Peer Effects and Retirement Decisions: Evidence from Pension Reform in Germany

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Abstract

Research on the role of peers in retirement decisions is limited, and existing studies focus on a single occupation or particular types of workers. Our study uses employee-employer matched administrative data, which include a wide range of occupations, industries and types of workers, to estimate the impact of peer retirements on individual retirement decisions. Using exogenous variation in changes in the retirement age by birth year and gender, we find robust evidence of peer effects. The reduction of one peer retirement in response to a reform that raised full-benefit retirement ages, causes an approximately 37 and 34 percentage point decreases in the individual retirement hazard rate for men and women, respectively. For 58-year-olds, these estimates translate into extensions of working life of approximately 1.4 and 1.1 years for men and women, respectively. The analogous estimates for 65-year-olds are smaller: 0.7 years for men and 0.4 years for women. Our findings are largely robust across industries and different occupational classifications, although we find peer effects are larger (smaller) in small (large) groups. Overall, the econometric evidence suggests that peers may amplify the magnitude of individual responses to changes in their own retirement incentives, which means reforms that provide incentives for specific cohorts of individuals to delay (advance) retirement may produce larger than anticipated savings (costs) to pension funds through spillovers to adjacent cohorts.

JEL Categories: J26, H55, C31

Key words: retirement, peer effects, public pensions, pension reform

1. Introduction

The majority of developed countries are facing a demographic transition that threatens the solvency of their social security programs (Gruber and Wise 2008). Private pension plans are under similar pressure as employers manage an aging workforce and combat legacy costs. As policymakers and employers consider options to address these issues, it is important to understand how individuals make retirement decisions. The literature has focused primarily on examining the impact of changes in individual pension incentives on retirement behavior (e.g., Mastrobuoni 2009).[1] Much less is known about the impact of peer retirements on individual retirement behavior, which could operate through, for example, information sharing and coordinated retirement decisions. If peer effects are present, changes in the retirement incentives facing one's peers may spillover and amplify the magnitude of individual responses to changes in individual retirement incentives. A social multiplier in the context of retirement could have important implications for social security fund balances in the future.

In this study, we use employee-employer matched administrative data to make four noteworthy contributions to the retirement literature. First, we produce the first estimates of peer effects generalizable to a wide range of industries and occupations by using data that is representative of about 50 percent of the German workforce. Previous researchers have studied specific occupations or types of workers, including school teachers in the Los Angeles School District (Brown and Laschever 2012; BL henceforth) and non-federal public-sector workers in Oregon (Chalmers, Johnson and Reuter 2008; CJR henceforth). Both studies find that individual-retirement decisions are positively affected by peer retirements, but it is impossible to know

[1] Lumsdaine and Mitchell (1999) provide a comprehensive but technical review of empirical strategies for modeling individual retirement behavior; Leonesio (1996) provides a nontechnical summary of the findings from the literature; and Gruber and Wise (2008) compare individual retirement incentives in public pension schemes across countries.

whether these findings generalize to the broader economy or the private sector. Second, we test for heterogeneous peer effects across occupations and industries, as workers in physically demanding jobs or jobs that require collaboration among peers may respond differently to peers than workers in white-collar jobs and jobs for which tasks are performed on an individual basis. Third, we examine more narrowly defined peer groups than those used in prior work, which have measured peer effects at the school level (BL) and establishment level (CJR). Our peer group is defined as workers aged 50 and older employed in the same occupation within the same establishment. Fourth, we estimate peer effects for different sized peer groups, which is often not possible in other studies due to data limitations.

Our identification strategy is based on stepwise changes in pensionable ages affecting persons born after 1937 that gradually increased ages of eligibility for full pension benefits from age 60 to age 65. During the phase-in, adjacent single-year birth cohorts and persons of different sexes born in the same year had pensionable ages six or more months apart. The policy changes did not directly alter incentives for workers born before 1938. The reform provides a source of exogenous variation with which to identify the causal impact of peer retirements on individual retirement decisions, as it incentivized later retirements among an identifiable demographic group but did not impact incentives of all workplace peers. The reliance on changes in pensionable ages, which vary by birth cohort and gender, circumvents the potential endogeneity problems associated with using changes in benefits, which are a function of past earnings and, as a result, could affect old-age labor-supply decisions through accumulated wealth (See Mastrobuoni 2009).

We find the retirement of one additional peer triggers a sizeable 37.4 and 33.8 percentage point increases in the individual retirement hazard rate among men and women between 58 and

65 years-old, respectively. Based on existing retirement patterns for men and women prior to 1998, these estimates imply one fewer peer retirement would extend a man's employment duration by 0.7 months if the peer retirement happened when he was age 65 to approximately 1.5 years if it occurred when he was 58. The same calculations yield changes in duration of 0.4 months to 1.0 years for women. These calculations assume no change in subsequent hazard rates, which is unlikely. A reduction in peer retirements at age 58 may reduce that hazard rate, but "catch-up" may occur at age 59 and, at present, we do not model these dynamics. In subsequent versions of this paper, we intend to further refine these calculations.

In total, we conclude workplace peers have an important impact on retirement timing peer and own retirements are positively correlated, and policies that encourage later retirements spillover to adjacent cohorts. These peer effects lead to larger postponements of retirement than models examining only the effects of changes in retirement incentives on affected cohorts have implied. Even using the lowest implied durations, our estimates indicate peer effects may increase the duration of working life by an additional six percent beyond the direct effect. In the case of reforms that raise pensionable ages, these effects will occur in the early years as the policy begins to bind and grandfathered cohorts further magnify their importance in present value calculations of anticipated savings.

2. Pension Reform in Germany

Like most European countries, Germany has a pay-as-you-go pension system. The system covers approximately 80 percent of the German population.[2] Private retirement savings is

[2] Self-employed workers, civil servants and marginal workers are excluded. Marginal workers are those who earn less than 15 percent of the average gross monthly wage. During our study period, marginal and self-employer workers accounted for approximately six and nine percent of the overall labor force respectively (Börsch-Supan and Schnabel 1999).

uncommon in Germany for the retirement cohorts we study. In the 1990s, public pension benefits accounted for approximately 80 percent of income among households headed by persons aged 65 or older. 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; Börsch-Supan, Reil-Held, and Schunk 2007).[3] Public pension accrual is a simple function of one's own wages, years of service, age, and countrywide average wages and benefits in each year. Benefits are based only on one's own work history. Spousal benefits are not paid, only survivor benefits.

