary, firm, specifically those in less physically intensive industries, may see older workers as a large benefit and engage in efforts to retain and potentially even attract new more senior workers now that they will be able to work longer. Thus, this is not a theoretical question to be studied rather a question that needs to be looked at empirically.

1.2 Institutional Details

1.2.1 Pension System

The pension system in Germany is a pay-as-you-go system where current tax contributions fund payments to current pension claimants. This ensures a minimum standard of living for the pension claimants who were previously employed in either the private or public sector and were entitled to social security. The pension system covers about 90 percent of the German workforce (Richter and Himmelreicher, 2008). In addition, private savings were quite small for the pension claimants in the period studied. About 85 percent of retirement income for these individuals came from the public pension system. (Börsch-Supan, 2000).

In 1992, Germany announced a change to its pension system which gradually increased the age at which individuals were eligible to claim full benefits. Before this reform, women were eligible to claim full benefits at the age of 60. For men, the official pensionable age before the reform was 65. However, the effective retirement age was 58. This was possible because at age 60 men who were classified as unemployed could begin claiming their pension. In addition, men could also claim two years of unemployment benefits prior to being eligible for this pension. Thus, by combining these two benefits the effective pensionable age was 58. Prior to the 1992 reform, approximately 45 percent of 59-year old men self-identified as "retired" and only 20 percent of new pension claimants were age 65 (Börsch-Supan and Wilke, 2004). [1] This 1992 reform impacted all workers, except those eligible for disability pensions, beginning with those belonging to the 1938 birth cohort. This cohort reached age 58 in 1996, meaning the reform was effective in postponing pension claims only four years after it was announced. All changes in eligibility associated with the 1992 reform were phased in between the 1938 and 1945 birth cohorts and thus were fully implemented by 2011 (Börsch-Supan and Wilke, 2004).

Figure A.1(Börsch-Supan and Wilke, 2004) below shows how the 1992 pension reform impacted cohorts born between 1931 and 1944. In the figure the triangles show the pre-reform effective pensionable age of 58 for both men and women. The circles indicate the new effective pensionable age for women, whereas the squares show the new effective pensionable age for men. The reform created differences in pensionable age of 6 months to 18 months between adjacent birth cohorts.

The structure of this reform created heterogeneity across individuals with similar birth dates, and thus created variation in the impact of this reform across employers due to differences in pre-reform age distributions. This variation will provide a source of identification for estimating how the delayed retirements among older works impacted firms' hiring decisions.

Further reforms were passed in 2002-2004 known as the Hartz reforms. These reforms did impact unemployment benefits, however, most older workers were still able to claim two years of unemployment benefits before going on to an unemployment spell pension. In addition, in 2007 another pension reform was passed that increased the age of eligibility for full pension benefits from age 65 to age 67. This change became effect starting in 2011. For this reason I use only the years prior to 2011 in my sample.

1.3 Data

The data used for this analysis comes from German matched employer-employee administrative data: the cross-sectional Linked Employer Employee Data of the Institute for Employment Re-

[1] This unemployment pathway to retirement remained open after the 1992 reform, but the duration of unemployment benefits was still only two years during my study period.

search (LIAB).[2] One key advantage of the LIAB is the matching of survey data from a national stratified random sample of German establishments to social security employment records for all establishment employees covered by the system. However, one issue of using this data for the current analysis is that it begins in 1993 and the pensionable age change was announced in 1992. For this reason, I also use a custom file containing establishment demographic information in 1990.[3]

The data used in the analysis to calculate hires is at the individual-level with over 40 million person-year observations spanning the years 1993-2010. These observations are then aggregated to the establishment-year level resulting in a sample size of 68,407 establishment-years. Summary statistics on the sample used for the following analysis are described in Table A.1.

1.3.1 Sample Restrictions

The current sample is restricted to firms with at least five employees in all years that they are observed. This is to prevent one entry or exit into the firm from vastly changing the age composition of the firm. However, the robustness to several different minimum firm size thresholds has been assessed. In addition, for the construction of the shift-share instrument, I need information on firm age-distributions before the policy was enacted. To construct these instruments, I use data from a custom demographic file from 1990. Therefore, an additional restriction is that the firm must have existed in 1990 and be present in the demographic file. This limits the sample to West German firms[4]

[2]This study uses the Linked-Employer-Employee Data (LIAB) [cross-sectional model 2 1993-2010 (LIAB QM2 9310)] from the IAB. Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access.

[3]The demographic file comes from the Employment History data (BeH) and was provided by the Research Data Centere of the Institute for Employment Research (FDZ). I thank Andreas Ganzer for sampling the data for me and supporting me with de-identification of the data.

[4]East German records are reliable beginning in 1993.

1.3.2 Measuring Hiring

The preferred method of measuring hiring is constructed using a variable that indicates the date that an individual was hired at their current establishment in their current job spell. First, a dummy variable is created for each individual which indicates if they were hired at that establishment in the previous year. As the survey is conducted on June 30th of each year an individual is coded as hired in the previous year if they were hired by June 30th of that year but after June 30th of the previous year. One limitation of measuring hiring in this way is that it misses hires that occur after June 30th which are followed by an exit from that establishment before the next June 30th. However, due to constraints on firing and overall low levels of churn in this setting the number of these cases is most likely small. These hires are then aggregated at the establishment-year level to get a raw count of hires in the given year. The raw counts of hires in each year are scaled by the contemporaneous firm size to construct the variable that will be used in the analysis which is the share of employees in the establishment that were hired in that survey year.[5]. Constructing the share of new hires variable in this way allows me to capture individuals who are trainees being hired as regular workers, as these are considered different jobs.

1.3.3 Measuring Working Longer

As explained above, the pre-reform effective retirement age was 58. Therefore, to measure the impact of the reform on firms by individuals working longer I construct, $share58_{jt}$, yearly shares of workers in each establishment age 58 and older who were not hired during the previous year. This is important because if these new hires are included in the share variable they would be impacting both the key independent variable and dependent variable simultaneously.

[5]Other scaling factors were checked for robustness but are not presented here due to data clearance constraints.

1.4 Empirical Strategy

In order to study the impact of the increased pensionable age and firms' hiring behavior I estimate the following OLS regression.

$$hired_{jt} = \beta_1 share58_{jt} + \gamma X_{jt} + \alpha_t + \epsilon_{jt}. \tag{1.1}$$

In (1.1), $share58_{jt}$ is the share of workers in establishment j who are age 58 and older in year t. The dependent variable, $hired_{jt}$, is a measure of an establishment $j's$ hires in year t. X_{ijt} is a vector of controls including a set of establishment characteristics (industry, share female, share medium skilled, share high skilled, total wage bill, firm size, share part-time, collective agreement status, legal form, works council status, state), and year dummies.

1.4.1 Endogeneity Concern

There are at least two reasons that the OLS estimates of equation (1.1) would be biased. The first is that if firms that have more older workers in-fact hire fewer younger workers these workers may not even apply to these jobs. This could be due to the fact that these younger workers may have preferences for working at younger firms or they may fear that theses additional older workers may limit their potential for advancement.

A second concern is that employers may attempt to reduce any impact on hiring by using buyouts or partial retirement offers with their older employees. Establishments that can afford these offers may have more capital available to them to continue to hire as they would without these changes in pensionable age. This would positively bias the estimates. On the other hand, these buyouts may constrain already tight budgets for some establishments, thus reducing hiring abilities of these firms. This would negatively bias the estimates. I can control for these responses if the establishment reports this in their survey. However, there is likely informal offers and other implicit pressures and preferences that I cannot control for.

1.4.2 Identification Strategy

In order to avoid these potential biases,I instrument for the contemporaneous share of workers older than 58, $share58_{jt}$, using share of workers predicted to have retired under the previous policies. Thus, I use the 1990 (pre-reform) age distributions for each of the establishments in my analysis. Then using these distributions, I construct a variable that captures the share of workers that fall between their old pensionable age and their new pensionable age in each year t assuming the establishment followed the entry and exit probabilities by age and sex for their industry as a whole. This approach is commonly referred to a shift-share instrument. The subsequent discussion explains these instruments in greater detail.

As shown in figure A.1, the 1992 pension reform created a gap between the old and new pensionable age for cohorts born after 1937. I then use the custom data set containing the 1990 demographic information to calculate the pre-reform counts of workers by sex that fall in these gaps. These counts of workers are what make up the "share" portion of the shift-share instrument.

The shift part of the shift-share instrument is computed after estimating regressions for entries and exits using the 1993-2010 data separately for each of 11 industries by sex. This results in 44 regressions being run.

$$begin_{ij,t} = \beta_0 + \beta_1 age_{ij,t} + \beta_2 year_t + \beta_3 age_{ij,t} * year_t + \epsilon_{ij,t} \tag{1.2}$$

$$end_{ij,t} = \beta_0 + \beta_1 age_{ij,t} + \beta_2 year_t + \beta_3 age_{ij,t} * year_t + \epsilon_{ij,t} \tag{1.3}$$

In the above regressions $begin_{ij,t}$ is a binary variable equal to 1 for employees in their initial year of employment with establishment j in year t. It is then zero for all years following. Likewise, $end_{ij,t}$ is a binary variable equal to 1 for those in their last year of employment with the establishment. A vector of single year age dummy variables is constructed in $age_{ij,t}$ and $year_t$ is a vector of dummy variables for each year 1994-2014. Next, I get fitted values $\widehat{begin_{ij,t}}$ and $\widehat{end_{ij,t}}$ for the age-distribution for each year from the 44 regressions specified above.

Using theses fitted values, I am able to age the 1990 age-distribution in each establishment forward in the following way:

$$workers_{a,jt} = workers_{a-1,jt-1} * [1 - \widehat{end_{a-1,ij,t-1}} + \widehat{begin_{a,ij,t}}] \tag{1.4}$$

Here workers of age a in year t is equal to the number of workers of age $a-1$ in year $t-1$ adjusted by the probabilities of ending employment at age $a-1$ in year $t-1$ and the probability of beginning employment in the industry at age a in year t. This measure is constructed separately by age and sex by industry sector. Using these constructed worker counts I can then find the projected number of workers in an establishment that will fall between their old and their new effective pensionable age. These counts are then scaled by the size of the firm in 1990 resulting in the instrument $ingap_{j,t}$.

This constructed instrument is an example of what is known as a shift-share instrument which are also known as "Bartik instruments" after Bartik (1991). Previously these have been used mostly in the context of immigration and the regional growth literature but have many other applications.

It is important to note that recent studies raise concerns about the validity of shift-share instruments(Goldsmith-Pinkham et al., 2018; Borusyak et al., 2018; Jaeger et al., 2018). Jaeger et al. (2018) groups these concerns into four major categories. The first concern is that of the exclusion restriction. This requires the shares, in my case the 1990 age distributions, to be exogenous. Next, the shares must exhibit sufficient variation so that the establishments receiving the same industry-specific shift are different. Third, analogous to the requirement in the immigration literature of no spatial spillovers, there cannot be spillovers across establishments. Lastly, the 1990 shares must be observed in a steady-state and not adjusting due to previous policies. I can address the first concern by using the 1990 data. This was prior to the reform being implemented and six years before the policy began to bind for the earliest cohorts. The second concern is addressed empirically below.

Using the 1990 demographic information I am able to summarize the share of workers in each firm born before 1937 (those unaffected), born between 1938 and 1945 (partially affected), and lastly those born after 1945 (those fully affected by the reform). If there is substantial variation in these shares, this provides strong evidence for variation in the instrument since the instrument uses single year birth cohorts. Next, these shares are compared to the shares constructed for the industry's share of employees in these cohorts. For example, if an establishment in the Trade/Food Service industry had 20% of their employees in the cohorts between 1938 and 1945 and the industry as a whole has 20% of their employees in those cohorts as well, the firm would have a ratio of 1. To visualize how this variation described above will translate into differences across establishments within the same industry, Figure A.2 plots the frequency distributions for the employment share ratios for the 1938-1945 cohorts for the Energy and Water Supply and Transportation sectors. These sectors are specifically chosen because Energy and Water Supply is the sector with the smallest standard deviation (0.41). Conversely, Transportation has the largest standard deviation (0.74).

The distribution for Energy and Water Supply is much more compact than that for Transportation, but there is still variation. Although about 50% of the establishments in Energy and Water Supply have ratios for the 1938-45 cohorts that are 50% to 99% of the industry employment share, there are about 8% that have ratios below 50% and about 5% have ratios above 150%.

The third concern would be a problem if a change in one firm's hiring decisions has a causal effect on the hiring decisions of other firms who do not experience the same change in their age-distribution. However, if this is true it would bias my results towards zero as it would cause firms that are affected differently to behave similarly. The fourth concern is mitigated from the fact that the last pension reform prior to the 1992 reform occurred in 1972. This provides evidence that the 1990 demographic data is coming from a period of relative stability.

Lastly to demonstrate that the constructed instruments have variation both overall and across time, Figure A.3 displays those variations in panel a and panel be respectively. As can be seen both panels provide evidence of substantial variation.

One final check for the validity of the instrument is to check for correlation between the instrument and observable characteristics of the establishment. This is done by running a series of regressions of the following format:

$$ingap_{jt} = \beta_0 + \beta_1 Characteristic_{jt} + \alpha_j + \theta_t + \epsilon_{jt} \tag{1.5}$$

Where $ingap_{jt}$ is the constructed shift-share instrument, $Characteristic_{jt}$ is the establishment level characteristic, and $alpha_j$ and $theta_t$ are industry and year fixed effects respectively. Ideally, the coefficient of interest $beta_1$ would not be statistically significant, giving an indication that the instrument is not correlated with firm level observables. Results for these regressions are presented in Table A.2. It can be seen that there is a statistically significant correlation between the instrument and many of the firm level observables. This means that when controlling for industry and year the given characteristic and the synthetic share of employees in the gap between their old and new full retirement age are correlated. There appears to be a negative correlation between the instrument and the share of female workers, the share of part-time workers, and the binary indicator for an establishment having a works council. This is not surprising as the reform did not bind for women until later so we would expect firms with more workers in the gap to be firms that employ more men in general. There appears to be a statistically significant positive correlation between the synthetic share of workers in the gap between their old and new retirement age and the share of high skill workers, the total wage bill, the firm size, the binary indicator for legal form, and the binary indicator for if the establishment is located in Berlin. Again, many of these are not surprising, for example the share of high skill workers is likely to be positively correlated with the instrument as high skill workers are likely in jobs that allow them to work longer than those that are classified as lower skill and likely working in more physically demanding jobs.

1.5 Results

The results when I estimate (1.1) and pool across all years and age groups are presented in Table A.3. We can see that in both the OLS and IV specification there is a negative impact of additional older workers on the share of new workers hired by establishments. Specifically when looking at the IV specification a one percentage point increase in the share of workers age 58 and older at an establishment leads to a 0.3243 percentage point decrease in the share of new hires at that establishment. This is an over 2% decrease in the share of new hires at the mean. A one standard deviation increase in the share of workers over age 58 would lead to over a 2 percentage point decrease in the share of new hires at the establishment.

These results are not surprising if establishments face either budget constraints imposed by workers delaying retirements and/or constraints on the size of their workforce. However, they may have important implications for establishments' trajectories if they are not able to bring in new workers. To better understand which groups are being directly impacted most at the establishment level, next I study the impact on the hiring of different age groups.

1.5.1 Heterogeneity by Age

To conduct the heterogeneity analysis by age, I construct new dependent variables similar to that constructed in 1.3.2. However, now instead of counting all individuals hired in a given year and then scaling by contemporaneous firm size, I only count those in the given age group that were hired in that year and then scale by contemporaneous firm size. I construct six new variables using six age groups, under age 25, age 25-34, age 35-44, age 45-54, age 55-64, and age 65 and older.

Results for the IV estimation of 1.1 with these six new dependent variables are presented in Table A.4.

The impact on the youngest groups, those under age 25 and those age 25-34 appears similar to that seen in Table A.3. For those under age 25 a one percentage point increase in the share of workers over age 58 is expected to decrease the share of new hires in this age group by about 0.3171 percentage points, an approximately 6.50% decrease at the mean. If an establishment were to experience a one standard deviation increase in the share of these older workers the share of new hires under 25 would be expected to decrease by over 48% at the mean. This is substantially larger than what was seen when all age groups were pooled. Contrary to the results in Table A.3, the share of new hires age 45-54, 55-64, and over 65 is expected to increase when there is an increase in the share of workers age 58 and older in an establishment. The largest positive effect in terms of percentage points is on the new hires 55-64, a one percentage point increase in the share of workers age 58 and older is expected to increase the share of new hires age 45-54 by approximately 0.0562 percentage points, which corresponds to an over 3% increase in the share of new hires of this age at the mean. When looking at the percent change at the mean the largest positive impact is seen for the age 55-64 hires which is expected to increase by over 7% at the mean for a one percentage point increase in the share of workers age 58 and over. Again, if the share of workers age 58 and older were to increase by one percentage point the share of new hires age 55-64 would be expected to increase by over 50% at the mean.

