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To my parents and Rainer

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ABSTRACT

This dissertation consists of three chapters that study issues related to unemployment and retirement decisions of workers.

The first chapter examines the impact of additional pension benefits on the retirement timing of low-income female workers in Germany. Using administrative pension insurance records from Germany, it studies the impact of a pension subsidy program on retirement decisions of recipients. The kinked schedule of the policy allows me to identify the causal effect using a regression kink design. The estimation suggests that 100 euros in additional monthly pension benefits induce female recipients to claim pensions earlier by about 10 months. A back-of-the-envelope calculation suggests that the ratio of the behavioral cost to the mechanical cost of this subsidy program is 0.3, which is smaller than that of other anti-poverty programs.

The second chapter studies the total labor supply effects of Unemployment Insurance (UI) for older workers — both at the extensive and the intensive margin. It documents sharp bunchings in UI inflows at age discontinuities created by UI eligibility for workers in their 50s. Using a combination of regression discontinuity designs and bunching techniques, we quantify the magnitude of these responses exploiting

a variety of thresholds, kinks, and notches induced by the UI and retirement institutions. We estimate the total effect using a dynamic life-cycle structural model. Results suggest that the impact of UI extension on non-employment durations for older workers is almost twice as large as the impact for younger workers.

The third chapter examines the impact of receiving written advance notification of layoff on labor supply of displaced workers by exploring the California Worker Adjustment and Retraining Notification (WARN) Act. The California WARN Act, implemented in 2003, expands the requirements of the federal WARN Act. It provides protection to workers in smaller firms and at smaller layoff events. Using the Displaced Worker Supplement to the Current Population Surveys from 1996 to 2018 and a differences-in-difference method, I find that the displaced workers affected by the mini WARN Acts are 3% more likely to claim unemployment insurance. Conditional on claiming UI, they are less likely to exhaust UI.

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List of Abbreviations

CPS	Current Population Surveys
ERA	Early Retirement Age
FRA	Full Retirement Age
NRA	Normal Retirement Age
RKD	Regression Kink Design
UI	Unemployment Insurance
WARN	Worker Adjustment and Retraining Notification

Chapter 1

The Effect of Pension Subsidies on Retirement Timing of Older Women: Evidence from a Regression Kink Design

1.1 Introduction

Retirement income adequacy is an important concern for vulnerable groups, such as female workers, who are at much greater risk of old-age poverty than older men. In Germany, the pension benefit of an average woman is only about half that of an average man. This issue is of particular importance during times of reducing public pension replacement rates due to the aging population.[1] Furthermore, low-income workers are disproportionately affected by the recent pension reforms that penalize claiming pension early.[2] One way to ensure workers have adequate incomes in old age is via income support programs. Many developed countries have provided safety nets for pensioners with low benefits. However, policymakers face an important trade-off: how to provide income support to elderly people without hurting incentives

[1] For example, the net pension replacement rates for future retirees with low wages in Germany are among the lowest in OECD countries. "German workers earning half the average wage and retiring after a full career may expect a net replacement rate of 53% in the long term against 75% on average across the OECD. For average-wage workers, replacement rates will also be below average, at 50% compared to 63% in the OECD" (OECD [2015]).

[2] Studies have found that the sick and the poor could not adjust their labor supply in responses to recent pension reforms by working longer and had to suffer the early retirement penalties (Hupfeld [2009], Hanel [2010]).

to work.[3] Therefore, it is important to understand the extent to which additional pension benefits affect low-income workers' retirement timing.

However, this question is understudied. It is partly due to the difficulty of isolating exogenous variation in the parameters of the public pension system, including benefit levels, pension eligibility age, penalties for claiming pension early, etc. In this paper, I explore a specific feature of the German pension system, which allows me to identify the effect of additional pension benefits on retirement decisions in an environment in which the statutory pension eligibility age is unchanged. Existing papers that analyze the change in labor supply in response to policy changes, such as recent pension reforms, tend to overestimate the effects of changes in pension benefits (Song and Manchester [2007], Coile and Gruber [2007], Duggan et al. [2007], Staubli and Zweimüller [2013]). For instance, pension reforms are often in the form of raising pension eligibility age accompanied by financial penalties for claiming pension early. The estimated overall impacts may be a combination of labor supply response to a change in lifetime income and response to a change in the focal reference point - the statutory pension eligibility age (Blundell et al. [2016], Cribb et al. [2016], Seibold [2017]). For example, Seibold [2017] has documented that workers' responses to discontinuities in lifetime budget constraint at statutory retirement ages are much larger than responses to other budget constraint discontinuities, which do not link to statutory ages.

In this paper, I explore a pension subsidy program for low pay workers in Germany, implemented in 1992. I exploit the very sharp kink in the schedule of benefits as a function of predetermined past contributions to implement a regression kink design. This empirical design allows me to identify the causal effect of additional pension

[3] Studies (Börsch-Supan and Schnabel [1998], Friedberg [2000], Eissa and Hoynes [2006], Eissa and Hoynes [2004], Schmieder et al. [2012a]) have shown the disincentive effects of similar welfare programs, such as disability insurance, the earned income tax credit and unemployment insurance.

benefits on retirement decisions. In detail, I use administrative data from the Research Data Centre of the German Pension Insurance to study a pension subsidy program for low pay workers (Mindestentgeltpunkte bei geringem Arbeitsentgelt, SGB VI § 262) introduced by the 1992 Pension Reform Act in Germany. Several features of this pension subsidy program make it a good instrument. First, it provides a source of exogenous variation in pension benefits. This is because the subsidy size is predetermined by contributions before 1992 and past relative wage position. It provides an exogenous variation in subsidy sizes without manipulation behaviors. Second, the subsidy size has a kinked relationship with average wage income before 1992. This enables me to implement the regression kink design as the empirical method. This approach has never been used to study the effect of additional pension benefits, to the best of my knowledge. Lastly, the change of pension benefits does not associate with a change in statutory retirement age. In other words, statutory pension eligibility age and other parameters of the pension system remain unchanged around the kink. This allows me to isolate the impact of changes in pension benefits.

The baseline sample consists of female subsidy recipients in West Germany who retired between 1994 and 2014. On average, the subsidy increases pension benefit by around 16%, and this creates an average implicit tax of approximately 8%. The estimation suggests that €100 additional monthly pension benefits induce female recipients to claim old age pension earlier by around ten months and the hazard rate to claim a pension at age 60 increases by 17%. The impacts on the age of exiting employment have the same magnitude but is noisy. The hazard rate to exit employment at age 60 increases by 14%. Because it is common for workers not to transition directly from full-time employment to retirement in Germany. I also assess the impacts of pension subsidies on workers' behaviors regarding using unemployment insurance (UI) and marginal employment as stepping stones to retirement. I find that more pension incomes reduce low-income female workers' time spent in marginal employment during the bridge years. More pension incomes also increase recipients' probability to use UI as a pathway to retirement and prolong their time spend in UI during the bridge years. The policy takeaway is that while additional pension benefits induce low-income female workers to claim pension earlier, it has little impact on the probability to exit regular jobs, which are jobs with mandatory social security contribution obligations. Therefore, additional pension benefits have low impacts on the contributions to the public pension system. A back-of-the-envelope calculation suggests that in order to increase the mechanical transfer to lifetime pension income by 1 euro, the government has to raise an additional 0.3 euro. It implies that the ratio of behavioral cost to mechanical cost of this subsidy program is 0.3. This number is smaller than that of other anti-poverty programs such as extending unemployment benefits and progressive taxation.