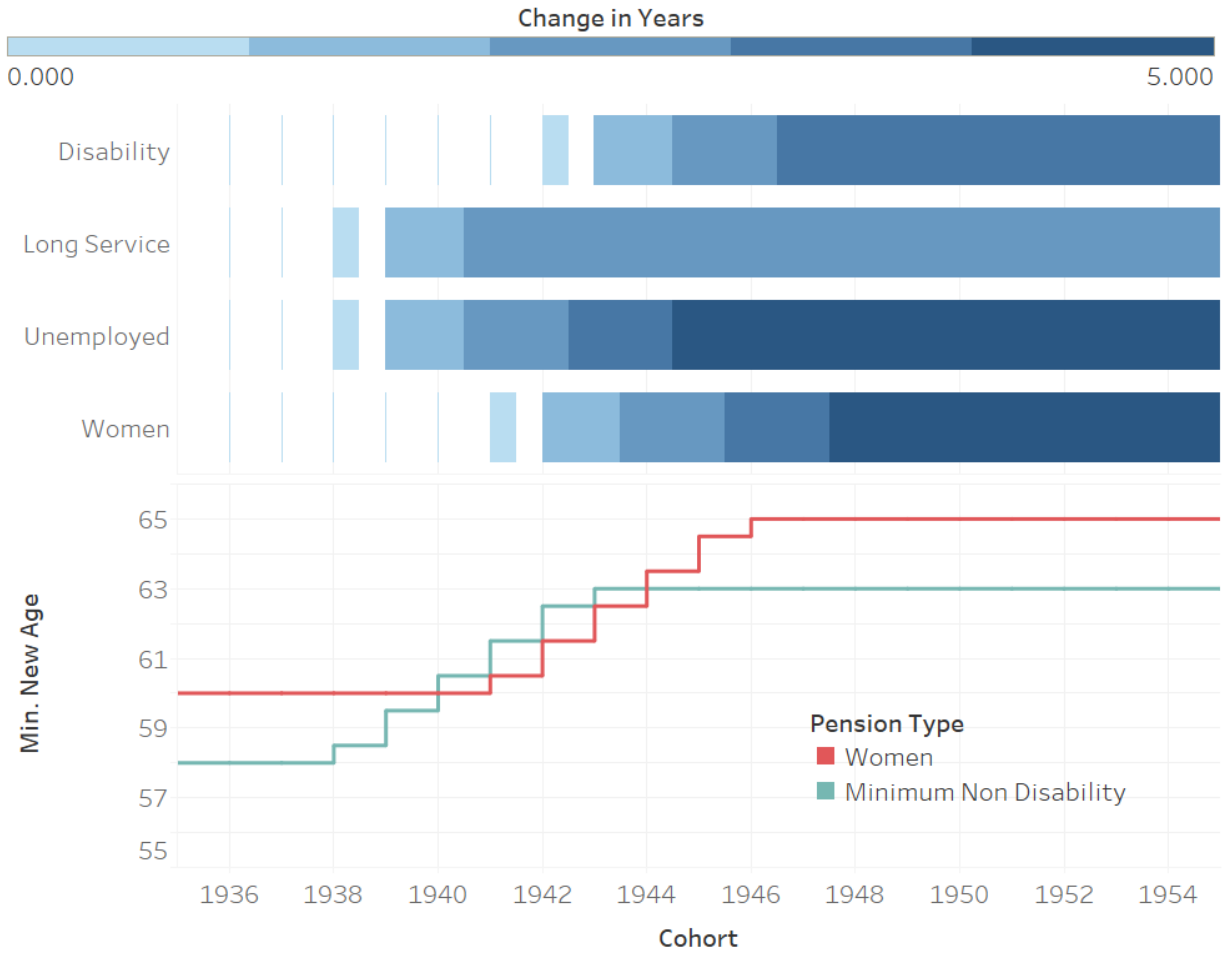
Germany's pension system contains many "pathways" to claiming old-age pension benefits. Like in the U.S., Germans can retire before reaching "normal pensionable age" and receive actuarially adjusted old-age pension benefits. At the start of our sample period, early disbursements of old-age pensions were lower because individuals retiring early had fewer years of contributions, but they were not otherwise actuarially adjusted (Berkel and Börsch-Supan 2004). Germans who claimed unemployment or disability benefits could also draw down old-age pension benefits. Germans with long service histories, defined as 35 years of work or more, could claim old-age pension benefits early, too. Also, women could claim old-age pension benefits earlier than men. In 1992, Germany introduced reforms that gradually eliminated pathways to retirement that pay full benefits (without actuarial adjustment) prior to age 65.

The reforms increased ages of pension eligibility in each pathway gradually by birth cohort. Figure 1 summarizes these changes by birth cohort and pension type. For example, the 1938 cohort was the first affected by the reform. Before the reform, exit from the labor force leading to retirement was possible at age 58 via the unemployment pathway as shown in the bottom panel of Figure 1. When the 1938 cohort reached age 58 in 1996, they had to work six months

[3] The Riester Reform was associated with increases in private savings among younger workers.

longer because of the increase in the age at which they could access old age pension benefits for reason of unemployment, as shown in the top panel of Figure 1. This change, combined with the increase in women's pension, created differences in pensionable age of 6 to 12 months across adjacent birth cohorts and differences of 6 to 12 months for men and women within the 1940 through 1944 cohorts.

Figure 1: Changes in Pensionable Age by Birth Cohort and Pathway



The 1938 cohort reached age 58 in 1996, but the schedule of increases was made public in 1992. For example, men born in 1940 knew at age 52 that they could not obtain full old-age pension benefits through the unemployment pathway at age 60 as previous cohorts had. They could either

receive a reduced benefit at age 60 or wait until age 60.5. The actuarial adjustment factor for claiming at age 60 was 0.3 percent per month. The reform also provided an actuarial increase of 0.5 percent per month for retirements after age 65. Previous research finds the early effects of these reforms on elderly labor supply were dramatic, especially for men. Berkel and Börsch-Supan (2004) estimate the reforms were associated with a two-year increase in the average retirement age among men and a nine-month increase among women.