These positive estimates may be unexpected, but they are multiple potential explanations for them. One potential explanation is, with working lives extended, establishments may be more willing to hire more senior workers as they have a longer period over which to recuperate the expenses of training their new employees. These positive point estimates on the hiring of more senior employees may have more macro level implications. As was shown in Gruber and Wise (2010), this 1992 reform was not found to increase youth unemployment. Yet here at the micro level I find that increases in the share of workers over age 58 at an establishment leads to a decrease in the share of new younger workers hired and an increase in the share of new older workers hired. These two factors together may imply that there is a polarization in the average workforce age of establishments at the economy level. In other words, firms with more older workers are getting older on average both by retaining more senior workers and by hiring more new more senior employees. Yet, since there is evidence of no increase in youth unemployment, that leads to the hypothesis that younger workers are being hired by other firms which have fewer senior workers, leading to the average age at those establishments to decrease. If this hypothesis is true it may have long run impacts for the firms that are increasing in their average age.

1.5.2 Heterogeneity by Contract Type

Lastly, to better understand if there is additional underlying heterogeneity I investigate if there are differences by contract type. Similarly to the age group analysis I construct four new dependent variables: share of new regular hires, share of trainee hires, share of partial retirement hires, and share of casual worker hires. The IV results of the estimation of 1.1 are presented in Table A.5. A pattern consistent with that seen in Table A.4 emerges here. An increase in the share of workers age 58 and older leads to decreases in the share of new regular workers and trainees hired. Notably, the decrease in the share of new trainee hires is over a 7% decrease at the mean for a one percentage point increase in the share of workers over age 58 and over a 55% decrease at the mean for a one standard deviation increase in the share of workers over age 58. This could have drastic impacts on establishments as trainees are a much more prevalent part of the German economy than they are in other countries such as the United States. In contrast, there is an increase in the share of new partial retirees and casual workers hired for an increase in the share of workers over age 58, albeit the latter is not statistically significant. For partial retirement workers a one percentage point increase in the share of workers over age 58 leads to an over 10% increase in the share of these workers hired at the mean. This is a sizable increase and is consistent with the finding of Berg et al. (2020a).

1.6 Conclusion

In this paper I use linked employer-employee micro data from Germany to study the impact of an increase in pensionable age on the hiring decisions of establishments. Using a 1992 pension reform as a source of exogenous variation, I find that establishments decrease the share of their employees who are new hired when there is a an increase in the share of workers over age 58. This is contrary to what is found when studying this reform at a macro level. However, this finding is not surprising when considering that additional senior workers may impose additional costs on firms and/or firms may not have hiring needs with workers delaying retirement.

When looking at heterogeneity by age group I find that the negative result found in the pooled age group result is also found for the under 25 age group, the 25-34 age group, and the 35-44 age group, however the estimate for the last group is not precisely estimated. The largest decrease is seen in the share of new hires under age 25, who for a one percentage point increase in the share of workers age 58 and older have a decrease of about 6.50% at the mean. In contrast, the share of new workers 45-54, 55-64, and over age 65 are estimated to increase when the share of workers age 58 and older at the establishment increases. These findings are significant for establishments and the economy as a whole as they point to a potential polarization of the labor force, with firms with more senior workers actually increasing the share of their workforce that is more senior. This polarization may have long run effects, for example, if there are skill complementarities between younger and more senior workers the polarization could lead these firms to be less efficient.

Finally, I study if there is underlying heterogeneity in the pooled results when looking at hiring by contract type. The results found here support those found when studying heterogeneity by age group. An increase in the share of workers age 58 and older is expected to decrease the share of new hires who are regular workers, and trainees, with the latter being comprised mostly of the youngest workers in the labor force. Whereas, an increase in the share of workers age 58 and older is expected to increase the share of new hires who are subject to partial retirement and who are classified as casual workers, the former who are the more senior workers in the labor force.

CHAPTER 2

EFFECT OF POSTPONED RETIREMENTS ON THE WAGE GROWTH OF YOUNGER WORKERS

2.1 Introduction

Populations around the world are aging and causing countries with pay-as-you-go pension systems to consider how they will remain solvent in the coming decades. These concerns have led many countries, including the United States, to increase their full retirement age. Debate over future increases continues. For example, in the U.S., the Simpson-Bowles plan included a proposal to raise the Social Security retirement age to 69 by 2075 (Horney et al., 2010). Even absent policy action, increasing longevity, uncertain healthcare and long-term care costs, and increased prevalence of defined contribution retirement accounts may all prolong working life. This paper investigates how later retirements affect earnings growth of younger employees.

The political debate over increases in pensionable age often includes concerns over harm to younger workers. A survey by Willis Towers Watson found that 33% of employers say that older workers staying in their jobs could block promotion opportunities for younger employees (Willis Towers Watson, 2018). Similarly, according to a poll from The Associated Press-NORC Center for Public Affairs Research, 47% of individuals under age 50 who have at least some college education say older persons postponing retirement is bad for employees in general, and 31% of those under 50 say it is bad for their career specifically (NORC, 2019). To date, most economic research does not provide empirical support for these concerns.

Economists have argued that the logic behind negative spillovers of postponed retirement to younger workers' employment opportunities may be flawed to the extent it assumes the supply of jobs is "fixed" (or at least approximately so). Economic theory predicts this may be false for the economy as a whole, and several older empirical studies support this prediction (Gruber and Wise, 2010; Brugiavini and Peracchi, 2003; Jousten et al., 2010; Munnell and Wu, 2012; Kalwij et al., 2010). The literature goes so far as to call presumptions of negative spillovers "the lump of labor fallacy" (Walker, 2007). Yet most of these empirical studies relied on macroeconomic data or labor market surveys, while the popular narrative operates on a far more granular level - within firms.

A small but growing literature examines spillovers of pension reform to younger workers' employment opportunities within local labor markets and firms. Bertoni and Brunello (2017), using data from Italian labor markets, indicate reductions in youth employment as the share of workers over 50 in the local labor market rises due to an increase in pensionable age. Using a similar approach to study spillovers within commuting zones in the U.S., Mohnen (2019) finds an increase in low-skill but a decrease in high-skill employment among younger workers. Exploiting within-firm variation, Nakazawa (2020) uncovers reductions in employment of younger workers after a Japanese reform raised mandatory retirement age. Bianchi et al. (2019) examine within-firm spillovers using Italian data and a 2011 pension reform, showing evidence of slower wage growth and promotion for younger workers. Studying the same Italian reform, Carta et al. (2020) show that postponed retirements are associated with increases in employment of young and middle-aged workers, but Bovini and Paradisi (2019a) find the opposite. Recent evidence from the Netherlands also indicates postponed retirements reduce employment of younger workers within-firm and point to cash constraints as a potential mechanism (Hut, 2019). Finally, Meier (2018) examines colleagues' wages and employment after older workers retire, using Austrian data, and conclude retirements are associated with reductions in employment but increases in wages. Fueled by new sources of administrative data that can capture within-firm spillovers, these studies yield mixed results. Thus, the question of how younger workers are affected by postponed retirements remains open. Answering this question will only grow in importance as the pressure to further increase pensionable age intensifies.

In this paper, we examine whether postponsed retirements have detrimental effects on the wage progression of coworkers. Using German matched employer-employee data, we investigate potential spillovers of postponed retirements to earnings of younger colleagues after a pension reform that raised pensionable ages. We uncover little evidence of negative wage effects of postponed retirements on younger colleagues. Instead, we observe higher probabilities of large year-over-year increases in earnings in response to postponed retirements, especially among 51 to 57 year olds. Using proxy measures, we also find lower probabilities of promotion to managerial roles, as well as higher rates of employment separations. Yet, there is no systematic pattern of departures from firms by age or tenure that would be consistent with seniority-based layoffs. Instead, our results are consistent with skill complementarity, between older and younger workers, or with compensatory wage increases to those earlier in their careers for the postponement of promotions. We find the latter mechanism more plausible because the reform appears to have disproportionately expanded labor supply among less educated older workers and our estimates do not reflect higher wage gains among more educated workers. In fact, the most educated workers are the only group for which the point estimates suggest postponed retirements result in higher probabilities of wage losses. In total, we do not find postponed retirements harm younger workers' wage growth, and they may help.

2.2 Data

This study uses the Linked-Employer-Employee Data from the German Institute for Employment Research [cross-sectional model 2 1993-2014 (LIAB QM2 9314)], which matches administrative employment records to establishment survey information (Fischer et al., 2009). Our study is restricted to years 1993-2010, which encompasses 18 post-reform years. To measure pre-reform establishment age distributions, we received establishment level demographic data containing age distributions in 1990 by sex for each establishment included in the LIAB QM2 at any time from 1993 to 2010.[1] In total, we have data for 8,594 establishments, employing 2,927,326 workers for a total of over 10.5 million person-year observations. The sample excludes individuals working in establishments that did not exist in 1990, all East German establishments, part-time employees, marginal workers, and trainees. We also exclude establishments with fewer than 5 workers to avoid

[1]Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access. We thank Andreas Ganzer for support in sampling and de-identifying the 1990 demographic data.

extreme variation in the share of workers over age 58. Appendix Section B.1 describes the study inclusion criteria in detail and compares the individuals and establishments in our sample to the original (before exclusions) LIAB sample.

We define pre-retirement aged employees as employees aged 25 to 57. We exclude workers younger than 25 because many are still completing vocational education. As we will explain, age 58 was a common early retirement age during our study period (Knuth and Kalina, 2002). So, increasing pensionable ages should increase labor supply beyond age 58. For each person in the data we observe their daily wage, occupation, sex, year of birth, and educational attainment (Klosterhuber et al., 2016). We topcode earnings for people whose earnings are above the social security threshold (approximately 4% of the person-year observations) because earnings above the threshold are not consistently reported. To compute wage growth, we require at least two consecutive years of data. Shorter employment spells will be missed, and we do not observe wages for employees who leave the establishment (unless they are coincidentally employed in another sampled establishment). This limitation could lead to attrition bias if firms layoff employees who would have otherwise received smaller wage gains or a reduction in wages, or if these employees voluntarily leave the firm. To investigate this possibility, we also estimate the effects of postponed retirements on separation rates in an effort to investigate potential mechanisms behind our main findings.

2.2.1 Measuring Wage Growth

To measure wage growth among younger workers we compute the percentage change in real (inflation adjusted) wages, $wagegrowth_{ijt}$, for worker i in establishment j, with the previous year's wages as the denominator.

$$wagegrowth_{ijt} = 100 * \frac{wage_{ijt} - wage_{ijt-1}}{wage_{ijt-1}}. \tag{2.1}$$

Though straightforward to compute, there could be important effects at the extremes of the wage growth distribution this measure would miss. Large wage cuts and wage gains occur somewhat infrequently among workers ages 25 to 57. The interquartile range runs from wage losses of -1.4 percent to growth of 3.6 percent. Yet, large losses and gains are frequent enough to be potentially interesting. Wage losses of 5 percent or greater occur for nearly 10 percent of workers each year, and another 10 percent of workers experience wage gains of more than 8 percent. These amounts generally do not include one-time bonuses or overtime pay so they are likely to reflect important changes, not only for the current year's earnings, but for lifecycle earnings trajectories, too. Therefore, even if there is only a small effect on average wage growth, heterogeneous effects in the tails could be important.

To allow the effects of postponed retirement on the probability of wage losses and gains to vary across the wage growth distribution, we construct dummy variables for the 5th through 95th quantiles in steps of 5. These variables are equal to 1 for workers with wage growth equal to or above the quantile. In the left tail of the distribution, which represents wage losses, a value of 1 means the worker experienced a loss smaller than that threshold, or a gain. Consequently, in regressions, positive coefficients indicate that an increase in the share of older workers raises the probability of wage growth (or reduces the likelihood of losses) while negative coefficients suggest that wage losses or gains smaller than the threshold are more likely.

When we stratify the sample by age group or other worker characteristics, we continue to use the quantile thresholds from the full sample so that results are comparable. Importantly, we omit post-retirement aged employees when computing quantiles because large changes in labor supply for this group may influence the wage distribution (Steiner, 2017), and we compute quantiles across wages over the entire sample period (1993 to 2010) rather than allowing quantiles to adjust over time because we use deflated wages. We attempt to account for business cycle effects using year fixed effects.

2.3 Empirical Strategy

We are interested in estimating the causal relationships between the establishment's employment of post-retirement age workers, $share58pls_{jt}$ and wage growth among younger colleagues,

$wagegrowth_{ijt}$.

$$wagegrowth_{ijt} = \beta_0 + \beta_1 share58pls_{jt} + \theta X_{ijt} + \epsilon_{ijt} \tag{2.2}$$

There are many reasons why $share58pls_{jt}$ in Equation 2.2 above could be endogenously determined, leading to bias in β_1. Employers may act to influence retirement timing and employees may select into firms that offer early retirement or long-term contracts based on their preferences. For example, firms that use deferred compensation contracts may actively manage retirement by offering financial inducements, like defined benefit pensions, to retire at or before the break-even point (Lazear, 1983; Sundaresan and Zapatero, 1997). In settings where mandatory retirement is permitted, employers can (and empirical evidence indicates they do) manage retirements through dismissal (Lazear, 1979; Ashenfelter and Card, 2002; Warman and Worswick, 2010; Rabaté, 2019). In the U.S., where health insurance is tied to employment, prior studies show employer offers of health insurance can influence retirement timing (Marton and Woodbury, 2013; Nyce et al., 2013; Shoven and Slavov, 2014). A large literature examining worker responses to private pension incentives finds patterns in retirement timing consistent with such incentives (Brown, 2013; Manoli and Weber, 2016; Coile, 2015; Blundell et al., 2016), though the responses may be muted by imperfect knowledge of pension incentives (Kim, 2020). Therefore, it is reasonable to assume that firms actively, albeit imperfectly, manage retirements and that the demographics of an establishment workforce at any point in time are non-random. Though there is some information in the establishment survey that could capture some employer actions to manage workforce demographics, these questions are not fielded consistently enough during the study period to include in the vector of control variables X_{ijt}, and even if they were, they would not include tacit incentives or coercion to alter retirement timing.

To obtain plausibly causal estimates of β_1, we use a unique phased increase in pensionable ages that created incentives to postpone retirement, which varied by sex and birth cohort. The 1992 German pension reform gradually increased pensionable age from 60 to 65 beginning with the 1937 birth cohort, as depicted in Figure A.1 (Börsch-Supan and Wilke, 2004).[2] As mentioned above, 58 was a popular pre-reform early retirement age, but as shown in Figure A.1, the earliest age at which workers could claim old age pension benefits was 60 (720 months). Age 58 was popular because unemployed workers could receive unemployment insurance payments for up to two years, and unemployed workers were eligible for old age pensions at age 60 without actuarial adjustment or penalty. Men who were not unemployed or disabled would otherwise need to wait until age 65 (or 63 if they had at least 45 years of contributions). Unemployed older workers were exempted from job search requirements and other administrative rules aimed at encouraging reemployment. Unemployment spells of 18 to 24 months were still feasible after the reform, but as pensionable ages increased so did the age at which unemployment benefits would provide a “bridge" to retirement. The increase in pensionable ages differed by sex because women’s pre-reform pensionable age was 60, and the pensionable age for unemployed workers rose more quickly than women’s minimum pensionable age. The unemployment pathway to retirement remained very popular post-reform. As of 1999, the official unemployment rate among Germans ages 50 to 64 was 12 percent, the highest in the EU, and persons aged 55 to 60 comprised between 20 and 25 percent of the total unemployed population (Knuth and Kalina, 2002).

Because the 1937 cohort reached age 58 in 1995, the 1992 reform likely first began to postpone exits from employment to retirement (via unemployment) as early as 3 years after it was announced. The pensionable ages for persons of the same sex born in the same month of adjacent years differed by 6 months to 1.5 years. Between men and women born in the same month and year, pensionable ages differed by up to 3 years. When aggregated up to the establishment level, differences in the pre-reform age and sex composition of the workforce likely lead to variation in reform-induced retirement patterns. In the labor market as a whole, the employment rate of workers age 60 to 65 doubled from 2000 to 2014 and the unemployment rate among persons age 54 to 60 fell by roughly 50% (Steiner, 2017). This variation provides the identification for our analysis of wage growth

[2] During our study period, the German pension system was a pay-as-you-go scheme for all private and public sector employees entitled to social security. Private savings were negligible, self-employed workers and civil servants are excluded from the pension system, which covers about 90 percent of the German workforce (Richter and Himmelreicher, 2008).

among pre-retirement age colleagues.

If, as argued above, employers actively manage the age composition of their workforce, it follows that employers would adjust strategies for managing retirement in response to the pension reform, leading to unobserved heterogeneity in the impact of the policy. If so, then simply using pre-post reform variation or producing reduced form estimates based on contemporaneous measures of the share of workers eligible to retire may not address the endogenous determination of workforce demographics. For example, employers aiming to preserve their pre-reform demographics and retirement patterns may have offered more generous buyouts to bridge the larger gap between their planned retirement date and new date of benefit eligibility under the reform. Employers able to make buyout offers may also be able to offer better earnings trajectories to younger workers than other firms. To address this and other potential sources of endogeneity, we construct instruments using the 1990 (pre-reform) sex and age composition of each establishment.