This paper complements and extends earlier work. First, I build on past work on the effects of pension generosity on retirement decisions (Stock and Wise [1990], Krueger and Pischke [1992], Snyder and Evans [2006], Puhani and Tabbert [2011], Gelber et al. [2017a], Lalive et al. [2017]). It is undeniable that pension provision affects retirement decision making. Prior research has found pension subsidy schemes often reduce incentives to work, either in the form of a flat-rate minimum pension (Jiménez-Martín et al. [2007]) or as earning-tested income support programs for pensioners (Gruber and Wise [2004], Feldstein and Liebman [2002]). However, there are limited studies that credibly measure the causal impact of additional pension benefits. In the U.S., most of the evidence is based on an unanticipated decline in social security wealth for the US "notch" cohort born in the period 1917-1922. Both Krueger and Pischke [1992] and Snyder and Evans [2006] look at this variation. While Krueger and Pischke [1992] did not find significant impacts on employment, Snyder and Evans [2006] found that the affected cohorts are 5% more likely to work at older ages. In Germany, Puhani and Tabbert [2011] estimated the impact of a large pension cut for low-skilled workers using a regression discontinuity method. They found no significant response in retirement age. My paper provides further information on the impact of additional pension benefits on retirement timing. The sizes of estimators obtained in this paper are different from the above-mentioned studies but within a reasonable range. Second, this paper complements other efforts to elicit evidence on the labor supply of a particular population group - low-income older women (Hanel and Riphahn [2012], Lalive and Staubli [2015], Finkelstein et al. [2016], Gelber et al. [2016]). This group is of particular interest because women are more exposed to old-age poverty than men. Women on average have lower pension incomes because women experience more career interruptions and part-time work than men due to their childcare duties. Moreover, compared to men, women's labor supply elasticities are larger and women on average live longer. Therefore, older women's labor supply responses to additional pension benefits are more likely to have a larger financial consequence. Lalive and Staubli [2015] shows that a 3.5% deduction in pension wealth caused by raising the full retirement age in Switzerland strongly affects older women's labor supply. Affected female workers delay pension claim by 6.6 months in their paper. The magnitude of my result is slightly smaller than their finding. Lastly, this paper is related to literature on pension reforms (Hanel [2012], Manoli [2016], Engels et al. [2017]). For example, Engels et al. [2017] and Manoli [2016] exploit the kinked schedule as the reforms increase the early retirement age gradually by birth cohorts, and implement regressions kink designs, in the setting of Germany and Austria, respectively. Both papers found large responses in the age of exiting jobs and the pension claiming age.

This paper proceeds as follows. Section 2 describes the core features of German pension system and the details of the subsidy program. Section 3 presents the data and sample selection. Section 4 provides a simple conceptual framework. Section 5 explains the RKD setup and tests the RKD assumptions. Section 6 presents graphical evidence and reduced form evidence. I estimate both the changes in slopes of the treatment variable - subsidy size and the changes in slopes of the outcome variables around the kink. The RKD estimators are then obtained by dividing the change in slopes of outcome variable by the change in slopes of pension subsidies. I also present impacts on activities during the bridge to retirement periods. Heterogeneous impacts and robustness checks are also presented. Then I discuss the fiscal costs and policy implications of this subsidy program and conclude.

1.2 Institutional Details

1.2.1 Features of Public Pension Scheme in Germany

The statutory public scheme in Germany is an earnings-related points system financed on a pay-as-you-go basis. Participation is mandatory, except for civil servants and the self-employed. The pension system is financed with contribution payments, which are normally shared equally by employers and employees. In 2016, the total mandatory contribution rate was 18.9%. On average, the public pension replaces around 50% of pre-retirement wage, net of tax and contribution. In 2016, the average monthly pension benefit of the insured was €951 for men and €636 for women.

The statutory retirement age for a regular old-age pension remains at 65 throughout my sample period, with the only prerequisite being 5 years of contributions.[4] Several alternate pathways make retiring before 65 an option.[5] Notably, women born

[4]The age for regular old age pension increased gradually from 65 to 67 since 2012. It will reach age 67 in 2030.

[5] There are four main early retirement pathways. They are old-age pensions for long-term insured, old-age pensions for women, old-age pensions due to unemployment (and, later, part-time work) and old-age pensions for severely disabled persons (Börsch-Supan and Wilke [2004]).

before 1951 are eligible to claim pension at early retirement age (ERA) 60 via the old-age pension for women. The eligibility requirement for this pathway is 15 years of contributions of which at least 10 years must have occurred after age 40. Almost all recipients of this subsidy program are eligible for this pathway. The ERA via the women pension pathway stays at 60 during the sample period.

Moreover, workers know the expected pension benefits they will get when they retire. It is because letters with detailed pension information were sent to workers every 3 years from age 55 before 2005. Since 2005, letters have been sent annually to workers who are 27 years old and have contributed to the public pension for at least 5 years. Dolls et al. [2018] have shown that those letters inform workers their pension entitlements in a salient fashion. Therefore, workers do take into account the additional pension benefits when they make retirement decisions.

1.2.2 Pension Benefits

In Germany, pension benefit level is closely tied to employment. The main determinant of pension benefit is the sum of individual accumulated earnings points (Entgeltpunkte, EP). They are also referred to as pension points. Essentially, for each year of contribution, a worker will accumulate some earnings points $EP_{i\tau}$, which are decided by the worker's relative wage position compared to average wage of all the insured. For example, a worker whose wage is half of the average wage during this contribution year τ will accumulate 0.5 point in this year.

$$PB_{it} = [\underbrace{(\sum_{\tau} EP_{i\tau} + \text{ Subsidy}_i)}_{\text{Personal Pension Base}} \times PV_t] \times AF_{it} \text{ , where } EP_{i\tau} = \frac{w_{i\tau}}{\bar{w_\tau}}$$

The worker's personal pension base is the sum of the EPs accumulated over time plus additional EPs credited by the subsidy program. For example, an average wage earner with 15 contribution years accumulates 15 EPs. This personal pension base is scaled up by the pension value PV_t at the time of claim, which is determined aggregately by factors such as average wage of all insured, the contribution rate and demographic changes. For example, one EP was equivalent to €30 per month in 2015.

Personal pension base times pension value gives the total amount of pension benefit. This benefit level is then adjusted by an access factor AF_{it}.[6] This access factor penalizes early pension claim. Workers who claim pension at ERA face a 0.3% pension reduction per each month they retired in advance of the full retirement age. For female workers claiming old-age pension for women in our sample, only cohorts born after 1940 are affected by the access factor.

In sum, pension benefits increase with contribution year and relative wage income. On average, one additional year of full value contribution increases the gross replacement rate by around 1.17%. Therefore, workers with low wages or a short working history will have a low pension benefit in the German pension system.

