Direct and Indirect Effects of Social Policies on Labor Supply Decisions

Empirical Evidence from Germany

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Clara Welteke, M.Sc.
geboren in Kassel

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Dekan:

Prof. Dr. Dr. Andreas Löffler

Erstgutachter:

Prof. Dr. Peter Haan, Freie Universität Berlin

Zweitgutachter:

Prof. Dr. Albrecht Glitz, Universitat Pompeu Fabra Barcelona

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Contents

General Introduction

Population ageing and increasing old-age dependency ratios are posing a large burden on public finances in many OECD countries. This is particularly true for Germany, where the ratio of people aged 65 and over per 100 people of working age[1] was 35 in 2014 and is expected to rise to almost 50 by 2030 (OECD, 2015). One way to relieve public finances is to increase the share of net contributors of the pension system compared to beneficiaries by setting incentives for older workers to delay retirement entry. In Germany, the employment rate of 60 to 64 year-olds was only 52.6 percent in 2014 (OECD, 2015). A second group with low employment rates consists of mothers with young children. Only 31.4 percent of mothers with a child below the age of three were actively employed in 2013 in Germany (Statistisches Bundesamt, 2014). In this dissertation, I analyze direct and indirect effects of social policies on labor supply decisions of these two groups with relatively low employment rates: individuals approaching the retirement age and mothers with young children.

Social policies typically aim at changing individual behavior by setting financial incentives to promote a certain behavior. For example, family policies

[1] Between age 20 and age 64.

in Germany throughout the last decade had the goal to increase maternal employment, promote gender equality, and at simultaneously support families. At the same time, several pension reforms were intended to promote retirement delay of older workers. The first chapter of this dissertation explores whether the abolishment of an early retirement program for women effectively increased employment of the affected group, or instead led to increased program substitution into unemployment or disability pension programs.

While changes in statutory retirement ages directly affect individual retirement decisions through changes in legal requirement or financial incentives, individuals might be indirectly affected by social policies through peer effects and changes in the behavior of the relevant social network. Individuals are embedded within networks of relationships, such as families, neighbors or coworkers. There are several explanations why human behavior depends on the behavior of others. For example, the behavior of others can provide information about the possibilities and consequences of decisions. Furthermore, preferences for conformity to social norms may guide individual behavior. Consequently, individual labor supply decisions may be affected by social norms and the decisions of other people within their social networks. Social interaction effects are of large policy relevance because they can amplify policy effects through so-called social multipliers (see Glaeser et al., 2003). While peer and spillover effects gained attention for instance in research on education (see Epple and Romano, 2011, for a review) or crime (e.g. Glaeser et al., 1996), there has been little work in the context of labor supply decisions. Furthermore, there are only a few studies on peer effects among coworkers in the context of parental leave and retirement behavior (exceptions include Dahl et al., 2014; Brown, 2013), and there has been no study investigating peer effects in the context of labor supply decisions of German workers. It is the goal of this dissertation to close this gap and contribute to the literature that combines economic analysis with sociological explanations of human behavior. In the second and third chapter of my dissertation, I analyze the influence of peers at the workplace on labor supply decisions of workers approaching the retirement age and mothers with young children, who are frequently targeted by social policies.

In Chapter 1 of this dissertation, we present quasi-experimental evidence on the employment effects of an unprecedented large increase in the early retirement age for women, caused by the abolishment of the early pension for women as part the German pension reform of 1999. Raising the early retirement age has the potential to extend contribution periods and to reduce the number of pension beneficiaries at the same time, if employment exits can be successfully delayed. However, workers may not be able to work longer or may choose other social support programs as exit routes from employment. We study the effects of the early retirement age increase on employment and potential program substitution in a regression-discontinuity framework. Based on public pension insurance account data by the German pension fund (VSKT), we exploit that the early pension program for women was abolished from one cohort to the other for women born after 1951. The reform effectively raised the earliest possible retirement age for women from age 60 to at least 63. Our results suggest that the reform increased both employment and unemployment rates of women age 60 and over. However, we do not find evidence for active program substitution from employment into alternative social support programs. Instead, employed women remain employed and unemployed women remain unemployed beyond the former early retirement age of 60. Furthermore, the results suggest a potential increase in inequality within the affected cohorts. Women in East Germany are more affected by the reform than women in the West. In particular, unemployment rates of 60 to 62 year-old women increase more in East than West Germany. Furthermore, we find suggestive evidence for slightly higher employment effects for 60 to 62 year-old women with low income, poor health and women without children. This chapter contributes to the literature in various ways. Many studies focus on changes in the normal retirement age, while only a few analyze the effect of changes in the early retirement age. The existing evidence on the effectiveness of increasing the early retirement age and program substitution is mixed: Staubli and Zweimüller (2013) and Atalay and Barrett (2015) find that gradual increases in the early retirement age led to increased program substitution. In contrast, Manoli and Weber (2016) and Oguzoglu et al. (2016) do not find evidence for increased active substitution from employment into social security programs based on the same reforms. We shed light on this important research question by using a clean quasi-experimental research design. In contrast to previous studies, the reform that we study implies a large one-time change of pension rules. Usually changes of the retirement age are introduced in small steps over a longer time horizon, which generally requires stronger assumptions to separate the reform effect from time trends, other policy reforms, and cohort effects.

In economic evaluations, the central question is often how individuals respond to financial incentives. However, pension reforms and the introduction of new policy instruments are likely to affect individuals not only directly by the change in financial incentives, but also indirectly by a change in the behavior of the social environment. In the second and third chapter of my dissertation, I analyze whether labor supply decisions of older workers and mothers with young children depend on the employment decisions of their coworkers. Both chapters are based on high-quality German administrative linked employer-employee panel data (LIAB), which enable the assignment of a peer group, defined by firm and occupation, to each individual and provide me with individual employment histories of all individuals in these social groups. The empirical identification of peer effects is challenging due to endogenous sorting and common shocks within peer groups, which can lead to correlation of individual behavior even in the absence of causal peer effects. The LIAB dataset allows me to solve the challenges associated with the identification of peer effects using microeconometric and quasi-experimental methods.

In Chapter 2, I provide novel empirical evidence on the question whether individual employment exit decisions of older workers are affected by peers at the workplace. I estimate a probability model of employment exit decisions including a measure of financial incentives, similar to the option value by Stock and Wise (1990), and a measure of peer employment behavior. Following Cornelissen et al. (2017), I use a set of fixed effects to account for endogenous sorting and correlated effects within peer groups. The empirical design enables the estimation of causal peer effects under a set of justifiable assumptions. The regression results suggest that there is a significant negative effect of both the financial option value of employment and the share of older coworkers that is still working on individual employment exit probabilities. The coefficient of the option value is largest for the subgroups with low education or income, and for individuals working in small firms. I use the regression results to simulate a universal and immediate increase in the normal retirement age to 67 and estimate the direct and indirect effects of the reform through the change in financial incentives and peer behavior. I predict a modest increase in the average employment exit age by 2.4 months, resulting from the normal retirement age increase. A delay of peer employment exits by 2.4 months results in an additional increase in the individual exit age by 0.7 months, due to the change in the share of employed coworkers. I conclude that indirect effects through changes in peer behavior can lead to considerable amplification of pension reform effects.

Previous literature on peer effects has shown that coworkers affect individual decisions in various ways. For example, Mas and Moretti (2009) and Hesselius et al. (2009) show that peer effects play a role in the context of productivity of cashiers and worker absenteeism respectively. Cornelissen et al. (2017) find an effect of the long-term quality of a workers' peers on wages. Other papers have shown that peers at work can affect fertility (Pink et al., 2014; Asphjell et al., 2013), parental leave decisions (Dahl et al., 2014), and retirement plan decisions (Duflo and Saez, 2003). However, there is only one paper on peer effects in retirement timing of older workers, focussing a reform of teacher pensions in California (Brown and Laschever, 2012). I contribute to the literature by estimating peer effects in employment exits among coworkers, and thereby generalize the results found by Brown and Laschever (2012) to a general workplace context.

In contrast to the first two chapters, the third chapter of my dissertation is focussing on maternal labor supply instead of employment of individuals approaching the retirement age. In Chapter 3 of this dissertation, I analyze whether mothers' parental leave decisions depend on their coworkers' decisions. The challenges associated with the identification of peer effects are avoided by the exploitation of a quasi-random variation in the costs of parental leave induced by a policy reform, following Dahl et al. (2014). The reform encourages mothers with medium or high income to remain at home during the first year following childbirth. Our results suggest that maternal decisions regarding the length of their own parental leave are significantly influenced by their coworkers' decisions. We find that a mother is much more likely to stay at home for the first year if her peers decided to do so in response to the parental leave benefit reform. This effect corresponds to the Local Average Treatment Effect (LATE). Furthermore, the results suggest that parental leave decisions are particularly affected by coworkers' decisions in situations with high career-related uncertainty. The paper contributes to the growing literature on the question how peer effects influence individual decisions. Within this literature, this is the first paper to analyze peer effects among coworkers in the context of maternal leave decisions. We find that peer effects influence individual decisions significantly, using an innovative quasi-experimental research design.

Chapter 1

Closing Routes to Retirement: How do People Respond?

1.1 Introduction

Population aging presents enormous challenges for public pension systems (OECD, 2015). Most OECD countries answered the challenges posed by increasing old-age dependency ratios by reforming their pension systems. A central aim of these reforms is to extend working lives alleviating the decline of the working age population (OECD, 2006, 2011). Reforms include increases in the early retirement age (ERA) and increases in the normal retirement age (NRA), i.e. the age at which people can first draw full benefits without actuarial deductions. Germany is characterized by a particularly steep increase in the old-age dependency ratio and low employment rates of older workers. In order to relieve public finances by increasing the employment rates of older workers, in a 1999 pension reform, Germany abolished an important early retirement program for women born after 1951. The reform effectively raised the ERA for women from age 60 to at least 63. An increase in the retirement age has the potential to extend contribution periods and reduce the number of pension beneficiaries at the same time, if employment exits are successfully delayed. However, workers may not be able to work longer or may choose other social support programs as exit routes from employment. Large program substitution effects could undermine the potential positive fiscal effects of the pension reform. Therefore, it is important to empirically assess if an increase in the ERA induces increased inactivity or substitution into other government programs such as unemployment or disability benefits.

In this paper, we provide novel empirical evidence on this important research question. In more detail, we analyze the labor market effects of the substantial increase in the early retirement age for women. The change in ERA is a large negative wealth shock for the affected cohorts. The reform provides a clean quasi-experimental setting as it induces a large one-time shift in the ERA. We exploit the unprecedented sharp discontinuity in the ERA between cohorts to estimate the causal impact on female employment behavior in a regression discontinuity framework based on high quality administrative data. Our research design allows us to quantify the causal effects of the reform on female employment, take-up of disability pensions, unemployment, and inactivity rates. Furthermore, we focus not only on the effects on levels, but also on employment outflows into other social security programs as a response to the reform. In contrast to previous literature, we distinguish between *active program substitution* from employment into unemployment, disability pension or inactivity, and *passive program substitution*, which occurs due to continuance of the former status because an exit into early retirement is no longer attainable. Moreover, we examine whether the behavioral reactions are heterogeneous across different groups. Raising the ERA might have undesired distributional effects as the ability to work longer and the remaining life expectancy may depend on socio-economic status. In particular, workers with poor health and a weak labor market position might be negatively affected by the reduced retirement options (Staubli, 2011).

We contribute to the literature in various ways. Similar to other studies, we exploit cohort-specific variation in incentives to retire (e.g., Mastrobuoni, 2009; Hanel and Riphahn, 2012; Cribb et al., 2014; Lalive and Staubli, 2014; Atalay and Barrett, 2015; Manoli and Weber, 2016; Engels et al., 2016). In contrast to previous studies, the reform we analyze is a large one-time change of pension rules. Usually changes of the retirement age are introduced in small steps over a longer time horizon, which generally requires stronger assumptions to separate the reform effect from time trends, other policy reforms, and cohort effects. Moreover, many studies focus on changes in the NRA, while only a few analyze the effect of changes in the ERA. Increasing the ERA implies that the choice set of older workers is reduced and that the employment reaction of those who would have chosen to retire depends on the relative attractiveness of the remaining options. There is a large literature analyzing program substitution effects in the context of pension reforms (e.g., Duggan et al., 2007; Karlström et al., 2008; Li and Maestas, 2008; Coe and Haverstick, 2010; Staubli, 2011; Staubli and Zweimüller, 2013; Borghans et al., 2014; Atalay and Barrett, 2015; Inderbitzin et al., 2016). However, the existing evidence on the effectiveness of increasing the ERA and program substitution is mixed. Staubli and Zweimüller (2013) and Atalay and Barrett (2015) find that gradual increases in the ERA led to increased program substitution in Austria and Australia. In contrast, Manoli and Weber (2016) and Oguzoglu et al. (2016) do not find evidence for increased active substitution from employment into social security programs based on the same reforms.

Based on a linear regression-discontinuity design, we find that employment rates of women born in 1952 aged 60 and older increased markedly by 14.4 percentage points due to the reform. Interestingly, employment rates before age 60 remained unaffected by the reform, even though the reform was long anticipated. Although we find evidence for increased program substitution into unemployment, the increase in the unemployment rate is not due to active program substitution from employment but rather stems from the inability of unemployed women to retire early after the reform. We do not find evidence for increased unemployment, disability pension, or inactivity entry due to the ERA increase. Based on these results, our conclusions are mixed. First, the reform seems to be an effective tool to extend employment of employed women. Second, unemployed women remain longer in unemployment. Third, we do not find evidence for increased take-up of disability pensions. Fourth, the results suggests that the reform affected certain groups heterogeneously. We find larger positive effects on the employment rates of women with low income or poor health; however, the differences are statistically insignificant. We also find larger effects on unemployment rates in East Germany than in West Germany, which is consistent with the fact that unemployment rates were higher and early retirement was more prevalent in the East. The main distributional effects of the reform result from the persistence of labor market statuses: unemployed or inactive women remained in their respective status while employed women continued being employed. A tentative interpretation of these results is that the reform increased inequality within the affected cohorts.

The paper proceeds as follows: Section 1.2 briefly outlines the pension system and the 1999 pension reform. Section 1.3 derives hypotheses about potential behavioral reactions. Section 1.4 describes the administrative data that are used in our analysis. Section 1.5 describes our empirical strategy and Section 1.6 presents the results of the empirical analysis, including a discussion of the heterogeneity of the results across subgroups. Finally, Section 1.7 concludes.

1.2 Institutional background

In this section, we provide an overview of the German public pension system and its different early retirement programs. The 1999 pension reform, which this paper focuses on, is explained in detail. In addition, we discuss interactions of the pension system with other social security programs (unemployment and disability pensions) and highlight potential program substitution patterns.

1.2.1 The German public pension system

The statutory public pension system covers most private and public sector employees. It provides old-age pensions, disability pensions, and survivors benefits. The system is financed by a pay-as-you-go (PAYG) scheme. The calculation of pension benefits is based on a point system that takes into account the entire earnings history and insurance record of each individual. A year's contribution at the average earnings of contributors earns one pension point. Moreover, pension points can be acquired during other insurance periods (e.g. unemployment, child raising and while providing informal care). Pensions are roughly proportional to an individual's average lifetime labor income and feature few redistributive properties.[1]

Depending on the length of the insurance record and other qualifying conditions, the age at which pension benefits can be claimed lies between 60 and 65.5 for the cohorts under study (1951–1952). In addition to the regular pension, which requires 5 years of contributions, until 2012 there were four early retirement programs with different qualifying conditions:

1. Pension for women
2. Invalidity pension
3. Pension after unemployment or old-age part-time work
4. Pension for the long-term insured

The first two of these programs allowed retirement starting from 60 years of age, the *pension for women* and the pension for people with severe disability status (*invalidity pension*). The NRA, the age at which full benefits can be claimed, was different between these programs. The NRA of the pension for women and for the invalidity pension was 65 and 63, respectively. Early retirement was associated with actuarial deductions of 0.3 percent per month before the NRA. That is, retiring through the pension for women at age 60 is associated with permanent

[1] Börsch-Supan and Wilke (2004) provide an extended overview of the German pension system.

pension deductions of 18 percent. Deductions amount to only 10.8 percent for people of the same age who are eligible for invalidity pensions. The other two early retirement programs allowed retirement starting from age 63 for the cohorts studied in this analysis. The pension for the long-term insured enables individuals with particularly long insurance records of at least 35 years to retire early.

People who are not able to work due to severe health conditions can retire before the age of 60 through the disability pension program. See Table 1.4 in the Appendix for more details.[2]

1.2.2 The 1999 pension reform

The 1999 reform abolished the early retirement program for women in cohorts born after 1951. Effectively, the reform raised the earliest retirement age for most women to at least 63. Women born before 1952 could claim the pension for women if they fulfilled certain qualifying conditions. The eligibility criteria were: (i) at least 15 years of pension insurance contributions; (ii) at least 10 years of pension insurance contributions after the age of 40. These criteria ensured a minimum labor market attachment of eligible women. Our data show that about 60 percent of all women born in 1951 were eligible for the old-age pension for women. Out of

[2]Note that the German pension system provides two different types of pensions due to impaired health. The disability pension (*Erwerbsminderungsrente*) is similar to disability benefits in the US. Eligibility for full benefits requires that an individual is unable to work more than 3 hours a day for at least six months. Eligibility for partial disability benefits require that the individual is unable to work more than 6 hours a day. Eligibility requires 5 years of contributions. It is the only pension that is available before the age of 60. In addition, there is a second type of old-age pension: the *invalidity pension* is available from age 60 for people with a severe disability status under German law. Invalidity status requires a degree of disability of 50 percent or more and does not require work incapacity. The ERA of this pension has been increased since 2012.

those, about 26 percent retired with age 60 through the early retirement program for women. Take-up was particularly prevalent in East Germany, where most women have a strong labor market attachment and meet the qualifying conditions of this pension type.[3]

Due to the reform, women born in 1952 lose an important option to exit the labor market before age 63. At age 63, people with a long insurance record can retire with deductions. As explained above, the only remaining pension type before age 63 is the invalidity pension. However, even before the 1999 reform, if women had the the choice between the invalidity pension or the pension for women, the former implied lower deductions. In addition, the attainment of invalidity status already entailed several advantages before reaching a pensionable age. In other words, invalidity pension has always been advantageous compared to the pension for women. Therefore, we do not expect large substitution into the invalidity pension due to the reform. For women born after 1951 who want to exit the labor market before age 63, the remaining options are unemployment benefits, disability pensions, or inactivity.

1.2.3 Disability pension, unemployment insurance and inactivity

It is theoretically plausible that some women who would have otherwise claimed old-age pension benefits chose another social support program or withdrew from

[3] The pension due to unemployment or after old-age part-time work was abolished at the same time as the pension for women. However, this does not affect our analysis as the ERA for this pension type was already 63.

the labor force. In the following, we briefly describe the design of unemployment insurance and disability pensions in Germany, focusing on the potential for interdependencies and program substitution.

Unemployment benefits in Germany replace about 60 percent of previous net earnings and increase pension entitlements.[4] Eligibility and the entitlement period depend on the age and the previous working history. The maximum entitlement period for unemployment benefits did not change during our observation period. Specifically, the maximum entitlement period for individuals above the age of 57 was 24 months. Generally, there is a strong interdependence between unemployment benefits and pensions for older individuals. As documented in Grogger and Wunsch (2012), Giesecke and Kind (2013) and Engels et al. (2016), some older individuals use unemployment benefits as a bridge into retirement. In particular, there is evidence that unemployed individuals exhaust their full entitlement period for unemployment benefits before entering retirement. The design of the institution provides strong incentives for this behavior; unemployment benefits are relatively generous, periods in unemployment increase pension entitlements and, lastly, search requirements for unemployed persons close to retirement are very low. Therefore, an increase in the ERA is likely to affect the take-up of unemployment benefits in two ways: first, individuals have an increased incentive to postpone entry into unemployment, if unemployment benefits are indeed used as a pathway to retirement. This would lead to a shift in increased unemployment entry from 58 (cohort 1951) to 61 (cohort 1952) years; 24 months before reaching the cohort-specific ERA. Second, unemployment rates among 60 to 63 year-old

[4]People receiving unemployment benefits acquire pension entitlements as if they earned about 80 percent of their previous gross earnings.

women may increase due to program substitution because of the abolishment of the early retirement option, i.e. because women who want to exit employment between the old and new ERA must take another path to exit the labor market.

The disability pension (*Erwerbsminderungsrente*) is the only pathway to retirement before reaching the ERA. Eligibility requires the long-term (at least six months) inability to perform an activity under normal labor market conditions for at least six hours (partial disability pension) or at least three hours (full disability pension) per day. The pension is calculated based on the previous insurance biography and amounts to the pension that would be paid had the individual continued to work until she turned 60. When reaching the statutory retirement age, the disability pension is converted into an old-age pension usually of the same level. In Germany, health-related eligibility criteria for disability pensions are relatively strict, especially since a 2001 reform. About 40 percent of all applications are rejected. Therefore, using disability pensions as a pathway to a regular old-age pension is difficult and not typically an attractive option. Moreover, since 2001, actuarial deductions also apply to this type of pension. The pension is permanently reduced by 0.3 percent per month if retiring before the NRA. In 2012, the NRA of disability pensions was increased from 63 to 65, with deductions capped at a maximum of 10.8 percent. Virtually all of these pensions are reduced by maximum deductions since most people claim this pension before turning 60 (Deutsche Rentenversicherung, 2015, p.83).

Individuals who are neither eligible for disability pension nor unemployment benefits may choose inactivity, i.e. exit the labor force without benefit receipt. This is particularly relevant for women, who are often not the primary earner in their households.

1.3 Expected reform effects

The reform of 1999 is expected to have several effects on employment outcomes. We expect women born after 1951 to extend employment and to delay retirement entry compared to women unaffected by the reform. Since the reform did not just increase the penalty for early retirement but abolished the option altogether, we expect large effects on employment rates of women aged 60 to 63. However, not all women are able or willing to work until reaching the new ERA. Therefore, we expect increased program substitution, which is the response to similar reforms in other countries (see Staubli and Zweimüller, 2013; Atalay and Barrett, 2015).

Following Oguzoglu et al. (2016), we distinguish between two different kinds of effects: active and passive (mechanic) program substitution. Women who do not have an option to retire at 60, even though they would have retired in the absence of the 1999 reform, must divert into another employment status or continue their previous status. The continuation in a social security program due to the lack of a retirement option can lead to a *passive* increase in e.g. unemployment and inactivity rates. In contrast, *active* program substitution refers to flows from employment into other social security programs as a response to the elimination of the retirement option. In order to distinguish between passive and active program substitution, we analyze employment outflows into other social security programs around age 60. There are mainly the two aforementioned social security programs that are relevant in this context: unemployment insurance and disability pensions (explained in detail in Section 1.2.3). As a third option, we consider potential increases in inactivity rates.

There are several reasons why we expect the 1999 reform to affect employment outcomes before the former ERA of 60. First, women born after 1951 who have a preference for early retirement around the age of 60, may choose alternative pathways to exit employment, perhaps even before their 60^{th} birthday, instead of delaying until they reach the ERA. The ERA can serve as a reference age for retirement decisions (see Seibold, 2016), which leads to bunching of retirement entries at the ERA, and a reduced number of exits among individuals approaching the reference age. Consequently, an ERA increase may lead to an increase in employment exits of women approaching age 60. We refer to this *un-bunching*-effect as the *reversed reference-age effect*.

Second, women may bridge the last one or two years prior to reaching the ERA with unemployment. As explained in the previous section, German workers have an incentive to exit employment two years before reaching the ERA through unemployment benefits, which are paid for up to 24 months to older workers. An increase in the ERA leads to a shift in the bridge period. If women adjust their employment behavior and delay their (bridge into) retirement, we expect a negative effect on unemployment and disability pension rates for women approaching age 60, in particular among 58 and 59 year-old women. Instead we would expect women born after 1951 to enter unemployment at 61 or 62 years of age more often.

Third, the ERA increase was announced in 1999, while it only affected women turning 60 in 2012. Consequently, the ERA increase was known when the first affected cohort was 47 years old. It is not obvious *a priori* how younger women will adjust their labor supply in a response to the increased ERA. The reform can be interpreted as a strong negative wealth shock as it reduces the length of time that women receive pension benefits and thereby the total social security wealth. Theoretically, this should have a positive effect on labor supply. If these women were forward looking, they might have adjusted labor supply as soon as the legislation was passed. On the other hand, the ERA increase may discourage women from working because the same level of pension benefits can be achieved with fewer contributions in younger years, due to the extended contribution period.

Fourth, the eligibility criteria of the pension for women (namely after 15 years of pension contributions) are no longer relevant for cohorts born after 1951. However, early retirement at 63 requires the completion of a contribution period of 35 years, including child raising periods. It is not clear how the change in eligibility criteria for early retirement affects the labor supply of women between 47 and 59. For example, a woman in the 1951 cohort with 14 years of contributions has a strong incentive to work at least one additional year to qualify for early retirement. The incentive to accumulate 35 contribution years is higher for cohorts born after 1951.[5] As documented in Seibold (2016) bunching around these thresholds is present, but it is very small and we do not expect strong behavioral reactions to the changed incentives of the qualifying conditions. In fact, the vast majority of women who qualified for the pension for women were also eligible for long-term insured pension.

[5]There is a large overlap between women who fulfill the criteria of the pension for women and those who fulfill the eligibility criteria for early retirement for the long-term insured. A graphic representation of the distribution of pension contribution years is displayed in Table 1-9 in the Appendix.

1.4 Data

We use high-quality administrative data from public pension insurance accounts (VSKT: *Versicherungskontenstichprobe*).[6] The VSKT is a stratified random sample of all pension insurance accounts of people aged 30 to 67. If the appropriate sampling weights are used, the VSKT is representative of the German population of public pension insurance accounts. Since the data are process-produced, recall errors due to memory gaps and wrong temporal assignment are avoided, while panel mortality is negligible (Fachinger and Himmelreicher, 2006). Furthermore, individual employment behavior and retirement entry is reported with monthly accuracy. A drawback is that socio-economic variables are only recorded to the extent that they are relevant for the calculation of pension benefits. Consequently, information on education is missing in about half of the cases. Information about occupations is only available for the last occupation at the time of data collection, which may not be representative of entire employment histories. Furthermore, it is not possible to link spouses and other household members within the data.[7]

For our analysis we use the VSKT of 2014, the latest available wave at the time of analysis.[8] We restrict the sample to women born in 1951 and 1952. Furthermore, we exclude all women who paid contributions to a special miners' pension scheme (*Knappschaftliche Versicherung*) for at least one month, which applies to about 10 percent of all women. Another group excluded from our sample (about 6 percent of all women) consists of all women receiving an old-

[6]We use the full VSKT, which is roughly four times as large as the scientific-use file SUFVSKT and can only be accessed on-site.
[7]A detailed description of the data can be found in Himmelreicher and Stegmann (2008).
[8]We plan to extend the analysis when newer waves become available.

age invalidity pension at some point in their life. We argue that the invalidity pension is always superior to the pension for women since it is associated with lower pension deductions. Consequently, the group of women who claims invalidity pension benefits should not be affected by the 1999 reform. After dropping these groups, we are left with 7,365 women.[9]

The 1999 pension reform increases the ERA only for women who were eligible for the pension for women. Women who did not meet the qualifying conditions, are not likely to be eligible for any early retirement program. In our analysis, we therefore focus on women who fulfill the eligibility criteria for the pension for women.[10] Those criteria are the accumulation of at least 15 years of pension contributions, with 10 years after turning 40. About 60 percent of the women in our sample fulfill these eligibility criteria. Due to the traditionally stronger labor market attachment of women in East Germany, the share of eligible women amounts to more than 80 percent for East German women. Our final sample consists of 3,771 women who fulfill the eligibility criteria of the pension for women. About 30 percent of the eligible women in the sample (cohort 1951) retire early through the early pension for women before their 62^{nd} birthday.

The main variables of interest in this analysis are whether or not an individual is employed, unemployed, inactive, or receiving a disability pension at

[9]We account for regional differences in the empirical analysis since the employment behavior of women in East and West Germany differs markedly. About 17 percent of the sample collected most of their pension contribution points in East Germany. The assignment of a region to individual employment histories is straightforward because very few women in the relevant cohorts earned non-negligible pension points in both East and West Germany.

[10]We discuss potential bias due to sample selection in the Appendix. Furthermore, we include an analysis using a sample of all women regardless of pension eligibility in Table 1.10 and Table 1.11 in the Appendix.

any given age in months.[11] A woman counts as employed if she had a job which was subject to social security contributions or if she was marginally employed.[12] The best approximation of *inactivity* is the residual category, which comprises all statuses that are not employment, unemployment, or pension receipt. The residual category includes periods of education or training, insured self-employment, non-commercial care for children or elderly family members, illness, and unknown status (missing value). By far the largest group within the residual category consists of women with missing employment status. We refer to the residual category as *inactivity* in the remainder of this paper. These women could, in principle, be working as uninsured self-employed or as civil servants since these statuses are also recorded as missing values. However, we assume that this is unlikely in the sample we select: women older than 58 who qualify for the pension for women. That is, women who paid 10 years social security contributions after their 40^{th} birthday.[13]

In order to analyze heterogeneous effects by income and health, we use information on average pension points and periods of sick-pay to approximate income groups and health status. A woman is defined to belong to the low income group if she is in the lowest third of the distribution of average pension points

[11] We define disability pension periods as months with pension receipt before reaching the ERA; and by using the pension-type information for current pension spells. If a disability pension is converted into an old-age pension, the months of old-age pension receipt are re-coded as disability pension periods.

[12] Marginal employment (*geringfügige Beschäftigung*) is defined as a tax-free employment-relationship with earnings under a certain threshold (until 2013 up to €400 per month, since 2013 €450 per month).