Our instrument is based on the following thought experiment: What would have happened to the establishment's demographics if the establishment experienced the same post-reform rates of hiring (entry) and of retirement, layoffs, and other turnover (exit) by age and sex as all other establishments in its industry? This allows for industry differences in employment patterns but not for heterogenous responses across employers within the same industry. We estimate entry and exit rates by industry, age and sex, over the study period and use these estimates to "age" each establishment's 1990 workforce forward.[3] From this counterfactual age distribution, we compute the share of workers "in the gap" between the pre- and post-reform pensionable ages in each post reform year (i.e between the triangles and the circles (for women) and squares (for men) in Figure A.1). This is analogous to a shift-share instrument where 1990 demographics comprise the shares and industry entry and exit probabilities comprise the shifts. This share of workers in the gap, $ingap_{jt}$, is used to instrument the endogenous $share58pls_{jt}$ in the following system of equations:

$$y_{ijt} = \beta_0 + \beta_1 share58pls_{jt} + I'_{ijt}\gamma_1 + E'_{jt}\gamma_2 + \tau_t + \phi_j + \epsilon_{ijt} \tag{2.3}$$

[3]We describe our method in detail in Appendix Section B.2.

$$share58pls_{jt} = \delta_0 + \delta_1 ingap_{jt} + I'_{ijt}\theta_1 + E'_{jt}\theta_2 + t_t + F_j + u_{ijt} \tag{2.4}$$

y_{ijt} represents the wage growth measures discussed above. The vector I_{ijt} contains time-varying individual level control variables including educational attainment, sex, broad occupation (10 categories), a second order polynomial of years of work experience, and age dummy variables. E_{jt} are time-varying establishment level variables including total employment, and employment inflows and outflows as a share of total employment. Both equations include year and establishment fixed effects (τ_t and ϕ_j, t_t and F_j).

2.4 Results

Table B.2 contains OLS and IV estimates of the effect of an increasing share of workers over age 58 in the establishment on year-over-year wage growth of their younger colleagues (all ages 25 to 57) as a group, and separately by age group. The first stage estimates for the IV specifications are also reported. Though we have constructed the sample to ensure a consistent set of establishments across all specifications, the first stage estimates do not match exactly across sub-samples because the distribution of workers across age groups is not necessarily equal across establishments.

OLS estimates imply a 1 percentage point increase in the share of the post-retirement aged workforce (those over age 58) is associated with a small increase in wage growth relative to the average rate (0.090 percentage points or 1.9% relative to average growth). For reference, the average employment share for workers age 58 and older was 4.7% with standard deviation of 4.1 percentage points, meaning one standard deviation increase in the post-retirement aged employment share would lead to a nearly 8% increase in wage growth relative to the mean. The IV estimate is also positive and much larger than the OLS estimate. It implies a one standard deviation increase in the share of post-retirement age workers would lead to 50% faster growth for younger colleagues. However, neither estimate is very precise. Although estimates do vary by age group, and estimates for workers age 40 to 50 are actually negative, none are precisely estimated enough to conclude there are differential or any effects of postponed retirement on average wage growth.

The wage growth distribution is heavily skewed. Though the average annual wage growth is

4.7%, median wage growth is only 1.0%. Given this skewness, the estimates of effects at the mean could be misleading or incomplete. Figure B.2 summarizes the percentage changes implied by the IV estimates in our analysis of wage growth quantiles by age group. Because the baseline probabilities at each quantile are so different, these estimates are reported as percentage changes. Since the dependent variables are binary indicators equal to 1 if wage growth exceeds the quantile threshold on the x-axis, the estimates are percent changes in the predicted probability of year-over-year wage growth greater than or equal to the quantile threshold displayed on the X-axis. The intensity or shading of each point reflects the absolute value of t statistic for the underlying regression coefficient in a two tailed test with a null hypothesis of $\beta_1 = 0$. A full tabular reporting of point estimates and standard errors is provided in Table B.3.

If an increasing share of post-retirement aged workers had a negative impact on wage growth of younger colleagues, the points reflected in Figure B.2 would be negative. Instead, all points are positive, meaning at each quantile in the wage growth distribution, an increase in the share of post-retirement aged workers increases the predicted likelihood of year-over-year wage growth at least as large as the quantile threshold. Estimates in the right tail are larger for the youngest and oldest workers, which may explain why the IV estimates in Table B.2 were positive for these groups but negative for the prime aged group. The vertical reference line separates quantiles at points of negative growth (wage losses) from positive (gains) and the estimates indicate the largest effects are concentrated in the wage gain end of the wage growth distribution. The pattern of estimates across the three age groups suggests the impact is largest for the oldest and smallest for the youngest workers, although the underlying estimates are not precise enough to conclude there is any difference.

2.4.1 Potential Mechanisms

The results just discussed indicate that that postponed retirements did not hurt wage growth of younger coworkers and instead may have increased the probability of substantial wage gains. There are several potential mechanisms that could explain this finding, with very different welfare implications. First, positive effects could reflect complementarity between post- and pre-retirement aged workers. While presumably, employers should act to manage the mix of labor inputs if such complementarities exist, it is possible the gains from retaining workers after age 58 were too small to justify employer action. This is especially likely given the generous early retirement income available before the reform that any employment incentive would have had to compete with. From this perspective, the reform could have reduced employer costs of retaining workers after age 58 and resulted in productivity benefits that contribute to larger post-reform wage gains among all workers but especially among those with skills that complement the skills of post-retirement aged workers. Second, positive effects on wage growth could be selection driven if postponed retirements increase the incidence of layoffs or voluntary quits among workers who would have otherwise experienced slower wage growth or pay cuts. In this case, the apparent wage gains may mask job losses that could in fact have caused very large income reductions that our estimates do not capture. Third, if the postponement of retirements lead to delays in promotions and if younger employees value both wages and promotion to higher positions in the job ladder, then wage gains could reflect employers compensating wage differentials to offset the slower rate of career advancement (Stern (1987), Stern (1994)). Below, we examine the empirical support for each of these potential mechanisms, applying the same identification strategy used for our main results.

If wage growth among pre-retirement colleagues occurs due to complementarity of post-retirement and pre-retirement aged workers, we should expect to see the gains differ systematically by characteristics other than age. Specifically, we would expect gains to be concentrated among workers whose skills complement the skills of workers who postponed retirement. Looking at the characteristics of post-retirement aged workers over time, we find the share of university educated colleagues over age 58 was lower from 1996 forward (the year after the first reform affected cohort turned 58) than in 1994 and fell 18% overall. Thus, if skill complementarity were driving the wage growth we find among younger workers, we would expect to see faster wage growth among university educated young workers.

In Figure B.3, we reproduce Figure B.2 stratifying by worker tenure and education instead of by age group. If complementarity between workers with different levels of experience is the mechanism behind our positive wage estimates then we would expect to find wage gains are concentrated among workers with less tenure. We do not. All estimates are positive, and many are statistically significantly different from zero (especially for those with tenure of 20 years or more), but positive effects are largest among workers with the most tenure. However, imprecision prohibits strong conclusions about differences between the two groups.

Results by education level are also not consistent with skill complementarity. Post-reform, there were more workers without university degrees over the age of 58. Instead of finding larger wage gains among university educated younger workers, this is the only group for which we find any negative point estimates. The point estimates underlying the percentage changes are not precisely estimated enough to rule out zero-effect on wage losses, and the estimated effects on wage losses are small relative to the baseline probabilities and relative to effects on gains. This is not strong evidence of detrimental effects of postponed retirements on university educated workers. The estimated wage gains are at least as large as those we find for less educated workers. So overall, this is not the pattern we would expect if complementarity were the mechanism behind our wage gain results.

To assess the role of selection and evidence of compensating wage differentials, we construct two new measures: separation and promotion. Both have limitations. Separation is inferred from end of employment notifications in the administrative employment records for each individual, but we cannot tell whether separations were voluntary or involuntary and we do not have information about where workers go after they leave the establishment [4] To measure promotions, we would ideally like to know about job ladders within the organization, but the data lack any direct measures of career progression or promotion.[5] Instead, we follow Bender et al. (2018) and construct a proxy

[4]Though it is possible to follow employees after leaving an establishment in the LIAB Longitudinal Model it is an entirely separate data product and the sample is based on establishments that participated in the establishment survey between 2009 and 2016 and persons who were employed between 2008 and 2017 (Schmidtlein et al., 2019).

[5]The data contain each worker's occupation, at a level of detail similar to 3 digit Standard Occupational Classification codes. It is comprised of 330 codes that are generally too broad to capture moves within job ladders. Later versions of the LIAB offer a more detailed 4 digit coding scheme comprised of 1,300 that expands many of the classifications to denote supervisory roles. This measure is only available starting in 2011 and has substantial missing values due to employer non-reports. Though values are imputed for earlier years, the same level of detail cannot be achieved and use

measure for managerial positions from the establishment wage distribution, defining any workers in the top 25% of the establishment's wage distribution as manager. We then estimate the marginal effect of an increase in post-retirement age workers on the likelihood of switching into a managerial position. Though also based on quantiles, this measure of managerial level promotions is distinct from the quantile analysis we present above. That analysis is based on the distribution of wage growth across the entire labor market, whereas our proxy measure for managerial positions is based on movement into the top 25% of the establishment wage distribution.

Table B.5 presents our results from the analysis of separations and promotions across sub samples stratified by age group, by education, and by tenure. Separations are relatively uncommon in our sample, occurring in 3 to 7% of annual employment spells. Though none of our separation estimates are precisely estimated enough to rule out the possibility of no association, the point estimates indicate non-trivial changes in the likelihood of separation relative to the means. However, the magnitudes are not strikingly dissimilar across subgroups. For example, they do not appear larger among shorter tenured, younger, or less educated workers who we might expect to be the most likely groups to experience layoffs.

For promotions, we find consistently negative point estimates, meaning postponed retirements reduce the likelihood of promotion. All estimates are quite small except the estimate for workers with university education. Here, the point estimate implies a one percentage point increase in the share of colleagues over age 58 reduces the likelihood of advancement into the top 25% of the establishment earnings distribution by approximately 14%.

2.5 Discussion and Conclusion

This paper investigates the effect of postponed retirements on wage growth of younger workers employed in the same establishments. Though some popular narratives posit that postponed retirements slow the advancement of younger colleagues, we find little support for this assertion. For all except the most educated workers, our IV results indicate that the probability of wage

of this code across years is cautioned (Klosterhuber et al., 2016) We have produced estimates using a four category measure of task complexity derived from these more detailed codes and they are consistent with results presented here.

losses is unaffected by an increase in the share of older workers in the establishment, and for all workers the probability of wage gains increases. Though we cannot completely rule out attrition as a mechanism, the pattern of estimates across age groups, education, and tenure are not consistent with layoffs as a major driver.

The overall pattern of results is not consistent with a skill complementarity explanation for faster wage growth. The estimates, while imprecise, suggest that wage growth in response to postponed retirements may be fastest among the oldest and most experienced workers (those with 20 or more years of tenure) and does not differ by education.

Our findings by age group do fit with a compensating wage differentials explanation. Though we cannot observe promotions directly, our proxy measure indicates postponed retirements reduced the likelihood of promotions among all workers. The estimated reductions are largest among young and prime aged workers, though are not statistically significantly different from the estimate for the oldest group. Yet, if anything, our wage gain estimates appear to indicate smaller gains for the younger two groups which would seem to contradict a compensating wage differential explanation. However, if the wage gains we observe represent permanent increases, the net present value of smaller gains among the youngest workers may still be greater than larger gains among the oldest because younger workers have more remaining years of work ahead.

Our findings are also consistent with the possibility that longer working lives increase overall firm productivity. In particular, prior to the pension reform, employers might have preferred to extend working lives but not at the price required to induce postponed retirements. The gains resulting from the reform may then have been redistributed in part to workers through higher wages, and redistributed on the basis of seniority.

Our findings of wage losses among university educated employees are somewhat puzzling. One possibility is that they simply reflect statistical noise in the estimates. Alternatively, these workers are less likely to be protected by collective agreements and may receive more of their pay in bonuses, commissions, or profit sharing which are easier to reduce than base wage rates or salaries.

The foregoing discussion points to the potential importance of institutional features that may limit the comparability of these findings to other countries, like the United States, with quite different institutions. Of particular relevance for Germany is that hierarchical labor markets are prevalent and collective bargaining is wide-spread, so much so that only 5% of our overall observations are from establishments without collective agreements.[6] Germany also has specific protections for dismissal of older workers and layoffs above certain thresholds require filing a “social plan". This administrative hurdle may make layoffs a less likely response to postponed retirements in Germany than would be the case in other countries with different labor market regulations. Conversely, unlike in the U.S, Germany’s unemployment insurance system is not experience rated. That we find no higher incidence of separations for the youngest workers in this context is notable.

During our study period, Germany introduced other policies and programs that may have affected the employment of older workers. Steiner (2017) and Dietz and Walwei (2011) provide detailed discussions of these policies and offer descriptive evidence of the concomitant trends in labor force behavior of older workers.[7] Nearly all of these policies aimed to encourage longer working lives. However some did so by offering subsidies to employers for employing older workers (Dietz and Walwei, 2011). Partial retirement may have offered employers another mechanism for adjusting to the 1992 pension reforms. Subsidies for offering partial retirement to workers over age 55 were announced in 1996. To receive the subsidy, employers must replace the reduced hours of work of a partial retiree with an unemployed worker, however research indicates many employers offered partial retirement and did not claim subsidies (Berg et al., 2020b). Also, Germany used employer subsidies to encourage employment of “difficult to place" workers during our study period, and this likely included many older workers (Dietz and Walwei, 2011). We did not attempt to disentangle the causal effect of the 1992 reform from other policies during the study period; we

[6]We did attempt to analyze the group without collective agreements separately. The point estimates indicate postponed retirements may reduce the probability of wage gains, however, given the small sample, these estimates were very imprecise.

[7]Note, there are some apparent differences in the timing of the 1992 reform according to Steiner (2017) and Dietz and Walwei (2011) and our own explanation but the source of the difference is use of the announcement date rather than the effective dates and describing the phase in by calendar year rather than by birth cohort. For example, in Table 2 Dietz and Walwei (2011) states the pensionable age for women rose stepwise from 60 to 65 between 2000 and 2004. Our own Figure A.1 shows the same information. The 1940 cohort was age 60 in 2000, and those born in January of 1944 had a pensionable age of 64 years and 1 month (769 months). By January of 2005, the 1945 cohort turned 65 and their pensionable age was 65 (780 months).

used it as a source of identification to understand the relationship between postponed retirements and younger workers' wage growth. Nonetheless, the policy context and incentives encouraging employers to retain older workers may contribute to the positive outcomes of postponed retirement we find for younger workers.

CHAPTER 3

PENSION REFORMS AND THEIR IMPLICATIONS FOR FIRM DOWNSIZING

3.1 Introduction

Workforces are aging across the globe and a majority of OECD nations are raising ages of eligibility for public pension benefits currently or will in the near future (OECD, 2011). These changes incentivize later claiming of pension benefits, making retirement at younger ages less financially attractive. The political debate over consequences of these reforms focuses mostly on the employee. However, employers are affected by these changes as well.

Delays in retirements may lead to unexpected cost shocks that firms are forced to combat. As shown in Bönke et al. (2015), the most senior workers in Germany are some of the highest paid employees. Thus, if these individuals delay retirement firms may have a larger wage bill than they had anticipated and that larger wage bill may persist for a period of time.

Changes in pensionable age are generally phased in gradually across birth cohorts so the timing and extent of the reform's impact will vary with the cohort composition of the employer's workforce. This means idiosyncratic differences in the shares of workers affected by the reform can create variance in the degree of disruption of normal retirement patterns the policy creates across employers. Employers experiencing policy impacts may be more likely to actively manage workforce aging through incentives to retire earlier (e.g. buyouts) or may counterbalance postponed retirements with layoffs of less senior employees, who are generally younger. Both will result in downsizing of the establishment workforce.

In this paper, we investigate workforce downsizing associated with pension reforms. This is one potential lever that establishments may use when facing an unexpected cost shock imposed by the increase in retirement age. We hypothesize downsizing will be more likely in establishments that experience larger impacts of pension reform, and will be more likely to impact younger workers because there are fewer legal barriers to their dismissal. Finally, we anticipate any downsizing effects may be mitigated by works councils, who are likely to advocate for cost saving strategies that preserve employment.

Prior literature clearly establishes pension reforms influence worker retirement behavior in expected directions, though the magnitude of the effects can be small in settings where private savings are a large component of overall retirement wealth (Berkel and Borsch-Supan, 2004; Atalay and Barrett, 2015; Gustman and Steinmeier, 2009; Maestas and Zissimopoulos, 2010). Postponed retirements have the potential to impact firm profitability directly through increased labor costs and indirectly through possible productivity effects. Because the acquisition of human capital is related to age and tenure within establishments, shifts in the labor force participation of older workers due to pension reform may change the composition of the workforce in ways that affect firm performance and the risk of downsizing. For example, research on firm productivity shows that firm age and human capital are key determinants of firm productivity and profitability (Vandenberghe, 2013; Audretsch and Fritscht, 1994; Barron et al., 1994; Dunne et al., 1988; Lane et al., 1999; Mahlberg et al., 2013; Schnabel and Wagner, 2012). This potential linkage is also supported by evolutionary economics, which proposes that establishments make decisions under constraints and that the strategies firms adopt vary with these constraints (Alchian, 1950). Firms adopting strategies poorly suited to the conditions of their external environment will be eliminated through competition. Changing pensionable ages represents a shift in the constraints firms face. Assuming firms had optimized their workforce size prior to a pension reform, downsizing may be a necessary strategy to counteract the incentive the reform created for postponed retirements. Therefore, establishments that are differently affected by pension reforms should adopt different strategies.