1.2.3 Pension Subsidies to Low Pay Workers

The pension subsidy to low pay workers (Mindestentgeltpunkte bei geringem Arbeitsentgelt) essentially provides a built-in subsidy that offers additional EPs to workers with low lifetime contribution (SGB VI § 262). It was introduced by the 1992 Pension Reform Act in Germany. Along with reforms aiming at prolonging working life and raising the statutory retirement age, the primary policy consideration of this subsidy program is to ensure adequate old-age income for low wage workers. This pension subsidy program is large. According to the Research Data Centre of the German Pension Insurance, in December 2015, 14% of old age pensioners — 4% of

[6] Pension benefit is also adjusted according to the type of pension. This factor is one for old-age pension, and less than 1 for disability pensions. In my sample, almost all workers claim old age pension.

all male pensioners and 26% of all female pensioners — are recipients of this subsidy program. The total payments for this subsidy program were approximately €3 billions in 2015.

The target group of the subsidy constitutes workers with a relatively long work history and relative low wage income. To be more specific, there are two eligibility criteria. First, a worker should have at least 35 creditable years, which include contribution periods and parental years given to mothers with children.[7] The time of raising a child up to age 10 counts into the creditable years. The package is 10 years for one child, 15 years for two children and 20 years for more than two children. Therefore, the 35 years with pension rights is a relatively lenient criterion for mothers. Second, the average monthly EP of full-value contribution years before January 1992 and average monthly EP of full-value contribution years at retirement must both be less than 0.0625 — equivalent to 0.75 EPs annually.[8] This criterion guarantees that only workers with a wage position of less than 3/4 of an average earner can be the recipients. Once those two conditions are satisfied, a worker will be entitled to this subsidy.

The subsidy size is exogenous and predetermined. It depends on the total EP accumulated before 1992 and the average EP of full-value contribution periods before 1992. In the data, subsidy size is on average 3.19 EPs with a standard deviation of 1.77. It amounts to an increase of benefit by 90 euros per month, which is equivalent

[7] The creditable years consist of active contribution periods, credited periods and consideration periods. Active contribution periods (Beitragszeiten) are usually corresponding to regular employment or self-employment when a fixed percentage of wage is contributed to the pension system. Credited periods (Beitragsfreie Zeiten) includes periods such as maternity leave and vocational training periods. During those periods, EPs are accumulated even though no contribution was made. During consideration periods (Berücksichtigungszeiten), workers accumulate no additional EPs. The time of raising a child up to age 10 counts into the consideration periods.

[8] The contribution periods consist of full value contribution periods (Vollwertigen Beiträgen) and reduced contribution periods (Beitragsgeminderte). Full value contribution periods are periods when compulsory contributions are paid in according to the social security regulation. Reduced contribution periods including periods due to unemployment, sickness and vocational training.

to 17% increase in pension income. The exact formula is

$$Subsidy = min\left(0.5 \times \sum_{t<92} EP_t \ , \ 0.75T_{pre92} - \sum_{t<92} EP_t\right) \tag{1.1}$$

, where T_{pre92} is the years of full-value contribution before 1992. The subsidy equals to either 50% of total EP accumulated before 1992 or the difference between $0.75T_{pre92}$ and total EP before 1992, depending on which one is smaller. Essentially, the subsidy increases $\sum_{t<92} EP_t$ by 50%, but after the subsidy, the average annual EP before 1992 (denoted as aep_{92} from here onward) cannot exceed 0.75. It creates a kinked schedule of subsidy in relationship to aep_{92}. Figure 1 shows the policy schedule according to Equation 1. This kinked schedule enables me to causally identify the impact of this subsidy program. We can see from the figure, the slope of the subsidy changes discontinuously at the kink point; this is where I base on the identification. See Appendix 1.1 for an example illustrating the calculation of pension benefit and subsidy amount.

Equation 2 shows that average subsidy per years before 1992 has a slope of 0.5 before the kinked point 0.5 and a slope of -1 after the kink point. Figure A1a shows the policy schedule according to Equation 2.

$$\frac{Subsidy}{T_{92}} = \begin{cases} 0.5aep_{92} & , aep_{92} \leq 0.5 \\ 0.75 - aep_{92} & , 0.5 \leq aep_{92} \leq 0.75 \\ 0 & , aep_{92} > 0.75 \end{cases} \tag{1.2}$$

To illustrate graphically, Figure 2 plots actual total subsidies measured in 2010 euro against $ape92$ for the main sample. Figure A1b plots the average subsidy per years before 1992 against $ape92$. The actual subsidy exhibits the kinked relationship predicted by the formula. The maximum of average subsidy per year before 1992 in the data is 0.25 as the policy suggests. However, there are two deviations from the policy schedule. First, compared to the policy, the slope of average subsidy per year before 1992 is flatter to the left of the kink. Second, the observed kink is at 0.45 rather than 0.5. Those deviations are measurement errors coming from constructing $ape92$ in the data. This is because the majority of the sample are female workers who have had childcare periods, which involve complex accounting. When I look at sub-sample of workers who were employed during their entire working history, I could obtain an actual kink very close to 0.5. For more details see Appendix 2.1.

It is worth noting that this subsidy program will phase out eventually for workers who started contributing after 1992.[9] Low wage workers, like recipients who started work after 1992, will not receive any subsidy. As the gender pension gap widens, policymakers in Germany have started to consider a new subsidy program for younger cohorts. Therefore, understanding the impact of this program has immediate significance.

1.2.4 Bridge to Retirement: Unemployment Insurance and Marginal Employment

It is plausible that older workers do not transit directly from full-time employment to retirement. They may use unemployment insurance, marginal employment, and other social support programs as stepping stones into retirement (Inderbitzin et al. [2016b], Manoli [2016], Engels et al. [2017]).

The German unemployment insurance (UI) system provides about 60% income replacement to eligible workers who lose their job.[10] The maximum benefit duration

[9] The 1992 reform introduced parental pension credits for mothers who gave birth after 1992 during the first 3 years of childcare. At the time of the reform, the parental pension credits policy is considered as a compensation for the fact that this subsidy for low pay workers will phase out eventually. See Thiemann [2015] for more on parental pension credits.

[10] Replacement rates for UI were relatively stable over the period (67-68% for an individual with children and 63-60% for an individual without children).

for older workers ranges from 18 months to 32 months during our sample period, depending on the age and the previous working history.[11] Time spend on UI increases future pension benefits. Workers who exhausted UI benefits were eligible for unemployment assistance (UA) benefits with an effective average replacement rate of around 30%. Eligible workers can stay in UA until 65.[12] Time spent on UA does not increase pension benefits. The generosity of the unemployment insurance benefits and the lenient job search requirement for older workers make UI an attractive pathway to bridge to retirement.

Another alternative activity is marginal employment. The most popular type of marginal employment in Germany is the mini job, which is commonly called a "400 euro" job. It is because the jobs paying less than €400 per month are exempt from both social security contributions and income taxation.[13] Unemployed workers whose UA benefits are lower than €400/month have incentives to engage in marginal employment before pension becomes available. Additionally, it is possible to claim pension benefits while working in mini-jobs.[14] Gudgeon and Trenkle [2017] points out that the majority of exclusive mini jobbers are women and older workers.