[13] Note that *inactivity* is used here to describe the status of out of the labor force and of the social security system. Therefore, it includes e.g. housework or care for a family member, which should not be misinterpreted as leisure or idleness.

over all full contribution periods.[14] The low income group is defined by having accumulated on average less than 0.52 annual pension points. That is equivalent to 52 percent of average earnings or €18,126 for a West German woman in 2014.[15] Note that we use individual pension points, which are based on individual earnings. We approximated poor health using periods of sick-pay, which are only recorded if the sick leave exceeds six weeks or entails hospitalization for employed individuals. A person is defined as having a poor health status if she has at least one sick-pay spell between age 45 and 55, which holds true for about 26 percent of the sample of eligible women.[16]

1.4.1 Descriptive evidence

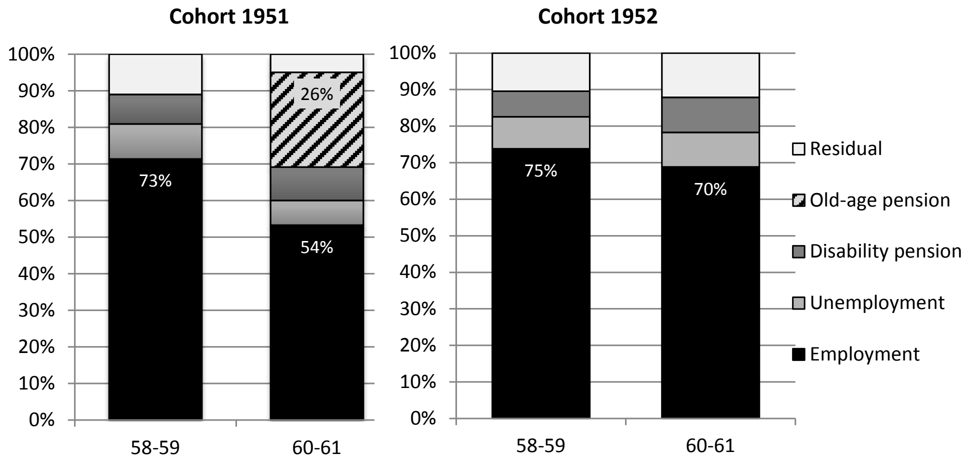
The distribution of employment status by age group is displayed in Figure 1-1 for the 1951 and 1952 cohorts. The employment rates of 58 and 59 year-old women are relatively high due to the sample restriction to women eligible for the pension for women. It shows that a large fraction (26 percent) of women born in 1951 receive an old-age pension from their 60^{th} birthday onward. This fraction disappears if we look at the 1952 cohort, due to the ERA increase. Employment, unemployment, and inactivity rates increased for 60 and 61 year-old women born in 1952 (treatment group) compared to women born in 1951 (control group). In particular,

[14] We used the pension points (*avegpt*) earned over full contribution periods only (*byvl*). Points earned in the East and West are treated equally; however, percentiles are constructed separately for East and West German women, using all women (not just eligible women).

[15] 18,126 = 0.52 x 34,857, where 34,857 was the average gross earnings of all insured individuals in West Germany in 2014.

[16] Employed women are more likely to receive sick-pay, therefore, the sub-sample of women with poor health is likely to be employed between 45 and 55.

Figure 1-1: Employment status by age group and cohort, sample of eligible women



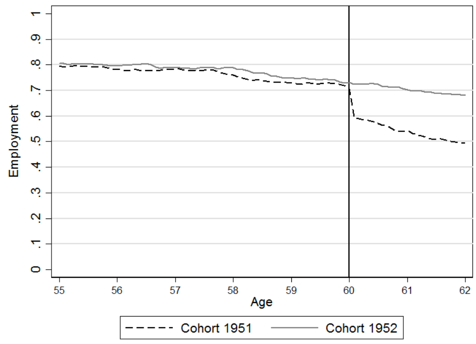
Source: VSKT 2014, own calculations.

the employment rate amounts to 70 percent in the cohort 1952, compared to 54 percent in the 1951 cohort.

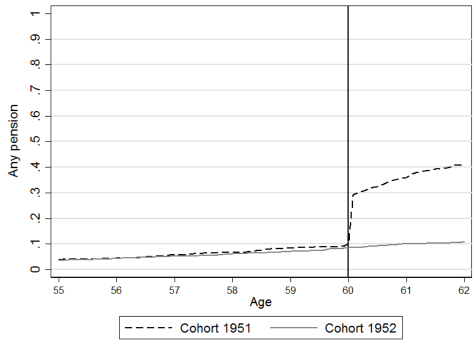
A closer look at the fractions of women in different employment status across age reveals that women born in 1951 exhibit a large drop in employment rates when reaching age 60, while this discontinuity is not observed for the 1952 cohort (Figure 1-2a). Not surprisingly, the fraction of women receiving a pension (including disability pensions) increases sharply at the ERA for the 1951 cohort (Figure 1-2b). There is no visible difference in employment and retirement rates between cohorts before reaching age 60.

Figure 1-3a and Figure 1-3b show that the fraction of women in marginal employment and the unemployment rate are slightly lower for the 1952 cohort for all ages between 55 and 60. At age 60 however, we observe a drop in marginal

Figure 1-2: Employment and pension recipient rates by age and cohort



(a) Employment rate

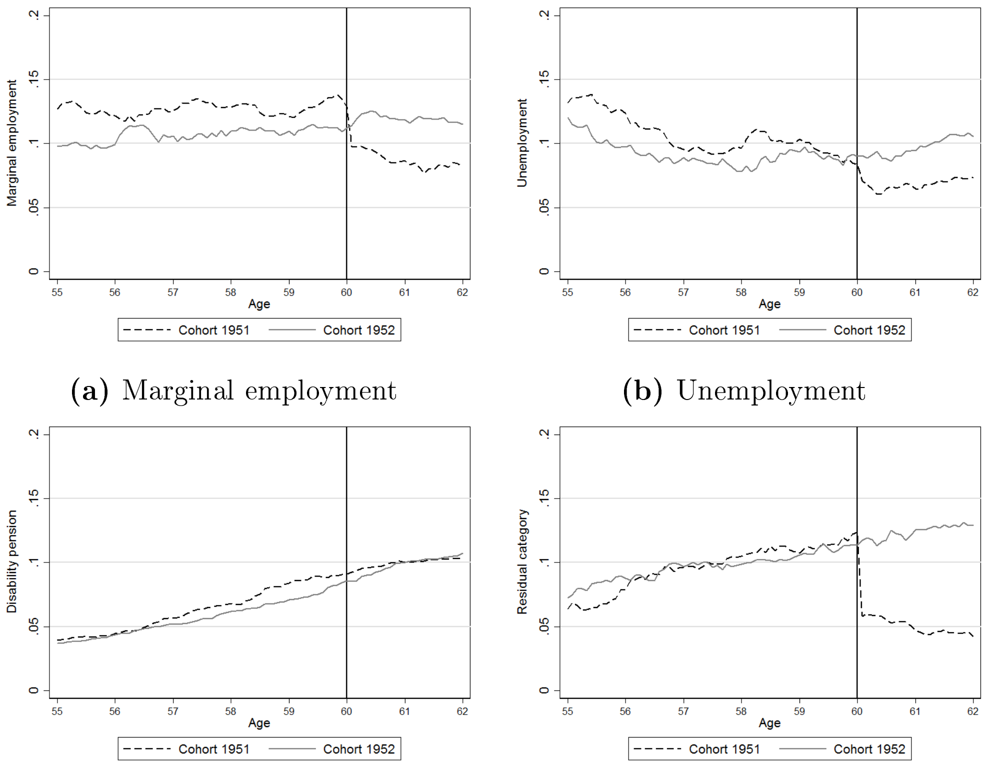


(b) Pension recipients

Notes: Employment includes marginal employment.
Source: VSKT 2014, own calculations.

employment and unemployment rates of the 1951 cohort. From age 60 to 62, the 1952 cohort is more likely to be marginally employed and unemployed. It can be seen in Figure 1-3d that the fraction of inactive women also drops sharply when reaching age 60, indicating that a large share of women who were previously inactive start receiving the pension for women. The fraction of women receiving a disability pension increases continuously with age for both cohorts (Figure 1-3c). It can also be observed that women born in 1951 are slightly more likely to receive a disability pension (Figure 1-3c), in particular between 57 and 61 years of age. Note that differences between cohorts and fluctuations over time can be due to e.g. time trends or macroeconomic shocks. However, our empirical identification strategy is not threatened as long as differences between cohorts are continuous over the month of birth (see Section 1.5 for a detailed description of our empirical strategy.)

Figure 1-3: Employment status by age and cohort



(a) Marginal employment **(b)** Unemployment

(c) Disability pension **(d)** Inactivity

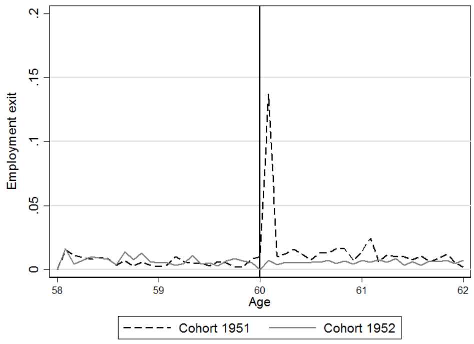
Notes: The inactivity category combines all status except (marginal) employment, unemployment, old-age pension, or disability pension receipt.
Source: VSKT 2014, own calculations.

While the sharp decrease in the proportion of women born in 1951 in several employment categories suggest an outflow into early retirement, an analysis of employment outflows is needed to gain further insights on employment exit behavior and potential program substitution effects. In particular, we cannot infer from Figure 1-3 whether the reform led to increased inflow into unemployment, disability pension, or inactivity from employment.

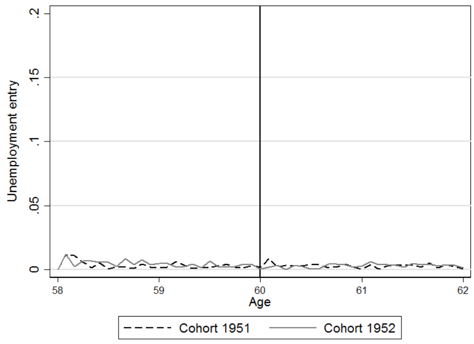
Employment outflows are displayed in Figure 1-4a to Figure 1-4d. The employment exit hazard rate is defined as the fraction of women exiting employment at age t, conditional on survival in employment (excluding marginal employment) up to age t, out of all women who were employed for at least six months when reaching age 58. Unemployment, disability pension, and inactivity entry rates are defined as the probabilities to enter the respective category conditional on having survived until t, and employment (including marginal employment) for at least six months at their 58^{th} birthday. Note that we do not condition on employment between age 58 and the first unemployment, disability pension or inactivity entry event. We only consider the first exit or entry, i.e. reentering the sample is not possible. It can be seen in Figure 1-4a that the employment exit hazard peaks at age 60 (one month after the 60^{th} birthday) and, to a lesser extent, at age 61 for the 1951 cohort, while the hazard remains flat for the 1952 cohort.

If women in our sample used 24 months of unemployment benefits receipt as a bridge to retirement, we would expect a peak in unemployment entry at age 58 for the 1951 cohort, and at age 61 for the 1952 cohort – or at least higher entry rates in the two years before reaching the ERA. With respect to active program substitution due to the pension reform, we expect increased entry into unemployment and disability for the 1952 cohort at around age 60. However, neither are observed in Figure 1-4b. Therefore, it would be surprising if we discovered a large shift in unemployment entry or increased program substitution in the regression discontinuity analysis. The entry rates into disability pension and inactivity do not exhibit notable peaks, nor are there observable differences between cohorts (Figure 1-4c and Figure 1-4d).

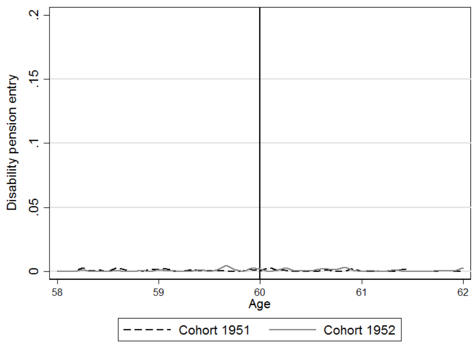
Figure 1-4: Employment exit and entry rates into other status by age and cohort



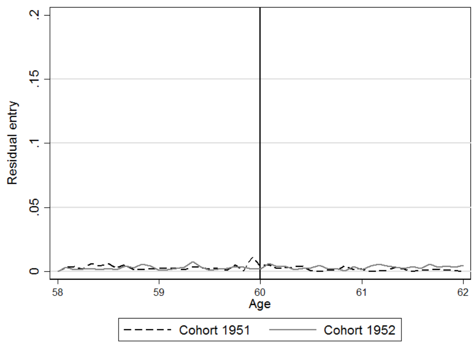
(a) Employment exit



(b) Unemployment entry



(c) Disability pension entry



(d) Inactivity entry

Source: VSKT 2014, own calculations.

These descriptive results suggest that there is no increased substitution from employment into unemployment, disability pension programs or inactivity due to the ERA increase. However, the hazard rates displayed here are descriptive only. At each age, the population that *survives* in employment is selective, based on previous hazard rates. Therefore, one cannot interpret the differences in hazard rates between cohorts in a causal sense. A more rigorous empirical analysis, described in the following section, is necessary to assess whether the ERA increase led to extended employment and substitution into unemployment and disability pension programs or inactivity around the former ERA.

1.5 Empirical strategy

The empirical identification of the effect of pension eligibility rules on labor supply and retirement behavior is challenging: employment histories and unobserved preferences for work and leisure affect both labor supply in old-age as well as eligibility for early retirement. One way to circumvent this endogeneity problem is to exploit exogenous variation in the pension system over time or cohorts due to policy changes. Our empirical strategy makes use of the 1999 pension reform, which eliminates the option to retire at age 60 for women born in 1952 and thereafter.

In the first part of our empirical analysis, we employ a linear regression discontinuity research design to estimate the causal effect of an increase in the ERA on employment rates, unemployment rates, the fraction of older women receiving a disability pension, and the fraction of inactive women. The regression discontinuity design solves the endogeneity problem by exploiting variation in the ERA by month of birth. It is valid, if we can assume that labor supply at a given age would be continuous over the month of birth in absence of the 1999 reform.

The research design is implemented by the following empirical model:

$$y_{it} = \alpha + \beta D_i + \gamma_0 f(z_i - c) + \gamma_1 D_i f(z_i - c) + X'_{it}\delta + \epsilon_{it} \tag{1.1}$$

Where the indicator $D_i = 1$, if the individual was born after January 1952. The subscript t refers to age in months and ranges from 721 to 744 (age 60 to 62) in the baseline specification. The month of birth z_i enters the empirical model in difference to the reform cutoff c, which is January 1952. In our baseline specifications, we include a linear trend in the running variable, $f(z_i - c) = z_i - c$. The specification allows for different slopes before and after the cutoff. All regressions include calendar month fixed effects, and dummies for three income groups, children, and region, summarized in X_{it}. However, dropping X_{it} does not change the point estimates (see Table 1.12 in the Appendix for regression results without covariates). Regression discontinuity analyses are naturally prone to model misspecification. A non-linearity in outcomes may falsely be interpreted as a discontinuity if it is unaccounted for. Therefore, we report linear regression results both with linear and quadratic trends in the running variable (RDD results with quadratic trends are displayed in Table 1.14 in the Appendix). Furthermore, we support our analysis by graphical analyses of local linear regression plots.

Employment status data are recorded for each individual at every age in months t. Therefore, we need to specify a time-window for the outcome variables of interest. In our baseline specification, we pool all observations from the month after the 58^{th} birthday to the 60^{th} birthday (*age 58-59*), and all observations from the month after the 60^{th} birthday to the 62^{nd} birthday (*age 60-61*).[17] In order to account for correlation between observations for the same individual or individuals born in the same month, we cluster standard errors by month of birth. The baseline specification allows us to estimate treatment effects for four outcome variables for

[17] In order to have equal treatment and control groups, we do not include observations after their 62^{nd} birthday, which are only available for the older women in the sample.

two age groups before and after age 60. However, it may be of interest to estimate a more flexible model that allows for heterogeneous effects for every age in months t. Consequently, we analyze the reform effects for every age in months separately by including age-treatment interactions into our empirical model. The inclusion of age-dummies and interactions with the treatment variable $D_i = 1$, allows us to interpret the coefficient of the interaction term as the reform effect on a specific age group (see Section 1.6.2).

In the second part of the empirical analysis, we focus on active program substitution due to the ERA reform. In more detail, using a sub-sample of women who were employed on their 58^{th} birthday, we estimate outflows from employment, and inflows into unemployment benefits, disability pension, and inactivity (see Section 1.6.4). If we look at the effects on the shares in different employment categories only, as in Staubli and Zweimüller (2013) and Atalay and Barrett (2015), we cannot distinguish between passive and active program substitution. In contrast, an analysis of employment outflows allows us to answer the question whether women increasingly used alternative social security programs to exit employment in response to the abolishment of the early retirement option.[18] We circumvented the dynamic selection problem by conditioning on employment at a fixed age in months. Formally, we estimate the same regression discontinuity model as described in Equation 1.1; however, the outcome of interest is the probability to exit employment (into unemployment or disability pension programs) within the following 2-4 years, conditional on employment for at least six months

[18] A drawback of survival data is that if one compares the hazard rates of two groups over time, the group composition changes if one group has a higher exit probability. This is called the dynamic selection problem. Consequently, we cannot estimate treatment effects by comparing the difference in hazard rates between cohorts over age in months.

at the 58th birthday. Conditioning on employment at certain age is problematic if it is itself an outcome that is potentially affected by the reform. However, we can show that there is no discontinuity in the employment rate at the sample entry age of 58 (Figure 1-7 in Section 1.6.4). Consequently, we argue that treatment effects on flow variables can consistently be estimated using the linear RD approach described above.

1.5.1 Threats to identification

The RD design is only valid if women cannot manipulate the treatment assignment variable (Lee and Lemieux, 2010), which is the month of birth in our research design. Evidently, it is impossible that women or their parents manipulated the date of birth in anticipation of the policy change, as the reform was introduced long after the cohorts in question were born. Furthermore, we are not aware of any changes in the incentive to give birth in 1951 as opposed to 1952.[19]

One of the most important assumptions of our analysis is that any discontinuities in the outcome variables at the cutoff are solely due to the 1999 pension reform. In particular, we need to assume that the differences between the cohorts in question are not caused by other policy changes. Two other pension policy changes also became effective for individuals born after January 1^{st} 1952. First, the old-age pension for the unemployed was abolished for all individuals born after 1951 as part of the 1999 pension reform. However, the ERA for this pension was already at 63. Therefore, this change did not affect women at age 60. Second, the

[19] It can be shown that the number of observations is relatively stable across all months of birth.

ERA of the invalidity pension program was increased from 60 to 63 in monthly steps starting with individuals born in January 1952. We exclude all women who received an invalidity pension because the ERA for the invalidity pension was also changed for the same cohorts as for the pension for women. It can be assumed that women eligible for either pension will choose the invalidity pension due to the significantly more generous pension benefits. Nevertheless, excluding all women who received an invalidity pension may induce a selection bias because women born earlier are older at the point of data collection and, therefore, are more likely to receive an invalidity pension. However, inflow rates are so small that we do not expect this to be a problem.

Even in the absence of other reform changes, women born in 1952 may still be different from women born earlier due to time trends in employment outcomes. Employment rates of women have been increasing over the past decades for every age. Including linear or quadratic trends in birth-dates should resolve this issue in an RD research design, as long as we can assume that women who were born close to the cutoff are not different from each other. This is tested by checking for discontinuities in covariates, using the same regression discontinuity framework. Results from the test for covariate discontinuities are displayed in Table 1.7 in the Appendix. We do not find significant discontinuities in covariates that are not inherently influenced by the 1999 reform. Furthermore, we perform a difference-in-discontinuities analysis in order to test whether our results are caused by a *turn of the year effect*. Reassuringly, the results of the difference-in-discontinuity analysis, displayed in Table 1.8 and Table 1.9 in the Appendix, do not differ significantly from our baseline results.

Another concern arises due to the selection of the sample by the eligibility criteria of the pension for women. Specifically, women born in 1951 may select into the sample by extending their pension contribution period in order to be eligible for early retirement. In contrast, women born in 1952 do not have the same incentives to fulfill the eligibility criteria. We discuss the problem of sample selectivity in the Appendix. We argue that the potential bias due to selection is negligible because there was no change in the fraction fulfilling early retirement eligibility criteria due to the reform. We repeat the analysis without sample restrictions using all women born 1951 and 1952 regardless of pension eligibility criteria. The results are displayed in Table 1.10 and Table 1.11 in the Appendix.

1.6 Results

1.6.1 Baseline results

The results of the linear regression discontinuity analysis are displayed in Table 1.1. Figure 1-5a to 1-5d visualize the results using local linear regression on both sides of the cutoff, a triangular kernel, and a bandwidth of 12 months.

The increase in the ERA had an average positive effect of 14.4 percentage points on the employment rate of 60 and 61 year-old women (see Column 1, Table 1.1). The coefficients can be interpreted as the average percentage point change in employment rates of all women in this age group due to the pension reform. Compared to the pre-reform mean the relative increase amounts to more than 26 percent. The fraction of women receiving a pension mechanically dropped by 25 percent percentage points to zero (Column 5). About 58 percent of those women, who would have retired if they had the option, continue to work due to the reform. The remaining women split equally in unemployment and inactivity.

In addition to the effects on employment rates, we estimate the effects of the ERA increase on the unemployment rate (Column 2), the fraction of women receiving a disability pension (Column 3), and the fraction of inactive women (Column 4). The unemployment rate and the fraction of women in the inactive category increased significantly by 5.2 percentage points on average due to the ERA increase. The positive effect on the unemployment rate can be due to either a passive increase in the unemployment rate or an active program substitution from employment into unemployment. The zero effect on disability pension participation rates suggests that there is no program substitution into the disability pension program. i.e. the disability pension program is not used as an alternative pathway to enter retirement.

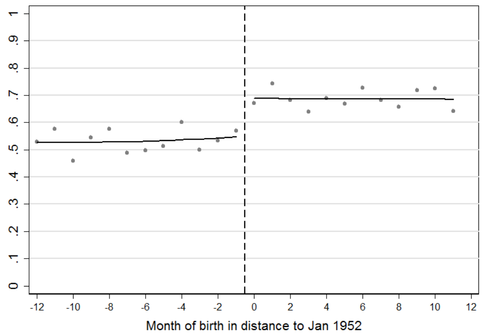
Our results suggest that the linear trend in month of birth does not affect the outcome on either side of the cutoff. Whether or not a woman has children does not enter the regression significantly. Women in West Germany are more likely to be employed, to receive a disability pension, or to be inactive, while East German women are more likely to be unemployed. Note that the results do not change if we drop all covariates (see Table 1.12 in the Appendix). Including a quadratic function of month of birth does not alter the result considerably (see Table 1.14 in the Appendix).

Table 1.1: Linear regression results, age 60-61

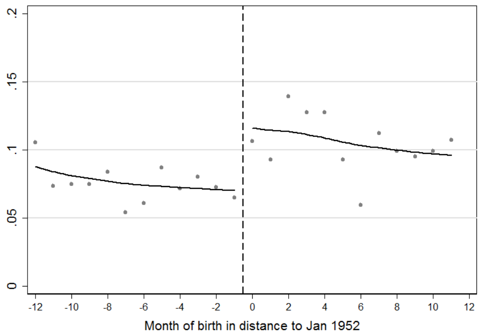
	(1) Employment	(2) Unemployment	(3) Disability	(4) Inactivity	(5) Pension
D_i	0.144***	0.052***	-0.004	0.052***	-0.249***
	(0.0271)	(0.0111)	(0.0232)	(0.0123)	(0.0270)
mob_i	0.002	-0.002	-0.001	0.001	-0.001
	(0.0029)	(0.0013)	(0.0020)	(0.0010)	(0.0026)
$D_i * mob_i$	-0.003	0.001	0.003	0.001	0.003
	(0.0040)	(0.0016)	(0.0029)	(0.0018)	(0.0033)
Children	0.010	-0.013	-0.006	-0.017	0.000
	(0.0164)	(0.0119)	(0.0132)	(0.0128)	(0.0142)
West	0.051**	-0.067***	0.022*	0.029**	-0.018
	(0.0206)	(0.0125)	(0.0109)	(0.0114)	(0.0161)
Constant	0.380***	0.181***	0.117***	0.074***	0.409***
	(0.0328)	(0.0167)	(0.0278)	(0.0206)	(0.0379)
N	3,771	3,771	3,771	3,771	3,771
R^2	0.058	0.037	0.005	0.018	0.090
Pre-treatment mean	0.538	0.068	0.092	0.050	0.262
*** p<0.01, ** p<0.05, * p<0.1					

Notes: Robust standard errors in parentheses. All linear regressions include calendar month fixed effects, income group dummies, and linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT 2014, own calculations.

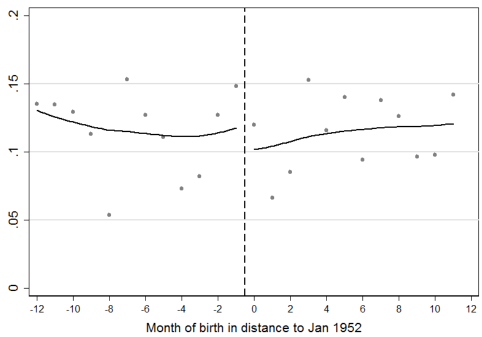
Figure 1-5: Local linear regression plots, age 60-61



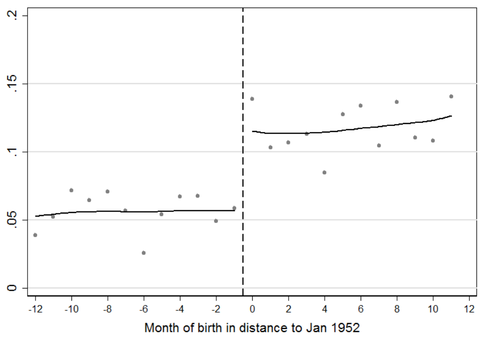
(a) Employment



(b) Unemployment



(c) Disability pension



(d) Inactivity

Notes: Scatter plots display mean outcome values using monthly bins. Local linear regression plots are based on triangular kernel functions with a bandwidth of 12 months.
Source: VSKT 2014, own calculations.

1.6.2 Effects across the age profile

In order to shed more light on the effects of an ERA increase on employment outcomes at different ages, we interact the entire age profile (in months) with the right hand side of our regression equation. Thereby, we allow for heterogeneous treatment effects by age in months. The resulting coefficients by age in months are displayed in Figure 1-6a to Figure 1-6d. The results suggest the effect on employment rates of 60 to 62 year old women is positive and increasing with age. The gradual increase after age 60 is due to pre-reform pension entry past age 60. As expected, we can observe a positive effect on unemployment and inactivity rates from age 60 onward due to the elimination of the option to retire early. Our results suggest that the fraction of women receiving a disability pension did not increase for any age-group.

There are several reasons why we expect the ERA increase to have an effect on employment outcomes before the former ERA of 60. The results of our baseline linear regression analysis on employment outcomes of 58 and 59 year-old women are displayed in Table 1.5 in the Appendix. As described in Section 1.3, we expect a decrease in the unemployment rate of 58 to 60 year-old women and an increase in unemployment rates for 61 year-old women, if women are bridging the last 24 months before retirement entry with unemployment benefits. However, we do not find evidence for bridging behavior (Figure 1-6b). The pooled regression for women aged 58 and 59, displayed in Table 1.5, confirms that there is no significant increase in unemployment rates for this age group. However, we can see in Figure 1-6b that there is a small positive effect on the unemployment rate of some age-groups approaching age 60, which is consistent with a reversed reference age effect.

Furthermore, we do not find evidence for cohort differences in employment and inactivity rates before age 60, as shown in Figures 1-6a, and Figure 1-6d. We conclude that, even though the ERA increase was long anticipated, there was little or no adjustment in labor supply in anticipation of the ERA increase.

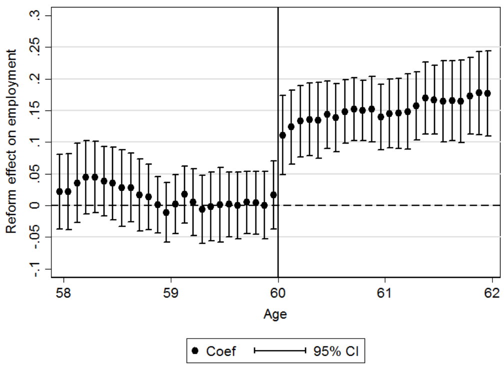
1.6.3 Effect heterogeneity by subgroups

In order to understand the impact of the ERA increase, it is necessary to learn more about the group affected by the reform. A comparison of women who retire early with those who retire with 62 or later, displayed in Table 1.16 in the Appendix, provides insights on the characteristics of the group affected by the pension reform. Women who retire early have fewer pension points on average, i.e. lower average earnings. The sum of pension points, contribution points after age 40, and the total contribution period are also lower for early retirees, which is not surprising due to the shorter working lifetime. Furthermore, women who retire early are more likely to have poor health. Women who retire late are more likely to be employed and less likely to be unemployed when they reach age 60.

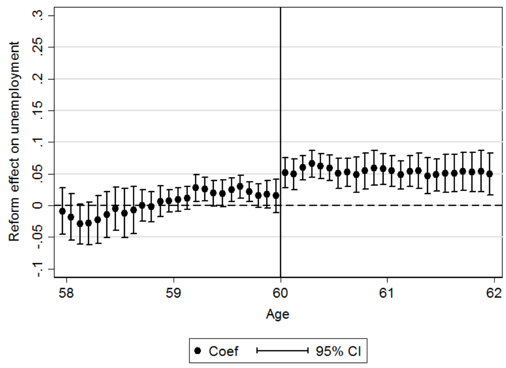
If women who make use of the early retirement option differ from those working longer, we expect the abolishment of the early pension for women to have heterogeneous effects on different subgroups. Therefore, we split our sample into several sub-samples to evaluate whether the reform had heterogeneous effects. In particular, we distinguish between East and West Germany.[20] Furthermore, we

[20] A woman is defined as West German if she collected the majority of her pension contribution points in West Germany.

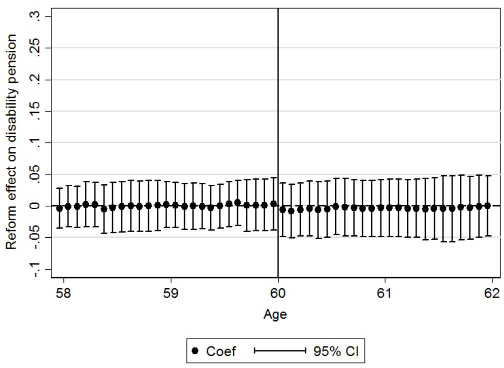
Figure 1-6: Coefficients of ERA increase by age in months



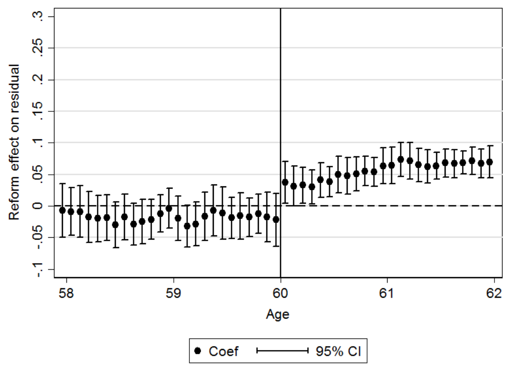
(a) Employment



(b) Unemployment



(c) Disability pension



(d) Inactivity

Notes: The coefficients of the treatment dummy interacted with the age profile are estimated using a linear regression model including age fixed effects, linear trends in month of birth and the interaction with age in months, calendar month fixed effects, income groups, and a dummy for West Germany. Confidence intervals of clustered standard errors are displayed using error bars.
Source: VSKT 2014, own calculations

distinguish women with low income, poor health and women with and without children.[21]

The results for the analysis of different subgroups are displayed in Table 1.2 (and Table 1.6 in the Appendix for 58-59 year-old women). Women in East Germany are much more likely to be eligible for the woman's pension. Consequently, we find larger, although not significantly, employment effects for East Germany than for West Germany. While the reform effect on unemployment rates of 60 and 61 year-old women is negligible in West Germany, there is a large positive effect of about 15 percentage points on the unemployment rate of women in East Germany. This is likely to be due to larger overall unemployment rates in the East.