Currently there is little known about the potential effects of pension reforms on firms' labor demand. There are at least two channels through which firms could adjust their workforce composition after a pension reform takes place: hiring and downsizing. In the context of this German reform, a working paper by Eckrote-Nordland (2021) finds that establishments with larger shares of older workers reduce their hiring when pensionable age is raised, with the reduction mostly impacting young workers. A recent working paper by Bovini and Paradisi (2019b) investigates layoffs after a change in pensionable ages in Italy. They find delayed retirements increase layoffs among workers of all ages. The German context may differ from the Italian context due to the importance of works councils and additional protections in place for workers, especially more senior workers (Bhankaraully, 2019). In a related study that supports this assertion, Muñoz-Bullón and Sánchez-Bueno (2014) find downsizing among Spanish firms is associated with both labor law and behavior of industry peers.

Studying the effect of pension reform on employers is challenging because there are few data sources that contain demographic information needed to infer pensionable age for all workers across many firms. Where these data do exist, they often do not encompass a long enough time series to estimate the effects of a gradual increase in pensionable age or contain a large enough sample of firms to examine heterogeneity. Finally, the reforms themselves are often phased in so slowly that they do not create enough variation to convincingly separate employer responses from other factors.

We address these problems using over two decades of administrative data from German social security notifications encompassing a 1992 reform that raised pensionable ages by 5 years. This reform was fully phased in over a span of 19 years and 8 birth cohorts. For comparison, the increase in age of eligibility for Social Security benefits in the US from age 65 to 67 was announced in a 1983 amendment, did not begin to bind on the first affected cohorts until 2003, and included an 11 year hiatus during which the age remained constant at 66 for cohorts born in 1943 through 1954 (Social Security Administration, 2019). The full two year increase will not bind for US workers until the 1960 birth cohort reaches age 67 in 2027. So, while the relationships between pension reform and downsizing we hypothesize are also relevant in other countries, the rich administrative data available in Germany coupled with larger and more expedient changes in pensionable age provide a uniquely advantageous study setting. Our data follow a representative sample of West German establishments existing in 1990 through 2010. We find in establishments where the reform lead to a larger share of workers over the age of 58 than otherwise similar establishments, downsizing is more likely. The effects are largest for establishments without works councils. Although the percentage point changes in the probabilities of downsizing are similar across age segments of the workforce, this means the relative risk increases far more for older workers who generally have the lowest risk of downsizing.

3.2 The German Pension System and the 1992 Reform

The German pension system is designed as a pay-as-you-go scheme, providing pension benefits for all private and public sector employees entitled to social security.[1] It covers about 90 percent of the German workforce (Richter and Himmelreicher, 2008) and accounts for approximately 85 percent of retirement income (Börsch-Supan, 2000). Public pension accrual is a function of one's wages relative to countrywide average wages, years of service, and age, calculated every year. In 2005, estimates indicate that less than five percent of households headed by older workers had private pensions, despite incentives for private savings introduced in the 2001 Riester Reform (Börsch-Supan, 2000). The German pension system has historically offered a "window" of ages at which workers can begin claiming pension benefits, beginning as early as age 58 if combining early retirement with the unemployment insurance (Börsch-Supan and Wilke, 2004). Statistics show that in years 1993 to 1995, at age 60, approximately 60 percent of German men had retired and 45 percent were receiving pension benefits.

In 1992, a reform gradually increased ages of eligibility for full benefits to 65. This reform was the first in a 15 year period of pension reform in Germany (Bonin, 2009). The goal was to stop access to full pension benefits at age 60 for persons born after 1936. Figure A.1 provides a graphical overview of the minimum pensionable ages of the historical pension insurance compared to after the 1992 reform. As explained above, workers were able to effectively retire up to two years before the ages depicted in Figure A.1 if they claimed unemployment benefits to bridge the gap between the end of employment and claiming. So, raising the age of claiming above 60 should lead to an increase in labor supply at age 58 and above.

Prior research finds, as of 2004, the 1992 reform lead to a two-year increase in the average

[1] Self-employed workers and civil servants are excluded from the pension system.

retirement age among men and a nine-month increase among women (Berkel and Borsch-Supan, 2004). The 1992 reform first began to postpone claiming benefits as early as 1 year after it was announced. All changes in eligibility for full benefits were phased in between the 1937 and 1944 birth cohorts and thus were fully implemented by 2011. Overall, the 1992 reform created differences in pensionable ages of 1 to 12 months across adjacent birth cohorts and differences of 6 to 12 months for men and women within the 1940 through 1941 cohorts (Börsch-Supan and Wilke, 2004).

This outlined heterogeneity in incentives to retire among older workers creates variation in the impact of the reform across employers. Small differences in the age distribution may lead to large differences in retirement patterns. In total, the reform should lead to an increase in the share of workers who continue to work past age 58, and this increase should be largest in establishments that employ more workers from the affected cohorts, and during the later years in the phase in period. We use this heterogeneity as a source of identifying variation to estimate the impact of pension reforms on establishment survival (as outlined in Section 3.5).

3.3 Analyses Sample

We use the Linked-Employer-Employee Data (LIAB) [cross-sectional model 2 1993-2014 (LIAB QM2 9314)] from the Institute for Employment Research which is provided for academic research use.[2] The LIAB matches administrative employment records to establishment survey information. Baseline for the sampling is the IAB Establishment Panel (IABBP), which collects data on about 15,500 establishments per year (Fischer et al., 2009). In the LIAB QM2 9314 all individuals who work in these surveyed establishments on June 30th in each year are sampled. For each of these workers we know their employment state on June 30th, as well as a rich set of variables describing the employment characteristics, including wages, detailed occupations, and industry. Socio-demographic variables including sex, age and education attainment are included as well (Klosterhuber et al., 2016). Having all the workers in each establishment in a given year allows

[2] Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access.

us to aggregate individual information on an establishment level and describe the entire workforce in an establishment. These administrative records can be combined with information from the IABBP, which allows us to add information on establishments legal entities, personnel policies and operating strategies to the analyses.

The LIAB data begin in 1993, which is after the 1992 reform was introduced. Thus, to construct a measure of policy impact that is exogenous to any policy response, we need data that includes pre-policy information. We use a custom extract from the Employment History data (BeH) provided by the FDZ[1], which contains age distributions by gender for each establishment in the LIAB QM2 9314 that existed in 1990. Thus, our analytic sample is based on all establishments which are part of the LIAB QM2 9314, but also existed in 1990. This restricts our sample to West German establishments, because data on East German establishments is lacking prior to 1993. Overall, we can follow 74,985 establishments during the time period from 1993 to 2010. However, this is an unbalanced sample (cross sectional yearly sample) as we only have an observation for the years the establishments participated in the survey.

3.4 Indicators of Establishment Downsizing

We are measuring downsizing by relying on the wide literature of displacement studies. There are different ways that this strand of literature constructs measures, such as layoffs, outflow, turnover, etc (Lengermann and Vilhuber, 2002; Jacobson et al., 1993; Dustmann and Meghir, 2005; Bowlus and Vilhuber, 2002; Abowd et al., 2009). This paper relies on establishment wide and age specific net employment following Flaaen et al. (2017).

We measure overall downsizing on the establishment level as follows, whereas the downsizing rate D in year t at establishment j is defined as the count of total workers (EMP(t)) in establishment j at the end of June in year t, divided by the number of total workers (EMP(t+1)) in establishment j at the end of June in the following year. Because small changes in the workforce in small establishments can result in large percentage changes in employment, we require the total workforce

[1]We thank Andreas Ganzer for sampling the data for us and supporting us with de-identification of the data.

in an establishment in year t has to be at least 50 workers to be included in the analysis.

$$D_{jt} = \frac{EMP(t)_{jt}}{EMP(t+1)_{jt}}$$

As for our main definition, we also define an age specific downsizing indicator for the alternative measure for younger, prime and older workers:

$$D_{jta} = \frac{EMP(t)_{jta}}{EMP(t+1)_{jta}}$$

The downsizing rate D in year t of workers in age group a at establishment j is defined as the count of total workers (EMP(t) in age group a in establishment j at the end of June in year t, divided by the number of total workers (EMP(t+1)) in age group a in establishment j in the following year. The total workforce in an establishment in year t has to be at least 50 workers in order to be able to experience downsizing.

Conceptually, these measures reflect a segmentation of establishment employees by age and allow us to capture cases where one age segment experienced a large outflow of workers relative to their age group's total employment, but the firm as a whole may not appear to have downsized because that segment's share of total employment is relatively small.

After calculating D_{jt}, respectively, D_{jta} we construct outcomes to study the effect of pension reform at different thresholds of downsizing: 10%, 20%, and 30%. In each of the cases the outcome y is a binary variable which takes on the value 1 if the downsizing measure (D_{jt} or D_{jta}) is higher than the thresholds, and is zero otherwise. We end up with 12 different outcome variables using the different thresholds and age groups.

3.5 Empirical Strategy

3.5.1 Estimation of Reform Impact on Downsizing

We measure the impact of the reform via the share of workers 58 years and older in an establishment, as the pension reform in 1992 leads to a higher share of older workers in establishments. Using variance in the share of workers over 58 attributable to the reform, we hypothesize establishments with more workers working past the old effective retirement age of 58 will consequently show a higher risk of workforce downsizing. Thus, we construct yearly shares of employees in each establishment age 58 and older, $share58_{jt}$. We use age 58 as the threshold because, as explained, this was the earliest age in the pre-reform retirement window that workers could finance retirement through a combination of pension and unemployment benefits. The estimated OLS equation will then be

$$y_{jta} = \beta_1 share58_{jt} + \beta_2 X_{jt} + u_t + \epsilon_{jt}, \tag{3.1}$$

Our main outcomes y_{jta} are binary indicators for whether an establishment j in year t experiences a downsizing of workers in age group a of at least 10%, 20% or 30%. $share58_{jt}$ is the share of workers in establishment j who are age 58 and older in year t. X_{jt} is a vector of establishment controls including, industry, inflows, outflows, establishment size, legal form, existence of collective bargaining agreements and work councils. Furthermore, we include year fixed effects u_t.

Our estimates of β_1 in Equation 3.1 could be attenuated if our hypothesized linkage between policy induced postponement of retirements and firm financial distress is correct and some firms opt to fully shut down rather than downsize. To check for this possibility, we also model establishment closures using the same functional form as Equation 3.1 above and substituting a binary indicator of closure for the dependent variable y_{jta}.

Our estimates of β_1 in Equation 3.1 could be also be biased if the employers who have higher shares of older workers are more likely to engage in practices, like buyouts, that also impact the probability of downsizing. In the case of buyouts, if buyouts are more common among employers with more older workers (or in years when the share of older workers is high), and if buyouts of the oldest workers reduce the probability of downsizing in other age groups, then our OLS estimate of β_1 would be negatively biased. The importance and durability of firm specific human capital is another source of possible bias. Employers where firm specific human capital is important and highly durable tend to retain their older workers. They also may, by virtue of their specific human capital, have competitive advantages that reduce the likelihood of buyouts. If so, these unobserved factors would also lead to negative bias in β_1. The overall desirability of the employer to workers would also lead to negative bias. To address these potential sources of bias, we instrument the share of workers age 58 and older with what we call our ingap measure z_ingap_{jt}. It represents the number of workers in the gap between the old pensionable age and the lowest new pensionable age, based on projected workforce estimates. It is constructed following a shift share approach which is outlined in the following section.

3.5.2 Shift Share Instrument Construction

We use the 1990 BeH custom data extract containing pre-policy information to construct a shift share instrument. Shift-share instruments, sometimes called “Bartik instruments” after Bartik (1991), have been widely used in the immigration and the regional growth literature but have many other applications (Goldsmith-Pinkham et al., 2018).

We first predict differences in the shares of employees eligible to retire in each establishment, relative to industry-average shares in each post policy year, that are attributable only to the differences in pre-policy employment of cohorts affected by the reform using pre-reform employment information. Specifically, we construct counts of workers in each affected cohort by sex in each of the establishments in our analytic sample. These counts comprise the “share” portion of the instrument.

To demonstrate the relevance of these shares for predicting future workforce aging, Figure C.1 shows the correlation between 1990 shares of workers across all cohorts who will be affected by the reform by 2014 (those aged 41 to 53 in 1990) and actual shares of workers over age 58 across the subsequent years. Whereas all establishments experience a growing older workforce, we can see that establishments that employ fewer affected workers before the reform do so to a lower extent, even 24 years later. The more detailed shares we construct by sex and single year birth cohort are also strongly correlated with the employment of older workers in subsequent years.

The shifts are computed from the fitted values after estimating the following two regressions using 1993-2014 data separately for each of 11 industry sectors by sex.

$$begin_{ijt} = \beta_0 + \beta_1 age_{ijt} + \beta_2 year_t + \beta_3 age_{ijt} * year_t + \epsilon_{ijt},$$

$$end_{ijt} = \beta_0 + \beta_1 age_{ijt} + \beta_2 year_t + \beta_3 age_{ijt} * year_t + \epsilon_{ijt},$$

Where $begin_{ijt}$ is equal to 1 for employees in their first year of employment with establishment j in year t and equal to 0 for all subsequent years. end_{ijt} is equal to 1 in the last year of employment with establishment j, which is indicated when the employer files an end of employment notification. age_{ijt} is a vector of age dummy variables for ages 19 through 67 with age 18 as the omitted age group, and $year_t$ is a vector of dummy variables for years 1994 through 2014 with 1993 as the omitted year. After estimating each equation for men and for women by industry, we obtain fitted values $\widehat{begin_{ijt}}$ and $\widehat{end_{ijt}}$ at each age for each year from each equation.

We then take the averages of these fitted values for each age in each year. This yields estimates of the probabilities of being in the first year of employment with establishment j in year t conditional upon working for establishment j and of ending employment with establishment j in year t conditional upon working for establishment j in each of the 11 industry sectors for men and for women. We use these probabilities to "age" the 1990 workforce for each establishment as follows:

$$workers_{at} = workers_{a-1,t-1} * [1 - \widehat{end_{a-1,t-1}} + \widehat{begin_{at}}]$$

Where the number of workers age a in year t is equal to the number of workers at age a-1 from the prior year adjusted by the probabilities of ending employment in the prior year at age a-1 and beginning employment at age a in year t. $workers_{at}$ is computed for each age separately for men and women by industry sector.

Using $workers_{at}$, we calculate the number of workers in the gap between the old pensionable age and the lowest new pensionable age where full benefits can be claimed without disability for each establishment j in each year t. We then divide those counts by the size of the establishment workforce in 1990. The resulting z_ingap_{jt} is our instrument.

3.5.3 Validity of the Instrument

Recent studies raise concerns about the validity of these instruments. First, to meet the exclusion restriction, the initial shares used to construct the instruments must be exogenous. In our setting, this means the shares of employees in each establishment who are affected by the change in pensionable age must be exogenous to future survival probabilities of the establishment. To ensure this, we measure the shares of employees in affected cohorts before the policy is announced.

Second, there must be sufficient variation in initial shares to ensure the instruments for units receiving the same shift will be different. In our setting, this means the distributions of workers from different birth cohorts and of different sexes within the same cohort must vary across establishments in the same industry. Figure A.3 provides visual proof of sufficient variation of our instrument.

Panel a) shows our projected measure puts most establishments between 0 and 20 percent of workers in the gap between the old and new pensionable age. Next, Panel b) plots the 25th, 50th and 75th percentiles of the in gap distribution across all establishments by year. Both figures indicate there is substantial variation in the in gap measure across establishments, and over time. The between establishment standard deviation in the in gap measure from 1996 forward is 2.79 percentage points and the within is 4.26 percentage points.

Overall, these statistics are convincing that we have a valid instrument to deal with the endogeneity introduced in the OLS estimates. Thus, we use these predicted retirement eligibilities to instrument our contemporaneous policy measure, using two-stage least square models. Estimates of OLS and 2SLS are discussed in Section 3.7.

3.6 Descriptive Results

This section provides a descriptive overview on establishment downsizing and sample statistics. Our sample period comprises the years 1993 to 2010. Starting in 1993 we can follow establishments and see how they progress. As shown in Table C.4 we observe at a minimum 2,691 (in 1998) and at a maximum 5,821 establishments (in 2001) in our sample. The sample sizes differ each year as our sample is a non balanced cross section and not every establishment participates in the survey every year. Some establishments are joining the panel later when the IABBP did refresher samples due to panel attrition. This explains certain jumps in specific years. However, establishments are only in our sample if they existed in 1990. Table C.4 also shows that the average establishment size decreases each year, from 730 employees in 1993 to 265 employees in 2010. This is also due to the structure of the IABBP. Large establishments were over sampled when the survey started in 1993. We account for this in the regression by adding all the sample strata as control variables. As expected we can see that the share of workers 58 and older rises on average every year. Whereas in 1993, on average 5% of the workforce in our sample was 58 and older, in 2010 this number increased to 14%.