1.3 Data and Sample Selection

The dataset employed in this paper is based on the anonymized Scientific Use File (SUF) of the Insurance Account Sample (Versicherungskontenstichprobe, VSKT) of the German Federal Pension Register. The main dataset is assembled from 11 years of cross-sectional SUFVSKT (2002, 2004 to 2014). SUFVSKT contains 5% of

[11] SeeBörsch-Supan and Wilke [2004] and Gudgeon et al. [2017] for more details in the institutions

[12] From 2005 on, UA was replaced by unemployment insurance benefits 2 (UIB 2), a completely means-tested program. Both UA and UIB 2 are unlimited in duration.

[13] This threshold was €325 before 2003 and €450 after 2013. During most of our sample period, it stayed at €400 per month.

[14] Pension benefits claimed before age 65 face earnings test. If pensioners work in jobs that pay more than 400 euros per month, their pensions are fully withdrawn.

all individuals with an active public pension insurance account, who were between the ages of 30 and 67 at time of data collection. Each cross-sectional SUFVSKT contains around 50 to 60 thousand individuals, among which around 7 to 8 thousand are subsidy recipients. The SUFVSKT includes time-invariant information of the insured person at the time of data collection, such as accumulative pension points, gender, birth month, number of children and age claim pension. It also contains monthly biographical information from age 14 up to the data collection year for each insured person, such as social employment status that are relevant for pension benefit calculation and pension points. However, information on years of education and occupation are not accurately measured. Additionally, it is not possible to observe martial status and link spouses in the data.

1.3.1 Sample Construction

The sample is restricted to female subsidy recipients who are at least 63 years old at the sample year, who have at least 35 service years and have never worked in East Germany.[15] I only look at females in the analysis. It is because more than 80% of the recipients are female around the kink. There aren't enough observations to analyze the causal impacts on male workers. Moreover, individuals who worked in East Germany are excluded because they have a different set of rules regarding pension benefits and contribution, which is not comparable to that of West Germans. Besides, I exclude people who are civil servants and self-employed, because they face different pension systems. I further restrict the sample to workers who are older than cohort 1952 and have at least 15 years of contribution. It is to ensure that all individuals in the sample are eligible to retire at age 60 via old age pension for

[15] See Appendix for more details on the sample construction

woman.[16] Because most female workers have claimed pension by age 63, the observed retirement age in the sample is closer to the actual retirement age by restricting the sample to workers who are at least 63 years old at the sample year.[17] The final sample contains 6,021 individuals, covering cohorts from 1935 to 1951. It amounts to 3.7 million person-month observations.

1.3.2 Summary Statistics

In 2015, around 25.5% of all female pensioners was subsidy recipients. More than 80% of subsidy recipients are female. Two-thirds of the recipients have never worked in East Germany. The recipients' distribution of post-subsidy monthly pension benefits is centered around €750. The majority of the recipients' monthly pension benefits are in between €500 and €1000. Table 1 reports descriptive statistics of some key variables for the baseline sample of female workers, female recipients around the kink and female non-recipients who have at least 35 pension years. The baseline specification focuses on the window of recipients whose aep_{92} are from 0.25 to 0.65, 0.2 EPs around the kink 0.45. There are 5,218 individuals in this window. The average size of the subsidy is 3.19 EP with a standard deviation of 1.77, which is equivalent to 90 euros per month and around 17% of the monthly pension benefits.[18] The recipients in the baseline sample on average have 24 EPs and 42 years of the creditable period, within which 17 EPs and 32 years are from full-value contribution. They on average worked 19 years before 1992. Compared to the non-recipients, the recipients only differ significantly regarding average EP and total EPs. It is in line with the design of

[16] Old age pension for women is one of the early retirement pathways in Germany. For cohorts older than 1952, women can retire as early as age 60 by claiming old age pension for women if they have at least 15 years of contribution. Women who were born in 1952 and later can no longer retire at 60.

[17] We test for robustness to sample construction in section 7. We use a sample of female subsidy recipients who are at least 60, 61, 62 and 64 years old at the sample year.

[18] All monetary values are CPI adjusted and expressed in 2010 euro.

the subsidy program. There are no significant differences in the outcomes variables, such as retirement age and age of last employment, or other individual characteristics, such as the number of children and age of first birth. The non-recipients had worked fewer years before 1992 and had higher EPs. The recipients around the kink are the ones whose aep_{92} are from 0.4 to 0.5. Their average subsidy size is around 3.76 EPs with a standard deviation of 1.9, which is slightly higher than the sample average.

1.4 Conceptual Framework

Here I describe a simple life cycle model to illustrate how the subsidy plays a role. All individuals maximize lifetime utility subject to their lifetime budget set. I assume individuals earn a constant (after tax and pension contribution) wage w at regular jobs and v at marginal employment and at retirement receive total pension pb. Let T be the last period of life, C be total consumption, Y be lifetime income, T^E be the year of exit from the labor force, T^R be the year claim pension and workers start work from period 0. I assume no discounting and that T is known with certainty. Here I assume T is 80. The lifetime budget constraint takes the following form:

$$C = Y = w \times T^E + v \times (T^R - T^E) + pb \times (T - T^R)$$

, where pb is the pension benefit per year and $pb = \frac{w}{\bar{w}} \times T^E \times AR + b$. b is the subsidy amount. I denote the pension replacement rate for each year of contribution as p, where $p = \frac{AR}{\bar{w}}$. Therefore, $pb = p * w * T^E + b$. I also ignore the adjustment due to early claiming.

For simplicity, I make two assumptions: 1) If one leaves a job before early retirement age 60 ($T^E < 60$), then $T^R = 60$. Worker claims pension immediately as pension becomes available at early retirement age. In the sample, among the individuals who leave employment before 60, half retire at 60. 2) If one leaves their job after age 60, then the worker claims a pension immediately ($T^E = T^R$). In the sample, among the individuals who exit employment after age 60, 70% claim immediately. Illustrated in Figure 3, we can see a kink in lifetime budget set at age 60. The solid black line in Figure 3 is the budget without subsidy, and the blue dashed line is the budget with subsidies. These two lines intersect at the age of death ($T = 80$). In other words, if a worker passes away without claiming any pension benefits, then additional pension benefits have no impact on lifetime consumption.

The effect of the subsidies is a combination of wealth effect and substitution effect. Additional pension benefits not only shift the budget set upwards but also change the slope of the budget set. To be more specific, subsidies shift up in parallel for all exit ages before 60. Workers who exit employment before 60 when there is no subsidy will exit earlier due to the wealth effect. After age 60, subsidies change both the level and slope of the budget set.[19] Both wealth and substitution effects make workers who would exit employment after 60 when there is no subsidy exit earlier. The slope slightly changes because the trade-off of delaying exiting employment by one period changes. The gain of one additional year of regular employment includes a year of wage income and an increase of total pension income due to one more year of contribution. The cost is the one year of the forgone pension benefit. Pension subsidies increase the cost and make work less attractive. No matter where the individual was located on the budget line when there was no subsidy program, additional pension benefit will induce them to exit earlier. Moreover, I expect the impact on age of claiming pension is relatively larger than the impact on age of exiting employment. The earliest age a worker can claim a pension is 60, therefore in absence of the subsidy, T^R can not be smaller than 60. Because the change in lifetime budget with subsidy

[19] In Appendix, I show the detailed expression of lifetime budget and the equation of its slope.

after age 60 creates a stronger incentive than the change in segment before age 60, I expect the the impact on age of claiming pension is relatively larger and the impact of age of exiting employment relatively smaller and noisier. It is also worth noting that for workers who left employment before age 60 in absence of the subsidies, their relevant segment of lifetime budget line (before age 60) levels up without changing the slope. The incentives are coming from pure wealth effect for those individuals.