3. Data

3.1 Sample Construction

Our data are a sample from establishment- and employee-level administrative records of the German pension system. We create a custom extract from the Establishment History Panel (BHP) matched to Integrated Employment Biographies (IEB) for all workers within each establishment. Our data include employment histories for all workers within an establishment, necessary information to determine the pensionable age for each individual, and important covariates that relate to retirement decision (e.g., detailed work histories, earnings, work hours, educational attainment, occupation and industry).[4] Having data for all workers within an establishment is crucial for studying peer effects, which has resulted in previous studies using smaller samples that must rely on ad-hoc peer-group definitions (Halliday and Kwak 2012).

From the BHP population, we first select all establishments recorded at least once between 1990 and 2010. Within the outlined observation period, we only include establishments that employ at least 100 workers at any point in time. In addition, we restrict the sample to establishments that are located in West Germany over the observation period. After these

[4] As this is a custom sample, there is no data manual but the information included in our data is similar to linked employer-employee data of the RDC of the Federal Employment Agency (see Klosterhuber et al. 2016).

restrictions, our data set includes between 11,342 and 12,525 establishments per year. The majority of observed establishments belong to the mining and manufacturing sectors, followed by trade and food services, finance and real estate.

We then select every individual born between 1925 and 1950 who has worked in the sampled establishments in a job liable to social security at least one day in between 1990 and 2010. Our sample is composed of 4,186,978 workers and 60,072,136 employment-spell observations. We organize the data at the person-year level.

Lastly, we sample the daily employment histories of the outlined workers from 1975 to 2011. Employment histories come from the notification process of the social security system and the internal procedures of the Federal Employment Agency. The individual data contain exact information on the daily employment and unemployment history of the individuals. In addition, the data contain a rich set of variables describing employment or benefit receipt in more detail. For example, the data include precise information on daily wages and income from benefits, such as unemployment. Moreover, spatial information and a rich set of demographic variables are available. We use these employment histories to determine workforce exit for each worker in the sample.

To obtain characteristic of the full establishment population we use another custom data extract similar to the BHP. The Institute for Employment Research constructed data for us that hold aggregated workforce statistics on the workforce composition on the establishment-occupation level. The information is identical to the BHP (see Gruhl et al. 2012), however, we have all the information by establishment and 3-digit occupation.[5]

[5] 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.

3.2 Definition of the Peer Group

The data also include detailed occupational codes, from which an aggregation to 12 occupational groups according to the Blossfeld classification (Blossfeld 1987) is created. The 12 groups are: agricultural jobs, simple manual jobs, simple services, simple sales jobs, medium-skilled manual jobs, medium-skilled services, technicians, medium-skilled sales jobs, engineers, semi-professionals, professionals, and managers. We define peer groups to include all workers age 50 to 65 working in same occupational group in a given establishment. Our sample includes 1,109,586 peer groups, with an average size of approximately 25. However, we have examined the sensitivity of our estimates to definitions where we restrict the workers in each peer group to only include workers age 60 to 64. Also, we examine possible heterogeneity in peer effects by peer group size, industry, and broad occupational type (e.g. professional, technical, service).

3.3 Summary Statistics

Before turning to our econometric approach and the multivariate findings we would like to provide a brief overview of our sample. Table 1 contains means and standard deviations of our outcome variable and dependent variables of the second stage. Our instrument, the count of peers who are eligible to retire has a mean of 28.98 with a standard deviation of 11.89. The average size of the peergroup is approximately 25 with a standard deviation of 241.

Table 1: Mean and Standard Deviation of Main Variables

Variable	Mean	Std.	Mean	Std.
Count of Individuals Retiring (cohorts before 1938)	0.274	0.446	0.334	0.472
Ave Change Peer Pensionable Age (Months)	0.000	0.000	0.000	0.000
Age 59	0.101	0.301	0.159	0.365
Age 60	0.165	0.371	0.276	0.447
Age 61	0.179	0.383	0.121	0.326
Age 62	0.192	0.394	0.123	0.328

Age 63	0.200	0.400	0.117	0.320
Age 64	0.087	0.282	0.103	0.303
Age 65	0.076	0.265	0.100	0.300
Time in Full TIme Job (in days)	7713	1740	4100	3353
Time in Part Time Job (in days)	90.75	508.0	2144	2723
Time in Peergroup (in days)	3029	2389	2566	2155
Daily wage	98.29	56.00	57.51	45.50
German nationality	0.925	0.263	0.943	0.232
Vocational training	0.653	0.476	0.532	0.490
Some college	0.063	0.243	0.011	0.103
University	0.086	0.280	0.035	0.184
1997	0.237	0.425	0.238	0.425
1998	0.170	0.376	0.102	0.302
1999	0.131	0.337	0.117	0.320
2000	0.081	0.273	0.080	0.271
2001	0.036	0.185	0.046	0.209
2002	0.015	0.122	0.020	0.140
2003	/	/	/	/
Average groupsize	69.08	49.08	73.09	49.60
Share of Females in Peergroup	0.241	0.248	0.655	0.235
Share of Low-Skilled in Peergroup	0.197	0.236	0.272	0.276
Share of High-Skilled in Peergroup	0.153	0.283	0.065	0.160
Share of People under 30 in Peergroup	0.153	0.121	0.168	0.125
Share of People over 50 in Peergroup	0.684	0.167	0.692	0.156
Share of Foreigners in Peergroup	0.090	0.131	0.100	0.142
Median Age of Peergroup	655.9	30.02	651.1	31.10
Interquartile Age Range of Peergroup	85.64	24.30	88.22	21.40
Average wage of peergroup	95.26	29.21	67.80	26.00
Mining/Manufacturing	0.491	0.500	0.242	0.428
Energy/Water Supply	0.024	0.152	0.007	0.085
Construction	0.046	0.210	0.006	0.079
Trade/Food Services	0.057	0.232	0.094	0.291
Transportation	0.030	0.171	0.020	0.141
Finance	0.040	0.197	0.052	0.222
Real Estate	0.050	0.218	0.055	0.228
Public Administration	0.207	0.405	0.458	0.498
Administration	0.053	0.224	0.063	0.243
Number of Workers in Establishment	1949	5204	1339	260.0
Share of Females in Establishment	0.317	0.211	0.560	0.209
Share of Low-Skilled in Establishment	0.204	0.158	0.206	0.168
Share of High-Skilled in Establishment	0.115	0.140	0.109	0.143
Share of Part-Time in Establishment	0.111	0.134	0.239	0.176
Share of People under 30 in Establishment	0.194	0.079	0.211	0.089
Share of People over 50 in Establishment	0.745	0.083	0.755	0.080
Share of Foreigners in Establishment	0.095	0.101	0.092	0.100
Share of Workers in Agricultural Jobs in c	0.007	0.037	0.009	0.035
Share of Workers in Simple Manual Jobs in Estab.	0.200	0.222	0.108	0.194
Share of Workers in Simple Service Jobs in Estab.	0.118	0.166	0.140	0.182
Share of Workers: Simple Sales Jobs in Estab.	0.057	0.089	0.091	0.154
Share of Workers: Skilled Manual Jobs in Estab.	0.155	0.155	0.081	0.110