Figure C.2 shows a Kaplan Meier Survival probability for the establishments in our sample to get an impression how many establishments are experiencing a downsizing event. For illustration purposes we choose to display the probability of downsizing at the 30% threshold. For this purpose we balanced our panel by computing the number of years until the downsizing event happens starting from 1990. We can see that over the sample period more than half of the observed establishments experience a downsizing event of at least 30% of the workforce. Downsizing happens more frequently within the first five years in our observation window. Looking at the sub sample of establishments that experienced a downsizing event at the 30% threshold we can see in Figure C.3 that most of these establishments experience this event about 6 years into our study period, which is when the pension reform started hitting the first worker cohorts.

3.7 Main Results

Table C.6 displays the results of the first stage for our IV estimates. As the endogenous regressor, $share58$, and control variables are the same across all regressions and the sample of establishments is consistent across specifications, the first stage estimate is identical for all results reported. The estimated coefficient for the instrument z_ingap_{jt} has the expected positive sign and implies a one percentage point increase in the projected share of workers in the gap the reform created between the old and new pensionable ages is associated with a 0.18 percentage point increase in the share of the establishment's workforce age 58 or older.

Table C.5 contains the estimated effects of policy induced workforce aging on the likelihood of downsizing events overall, and by age group. Ignoring possible heterogeneity across age groups, overall our IV estimates indicate establishments with a higher share of workers over the age of 58 are more likely to experience downsizing at each of the thresholds we considered. Specifically, a one percentage point increase in the share of workers over age 58 is associated with a 0.3 percentage point increase in the likelihood of a downsizing event involving 10 percent or more of the establishment's workforce (Table C.5 - All Workers - column (1)). The OLS estimates, are negative, which is consistent with the expected bias discussed above.

When we segment establishment workforces by age and consider separations within subpopulations, the IV estimates again consistently reveal positive associations between policy induced workforce aging and the likelihood of downsizing within each demographic segment at each threshold considered. As different age groups have different baseline probabilities of downsizing, we report the coefficient estimates alongside the percentage change they imply relative to the baseline probability for each age group.

The coefficient estimates are highest for the youngest age group at all three thresholds. However, because downsizing events are most common in the younger segment of the workforce and least common among the oldest, in some cases these estimates imply a larger percentage change in the probability of downsizing among the oldest workers, here for the 10% and 30% threshold. For example, the impact of a 1 percentage point increase in the share of workers over age 58 is associated with a nearly 7 percent increase in the likelihood of downsizing events involving 10 percent or more of the older workers in an establishment, whereas we observe an slightly over 5 percent increase in the younger worker segment. For downsizing events involving 20 percent or more of the age group, both the percentage point change in the likelihoods and the percentage changes relative to baseline probabilities are larger for the younger rather than the older workers.

The reported estimates could be attenuated if establishments with higher shares of workers over age 58 close down during our study period. To address this, we estimate the same regression models using a closure indicator as outcome. The IV and OLS results, as well as the First Stage estimate are presented in the Appendix in Table C.3 and Table C.2 respectively. We find no significant associations between the share of workers over age 58 and the probability of closure, and the point estimate for the full sample is negative.

3.8 What is Driving the Main Results?

To better understand the role of the institutional mechanisms that may influence the downsizing events we are studying, we separate our establishment sample into two groups: establishments with and without works councils. These absolute results are summarized graphically in Figure C.4, whereas the percentage change in the probability is illustrated in Figure C.5

These results reveal that the positive relationships between policy induced workforce aging and downsizing events of 10% or greater in the prime-aged segment of the workforce shown in Table C.5 was driven entirely by the establishments without works councils. For prime workers, works councils appear to mitigate the effects of workforce aging on downsizing probabilities, and they may reduce the likelihood of downsizing events involving over 30% of the workforce for all age segments.

In all establishments, we find the largest percentage increases in downsizing probabilities occur in the oldest segment of the workforce, workers aged 50 and above at the 10% and 30% threshold. This is notable because many of these workers are in the earliest cohorts affected by the pension reform and had the least amount of time to adjust to the increase in their pensionable ages. In establishments with works councils, it appears older workers are the only segment of the workforce that experiences a statistically significantly higher likelihood of downsizing as the share of the workforce over the old effective retirement age increases. Among prime aged workers in establishments with works councils, the likelihood of downsizing appears to be decreasing in the share of workers over age 58. This relationship is not simply mechanical. Our measures of downsizing within each age segment are relative to the number of workers within that age segment, not total establishment employment. These negative relationships could be the result of works council efforts to protect employment of prime aged workers in the negotiation of a social plan to facilitate downsizing as required under German law.

We note the results presented in this section should be interpreted with caution because works council information is missing for some establishments in our sample, and is not likely missing at random.

3.9 Conclusion

This paper finds positive associations between workforce aging and the likelihood of downsizing events. Using an increase in pensionable age as a source of exogenous variation in establishments' employment of workers over age 58 (the old effective retirement age), we find a one percentage point increase in the share of workers aged 58 and older is associated with as much as a 10 percent increase in the likelihood of downsizing events. Also, this impact appears to vary across age segments within establishments, and differs between establishments with and without works councils. Whereas, the coefficient estimates are highest for the youngest age group at all three thresholds, in some cases the estimates however imply a larger percentage change in the probability of downsizing among the oldest workers. Our results suggest that work councils seem to take on a protective function for prime age workers.

The welfare implications of our findings for older workers are unclear. On the one hand, older workers appear to bear both the burden of a shorter planning horizon in which to adjust to increases in pensionable age under the law, and a greater risk of employment separation due to the postponed retirement incentives the law created. If, however, the downsizing we observe in this segment is primarily the result of buyouts that include compensation, it is possible older workers are able to achieve retirements on a similar timeline as was feasible before the reform with little or no loss of retirement wealth. If instead, the downsizing we observe reflects layoffs, older workers may be reliant upon public income support programs which were becoming less generous over this period. Unfortunately our data do not contain information about buyout payments and we cannot empirically investigate these possibilities.

Our current analysis is limited in its ability to convey the importance of the downsizing events for the labor force as a whole because we measure downsizing relative to the size of the establishment workforce and our estimates are unweighted. This means that an establishment with 100 employees and an establishment with 10,000 employees that each layoff 10 percent of their workers are treated the same, yet these events have very different implications for the well-being of the population and for social programs that support displaced workers. In future iterations of this work, we intend to produce weighted estimates to account for differences in establishment size.

APPENDICES

APPENDIX A

Figure A.1: Gap between the pre- and post-reform effective pensionable ages by cohort and sex

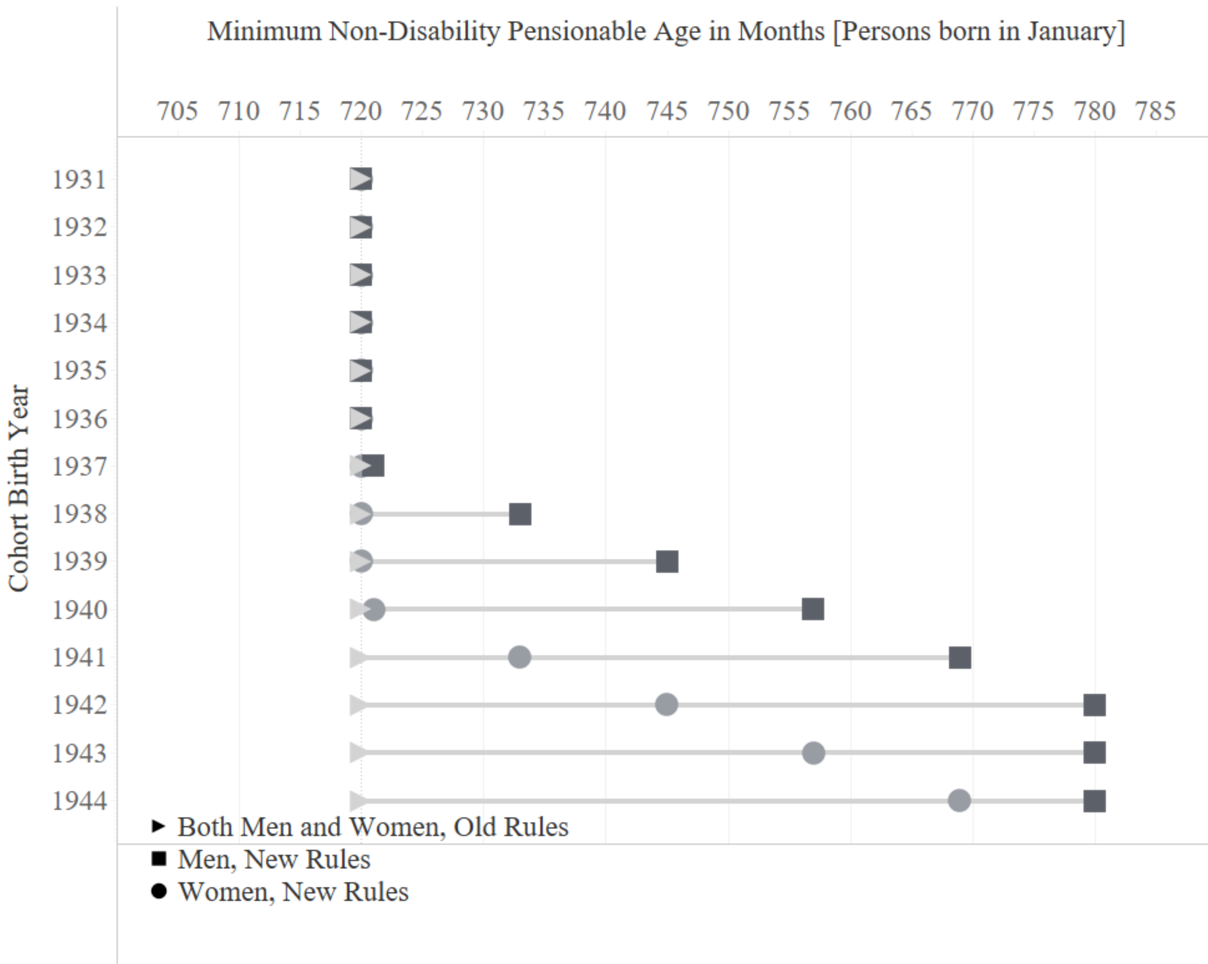


Figure A.2: Distribution of Employment Share Ratios by Industry, 1938-45 Cohort

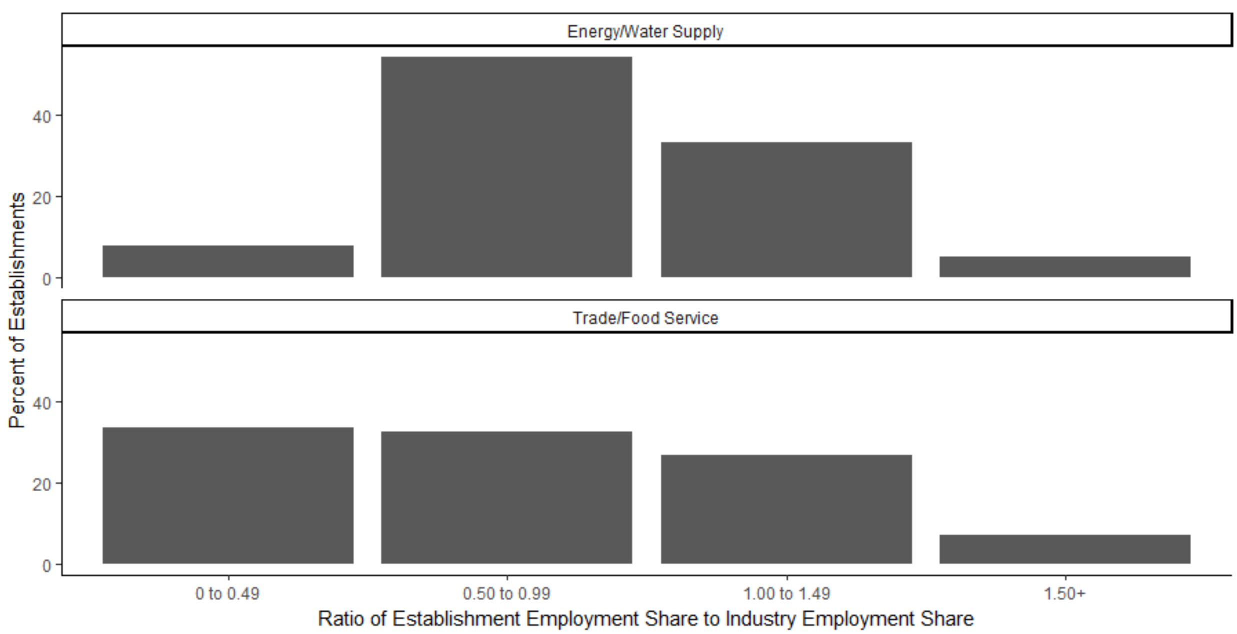
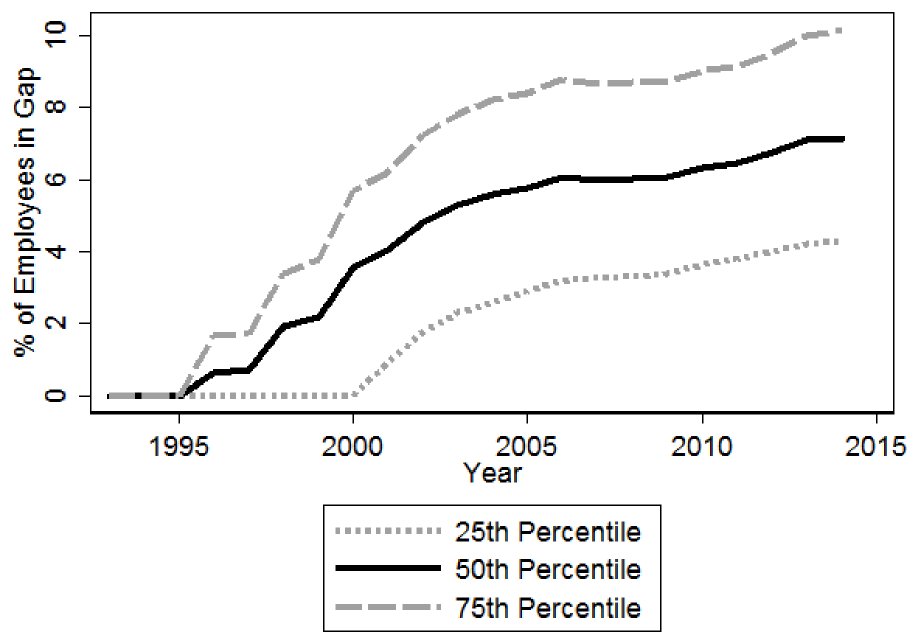


Figure A.3: Instrument Validity

(a) Distribution of ingap variable pooled across time periods



(b) Distribution of ingap variable over time

Table A.1: Summary Statistics

Variable	Mean	Standard Deviation	Min	Max
Share 58+	8.83	7.44	0	100
Firm Size	396	1382	5	55987
Share Female	43.73	27.84	0	100
Share Part-Time	22.37	22.15	0	100
Collective Agreement	76.30	42.41	0	100
Works Council	61.15	49.57	0	100
Establishment-Year Observations: 68,407				
Number of Unique Establishments: 14,723				

Table A.2: Shift-Share IV Identification Regressions

Characteristic	β_1
Share Female	-0.5564^{***}
	(0.0521)
Share Medium Skill	0.0152
	(0.0465)
Share High Skill	0.1610^{***}
	(0.0300)
Total Wage Bill	836.797^{***}
	(171.197)
Firm Size	6.9748^{***}
	(1.5017)
Share Part-Time	-0.0385
	(0.0428)
Legal Form	0.0325^{**}
	(0.0148)
Works Council	-0.0423^{***}
	(0.0129)
Collective Agreement	0.0126^{**}
	(0.0059)
Berlin	0.0019^{***}
	(0.00045)

Heteroskedasticity robust standard errors clustered at the establishment level (N Clusters = 14,723) are reported with each point estimate. The unit of observation is the establishment-year. One star, two stars, and three stars denote statistical significance at the 10-, 5-, and 1-percent confidence levels, respectively.

Table A.3: All Hires

	OLS	IV
$share58+$	-0.4023^{***}	-0.3243^{***}
	(0.0117)	(0.0401)
Share Hires Mean	14.99	
% change at the mean	-2.68%	-2.16%

Heteroskedasticity robust standard errors clustered at the establishment level (N Clusters = 14,723) are reported with each point estimate. The unit of observation is the establishment-year. Point estimates are interpreted as percentage point changes in the share of new hires. In addition, each regression includes a set of establishment characteristics: workforce demographics, wage bill, size, existence of a work council and any industry agreements. Furthermore year fixed effects are included. The instrumental variable regressions are estimated by two-stage least squares with a first-stage F-statistic of 90.99. One star, two stars, and three stars denote statistical significance at the 10-, 5-, and 1-percent confidence levels, respectively.