[21] Low income is defined by the lowest third of the distribution of pension points collected in full contribution periods. Poor health is defined as having at least one sick-pay spell from age 45 to age 55. Note that women who received sick pay are more likely to be employed or unemployed than inactive. Due to data limitations, we cannot divide the sample into married and unmarried women, even though this would be another sub-group analysis of interest.

Table 1.2: Subgroup analysis - linear regression results, age 60-61

	(1) Employment	(2) Unemployment	(3) Disability pension	(4) Inactivity	N
Baseline	0.144***	0.052***	-0.004	0.052***	3771
	(0.0271)	(0.0111)	(0.0232)	(0.0123)	
West Germany	0.124***	0.015	0.007	0.062***	2727
	(0.0430)	(0.0147)	(0.0283)	(0.0197)	
East Germany	0.184**	0.149***	-0.028	0.026	1044
	(0.0675)	(0.0375)	(0.0381)	(0.0212)	
Low income	0.178***	0.028	-0.032	0.067**	1046
	(0.0443)	(0.0251)	(0.0304)	(0.0310)	
Poor health	0.159***	0.045**	-0.008	0.051*	988
	(0.0512)	(0.0206)	(0.0669)	(0.0252)	
Children	0.144***	0.053***	0.005	0.042**	3198
	(0.0274)	(0.0140)	(0.0245)	(0.0156)	
No children	0.152***	0.039	-0.075	0.099***	573
	(0.0446)	(0.0308)	(0.0472)	(0.0291)	

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors in parentheses. All linear regressions include calendar month fixed effects, income group dummies, and linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT2014, own calculations.

We expect women to suffer disproportionately by an ERA increase, if they have a stronger preference to retire early than the average population. Retirement incentives with respect to income groups are not unambiguous due to income and substitution effects. We find slightly larger effects on employment rates for the subgroup of women with low average earnings. We do not find a significant increase in unemployment for this group.

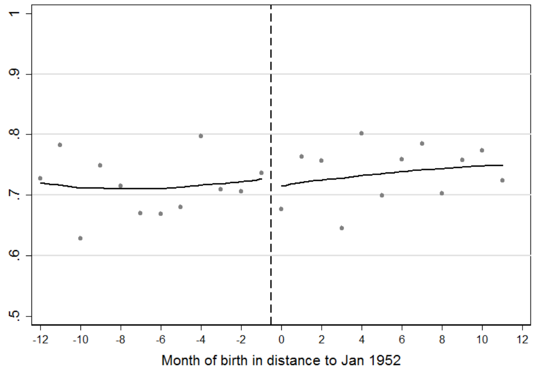
Women with poor health can be expected to have strong preferences for early retirement and inelastic labor supply at high ages. Consequently, we expect women with poor health to shirk into alternative employment-exit paths when the ERA is increased. In particular, we expect larger unemployment rates and an increase in disability pension participation rates. Our results show that the disability pension rates did not increase for any subgroup as a response to the ERA reform. Among women without children, the effect on inactivity rates is larger than for the whole sample including women with children.

Overall, we conclude that the ERA increase affected certain groups heterogeneously. Women in East Germany are more affected than women in the West. In particular, unemployment rates of 60 to 62 year-old women increase more in East than West Germany. Furthermore, we find suggestive evidence for slightly higher employment effects for 60 and 61 year-old women with low income, poor health and women without children.

1.6.4 Employment outflows and program substitution

The results described in the previous sections suggest that the ERA increase led to increased program substitution into unemployment. Furthermore, we find evidence for increased inactivity of 60 to 62 year-old women as a response to the reform. This could be caused by passive (women remain in their respective labor market status) or active substitution from employment into unemployment or

Figure 1-7: Test for discontinuity in employment rate at 58^{th} birthday



Notes: The scatter plot displays mean outcome values using monthly bins. The local linear regression plot is based on triangular kernel functions with a bandwidth of 12 months.
Source: VSKT2014, own calculations.

inactivity because women are not willing or able to work until the new ERA. In order to distinguish between passive and active program substitution, we estimate the effect of the ERA increase on the probability to exit employment (and enter unemployment, disability pension program, or residual category) in a specific age window, conditional on employment for at least 6 months at the start of this window. In particular, we condition on employment for at least 6 months at the 58^{th} birthday and estimate the effects on the probability to exit employment in the following two and four years. For identification of the treatment effect, we have to assume that employment rates at age 58 are unaffected by the reform. A test of discontinuity in the employment rate at the 58^{th} birthday is displayed in Figure 1-7. There is no statistically significant discontinuity in the employment rate at the cutoff.[22]

[22]This is supported by a regression analysis that is not reported here.

The results for the employment outflow analysis are displayed in Table 1.3, where the coefficients in the odd columns can be interpreted as the reform effect on the probability to exit/enter the respective category between the 58^{th} and the 60^{th} birthday. The coefficients in the even columns correspond to the effects on the probability to exit/enter the respective categories in the four years between the 58^{th} and the 62^{nd} birthday. We find a large negative effect of 21 percentage points on the probability to exit employment between age 58 and 62, which is solely due to decreased exit rates between 60 and 62. Furthermore, we find small positive effects on unemployment, disability pension and inactivity entry rates of 58 and 59 year-old women who were employed at their 58^{th} birthday. This could be explained by a small reversed reference age effect, i.e. by a lack of entries in these categories in the 1951 cohort. However, the effects are small and only significant at the 10 percent level. The lack of any increase in program entry for women aged 60 to 62 suggests that there the ERA increase did not lead to increased active program substitution.

Table 1.3: Effects on employment outflows, conditional on employment with age 58

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Employment exit		Unemployment entry		Disability entry		Inactivity entry	
Age window	58-59	58-61	58-59	58-61	58-59	58-61	58-59	58-61
D_i	0.013	-0.206***	0.028*	0.023	0.011*	0.015	0.028*	0.023
	(0.0189)	(0.0442)	(0.0136)	(0.0209)	(0.0063)	(0.0145)	(0.0136)	(0.0209)
mob_i	0.002	0.006	0.001	-0.000	-0.002**	-0.001	0.001	-0.000
	(0.0021)	(0.0037)	(0.0010)	(0.0007)	(0.0009)	(0.0014)	(0.0010)	(0.0007)
$D_i * mob_i$	-0.003	-0.000	-0.002	0.003	0.002*	0.002	-0.002	0.003
	(0.0025)	(0.0056)	(0.0017)	(0.0033)	(0.0011)	(0.0024)	(0.0017)	(0.0033)
West	-0.021	-0.064***	-0.051***	-0.068***	-0.009	-0.009	-0.051***	-0.068***
	(0.0154)	(0.0168)	(0.0130)	(0.0169)	(0.0055)	(0.0078)	(0.0130)	(0.0169)
Constant	0.258***	0.640***	0.154***	0.238***	0.016**	0.041***	0.154***	0.238***
	(0.0202)	(0.0397)	(0.0155)	(0.0159)	(0.0069)	(0.0101)	(0.0155)	(0.0159)
N	2,447	2,447	2,732	2,732	2,732	2,732	2,732	2,732
*** p<0.01, ** p<0.05, * p<0.1								

Notes: Robust standard errors in parentheses. All linear regressions include calendar month fixed effects, income group dummies, and linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT 2014, own calculations.

1.7 Conclusion

This paper provides novel insights about the causal effects of pension reforms on labor market outcomes. We exploit a large exogenous increase in the ERA for women. In more detail, we focus on the 1999 pension reform that increased the ERA by at least three years for women born after December 1951. Previous studies show that labor market exits increase significantly at the pension eligibility age. If women shift their employment exit to the new ERA, it might be an effective tool to increase old-age employment. However, it could imply that some women who are not able to extend their working life are adversely affected by this reform. The estimation is based on high-quality administrative data from the German pension insurance.

The sharp discontinuity in the ERA by cohorts allows us to analyze the behavioral responses using a regression discontinuity design. Our results show that employment rates among women between the old and new ERA increased by 14.4 percentage points – which corresponds to an increase of about 26 percent compared to the pre-reform employment rate. Employment rates before age 60 remain unaffected by the reform, even though the reform was long anticipated. This is also surprising since previous studies show that earlier cohorts often used unemployment benefits as a bridge to retirement.

Furthermore, we find a positive reform effect on the unemployment and inactivity rates rate of 60 to 62 year-old women, which is caused by passive rather than active program substitution. That is, women who lost the early retirement option remained in their respective labor market status, i.e. in unemployment or inactivity, instead of retiring early. In order to distinguish between passive and active program substitution, we analyze the effects on employment outflows, and unemployment and disability pension inflows. We do not find increased unemployment, inactivity or disability pension entry among 60 to 62 year-old women. In other words, unemployed or inactive women did not return to the labor market. Employed women of the 1952 cohort remained in employment.

The ERA increase might have undesired distributional effects as the ability to work long and the remaining life expectancy may depend on socio-economic status. In particular, workers with poor health and weak labor market position might be negatively affected by fewer retirement options. Consequently, we examine whether the behavioral reactions differ by income and health status. We find women in East Germany are more affected than those in West Germany. In particular, unemployment rates of 60 to 62 year-old women increase more in East than in West Germany. East German women are less likely to be inactive. Furthermore, we find suggestive evidence for slightly higher employment effects for 60 and 61 year-old women with low income, poor health, and women without children.

The main distributional effects of the reform result from the persistence of labor market status. Unemployed or inactive women remained in their respective status. For these women, the time between employment exit and retirement entry was simply extended, and the period of pension benefits receipt shortened. This is a large negative wealth shock for this group only partly compensated by lower deductions. Employed women were able to compensate this wealth shock by continuing to work and to increase their pension entitlements.

1.8 Appendix

Pension types

Table 1.4: Pathways to pensions

Pension type	Early (ERA)	Normal (NRA)	Contribution Period	Notes
Regular	-	65 ⇒ 67	5	Retirement age has been increased to 67 since 2012; fully phased-in with cohort 1964
Women	60	65	15 (10 after age 40)	Abolished for cohorts born after 1951
Invalidity	60 ⇒ 62	63 ⇒ 65	35	Starting with cohort 1952 ERA and NRA increase by two years; fully phased-in with cohort 1964
Long-term insured	63 ⇒ 65	65⇒67	35	ERA increases to 65, NRA increases to 67; fully phased-in with cohort 1964
	-	63 ⇒ 65	45	Special scheme for people with particularly long insurance records
Unemployed/old-age part-time	63	65	15 (8 in last 10 years)	Abolished for cohorts born after 1951
Disability pension	no threshold	63 ⇒ 65	5 (3 in last 5 years)	Maximum deductions amount to 10.8 percent; since 2012 the NRA increases to 65; fully phased in with cohort 1964

Notes: The ERA denotes the age at which the pension type becomes available if eligibility criteria are fulfilled. Early retirement is associated with deductions of 0.3 percent per month before the NRA. The NRA denotes the age at which a full pension, i.e. without deductions becomes available.

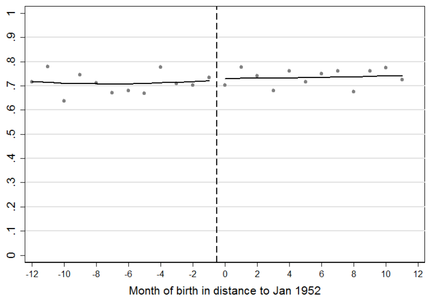
Results for 58 and 59 year old women

Table 1.5: Linear regression results, age 58-59

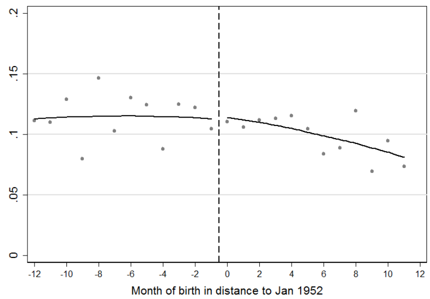
	Employment	Unemployment	Disability pension	Inactivity
D_i	0.015	0.004	-0.000	-0.017
	(0.0259)	(0.0099)	(0.0185)	(0.0169)
mob_i	0.000	-0.000	-0.002	0.000
	(0.0030)	(0.0011)	(0.0017)	(0.0020)
$D_i * mob_i$	0.000	-0.002	0.003	0.001
	(0.0041)	(0.0016)	(0.0024)	(0.0024)
Children	0.006	-0.017*	-0.008	-0.007
	(0.0131)	(0.0100)	(0.0162)	(0.0137)
West	0.022	-0.078***	0.019*	0.026**
	(0.0174)	(0.0086)	(0.0101)	(0.0121)
Constant	0.579***	0.272***	0.085***	0.126***
	(0.0345)	(0.0165)	(0.0282)	(0.0264)
N	3,771	3,771	3,771	3,771
R^2	0.033	0.053	0.004	0.006
Pre-treatment mean	0.731	0.098	0.082	0.112
*** p<0.01, ** p<0.05, * p<0.1				

Notes: Robust standard errors in parentheses. All linear regressions include calendar month fixed effects, income group dummies, and linear trends in the running variable on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT 2014, own calculations.

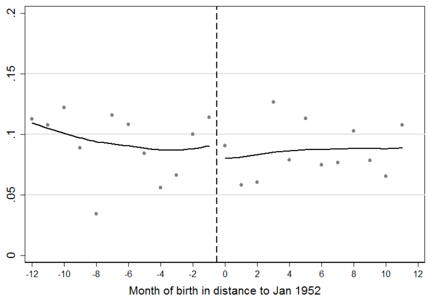
Figure 1-8: Local linear regression plots, age 58-59



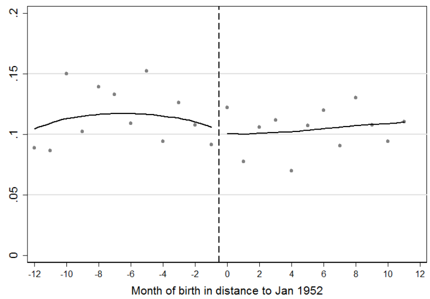
(a) Employment



(b) Unemployment



(c) Disability pension



(d) Inactivity

Notes: Scatter plots display mean outcome values using monthly bins. Local linear regression plots are based on triangular kernel functions with a bandwidth of 12 months.
Source: VSKT 2014, own calculations.

Table 1.6: Subgroup analysis - linear regression results, age 58-59

	Employment	Unemployment	Disability pension	Inactivity	N
Baseline	0.015	0.004	-0.000	-0.017	3771
	(0.0259)	(0.0099)	(0.0185)	(0.0169)	
West Germany	-0.007	0.001	0.018	-0.013	2727
	(0.0383)	(0.0134)	(0.0222)	(0.0195)	
East Germany	0.065	0.021	-0.044	-0.035	1044
	(0.0570)	(0.0280)	(0.0357)	(0.0290)	
Low income	0.059	-0.048	-0.010	-0.012	1046
	(0.0460)	(0.0332)	(0.0382)	(0.0282)	
Poor health	0.017	0.009	0.010	-0.012	988
	(0.0597)	(0.0408)	(0.0630)	(0.0300)	
Children	0.023	0.007	0.013	-0.034*	3198
	(0.0255)	(0.0122)	(0.0204)	(0.0198)	
No children	-0.007	-0.019	-0.084	0.064*	573
	(0.0486)	(0.0226)	(0.0507)	(0.0318)	

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors in parentheses. All linear regressions include calendar month fixed effects, income group dummies, and linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT 2014, own calculations.

Robustness and validity of the empirical strategy

We identified several potential threats to our identification strategy. These are discontinuities in covariates, and the turn of the year effect, and bias due to sample selection. While Section 1.5 describes our empirical strategy, we address all possible identification threats in greater detail in this section, and check whether our results are robust to several alternative specifications of the empirical model.

Discontinuities in covariates

A main concern for every analysis based on cohort discontinuities is that something other than the policy change of interest is affecting the relevant cohorts. This may lead to discontinuities in covariates that may in turn affect the outcome variables of interest. One way to account for this concern is to check for discontinuities in covariates that should not be affected by the reform. The analysis of outcomes for 58 and 59 year-old women can be interpreted as a test for covariate-discontinuities. However, although these age groups are not directly affected by the reform, they may have adapted their employment behavior in anticipation of the ERA increase. Consequently, it is difficult to find covariates that are truly unaffected by the reform. We compare several time-invariant covariates as average and sum of pension points, health status, number of children, and contribution period by month of birth and do not find any discontinuities between cohorts.

Table 1.7: Test for discontinuities in covariates

Variable	Linear RDD		Quadratic RDD		Sample mean
Average pension points (month)	-0.000	(0.000)	0.001	(0.002)	0.064
Sum of pension points	-0.444	(0.714)	0.009	(0.787)	31.66
Poor health status	0.015	(0.026)	0.004	(0.032)	0.262
Has at least one child	0.000	(0.032)	0.082	(0.065)	0.848
Contribution period	0.296	(0.353)	0.082	(0.445)	37.19
Sum contribution months after 40	-0.820	(2.204)	-1.724	(2.837)	213.2

Notes: Covariates are measured at age 60. All regressions include calendar month fixed effects, income group dummies, and linear or quadratic trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors (in parentheses) are clustered by month of birth.
Source: VSKT2014, own calculations.

Difference-in-discontinuities approach

We refer to differences between women born at the end of a year in comparison to women who were born at the beginning of a year as *turn of the year effect.* In particular, there may be discontinuities in labor market outcomes for women born between December and January that are unrelated to the ERA increase reform. In order to address this concern, we performed a difference-in-discontinuities analysis using the discontinuity between cohorts born in 1950 and 1951 as counterfactual with a hypothetical policy-cutoff in the running variable at the turn of the year. The difference-in-discontinuities estimation is implemented by interacting the regression equation with an indicator function T_i, equal to one for the real sample around the actual reform cutoff-date, and zero otherwise. The results for 60-61 and 58-59 year-old women are displayed in Table 1.8 and Table 1.9). Reassuringly, the results are similar to those of the baseline specification presented in Section 1.6.1.

The coefficients of the interaction term $T_i * D_i$ do not differ significantly from the corresponding coefficients in Tables 1.1 and 1.5. We conclude that discontinuities between cohorts can be attributed to the ERA increase and therefore select a standard RD framework as our baseline specification.

Table 1.8: Difference-in-discontinuities results, age 60-61

	Employment	Unemployment	Disability pension	Inactivity
$T_i * D_i$	0.172***	0.031**	-0.017	0.062***
	(0.0241)	(0.0145)	(0.0212)	(0.0161)
$T_i * mob_i$	-0.002	0.001	-0.004***	0.001
	(0.0021)	(0.0012)	(0.0014)	(0.0011)
$T_i * D_i * mob_i$	-0.001	-0.001	0.007**	-0.001
	(0.0051)	(0.0022)	(0.0032)	(0.0023)
D_i	-0.022	0.009	0.021	-0.011
	(0.0248)	(0.0097)	(0.0148)	(0.0101)
mob_i	0.003*	-0.001	0.002*	-0.000
	(0.0020)	(0.0014)	(0.0013)	(0.0011)
$D_i * mob_i$	-0.002	-0.000	-0.004	0.001
	(0.0036)	(0.0017)	(0.0025)	(0.0014)
Constant	0.536***	0.075***	0.102***	0.061***
	(0.0118)	(0.0056)	(0.0085)	(0.0072)
N	7286	7286	7286	7286
R^2	0.022	0.002	0.002	0.011

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors in parentheses. All linear regressions include calendar month fixed effects, income group dummies, and linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT2014, own calculations.

Table 1.9: Differences-in-discontinuities results, age 58-59

	Employment	Unemployment	Disability pension	Inactivity
$T * D_i$	0.020	0.002	-0.021	-0.015
	(0.0277)	(0.0083)	(0.0186)	(0.0195)
$T * mob_i$	-0.001	0.002**	-0.003**	0.002*
	(0.0020)	(0.0009)	(0.0013)	(0.0012)
$T * D_i * mob_i$	0.002	-0.005**	0.006**	-0.001
	(0.0048)	(0.0018)	(0.0028)	(0.0030)
D_i	-0.011	-0.005	0.025*	0.001
	(0.0274)	(0.0098)	(0.0129)	(0.0171)
mob_i	0.003*	-0.001*	0.001	-0.002**
	(0.0013)	(0.0007)	(0.0010)	(0.0009)
$D_i * mob_i$	-0.002	0.001	-0.003	0.003
	(0.0034)	(0.0012)	(0.0021)	(0.0022)
Constant	0.717***	0.119***	0.079***	0.113***
	(0.0126)	(0.0054)	(0.0060)	(0.0092)
N	7286	7286	7286	7286
R^2	0.001	0.001	0.001	0.001
*** p<0.01, ** p<0.05, * p<0.1				

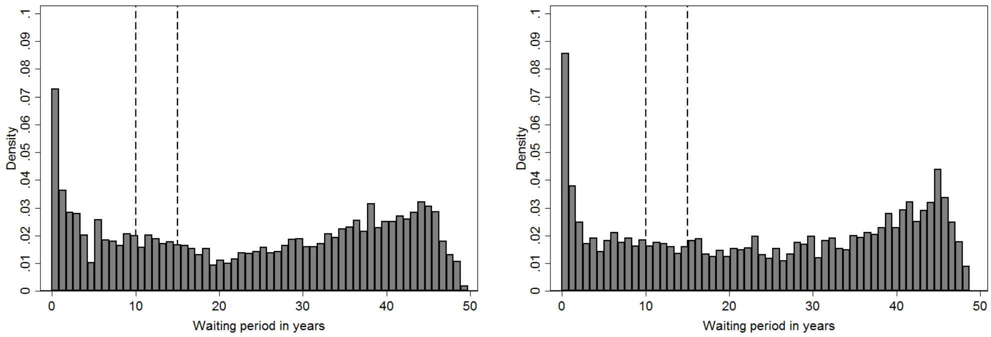
Notes: Robust standard errors in parentheses. All linear regressions include calendar month fixed effects, income group dummies, and linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT2014, own calculations.

Sample selection

A major concern arises due to the selection of the sample by the eligibility criteria of the pension for women. Specifically, women born in 1951 may select into the sample by prolonging their pension contribution period in order to be eligible for early retirement. In contrast, women born in 1952 do not have the same incentives to fulfill the eligibility criteria. The eligibility criteria for claiming a pension for women are: (i) at least 15 years of pension insurance contributions; and (ii) at least 10 years of pension insurance contributions after the age of 40.

Consequently, we expect bunching in the density distribution after 15 contribution years for the 1951 cohort, but not for the 1952 cohort. However, when the reform was introduced in 1999, women born in 1951 and 1952 were already 47-48 years old. At that age, most women have already collected at least 5 contribution years. Therefore, cohorts have different incentives primarily with respect to the second eligibility criterion of a contribution period of at least 10 years after age 40. We show in Figure 1-9 and Figure 1-10 that there is no bunching: neither after 15 years, nor after 121 contribution months for the 1951 cohort, when compared to the 1952 cohort. Seibold (2016) also looks at bunching in pension contribution years and finds only little bunching at the relevant cutoffs for women. Furthermore, we test for a discontinuity (1) in the fraction of women fulfilling the eligibility criteria for early retirement, (2) the number of contribution years, (3) the number of contribution months after age 40, (4) eligibility for the old-age pension for the long term insured, and (5) the sum of years worked up to age 60. We find that there is no significant discontinuity at the cohort-cutoff. The corresponding local linear and local polynomial regression plots for the fraction of women fulfilling the eligibility criteria are displayed in Figure 1-11. We conclude that bias due to sample selection is negligible.

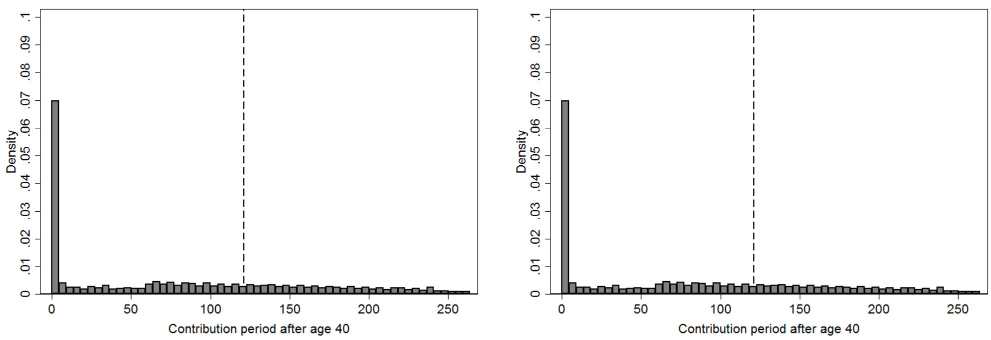
Figure 1-9: Distribution of contribution years by cohort



(a) 1951 Cohort **(b)** 1952 Cohort

Notes: Eligibility requires 15 years of contributions.
Source: VSKT2014, own calculations.

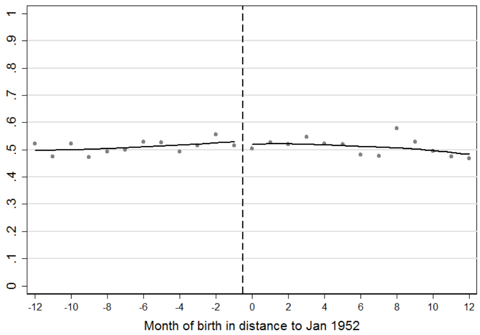
Figure 1-10: Distribution of contribution months after age 40 by cohort



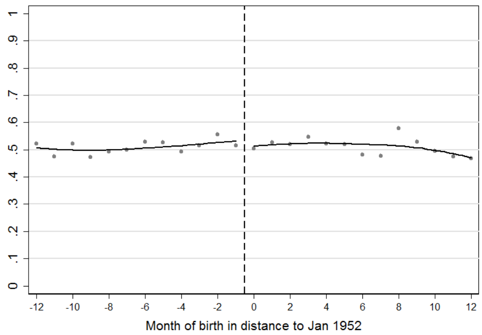
(a) 1951 Cohort **(b)** 1952 Cohort

Notes: Eligibility requires 121 contribution months after age 40.
Source: VSKT2014, own calculations.

Figure 1-11: Testing for discontinuity in fulfillment of eligibility criteria



(a) Local linear regression



(b) Local quadratic regression

Notes: Scatter plots display mean outcome values using monthly bins. Local linear regression plots are based on triangular kernel functions with a bandwidth of 12 months.
Source: VSKT2014, own calculations.

RDD results for all women

Table 1.10: Linear regression for all women, age 60-61

	Employment	Unemployment	Disability pension	Inactivity
D_i	0.076***	0.035***	0.019	0.012
	(0.0191)	(0.0067)	(0.0175)	(0.0202)
mob_i	0.002	-0.001	-0.000	-0.001
	(0.0021)	(0.0009)	(0.0013)	(0.0021)
$D_i * mob_i$	-0.003	-0.000	-0.000	0.004
	(0.0031)	(0.0011)	(0.0023)	(0.0029)
Children	0.174***	0.030***	0.036***	-0.277***
	(0.0105)	(0.0059)	(0.0085)	(0.0144)
West	-0.073***	-0.057***	-0.031***	0.212***
	(0.0199)	(0.0105)	(0.0106)	(0.0137)
Constant	0.230***	0.154***	0.101***	0.367***
	(0.0219)	(0.0112)	(0.0196)	(0.0249)
N	7289	7289	7289	7289
R^2	0.055	0.042	0.007	0.109
Pre-treatment mean	0.377	0.059	0.082	0.329
*** p<0.01, ** p<0.05, * p<0.1				

Notes: Robust standard errors in parentheses. All linear regressions include calendar month fixed effects, income group dummies, and linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT2014, own calculations.

Table 1.11: Linear regression results for all women, age 58-59

	Employment	Unemployment	Disability pension	Inactivity
D_i	0.006	0.015	0.021	-0.044**
	(0.0157)	(0.0103)	(0.0141)	(0.0205)
mob_i	0.002	-0.001	-0.001	-0.001
	(0.0019)	(0.0012)	(0.0010)	(0.0022)
$D_i * mob_i$	-0.003	-0.002	-0.001	0.005*
	(0.0024)	(0.0013)	(0.0019)	(0.0026)
Children	0.210***	0.035***	0.024**	-0.265***
	(0.0115)	(0.0059)	(0.0090)	(0.0113)
West	-0.120***	-0.073***	-0.028**	0.207***
	(0.0170)	(0.0077)	(0.0104)	(0.0133)
Constant	0.340***	0.204***	0.081***	0.410***
	(0.0215)	(0.0129)	(0.0190)	(0.0240)
N	7289	7289	7289	7289
R^2	0.066	0.058	0.005	0.099
Pre-treatment mean	0.493	0.078	0.073	0.375
*** p<0.01, ** p<0.05, * p<0.1				

Notes: Robust standard errors in parentheses. All linear regressions include calendar month fixed effects, income group dummies, and linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT2014, own calculations.

RDD results without covariates

Table 1.12: Regression without covariates, age 60-61

	Employment	Unemployment	Disability pension	Inactivity
D_i	0.145***	0.050***	-0.003	0.053***
	(0.0287)	(0.0121)	(0.0239)	(0.0119)
mob_i	0.002	-0.002	-0.001	0.000
	(0.0030)	(0.0010)	(0.0021)	(0.0009)
$D_i * mob_i$	-0.002	-0.000	0.003	0.001
	(0.0043)	(0.0016)	(0.0030)	(0.0018)
Constant	0.543***	0.065***	0.108***	0.058***
	(0.0214)	(0.0059)	(0.0180)	(0.0064)
N	3771	3771	3771	3771
R^2	0.025	0.003	0.000	0.012

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors in parentheses. All regressions include linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT2014, own calculations.

Table 1.13: Regression without covariates, age 58-59

	Employment	Unemployment	Disability pension	Inactivity
D_i	0.015	0.002	0.001	-0.016
	(0.0275)	(0.0087)	(0.0192)	(0.0168)
mob_i	0.000	-0.000	-0.002	0.000
	(0.0032)	(0.0010)	(0.0018)	(0.0020)
$D_i * mob_i$	0.001	-0.003*	0.003	0.001
	(0.0043)	(0.0014)	(0.0025)	(0.0025)
Constant	0.712***	0.114***	0.081***	0.116***
	(0.0198)	(0.0078)	(0.0144)	(0.0131)
N	3771	3771	3771	3771
R^2	0.001	0.001	0.000	0.000
*** p<0.01, ** p<0.05, * p<0.1				

Notes: Robust standard errors in parentheses. All regressions include linear trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT2014, own calculations.