Share of Workers: Skilled Service Jobs in Estab.	0.027	0.081	0.055	0.111
Share of Workers: Skilled Sales Jobs in Estab.	0.210	0.204	0.231	0.230
Share of Workers: Techincian Jobs in Estab.	0.077	0.075	0.048	0.060
Share of Workers: Semi-Professional Jobs in Estab.	0.050	0.128	0.154	0.220
Share of Workers: Engineering Jobs in Estab.	0.053	0.089	0.028	0.060
Share of Workers: Professional Jobs in Estab.	0.019	0.073	0.033	0.087
Share of Workers: Managerial Jobs in Estab.	0.022	0.046	0.018	0.044

4. Identification Strategy

The identification of peer effects has three main challenges: simultaneity, correlated unobservables, and endogenous group membership (Manski 1993). To convincingly address these issues in an observational study, an instrument that is strongly correlated with endogenous group outcome and uncorrelated with unobservable determinants of retirement decisions is required. However, more is needed to identify the causal effect of peer behavior on individual outcomes: the instrument must also differentially affect members of the peer group (Brock and Durlauf 2001). In the peer-effects literature, such instruments are referred to as Partial Population Interventions (PPI), which requires an exogenous intervention that affects some but not all individuals in the peer group (Moffitt 2001). Brown and Laschever (2012) extend the PPI-approach via the use of a policy intervention that affects members of peer groups differently, which they refer to as a Differential Population Intervention (DPI). The requirements for identification are similar to traditional instrument-variable estimation (i.e. relevance and exogeneity), except the PPI/DPI must affect some but not all peer-group members and/or affect peer-group members differentially. In addition, the PPI/DPI must exhibit between-peer-group variation.

The typical research design for estimating peer effects includes a sample of individuals who are subjects as well as peers. Because of the endogeneity issues, estimation of peer effects in the literature relies on different types of exogenous variation in an effort to recover causal estimates.

However, even with a valid instrument, Angrist (2014) argues that econometrically estimated peer effects are approximately equal to the ratio of the 2SLS to OLS estimates. As such, one may attribute behavioral responses to one's peers when in fact the estimated peer effect could simply be due to differences in 2SLS and OLS estimates that do not reflect a response to peer behavior. Furthermore, estimating peer effects via 2SLS is problematic in the sense that the first-stage regression of the group behavior on the instruments is identical to a reduced-form regression of the individual behavior on the instrument, as every individual in the sample is also a different individual's peer. Thus, the instrument appears in both the first- and second-stage equations.

Angrist (2014) argues that two design features are important to credibly identify peer effects. First, the mechanical link between individuals and their peers must be broken. Our research design breaks the mechanical link by studying the behavioral responses of workers unaffected by the pension reform to their peers in cohorts directly affected by pension reform. Second, OLS and 2SLS estimates should be equivalent when peer effects are absent. Random assignment to peer groups would create an expectation of equivalence, but randomized group assignment is rare. Although random assignment to groups is absent in our setting, the cutoff between affected and grandfathered cohorts in the 1992 reform was arbitrary and changes began to bind as early as 1996. Furthermore, changes in establishment and job are rare among older workers in Germany, making selection into and out of peer groups in response to the reform unlikely.

Formally, we estimate the following group-level regression for workers born in 1938 and after:

$$\overrightarrow{\tilde{y}_{1938+}}_{g,t} = \delta_0 + \delta_1 P_{g,t} + \delta_2 ind_{g,t} + \phi_g + \phi_t + \eta_{g,t} \quad (1)$$

The subscripts g and t index peer groups and years, respectively. The variable $\overrightarrow{\tilde{y}}_{g,t}^{1938+}$ is the count of peer retirements for workers born in 1938 and after who belong to peer group g in year t; $P_{g,t}$ is the PPI/DPI, which is the average increase in the age of eligibility (in months) for full pension benefits via any stream for peer group g in year t; $ind_{g,t}$ is a set of dummy variables indicating the industry to which peer group g belongs in year t; ϕ_g represents peer-group fixed effects; ϕ_t represents year fixed effects; $\eta_{g,t}$ is the error term; and the δ_j are parameters to be estimated.