Table A.4: Age Group Hires: IV

	Under 25	25-34	35-44	45-54	55-64	65+
$share58+$	- 0.3171***	-0.1110***	- 0.0167	0.0562***	0.0534***	0.0109***
	(0.0219)	(0.0174)	(0.0132)	(0.0112)	(0.0080)	(0.0042)
Share Hires Mean	4.88	4.18	3.18	1.78	0.75	0.22
% change at the mean	-6.50%	-2.66%	-0.53%	3.16%	7.12%	4.95%

Heteroskedasticity robust standard errors clustered at the establishment level (N Clusters = 14,723) are reported with each point estimate. The unit of observation is the establishment-year. Point estimates are interpreted as percentage point changes in the share of new hires in the given age group. In addition, each regression includes a set of establishment characteristics: workforce demographics, wage bill, size, existence of a work council and any industry agreements. Furthermore year fixed effects are included. The instrumental variable regressions are estimated by two-stage least squares with a first-stage F-statistic of 90.99. One star, two stars, and three stars denote statistical significance at the 10-, 5-, and 1-percent confidence levels, respectively.

Table A.5: Contract Type

	Regular Workers	Trainees	Partial Retirement	Casual Workers
$share58+$	-0.1038***	-0.1686***	0.0023***	0.0003
	(0.0328)	(0.0148)	(0.0006)	(0.0006)
Mean	10.285	2.27	0.022	0.0047
% change	-1.01%	-7.43%	10.45%	6.38%

Heteroskedasticity robust standard errors clustered at the establishment level (N Clusters = 14,723) are reported with each point estimate. The unit of observation is the establishment-year. Point estimates are interpreted as percentage point changes in the share of new hires for the given contract type. In addition, each regression includes a set of establishment characteristics: workforce demographics, wage bill, size, existence of a work council and any industry agreements. Furthermore year fixed effects are included. The instrumental variable regressions are estimated by two-stage least squares with a first-stage F-statistic of 90.99. One star, two stars, and three stars denote statistical significance at the 10-, 5-, and 1-percent confidence levels, respectively.

APPENDIX B

B.1 Sample Construction

Our analysis focuses on workers age 25 to 58 employed in West German establishments as regular (not marginal) employees and who are subject to social security. We eliminate East German establishments due to incomplete employment records in the early part of our study period. These inclusion criteria yield a potential sample of 20,932,671 person-year spells for 5,737,222 individuals working in 25,194 establishments.

From there, we eliminate establishments with fewer than 5 workers. This results in a loss of 234,851 person-year observations and 1,933 establishments. We also drop any spells for which the daily wage falls below €10 to further ensure all nonstandard employment is excluded, but we retain spells with wages above the threshold for persons who ever have wages below the threshold and these individuals will show up in our analysis of separations. This results in a loss of 92,081 person-year observations and only 13 establishments. We drop part-time workers, resulting in a loss of 2,650,881 person-year spells, 721,825 individuals, and 293 establishments.

Our identification strategy requires information about pre-reform establishment demographics. Any establishment that did not exist in the 1990 establishment history panel must be excluded due to missing values for our instrument. This results in a loss of 2,162,354 person-year observations, 680,892 individuals, and 8,830 establishments.

To compute wage growth, we require two adjacent years of employment. Eliminating first spells and other spells with missing wage growth data leads to a loss of 4,382,963 person-year observations, 1,145,044 individuals and 3,090 establishments. These losses are primarily due to inconsistent participation in the establishment panel.

Finally, to ensure the estimates by age group are based on a consistent sample of establishments, we eliminate any establishments that do not contain workers in all three age groups. This results in a loss of 849,798 person-year observations, 156,385 individuals, and 2,441 establishments. Our final sample includes 10,559,743 person-year spells, 2,927,326 individuals, and 8,594 establishments.

To assess the implications of these sample inclusion and exclusion decisions, we provide descriptive statistics for our final sample and the unrestricted sample (including marginal workers and East German establishments) in Table B.1. Despite including just over 20% of the full sample of establishments, our sample is quite similar to the full sample and where differences exist they are generally as expected. Our sample has more stable employment inflows and outflows, which is expected for older and larger establishments (the average establishment in our sample is over twice the size of the average establishment in the full sample). Average and median worker ages are very similar, and within the 25 to 57 age range, the full sample has only slightly more in the youngest and slightly fewer in the oldest age group. So, despite selecting a systematically older sample of establishments, the age composition of the workforce is quite similar across samples. Among workers over age 58, the share with university education and the share in the top 25% of the establishment wage distribution are both similar across samples.

Our sample contains a higher share of manufacturing and public service establishments and fewer construction and wholesale trade establishments. We have far fewer individually owner firms and more corporations. Not surprisingly given these differences, establishments in our sample are also far more likely to have a works council and more likely to have a collective agreement.

Table B.1: Characteristics of Workers and Establishments: With and Without Sample Restrictions

	Our Sample	**Unrestricted Sample**
Establishment Characterstics		
Employment Inflows/Total Employment	0.132	0.206
	(0.099)	(0.199)
Employment Outflows/Total Employment	0.166	0.265
	(0.388)	(1.976)
		Continued on next page

Table B.1 – (cont'd)

	Our Sample	**Unrestricted Sample**
Total Employment	413.908	188.221
	(1,297.697)	(718.994)
Median Worker Age	41.954	40.205
	(3.671)	(6.742)
Average Worker Age	41.845	40.382
	(2.675)	(5.483)
Industry 1	0.87%	1.68%
Industry 2	32.45%	25.89%
Industry 3	1.65%	1.06%
Industry 4	6.16%	9.20%
Industry 5	14.24%	17.77%
Industry 6	4.17%	5.06%
Industry 7	5.75%	2.89%
Industry 8	5.34%	9.77%
Industry 9	24.04%	19.01%
Industry 10	3.92%	4.93%
Industry 11	1.41%	2.35%
Individually Owned Firm	4.75%	16.76%
Partnership	7.02%	6.11%
Limited Partnership	47.50%	51.10%
Capital Corporation	9.25%	5.29%
Public Corporation	21.70%	12.04%
Other	8.18%	6.75%
Have a Works Council	68.06%	40.82%

Continued on next page

Table B.1 – (cont'd)

	Our Sample	**Unrestricted Sample**
Have an Industry Collective Agreement	62.17%	49.39%
Have a Company Collective Agreement	7.75%	8.37%
No Collective Agreement	19.92%	39.69%
Pre-Retirement Age Distribution		
Share Age 25 to 39	0.404	0.446
	(0.137)	(0.195)
Share Age 40 to 50	0.388	0.372
	(0.102)	(0.146)
Share Age 51 to 57	0.207	0.183
	(0.093)	(0.127)
Post-Retirement Age Worker Characteristics		
Share Managers	0.520	0.492
(Top 25% of earners)	(0.250)	(0.320)
Share University Educated	0.070	0.088
	(0.142)	0.196)
Characteristics of Person-Year Spells		
Blossfeld 1	0.46%	0.93%
Blossfeld 2	23.64%	18.42%
Blossfeld 3	16.86%	14.49%
Blossfeld 4	8.76%	6.84%
Blossfeld 5	4.40%	6.84%
Blossfeld 6	10.40%	11.41%
Blossfeld 7	2.17%	3.02%
Blossfeld 8	4.74%	8.23%

Continued on next page

Table B.1 – (cont'd)

	Our Sample	**Unrestricted Sample**
Blossfeld 9	1.68%	2.49%
Blossfeld 10	3.94%	5.13%
Blossfeld 11	20.37%	21.57%
Blossfeld 12	2.44%	2.65%
Blossfeld 13	0.14%	1.19%

B.2 Construction of Instruments

To construct our instruments, we first estimate rates of entry and exit from employment by age, sex and industry from 1993 through 2014 for each of 11 industry sectors as follows:

$$begin_{ijt} = \beta_0 + \beta_1 age_{ijt} + \beta_2 year_t + \beta_3 age_{ijt} * year_t + \epsilon_{ijt},$$

$$end_{ijt} = \beta_0 + \beta_1 age_{ijt} + \beta_2 year_t + \beta_3 age_{ijt} * year_t + \epsilon_{ijt},$$

Where i indexes individuals, j indexes industry, and t indexes year. $begin_{ijt}$ is equal to 1 for employees in their first year of employment with establishment j in year t and equal to 0 for all subsequent years. end_{ijt} is equal to 1 in the last year of employment with establishment j, which is indicated when the employer files an end of employment notification. age_{ijt} is a vector of age dummy variables for ages 19 through 67 with age 18 as the omitted age group, and $year_t$ is a vector of dummy variables for years 1994 through 2014 with 1993 as the omitted year.

Using Equations B.2 and B.2 we predict exit and entry at each single year of age, a, by sex, s, by industry, j, in each year, t as $\widehat{begin}_{asjt}$ and $\widehat{end}_{asjt}$.[1] We then calculate the net rate at each age (by sex, industry, and year) as:

[1]More precisely, these are estimated probabilities of being in the first year of employment with establishment j in year t conditional upon working for establishment j, and of ending employment with establishment j in year t conditional upon working for establishment j.

$$net_{asjt} = 1 + \widehat{begin_{asjt}} - \widehat{end_{asjt}}$$

We then merge the net_{asjt} matrix by age, sex, and industry with the establishment age distribution information we have in 1990. The unit of analysis in the 1990 file is establishment. It contains a separate variable containing the counts of workers for each single year of age from 18 to 67 for men and for women (and variables containing the total number of workers under age 18, and the total over age 67). This file does not contain information about the establishment's industry. We extract and merge this information from the LIAB panel. For establishments that change industry, we use the modal industry to determine which entry and exit rates to apply. We then multiply the counts of workers at each age by sex in 1990 by the corresponding $net_{asj,1993}$ to create estimated counts in 1994. These counts are then multiplied by $net_{asj,1994}$. We continue aging the population forward one year at a time through the year 2010 to create the "counterfactual" age distribution. Then using this age distribution, we calculate the share of each establishment's workforce that is in the gap between age 58 (the pre-reform early retirement age) and the post-reform age for their birth cohort and sex based on this distribution. This is our instrument, $ingap_{jt}$. Figure B.1 plots the distribution of the actual share of workers age 58 and older and the instrument.

Figure B.1: Kernel Density Plot of Actual Share Age 58 and Above and Instrument (ingap)

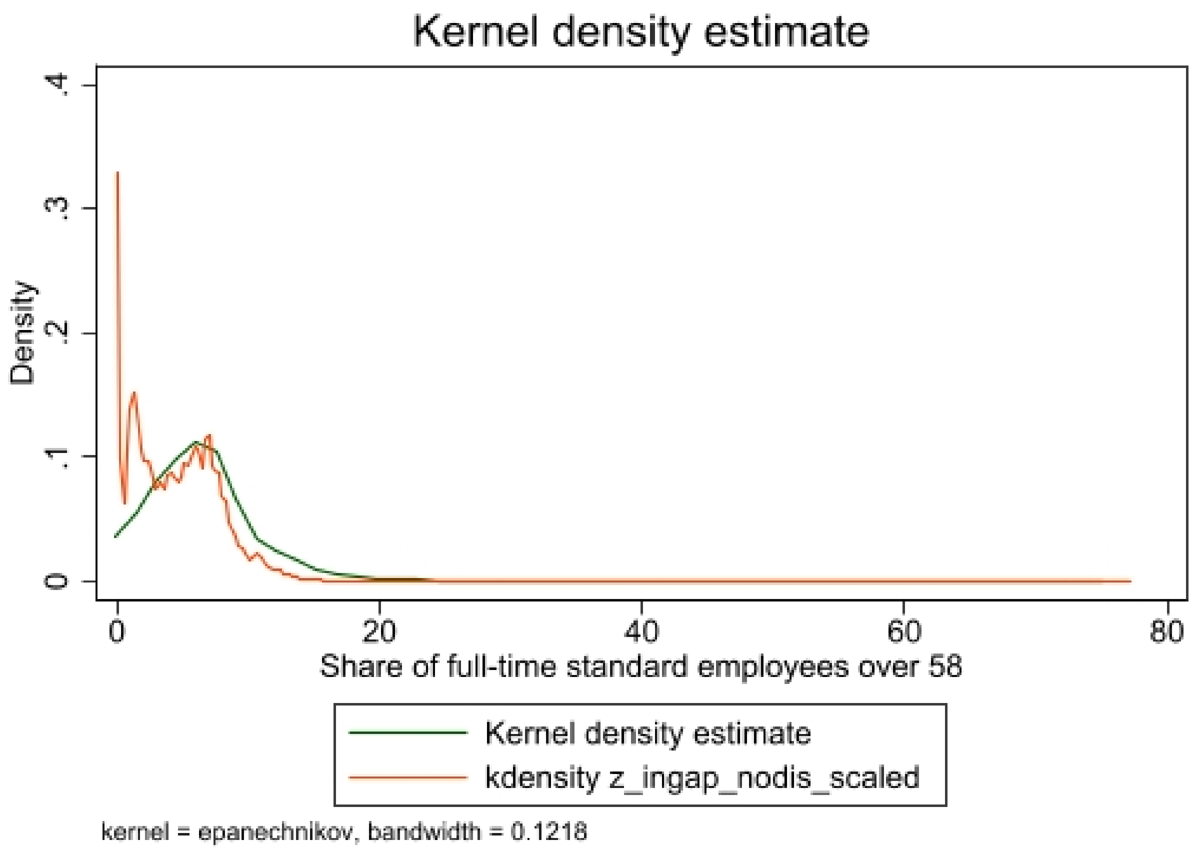
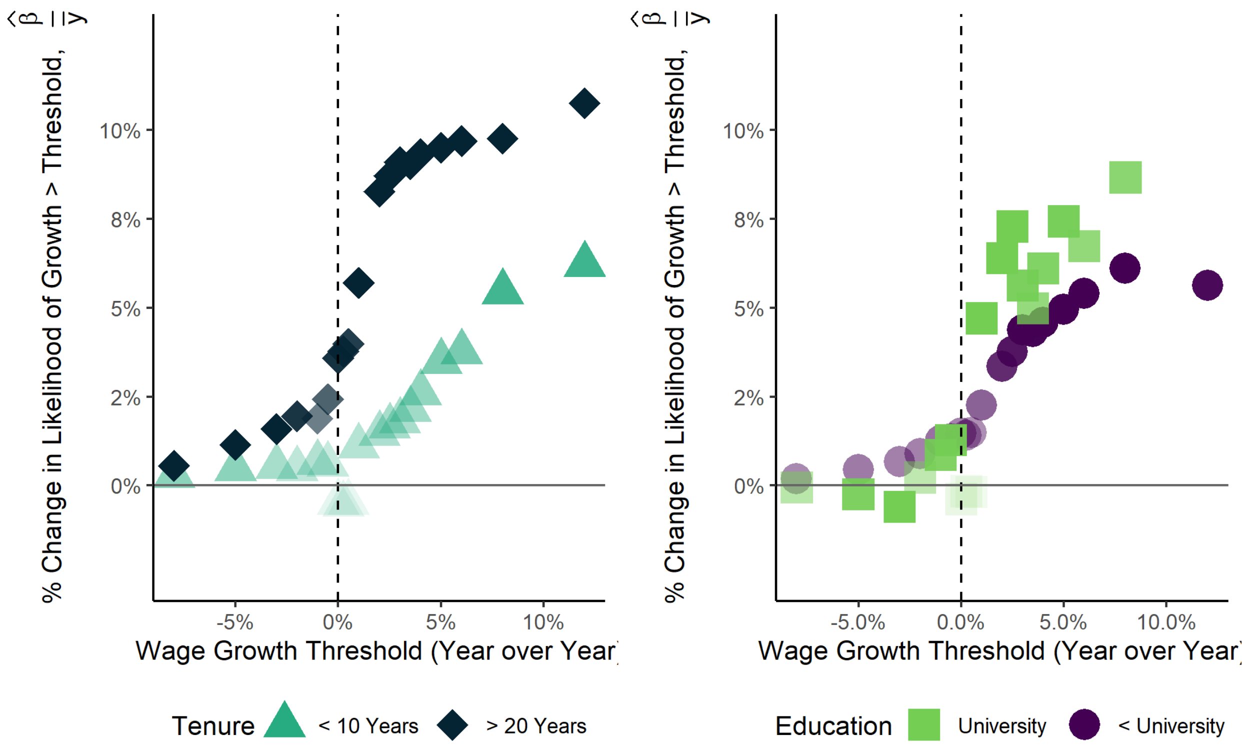


Figure B.2: Graphical Summary of Quantile Results

Hue reflects precision of point estimates in a two tail test with $H_0 : \beta = 0$. A full tabular reporting of point estimates with standard errors is provided in Table B.3.

Figure B.3: Graphical Summary of Quantile Results by Tenure and Education



Hue reflects precision of point estimates in a two tail test with $H_0 : \beta = 0$. A full tabular reporting of point estimates with standard errors is provided in Table B.4.