RDD results with quadratic trends

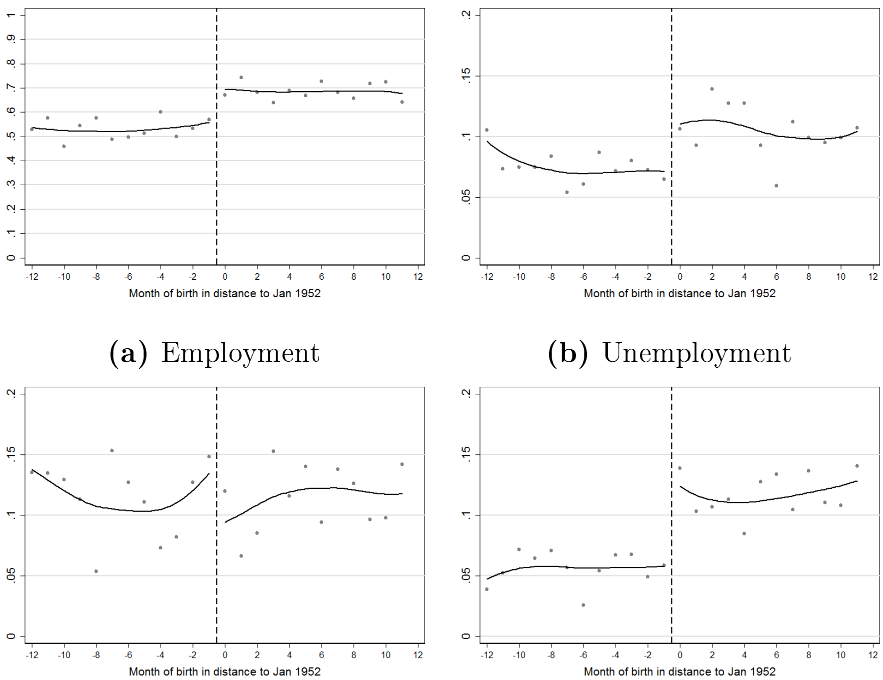
Table 1.14: Regression with quadratic trends, age 60-61

	Employment	Unemployment	Disability pension	Inactivity
D_i	0.125***	0.032**	-0.045	0.071***
	(0.0318)	(0.0156)	(0.0301)	(0.0152)
mob_i	0.010	0.006	0.013	-0.003
	(0.0101)	(0.0044)	(0.0098)	(0.0049)
mob_i^2	0.001	0.001*	0.001	-0.000
	(0.0008)	(0.0003)	(0.0007)	(0.0004)
$D_i * mob_i$	-0.012	-0.008	-0.007	-0.002
	(0.0127)	(0.0070)	(0.0122)	(0.0073)
$D_i * mob_i^2$	-0.001	-0.001	-0.002	0.001
	(0.0011)	(0.0006)	(0.0010)	(0.0006)
Children	0.011	-0.012	-0.005	-0.017
	(0.0164)	(0.0119)	(0.0130)	(0.0127)
West	0.051**	-0.067***	0.021*	0.030**
	(0.0207)	(0.0124)	(0.0111)	(0.0114)
Constant	0.400***	0.201***	0.151***	0.066***
	(0.0366)	(0.0203)	(0.0339)	(0.0221)
N	3771	3771	3771	3771
R^2	0.059	0.037	0.006	0.018

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors in parentheses. All regressions include calendar month fixed effects, income group dummies, and quadratic trends in the running variable (month of birth) on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT2014, own calculations.

Figure 1-12: Local polynomial regression plots, age 60-61



(a) Employment

(b) Unemployment

(c) Disability pension

(d) Inactivity

Notes: Scatter plots display mean outcome values using monthly bins. Local linear regression plots are based on triangular kernel functions with a bandwidth of 12 months.
Source: VSKT2014, own calculations.

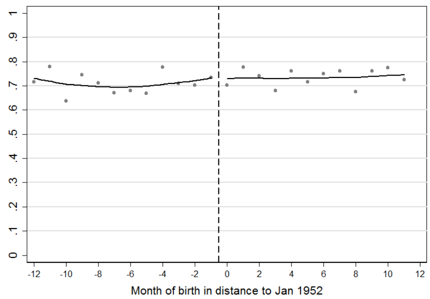
Table 1.15: Regression with quadratic trends, age 58-59

	Employment	Unemployment	Disability pension	Inactivity
D_i	-0.015	-0.003	-0.034	0.028*
	(0.0286)	(0.0148)	(0.0225)	(0.0160)
mob_i	0.014	-0.000	0.011	-0.017***
	(0.0081)	(0.0053)	(0.0083)	(0.0042)
mob_i^2	0.001	0.000	0.001	-0.001***
	(0.0007)	(0.0004)	(0.0006)	(0.0003)
$D_i * mob_i$	-0.014	0.002	-0.007	0.016**
	(0.0117)	(0.0059)	(0.0099)	(0.0071)
$D_i * mob_i^2$	-0.001	-0.000	-0.001	0.002**
	(0.0010)	(0.0004)	(0.0008)	(0.0006)
Children	0.007	-0.017	-0.007	-0.008
	(0.0132)	(0.0100)	(0.0161)	(0.0138)
West	0.022	-0.078***	0.018*	0.027**
	(0.0174)	(0.0086)	(0.0102)	(0.0120)
Constant	0.610***	0.273***	0.115***	0.085***
	(0.0361)	(0.0210)	(0.0307)	(0.0245)
N	3771	3771	3771	3771
R^2	0.033	0.053	0.005	0.007

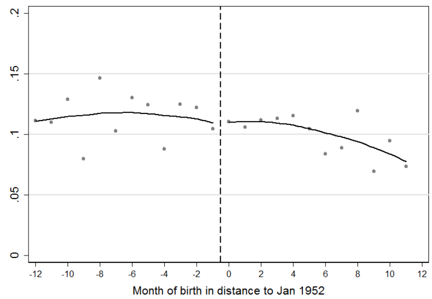
*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors in parentheses. All regressions include calendar month fixed effects, income group dummies, and quadratic trends in the running variable on both sides of the policy cutoff. Standard errors are clustered by month of birth.
Source: VSKT2014, own calculations.

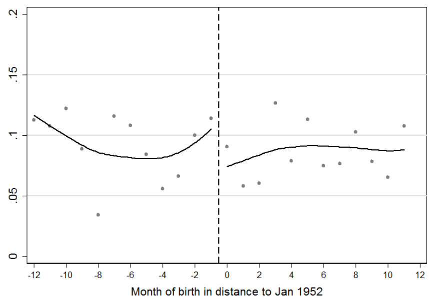
Figure 1-13: Local polynomial regression plots, age 58-59



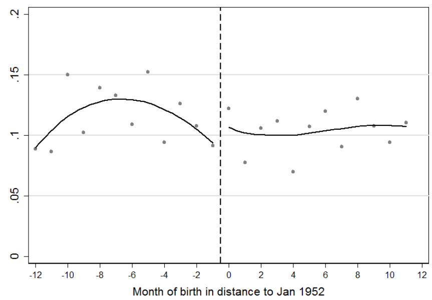
(a) Employment



(b) Unemployment



(c) Disability pension



(d) Inactivity

Notes: Local polynomial regressions of 2nd degree, with triangular kernel and a bandwidth of 12 months.
Source: VSKT2014, own calculations.

Sample characteristics: early vs. late retirement

Table 1.16: Comparison of women who retire early (< 62) and late (≥ 62)

	Retire late (≥ 62)		Retire early (< 62)			
	Mean	**SD**	**Mean**	**SD**	**Diff**	**t-value**
Average monthly pension points	0.068	0.025	0.064	0.023	-0.004	**-3.15**
Sum of pension points	35.082	14.031	31.425	12.185	-3.657	**-5.65**
Poor health status	0.220	0.414	0.282	0.450	0.062	**2.78**
Has at least one child	0.863	0.344	0.858	0.350	-0.005	-0.28
Sum of children	1.697	1.134	1.674	1.068	-0.024	-0.43
Sum contribution months after 40	215.118	36.650	211.511	35.752	-3.608	**-1.97**
Eligible for early retirement with 63	0.906	0.292	0.918	0.275	0.012	0.84
contribution period in years	38.423	7.448	36.781	6.704	-1.642	**-4.67**
Employed at 60^{th} birthday	0.711	0.454	0.480	0.500	-0.231	**-9.35**
Unemployed at 60^{th} birthday	0.062	0.242	0.135	0.342	0.073	**4.54**
N	1288		557		1845	

Notes: Including all women born in 1951 who fulfill the eligibility criteria for the early pension for women.
Source: VSKT2014, own calculations.

Chapter 2

Peer Effects in Employment Exit Decisions

2.1 Introduction

Many OECD countries have answered to the challenges posed by population aging by reforming their pension systems (OECD, 2015). A central aim of these reforms is to relieve public finances by increasing employment rates of older individuals. This is particularly relevant in Germany, where a steeply increasing old-age dependency ratio is paired with low realized retirement ages.[1] Consequently, it is of great interest to understand the determinants of employment exit and retirement entry behavior. In economic evaluations, the central question is often how individuals

[1]The average realized retirement age was 61.9 for West German men in 2015 (Deutsche Rentenversicherung Bund, 2015).

respond to financial incentives. However, pension reforms and the introduction of new policy instruments are likely to affect individuals not only directly by the change in financial incentives, but also indirectly by a change in the behavior of the social environment. Such peer effects can lead to social multipliers and thereby magnify the impact of a policy (Glaeser et al., 2003). If peer effects are relevant but neglected in models used for policy simulations, the resulting predictions are likely to underestimate true policy effects. It is therefore of great importance to understand the relevance and magnitude of peer effects in labor supply decisions, as well as the consequences for social policies.

Peer effects can arise within different social environments, such as families or neighborhoods. Previous literature on peer effects has shown that coworkers may affect productivity and labor supply decisions. For example, Mas and Moretti (2009) and Hesselius et al. (2009) show that peer effects play a role in the context of productivity of cashiers and worker absenteeism respectively. Cornelissen et al. (2017) find an effect of the long-term quality of a workers' peers on wages. Other papers have shown that peers at work can affect fertility (Pink et al., 2014; Asphjell et al., 2013), parental leave decisions (Dahl et al., 2014; Welteke and Wrohlich, 2016), and retirement plan decisions (Duflo and Saez, 2003). Brown and Laschever (2012) find peer effects in retirement timing of teachers, using a reform of teacher pensions in California.

In this paper, I provide novel empirical evidence on the question whether individual employment exit decisions of older workers are affected by peers at the workplace. I estimate a probability model of employment exit decisions including a measure of financial incentives, similar to the option value by Stock and Wise

(1990), and a measure of peer employment behavior. High-quality German administrative panel data (LIAB) enable the assignment of each individual to a peer group, which is defined by firm and occupation. The empirical identification of peer effects bears some challenges. Namely, endogenous sorting into groups and common shocks can lead to correlation of individual behavior within groups even in the absence of causal peer effects. I follow Cornelissen et al. (2017) by using use a set of fixed effects to account for correlated effects within peer groups. The empirical design enables the estimation of causal peer effects under a set of justifiable assumptions. Furthermore, I simulate a universal and immediate increase in the normal retirement age to 67. I use the results to estimate the indirect effects of the policy through the change in peer behavior by artificially delaying peer employment exits.

The baseline regression results suggest that there is a significant negative effect of both the measure of financial incentives to continue working and the share of older coworkers that is still working on the individual probability to exit employment. More precisely, the average individual exit hazard decreases by about 0.4 percentage points if the entire peer group is still employed, compared to a situation where all former coworkers left employment. A larger option value of remaining employed decreases the probability to exit employment by about 0.5 percentage points per period. The coefficient of the option value is largest for the subgroups with low education or income, and for individuals working in small firms. A simulated delay of peer employment exits by 2.4 months results in an increase in the individual exit age by additional 0.7 months. I conclude that peer effects can amplify direct reform effects substantially.

The paper is structured as follows: first, the theoretical framework of employment exit decisions in the German institutional context is described in Section 2.2.1, followed by a discussion of how financial incentives and peer effects affect individual decisions in Section 2.2.2 and Section 2.2.3. Subsequently, data and empirical strategy are described in Section 2.3, including a discussion of the potential threats to the identification strategy. The baseline results, the model fit, the results for different subgroups, and the policy simulation are described in Section 2.4.1, Section 2.4.2, Section 2.4.3, and Section 2.4.4, respectively. Section 2.5 concludes.

2.2 Institutional setting and theoretical framework

2.2.1 Institutional setting

It is important to understand the German institutional setting in order to accurately model employment exit and retirement decisions of older workers. Most German workers (about 85 percent) are covered by the public retirement insurance (Berkel and Börsch-Supan, 2004), which is organized as a pay-as-you-go system. Individual benefit claims depend on the employment history using a system of pension points which are accumulated individually over the lifetime. One pension point corresponds to one year of average pension contributions. All contributions are weighted equally over the lifetime. At the normal retirement age (NRA) full pension benefits can be drawn, regardless of the employment status, and no further contributions have to be paid. A stepwise increase in the normal retirement age from 65 to 67 by year of birth was decided as part of a pension reform (*RV-Altersgrenzenanpassungsgesetz*) in 2007. The first affected cohort, born in 1947, can retire without pension deductions with an age of 65 years and one month. The NRA increase is fully phased in with the cohort of 1964, which will reach the new NRA of 67 in the year of 2031. In the sample used for this analysis, which includes cohorts born between 1926 and 1949, the NRA varies between 65 and 65 + 3 months. In Section 2.4.4, I simulate an immediate NRA increase to 67 for all cohorts.

There are several early retirement schemes which enable some groups of workers to retire before the NRA. These groups include women, participants of the partial retirement program (*Altersteilzeit*), unemployed and disabled workers, and individuals with very long insurance contribution histories. The earliest retirement age (ERA), defined as the earliest age at which pension benefits can be claimed, varies between 60 and 63, depending on whether the eligibility criteria for the different early retirement programs are met. Early retirement is penalized by a lifelong deduction of monthly pension benefits by 3.6 percent per year in distance to the NRA, with a maximum deduction of 18 percent. Employment exit does not equal retirement entry. In fact, it is quite common among German workers to be unemployed before claiming pension benefits (see e.g. Knuth and Kalina, 2002). A period of unemployment or inactivity between employment exit and pension claiming can be used to avoid pension deductions associated with early retirement.

2.2.2 Measuring financial employment exit incentives

In this analysis, I primarily aim to distinguish peer effects from other (financial) incentives to exit employment, while the difference between the effects of wages and social security income is not of particular interest. Previous research used different approaches to specify the financial incentive to retire. Early work modeled retirement decisions simply as a function of social security wealth and pension levels (see Fields and Mitchell, 1984b, for a review). The key limitation of this approach is that social security effects are considered only at a point in time, while the impact on the retirement decision arising from the time pattern of social security wealth accruals is ignored. Subsequent literature took the dynamic structure into account by estimating structural life-cycle models of retirement decisions (Gustman and Steinmeier, 1986; Rust and Phelan, 1997). Another approach was to estimate reduced form models including the accrual of social security wealth (defined as the net present value of all future benefits) with an additional year of work (Fields and Mitchell, 1984a; Hausman and Wise, 1985).

Stock and Wise (1990) have shown that it is not simply the level of retirement wealth or the accrual with one additional year of work that matters, but the entire evolution of future wealth with future work. Because by working, the worker is also buying the option to attain additional income that results from work in future years. If early retirement is punished by benefit deductions as in Germany, the retirement decision has four effects on individual utility: (i) utility decreases due to the wage loss in the time between employment exit now and a later exit, (ii) utility increases due to the additional leisure time in this time period, (iii) future benefits are decreased due to deductions for early retirement, and (iv) the total amount of future benefits increases due to a longer period of benefit receipt. All these effects can be captured in one incentive measure, the option value, which can be understood as the expected gain from postponing retirement.

The option value, developed by Stock and Wise (1990) and adapted for example by Börsch-Supan (2000) and Berkel and Börsch-Supan (2004) in the German context, consolidates the dynamic optimization problem by focusing on the value of retaining the option of postponing retirement. In each period, individuals compare the best of expected future outcomes with the value of retirement in the current period. I employ a modified version of the option value as a measure of the financial incentives associated with exiting employment. Instead of the difference between the value of exiting in the current period and the best of expected future outcomes, I use the difference between the value of exiting in the current period ($r = t$) and the value of remaining employed until the normal retirement age ($r = r^*$). The choice of the optimal exit age as the NRA is discussed below.

$$OV_t(t) = V_t(r^*) - V_t(t) \tag{2.1}$$

The resulting option value OV_t can be interpreted as the utility gain from staying at work up to the normal retirement age compared to an early exit in period t. The expected discounted value in period t of exiting in period r is defined as the sum of discounted future utility from labor and retirement:

$$V_t(r) = \sum_{s=t}^{r-1} \delta^{s-t} U(Y_s) + \alpha \sum_{s=r}^{T} \delta^{s-t} U(B_s(r)) \tag{2.2}$$

where $U(Y_s)$ is the utility from labor market earnings and $U(B_s(r))$ is the utility from receiving unemployment, social security, or retirement benefits associated with an exit in period r. Future income streams are discounted by a discount rate of 3 percent per year. This analysis is focusing on employment exits rather than retirement entries. Consequently, the option value has to be adapted for potential employment exits before the earliest age at which pension benefits can be claimed (ERA). Employment exit before the ERA is associated with unemployment benefits (ALG I), if the eligibility criteria are met, or social security benefits (ALG II) up to the ERA, and pension benefits thereafter.

In the option value model described in Berkel and Börsch-Supan (2004), a preference for leisure is introduced by multiplying the second term on the right hand side with a factor $\alpha \geq 1$, i.e. leisure entered the value function by weighting retirement income more heavily. This approach has the disadvantage of implying a certain way leisure enters the utility function. Coile and Gruber (2001) suggest calculating income streams rather than utility streams, since no assumptions regarding the utility function are needed then. I follow a similar approach in this analysis by setting $\alpha = 1$ and thereby weighing income from labor and social security equally. Furthermore, since $U(Y) = Y$, the option value can be interpreted as purely financial value of postponing the employment exit. In an alternative specification I repeat the analysis with a relative utility of leisure of $\alpha \geq 2.8$ as in Berkel and Börsch-Supan (2004), who estimate α using a grid search algorithm.

The resulting financial option value with $\alpha = 1$ is similar to the peak value suggested by Coile and Gruber (2001), who suggest an incentive measure which incorporates only the variation in retirement income and not wages. The peak value calculates the difference between social security wealth at its maximum expected value and at today's value. Coile and Gruber (2007) use both incentive measures, option value and peak value, and compare their relative performance. They conclude that both models perform better in explaining retirement decisions than a model which is solely based on the one-period accrual of social security wealth. The option value model has the largest explanatory power; however, it comes with the disadvantage that there is no straightforward decomposition of the option value that could be used to assess the different responses to financial incentives from different retirement income sources. However, in this analysis, the difference between the effects of wages and social security income is not of particular interest, because I primarily aim to distinguish peer effects from other (financial) incentives to exit employment.

Due to the German pension system, the present discounted value of social security wealth is either monotonically decreasing or increasing with age for most workers (Hanel, 2010), depending on the choice of the leisure preference parameter α. Consequently, the best of expected future outcomes is achieved either at the earliest or the latest possible age for most individuals. When α is large, it is optimal for most workers to exit employment immediately and claim pension benefits as soon as legally possible, because the deduction in pension benefits does not suffice to compensate for the utility gain that is associated with an early exit. When $\alpha = 1$, it is always optimal to continue working, even after reaching the normal retirement age. However, this is empirically implausible as most workers retire at or before the NRA. Hanel (2010) solves this problem by setting the reference age to the NRA in one of her specifications. I follow this approach and set the NRA as the optimal age for the computation of the option value, which is then to be interpreted as the value of the option to remain employed until the NRA compared to an exit in the current period. Consequently, the option value of remaining in employment is positive and decreasing with age up to the NRA, and zero thereafter.

2.2.3 How peer effects enter employment exit decisions

Employment exit decisions of peers at work can influence individual employment exit behavior through several channels. One possibility is that peer behavior reveals information about the existence of early retirement or unemployment programs. Several studies focus on leisure complementarities interdependencies of retirement decisions within couples (e.g. Gustman and Steinmeier, 2004; Zweimüller et al., 1996). Another possibility is that workers get utility from continuing to work with their current coworkers of the same age-group. It is reasonable to assume that workers form social ties at work and that the departure of a team member might decrease the value of work compared to the value of exiting for some of the remaining coworkers. Consequently, I assume that work complementarities may be an important driver of peer effects in this context. Finally, the employment exit behavior of coworkers may create or change social norms or reference ages within a firm and occupation. Individuals may have a preference for a behavior that is in accordance with social norms within their reference group. For example, it has been shown that statutory retirement ages serve as strong reference points for retirement beyond financial incentives (Seibold, 2016). If coworkers exit employment before reaching the NRA, individuals may change their perception of the appropriate exit age. Furthermore, it could also reduce the stigma of receiving unemployment benefits if a coworker previously used unemployment as a pathway to retirement. Markussen and Røed (2015) find peer effects in social insurance claims using Norwegian data. All of these channels can lead to causal peer effects in employment exit decisions among coworkers. However, it is beyond the scope of this paper to distinguish between these channels empirically.

There are several ways to define the peer group and how to measure peer group behavior. The peer group has to be defined carefully in order to avoid the problem of simultaneity (Manski, 1993). The simultaneity problem occurs if an individual is influenced by his or her peer group while the peer group is in turn influenced by the behavior of the individual, even if the individual is not directly included in the peer group. This leads to reverse causality, in particular in small groups. The simultaneity problem can be avoided by assuming a certain direction of the peer effect, for example by imposing a hierarchical or chronological structure on social interactions. I assume that individuals are only influenced by older peers to avoid the simultaneity problem. I argue that this is a reasonable assumption, because older coworkers typically face employment exit and retirement decisions at an earlier point in time.

Ideally, I would use network data including information on individual ties between workers. However, such data are rarely available and do not exist in the scope necessary for this analysis. Therefore, this analysis is based on the assumption that workplace and occupation form a relevant social reference group. In more detail, the peer group is defined as the group of coworkers in the same firm and occupational group, which are older than the individual, excluding the individual himself. However, I cannot observe whether individuals within a firm and occupational group interact with or know each other. Given the definition of the peer group, not every coworker is a relevant peer and some peers may have a different occupation or employer. Cornelissen et al. (2017) show that omitting relevant peers generally leads to a downward bias of the estimated peer effect. Including irrelevant peers causes no bias as long as workers randomly choose their peers within the firm and occupation.

Peer group outcomes can, for example, be specified as the average employment exit propensity, or as the difference between individual age and the average exit age in the peer group. In this analysis, individuals are assumed to be influenced by the share of peers at work that is still employed at time t, out of all peers that were working in the same firm and occupation as the individual i at $t = 0$. The exact specification will be explained in the next section.

2.3 Empirical strategy

2.3.1 Data

When estimating peer effects, the challenge is to find a data set that contains micro data on an individual's whole relevant network. For Germany, not many such data sets exist. This paper is based on the longitudinal model 1993-2014 (LIAB LM 9314) of the Linked Employer-Employee Data of the Institute for Employment Research (IAB). The data were accessed via a guest stay at the Research Data Center (FDZ) of the German Federal Employment Agency (BA) at the IAB and subsequently by means of controlled remote data processing at the FDZ.[2] One of the main advantages of the LIAB is that it includes a large number of firms and individuals (more than 10.000 firms and their employees per year) and that all employees working at the same firm can be identified. Therefore, this dataset has already been used by several studies to analyze peer effects among coworkers in different contexts. For example, Pink et al. (2014) analyze social interaction effects related to fertility decisions. They analyze whether the pregnancy of a coworker affects the pregnancy of female colleagues and conclude that there is a significant peer effect. Cornelissen et al. (2017) use the LIAB to analyze peer effects in wages. The LIAB includes individual employment histories generated from administrative data provided by firms and social security data from 1993 to 2014. Individual employment histories are matched with the annual IAB Establishment Panel data, which contain detailed firm characteristics in addition to firm and branch size, and industry. I merge the LIAB dataset with several regional characteristics on district level (Kreis) from INKAR,[3] which contains regional-statistic indicators on many social and economic topics such as the labor market, education, demography, housing, transport and the environment.

The sample used for this analysis consists of West German men, who had their 55^{th} birthday at some point between 2000 and 2004. I consider only individuals who were employed in a firm that was drawn as part of the original LIAB panel.[4] Peer groups are constructed using the firm identifier, which differs

[2] See Fischer et al. (2009) and Heining et al. (2016) for more information on the LIAB longitudinal model 1993-2014 (LIAB LM 9314) data documentation and methodology.

[3] Bundesinstitut für Bau-, Stadt- und Raumforschung (BBSR) im Bundesamt für Bauwesen und Raumordnung (BBR), Bonn (2016).

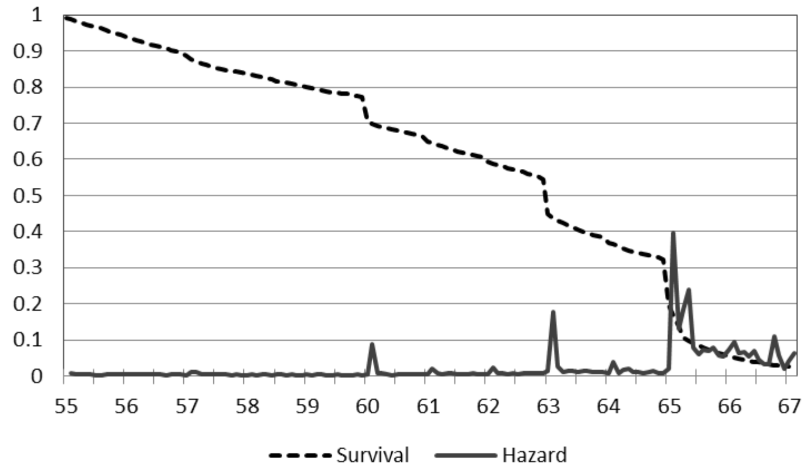
[4] The LIAB data contain many more firms through the individual employment histories. See Heining et al. (2016) for an overview of the data structure.

by location, and occupational main-groups (two ciphers) defined by the German classification of occupations 2010 (*KldB 2010*).[5] Data are aggregated to a monthly panel starting with the month of the 55^{th} birthday ($t = 0$) until employment exit or right-censoring. Note that t corresponds to the age of the primary observation unit in months, in distance to the month of the 55^{th} birthday. I keep the youngest individual within a group as primary observation unit, and drop those who do not have any older coworkers at $t = 0$. The youngest cohort is born in 1949 and workers typically exit employment at or before they reach the normal retirement age. Therefore, most exits are observed. The final sample used for the analysis comprises $3,203$ men (primary observation units), who are observed from their 55^{th} birthday until they exit employment. For every individual in the primary sample, I select the respective peers, which are defined as coworkers at the point in time when the individual turns 55 ($t = 0$). While I only consider men as primary observation units, there is no restriction on peer gender. On average, every individual has 13.5 peers. Peer variables are coded as characteristics of the individual defined as the primary observation unit within the same peer group. Sample characteristics can be found in Table 2.6 in the Appendix.

The dependent variable is the probability of the current period being the last period of employment, conditional on employment up to the current period. Whether an individual actually exits into retirement or another status is only observed in a few cases. Therefore, I focus on employment exits in this analysis. The first employment exit above age 55 is coded as final exit (absorbing state). In the baseline specification, individuals are assumed to exit employment when they

[5] *Bundesagentur für Arbeit.* The KldB 2010 has a high compatibility with the Classification of Occupations 2008 (ISCO-08). See Paulus and Matthes (2013) for a methodological report on the German classification of occupations.

Figure 2-1: Empirical survival rate and employment exit hazard by age



Notes: These statistics are unweighed and based on a subsample selected for analysis. Right-censored observations are excluded.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2014.

reach the age of 67, if they are still employed at that age.[6] The empirical survival rate in employment and the corresponding exit hazards are shown in Figure 2-1. Exits peak at the ages 60, 63, and 65.

The employment exit can be followed by either retirement, unemployment, or any other status without positive labor income. Out of the known statuses after the last observed employment spell, 39 percent exit through unemployment. However, in most cases, the status after exit is unknown (71 percent). This could be due to retirement or inactivity, but also because individuals enter self-employment or the public sector. In this analysis, I presume that employment exits after the age of 55 are unlikely to be followed by self-employment or em-

[6]I disregard employment spells beyond the age of 67. However, this concerns only very few observations.

ployment in the public sector. The main independent variable of interest is peer employment, which is defined as the share of peer workers who are still employed out of the total number of peers at $t = 0$. Even though it is possible that workers join or leave the firm or occupation, the peer group is defined to be constant in this analysis. Some workers change the occupation at a high age, however, most workers (98 percent) do not switch to another firm over the observation period, and may therefore still be a peer for the primary observational unit. If a peer is leaving the firm into non-employment, this is considered as an exit.

The financial incentive of exiting employment is modeled as described in Section 2.2.2. The option value is defined as the difference between the present discounted value of employment up to the normal retirement age $r = r^*$ and exiting in the current period $r = t$ (see Equation 2.1). The value of exiting employment at age r is computed as the sum of expected future labor earnings and social assistance and pension claims up to the average life expectancy T by district and gender (see Equation 2.2). Expected future earnings equal average gross labor earnings over the past 12 months. Expected pension benefits depend on the entire employment history in Germany. However, pension contributions and earnings histories are only partially observed over the work life in the LIAB data. Missing values had to be imputed using the current position in the earnings distribution and the estimated date of first labor market entry in case of left-censoring.[7] In this specification of the the option value, individuals are expected to claim pension benefits if they exit employment at or after the early retirement age. Individuals who exit before reaching the ERA are assumed to claim unemployment benefits in proportion to their previous income. Unemployment benefits can be claimed for

[7] The date of first labor market entry is estimated using gender and education.

a maximum of two years, depending on age and eligibility, or until the ERA. If an individual is neither eligible for unemployment nor pension benefits, I assume a minimum social assistance benefit of €631.[8]

Private savings and assets are not observed and it is not possible to link household members in the LIAB dataset. Consequently, wealth, spouses' employment status, and household income cannot be included in the empirical model, even though these variables might also be important determinants of employment exit behavior. However, omitting these variables does not bias the peer effect estimates as long as they are uncorrelated with peer behavior.