Furthermore, the impact of subsidies should be larger for workers who transit from the mini jobs to claiming a pension than for the ones who transit from regular employment to claiming a pension. This because wage income w at regular jobs is higher than wage income at the mini jobs v. Moreover, one more year of work in marginal employment will not increase the pension benefits.

1.5 Empirical Methodology

1.5.1 Regression Kink Design

The kinked schedule of this subsidy policy allows me to identify the causal effect of pension subsidies on retirement timing. Specifically, I use a Regression Kink Design to estimate the local average treatment effect, following Landais [2015], Card et al. [2015c] and Card et al. [2017].

RKD examines the induced change in the slope of the relationship between the outcome of interest (Y) and the assignment variable (r) at the exact location of the kink in the policy formula. The average treatment effect of subsidy B on Y at the kink ($r = 0$) is expressed as

$$\mathbb{E}(\frac{dY}{dB}|r=0) = \frac{\lim_{r_0 \to 0^+} \frac{d\mathbb{E}(Y|r)}{dr}|_{r=r_0} - \lim_{r_0 \to 0^-} \frac{d\mathbb{E}(Y|r)}{dr}|_{r=r_0}}{\lim_{r_0 \to 0^+} \frac{d\mathbb{E}(B|r)}{dr}|_{r=r_0} - \lim_{r_0 \to 0^-} \frac{d\mathbb{E}(B|r)}{dr}|_{r=r_0}} \tag{1.3}$$

Equation 3 shows the RKD estimand. Here, the slope change in the outcome variables is scaled by the slope change in the pension subsidy with respect to aep_{92}. Because the observed relationship between pension subsidy B and r varies from the policy rule, I adopt a fuzzy RKD approach. I obtain the estimates of both the numerator and denominator by running parametric polynomial regression as the following:

$$Y_i|(r=0) = \alpha_y + [\sum_{p=1}^{p=\bar{p}} \rho_p r_i^p + \beta_p r_i^p \times \mathbb{1}(r_i \geq 0)] + \theta_y X_i + \epsilon_i \text{ , where } |r_i| \leq h \quad (1.4)$$

$$B_i|(r=0) = \alpha_b + [\sum_{p=1}^{p=\bar{p}} \tau_p r_i^p + \gamma_p r_i^p \times \mathbb{1}(r_i \geq 0)] + \theta_b X_i + \epsilon_i \text{ , where } |r_i| \leq h \quad (1.5)$$

where r is the assignment variable. It is aep_{92} centered around kink 0.45. $\mathbb{1}(r_i \geq 0)$ is an indicator for aep_{92} being above the kink, p is polynomial order, h is the bandwidth size. For our baseline estimation, we set p as 1 and h as between 0.25 and 0.65. Y are the outcome variables, such as age claiming a pension, age of exiting employment, the hazard rate to claim a pension at 60, etc. B is the pension subsidy, which are measured per month of additional pension income in 2010 euros The change in the slope of Y around the kink $\frac{dY}{dr}|r=0$ is given by β_1, the change in slope of B around the kink $\frac{dB}{dr}|r=0$ is given by γ_1.

1.5.2 RKD Assumption

The key assumption for a valid inference in a fuzzy regression kink design is to make sure there are no manipulations. The workers to the left and the right of the kink are comparable. One test for no manipulation is to test whether the density of the assignment variable is smooth at the kink point. Intuitively, smoothness in density rules out the possibility that the induced changes in Y are not due to changes in B, but rather due to sample selection or changes in other predetermined covariates. Figure 4 shows the number of individuals observed in each bin of average EP from full-value contribution before 1992. The bin size is 0.01525 aep_{92}, which is equivalent to €40 in annual wage income. We observe a small dip in density of the recipients to the left of 0.45. Additionally, the density shows a quadratic relationship with aep_{92} with the mode of the density around the kink point. McCrary tests are performed to formally test for discontinuities in both density and the derivative of density. The McCrary tests suggest that there is no statistically significant discontinuities in density. However, the derivative of density changes discontinuously at the kink.

The fact that the subsidy program was announced in 1992 and the assignment variable is average EP from full-value contribution before 1992 makes it very difficult for workers to manipulate the system. It is very unlikely that an individual sort themselves to one side of the kink. Furthermore, Figure A2 has shown that the shape of the density is not unique for female subsidy recipients but rather a pattern that is common for all female workers in the pension system. The red squares show the distribution of female workers in the west which is bell-shaped and centers at the kink. The blue triangles show the distribution of male workers in the west, which is also bell-shaped, but centers at 0.6 EP to the left of the kink.

A second test for no manipulation is to check whether the slopes of the control variables are smooth around the kink. Table 2 presents the regression results in the form of Equation 4, where the changes in slopes of the predetermined covariates are estimated. I look at individual characteristics, such as the number of children, the age of first birth and age of first employment. Social economics status (SES) is also investigated. Months spend in unemployment insurance (UI), unemployment assistant (UA), childcare and sickness leaves before 1992 and before age 50 are tested for nonlinearity at the kink. The p values of all covariates are larger than 0.05. This suggests that the covariates evolve smoothly at the kink. Figure 5 visually shows the means of those covariates in each bin of aep_{92} and the slopes at two sides of the kink.

1.6 Effect of Subsidies on Labor Supply

1.6.1 Graphical Evidence

Figure 6 shows the relationships between aep_{92} and subsidy size, age claiming pension and hazard rate to claim a pension at age 60 around the kink. The bin sizes are the same as Figure 4. The estimated changes in slopes of the outcome variables obtained by reduced-form regressions are listed in the figures. There is a clear kinked relationship between aep_{92} and age claim pension. The slope becomes flattered at the left of the kink. Visually, we can see that additional pension benefits induce workers to claim pension earlier. Around the kink, the retirement age is 61.5 on average. If we assume retirement age decreases linearly as aep_{92} increases, the age claim pension would be 62 years old if the subsidies do not exist – that is the average pension claim age for workers 0.2 EP away from the kink. There is also a clear kinked relationship between aep_{92} and hazard to claim pension at age 60. The slope becomes flattered at the left side. At the kink, the hazard to claim pension at 60 is around 45%.

Figure 7 investigates the relationships between aep_{92} and age of exiting employment and hazard rate to exit employment at age 60. I define age exit employment as the age of the last job, including both regular jobs that contribute to the pension system and marginal employment. It suggests that the age of exiting employment doesn't show a clear change of slope around the kink. The change in slope of hazard to exit employment at 60 is relatively more sizable than age of exiting employment. It is consistent with the predictions made in the conceptual framework. If a large proportion of recipients would have left employment before age 60 in absence of the subsidies, then we expect to see a small impact of subsidies on the age of exiting employment. It is because the incentives are coming from pure wealth effect for those individuals. Two-thirds of recipients in the sample leave employment before age 60.

Therefore, it is not surprising that the change in slope of age to exit employment is nosier than that of pension claim age.