After estimating equation (1), we obtain the predicted value for the dependent variable, which we label as $\tilde{y}_{g,t}$, and include it as the right-hand-side variable of interest in the following regression equation:

$$\tilde{y}_{i,g,t}^{<1938} = \beta_0 + \beta_1 \tilde{y}_{g,t} + X'_{i,t}\beta_2 + Z'_{g,t}\beta_3 + \phi_g + \phi_t + u_{i,g,t}. \quad (2)$$

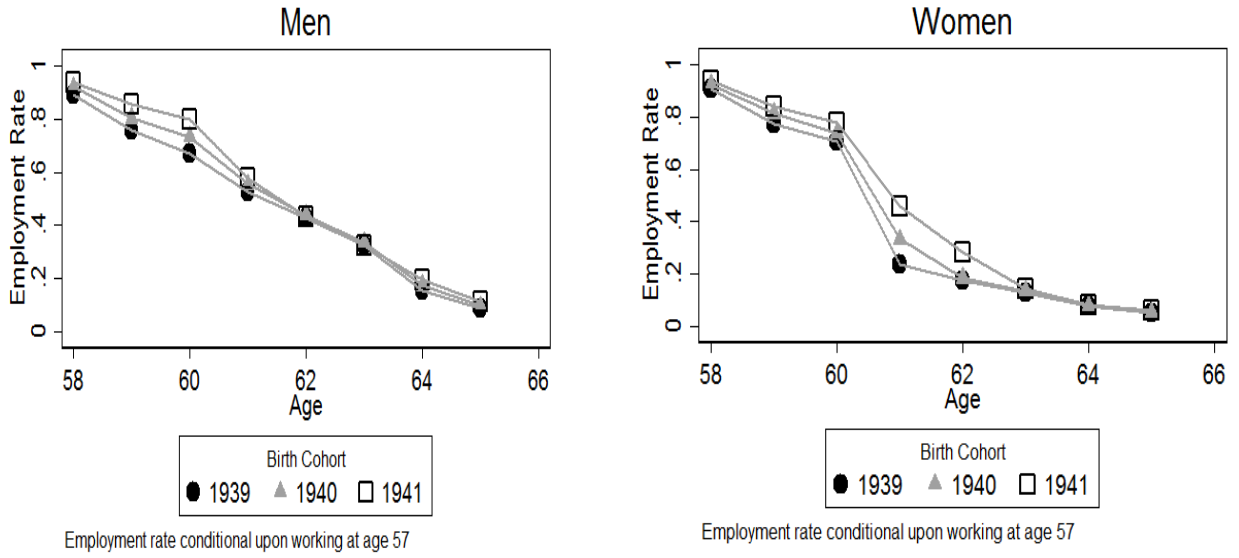
The subscript i indexes individuals, and the subscripts g and t are defined above. The dependent variable, $\tilde{y}_{i,g,t}^{<1938}$, in equation (1) is a binary variable that equals zero when individual born before 1938 who belong to peer group g in year t is working, one when they retire, and missing thereafter.[6] The remaining variables in equation (1) account for individual i's characteristics at year t $(X_{i,t})$, time-varying and time-invariant characteristics of the peer group $(Z_{g,t}$ and $\phi_g)$,[7] year-specific effects (ϕ_t), and $u_{i,g,t}$ captures variables that affect $\tilde{y}_{i,g,t}^{<1938}$ and are not held constant. The β_j are parameters to be estimated.

[6] Such a specification is referred to in the retirement literature as a discrete time hazard model, and the coefficient estimates can be interpreted as percentage-point changes in the retirement hazard rate.

[7] The variables included in $Z_{g,t}$ include $ind_{g,t}$ from equation (1), other peer-group controls, and time-varying establishment-level characteristics as well.

Our two-step procedure requires that equation (1) include only exogenous variables, which is the justification for the limited set of right-hand-side variables. In addition, the endogeneity issues inherent in the estimation of peer effects necessitates a source of exogenous variation, which comes from the inclusion of $P_{g,t}$ in equation (1). To observe visually how the reform affected work/retirement behavior of affected workers, we plot unadjusted employment rates by age and gender for three cohorts affected by the reform, which illustrates the differential impact of their pension eligibility rules by birth year and gender (Figure 2). It is clear that cohorts that experienced larger changes in pensionable age under the reform have higher employment rates between the ages of 59 and 62. For men, access to full retirement for reason of unemployment, which was the most popular pathway to retirement before the reform, changed from age 60 to age 63.5 across these cohorts. Also, the 1941 cohort experienced a change in pensionable age for women of six months (from 60 to 60.5).

Figure 2: Unadjusted Employment Rates for Men and Women



Moreover, the estimated effect of $P_{g,t}$ reveals economically and statistically significant effects of the reform on peer retirement behavior, which provides an exogenous shock to peer retirements.[8] As such, we contend that our estimate of β_1 has a causal interpretation. However, adjustments to the standard errors are likely needed, as ignoring sampling error in the computation of the predicted value of the dependent variable in equation (1) could produce unreliable test statistics. In subsequent revisions of this analysis, we will compute bootstrapped standard errors with clustering on establishments. The current version has heteroskedasticity robust standard errors clustered at the establishment level.

5. Results

Table 2 contains the first-stage regression estimates. For brevity, we report only the coefficient on the instrument, which is the average of the changes in earliest age of eligibility for full pension benefits. These changes are measured in months. As such, the estimate indicates a one-month increase in the average change in pensionable age is associated with -0.013 fewer peer retirements. The sign is consistent with the expected impact of the policy and the magnitude is reasonable given that the peer group includes all persons aged 50 to 65. In analyses where the peer group is restricted to persons ages 55 to 65, we find the coefficient remains negative but increases in magnitude.

Table 2: First Stage Regression Estimates

	Peer Group Level		
	coef		std. err.
Ave Change Peer Pensionable Age (Months)	-0.0130	***	0.000
N	987,673		

* p<.1; ** p<.05; *** p<.01

[8] The estimated effect of $P_{g,t}$ on peer retirements is presented in the tables in Section 5.

1. Additional covariates include individual controls (age, experience, wage, nationality, education), group controls (share of females, share of high- and low qualified, share of foreigners, median in and interquartile age, mean wage paid in occupation), establishment controls (share of females, share of high- and low qualified, share of foreigners, share of different occupations), year fixed effects and peer group fixed effects.
2. Standard errors are clustered on the establishment level

In Table 3, we report the second-stage estimates of peer effects for men and women separately. Again, the second stage regressions include only individuals born before 1938. These persons were not included in the first-stage, peer-level regressions. For men, we find one additional peer retirement leads to a 37.4 percentage point reduction in the retirement hazard rate. For reference, the average retirement hazard rate among men in this sample is 27.4 percent. Our estimate for women is 33.8 percentage points and the average hazard rate among women is 33.4 percent. Thus, the estimates indicate an additional peer retirement roughly doubles one's own retirement hazard rate.