2.3.2 Econometric model

The estimation of peer effects bears several challenges. One challenge for the identification of peer effects is posed by the potential endogeneity of social networks. The social network or peer group is defined as the group of people with the same occupation and age group employed in the same firm. However, employees are not randomly matched to firms but rather choose their employer based on observed or unobserved firm characteristics. One could argue that an employee with a high preference for leisure is more likely to end up in a firm with low productivity and more likely to exit early. Consequently, unobserved factors like preferences for leisure affect both the network formation and the employment exit decisions. A second challenge is posed by correlated effects within peer groups. It can often not be excluded that contextual factors, such as workplace conditions, or common

[8]Social assistance benefits for singles in 2008, including an average housing allowance (Bundesagentur für Arbeit, Statistik, 2008).

shocks, such as downsizing of a firm, affect all members within a peer group and therefore lead to correlated outcomes. Consequently, I need to account for sorting of workers into firms and occupations as well as peer group specific shocks or time trends that affect employment exit behavior in the entire occupation or firm and could mistakenly be interpreted as peer effect (see Manski, 1993, for a discussion of the challenges associated with the estimation of peer effects).

Cornelissen et al. (2017) suggest to use several fixed effects to avoid these identification issues. I follow a similar approach and introduce year, occupation, and firm fixed effects to account for sorting of workers into firms and occupations and to capture common shocks.[9] If I can control for all characteristics of the firm and occupation that affect the decisions of the employees at the point of network formation, it can be assumed that exogeneity of the network holds conditional on these characteristics. Firm and occupation specific fixed effects account for observed and unobserved similarities of employees within a firm and within occupational groups. Consequently, endogenous sorting into peer groups is accounted for despite the fact that workers sort into firms and occupations non-randomly. However, I need to assume that there is no selection on unobserved firm-occupation specific characteristics. Furthermore, occupation specific shocks are accounted for by the inclusion of year-occupation interactions in an alternative specification. In addition, I include a broad set of individual and time-varying

[9]The identification strategy resembles that of Cornelissen et al. (2017), who use similar data and peer group definition in their paper on peer effects in the context of productivity and wages. Their data differ from the dataset we are using as they have information on all firms in the Munich region while we are using a random sample of firms in Germany. Furthermore, they include individual and group-specific fixed effects, which is not possible in my setting because I only observe one transition per individual.

characteristics in several specification tests to account for observable differences between peer groups.

Employment exit decisions can be modeled as the probability to exit in t, given financial incentives, the retirement behavior of the peer group and several observed taste shifters (e.g. individual and labor market characteristics). According to Berkel and Börsch-Supan (2004), inserting the option value into a simple regression model of retirement or employment exit decisions can be interpreted as a flexible discrete-time duration model explaining the timing of the exit, even though the structure of the dynamic optimization that underlies individual decisions is ignored.[10]

The empirical model is formulated as a linear probability model, where E_{it} is the probability that individual i exits employment with age t, conditional on still being employed up to t:

$$\begin{aligned} E_{it} = {} & \sum\nolimits_f \psi_{f,i} + \sum\nolimits_o \phi_{o,i} + \sum\nolimits_y \lambda_{y,t} + \beta OV_{it} + \gamma PE_{it} \\ & + \theta_1 AGE_{it} + \theta_2 AGE_{it}^2 + \theta_3 I[AGE \geq ERA]_{it} + X_{it}'\theta_4 + \epsilon_{it} \end{aligned} \tag{2.3}$$

In addition to the firm ($\psi_{f,i}$), occupation ($\phi_{o,i}$) and year ($\lambda_{y,t}$) fixed effects, the empirical model includes linear and quadratic age trends and a measure that captures the pure financial incentive to exit employment in each period, the option value OV_{it}. The peer effect enters the linear probability model as the coefficient of PE_{it}, which is defined as the share of i's coworkers who are still employed in period t, out of the total number of peers at $t = 0$. Depending on the specification,

[10]See Lumsdaine et al. (1992) for a comprehensive account of the relative predictive properties of the full dynamic optimization model and the option value approach.

I include a different set of individual, group, and time-varying regional characteristics in X_{it}. In the baseline specification, X_{it} includes indicators for migrant status, high and low education, the share of peers with high and low education, the number of peers, average peer age, the share of women in the peer group, the district unemployment rate, the share of over 65 year-old in the district, and the district population density.

The identification strategy is not valid if individuals within a peer group behave similarly for unobserved reasons that are neither peer effects nor correlated effects that have been accounted for in the empirical model as specified in Equation 2.3. Firm-occupation specific factors such as management decisions potentially influence the employment exit behavior of employees within a specific occupation, but not in the whole firm. Therefore, the estimated coefficients of peer employment can only be interpreted as pure peer effects under the assumption that there are no unobserved firm-occupation (peer group) specific correlated effects or shocks.

Ideally, causal peer effects are identified using a quasi-experimental research design. This could, for example, be done using a pension reform that affects coworkers heterogeneously, depending on the year of birth, while it does not affect the primary observation units. The peer effect would then be identified using the exogenous variation in peer employment exit behavior. A similar research design was used by Welteke and Wrohlich (2016), based on a German parental leave benefit reform. However, this is beyond the scope of this paper.

2.4 Results

2.4.1 Baseline estimates

I estimate the model described in Equation 2.3 in Section 2.3.2 using different sets of covariates. A negative coefficient indicates that the explanatory variable decreases the probability of an employment exit. All coefficients and standard errors are multiplied by 100. Consequently, a coefficient of -0.5 corresponds to a 0.5 percentage point decrease in the average exit hazard per period if the explanatory variable increases by one unit. The option value variable (OV_{it}) is standardized with mean zero, i.e. the coefficient corresponds to an increase in the option value by one standard deviation. In Column (1) of Table 2.1, the results of a linear regression model without peer outcomes and characteristics are displayed. The model includes year, firm and occupation fixed effects. The results of a *naive* linear regression model with neither covariates nor firm and occupation fixed effects are displayed in Column (2) of Table 2.1. I expect the peer effect in this specification to be biased, because correlations within peer groups due endogenous sorting or correlated effects might be captured by the coefficient of the peer parameter. The results displayed in Column (3) correspond to the baseline regression model, including year, firm and occupation fixed effects. This specification is used for the policy simulation in Section 2.4.4. The model that underlies the results that are displayed in Column (4) differs from the baseline specification in (3) only by the interaction of occupation and year fixed effects. I use specification (3) as the preferred specification because the inclusion of year-occupation interactions increases the computational effort without changing the results. In Column (5), the regression includes a full set of age (in months) dummies to non-parametrically capture other systematic age effects on employment exit decisions.

There are two coefficients that are of particular interest in this analysis. First, I discuss the effects of the share of peers that is still employed. The coefficients of the share of employed peers PE_{it} are significantly different from zero, negative, and of similar magnitude, ranging from -0.224 to -0.372, over all specifications. The peer effect estimate of the baseline specification, displayed in Column (3), can be interpreted as the effect of a change in the share of employed peers from zero to one: if no coworker has left employment, the average individual hazard to exit employment decreases by 0.371 percentage points, compared to a situation where all coworkers left.[11]

Second, I discuss the influence of the measure of financial incentives. A larger option value decreases the probability to exit employment with sizeable negative coefficients. This is in line with intuition: the more attractive it is to stay employed, the lower is the probability to exit now.[12]

The probability to exit employment increases with age, in particular if early retirement age thresholds are reached at age 60 and 63. The coefficient of the indicator for reaching the NRA ($Age \geq NRA$) is only significant at the 10 percent level.

[11] Estimating a nonlinear probability model (logit or probit model) of the specification displayed in Column (2) leads to very similar peer effect estimates (not displayed here).

[12] Other authors Berkel and Börsch-Supan (2004, e.g.) use different specification of the leisure preference parameter α in the option value. I repeat the analysis with $\alpha = 2.8$. If α is large, income from retirement is valued higher than income from labor. Consequently, the option value of remaining employed decreases and can be negative. While the interpretation of the results does not change, the coefficient of the option value decreases in magnitude in this specification. Results are not displayed here.

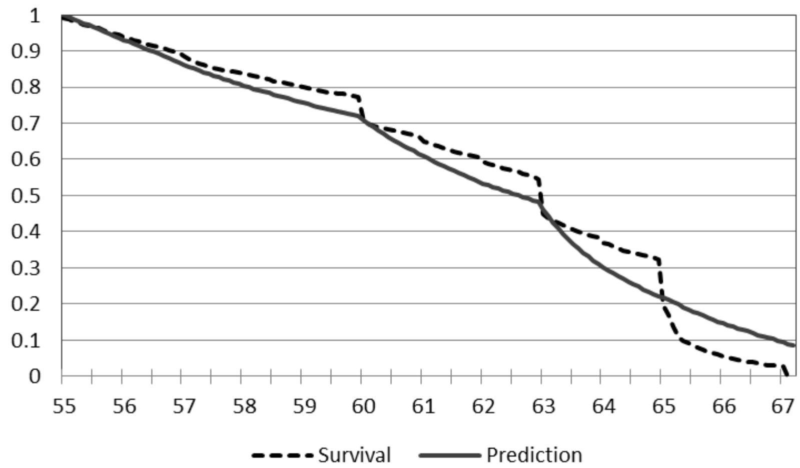
Table 2.1: Baseline regression analysis

E_{it}	(1)	(2)	(3)	(4)	(5)
PE_{it}		-0.224***	-0.371***	-0.372***	-0.326***
		(0.050)	(0.101)	(0.101)	(0.097)
OV_{it}	-0.480***	-0.351***	-0.485***	-0.520***	-0.503***
	(0.049)	(0.029)	(0.050)	(0.052)	(0.048)
Age	0.004	-0.017***	0.016***	0.015***	
	(0.004)	(0.005)	(0.005)	(0.005)	
Age^2	0.000	0.000***	0.000**	0.000**	
	(0.000)	(0.000)	(0.000)	(0.000)	
$Age \geq 60$	1.010***	0.804***	1.144***	1.189***	
	(0.128)	(0.136)	(0.136)	(0.140)	
$Age \geq 63$	3.157***	2.301***	3.751***	3.870***	
	(0.289)	(0.330)	(0.351)	(0.362)	
$Age \geq NRA$	-0.148	2.187***	0.957*	0.917*	
	(0.355)	(0.598)	(0.516)	(0.536)	
High education	-0.091		0.009	0.109	0.009
	(0.114)		(0.121)	(0.121)	(0.113)
Low education	-0.046		-0.041	-0.021	-0.055
	(0.072)		(0.071)	(0.072)	(0.069)
Migrant	0.098		0.055	0.046	0.032
	(0.101)		(0.097)	(0.098)	(0.095)
Number of Peers			0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)
Peer age			0.001	0.001	0.002
			(0.001)	(0.001)	(0.001)
Share peer women			0.108	0.053	0.046
			(0.122)	(0.123)	(0.118)
Peers high education			0.079	0.085	0.036
			(0.181)	(0.186)	(0.173)
Peers low education			0.011	-0.010	-0.050
			(0.104)	(0.106)	(0.101)
Unemployment rate	-0.004		-0.003	0.001	0.034***
	(0.000)		(0.016)	(0.016)	(0.013)
Share over 65	0.028		0.022	0.021	-0.038*
	(0.000)		(0.024)	(0.024)	(0.022)
Population density	0.000*		0.000**	0.000**	0.000***
	(0.000)		(0.000)	(0.000)	(0.000)
N_c	3131	3152	3130	3130	3130

*** p<0.01, ** p<0.05, * p<0.1.

Notes: All coefficients are multiplied by 100. N_c is the number of individuals in the sample. The option value variable (OV) is standardized with mean zero. Standard errors are clustered by person. Regression specifications (1) and (2) include year dummy variables; (3) includes year, firm and occupation fixed effects; in (4) occupation and year fixed effects are interacted; (5) includes year and age (in months) fixed effects.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2014.

Figure 2-2: Empirical survival rate and predicted survival by age



Notes: These statistics are unweighed and based on a subsample selected for analysis. Right-censored observations are excluded.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2014.

2.4.2 Model fit

I use the baseline regression model displayed in Column (3) of Table 2.1 to predict individual exit hazards for every age and compute the corresponding survival curve, which is displayed in Figure 2-2, along with the empirical survival rate.

It can be seen in Figure 2-2 that the model captures the general employment exit dynamic relatively well. The predicted average exit age is one month lower than the empirical exit age of 62 and 2 months. However, the model fails to accurately predict the steep decrease in the survival curve, or peaks in the hazard function, at age 60, 63 and 65. This can be explained by the inclusion of age thresholds instead of dummy variables for the exact ages. As expected, the inclusion of a full set of age dummy variables improves the fit of the model. However, this specification has the disadvantage that there is no real interpretation of the age effects, which non-parametrically capture all age-specific exit patterns. The advantage of a model with linear age trends and age thresholds at the statutory retirement ages is that the thresholds-indicators can be interpreted as legal barriers. Further financial retirement incentives are captured by the option value.

2.4.3 Heterogeneity by subgroups

Workers with different characteristics may respond heterogeneously to financial incentives and peer behavior. For example, it is possible that some groups do not have the flexibility to respond to employment incentives due to financial or physical constraints. In the following, I look into the question whether different subgroups are affected heterogeneously by financial incentives and by their peers. First, I look at several subgroups by income, education, part-time employment, firm size, group size and district population density. The sample is split at the median in the case of continuous variables. The results are displayed in Table 2.2.

The results suggest that peer effects are larger for low income workers, for part-time workers, in small firms, and in districts with a low population density. However, these differences are not statistically significant. The only subgroup with a significantly larger peer effect is characterized by above median peer group size (defined as having more than 5 members). One interpretation could be that in very small groups, the exit of one coworker makes the remaining workers more indispensable, which would lead to smaller or even negative peer effects. Overall, I do not find evidence for large heterogeneity in peer effects for different subgroups. The coefficient of the option value is largest for the subgroups with low education

Table 2.2: Regression results for different subgroups

	Low income	High income	Low education	High education
PE_{it}	-0.545***	-0.423***	0.231	-0.347***
	(0.166)	(0.148)	(0.344)	(0.110)
OV_{it}	-0.821***	-0.436***	-1.062***	-0.45***
	(0.118)	(0.089)	(0.214)	(0.055)
N_c	1434	1452	921	2641

	Small firm	Large firm	Small group	Large group
PE_{it}	-0.495***	-0.376***	-0.368***	-0.855***
	(0.149)	(0.140)	(0.124)	(0.258)
OV_{it}	-0.654***	-0.348***	-0.56***	-0.405***
	(0.083)	(0.061)	(0.080)	(0.071)
N_c	1536	1563	1748	1375

	Part-time	Full-time	Low density	High density
PE_{it}	-0.636**	-0.31***	-0.469***	-0.339**
	(0.290)	(0.113)	(0.142)	(0.154)
OV_{it}	-0.24*	-0.586***	-0.504***	-0.511***
	(0.132)	(0.058)	(0.076)	(0.073)
N_c	1014	3050	1567	1554

*** p<0.01, ** p<0.05, * p<0.1.

Notes: All coefficients are multiplied by 100. N_c is the number of individuals in the sample. The option value variable (OV) is standardized with mean zero. Standard errors (in parentheses) are clustered by person. All regressions include age, age^2, retirement age thresholds, indicators for migrant status, high and low education, the share of peers with high and low education, the number of peers, average peer age, the share of women in the peer group, the district unemployment rate, the share of over 65 year-old in the district, and the district population density, and year, firm and occupation fixed effects.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2014.

or income, and for individuals working in small firms. This is in line with the intuition that financial incentives are stronger for individuals with lower earnings.

The analysis is repeated for four different groups of occupations. The German classification of occupations 2010 (*KldB 2010*) includes 10 different occupational main categories (*Berufsbereiche*). However, some of these categories have very few observations in my sample. Therefore, I had to aggregate several categories into bigger groups.[13] The first group (A) includes all occupations in the fields of agriculture, construction, natural science, computer sciences, and information technology. The second group (B) comprises production, resource extraction, and manufacturing. Occupations in the fields of administration, management, social sciences, culture, arts, and languages are summarized in group (C); and transport, logistics, and security in group (D). The regression results by occupational group are displayed in Table 2.3.

Table 2.3: Regression results for different occupational groups

	(A)	(B)	(C)	(D)
PE_{it}	-0.372	-0.480**	-0.363*	-0.568**
	(0.317)	(0.196)	(0.212)	(0.283)
OV_{it}	-0.566***	-0.275***	-0.213***	-0.437***
	(0.121)	(0.070)	(0.077)	(0.115)
N_c	588	1151	811	714

Notes: All coefficients are multiplied by 100. N_c is the number of individuals in the sample. The option value variable (OV) is standardized with mean zero. Standard errors (in parentheses) are clustered by person. All regressions include age, age^2, retirement age thresholds, indicators for migrant status, high and low education, the share of peers with high and low education, the number of peers, average peer age, the share of women in the peer group, the district unemployment rate, the share of over 65 year-old in the district, and the district population density, and year, firm, and occupation fixed effects.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2014.

[13] I aggregated categories 1, 3, and 4 to group (A). Categorie 2 equals group (B) and category 5 equals group (D). Categories 6, 7, 8, and 9 are summarized by group (C).

Peer effects are strongst in the groups of production, resource extraction, and manufacturing (B) and transport, logistics, and security (D). A closer look at the characteristics of this group (see Table 2.7 in the Appendix) reveals that individuals who are working in these sectors sector have lower earnings and education, and a larger probability of working in small firms than the average individual in the overall sample. Note that the coefficients for all subgroups have large standard errors and are therefore not significantly different from the baseline estimate.

The option value has a significant negative coefficient on all four occupational groups. The coefficients are smaller than the baseline estimate for the groups working in production, resource extraction, and manufacturing (B), and occupations in the fields of administration, management, social sciences, culture, arts, and languages (C). Group (C) is characterized by high earnings and education, and more large firms on average (see Table 2.7 in the Appendix).

2.4.4 A policy simulation of peer effects

One of the central research questions of this paper is to what extent peer effects can magnify the effect of policies in addition to the direct effect through changes in financial incentives. In 2007, the German government initiated a reform that increases the normal retirement age stepwise from 65 to 67 by year of birth until 2031. The first affected cohort, born in 1947, can retire without pension deductions with an age of 65 years and one month. The NRA increase is fully phased in with the cohort of 1964, which will reach age 67 in the year of 2031.

I apply the estimated baseline coefficients (Column 4, Table 2.1) to simulate the effects of a universal and immediate increase of the NRA to 67. The reform enters the model through a change in the option value, which is adjusted accounting for higher pension benefit deductions for the same retirement ages and an increased optimal exit age, $r^* = 67$. Early retirement age thresholds remain unchanged. I use the predicted pre- and post-reform survival and hazard rates to compute the exit probabilities for every age. The resulting probabilities are then used to compute average exit ages, which are displayed in Table 2.4. I find that the NRA reform results in an increase in the average exit age by 2.4 months.[14]

Table 2.4: Direct reform effects on average exit ages

Variable	Mean	in years	N
Pre-reform exit age	735.8	61.3	2,945
Post-reform exit age	738.2	61.5	2,945
Difference	2.4	0.2	

Notes: Exit ages are calculated using predicted exit hazards for every age, based on the baseline regression model including firm, year, and occupation fixed effects. Note that individuals are assumed to exit employment at the latest when they reach their respective normal retirement age.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2014.

The predicted effect on the effective employment exit age is relatively low compared to the literature. There are several reasons why the NRA increase does not result in a larger increase in predicted exit ages using this model. One

[14]Note that in the baseline regression used here, individuals are assumed to exit at the latest when they reach the NRA regardless of their employment status. I made this assumption because my model fails to predict the large drop in employment at the NRA and therefore does not capture the NRA increase accurately.

explanation is that individuals respond to increases in legal retirement thresholds, while their behavioral response to changes in pension deductions associated with early retirement is small. However, the small size of the reform effect could also be due to imprecisions in the empirical model. The simulated reform amounts to an increase in the option value for most observations, which stems from (a) larger pension benefit deductions between the ERA and the new NRA; and (b) an increased reference age r^*. If current labor earnings are lower than expected pension benefits, the option value of remaining employed can also be lower than without the reform, which would lead to an earlier employment exit. Expected income streams from pension benefits are imprecisely estimated using imputed earnings histories. Consequently, the results have to be interpreted with caution.

Berkel and Börsch-Supan (2004) simulate a change in the normal retirement age of 65 by one and two years. They find that a two year NRA increase would increase the average effective retirement age by 9 months. There are several reasons why my results cannot be compared directly to those of Berkel and Börsch-Supan (2004). Apart from methodological differences,[15] the dependent variable in their analysis is retirement, not employment exit. I expect that a change in the NRA affects retirement entry stronger than employment exit behavior, because some workers might exit employment regardless of the reform, using alternative paths such as the disability pension program, inactivity or unemployment. Furthermore, the simulation performed in Berkel and Börsch-Supan (2004) assumes a simultaneous increase in all statutory retirement ages, that is the normal and

[15]Berkel and Börsch-Supan (2004) use a similar empirical model, however, they use a different option value specification with a larger valuation of retirement wealth ($\alpha = 2.8$), based on the GSOEP 1984-1997.

early retirement age, while I only increase the NRA. Therefore, it is not surprising that Berkel and Börsch-Supan (2004) find a larger simulated reform effect.

In the following, I use the 2.4 month increase in the average employment exit age to simulate the indirect policy effects through the change in peer behavior. Peer employment exits are artificially delayed by 2.4 months on average, as if all peer coworkers were affected by the NRA increase, while option value and age thresholds remain constant for the primary observation units. Thereby, the peer employment variable PE_{it} is increased for every t. Hazard and survival rates are again predicted based on the empirical model described in the last section, and exit ages are computed based on the predicted exit probabilities. The resulting exit ages are displayed in Table 2.5.

Table 2.5: Indirect reform effects on average exit ages through changes in peer exits

Variable	Mean	in years	N
Exit age, baseline	735.8	61.3	2,945
Exit age, simulated peer exits	736.5	61.4	2,945
Difference	0.7	0.1	

Notes: Exit ages are calculated using predicted exit hazards for every age, based on the baseline regression model including firm, year, and occupation fixed effects. Note that individuals are assumed to exit employment at the latest when they reach their respective normal retirement age.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2014.

The effect of a 2.4 months delay in peer exits on the predicted exit age amounts to a 0.7 months increase, if the option value and statutory retirement age thresholds are kept constant (see Table 2.5). The total effect of both the change in the individual financial exit incentives and adapted peer behavior amounts to a 3.1 months increase in the effective exit age. In other words, I find that the direct policy effect is magnified substantially by 29 percent through the change in peer behavior. I conclude that peer effects can amplify direct reform effects substantially.

2.5 Conclusion

In this paper, I estimate how peers at the workplace and financial incentives affect individual employment exit decisions. The baseline regression results suggest that there is a significant negative effect of both the option value of continued employment and the share of older coworkers that is still working on individual employment exit probabilities. The average individual exit hazard decreases by about 0.4 percentage points, if all coworkers are still employed, compared to a situation where all former coworkers left employment. A larger option value decreases the probability to exit employment by about 0.5 percentage points per period. The coefficient of the option value is largest for the subgroups with low education or income, and for individuals working in small firms. Furthermore, I simulate an increase in the normal retirement age to 67, which leads to a modest increase in the predicted exit age by 2.4 months. A delay of peer employment exits by 2.4 months results in an increase in the individual exit age by additional 0.7 months. I conclude that peer effects can amplify direct reform effects substantially.

However, the identification of peer effects bears some challenges. Peer effects are only identified under the assumption that there are no unobserved firm-occupation (peer group) specific correlated effects. A possible future extension of the paper would therefore be a reduced form analysis of peer effects in employment exit decisions. Examples of previous research exploiting quasi-experimental data to identify peer effects include Rege et al. (2012), Brown and Laschever (2012), Dahl et al. (2014) and Welteke and Wrohlich (2016). Results from a regression discontinuity research design could be used to verify our findings using a discontinuity in employment exit incentives created by a pension reform.

2.6 Appendix

Table 2.6: Baseline sample characteristics

	Mean	SD	N
Migrant status	0.07	0.26	3203
Waiting period	29.00	7.25	3127
Average gross monthly earnings at age 55	3338.75	1073.12	2951
Option value (in 1000)	234.26	124.45	3148
High education	0.05	0.22	3203
Low education	0.15	0.36	3203
Number of coworkers in peer group	13.52	50.22	3203
Average peer option value (in 1000)	90.08	58.39	3202
Average peer age	709.42	26.22	3203
Share of women in peer group	0.16	0.27	3203
Average peer earnings at age 55	3082.04	920.53	2387
Share of peers with high education	0.04	0.16	3203
Share of peers with low education	0.18	0.28	3203
Share part-time workers	0.00	0.02	1238
Collective wage agreement	0.80	0.40	1236
Age of firm in years	13.42	3.13	1223
Firm size	271.03	1324.55	2752
Partial retirement available	0.30	0.46	2893
District unemployment rate	8.68	3.68	333
District share over 65	18.47	2.07	333
District population density (per km^2)	554.62	706.37	333
District life expectancy at age 60 (in months)	299.24	24.19	346

Notes: These statistics are unweighed and based on a subsample selected for analysis.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2014.

Table 2.7: Subsample characteristics

Group	Earnings at 55	High edu	Low edu	Firm size	Group size	Density	Part time	Migrant	N
Low income	2460.0	0.012	0.249	833.1	12.7	738.6	0.056	0.108	1434
High income	4218.4	0.092	0.056	1966.0	17.5	763.8	0.009	0.036	1452
Low education	2616.5	0.000	0.970	1110.1	13.0	829.1	0.046	0.221	921
High education	3506.2	0.063	0.000	1399.9	14.8	734.6	0.030	0.039	2641
Part-time	1387.2	0.101	0.193	943.1	16.8	654.2	1.000	0.101	1014
Full-time	3404.4	0.049	0.150	1369.3	14.4	759.0	0.000	0.072	3050
Small firm	3127.5	0.049	0.129	148.3	6.9	664.6	0.055	0.067	1536
Large firm	3564.3	0.054	0.173	2574.2	22.3	841.8	0.019	0.078	1563
Small group	3280.7	0.045	0.148	745.4	3.5	710.8	0.031	0.063	1748
Large group	3410.1	0.059	0.157	2131.2	28.5	807.3	0.045	0.085	1375
Low density	3287.8	0.040	0.136	1090.4	13.6	172.1	0.039	0.042	1567
High density	3393.7	0.062	0.165	1619.9	15.5	1334.4	0.035	0.094	1554
Occupations (A)	3377.7	0.059	0.157	1571.6	10.8	814.6	0.023	0.086	588
Occupations (B)	3295.4	0.017	0.136	1199.6	16.7	697.5	0.015	0.087	1151
Occupations (C)	3962.4	0.134	0.030	1607.7	15.8	750.1	0.049	0.019	811
Occupations (D)	2634.3	0.006	0.323	1173.6	12.2	815.0	0.067	0.102	714

Notes: These statistics are unweighed and based on a subsample selected for analysis.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2014.

Chapter 3

Peer Effects in Parental Leave Decisions

3.1 Introduction

Parental leave policies and maternal reactions to it are a widely discussed topic among policymakers and researchers. Policies that enable prolonged parental leave spells, including parental leave benefits and job protection policies, may help protect young families and encourage fertility. However, mothers who take long leaves after giving birth might loosen their labor market attachment with the well-known consequences of decreased career prospects and life-time earnings.[1] Furthermore,

[1] For the non-linear relationship between maternal leave duration and labor market outcomes, see Ruhm (1998).

employment interruptions due to motherhood can result in greater gender inequality.[2]

Parental leave behavior, and more generally labor supply of mothers with young children, differs greatly across time and regions.[3] Numerous studies explain part of these changes over time or the variation between countries based on standard economic models, attributing them to differences in financial incentives caused by institutional diversity. However, institutional differences cannot explain all divergence in the labor supply behavior of mothers across countries.[4] More recently, a growing strand of the literature on female labor supply and parental leave decisions suggests alternative or complementary explanations for differences between countries or over time. For example, Fernandez (2013) attributes part of the increase in women's labor supply in the US over the last century to changes in culture. While there is a tradition in sociology and anthropology of focusing on the importance of social structure, norms, and culture, most economists have long neglected social influences on individual behavior. However, an increas-

[2]Increasing maternal labor supply over the life-cycle is a key factor in closing the gender wage gap (e.g. Polachek and Xiang, 2006).

[3]Classical references for the evolution of women's employment in the US include, among others, Goldin (1990) and Blau and Kahn (2006). For Germany, time trends in female employment patterns are documented e.g. by Fitzenberger and Wunderlich (2004). Cross-country differences explored e.g. by Bick and Fuchs-Schündeln (2014) and Blundell et al. (2013).

[4]For example, the paper by Bick and Fuchs-Schündeln (2014) shows that differences in male labor supply behavior between US and Western Europe can be largely explained by economic variables such as the tax system and the distribution of wages. However, the same model can only explain about 40 percent of the difference in female labor supply based on these economic variables. Similarly, a paper by Dearing et al. (2007) comparing two culturally very similar countries – Austria and West Germany – shows that differences in financial incentives only explain 20 percent of the total difference in the full time employment rate of mothers with children under age 10 in both countries. Moreover, several papers show that, although mothers in East and West Germany have shared the same institutional setting for more than 20 years, there are still persistent differences in labor supply behavior (see Rosenfeld et al., 2004; Grunow and Müller, 2012).

ing number of economic studies are based on the assumption that individuals do not exist in isolation but are embedded within networks of relationships, such as families, coworkers, neighbors, friends, or socio-economic groups. For instance, several studies analyze the influence of social interaction on labor supply within geographic neighborhoods (e.g. Weinberg, Reagan, and Yankow, 2004; Fogli and Veldkamp, 2011) and family networks (e.g Del Boca, Locatelli, and Pasqua, 2000; Dahl, Løken, and Mogstad, 2014; Neumark and Postlewaite, 1998; Nicoletti, Salvanes, and Tominey, 2016).

In this study, we focus on the parental leave decisions of mothers and how these are affected by peers at the workplace. While many different social networks are important for individual decisions, we believe that workplace social networks play a particularly important role as far as labor supply related decisions are concerned, because the workplace facilitates the formation of social ties and, thereby, the transmission of behavioral norms and career-related information.

In the following, we refer to peer effects as the effects of a social reference group's behavior on individual outcomes. There are several channels through which the change in parental leave behavior of a social reference group can influence individual decisions. These include preferences for conformity to social norms, learning about the costs and benefits of parental leave, as well as leisure complementarities.

We argue that preferences for conformity and the transmission of information about the costs and benefits of a (long) parental leave are both possible mechanism of peer effects in our context. Information transmission is expected to be particularly important in situations with high career-related uncertainty. Observing peer mothers at the workplace, more specifically the employers' reaction to peer mothers' parental leave choices, may reduce uncertainty concerning the consequences of leave choices. Other channels that can give rise to peer effects include the transmission of practical knowledge about the existence and organizational details of the parental leave program, as well as leisure complementarities or work externalities. Leisure complementarities would imply that mothers benefit from taking leave simultaneously, whereas work externalities occur if the absence of one mother makes it more difficult for her coworkers to take leave. While the the transmission of practical knowledge can be ruled out as a mechanism that drives our results because the parental leave benefit program is universal and well-known, leisure complementarities and work externalities are unlikely to be relevant because of the temporal distance of the parental leave of peer mothers and their coworkers.