1.6.2 Effect of Subsidies on Age of Claiming Pension

In Table 3, I present fuzzy RKD estimates of the responses concerning the location of aep_{92} along with the first-stage estimates. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification of Equation 4 and Equation 5. In each column, I report the estimated change in slope of Y around the kink and the estimated change in slope of benefit level B around the kink. Here, subsidies are measured in 2010 euros, and the unit is €100 per month. The local average treatment effects are reported in row 3 as $\frac{dY}{dB}$. The standard errors are obtained using delta method.[20] Columns 1 to 3 measure the impacts on pension claiming age. Columns 4 to 6 measure the impacts on the hazard rate to claim pension at age 60. Columns 1 and 4 show results of linear regressions without controls. Columns 2 and 5 show results of linear regressions with controls, such as the number of children, the age of first employment and SES before 1992, etc. Columns 3 and 6 further add cohort fixed effects to the regression. The cohort fixed effects take into account incentive changes caused by raising the statuary retirement age, which was implemented gradually by cohorts. The average values of subsidy size, retirement age and hazard to claim at age 60 are also reported in Table 3. Recipients around the kink on average receive €108/month of subsidy, and they on average claim pension at age 61; the hazard to claim pension at 60 is 42%. The coefficients do not vary much across specifications. My preferred specification is the one with controls and cohort fixed effects. The RKD estimates suggest that an extra €100 of pension benefit per month makes the recipients retire earlier by 0.8556 years, which is around 10 months.

[20] I have also calculated standard errors using bootstrap method. The results are similar.

An extra €100 of pension benefit per month makes recipients 17% more likely to claim pension at age 60.

To better understand how the subsidies affect workers' labor market decisions at older ages, we further look at the hazard rate of claiming pension at different ages. Figure 8a plots estimated change of hazard to claim pension from age 50 to age 65, when there is a €100 increase of pension subsidy per month. Figure 9a plots the survivor curve in blue dots when there is a €100 increase of pension subsidy per month. I observe that most of the actions happen at statutory retirement ages - age 60, 63 and 65. It is reasonable given the institutional setting of the German pension system.[21] Subsidies increase the hazard of claiming old age pension at age 60 and 63 significantly and decrease the hazard of claiming pension at age 65 but the impact is not statistically different from zero.

1.6.3 Effect of Subsidies on Age of Exiting Employment

Individuals facing additional pension wealth potentially could also adjust their decisions to exit employment. In the sample, half of the recipients leave employment before age 60. In Table 4, we present fuzzy RKD estimates of impacts on age of exiting employment. The regression specifications are the same as Table 3. Consistent with the graphical evidence in Figure 7, the estimated impacts on age of exiting employment are not significant. The magnitude of RKD estimate of the impact on age of exiting employment is close to the impacts on pension claim age but much nosier. In responses to additional pension benefits, hazard to exit at 60 increased by 14% with a significance level of 0.05. Figure 8b plots the change of hazard to exit employment when there is a 100 euro more pension subsidy per month. It has a similar pattern as the impact on hazard to claim pension. Apart from hazard to

[21] The liquidity effects of public pension cause this kind of behavior.

exit at age 60, the hazard to exit at age 57 declines slightly when there is a higher pension. The impact is 2% at 10% significance level.

1.6.4 Bridge to Retirement

In Germany, it is common that older workers do not transit directly from full-time employment to retirement. On average, 43% of the female recipients enter to pension claiming via regular employment; 5% of them enter via marginal employment; 30% of them enter via unemployment. In this section, I investigate the impacts of having additional pension benefits on worker's activities during those bridge years. Table A7 shows that conditional on claiming pension benefits at time t+1, the probability of being in different activities at time t change as pension benefits change. The results suggest that workers are more likely to use UI as a pathway to early retirement when pension benefits are higher.

First, I investigate the impact on age of last regular jobs. Table A2 shows that the effects of pension subsidy on age of last regular jobs is noisy, but with a magnitude close to zero. Then, I examine the impacts on transitions from regular employment and from unemployment to other social economic statuses for women aged 50- 59. Table 5 displays estimates of impacts on the conditional probability to transit from employment. Conditional on participation in regular employment at time t-1, €100 more pension benefits increases the probability to transit from jobs to unemployment insurance by 0.84%. The probability to stay employed, transit to marginal employment or to other residual activities between age 50 to 59 are not statistically significant. Conditional on participation in unemployment insurance at time t-1, €100 more pension benefits per month increases the probability to transit from unemployment to other residual activities by 0.25%. The impacts on probability to transit to other status are not significant.

Additionally, given that workers claim pension earlier but do not change age exiting regular employment at a large magnitude, It would be interesting to understand how workers alter their behaviors during those bridge years. On average, the gap between the age of last regular employment and age of pension claim is around 8 years: 16 months in UI, 9 months in UA, 5 months in marginal jobs and 3 months in sickness and the rest time are coded as self-insured. Figure 10 plots the distribution of months spent in marginal employment and unemployment with respect to aep_{92}. Table 6 shows the regression results. The estimates suggest that €100 additional pension per month induces workers reduce the time spend in marginal jobs during the bridge years by about 4 months. It is reasonable because the gain of delaying pension claim for one period is relatively smaller for workers engaged in marginal jobs. The forgoing wage is lower and the time spent in mini jobs won't increase their pension entitlements. A wealth effect induce those workers to quit working earlier. €100 additional pension benefits per month induce workers to increase their time spent on UI by 4 months. However, the estimator is not significant. Combining this finding with the impacts on probability to enter unemployment between age 50 to 59 and impacts on probability to enter pension via unemployment, I infer that overall additional pension benefits induce workers to bridge via UI earlier. Workers are more likely to use UI as a pathway to retirement and they stay in UI longer.

1.6.5 Interaction with Pension Reform

The early retirement pathway through old age pension for women stayed at age 60 for our sample. The full retirement age without any actuarial adjustment has increased from 60 to 65 by cohorts in monthly steps. In other words, beginning with cohort 1940 January, there is a penalty for claiming old age pension for women at age 60. The penalty to retire at age 60 increases gradually from 0 up to 18% in monthly steps from cohort 1941 to 1944. I am interested in the impacts of the penalties for claiming pension early on the results. For the affected cohorts, the penalty essentially discounts the value of each euro of pension benefit. Therefore, I expect to see a smaller effect on retirement age for the younger cohorts who face early retirement penalties. I separate the sample into three groups: 1) group with no penalty: cohort 1935 to 1940; 2) transition group: cohort 1940 to 1944; 3) maximum penalty group: cohort 1945 to 1951. Table A5 shows the results. The hazard rate to claim pension at age 60 is slightly smaller for the younger cohort and insignificant. It is consistent with the fact that each unit of subsidy is worth less if the pensioners choose to claim pension at 60. On the contrary, the impact of subsidies on retirement age is larger for younger cohorts.