Table B.2: Effects on Year-Over-Year Wage Growth

	OLS	IV	First Stage
Estimate	0.090	0.586	0.144
Std. Error	(0.154)	(1.049)	(0.023)
N	10,559,743	10,559,743	10,559,743
1st Stage F			39.15
Mean DV	4.656%	4.656%	6.321%
Std.Dev. X	4.073	4.073	3.687
% Change	1.933%	12.586%	1.804%
Young: Age 25 to 39			
Estimate	0.448	1.578	0.138
Std. Error	(0.334)	(3.264)	(0.027)
N	4,599,709	4,599,709	4,559,709
1st Stage F			25.40
Mean DV	6.131%	6.131%	5.775%
Std.Dev. X	5.775	5.775	3.553
% Change	7.307%	25.738%	2.390%
Prime: Age 40 to 50			
Share Age 58+	-0.002	-0.084	0.141
Std. Error	(0.131)	(0.557)	(0.024)
N	4,028,155	4,028,155	4,028,155
1st Stage F			35.90
Mean DV	3.571%	3.571%	6.564%
Std.Dev. X	4.144	4.144	3.662
% Change	-5.601%	-2.352%	2.148%
Senior: Age 51 to 57			
Share Age 58+	-0.230	0.755	0.151
Std. Error	(0.271)	(0.836)	(0.017)
N	1,931,879	1,931,879	1,931,879
1st Stage F			82.04
Mean DV	3.410%	3.410%	7.115%
Std.Dev. X	4.382	4.382	3.995
% Change	-6.745%	22.141%	2.122%

Heteroskedasticity robust standard errors clustered at the establishment level (N Clusters = 8,594) are reported with each point estimate. Point estimates are interpreted as percentage point changes in the year-over-year change in daily wages relative to the prior year's wage. Std. Dev X for OLS and IV is the standard deviation of the endogenous variable share of workers over age 58 and for the first stage is the standard deviation of the instrument. Percentage changes are relative to dependent variable means. Average wage growth is 1.49%, with standard deviation of 7.92 percentage points. First Stage F Statistics are Kleibergen-Paap.

Table B.3: Wage Bin Results

	Pooled		Young		Prime		Senior	
Cutoff-Point	OLS	IV	OLS	IV	OLS	IV	OLS	IV
-8	0.0023**	0.00141	0.00288	0.00168	0.00213**	0.001	0.00176**	0.00216
	(0.00113)	(0.00313)	(0.00176)	(0.00406)	(0.000878)	(0.00345)	(0.000747)	(0.00318)
-5	0.00283**	0.00352	0.00328*	0.00274	0.00276***	0.00355	0.00248***	0.00499
	(0.00122)	(0.00642)	(0.00184)	(0.00697)	(0.000992)	(0.00688)	(0.00088)	(0.00618)
-3	0.00336***	0.00475	0.00351*	0.00264	0.00358***	0.00473	0.00317***	0.00845
	(0.0013)	(0.00946)	(0.00181)	(0.00954)	(0.00117)	(0.0101)	(0.00105)	(0.00888)
-2	0.0034**	0.00572	0.00366**	0.00222	0.00365***	0.00536	0.00325***	0.013
	(0.00133)	(0.0123)	(0.00179)	(0.0117)	(0.00122)	(0.0133)	(0.00118)	(0.0111)
-1	0.00352**	0.00675	0.00364**	0.00289	0.00399***	0.00677	0.00355**	0.0157
	(0.00138)	(0.0141)	(0.00173)	(0.0135)	(0.00134)	(0.0153)	(0.00138)	(0.0121)
-0.5	0.00265*	0.0061	0.00305*	0.00201	0.00309**	0.00815	0.00232	0.017
	(0.0015)	(0.0154)	(0.00179)	(0.0145)	(0.00154)	(0.0158)	(0.0015)	(0.0121)
0	0.000763	0.00421	0.0015	-0.00199	0.000935	0.00983	0.000241	0.0192
	(0.00157)	(0.0182)	(0.00183)	(0.0171)	(0.00161)	(0.0182)	(0.00149)	(0.0133)

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Table B.3 cont.

	Pooled		Young		Prime		Senior	
Cutoff-Point	OLS	IV	OLS	IV	OLS	IV	OLS	IV
0.25	0.000821	0.00364	0.00133	-0.00195	0.00104	0.00966	0.000635	0.0186
	(0.00157)	(0.0189)	(0.00183)	(0.0176)	(0.00161)	(0.0184)	(0.00145)	(0.0146)
0.5	0.000849	0.00373	0.00126	-0.00147	0.00108	0.00972	0.00078	0.0184
	(0.00157)	(0.0192)	(0.00181)	(0.0178)	(0.00161)	(0.0188)	(0.00145)	(0.0149)
1	0.00241	0.0103	0.0023	0.00639	0.00285*	0.0159	0.00257*	0.0226**
	(0.00154)	(0.015)	(0.00174)	(0.0142)	(0.0016)	(0.0142)	(0.00145)	(0.0112)
2	0.00231	0.0123	0.00193	0.00955	0.00307*	0.0179	0.00256*	0.0238***
	(0.00159)	(0.013)	(0.00174)	(0.0133)	(0.00168)	(0.0124)	(0.00151)	(0.00796)
2.5	0.00201	0.0127	0.00169	0.0106	0.00277	0.0183	0.00213	0.0221***
	(0.00161)	(0.0119)	(0.00176)	(0.0129)	(0.00171)	(0.0114)	(0.00148)	(0.00698)
3	0.00183	0.0135	0.00156	0.0117	0.00256	0.0181*	0.00172	0.0175**
	(0.00162)	(0.0106)	(0.00181)	(0.0123)	(0.0017)	(0.0106)	(0.00146)	(0.00704)
3.5	0.00166	0.012	0.00135	0.0118	0.00236	0.0162	0.0017	0.0141**
	(0.00158)	(0.01)	(0.0018)	(0.0117)	(0.00165)	(0.00997)	(0.00136)	(0.00672)
4	0.0015	0.0117	0.00119	0.0123	0.00223	0.0153	0.00152	0.0134**

Continued on next page

Table B.3 cont.

Cutoff-Point	Pooled OLS	Pooled IV	Young OLS	Young IV	Prime OLS	Prime IV	Senior OLS	Senior IV
	(0.00155)	(0.00933)	(0.00178)	(0.0113)	(0.00161)	(0.00933)	(0.0013)	(0.00619)
5	0.00125	0.0107	0.000859	0.0127	0.00204	0.0134*	0.00133	0.0113**
	(0.00147)	(0.00804)	(0.00173)	(0.0101)	(0.00151)	(0.00811)	(0.00119)	(0.00562)
6	0.00102	0.00967	0.000584	0.0124	0.00178	0.0116	0.0012	0.00956*
	(0.00136)	(0.0072)	(0.00162)	(0.00914)	(0.00138)	(0.00724)	(0.00108)	(0.00552)
8	0.000664	0.00794	0.000158	0.012*	0.00134	0.0079	0.00105	0.00779
	(0.00117)	(0.00579)	(0.00143)	(0.00718)	(0.00118)	(0.00589)	(0.000924)	(0.00505)
12	0.000298	0.00437	-0.000483	0.00669	0.000972	0.00429	0.000941	0.00509*
	(0.000852)	(0.00346)	(0.00103)	(0.00453)	(0.000875)	(0.00356)	(0.0007)	(0.00285)

Heteroskedasticity robust standard errors clustered at the establishment level (N Clusters = 8,594) are reported with each point estimate.The unit of observation is the individual-year Point estimates are interpreted as percentage point changes in the probability of receiving a wage increase of at least the cutoff percent. In addition, each regression includes a set of establishment characteristics: workforce demographics, wage bill, size, existence of a work council and any industry agreements. Furthermore year fixed effects are included. The instrumental variable regressions are estimated by two-stage least squares. One star, two stars, and three stars denote statistical significance at the 10-, 5-, and 1-percent levels, respectively.

Table B.4: Mechanisms: IV estimates

	Tenure		Education	
Cutoff-Point	<10 Years	>20 Years	< University	University
-8	0.00275	0.00537**	0.00174	-0.000574
	(0.00325)	(0.00222)	(0.00331)	(0.000998)
-5	0.00444	0.010**5	0.00403	-0.00238
	(0.00527)	(0.00476)	(0.00672)	(0.00211)
-3	0.00467	0.0137*	0.00562	-0.00577
	(0.00705)	(0.00774)	(0.00983)	(0.00376)
-2	0.00395	0.0158	0.0072	0.00203
	(0.0079)	(0.0112)	(0.0127)	(0.00386)
-1	0.00512	0.0138	0.00923	0.00723
	(0.00876)	(0.015)	(0.0142)	(0.00654)
-0.5	0.0045	0.0162	0.00889	0.00975
	(0.0102)	(0.0147)	(0.0152)	(0.00836)
0	-0.00336	0.0211	0.00918	-0.00254
	(0.0143)	(0.0147)	(0.0164)	(0.0185)
0.25	-0.00364	0.0212	0.00847	-0.00104
	(0.0149)	(0.0154)	(0.0171)	(0.0185)
0.5	-0.00315	0.0213	0.00858	-0.00117
	(0.0154)	(0.0157)	(0.0174)	(0.0189)
1	0.0067	0.0244**	0.0114	0.0205**
	(0.0119)	(0.0123)	(0.0156)	(0.00832)
2	0.00754	0.0264***	0.0136	0.0209**
	(0.0121)	(0.00926)	(0.0134)	(0.00998)

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Table B.4 cont.

	Tenure		Education	
Cutoff-Point	<10 Years	>20 Years	< University	University
2.5	0.00797	0.0248***	0.0138	0.022**
	(0.012)	(0.00843)	(0.0123)	(0.0102)
3	0.00787	0.0232***	0.0142	0.0128
	(0.0122)	(0.00787)	(0.0111)	(0.0112)
3.5	0.0085	0.0205***	0.0126	0.0104
	(0.0121)	(0.00739)	(0.0104)	(0.0114)
4	0.00953	0.019***	0.0121	0.0119
	(0.012)	(0.00705)	(0.00964)	(0.0109)
5	0.0108	0.0158**	0.0107	0.0127
	(0.0113)	(0.00664)	(0.00827)	(0.0107)
6	0.00986	0.0133**	0.00966	0.0102
	(0.0101)	(0.00648)	(0.00734)	(0.0109)
8	0.0104	0.0094	0.00759	0.0105
	(0.00772)	(0.00591)	(0.00575)	(0.0104)
12	0.00661	0.00538*	0.0036	0.0109
	(0.00483)	(0.00319)	(0.00305)	(0.00904)

Heteroskedasticity robust standard errors clustered at the establishment level are reported with each point estimate.The unit of observation is the individual-year Point estimates are interpreted as percentage point changes in the probability of receiving a wage increase of at least the cutoff percent. In addition, each regression includes a set of establishment characteristics: workforce demographics, wage bill, size, existence of a work council and any industry agreements. Furthermore year fixed effects are included. The instrumental variable regressions are estimated by two-stage least squares. One star, two stars, and three stars denote statistical significance at the 10-, 5-, and 1-percent levels, respectively.

Table B.5: Effects on Promotions and Separations

	Separations	Promotions
Young: Age 25 to 39, N = 4,599,709		
Estimate	0.005	-0.002
Std. Error	(0.004)	(0.002)
Mean DV	0.052	0.068
% Change	9.612%	-2.941%
Prime: Age 40 to 50, N = 4,028,155		
Share Age 58+	0.004	-0.001
Std. Error	(0.003)	(0.001)
Mean DV	0.033	0.042
% Change	12.121%	-2.381%
Senior: Age 51 to 57, N = 1,931,879		
Share Age 58+	0.005	-0.001
Std. Error	(0.003)	(0.001)
Mean DV	0.049	0.035
% Change	10.204%	-2.857%
< University, N = 9,181,655		
Share Age 58+	0.003	-0.002
Std. Error	(0.003)	(0.001)
Mean DV	0.041	0.056
% Change	7.317%	-3.571%
University, N = 1,377,668		
Share Age 58+	0.008	-0.004
Std. Error	(0.006)	(0.002)
Mean DV	0.067	0.028
% Change	11.940%	-14.286%
Tenure $\leq$ 10 *Years, N = 2,560,404*		
Share Age 58+	0.005	-0.003
Std. Error	(0.004)	(0.003)
Mean DV	0.067	0.075
% Change	7.463%	-4.000%
Tenure $\geq$ 20 *Years, N = 3,630,430*		
Share Age 58+	0.003	-0.001
Std. Error	(0.002)	(0.001)
Mean DV	0.034	0.041
% Change	8.824%	-2.439%

Standard errors, clustered at the establishment level, are in parentheses. The unit of observation is the establishment-year. Each regression includes a general set of controls: location, share of younger and share of older people at workplace, and a marker to indicate missing years in the layoff calculation. In addition, each regression includes a set of establishment characteristics: workforce demographics, wage bill, size, existence of a work council and any industry agreements. Furthermore year fixed effects are included. The instrumental variable regressions are estimated by two-stage least squares. One star, two stars, and three stars denote statistical significance at the 10-, 5-, and 1-percent confidence levels, respectively. Baseline represents the mean of share 58.

APPENDIX C

Addressing Sample Attrition

To make sure that our main downsizing estimates are not biased through attrition of establishments in our panel because they completely go out of business we also produce a set of estimates on how workforce aging induced by pension reform relates to the death of an establishment. In a non biased sample we would expect the coefficients to not be statistically significant. To test this hypotheses we construct a closure indicator from the administrative establishment data. These contain an indicator for each establishment telling denoting the kind of closure that happened in case an establishment closed down. The different closures are outlined in Table C.1. We see that out of the establishments that close a bit more than 60% experience a death closure.

Table C.1: Construction of Closure Indicator

Label	absolute	percent
ID Change	2,121	7.98
Take-over	1,563	5.88
Spin-off	3,286	12.36
Small Death	2,640	9.93
Atomized Death	7,042	26.49
Chunky Death	8,467	31.85
Reason unclear	1,462	5.50

Thus, we define an establishment to be closed in a given year if we observe one of the three categories of death (small, atomized, chunky). These categories represent firm closures to differing degrees of impact (for detailed information on how this was constructed please see Hethey-Maier and Schmieder (2013)). Thus, the closure outcome for the regressions shown below takes on the value 1 if an establishment experiences a closure in year t and 0 otherwise. We estimate the effect of workforce aging on closure in the full sample and stratified by works councils. Table C.2 shows the First Stage estimates of all the models, whereas Table C.3 displays the IV and OLS results. As expected we find no significant effects.

Table C.2: First Stage: Shift-Share Instrument (on share 58)

	Coef.	Std. Error	N
Full Sample	0.184***	0.016	74,985
Employer has Work council	0.150***	0.022	39,770
Employer has no work council	0.230***	0.021	28,930

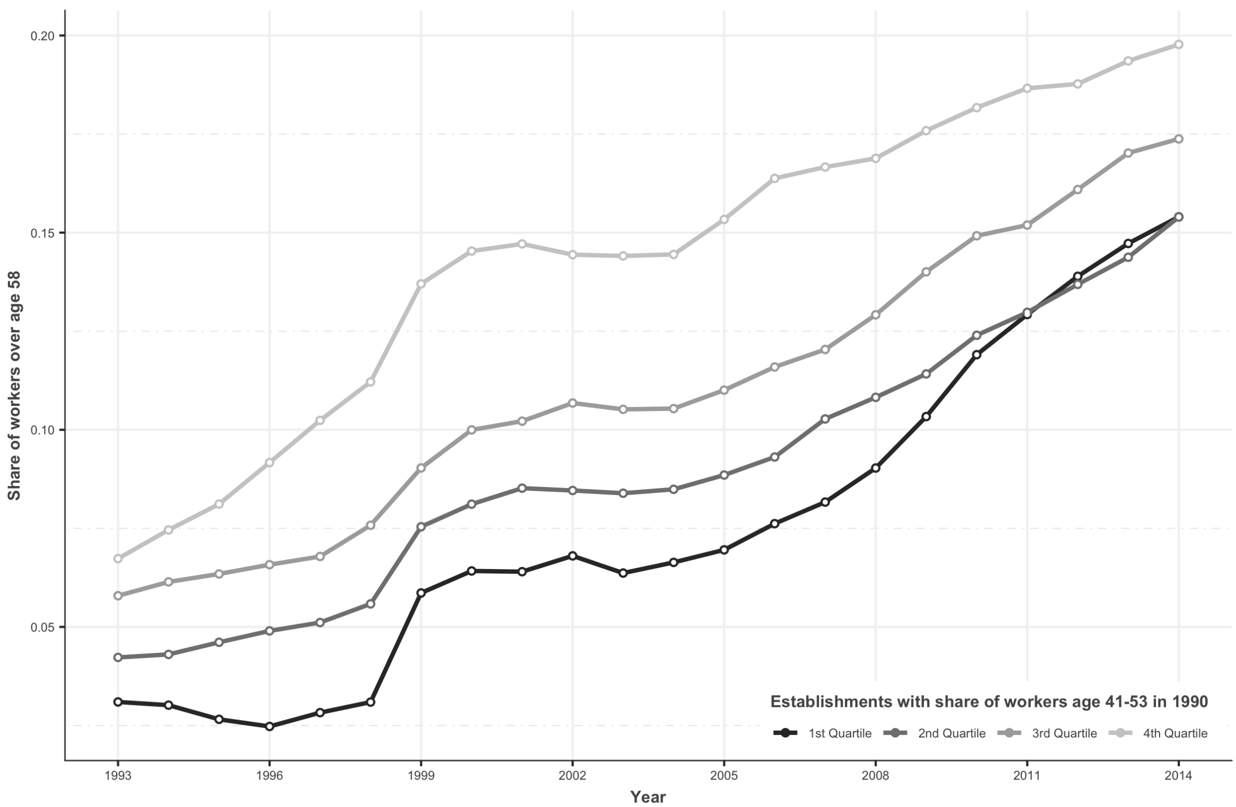
The unit of observation is the establishment-year. Standard errors are clustered at the establishment level. Each regression includes a set of establishment characteristics (workforce demographics, wage bill, size, flows, existence of a work council and any industry agreements, location) and year dummies as controls. Three stars denote statistical significance at the 1-percent confidence level.