In addition to presenting empirical estimates of the magnitude of peer effects in the context of parental leave decisions of mothers, we believe that our study has some policy relevant implications. When social interaction effects are quantitatively important, policy interventions on single agents might have large effects through so-called social multipliers (see Glaeser, Sacerdote, and Scheinkman, 2003). Although empirical studies frequently attempt to infer individual behavior from observed aggregate outcomes, when there is social interaction, aggregate coefficients will be larger than individual coefficients because there is a direct effect of policy changes on individual behavior and an indirect effect through the effects on the social reference group.

The identification of peer effects is challenging due to correlated characteristics within social groups and endogenous group membership (see Manski, 1993; Blume, Brock, Durlauf, and Ioannides, 2010, for an overview). Several studies (e.g. Dahl et al., 2014; Brown, 2013) suggest using policy reforms as instruments to address the identification challenges. We follow this suggestion and identify social interaction effects in the labor supply of mothers with young children using the exogenous variation introduced by the 2007 reform of the parental leave benefit (*Elterngeld*) in Germany, which, in particular, encourages high-income mothers to remain at home during the first 12 months following childbirth.[5] We use administrative linked employer-employee data from the Institute for Employment Research (IAB), which enables us to assign a peer group to all individuals who work in the same establishment and occupational group. The identifying variation stems from the exposure of our sample to peer mothers who gave birth within a narrow window either before or after the parental leave benefit reform. While other papers used German administrative labor market data to identify peer effects in the context of fertility (see Pink et al., 2014), productivity (see Cornelissen, Dustmann, and Schönberg, 2017), and job searches (see Dustmann, Glitz, and Schönberg, 2011), this is the first paper to focus on peer effects in the context of maternal leave behavior.

Our results suggest that maternal decisions regarding the length of their own parental leave are significantly influenced by their coworkers' decisions. We find that a mother is about 30 percentage points more likely to stay at home for the first year if her peer(s) decide(s) to do so in response to the parental leave benefit

[5]See Bergemann and Riphahn (2015), Geyer et al. (2015), Kluve and Schmitz (2017), and Kluve and Tamm (2013) for an analysis of the effects of the 2007 parental leave benefit reform on maternal employment.

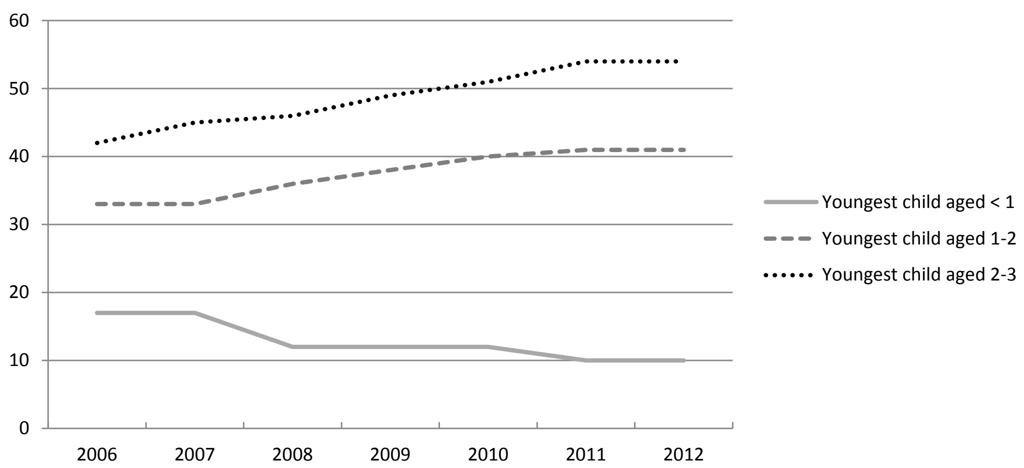
reform. This effect corresponds to the Local Average Treatment Effect (LATE). We also estimate the Intention to Treat Effect (ITT), showing that having peers who gave birth after the introduction of the new parental leave benefit increases the probability that a mother takes a leave of at least one year by 7 percentage points in contrast to mothers who have peers who gave birth shortly before this date. The results of analyses for those subgroups for whom uncertainty regarding the employer's reaction to parental leave decisions is expected to be higher, suggest that information transmission and the reduction of uncertainty that comes with observing peer behavior may be among the critical channels driving peer effects in our context.

The paper is organized as follows. In the next section we describe some stylized facts on maternal employment in Germany and discuss the institutional details of the 2007 parental leave reform that we use as an instrument in the empirical analysis. Section 3.3 sketches our methodological approach and explains our identification strategy. In section 3.4, we describe our data set and present some selected descriptive statistics. The baseline results of our empirical analysis and several robustness checks are presented in Section 3.5. This is followed by a discussion of the possible peer effect mechanisms based on heterogenous effects for different subgroups in Section 3.6. Section 3.7 concludes.

3.2 Institutional setting and stylized facts

Maternal employment is of increasing policy relevance in many OECD countries, because low fertility rates and an increasing old-age dependency ratio underlie a growing imbalance in public finances. Traditionally, employment rates of mothers are relatively low in West Germany, compared to e.g. France, the UK or the Nordic countries. However, since 2006, the employment rate of mothers with children above the age of one has been increasing, as seen in Figure 3-1. For example, the employment rate of mothers with a child aged 2 to 3 years was 42 percent in 2006, increasing to 54 percent in 2012. At the same time, the employment rate of mothers with children aged 1 to 2 years or 3 to 6 years has also been increasing. Over this same period, the employment rates of mothers with children below the age of one decreased. Consistent with this evidence, the mean duration of employment interruption following childbirth decreased between 2004 and 2010 (see Wrohlich et al., 2012).

Figure 3-1: Maternal employment rates by age of youngest child



Source: Federal Ministry of Family Affairs, Senior Citizens, Women and Youth, 2014.

Since 2005, several policy reforms have affected maternal employment. A major family policy reform was the expansion of subsidized child care for children under three years. Furthermore, several child care reforms have been carried out that have successively increased the availability of subsidized child care for children below three years. As of August 2013 every child has a legal claim to a slot in a publicly subsidized childcare institution after the first birthday.[6]

Another prominent policy reform is the parental leave reform introduced in 2007. Before implementation of this reform, families with a new born child were paid a cash benefit amounting to €300 per month for a maximum period of 24 months (chosen by most parents), or €450 per months for a period of 12 months, if at least one parent did not work more than 30 hours per week. This benefit, *Erziehungsgeld*, was means-tested at the household level. Less than 50 percent of the families with new born children were eligible due to the income test.

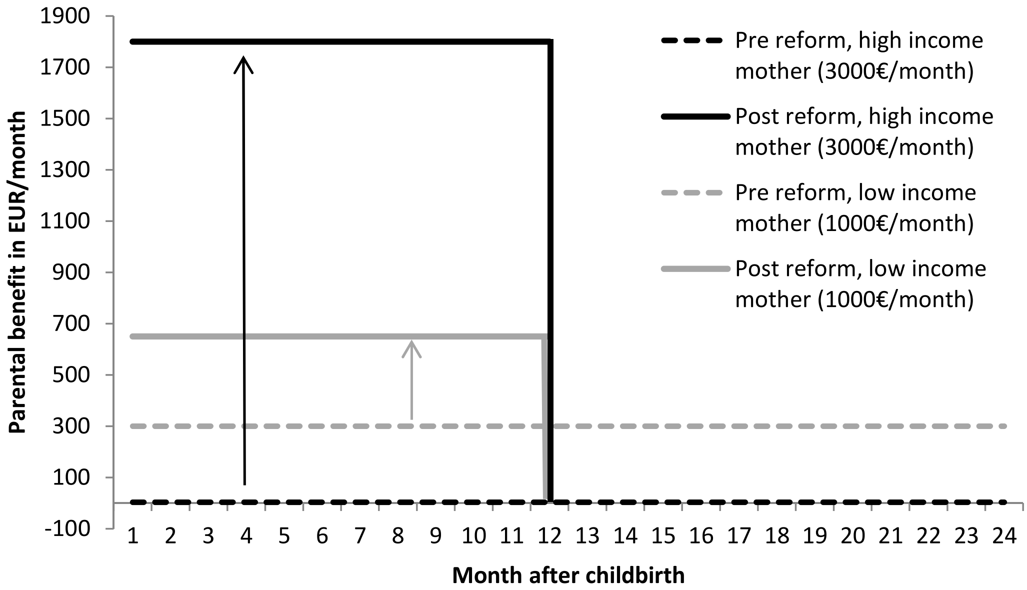
Starting in 2007, the new parental leave benefit, *Elterngeld*, replaced the *Erziehungsgeld*. Paid for a shorter period of time – 12 months if only one parent takes leave or 14 months if both parents take leave – *Elterngeld* is not means-tested on household income. The benefit awarded to parents depends on their earnings prior to birth, replacing 67 percent of previous net earnings, not to exceed 1,800 euro per month. The minimum amount of *Elterngeld* awarded is €300 per month, which is equivalent to the monthly benefit paid under the previous *Erziehungsgeld*.

Thus, the financial incentives induced by this reform differ between high- and low-income mothers as well as between the first and second year after giving

[6] In the same period, also the supply of afternoon care for school-children has been increased dramatically by the large expansion of all-day schools (see Beblo et al., 2005; Marcus et al., 2013).

birth. Figure 3-2 shows the amount of parental leave benefits paid to mothers with a monthly gross labor income of €3000 (high income) and €1000 (low income) respectively, before and after the introduction of the reform. For low-income mothers, financial incentives did not change as much during the 12 months after giving birth, however there clearly is the incentive to shorten their leave after their child turns one. Only mothers with a very low income, which entitles them to less that €450 *Elterngeld*, are incentivized by the reform to return to work in the first year. For medium- and high-income mothers, however, the reform provides incentives to stay at home during the first year after childbirth. These mothers were not eligible for a benefit under the old scheme and can now draw generous benefits amounting to about 67 percent of their prior-to-birth earnings.

Figure 3-2: Benefits paid before and after the reform for exemplary mothers



Source: Hypothetical benefits that can be received by married mothers with a net income of €3000 and €1000 per month respectively, based on the online benefit calculator of the Federal Ministry of Family Affairs, Senior Citizens, Women and Youth. https://www.familien-wegweiser.de/Elterngeldrechner/index.xhtml, accessed July 20, 2014.

By setting strong incentives to interrupt working by staying at home for (exactly) 12 months, the introduction of the *Elterngeld* set an institutional norm that children should be cared for by their parents at home until their first birthday.

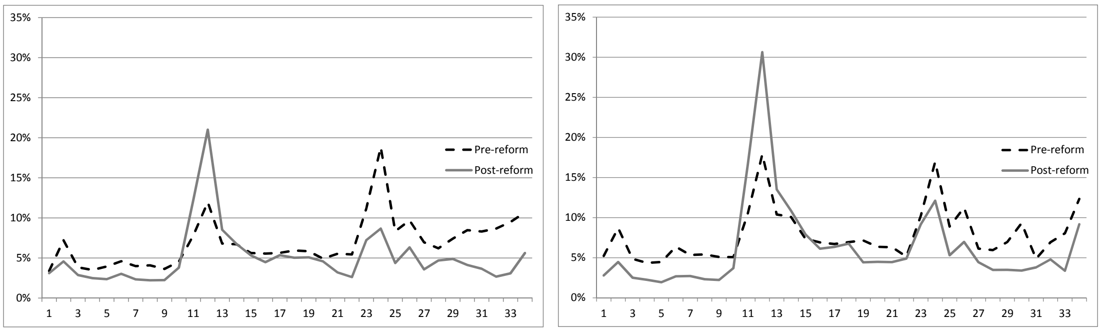
Norms regarding parental leave still differ greatly between East and West Germany. Before German reunification in 1990, East German mothers were much less likely to be out of the labor force or work part-time than West German mothers (see Rosenfeld et al., 2004). In East Germany, parental leave benefits were paid for one year, called the *Geburtsjahr*, and public childcare was generally available. In

West Germany, maternal labor supply was comparatively low and, by family policy, mothers were encouraged to stay at home or work part time. After reunification in 1990, social norms regarding maternal labor supply converged with both longer employment interruptions and part time employment becoming more common in the East. However, we argue that the East German *Geburtsjahr* created a social anchor point that prevailed after reunification despite the increasing convergence of social norms. However, in West Germany it was common - before the 2007 parental leave reform – for children to be cared for at home until they were old enough for *Kindergarten* at three years of age. Even though this was the leading role model and the expectation of the majority, 25-30 percent of mothers with young children did not interrupt employment significantly (see John and Stutzer, 2002). This was particularly true for mothers with higher income and education (see Weber, 2004). Since 2007, however, *Elterngeld* provides strong financial incentives to interrupt market work for the 12 months following childbirth, particularly for this group.

3.2.1 Employment effects of the parental leave benefit reform

A graph of the hazard rates of mothers whose children were born before and after the reform, displayed in Figure 3-3, reveals that there are peaks after the mandatory maternity leave period of 2 months and then again at both 12 and 24 months. It can also be seen that with the introduction of the *Elterngeld*, mothers became much more likely to exit parental leave after 12 months and less likely to return to work during the first 10 months. This is especially true for medium- and high-income mothers (see Figure 3-3b).

Figure 3-3: Hazard rates of returning to work, by length of parental leave spell in full months, before and after the parental leave benefit reform



(a) Low income **(b)** High and medium income

Notes: The figures are based on all mothers who gave birth between 2000 and 2007, who returned to work within 36 months. The income threshold corresponds to a gross labor income of about €1800 per month.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Based on different methods and data-sets, several studies analyze the causal relationship between the parental leave benefit reform of 2007 and the development of maternal employment over time. As expected, Kluve and Tamm (2013), Kluve and Schmitz (2017) and Geyer, Haan, and Wrohlich (2015) find that the probability that mothers return to work during the 12 months following childbirth has declined, in particular for high-income mothers, as a result of *Elterngeld* being introduced. Furthermore, Geyer et al. (2015), Bergemann and Riphahn (2015), and Kluve and Schmitz (2017) find that the employment of mothers who gave birth after the reform was implemented generally increases after the first 12 months compared to employment of mothers who gave birth before the reform.

Kluve and Schmitz (2017) analyze not only the effect of the *Elterngeld* introduction on the labor supply of mothers during the first and second year following childbirth but also on the third to fifth year after childbirth. Based on data from the German Microcensus, they find a large and significant increase in the employment rate of mothers with three to five year old children. However, the authors can only speculate about the mechanism that explains this causal (in a statistical sense) effect of the *Elterngeld*. As a possible explanation, the authors suggest that the new parental leave benefit changed social norms in the sense that it created a new social anchor point in time at which mothers go back to work. Bergemann and Riphahn (2015) also analyze the short- and medium-term maternal employment effects of the 2007 parental leave reform. In line with Kluve and Schmitz, they find that employment of young mothers increases and the average duration of the employment interruptions declines. The authors argue that a change in social norms might partly explain the strong employment effect of the reform; in particular they show that mothers who may be more likely to be restricted by social norms, such as mothers living in the countryside, living in West Germany, or those with an external locus of control show a stronger response to the reform.

To sum up, the empirical findings concerning the 2007 parental leave reform in Germany find that the reform induced mothers to stay at home for the first year after childbirth, but increased the likelihood of maternal employment thereafter. Moreover, empirical findings show that the employment of mothers has changed more than can be explained by financial incentives alone. Some authors speculate that the reform changed social norms concerning the labor supply of mothers with young children. However, a formal test of this hypothesis is, so far, missing. In the next section, we outline how we use the introduction of *Elterngeld* as an instrument for the identification of peer effects with respect to the labor supply decisions of mothers with young children.

3.3 Methodological approach

3.3.1 Identifying peer effects

The identification of social interaction is challenging because there are several explanations as to why members of a social group behave similarly or exhibit common characteristics. Manski (1993) distinguishes three types of effects that can explain why researchers observe similar outcomes of individuals belonging to the same group. The first is the endogenous effect or peer effect, which we aim to identify. Endogenous effects measure the influence of the decisions of the relevant peer group on individual decisions. The second explanation is concerned with contextual effects, meaning that the individual is influenced by exogenous group characteristics. The third explanation originates from correlated effects, which means that individuals belonging to the same group tend to behave similarly because they share unobserved characteristics. Correlated effects can be distilled into two challenges to the identification of peer effects: endogenous group formation and correlated unobservable characteristics due to common shocks. The specific challenges associated with the identification of peer effects in the context of labor supply of mothers with young children can be summarized as follows:

- Peer effects are difficult to identify in the case of **correlated effects**, which are confounded by unobserved variables that are correlated among women who belong to the same social group. Often it cannot be excluded that contextual factors, such as workplace conditions, affect the decisions of employees. Imagine, for instance, a manager who openly supports women who want to take parental leave. This would yield longer average duration of leave spells within a group, which could be incorrectly interpreted as a peer effect.

- The **endogeneity of social networks**, due to sorting into an occupation or firm based on unobservable preferences and firm characteristics, poses another challenge for identification. For example, if women with strong preferences for leisure sort into specific firms and occupations that signal family-friendliness and are more likely to take long parental leaves, then peer effects are likely to be overestimated.

- Another challenge associated with the identification of social interaction effects stems from the **simultaneity** of interactions within a social group. Thus, it is not possible to determine whether an action is the cause of, or the result of, peer influence.

Several papers focus on the theoretical identification of interaction effects in social groups and networks. For example, Blume et al. (2010) address the problems of reflection, self-selection into social groups, and correlated unobservable group characteristics, in the context of the identification of linear, spatial and discrete choice models with social interaction. Furthermore, Brock and Durlauf (2001), Bramoullé, Djebbari, and Fortin (2009), and Blume, Brock, Durlauf, and Jayaraman (2013) formulate conditions under which economic models with social interactions are identified. Most importantly, the researcher must know the structure of the social network and individual data on the behavior of the members of the social network must be available. In most cases, the natural exclusion restriction induced by the structure of a social network enables the identification of the model. However, data with a known network structure are rarely available.

Thus, several studies assume that social interaction with respect to labor supply takes place within observed groups, including geographic neighborhoods (e.g. Weinberg et al., 2004; Maurin and Moschion, 2009) and family networks (e.g. Del Boca et al., 2000; Dahl et al., 2014; Neumark and Postlewaite, 1998; Nicoletti et al., 2016). We focus on the workplace as the relevant social network. This is based on the assumption that workplace peers matter for decisions regarding employment behavior. There are several studies suggesting that peer effects at the workplace play an important role. Dahl et al. (2014) argue that there are peer effects in program participation among coworkers; Hesselius et al. (2009) shows that peer-effects also exist in the context of absenteeism; Mas and Moretti (2009) focus on workplace peer effects in the context of productivity of cashiers for a large grocery chain, while Cornelissen et al. (2017) use linked employer-employee data to estimate the effect of the long-term quality of a worker's peers (measured by the average wage fixed effect of coworkers in the same firm and occupation) on worker's wage.

Given the identification challenges, empirical studies employ sophisticated strategies to identify peer effects. The use of natural experimental approaches is an increasingly popular way to identify peer effects. For example, Brown (2013) analyzes the retirement decisions of teachers using a reform that affected the retirement age of Los Angeles Unified School District (LAUSD) school teachers. She is able to identify peer effects among teachers of the same schools using random variation in the age composition between LAUSD schools.

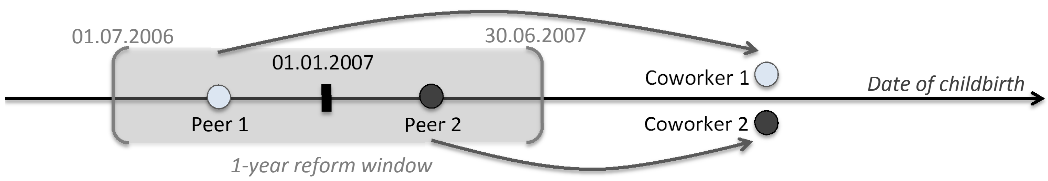
We use a quasi-experimental research design similar to Dahl et al. (2014), who estimate peer effects among brothers and coworkers in the context of paternity leave take-up in Norway. The problems of correlated effects, reflection, and endogenous group membership are avoided by using a quasi-natural experiment exploiting variations in the costs of paternity leave induced by a family policy reform. They find that coworkers and brothers are substantially more likely to take paternity leave if their peer was induced to take up leave by the reform. An analysis of the channels of social interaction suggests that information transmission regarding costs and benefits is most likely driving the peer effects.

3.3.2 Empirical strategy

To overcome the identification challenges mentioned above, we employ an instrumental variable research design that exploits the quasi-random variation in maternal leave spells due to the introduction of the new *Elterngeld* in January 2007. The parental leave benefit reform encourages, in particular, high-income mothers to stay at home during the first 12 months following childbirth and to benefit from the high income replacement rates under the new *Elterngeld*. This creates a change in the fractions of working mothers in the first year after childbirth. In this analysis, we focus on the reform-effect on the behavior of mothers during the first 12 months following childbirth.

Our preferred sample consists of mothers who gave birth on or after July 1, 2007, but on or before December 31, 2009. Those mothers, referred to as *coworkers*, must have at least one *peer* who gave birth sometime between July 1, 2006, and June 30, 2007. The sample is then divided into two groups, the control group consisting of those coworkers whose peers gave birth prior to the reform's implementation and the treatment group consisting of those whose peers gave birth after its implementation. The parental leave benefit reform is exploited using an instrumental variable research design. Thus, the treatment and comparison groups differ only in whether their peers gave birth before or after the reform. This is illustrated (for a simplified world with two groups) in Figure 3-4. In both groups we observe only one birth within the reform window. *Peer 1* in group 1 gives birth before the reform cutoff-date, and *Peer 2* in group 2 gives birth after the cutoff. Consequently, *Coworker 1* and *Coworker 2*, who both give birth after the reform was introduced, vary only in their exposure to peers who gave birth at different points in time.

Figure 3-4: Sampling and identification



One challenge is that an individual may be affected by several peers. Thus, it is necessary to not just specify a window around the cutoff date, but also to the treatment assignment variable in cases where the coworker-mother had more than one peer giving birth in the window around the cutoff. For large peer groups, our research design has little power because with an increasing time-window around the reform date, the variation in exposure decreases. One specification that sidesteps these issues is to consider peer groups where there are one or more peer mothers in the reform window, but only if they gave birth on the same side of the reform. We present estimation results from several alternative sample specifications in Section 3.5.2.

The estimation of the peer effect is made using a two-stage least squares estimator (2SLS), where the reform is employed as an instrument to estimate the effect of peer mothers on their coworkers[7]. The problem can be described as a system of two simultaneous equations, where the dependent variables of the first and second equation are the average maternal leave decisions ($\bar{y}_{Pg}$) of all peer mothers in group g, and their coworkers' individual outcomes (y_{Cg}) respectively. Our outcome of interest is a binary variable that equals one if the mother does not return to work within the first 10 months following childbirth.[8] In the first stage, average peer outcome $\bar{y}_{Pg}$ in peer group g is regressed on the binary instrumental variable z_g, which is equal to one if peer mothers' children were born after the reform.

$$\text{First stage: } \bar{y}_{Pg} = \alpha_0 + W_g'\alpha_1 + \lambda z_g + \epsilon_{Pg} \tag{3.1}$$

Since the policy reform z_g is exogenous, the reform effect on peer leave behavior λ can be identified using a linear regression of average peer maternal leave decisions $\bar{y}_{Pg}$, on the treatment variable z_g.[9] To balance observable differences between treated and non-treated mothers, we include individual, peer and firm characteristics W_g in both equations in some specifications. The results from the first stage

[7] Note that in contrast to Dahl et al. (2014) we do not employ a regression discontinuity research design due to data limitations.

[8] We use 10 months instead of one year because the length of the leave spell is not observed exactly.

[9] As a specification test, we include a function of the date of birth of the peer mothers' child, equal to $f(x_{Pg}) = x_{Pg}$, in the first stage regression to account for a (linear) time-trend in outcomes. However, while including peer mother date of childbirth does not change the point estimates, it does decrease efficiency. Therefore, we do not include $f(x_{Pg})$ in our baseline specification.

are then used to estimate the peer effect $\hat{\delta}$ in the second stage (Equation 3.2). In the second stage regression, we estimate the probability that a coworker mother stays at home at least 10 months following childbirth (y_{Cig}), including the first stage fitted values $\hat{\hat{y}}_{Pg}$.

$$\text{Second stage: } y_{Cig} = \beta_0 + W_g'\beta_1 + \delta\hat{\hat{y}}_{Pg} + \epsilon_{Cig} \tag{3.2}$$

The treatment effect, δ, is the local average treatment effect (LATE) of the reform induced employment interruption decision of the peer mothers on their coworker's maternal leave decisions. It is important to note that the LATE is not equal to the average treatment effect on the treated (ATT), which is equal to a weighted average of the effects on the subpopulations of *always-takers* and *compliers*, while the LATE measures the effect on the subpopulation of *compliers*. As common in the literature, we can only identify the LATE, not the ATT. In this context, *compliers* are those mothers who have peers who stayed at home throughout the first 10 months after childbirth in response to the parental leave benefit reform, and would not have done so in absence of the reform. Consequently the LATE is equal to $\delta_{LATE} = E[y^{(1)}_{Cig} - y^{(0)}_{Cig} | y^{(1)}_{Pg} > y^{(0)}_{Pg}]$, where $y^{(1)}$ and $y^{(0)}$ are the potential outcomes for treated and untreated individuals, or groups, respectively.

In the case of a single binary instrument, the 2SLS estimator is equivalent to a Wald estimator, which is equal to the reduced form estimate of the effect of the policy change on coworkers leave spells divided by the jump in peer outcomes due to the policy change:

$$\delta_{LATE} = \frac{E[y_{Cig}|z_g = 1] - E[y_{Cig}|z_g = 0]}{E[y_{Pg}|z_g = 1] - E[y_{Pg}|z_g = 0]} \tag{3.3}$$

The reduced form effect $E[y_{Cig}|z_g = 1] - E[y_{Cig}|z_g = 0]$ can be interpreted as the intention to treat effect (ITT) of having a peer mother who gave birth after the reform on the coworkers' probability to stay at home for at least 10 months following childbirth.

Our empirical strategy yields consistent estimates of the LATE if several identifying assumptions are met. The first assumption is *independence* of the instrumental variable and potential outcomes. Peer mothers have to be as good as randomly assigned to the treatment group, receiving the new parental leave benefit, and the comparison group exposed to the previous regulation within a window around the reform.[10] For this to be true, we need to assume that individuals are not able to control the treatment assignment variable. Assuming that the timing of birth can only be influenced within a small time window, we can avoid cases where the date of birth is purposefully delayed by dropping observations very close to the first of January 2007. While mothers may have influenced the date of childbirth around the cutoff, it is very unlikely that mothers pre- or postponed childbirth for more than two weeks (see Tamm, 2013). To be certain, we drop all birth events that occur two weeks before and after the cutoff-date from our sample. Mothers who give birth before and after the reform may still differ due to selective fertility in anticipation of the reform. For example, high-income mothers might have delayed pregnancy or even decided to have a child due to the new generous parental leave benefits. We argue that before September 2006 there was no definitive knowledge that the policy would be implemented. The legislative process that led to the reform proceeded rapidly. The government coalition agreed on the reform only

[10] The 2007 parental leave benefit reform is used as a natural experiment in several evaluations of the policy (Kluve and Tamm, 2013; Kluve and Schmitz, 2017; Bergemann and Riphahn, 2011; Wrohlich et al., 2012; Bergemann and Riphahn, 2015).

in May 2006, and the law passed parliament in September 2006. Consequently, most children born during the six months before and after January 1, 2007, were conceived before their parents knew that the reform would be in place by the time of birth (Kluve and Tamm, 2013). Even if the reform encouraged some women to get pregnant after September 2006, their babies were unlikely to be born before July 1, 2007. In line with this argument, Raute (2014) finds evidence for a positive fertility effect of the 2007 parental leave benefit reform starting nine months after the law was passed.

Mothers who give birth in the second half of 2006 can also differ from mothers who give birth in 2007 for reasons unrelated to the parental leave benefit reform. The seasonality of births and the existence of contemporaneous family policy reforms may result in differences in the characteristics of mothers. In particular, several child care reforms carried out between 2005 and 2007 successively increased the availability of subsidized child care for children younger than three,[11] which had sizeable employment effects on mothers (see Geyer et al., 2015). We show in Table 3.6 of the Appendix that most observable characteristics of peer mothers in the treatment and the comparison group do not differ significantly. Given that peer mothers were quasi-randomly assigned to the treatment group, it can be assumed that their coworkers do not differ except in their exposure to peers who gave birth on different sides of the reform cutoff-date. To strengthen the argument, a comparison of treated and non-treated coworkers can be found in Table 3.7 (Appendix).

[11] For an overview of the child care reforms see Spieß (2011).

While most covariates are balanced across treatment and comparison groups of both peer mothers and their coworkers, a closer look at Table 3.6 reveals that the treatment group has more observations. In other words, the sample of mothers who gave birth from January to June 2007 is larger than the sample of mothers who gave birth between July and December 2006. Our identification strategy is threatened if this is due to selective fertility around the introduction of the reform, or if mothers who give birth in the first half of the year differ from those who give birth in the second half of the year for other reasons. We repeat our sampling procedure for seven hypothetical reform dates on January 1^{st} of the respective years from 2002 to 2009 to see whether the year around the actual reform (2007) differs from previous and later years. The distribution of births per month within the different samples, displayed in Figure 3-6, shows that there were comparatively few births in the second half of 2006. A formal difference-in-difference test of the difference in the number of births around the reform-cutoff compared to all non-treated years yields a significant treatment coefficient. The treatment coefficient can be interpreted as a 5 percentage point increase in the number of births in the six months following the reform-cutoff. This result suggests that selective fertility may be a problem. However, we argue that the coefficient is small in magnitude and therefore unlikely to change our peer effect estimates considerably. Furthermore, balanced covariates between treated and non-treated mothers suggest that, even if there are more mothers who give birth after the reform, these mothers do not differ substantially from those who give birth before the reform.

Another identifying assumption is the *exclusion restriction*, which requires that the instrument operates through a single known channel, i.e. coworker outcomes are not affected by the parental leave benefit reform through channels other than peer behavior. All coworker outcomes are observed for mothers who give birth after the reform was implemented. Therefore, it can be assumed that whether a peer mother gave birth before or after the reform had no effect on coworkers' behavior other than through peer behavior. Another necessary assumption for the validity of our research design is that the reform effect is *monotone*, i.e. that no mother is more likely to get back to work within 10 months after giving birth as a response to the reform. This is granted because the parental leave benefit reform did not reduce benefits for any mother in our sample, which excludes low-income mothers, during the first year after childbirth; for most mothers, benefits increased. This increase in benefits was particularly high for our sample of medium- and high-income mothers.