Table 3: Second Stage Regression Estimates

	Men			**Women**		
	coef		std. err.	coef		std. err.
Ave Change Peers	0.3742	***	0.079	0.3382	***	0.131
Change Individual	omitted			omitted		
N	310,921			85,295		

* p<.1; ** p<.05; *** p<.01
1. Additional covariates include individual controls (age, experience, wage, nationality, education), group controls (share of females, share of high- and low qualified, share of foreigners, median in and interquartile age, mean wage paid in occupation), establishment controls (share of females, share of high- and low qualified, share of foreigners, share of different occupations), year fixed effects and peer group fixed effects.
2. Standard errors are clustered on the establishment level

Retirement hazard rates vary substantially by age, and it is reasonable to expect the effect of peers on retirement hazard rates may vary by age as well. In Table 4, we report the results of interacting the instrumented peer retirement variable with age dummy variables (omitting age

58). For men and for women, these models indicate the impact of peers does not vary much by age. However, because the retirement hazard rates themselves do vary by age, the impact on employment survival and duration of the same size peer effect on employment varies considerably by age.

Table 4: Second Stage Regression Estimates, Age Interaction

	Men			Women		
	coef		std. err.	coef		std. err.
Ave Change Peers	0.387	***	0.082	0.327	**	0.138
Age 59 (ref 58)	0.067	**	0.023	-0.106		0.110
Age 60	0.151	***	0.023	0.349	***	0.110
Age 61	0.118	***	0.023	0.129		0.110
Age 62	0.106		0.023	0.127		0.110
Age 63	0.544	***	0.023	0.187		0.110
Age 64	0.325	***	0.023	0.145		0.110
Age 65	0.659	***	0.024	0.383	***	0.110
Ave Change Peers X age 59	-0.015		0.011	0.020		0.035
Ave Change Peers X age 60	0.000		0.011	0.024		0.035
Ave Change Peers X age 61	-0.012		0.011	0.009		0.035
Ave Change Peers X age 62	-0.016		0.011	0.036		0.035
Ave Change Peers X age 63	0.004		0.011	0.020		0.035
Ave Change Peers X age 64	-0.012		0.011	0.015		0.035
Ave Change Peers X age 65	0.016		0.016	0.042		0.035
N	310,921			85,295		

* p<.1; ** p<.05; *** p<.01

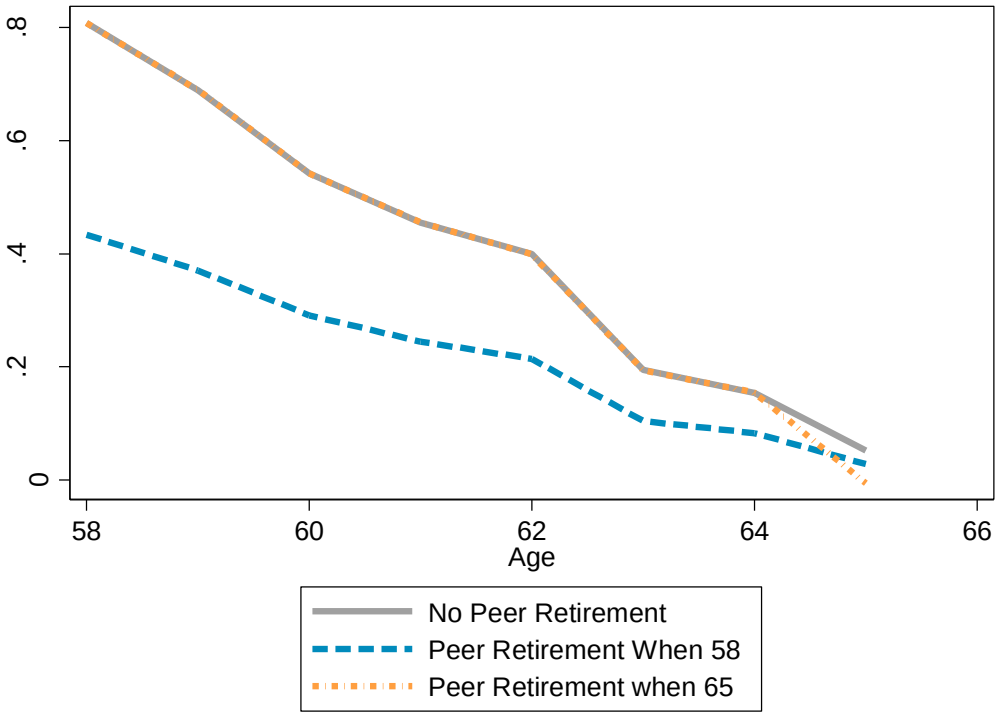
1. Additional covariates include individual controls (age, experience, wage, nationality, education), group controls (share of females, share of high- and low qualified, share of foreigners, median in and interquartile age, mean wage paid in occupation), establishment controls (share of females, share of high- and low qualified, share of foreigners, share of different occupations), year fixed effects and peer group fixed effects.

2. Standard errors are clustered on the establishment level

Figure 3 illustrates this point. If a peer retires when one is age 58, the 37.4 percentage point reduction in the hazard rate shifts the survival at age 58 and subsequent survival probabilities are also lower because they are a function of survival at age 58. Alternatively, if a peer retires when

one is age 65, the survival functions differ only at that age (and beyond) but are identical before. The change in employment duration is equal to the area between the survival curves. The estimated reduction in employment duration is 1.5 years if a peer retires when age 58 but is only 0.7 months if a peer retires when age 65. Figure 4 plots the corresponding survival functions for women. The comparable changes in employment duration are 1.0 years and 0.4 months, respectively.