1.6.6 Heterogeneity and Robustness

Heterogeneous Behaviors

In this section, I look at heterogeneous responses for subgroups by pension subsidy size, health status, and family attachment. Table A3 shows estimates for recipients with higher than average subsidies and recipients with lower than the average subsidies. The regression results suggest that the impacts are only significant for workers whose subsidies are higher than the average, which is €80 per month. It is partly due to the fact that the change in slope of subsidy size around the kink is not statistically significant, for workers with lower than the average subsidy. However, the difference between the impact for high and low subsidy groups is not statistically significant. The main factor that changes the magnitude of subsidies along aep_{92} is years worked before 1992. Table A3 also shows estimation results when we separate the sample into groups with more years worked before 1992 and fewer years worked before 1992. The results are similar as separating the sample by subsidy size. The results suggest that when subsidy size is lower than a certain threshold, workers' labor supply is not responsive. It would be interesting to measure the continuous impact of subsidy on retirement behavior.

Health status is a key factor that affects retirement decisions. Poor health makes it hard to stay in employment and makes workers more likely to claim pension earlier. Workers with poor health also value leisure more. I proxy healthiness using a dummy for never spending any time on sickness leave before age 50. The estimation result suggests that unhealthy workers claim pension earlier by around 1 year and healthy workers claim pension earlier by around 8 months. The difference in impacts on pension claim age, however, is not statistically significant. The difference in estimated impacts of hazard to claim pension at 60 of those two groups is significant at 10% level.

Lastly, I separate women by the number of children. It is because the labor force attachment of women is largely affected by their child-bearing activities. Mothers with more children are less likely to be strongly attached to employment. I expect mothers with more than one child are more responsive to additional pension income. Row 4 in Table A3 confirms this hypothesis. The impact of additional income to women with no child or with only one child is an order of magnitude smaller than the impacts to mothers with more than one child.

Estimates by Polynomial Order and Bandwidth

Several exercises further establish the robustness of the estimates. Table A4 reports the results of the estimation of equation 4 for a linear, a quadratic, and a cubic specification. For all three specifications, bandwidth is 0.2 around the kink, same as all baseline analyses. Aikake Information Criterion (AIC) and Bayesian information criterion (BIC) and AIC with a correction are reported as well. The estimates are quite sensitive to polynomial orders; however, the difference among AIC, BIA and AICc are small across specifications. According to those criteria, the linear specification fits slightly better than the other two specifications. One explanation for the sensitivity to polynomial order could be that high-order polynomial regression takes on extreme values to the weights. Gelman and Imbens [2017] suggests that high-order polynomial regression is a poor choice in regression discontinuity analyses. For causal inference, they recommend local linear or quadratic polynomials for RD design. In the case of regression kink design, Card et al. [2017] have shown that the quadratic estimator is typically larger than the mean squared bias for the linear estimator with the same bandwidth selection and bias correction. In this paper, the mean squared bias, obtained by Monte Carlo simulations based on data generating process that closely resemble the sample, also suggests that linear specifications dominate quadratic models.

Figure 9 shows the point estimates and 95 percent confidence intervals for the effect of a €100 increase in monthly pension benefits on age of claiming a pension and hazard rate to claim pension at age 60. All the estimations use the linear specification with controls and cohort fixed effect. The blue dotted line shows the number of observations. The four red vertical dash-dot lines correspond to four different bandwidth selections: the Imbens and Kalyanaraman [2012] bandwidth for fuzzy RKD (Fuzzy IK), the bias-corrected estimates per Calonico et al. [2014] (Fuzzy CCT), the "rule-of-thumb" bandwidth based on Fan et al. [1996] (FG), and the baseline bandwidth used in the baseline analysis. The four bandwidths are 0.114, 0.102, 0.28 and 0.2, respectively. Even though 0.1 is the optimal bandwidth suggested by both Fuzzy IK and Fuzzy CCT , the result is compromised by the small sample size at this bandwidth. I find that the results are significant and relatively stable over bandwidths between 0.125 and 0.25, which is equivalent to €325 to €650. Once the observation number falls below 3000, the results are very sensitive to the choice of bandwidth.

Placebo Kinks and Placebo Forcing Variable

As Card et al. [2015c] and Landais [2015] point out, one main concern with the RKD identification assumptions is the functional dependence between the assignment variable and the outcome variable. In order to further test that the estimated impact on pension claim age is not caused by the quadratic functional form but by the kinked schedule in subsidy, I run some placebo tests. First, I test for existence and location of the kink. Figure A3a shows the R-square and adjusted R-square of the baseline model when the kink is placed at "placebo" locations around the kink. Following Landais [2015], I run regressions of Equation 4 for a series of virtual kink points and look for the kink that maximizes the R-square. In Figure A3a, we can see that both R-square and adjusted R-square increase sharply as one moves closer to the actual kink point and then decrease when moves away from the kink point. I also perform a permutation test in the spirit of Ganong and Jäger [Forthcoming]. Figure A3b shows that the estimate with the kink placed at the actual kink point is statistically significantly larger in magnitude than the distribution of estimates with placebo kinks.

Moreover, I use average EP after last employment as a placebo forcing variable instead of aep_{92}. The average EP after last employment is a good proxy for lifetime earnings but not directly correlated with aep_{92}. Figure A4 shows scatter plots using mean EP 5 years after last regular employment, with 4 years, 3 years, 2 years and 1 year after last regular employment as the placebo forcing variables. Figure A4 suggests that the subsidy amount and outcome variables have no obvious kinked relationship with the placebo forcing variables. Table A5 presents the RKD results by using those placebo forcing variables. It shows that none of the $\frac{dY}{dB}$ estimates are significant across all the placebo specifications.

Lastly, I use female recipients in West Germany who credited less than 35 years to the pension system as the counter-factual sample. Those female workers are not eligible for the subsidy. Figure A5 shows scatter plots of age claiming pension and hazard to claim pension at age 60 using the counter-factual sample. It shows that without the subsidy the slope of the age claiming pension doesn't change at the kink.

1.7 Implications and Discussions

The primary objective of this subsidy program was to provide additional income support to older workers at retirement. However, as I mentioned earlier, this program is being phased out gradually. Low-income workers who never contributed to the pension system before 1992 won't benefit from this subsidy program. The average subsidies of female workers in West Germany declined from €33 /month for cohort 1935 to €20 /month for cohort 1948. Over time, the average subsidy size also decreased from €50 /month to below €10 /month from 1996 to 2014. The red dashed line in Figure 11a and Figure 11b shows these declining patterns. From a policy perspective, It would be interesting to know what the retirement age of female workers would be if the subsidy amount stays at a high level. In other words, how much the phasing out of this subsidy program account for the increase in pension claim age for female cohorts between 1935 and 1948, and for female workers from year 1996 to 2014. The blue dash-dot line in Figure 11a and Figure 11b displays the profile of actual retirement age for female cohorts between 1935 and 1948, and for female workers from year 1996 to 2014. The RKD estimates suggest that a one-euro increase of pension benefit induces workers to claim pension earlier by 0.00856 years, which is around 3 days. Based on this estimate, I draw the profile of retirement age if the subsidy level remained at the average level of the 1935 cohort in Figure 11a;

and if the subsidy level remained at the average level of year 1996 in Figure 11b. The corresponding changes in retirement age are shown as the grey area between the black solid line and blue dash-dot line. We can see that the phasing out of subsides can account for some of the increasing trend in age of claim pension over the past decade.