Our empirical strategy is able to circumvent the standard identification issues associated with social interaction effects. The problem of simultaneity is solved by the time dimension, which excludes the possibility of peer decisions being influenced by their coworkers who gave birth afterwards, assuming that mothers do not coordinate their leave beforehand. Bias due to correlated effects and endogenous group formation can be avoided because the parental leave reform is orthogonal to unobserved characteristics and therefore treated and non-treated mothers differ only in their exposure to peers who gave birth before and after the parental leave reform respectively. Consequently, we argue that the estimated effect can be attributed solely to the influence of peer mothers' behavior.

3.4 Data

The empirical analysis is based the longitudinal model 1993-2010 (LIAB LM 9310) of the Linked Employer-Employee Data of the Institute for Employment Research (IAB). The data were accessed via a guest stay at the Research Data Center (FDZ) of the German Federal Employment Agency (BA) at the IAB and subsequently by means of controlled remote data processing at the FDZ.[12] The LIAB includes individual employment histories generated from administrative data provided by firms and social security data from 1993 to 2010. Individual employment histories are merged with annual IAB establishment panel data, which includes detailed firm characteristics such as developments in employment (production, turnover, working hours, investment, capacity utilization), and demand for personnel and labor expectations (vacancies, open positions, fluctuations, establishment employment policies). In addition, the LIAB includes information on firms' technology, organization structure, determinants of productivity, firm size and industry. We merged the LIAB with regional information on childcare coverage, population density, and unemployment rates from INKAR.[13]

When estimating peer effects, the challenge is to find a data set that contains micro data on an individual's social network. The researcher has to know (or assume to know) the relevant reference group. One of the main advantages of the LIAB is that it includes a large number of firms and individuals (in 2007, we observe 5,364 firms), and that the full network of employees working at the same

[12]See Klosterhuber et al. (2013) for more information on the LIAB longitudinal model 1993-2010 (LIAB LM 9310) data documentation and methodology.

[13]Bundesinstitut für Bau-, Stadt- und Raumforschung (BBSR) im Bundesamt für Bauwesen und Raumordnung (BBR), Bonn (2015).

firm can be identified. Therefore, this data set has already been used by several studies to analyze peer effects in different contexts. For example, Cornelissen et al. (2017) use the LIAB to analyze peer effects on wages, and Pink et al. (2014) study workplace peer effects on fertility.

A disadvantage of the LIAB data, however, is that parental leave spells and events of childbirth are not directly observed. We only observe gaps in employment histories, which could also be due to periods of military service, illness, disability, or early retirement. However, Schönberg (2009) shows that it is possible to identify maternal leave spells and events of childbirth with sufficient accuracy by using the starting point and duration of employment interruptions.[14] Employment interruptions of at least 14 weeks are likely to be maternity leave spells due to the obligatory maternity leave period of 6 weeks before and 8 weeks after childbirth. The likelihood that an employment interruption is due to childbirth is increased by restricting the sample to women between the age of 18 and 40. Following Schönberg and Ludsteck (2014); Dustmann and Schönberg (2011), we approximate the child's date of birth as six weeks after the mother went on leave, which leads to some measurement error in the child's month of birth.[15]

As previously noted, and given our identification strategy, the sample is restricted to those female coworkers who gave birth after July 1, 2007, and before December 31, 2009, conditional on these mothers having a peer who gave birth sometime between July 1, 2006 and June 30, 2007. The peer had to belong

[14] Schönberg (2009) shows that after some appropriate restrictions are imposed, at least 90 percent of leave spells in the data are due to maternity leave in West Germany. The child's birth month is correctly estimated for at least 70 percent, and over- or underestimated by one month for about 25 percent of mothers.

[15] We specifically thank Dana Müller and Katharina Strauch for their Stata dofiles and assistance.

to the same peer group as determined by occupation and firm identifier. For small firms with 99 or fewer employees, peer groups are formed only by the firm identifier. Occupations are defined using two-digit occupational groups according to the German classification of occupations KldB 1988 (Bundesanstalt für Arbeit, Nürnberg, 1988), which summarizes related occupations into 86 larger groups. Firm identifiers differ by establishment.

The sample selection proceeds as follows. First, employed mothers who give birth in a one-year window around the parental leave benefit reform (January 1, 2007) are marked as potential *peer* mothers. We observe 4,375 births in the reform-window. In the next step, we group all women who work in the same firm and occupation and then count the number of births before and after January 01, 2007 within the reform-window. In our baseline specification, we keep only those peer groups where there are either births before or after the reform, dropping all groups where there are births on neither or both sides of the cutoff-date.[16] We lose another 2,845 birth events because there is at least one birth before and one after the reform within the specified window. The number of potential peer mothers is further reduced to 1,413 by dropping all groups where a birth event falls within the 28-day window around the reform date because of potential measurement error in birth dates and concerns about manipulation of the date of childbirth. Thereafter, we identify all *coworkers* of potential *peer* mothers, who work in the same firm and occupation, and mark those who give birth after their peers and after the reform-window. One limitation is the right-censoring of the observed maternal leave spells. When looking at the fraction of mothers returning within the first 12

[16]Most groups (about 83 percent) are lost because no woman gives birth within the reform-window.

months following childbirth, we have to drop all observations of birth events that occur after December 31, 2009 because our observation periods ends on December 31, 2010. Furthermore, we include only the first observed birth if the same coworker gives birth more than once. In the next step, we generate the treatment assignment variable (peer mothers' date of childbirth) by taking the latest birth within a group and window, and define peer outcomes and characteristics by taking the mean if there is more than one peer mother.

Because the parental leave benefit reform had heterogeneous effects on different income groups, we divide the sample of *coworkers* by *peer* income.[17] We drop peer groups with a low average income to exclude the possibility that peer mothers' employment decreased in the first year as a response to the reform. After limiting our sample to peer mothers in the upper two-thirds of the income distribution (monthly income[18] $\geq$ €1,814), and their coworkers, we are left with a sample of 488 peer mothers and 1,340 coworkers.[19]

From the 12,069 birth events observed between July 2007 and December 2009, we are left with a sample of 1,340 coworkers, which amounts to 11 percent of all observed births over that time period (see Table 3.8 for an overview of the sample sizes of the baseline specification and several alternative sample and treatment assignment variable specifications). The resulting sample is not representative of all women who gave birth between July 2007 and December 2009. Table 3.9 com-

[17] Note that the sample is divided by peer, not coworker, income because peer mothers are heterogeneously affected by the parental leave benefit reform. There is no restriction on coworker income, however, the coworker income distribution is very similar to the peer income distribution due to the definition of peer groups.

[18] Monthly gross labor income is computed as an average over the last 12 months before beginning of the maternity leave period.

[19] It is not possible to use the exact income threshold because parental leave benefit eligibility was means-tested on the household level before the reform and we only have individual earnings.

pares the characteristics of women in our baseline sample to all women in the LIAB who gave birth in the same time period. Due to the sample selection by income, it is not surprising that women in our sample have a higher average income and are more likely to be highly educated (tertiary degree). Furthermore, women in our sample have more overall work experience and are less likely to be unemployed or part-time employed. Descriptive statistics of all other sample specifications can be found in Table 3.10 in the Appendix.

3.5 Results

3.5.1 Baseline results

Women who work in the same firm and occupational group may have similar unobserved characteristics and preferences regarding employment and family-life. Consequently, parental leave behavior may be correlated within a group of coworkers regardless of peer effects. In order to single out the peer effect, we employ a two-staged least squares (2SLS) regression using whether or not peer mothers gave birth after the parental leave benefit reform as the instrumental variable for the endogenous peer behavior.

The first stage regression estimates the reform effect on the probability that a *peer* mother stays at home for at least 10 months after giving birth. A comparison of pre- and post-reform means reveals that the outcome variable increased from 56.3 percent to 76.0 percent for the group of medium- and high-income peer mothers in our sample (displayed at the bottom of Table 3.1). Consistent with the descriptive evidence and findings of previous literature, we find that the reform significantly increased the likelihood of high-income mothers to stay at home during the first year. Our estimates of a 21.5 percentage point increase (see column 1 in Table 3.1) in the probability to stay home in the first 10 months exceed previous results, which is due to the specific sample used in this analysis (i.e. mothers who were employed before giving birth and who have a relatively high income). Including various control variables lowers the point estimate of the reform effect only marginally. Significant first stage coefficients and an F-statistic above 10 alleviate concerns about a weak instrumental variable.

We estimate the reduced form effect of having a peer mother who gave birth after the reform on the coworkers' probability to stay at home for at least 10 months after childbirth. This can be interpreted as intention to treat (ITT) effect. We find that the ITT is positive and significant, amounting to 6.8 percentage points without covariates and 5.3 percentage points if we include covariates (see Table 3.1), suggesting that having a peer who gave birth before or after the reform has an important impact on coworkers' parental leave behavior.

In the simple case of a single binary instrument, the peer effect can be computed by dividing the reduced form (ITT) by the first stage estimate. As shown in Table 3.1, we find a significant peer effect of about 28.2 to 31.5 percentage points, i.e. a mother is about 30 percentage points more likely to stay at home during the first 10 months if her peer mothers decide to do so as a response to the parental leave benefit reform. Note that the point estimate has to be interpreted with caution because small imprecisions in reduced form or first stage point estimates can lead to large changes in the estimated peer effects. Including linear trends in the date of childbirth and several control variables does not change the results substantially. Note that in cases where a mother has several peers who gave birth in the one-year window around the reform, the treatment variable can lie between zero and one because it is defined as the average peer outcome. The peer effect is estimated as the effect of a change from zero to one, i.e. the effect of all peer mothers deciding to stay at home in the first year compared to none.

The large magnitude of the estimated peer effect could be due to the specific subgroup for which the effect is estimated. The effects have to be interpreted as local average treatment effects (LATE) of reform-induced changes in peer mothers' parental leave behavior on the leave taking behavior of coworkers. In other words, we cannot identify the average treatment effect on the treated (ATT) because we estimate the effect only for a subpopulation of those coworkers whose peers stay at home during the first 10 months after childbirth if their child is born after the reform, and would have returned to work within 10 months if their child were born before the reform. The LATE can be expected to differ from the ATT because peer effects are likely to be heterogeneous across firms and occupations. For example, we expect peer effects to be larger in the subgroup of compliers, which contains groups where peer mothers respond to the reform, and would have returned early in absence of the *Elterngeld*. We know from the first stage estimation that the subgroup of compliers amounts to about one-fifth of the total sample. However, we cannot observe the compliers directly because we do not observe counterfactual outcomes. One way to learn more about the group of compliers is to use the variation in the first stage across covariate groups (see Angrist and Pischke, 2009). Dividing the subgroup estimate by the baseline first-stage result yields the relative likelihood that a complier belongs to a certain

Table 3.1: First stage, reduced form (ITT) and peer effect (LATE) 2SLS-estimation results

y_C	First stage		Reduced form		Peer effect	
z_g	0.215***	0.188***	0.068***	0.053**		
	(0.060)	(0.058)	(0.024)	(0.025)		
$\bar{y}_P$					0.315***	0.282*
					(0.116)	(0.148)
x_C (Month of childbirth)		0.000		-0.001		-0.001
		(0.001)		(0.001)		(0.001)
Age at childbirth		-0.003		-0.004		-0.003*
		(0.003)		(0.003)		(0.003)
Peer age		-0.011		-0.002		0.001
		(0.008)		(0.003)		(0.005)
Prior earnings		0.000		0.000		0.000*
		(0.000)		(0.000)		(0.000)
Peer earnings		0.000		0.000		0.000*
		(0.000)		(0.000)		(0.000)
High education		-0.038		-0.004		0.006
		(0.045)		(0.036)		(0.036)
Low education		-0.084		0.010*		0.124**
		(0.063)		(0.052)		(0.058)
Peer high education		0.025		0.005		-0.002
		(0.071)		(0.035)		(0.035)
Peer low education		0.265***		0.006		-0.068
		(0.102)		(0.125)		(0.125)
Firm size		0.000**		0.000*		0.000
		(0.000)		(0.000)		(0.000)
Number of peer births		-0.050**		-0.014		0.000
		(0.020)		(0.010)		(0.011)
Peer group size		0.001***		0.000*		0.000
		(0.000)		(0.000)		(0.000)
West Germany		0.074		0.007		-0.014
		(0.061)		(0.029)		(0.034)
Constant	0.533***	1.016***	0.748***	0.931***	0.580***	0.645***
	(0.060)	(0.258)	(0.019)	(0.142)	(0.079)	(0.250)
*** p<0.01, ** p<0.05, * p<0.1						
N	1340	1336	1340	1336	1340	1336
R^2	0.067	0.117	0.006	0.016		
Robust F(1,310)	13.003***	10.609***				
Pre-mean ($z_P = 0$)	0.563		0.748			
Post-mean ($z_P = 1$)	0.760		0.816			

Notes: The dependent variable y_C is defined as an indicator equal to one if the individual does not return to work within 10 months after childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on firm level. Corvariates are measured at the time of coworker or peer date of childbirth respectively.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

group. A first stage analysis across covariate groups, displayed in Table 3.11 in the Appendix, reveals that compliers are more likely to have university education, be in the upper third of the income distribution and work in large and old firms. This is in line with our intuition that compliers have a greater labor market attachment.

3.5.2 Robustness and specification tests

A crucial underlying assumption of our results is the comparability of treated and non-treated individuals. Given our assumption that whether a mother is exposed to peers who gave birth before or after the reform is purely random, treated and non-treated mothers should have the same distribution of covariates. A simple mean comparison of treated and non-treated coworkers (Table 3.7) points to significant differences in only one variable, namely the number of days employed in the firm. To correct for imbalances, thereby improving the precision of our estimates, we include a number of additional covariates that potentially affect maternal leave decisions in Table 3.2. Including different sets of covariates does not change the point estimates considerably, nor does it lead to large gains in precision of the estimates.

Another set of robustness checks concerns the definition of the sample. While the sample used for the baseline specification shown in Table 3.1 consists of groups in which we observe peer mothers giving birth to a child either before or after the reform, Table 3.3 presents the results from four alternative definitions of the estimation sample.

Table 3.2: Inclusion of additional individual, firm and regional characteristics

	First stage	Reduced form	Peer effect	N	Controls
Baseline	0.215***	0.068***	0.315***	1340	No
	(0.060)	(0.024)	(0.116)		
Additional peer chars (1)	0.188***	0.049*	0.259*	1336	Yes
	(0.056)	(0.026)	(0.151)		
Additional coworker chars (1)	0.191***	0.052**	0.272*	1336	Yes
	(0.057)	(0.024)	(0.142)		
Firm level covariates (2)	0.222***	0.085**	0.385**	779	Yes
	(0.063)	(0.036)	(0.192)		
Regional covariates (3)	0.164***	0.062**	0.378**	1295	Yes
	(0.056)	(0.025)	(0.189)		
Occupational Fixed Effects	0.176***	0.057**	0.322*	1336	Yes
	(0.059)	(0.025)	(0.165)		
Industry Fixed Effects	0.209***	0.062*	0.297*	968	Yes
	(0.063)	(0.033)	(0.175)		
Interaction of treatment	0.028***	0.010***	0.371***	1340	No
and number of peer births	(0.010)	(0.004)	(0.096)		
	0.115***	0.037**	0.327**	1336	Yes
	(0.029)	(0.014)	(0.129)		
*** p<0.01, ** p<0.05, * p<0.1					

Notes: The dependent variable y_C is defined as an indicator equal to one if the individual does not return to work within the first 10 months after childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on the firm level. Control variables include month of childbirth, age of the mother, prior-to-birth earnings, as well as the education level of both coworkers and peer mothers. Firm size, number of peer births in reform window, peer group size and a dummy for West Germany. Additional individual characteristics (1) include experience, tenure in firm, days in unemployment, part-time employment prior-to-birth and the number of children. Firm level covariates (2) include a dummy for old firms (> 10 years) and standardized wages, churn rate, median firm income, and the share of female employees, part-time and temporary workers. Regional covariates (3) include district childcare coverage, population density and unemployment rate.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Sample B is specified as the baseline sample, however here we drop all observations with peer births occurring in December 2006 or January 2007 (rather than just a 4-week window around the reform date). Dropping peer groups where there is a birth in a 60-day window around the reform date does not change the results compared to our baseline specification. In our baseline specification, peer groups are defined by firm (establishment) and occupational group. In contrast, we assume that mothers interact with each other across occupational groups in sample specification C, where the whole firm is defined as the relevant peer group. This results in a sample with smaller firms on average, because the sample selection is still conditional on observing births on only one side of the reform cutoff date. We find smaller, insignificant first stage and ITT effects using this specification. In sample D we restrict the sample to coworkers who gave birth to a child at least 9 months after the last peer mother within the group has given birth to a child in order to make sure that enough time has gone by that the coworkers may observe reactions to the peer's decisions. Reassuringly, point estimates for all three parameters of interest are very similar to the baseline specification.

The main threat to our identification strategy is that mothers may have selected into treatment by timing their pregnancy in anticipation of the parental leave benefit reform. In Section 3.3.2, we note that it is possible, although unlikely, that mothers anticipated the reform in mid-2006, and reacted by increased fertility in the first half of 2007. By limiting the sample of peer mothers to those who gave birth in an six-month (sample E) and eight-month window (sample F) around the reform, we minimize the probability of selected fertility. Reassuringly, we find positive reduced form and peer effects using a shorter window, despite the smaller variation in peer behavior, even though the coefficients are only partly significant.

Table 3.3: Results from alternative sample specifications

	First stage	Reduced form	Peer effect	N	Controls
	(0.053)	(0.023)	(0.123)		
B. 30 days donut around	0.223***	0.065***	0.293**	1245	No
cutoff date	(0.063)	(0.024)	(0.113)		
	0.197***	0.049*	0.247*	1241	Yes
	(0.061)	(0.025)	(0.144)		
C. Peer groups = firms	0.103	0.033	0.315	1089	No
	(0.071)	(0.027)	(0.296)		
	0.116*	0.034	0.296	1083	Yes
	(0.067)	(0.027)	(0.270)		
D. Coworker birth at least	0.206***	0.070***	0.340***	1107	No
9 months after peer birth	(0.061)	(0.025)	(0.130)		
	0.169***	0.064**	0.380*	1105	Yes
	(0.060)	(0.027)	(0.195)		
E. Six-month reform-window	0.196***	0.046*	0.236	1482[1]	No
(±3 months)	(0.067)	(0.025)	(0.152)		
	0.256***	0.028	0.111	1480[1]	Yes
	(0.075)	(0.024)	(0.101)		
F. Eight-month reform window	0.175***	0.047*	0.267*	1438[1]	No
(±4 months)	(0.054)	(0.024)	(0.157)		
	0.183***	0.040*	0.222	1434[1]	Yes
	(0.057)	(0.023)	(0.143)		
G. Using all income groups	0.112	0.023	0.202	2158	No
	(0.046)	(0.020)	(0.176)		
	0.086	0.011	0.130	2149	Yes
	(0.044)	(0.020)	(0.236)		
*** p<0.01, ** p<0.05, * p<0.1					

Notes: The dependent variable y_C is defined as an indicator equal to one if the individual does not return to work within 10 months after childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on the firm level. Control variables include month of childbirth, age of the mother, prior-to-birth earnings and education level of both coworkers and peer mothers. Firm size, number of peer births in reform window, peer group size and a dummy for West Germany.
[1]A smaller reform-window can lead to a larger sample of coworkers due to a longer time window for coworker births, and/or through the selection of more peer groups with peer births on only one side of the cutoff.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Finally, we repeat the analysis using a sample without any restrictions on peer income (sample G). In contrast to our baseline specification, we also include those peer groups where the (mean) peer income is in the bottom third of the income distribution. We exclude this group in our baseline specification due to heterogenous reform effects (low income mothers can be induced to return to work earlier due to the parental leave benefit reform) and the implied violation of the monotonicity assumption. The first stage effect disappears when we include all income groups, suggesting that the reform indeed did not induce low income mothers to stay at home for the first year following childbirth. Without a significant first stage, the estimates of reduced form and peer effects are meaningless.

Our results, presented in Table 3.1, suggest that individual parental leave decisions are substantially influenced by the parental leave behavior of coworkers working in the same firm and occupation. If these results are truly peer effects, we expect both first stage and reduced form effects to disappear when we center the reform-window around a date when there was no change in the parental leave benefit regime. To test this hypothesis, we create a *placebo*-sample by re-centering the reform-window around January 1, 2006, using the same sample selection procedure as in our baseline specification. The results are displayed in Table 3.4. A significant first stage would hint at a difference in parental leave behavior of women who give birth in the first vs. the second half of a year (in this case January to June 2006 vs. July to December 2005). A significant reduced form effect would suggest that mothers who have a peer who gave birth between January and June 2006 differ from those who give birth in the second half of 2005. However, we find no first stage, reduced form, and consequently no peer effects using the placebo sample. We find a zero first stage reform effect if we include covariates in the regression. Therefore, the peer effect estimate, which is computed by dividing the reduced form by the first stage, is undefined.

Table 3.4: Results from placebo sample

	First stage	Reduced form	Peer effect	N	Controls
Placebo sample using	-0.022	-0.017	0.805	1180	No
a fake reform date	(0.062)	(0.027)	(2.611)		
	0.000	-0.017	–	1179	Yes
	(0.055)	(0.026)	–		
*** p<0.01, ** p<0.05, * p<0.1					

Notes: The dependent variable y_C is defined as an indicator equal to one if the individual does not return to work within 10 months following childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on firm level. Control variables include month of childbirth, age of the mother, prior-to-birth earnings and education level of both coworkers and peer mothers. Firm size, number of peer births in reform window, peer group size and a dummy for West Germany.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

3.6 Mechanisms

Peer effects can operate through several channels of social interaction. One mechanism through which peer effects could arise is the transmission of information about the parental leave benefit program. Mothers may learn from their peers about the existence of the parental leave benefit program, its eligibility criteria, and the application procedure. However, we argue that this is not likely to be a relevant mechanism of peer effects in our context, because the program is well known and parents are generally informed about the organizational details of parental leave benefits. There is an easy-to-use online tool to compute expected benefits and the application form has to be filled in regardless of the length of the parental leave. An almost 100 percent take-up rate of at least some parental leave indicates that there is no lack of practical information in this context. Despite the general knowledge about parental leave benefits, observing peers at work could reveal information on career related consequences as well as the workplace-specific costs and benefits of parental leave. Women may, for example, acquire information from their female peers that reduces uncertainty about post-birth career opportunities, wage-trajectories, and the possibility of combining family and work. In a standard social learning model, we would expect that women with more diffuse priors about the costs and benefits of parental leave should be more strongly influenced by their peers (see Goyal, 2011, for an overview of models of social learning in networks). Consequently, we expect stronger peer effects in situations with greater career-related uncertainty, if the revelation of information is a driving mechanism of peer effects in our context. For example, we expect stronger peer effects for women who have their first child and for mothers who have short tenure in the firm. Furthermore, we expect stronger peer effects in younger firms, firms with high turnover and job uncertainty, where workplace specific information is more valuable.

In order to shed light on the mechanisms of peer effects, we split the sample into groups of coworkers with up to two, three, and four years of work experience at the present firm, and those with longer tenure (more than four years at the same firm). *Ceteris paribus*, we expect mothers with shorter tenure to face more uncertainty concerning the firm's reaction to their leave decision than mothers with longer tenure. As Table 3.5 shows, we find a larger peer effect of 73.5 percentage points for mothers with up to two years of tenure. The effect fades away as tenure increases. For mothers with more than four years of tenure, the point estimate falls to 16.4 and becomes statistically insignificant. We interpret

Table 3.5: Heterogenous effects - results for different subgroups

	First stage	Reduced form	Peer effect	N
Baseline	0.215***	0.068***	0.315***	1340
	(0.060)	(0.024)	(0.116)	
Tenure $\leq$ 2 years	0.241***	0.177***	0.735**	374
	(0.086)	(0.049)	(0.289)	
Tenure $\leq$ 3 years	0.222**	0.119***	0.535**	542
	(0.086)	(0.042)	(0.241)	
Tenure $\leq$ 4 years	0.285**	0.070*	0.376	646
	(0.081)	(0.040)	(0.244)	
Tenure > 4 years	0.221***	0.036	0.164	690
	(0.062)	(0.031)	(0.140)	
Including only first births	0.166***	0.060**	0.364	1028
	(0.059)	(0.028)	(0.199)	
High layoff rate	0.146**	0.033	0.228	663
	(0.068)	(0.042)	(0.291)	
Low layoff rate	0.249***	0.043	0.171	559
	(0.071)	(0.035)	(0.149)	
East Germany	0.169*	0.019	0.110	463
	(0.099)	(0.043)	(0.258)	
West Germany	0.201***	0.074**	0.369**	873
	(0.066)	(0.030)	(0.167)	

*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable y_C is defined as an indicator equal to one if the individual does not return to work within 10 months after childbirth. First stage and reduced form regressions include the same control variables as the corresponding 2SLS regression. Standard errors (in parentheses) are clustered on firm level. Control variables include month of childbirth, age of the mother, prior-to-birth earnings and education level of both coworkers and peer mothers. Firm size, number of peer births in reform window, peer group size and a dummy for West Germany.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

these results as suggestive evidence for the hypothesis that peer effects are at least partly driven by the reduction of career-related uncertainty for mothers who wish to take longer leaves (longer than 10 months), because they can observe peers who took longer leaves in response to the parental leave benefit reform, as well as their employers' reactions to it. Similarly, we expect mothers to face higher uncertainty regarding the consequences of an extended parental leave if they gave birth to their first child, compared to higher order births. Most birth events observed in our sample are first births, therefore we cannot compare the different effects by number of children. An analysis including only first births yields a slightly higher point estimate of the peer effect. However, the difference is not statistically significant from the baseline estimate. Career-related uncertainty is also expected to be high in firms where the layoff rate is high. We split the sample into firms with high and low layoff rates and find that point estimates for peer effects are only insignificantly larger in the former group. Further sample divisions, e.g. by age or education, are impeded by the small number of observations per group.

Competing explanations to the transmission of career-related information include imitation and herding behavior (Akerlof and Kranton, 2000; Banerjee, 1992). We summarize these explanations as preferences for conformity to norms within social reference groups. This can include peer pressure. Social norms regarding parental leave may differ by socio-economic status, employer, occupational group and region. For example, norms regarding parental leave differ greatly between East and West Germany. Even though the overall duration of parental leave before the reform was longer on average in the West, it was also more common among high-income mothers to return within the first 10 months in West Germany compared to East Germany, where a one-year leave was a long standing tradition (see Figure 3-5 in the Appendix). The parental leave benefit reform changed societal norms toward a parental leave duration of one year. Consequently, this amounts to a larger change in social norms in West Germany than in the former East. Separate estimations for East and West Germany show that all three effects (first stage, reduced form and LATE) are larger in West than East Germany. Reduced form and peer effect estimates for East German mothers are not statistically significant. This could also be due to the relatively small number of observations in the sample of East German mothers. However, smaller effects in East Germany are consistent with a larger change in social norms in West Germany. A large fraction of East German mothers stayed at home for at least 10 months before the reform. As a result, the reform effect on our outcome variable is not as strong in the East as in the West.

Where the parental leave benefit reform did not change social norms substantially (e.g. in East Germany), peer effects are expected to be smaller. Note that we distinguish between information transmission in settings with career-related uncertainty and settings where social norms are in flux. We are, however, unable to clearly distinguish these two channels of peer effects empirically. Based on stronger peer effects for mothers with short tenure and East German mothers, we argue that information transmission about the costs and benefits of a long parental leave, as well as preferences for conformity to changing social norms may constitute relevant channels of peer effects in settings where the reform had large direct effects. Our results are consistent with a social learning model where the information provided by peers reduces social and career-related uncertainty.

Previous literature suggests that leisure complementarities are another potential source of peer effects in labor supply decisions (see Alesina et al., 2006). A peer mother who is enjoying a long parental leave may induce her coworker to do the same, so that time can be spent together. On the other hand, the opposite could be true and the absence of one mother may reduce the probability that her coworkers will simultaneously take a long leave. In particular, in small firms, the absence of an employee can increase the workload and responsibilities of her coworkers and thereby make a (long) parental leave more costly. However, in our context, peer mothers and their coworkers give birth with a temporal distance and hence do not, generally speaking, take leave at the same time. Consequently, the scope for complementarities is limited because leave spells of peers and their coworkers often do not overlap. As a test of leisure complementarities, we restrict the sample to mothers whose peers gave birth at least nine months earlier. We find similar effects (displayed in Table 3.2) and, hence, conclude that this is unlikely to be an important channel of peer effects in our context.

3.7 Conclusion

The decision of mothers regarding how long to take parental leave in order to take care of her children is influenced not only by financial considerations but also by peer behavior. In this paper, we estimate the quantitative importance of peers' decisions on the parental leave decisions of mothers, in particular on the probability to return to work within the first 10 months after giving birth.

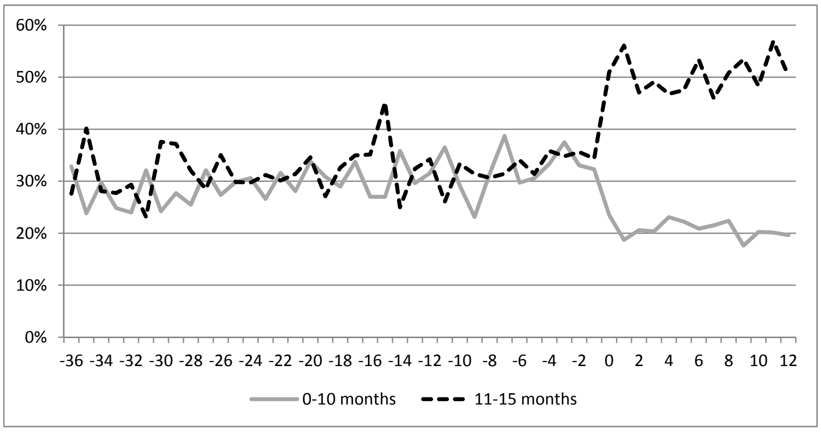
We use exogenous variation in the length of parental leaves of mothers induced by a parental leave benefit reform in Germany in 2007 to identify causal peer effects. The reform strongly increased financial incentives to take a leave of one year, especially for medium- and high-income mothers. Using linked employer-employee data, this methodology allows us to identify the peer effect as the local average treatment effect for the group of mothers with coworkers who decided for a longer leave due to the reform. For this group, we find a statistically significant and large peer effect: if a mother has a peer who opted for a longer leave due to the reform, the probability that she will take parental leave for at least 10 months is about 30 percentage points higher than if her peer returned to work after no more than 10 months. This strong effect shows that the influence of peers is quantitatively important. The results are robust to a large set of different specifications with respect to the definition of the peer group, the definition of the estimation sample, as well as the inclusion of covariates.