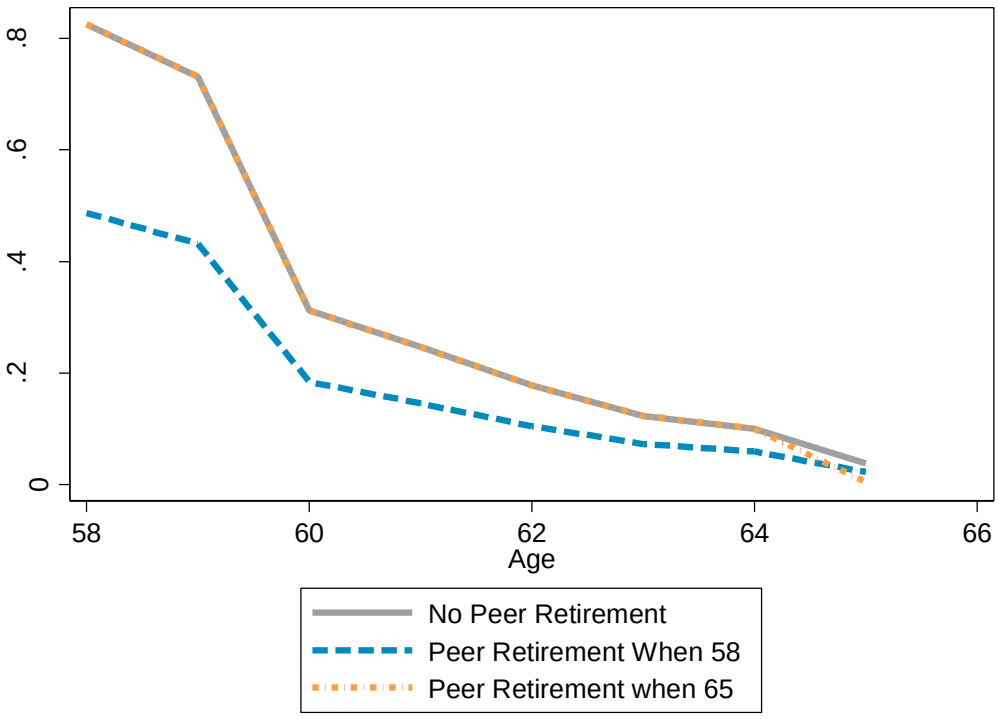
Figure 3: Effect of Peers on Employment Survival Functions, Men



The survival calculations in Figures 3 and 4 all assume peer retirements only affect the hazard rate in the year when the peer retires. This seems implausible. One more (fewer) peer retirement in the present year may increase (reduce) the current retirement hazard rate, but it may reduce (increase) the hazard rate in the next period. At present, we have not formally modelled

the dynamic effects of peer retirements. However, in Figure 5, we show what employment survival functions for men would look like if we assume retirements become less likely in the years following a peer retirement and returns the survival function to its original level. In this hypothetical example, the estimated change in employment duration falls from 1.5 years to 0.8 years.

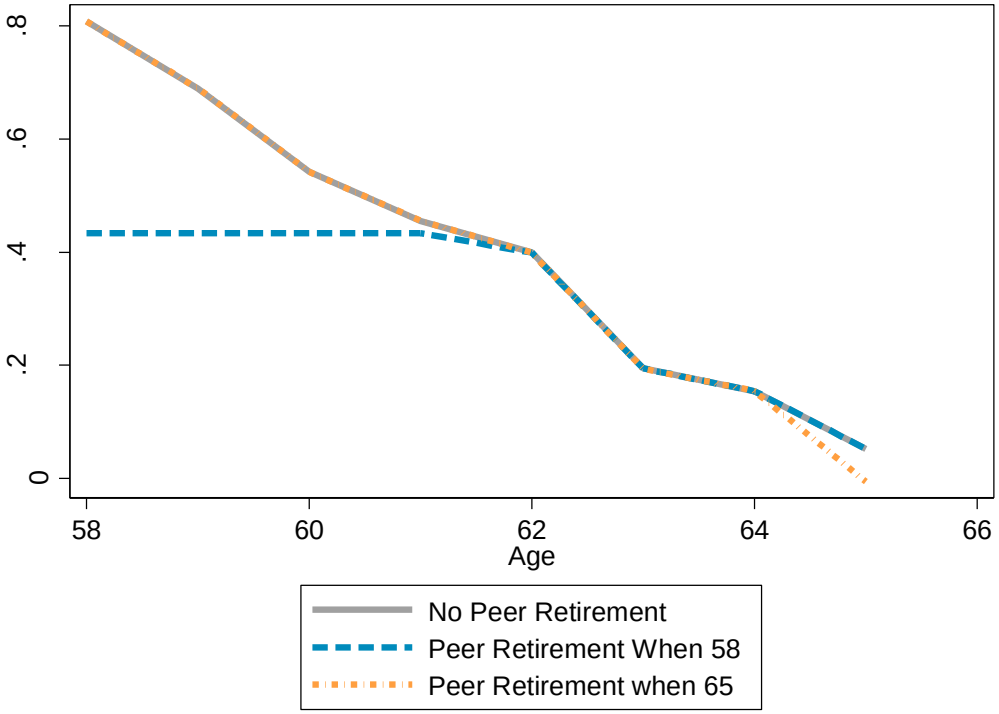
Figure 4: Effect of Peers on Employment Survival Functions, Women



In addition to our main estimates, we also examine heterogeneity in peer effects by industry and peer group size of the individual worker. For men, we find peer effects range from 54 percentage points in smallest quintile of peer groups to 28 percentage points in the largest. Peer effects are smallest (19 percentage points) in the real estate industry and largest in retail trade and food service (58 percentage points). Among women, peer effects are also largest in retail

trade and food service but smallest in transportation. Peer effects appear to be even larger among women working in the smallest quintile of peer groups and decline with peer group size as found for men, but we again find no important differences by educational attainment for women. The exact estimates can be found in the Appendix.

Figure 5: Effect of Peers on Employment Survival Functions Assuming Catch-Up, Men



6. Conclusion

We use employee-employer matched administrative data to estimate impact of peer retirements on individual retirement timing. Our study advances the literature on peer effects in the context of retirement in four ways. First, our estimates are generalizable to about half of the German workforce, whereas existing studies are often forced, due to data limitations, to examine

peer effects in settings with only one occupation or particular types of workers. Second, our data set provides a way to test for heterogeneous peer effects by industry and occupation groupings. Third, our peer group definition is more narrowly defined than those used in prior work. In particular, we define the peer group as workers aged 50 and older employed in the same occupation *within* the same establishment. Lastly, our data provide a way to test whether the importance of peer effects vary with the size of the peer group.