Table C.3: Closures, different subsamples, establishment controls

	Full Sample		Works Council		No Works Council	
	(1)	(2)	(3)	(4)	(5)	(6)
Model	IV	OLS	IV	OLS	IV	OLS
Share 58	-0.000	0.000	-0.001	-0.000	0.001	0.000
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
%	-0.008	0.017	-0.104	-0.012	0.041	0.013
baseline	0.009		0.005		0.015	
N	74,985	74,985	39,770	39,770	28,930	28,930

Standard errors, clustered at the establishment level, are in parentheses. The unit of observation is the establishment-year. Each regression includes a general set of controls: location, share of younger and share of older people at workplace, and a marker to indicate missing years in the layoff calculation. In addition, each regression includes a set of establishment characteristics: workforce demographics, wage bill, and size. Furthermore year fixed effects are included. The instrumental variable regressions are estimated by two-stage least squares. One star, two stars, and three stars denote statistical significance at the 10-, 5-, and 1-percent confidence levels, respectively. Baseline represents the mean of share 58.

Figure C.1: Share of Workers over Age 58



Source: Authors' calculations

Figure C.2: Survival rate of Downsizing at the 30% threshold

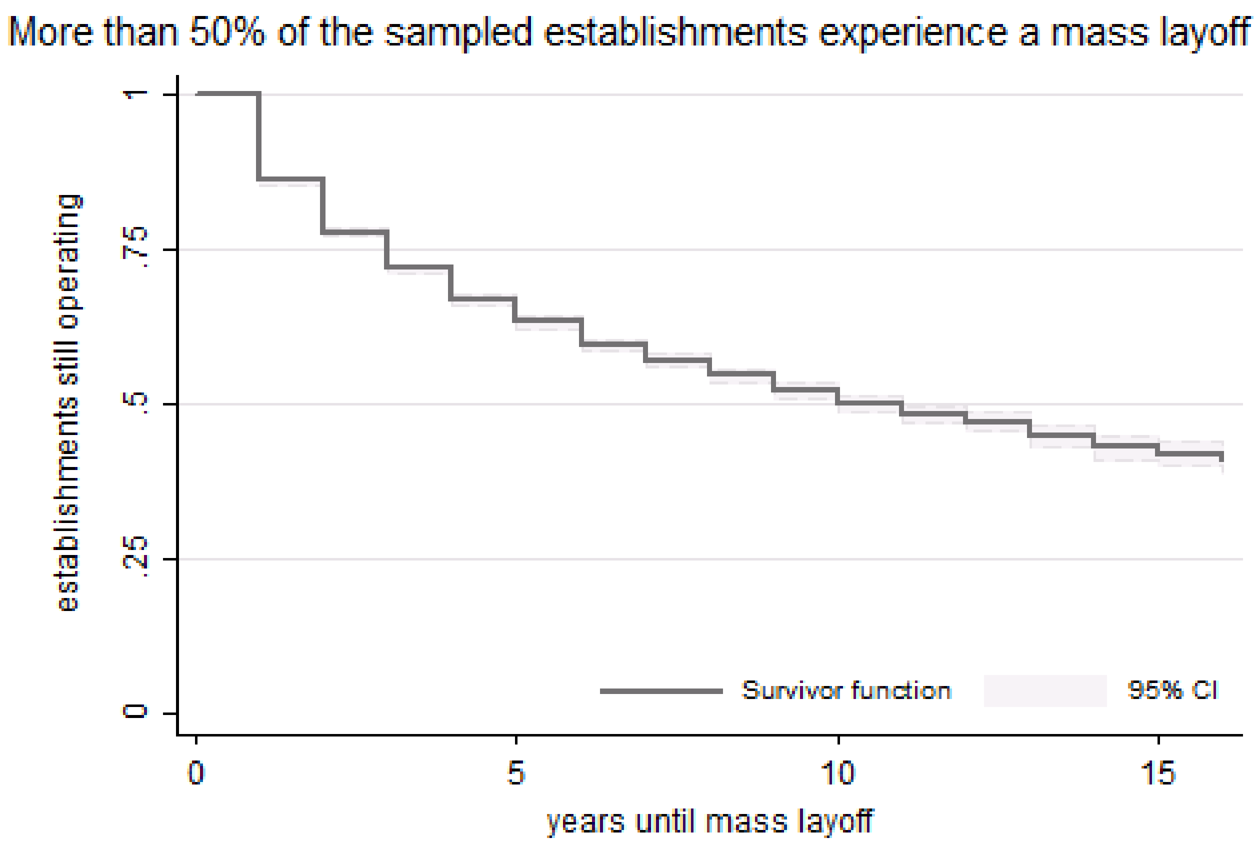


Figure C.3: Survival rate of Downsizing at the 30% threshold - downsizing sample

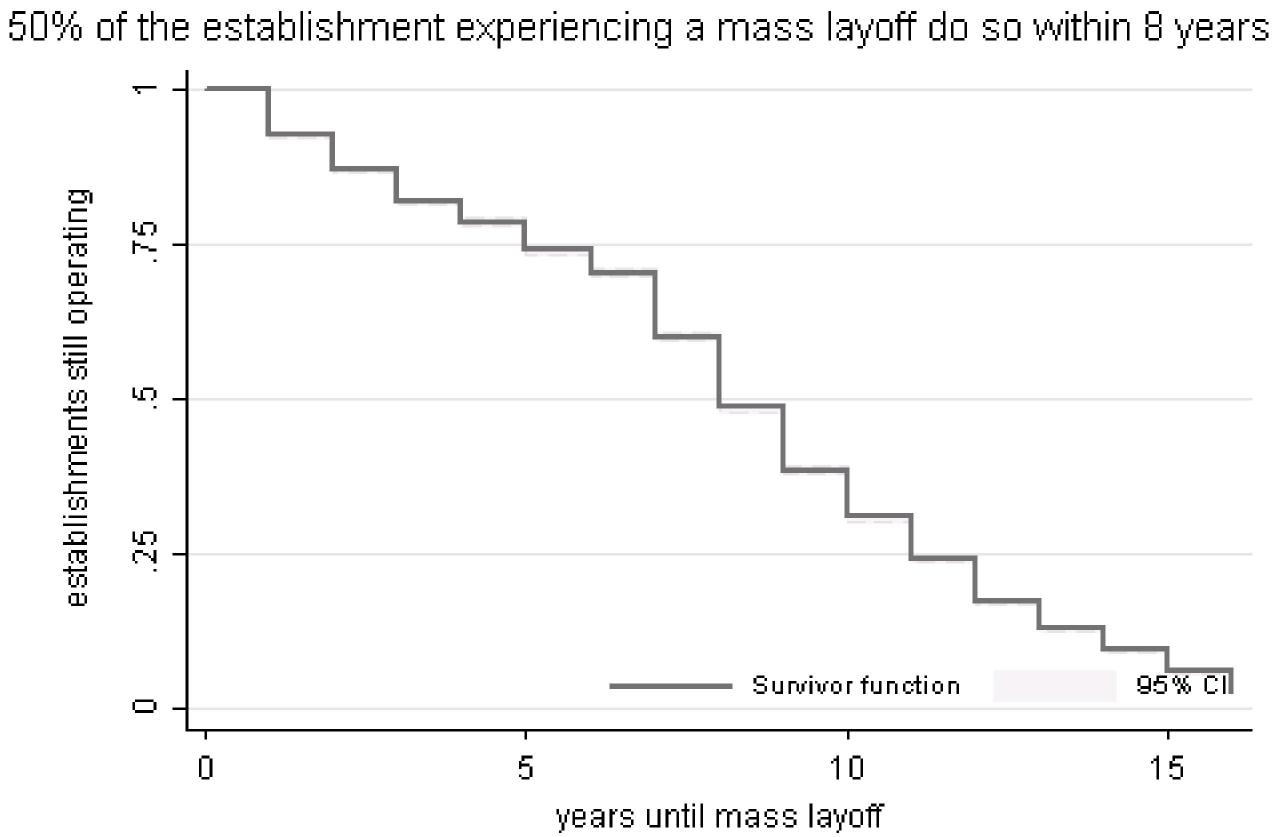


Figure C.4: Coefficients Age Group Estimates by Work Council

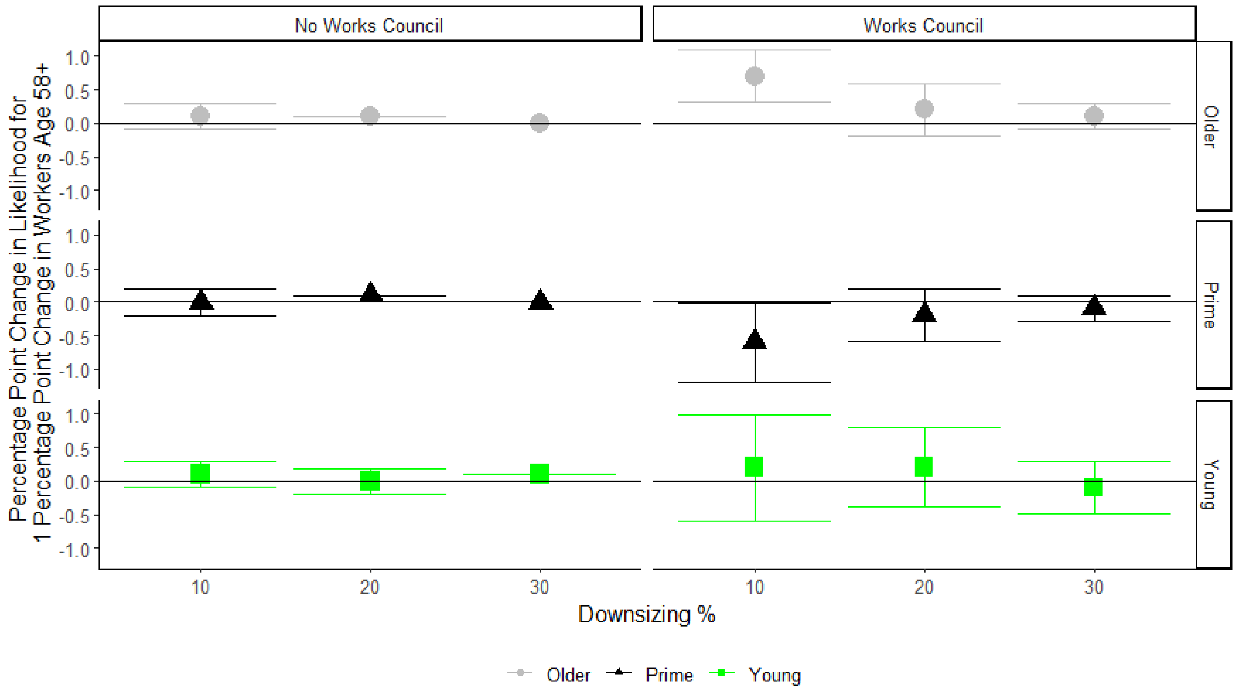


Figure C.5: Relative to Baseline Age Group Estimates by Work Council

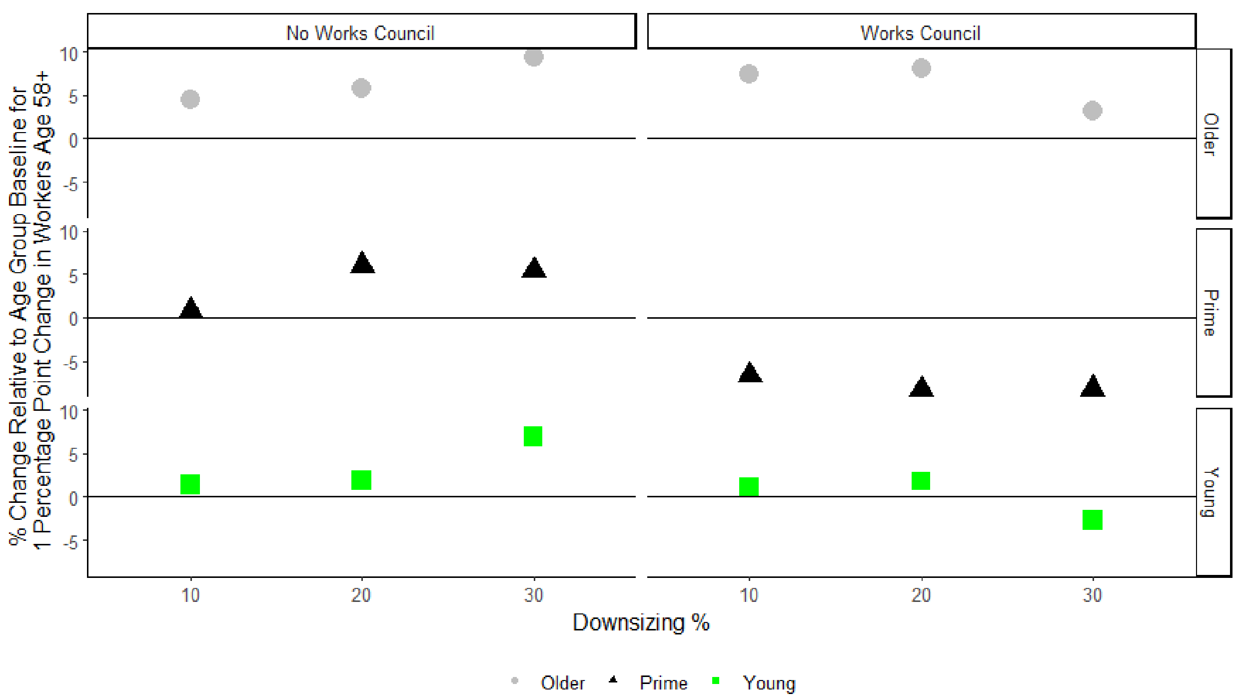


Table C.4: Sample Descriptives

year	observations (count)	estab. size (mean)	share 58 (mean)
1993	3,594	730	0.0509
1994	3,307	661	0.0535
1995	2,908	629	0.0558
1996	2,719	578	0.0596
1997	2,353	580	0.0643
1998	2,691	494	0.0704
1999	2,820	446	0.0913
2000	5,166	296	0.0989
2001	5,821	282	0.1003
2002	5,842	270	0.1012
2003	5,505	258	0.0991
2004	5,409	298	0.1005
2005	5,216	307	0.1056
2006	4,840	302	0.1121
2007	4,570	278	0.1178
2008	4,292	284	0.1232
2009	4,190	287	0.1327
2010	3,742	265	0.1432

Table C.5: Downsizing at different thresholds and age groups

	10% cutoff		20% cutoff		30% cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Model	IV	OLS	IV	OLS	IV	OLS
			Older Workers			
Share 58	0.004***	-0.000***	0.001**	-0.000***	0.001*	-0.000***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
%	0.067	0.000	0.043	0.000	0.077	0.000
baseline	0.060		0.023		0.013	
			Prime Workers			
Share 58	0.002**	-0.001***	0.001	-0.001***	0.000	-0.000***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
%	0.032	-0.016	0.048	-0.048	0.000	0.000
baseline	0.062		0.021		0.011	
			Younger Workers			
Share 58	0.008***	-0.002***	0.004***	-0.001***	0.002***	-0.001***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
%	0.055	-0.014	0.065	-0.016	0.069	-0.034
baseline	0.146		0.062		0.029	
			All Workers			
Share 58	0.003***	-0.001***	0.001**	-0.000***	0.001**	-0.000***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
%	0.058	-0.019	0.053	0.000	0.100	0.000
baseline	0.052		0.019		0.010	
N	74,985	74,985	74,985	74,985	74,985	74,985

Standard errors, clustered at the establishment level, are in parentheses. The unit of observation is the establishment-year. Each regression includes a general set of controls: location, share of younger and share of older people at workplace, and a marker to indicate missing years in the layoff calculation. In addition, each regression includes a set of establishment characteristics: workforce demographics, wage bill, size, existence of a work council and any industry agreements. Furthermore year fixed effects are included. The instrumental variable regressions are estimated by two-stage least squares. One star, two stars, and three stars denote statistical significance at the 10-, 5-, and 1-percent confidence levels, respectively. Baseline represents the mean of share 58.

Table C.6: First Stage Estimates

	Coefficient	Std. Error
Shift-Share Instrument (on share 58)	0.184***	0.016
N	74,985	

The unit of observation is the establishment-year. Standard errors are clustered at the establishment level. Each regression includes a set of establishment characteristics (workforce demographics, wage bill, size, flows, existence of a work council and any industry agreements, location) and year dummies as controls. Three stars denote statistical significance at the 1-percent confidence level.

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