Our results suggest that preferences for conformity and the transmission of information about the costs and benefits of a (long) parental leave may be relevant mechanism of peer effects in our context. Information transmission is expected to be particularly important in situations with high career-related uncertainty. We show that for the subgroup of women with short tenure at the same firm, a group that supposedly faces more uncertainty regarding the employer's reaction to leave decisions, the peer effect is larger than for the group with longer tenure. Preferences for conformity to peer behavior are expected to be particularly important in situations with changing social norms. In East Germany, where the parental leave benefit reform did not change social norms substantially, peer effects are expected to be smaller. Separate estimations for East and West Germany show that both direct reform effects and peer effects are larger in West than in East Germany. These results are consistent with a social learning model where information provided by peers reduces social and career-related uncertainty.

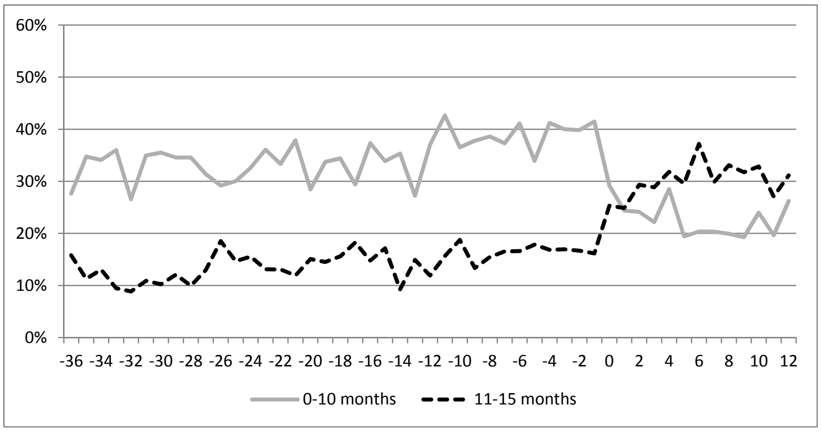
Our results are also interesting from a policy point of view. We show that just the fact that a mother (who gave birth to a child after the reform has been implemented) has a peer who gave birth shortly after the introduction of the new parental leave scheme increases her probability of taking a longer leave by 7 percentage points in contrast to mothers with peers who gave birth to a child shortly before the reform. This effect, which can be interpreted as intention to treat effect, shows that policy reforms have an impact on the individuals' choices that go far beyond the immediate behavioral reaction due to changes in financial incentives.

3.8 Appendix

Figure 3-5: Fraction of mothers who stays at home for 0-10 months and 11-15 months after childbirth by region



(a) East Germany



(b) West Germany

Notes: Fractions are calculated out of all women in the sample who gave birth in a given month, in distance to the parental leave benefit reform (January 2007 = 0).
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Table 3.6: Comparison of peer mothers giving birth before and after the parental leave benefit reform

	Before reform				After reform						
Variable	Median	Mean	SD	N	Median	Mean	SD	N	Difference	DE(Diff)	t
No return within 10 months		0.56	0.50	205		0.75	0.44	283	-0.18	0.04	-4.26
Age at childbirth	33	32.55	4.29	205	32	32.27	4.31	283	0.28	0.39	0.72
Prior earnings	2654	2888.59	929.46	205	2716.45	2929.80	942.36	283	-41.21	85.74	-0.48
Days in employment	2980	3382.79	1632.08	205	3007	3278.12	1547.76	283	104.67	146.49	0.71
Days in unemployment	0	100.53	265.37	205	0	59.96	185.71	283	40.57	21.57	1.88
Part-time employed		0.11	0.31	205		0.16	0.37	283	-0.05	0.03	-1.68
High education ($\geq$ college)		0.20	0.40	205		0.16	0.37	283	0.03	0.04	0.92
Low education ($<$ highschool)		0.04	0.19	205		0.02	0.13	283	0.02	0.02	1.36
Migration background		0.05	0.22	205		0.06	0.23	283	-0.01	0.02	-0.38
Number of children	1	1.15	0.39	205	1	1.22	0.45	283	-0.06	0.04	-1.69
Date of childbirth (Jan 01, 2007 = 0)	-100	-102.12	47.29	205	97	98.63	47.67	283	-200.75	4.35	-46.13
Group size	16	24.12	24.86	165	18	32.70	69.43	211	-8.58	5.16	-1.66
Births in reform window	1	1.22	0.53	165	1	1.33	0.90	213	-0.10	0.07	-1.41
Firm size	329	968.29	1965.43	144	281	748.74	1637.97	189	219.56	202.54	1.08
Old firm (older than years)		0.93	0.26	144		0.92	0.28	189	0.02	0.03	0.52
Standard wages in firm		0.32	0.47	115		0.42	0.49	144	-0.09	0.06	-1.58
Share of women in firm	0.45	0.47	0.26	140	0.54	0.52	0.26	182	-0.05	0.03	-1.68
Share of temporary workers in firm	0.04	0.09	0.15	140	0.04	0.09	0.13	182	0.00	0.02	0.12
District unemployment rate	9.9	11.20	4.72	123	9.2	9.8	4.82	147	1.41	0.58	2.41

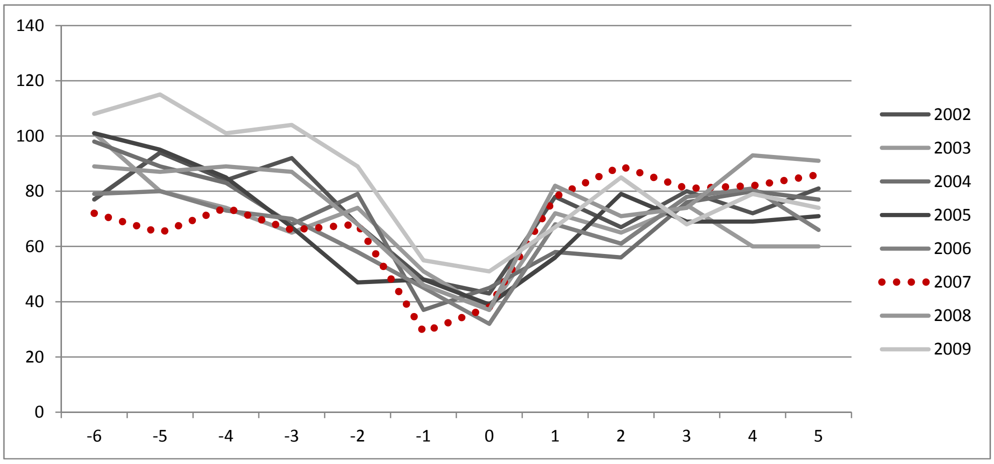
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Table 3.7: Comparison of mothers whose peers gave birth before and after the parental leave benefit reform

	Peer birth(s) before reform				Peer birth(s) after reform						
Variable	Median	Mean	SD	N	Median	Mean	SD	N	Difference	DE(Diff)	t
No return within 10 months		0.75	0.43	472		0.82	0.39	868	-0.07	0.02	-2.83
Age at childbirth	32	32.14	4.31	472	32	32.13	4.23	868	0.01	0.24	0.02
Prior earnings	2686	2865.14	1145.57	472	2729.62	2830.03	1177.36	868	35.11	66.16	0.53
Days in employment	2933	3176.90	1595.28	472	3071.50	3250.32	1537.27	868	-73.42	90.08	-0.82
Days in firm	2146	2525.32	1782.26	472	1158	1906.91	1745.04	868	618.41	101.18	6.11
Days in unemployment	0	98.31	225.45	472	0	84.46	245.03	868	13.85	13.30	1.04
High education ($\geq$ college)		0.24	0.42	472		0.20	0.40	868	0.04	0.02	1.61
Low education ($<$ highschool)		0.03	0.17	472		0.03	0.18	868	0.00	0.01	-0.49
Migration background		0.05	0.22	472		0.06	0.24	868	-0.01	0.01	-1.05
Number of children	1	1.21	0.41	472	1	1.25	0.46	868	-0.04	0.02	-1.64
Date of childbirth (Jan 01, 2007 = 0)	580	606.76	264.47	472	617	628.67	267.06	868	-21.91	15.18	-1.44
Group size	16	24.07	24.87	165	18.5	32.94	69.52	210	-8.87	5.17	-1.71
Number of peer mothers	1	1.24	0.54	165	1	1.33	0.90	213	-0.09	0.07	-1.15
Pre-sample births 07/2004-06/2006	1	2.19	2.22	165	1	1.98	2.44	213	0.21	0.24	0.89
Firm size	328	984.90	2034.47	144	272	770.35	1659.49	189	214.54	208.12	1.03
Old firm (older than years)		0.94	0.24	144		0.92	0.28	189	0.02	0.03	0.77
Standard wages in firm		0.38	0.49	116		0.40	0.49	145	-0.02	0.06	-0.34
Share of women in firm	0.47	0.46	0.25	140	0.51	0.52	0.25	178	-0.05	0.03	-1.84
Share of temporary workers in firm	0.04	0.10	0.15	140	0.05	0.09	0.13	176	0.00	0.02	0.24
District childcare coverage	15	21.59	15.64	158	13.9	20.80	15.45	207	0.79	1.64	0.48
District unemployment rate	8	8.59	4.19	158	7.4	8.21	4.03	207	0.38	0.44	0.88

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Figure 3-6: Distribution of births per month in distance to January of each year



Calculations are based on the baseline sample specification sample with either births before or after January 1^{st} in a one-year window around the (hypothetical) reform-date.

Table 3.8: Sample sizes relative to total number of births (07/2007 - 12/2009)

	Baseline	Sample B	Sample C	Sample D	Sample E
Coworkers	1340	1245	1089	1107	1482
Percent of births	*11.1 percent*	*10.3 percent*	*9.0 percent*	*9.2 percent*	*12.3 percent*
Peer mothers	488	450	334	467	345

Baseline specification: either births before or after the cutoff date in the reform window
Sample B: Limit reform window to births that occur at least 30 days before/after cutoff.
Sample C: Peer groups are defined to be equal to firms.
Sample D: Restrict coworker birth to be at least 9 months after last peer birth.
Sample E: Limit reform window to 6 months (3 before and 3 after reform).

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Table 3.9: Baseline sample characteristics in comparison to all observed mothers

	All mothers			Sample		
	Mean	SD	N	Mean	SD	N
No return within 10 months	0.79	0.41	12069	0.79	0.41	1340
Return within 11-15 months	0.40	0.49	11111	0.42	0.49	1222
No return within 24 months	0.25	0.44	7682	0.24	0.43	848
Age at childbirth	31.79	4.52	12069	32.14	4.25	1340
Prior earnings	2365.38	1159.50	12069	2842.40	1165.95	1340
Days in employment	3196.19	1562.36	12069	3224.46	1557.75	1340
Days in firm	2448.60	1770.78	12069	2124.74	1782.24	1340
Days in unemployment	128.17	339.66	12069	89.34	238.32	1340
Part-time employed	0.31	0.46	12069	0.25	0.43	1340
High education ($\geq$ college)	0.13	0.33	12069	0.21	0.41	1340
Low education ($<$ highschool)	0.04	0.20	12069	0.03	0.18	1340
Migration background	0.05	0.22	12069	0.06	0.23	1340
Number of children	1.32	0.52	12069	1.24	0.44	1340
Date of childbirth (Jan 01, 2007 is zero)	621.75	258.20	12069	620.95	266.26	1340
West Germany	0.59	0.49	12069	0.65	0.48	1340
Employer change upon return	0.18	0.38	9197	0.18	0.38	1028
Group size	31.29	110.59	3031	28.99	54.70	375
Births in group	1.09	4.75	3321	1.20	0.83	378
Firm size	268.90	722.53	1927	722.72	1482.01	311
Old firm ($\geq$ 10 years)	0.80	0.40	1933	0.93	0.26	311
Standard wages	0.33	0.47	1146	0.37	0.48	243
Median gross daily income in firm	83.51	30.88	1884	104.09	26.12	311
Share of part-time workers in firm	0.25	0.26	1812	0.21	0.20	294
Share of women in firm	0.53	0.28	1825	0.51	0.25	297
Share of temporary workers in firm	0.09	0.16	1815	0.09	0.14	295
District childcare coverage	17.91	13.40	380	20.26	14.85	252
District population density	530.71	689.02	380	556.53	731.64	252
District unemployment rate	7.74	3.80	380	8.13	4.09	252

Notes: 'All mothers' refers to all women in the LIAB who gave birth between July 2007 and December 2009.
Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Table 3.10: Discriptive statistics for alternative sample specifications

	Sample B		Sample C		Sample D		Sample E	
Variable	Mean	SD	Mean	SD	Mean	SD	Mean	SD
No return within 10 months	0.79	0.40	0.80	0.40	0.79	0.41	0.79	0.41
Return within 11-15 months	0.41	0.49	0.43	0.49	0.42	0.49	0.42	0.49
Age at childbirth	32.11	4.28	32.08	4.32	32.11	4.24	32.15	4.13
Prior earnings	2832.75	1167.70	2643.53	1128.26	2858.52	1169.13	2795.56	1145.58
Days in employment	3215.60	1558.36	3299.74	1544.15	3208.54	1574.58	3273.18	1541.68
Days in firm	2085.90	1773.23	2108.55	1764.13	2168.87	1771.72	2353.49	1763.51
Days in unemployment	91.05	243.27	99.70	292.32	87.86	238.03	77.13	225.53
Part-time employed	0.25	0.44	0.25	0.43	0.24	0.43	0.29	0.45
High education ($\geq$ college)	0.20	0.40	0.12	0.33	0.21	0.41	0.19	0.39
Low education ($<$ highschool)	0.03	0.17	0.04	0.21	0.03	0.17	0.03	0.18
Migration background	0.06	0.23	0.05	0.21	0.06	0.23	0.07	0.25
Number of children	1.24	0.45	1.26	0.47	1.25	0.45	1.30	0.49
Date of childbirth (01/01/07= 0)	622.89	267.28	632.75	268.06	695.57	238.93	613.85	261.57
West Germany	0.64	0.48	0.63	0.48	0.65	0.48	0.65	0.48
Employer change upon return	0.18	0.38	0.17	0.37	0.17	0.38	0.15	0.36
Group size	29.37	56.48	20.99	52.42	30.04	55.93	43.16	70.09
Births in reform window	1.21	0.85	0.57	0.94	1.23	0.84	2.09	1.81
Firm size	734.16	1528.60	284.48	395.10	740.71	1525.45	837.08	1709.27
Old firm ($\geq$ 10 years)	0.93	0.25	0.92	0.26	0.93	0.25	0.94	0.25
Standard wages	0.37	0.48	0.32	0.47	0.36	0.48	0.36	0.48
Median daily income in firm	104.21	25.35	99.29	24.14	104.55	25.99	105.14	25.73
Share of part-time workers	0.21	0.19	0.18	0.19	0.21	0.19	0.24	0.22
Share of women in firm	0.51	0.25	0.47	0.25	0.51	0.25	0.54	0.26
Share of temporary workers	0.10	0.14	0.07	0.11	0.10	0.14	0.08	0.10
District childcare coverage	20.61	14.93	20.69	15.18	20.83	14.88	19.69	14.77
District population density	566.95	737.46	587.36	773.88	559.39	736.35	588.78	765.47
District unemployment rate	8.21	4.15	8.16	4.18	8.07	4.08	8.19	3.92
N	1245		1089		1107		1482	

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

Table 3.11: Complier characteristics - first stage results across subgroups

Dep. Variable = y_P	Baseline		High education		High income (> 1/3)		Large firms ($\geq$ 300)		Old firms (> 10y)	
z_P	0.185***	0.175***	0.192**	0.188*	0.258***	0.237***	0.232***	0.222***	0.207***	0.203***
	(0.045)	(0.045)	(0.093)	(0.095)	(0.064)	(0.063)	(0.059)	(0.057)	(0.046)	(0.045)
Age at childbirth		-0.011		-0.022		-0.018		-0.009		-0.011
		(0.006)		(0.015)		(0.009)		(0.006)		(0.006)
Prior earnings		0.000*		0.000*		0.000		0.000		0.000
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
High education		-0.025		8		0.053*		-0.015		-0.024
		(0.057)				(0.072)		(0.066)		(0.060)
Low education		0.228**				0.320**		0.241**		0.296***
		(0.107)				(0.157)		(0.093)		(0.085)
Firm size		0.000		0.000		0.000		0.000		0.000
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
Number of peer births		-0.036		-0.046		-0.043		-0.033		-0.034
		(0.022)		(0.062)		(0.030)		(0.025)		(0.024)
Peer group size		0.000**		0.000		0.001**		0.000*		0.000***
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
West Germany		0.030		0.198*		0.075*		-0.009		0.046*
		(0.046)		(0.108)		(0.076)		(0.058)		(0.047)
Constant	0.561***	0.895***	0.525***	1.120**	0.526***	1.250***	0.560***	0.977***	0.564***	0.903***
	(0.036)	(0.176)	(0.074)	(0.512)	(0.054)	(0.289)	(0.048)	(0.191)	(0.037)	(0.190)
N	488	486	86	86	239	237	293	291	441	439
R-sq	0.037	0.062	0.039	0.104	0.074	0.123	0.062	0.097	0.048	0.076
Relative likelihood	1.00	1.00	1.04	1.07	1.39	1.35	1.25	1.27	1.12	1.16

*** p<0.01, ** p<0.05, * p<0.1

Source: IAB Linked Employer-Employee Panel (LIAB) LM 1993-2010.

General Conclusion

There are several conclusions that can be drawn from this dissertation: first, social policies can effectively change labor supply of mothers with young children and older workers. In detail, I find that the early retirement age increase that was brought about the 1999 pension reform effectively increased employment of older women in Germany. Furthermore, the parental leave benefit reform of 2007 induced women with medium or high income to remain at home with a larger probability during the first year after childbirth.

Second, social policies do not just have a direct effect through a change in legal requirements or financial incentives, but may also change individual behavior through a change in the behavior of the relevant social reference group. In particular, I show that peers at the workplace influence maternal leave taking in the first year after childbirth. Furthermore, I find that employment exit decisions of workers approaching the retirement age are affected by the employment status of their coworkers. I conclude that, in addition to changes financial incentives, indirect effects through peer behavior need to be taken into account when predicting social policy reform outcomes.

Third, the results of all three chapters of this dissertation are heterogenous with respect to different subgroups. For example, the early retirement age increase led to prolonged unemployment or inactivity of those women who were not employed in their late 50s, while employed women stayed in employment and thereby increased their current and future income streams. Furthermore, peer effects in maternal leave decisions are stronger for mothers that are likely to face higher uncertainty regarding their employers' reactions to a long leave. Finally, older workers with low income and those who work in small firms are more likely to be affected by financial incentives in their employment exit decisions.

This dissertation could be built upon in various ways. In general, there has been little work on peer effects among coworkers in the context of labor supply decisions. Consequently, there is room to expand the literature, for example by analyzing how fathers' parental leave take-up is affected by coworkers in response to the parental leave benefit reform in Germany.

As described in Chapter 2 and 3 of this dissertation, there are several challenges associated with the identification of peer effects. Quasi-experimental variation in peer outcomes induced by a policy reform can be exploited to separate pure peer effects from correlated characteristics and shocks within social groups. We were able to use the 2007 parental leave benefit reform in a quasi-experimental setting in the third chapter of this dissertation. However, due to the lack of exogeneous variation of peer behavior, the identification of peer effects in the Chapter 2 relies on relatively strong assumptions. An idea of future research would therefore be a quasi-experimental analysis of peer effects in employment exit decisions of older workers. For example, one could use the variation in employment exit behavior originating from a pension reform to verify our findings.

Finally, this dissertation finds suggestive evidence for heterogenous reform and peer effects for different subgroups. It would be of great policy relevance to enlarge upon these heterogeneities and explore distributional consequences of social policy reforms.

Summary (en)

The present dissertation comprises three chapters on the direct and indirect effects of social policies on labor supply decisions of two exemplary groups of individuals with particularly low employment rates: mothers with young children and workers approaching the retirement age. The first chapter of my dissertation explores whether the abolishment of an early retirement program for women effectively increased employment of the affected group, or instead led to increased program substitution into unemployment or disability pension programs. In the second and third chapter, I analyze whether the decisions of peers at the workplace have influence individual labor supply decisions of older workers and mothers with young children respectively.

In Chapter 1, we analyze the labor market effects of a substantial increase in the early retirement age for women. The 1999 pension reform induces a large one-time shift in the early retirement age through the abolishment of the early pension for women. We exploit the unprecedented sharp discontinuity in the early retirement age between cohorts to estimate the causal impact on female employment behavior in a regression discontinuity framework based on high quality administrative data (VSKT 2014). Raising the early retirement age has

the potential to extend contribution periods and to reduce the number of pension beneficiaries at the same time, if employment exits can be successfully delayed. However, workers may not be able to work longer or may choose other social support programs as exit routes from employment. Our results suggest that the reform increased both employment and unemployment rates of women after their 60^{th} birthday. However, we do not find evidence for active program substitution from employment into unemployment or disability pension programs. Instead, employed women remain employed and unemployed women remain unemployed beyond the pre-reform early retirement age. We do not find evidence for increased take-up of disability pensions benefits. Based on these results, our conclusions are mixed. The reform seems to be an effective tool to extend employment of employed women. Furthermore, the results suggest that the reform affected certain groups heterogeneously. We find larger effects on unemployment rates in East Germany than in West Germany, which is consistent with the fact that unemployment rates were higher and early retirement was more prevalent in the East. The main distributional effects of the reform result from the persistence of labor market statuses: unemployed or inactive women remained in their respective status while employed women continued being employed.

In Chapter 2, I provide novel empirical evidence on the question whether individual employment exit decisions of workers between age 55 and 67 are affected by their peers at the workplace. Furthermore, I simulate the direct and indirect effects of a pension reform through the change in financial incentives and peer behavior. Linked employer-employee panel data (LIAB LM 9314) by the IAB enable the assignment of a peer group to each individual. Occupation and firm fixed effects account for endogeneity due to sorting of workers into firms and occupations. The regression results suggest that there is a significant negative effect of both the financial option value of employment and the share of older coworkers that is still working on individual employment exit probabilities. The coefficient of the option value is largest for the subgroups with low education or income, and for individuals working in small firms. The policy simulation of an increase in the normal retirement age to 67 leads to a modest increase in the predicted exit age by 2.4 months. A delay of peer employment exits by 2.4 months results in an additional increase in the individual exit age by 0.7 months, despite constant financial incentives. I conclude that indirect effects through changes in peer behavior can lead to considerable amplification of pension reform effects.

In Chapter 3, we analyze whether mothers' parental leave decisions depend on their coworkers' parental leave durations. To solve the challenges associated with the identification of peer effects, we exploit quasi-random variation in the costs of parental leave induced by a policy reform. The 2007 parental leave benefit reform encouraged mothers to remain at home during the first year following childbirth. As in Chapter 2, administrative linked employer-employee data (LIAB LM 9310) enable us to assign a peer group to individuals who work in the same establishment and occupation. Our results suggest that maternal decisions regarding the length of their own parental leave are significantly influenced by their coworkers' decisions. We find that a mother is about 30 percentage points more likely to stay at home for the first year if her peers decided to do so in response to the parental leave benefit reform. This effect corresponds to the Local Average Treatment Effect (LATE). We also estimate the Intention to Treat Effect (ITT), showing that having peers who gave birth after the introduction of the new parental leave benefit increases the probability that a mother takes a leave of at least one year by 7 percentage points in contrast to mothers with peers who gave birth shortly before this date. The results of separate analyses for those subgroups for whom uncertainty regarding the employer's reaction to parental leave decisions is expected to be higher, suggest that information transmission and the reduction of uncertainty that comes with observing peer behavior may be among the critical channels driving peer effects in our context.

Summary (de)

Die vorliegende Dissertation umfasst drei Kapitel, welche die direkten und indirekten Auswirkungen von Sozialpolitiken auf die Arbeitsangebotsentscheidungen von zwei Personengruppen mit besonders niedrigen Beschäftigungsquoten untersuchen: Mütter mit kleinen Kindern und ältere Arbeitnehmer, die sich dem Ruhestand nähern. Das erste Kapitel meiner Dissertation untersucht, ob die Abschaffung der Altersrente für Frauen die Erwerbstätigkeit in der betroffenen Gruppe erfolgreich erhöht hat, oder stattdessen zu einem erhöhten Eintritt in die Arbeitslosigkeit oder die Erwerbsminderungsrente geführt hat. Im zweiten und dritten Kapitel analysiere ich, ob die Entscheidungen von Kollegen am Arbeitsplatz einen Einfluss auf individuelle Arbeitsangebotsentscheidungen von älteren Arbeitnehmern und Müttern mit kleinen Kindern haben.

In Kapitel 1 untersuchen wir die Auswirkungen der Anhebung des frühestmöglichen Renteneintrittsalters für Frauen. Die Abschaffung der Altersrente für Frauen im Zuge des Rentenreformgesetzes von 1999 führt zu einer deutlichen Anhebung des frühestmöglichen Renteneintrittsalters für die betroffenen Frauen. Wir nutzen diese außergewöhnliche und abrupte Diskontinuität zwischen den Kohorten, um die kausalen Auswirkungen auf das Erwerbsverhalten von Frauen in

einem *Regression Discontinuity Design* zu analysieren. Die Analyse basiert auf den qualitativ hochwertigen administrativen Daten der Deutschen Rentenversicherung (VSKT 2014). Die Anhebung des frühestmöglichen Renteneintrittsalters hat das Potenzial, die Beitragszeiten zu verlängern und gleichzeitig die Anzahl der Rentenempfänger zu senken, wenn Beschäftigungsaustritte erfolgreich verzögert werden. Allerdings kann diese Reform auch dazu führen, dass Arbeitnehmer, die nicht länger arbeiten können oder wollen, vermehrt andere Programme der sozialen Sicherung in Anspruch nehmen. Unsere Ergebnisse deuten darauf hin, dass die Reform sowohl die Beschäftigung als auch die Arbeitslosenquoten von Frauen nach ihrem 60. Geburtstag erhöht hat. Allerdings finden wir keine Evidenz für aktive Programm-Substitution aus der Erwerbstätigkeit in Arbeitslosigkeit oder Erwerbsminderungsrente. Stattdessen bleiben erwerbstätige Frauen länger erwerbstätig und arbeitslose Frauen bleiben länger arbeitslos. Wir finden keine Beweise für eine verstärkte Inanspruchnahme von Erwerbsminderungsrenten. Basierend auf diesen Ergebnissen sind unsere Schlussfolgerungen ambivalent. Die Reform scheint durchaus ein wirksames Instrument zur Erweiterung der Erwerbstätigkeit älterer Frauen zu sein. Darüber hinaus deuten die Ergebnisse darauf hin, dass die Reform bestimmte Gruppen heterogen beeinflusst hat. Wir sehen größere Auswirkungen auf die Arbeitslosigkeit in den neuen Bundesländern, was damit zusammenhängt, dass die neuen Bundesländer höhere Arbeitslosenquoten haben und der Vorruhestand dort weit verbreitet ist. Die wichtigsten Verteilungseffekte der Reform resultieren aus der Beibehaltung des Arbeitsmarktstatus: Arbeitslose oder inaktive Frauen blieben in ihrem jeweiligen Status, während die Beschäftigten länger beschäftigt bleiben.

In Kapitel 2 untersuche ich die Forschungsfrage, ob individuelle Erwerbsaustrittsentscheidungen älterer Arbeitnehmer von den Erwerbsentscheidungen der Kollegen am Arbeitsplatz beeinflusst werden. Darüber hinaus simuliere ich die direkten und indirekten Effekte einer Rentenreform durch die Veränderung der finanziellen Anreize und des Peer-Verhaltens. Linked Employer-Employee Daten des IAB (LIAB LM 9314) ermöglichen die Zuordnung aller Mitarbeiter einer Firma und Berufsgruppe zu einer Peer Gruppe und die Analyse der jeweiligen Erwerbshistorien. Berufsgruppen- und firmenspezifische Effekte lösen das Endogenitätsproblem, das aufgrund der nicht-zufälligen Selektion von Arbeitnehmern in Firmen und Berufe entsteht. Die Ergebnisse der Regressionsanalyse deuten darauf hin, dass der finanzielle Optionswert der Erwerbstätigkeit einen signifikanten negativen Effekt auf die durchschnittliche Erwerbsaustrittswahrscheinlichkeit hat. Außerdem sinkt die Erwerbsaustrittswahrscheinlichkeit, wenn die älteren Kollegen noch erwerbstätig sind. Der Koeffizient des Optionswertes ist für Individuen in kleinen Firmen, mit niedrigem Bildungsniveau oder niedrigem Einkommen am größten. Die Simulation einer Anhebung des normalen Renteneintrittsalters auf 67 führt zu einer geringen Zunahme des prognostizierten Erwerbsaustrittsalters um 2,4 Monate. Eine Verlängerung der Erwerbstätigkeit der jeweiligen Kollegen um 2,4 Monate, führt zu einer zusätzlichen Erhöhung des Erwerbsaustrittsalters um 0,7 Monate, trotz unveränderter finanzieller Anreize. Ich schlussfolgere daraus, dass indirekte Effekte durch Veränderungen des Peer-Verhaltens zu einer beträchtlichen Verstärkung von Reformeffekten führen können.

In Kapitel 3 analysieren wir, ob die Dauer der Elternzeit von Müttern von den Entscheidungen ihrer Kolleginnen abhängen. Um die Herausforderungen zu lösen, die mit der Identifizierung von Peer-Effekten verbunden sind, nutzen wir die

Veränderung der mit der Elternzeit einhergehenden Kosten, die durch eine Reform verursacht wurde. Die Elterngeldeinführung von 2007 bietet vor allem gutverdienenden Müttern finanzielle Anreize im ersten Jahr nach der Geburt zu Hause zu bleiben. Wie in Kapitel 2 ermöglichen administrative Linked Employer-Employee Daten (LIAB LM 9310) die Zuordnung von Personen, die in derselben Firma und Berufsgruppe arbeiten, zu einer Peer-Gruppe. Unsere Ergebnisse deuten darauf hin, dass Entscheidungen von Müttern über die Dauer ihrer Elternzeit deutlich von den Entscheidungen ihrer Kolleginnen beeinflusst werden. Konkret finden wir, dass Mütter etwa 30 Prozentpunkte häufiger im ersten Jahr zuhause bleiben, wenn ihre Kolleginnen – als Reaktion auf die Elterngeldreform – auch mindestens ein Jahr zuhause bleiben. Dieser Effekt entspricht dem Local Average Treatment Effect (LATE). Wir zeigen außerdem, dass Mütter, deren Kolleginnen nach der Einführung des neuen Elternurlaubs ein Kind bekommen haben, um 7 Prozentpunkte wahrscheinlicher mindestens ein Jahr zuhause bleiben als Mütter, deren Kolleginnen kurz vor diesem Datum ein Kind bekommen haben. Dies entspricht dem sogenannten Intention To Treat Effect (ITT). Die Ergebnisse der separaten Analysen für verschiedene Untergruppen deuten darauf hin, dass eine Verringerung der Unsicherheit über die Reaktion des Arbeitgebers, die mit der Beobachtung des Peer-Verhaltens einhergeht, zu den kritischen Wirkungskanälen für Peer Effekte in diesem Kontext gehören könnte.

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