Our empirical strategy separates the subjects from their peers by modeling the response of workers unaffected by a 1993 pension reform to the behavior of their peers who were affected by the reform. In particular, we exploit exogenous variation based on stepwise changes in pensionable ages affecting persons born after 1937 that gradually increased ages of eligibility for full pension benefits from age 60 to age 65. As the reform was phased in across cohorts, adjacent single-year birth cohorts and persons of different sexes born in the same year had pensionable ages six or more months apart. The exogenous variation by cohort and gender is used in econometric models to identify the causal impact of peer retirements on individual retirement decisions, as it incentivized later retirements among an identifiable demographic group but did not impact incentives of all workplace peers.

We find the retirement of one additional peer triggers a sizeable 37.4 and 33.8 percentage point increases in the individual retirement hazard rate among men and women between 58 and 65 years-old, respectively. Based on existing retirement patterns for men and women prior to 1998, these estimates imply one fewer peer retirement would extend the average man's employment duration by 0.7 months if the peer retirement happened when he was age 65 to approximately 1.5 years if it occurred when he was 58. The same calculations yield changes in duration of 0.4 months to 1.0 years for women, respectively. These calculations assume no

change in subsequent hazard rates, which is unlikely. A reduction in peer retirements at age 58 may reduce that hazard rate, but "catch-up" may occur at age 59. Currently, we do not model these dynamics. In subsequent versions of this paper, we intend to further refine these calculations.

Overall, we conclude workplace peers have an important impact on retirement timing peer and own retirements are positively correlated, and policies that encourage later retirements spillover to adjacent cohorts. These peer effects lead to larger postponements of retirement than models examining only the effects of changes in retirement incentives on affected cohorts have implied. Even using the lowest implied durations, our estimates indicate peer effects may increase the duration of working life by an additional six percent beyond the direct effect. In the case of reforms that raise pensionable ages, these effects will occur in the early years as the policy begins to bind and grandfathered cohorts further magnify their importance in present value calculations of anticipated savings.

7. Appendix

We report tables with estimates of our robustness checks.

Table A1: Second Stage Regression Estimates, Occupation Subsamples

	Men			**Women**		
	coef		std. err.	coef		std. err.
Manual Occupations	0.480	***	0.087	0.142		0.129
Professional Occupations	0.071		0.063	0.205	**	0.084
Service Occupations	0.221		0.140	0.392	**	0.153
Technical Occupations	0.156	*	0.087	-0.014		0.021

* p<.1; ** p<.05; *** p<.01

1. Additional covariates include individual controls (age, experience, wage, nationality, education), group controls (share of females, share of high- and low qualified, share of foreigners, median in and interquartile age, mean wage paid in occupation), establishment controls (share of females, share of high- and low qualified, share of foreigners, share of different occupations), year fixed effects and peer group fixed effects.

2. Standard errors are clustered on the establishment level

Table A2: Second Stage Regression Estimates, Industry Interaction

	Men			Women		
	coef		std. err.	coef		std. err.
Ave Change Peers	0.044		0.168	0.419		0.378
Mining/Manufacturing (ref. Agriculture)	0.155		0.201	2.438	***	0.614
Energy/Water Supply	0.093		0.221		omitted	
Construction	-0.136		0.219	2.163	***	0.600
Trade/Food Services	-0.311	*	0.190	2.597	***	0.553
Transportation		omitted			omitted	
Finance		omitted			omitted	
Real Estate	-0.390	***	0.149	2.087	***	0.515
Public Administration	-2.587	***	0.296	0.421		0.344
Administration	-0.073		0.130	2.147	***	0.595
Mining/Manufacturing X Ave Change Peers	0.554	***	0.160	0.098		0.354
Energy/Water Supply X Ave Change Peers	0.582	***	0.188	0.325		0.427
Construction X Ave Change Peers	0.210		0.167	-0.130		0.385
Trade/Food Services X Ave Change Peers	0.372	**	0.162	-0.040		0.356
Transportation X Ave Change Peers	0.185		0.176	0.114		0.367
Finance X Ave Change Peers	0.504	***	0.163	0.116		0.361
Real Estate X Ave Change Peers	0.527	***	0.164	0.223		0.360
Public Administration X Ave Change Peers	0.541	***	0.160	0.131		0.353
Administration X Ave Change Peers	0.445	***	0.168	0.158		0.359
N	310,921			85,295		

* p<.1; ** p<.05; *** p<.01

1. Additional covariates include individual controls (age, experience, wage, nationality, education), group controls (share of females, share of high- and low qualified, share of foreigners, median in and interquartile age, mean wage paid in occupation), establishment controls (share of females, share of high- and low qualified, share of foreigners, share of different occupations), year fixed effects and peer group fixed effects.

2. Standard errors are clustered on the establishment level

Table A3: Second Stage Regression Estimates, Groupsize Interaction

	Men			Women		
	coef		std. err.	coef		std. err.
Ave Change Peers	0.518	***	0.072	0.642	***	0.147
2nd Quartile (ref. 1st Quartile)		omitted			omitted	
3rd Quartile		omitted			omitted	
4th Quartile		omitted			omitted	
2nd Quartile X Ave Change Peers	-0.097	***	0.036	-0.174	***	0.090
3rd Quartile X Ave Change Peers	-0.137	***	0.034	-0.322	***	0.089
4th Quartile X Ave Change Peers	-0.242	***	0.036	-0.459	***	0.088
N	310,921			85,295		

* p<.1; ** p<.05; *** p<.01

1. Additional covariates include individual controls (age, experience, wage, nationality, education), group controls (share of females, share of high- and low qualified, share of foreigners, median in and interquartile age, mean wage paid in occupation), establishment controls (share of females, share of high- and low qualified, share of foreigners, share of different occupations), year fixed effects and peer group fixed effects.

2. Standard errors are clustered on the establishment level

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