1.7.1 In Comparison with other studies

It would be useful to compare the magnitude of the estimates in this paper with other studies. Due to the limitations of the data set, I could not take the family structure, spouse income, and other income sources into consideration. Husbands and wives may determine their labor supplies jointly. Wives who have access to husbands' income and other income sources are less responsive to the availability of subsidy, and vice versa. It needs to be taken into account when interpreting the estimation results. If we assume that the household income is in a range of 1 to 3 times of the female recipient's income, then we obtain an estimate of the elasticity of retirement age with respect to income in a range of -0.025 to -0.008. That is, a 1% increase of pension income decreases pension claim age by 0.08% to 0.25%. And the elasticity of hazard rate to enter old age pension at age 60 with respect to pension income is in a range of 0.075 to 0.025. That is, 1% increase of pension income increases the hazard to claim pension at 60 by 0.025% to 0.075%.

Compared to the elasticities measured using incentive changes caused by actuarial adjustments (Hanel [2012], Engels et al. [2017]) , the estimate in this paper is much smaller in magnitude. For example, Hanel [2012] reports a semi-elasticity of propensity to enter disability retirement with respect to the implicit tax rate 2.10. Engels et al. [2017] find retirement age increases by 6.5 months when facing a 18% penalty to retire at age 60. Regarding the impacts of income transfer due to wealth/income effect (Costa [1995], Marie and Castello [2012], Gelber et al. [2017b]), the magnitude of results in this paper is closer to the size of findings in those papers. For example, Marie and Castello [2012] measures labor supply response to a 36% increase in the generosity of disability insurance and find the labor force participation rate declines by 8%. Gelber et al. [2017b] find the annual employment rates decrease by 1.3% per $1000 of additional lifetime DI benefits. Assuming the pension recipients have 20 years of pension duration, I translate the increases of 100 euro per month to an increase of 24,000 in lifetime income. This result suggests that retirement age decreases by 0.4 months per €1000 of additional lifetime pension income and the hazard to claim pension at age 60 by 0.7%.

1.7.2 Fiscal implications

Policymakers are interested in the fiscal implication of providing one additional euro of pension benefit per month per worker. Here, I separate the fiscal cost into two parts, mechanical cost (MC) and behavioral cost (BC). The ratio of behavioral cost to mechanical cost (BC/MC ratio) is a context-robust measure of disincentive cost, which helps to compare the disincentive effect of this pension subsidy to low pay workers with other redistribution programs.

The mechanical cost would represent the increases in government spending if there were no behavioral responses. If we assume the duration of pension benefit to be 20 years, which is the length of the period between retirement and death, then the mechanical cost for one additional euro of pension benefit per month is €240 per worker. The behavior cost can be broken into four parts. The first part is the increase in pension benefit payment due to the change in age claim pension. Take the average monthly pension €600 as the baseline – the government will pay €60 per worker for the induced 3 days of the earlier claim. The second part is the increase in UI benefit payment caused by the change in UI claim behavior. Recall that one additional euro of pension benefit per month increases the duration spent in unemployment between exiting regular employment and claiming pension by around 1.3 days. Take into account that average daily UI benefit is roughly 67% of the average wage, 1.3 more days in UI increases government spending by €26. Additionally, on average, 30% of the subsidy recipients bridge retirement via UI/UA. Therefore, the expected value of the second part of the behavioral cost is €8. The third part is the decrease in revenue due to less contribution to the public pension system. The reduction in contribution comes from two sources: first, a change in age of exiting regular employment, second, a change in time spent in UI. The change in contribution due to change in regular employment is close to zero. This is because the impact on age of exiting regular employment has a magnitude close to zero (Table A2). The change in contribution due to change in time spend in UI is €8 multiplied by pension contribution rate 18%. The third part of the behavioral cost is approximately equal to €1.5. The last part is the decrease in revenue due to decline in taxation. This part of behavior cost is close to zero. It again is because the impact on age exit regular employment has a magnitude close to zero. To sum up, the total behavioral cost for one additional euro of pension benefit per month is around €70 per worker.

The resulting BC / MC under the assumption made above is approximately 0.3. It implies that in order to increase the lifetime income of the low-income pensioners by 1 euro, 1.3 euros have to be raised by the government, either via taxes or pension contribution.[22] Compared to the BC / MC ratios of other anti-poverty programs such as unemployment benefits and income tax credit, the BC / MC ratio of providing additional pension benefit to low income workers is smaller. For instance, Schmieder

[22] If we only take the fiscal cost on the public pension system, then BC is €62, MC is €240, the BC/MC ratio is 0.26.

and von Wachter [2017] report an average BC/MC ratio of UI benefit extensions of 1.35.[23] Saez et al. [2012] also define the a term in the same spirit as the BC/MC ratio - "marginal efficiency cost of funds" [24]. Saez et al. [2012] report an average BC/MC ratio of raising top tax rate of 0.76. Using the formula in Saez et al. [2012] and Saez [2001], I calculated the BC / MC ratio of a tax cut for low income workers using an elasticity estimate of 0.5 (Eissa and Hoynes [2006]).[25] The BC/MC ratio of a tax cut for low-income workers is approximately 0.75. The BC / MC ratio suggests that compared to other anti-poverty programs that aim to redistribute income to workers in danger of old age poverty, the pension subsidy program has a relatively small disincentive effect.

While the BC / MC ratio expresses the fiscal costs of increasing pension benefits, it is difficult to provide the welfare implication of the pension subsidy program. The social value of increasing pension benefit by €1 depends on the gap between the marginal utility of subsidy recipients relative to the marginal utility of other pension contributors. Evaluating the social value is beyond the scope of this paper. Additional lifetime income can change the marginal utility from many perspectives. One potential positive impact of additional lifetime income, for example, is the increase in life expectancy. Snyder and Evans [2006] use an exogenous cut in Social Security benefits in the US for the notch cohorts to identify the causal impacts of income on

[23]This is obtained under the assumption that nonemployment affects the social planner's budget by both income tax and UI payroll tax. If only the UI payroll tax is considered, BC/MC ratio of UI benefit extensions is on average around 0.35.

[24] The "marginal efficiency cost of funds" is expressed as $1 - \frac{BC}{BC+MC}$

[25]Consider a small tax cut $d\tau > 0$ for income below z^*, the mechanical decrease in tax revenue is $(z^* - z)d\tau$, and the behavioral responses is increase in tax revenue. $BC = \frac{\tau}{1-\tau} * \epsilon * z * d\tau$. Therefore, the BC/MC ratio is $\frac{\tau}{1-\tau} * \epsilon * \frac{z}{z^*-z}$. In Germany, the first €677 earned each month by a single worker is tax-free. Afterwards, the income tax increases from 14 % to 42 % incrementally. Let's assume z^* to be €1200 per month, and mean income of workers earn less than €1200 per month and above €677 per month is €900. The tax rate for low-income workers before a tax cut τ is assumed to be 20%. Plugging those numbers and the estimated labor supply elasticity in Eissa and Hoynes [2006] (0.5), we obtain the BC/MC ratio of a tax cut for low-income workers to be 0.75

mortality. They have found the notch cohort, who faced approximately $50 cut in pension per month, have a statistically significant 2% higher mortality. Additionally, in Germany, long-term health care is a part of the pension entitlement; claiming pension earlier could improve the longevity of workers who frequently experience health shocks (Coile [2004b]). Therefore, it is important to keep in mind that the subsidy recipients of this program might have a longer and healthier life after retirement due to additional pension income.

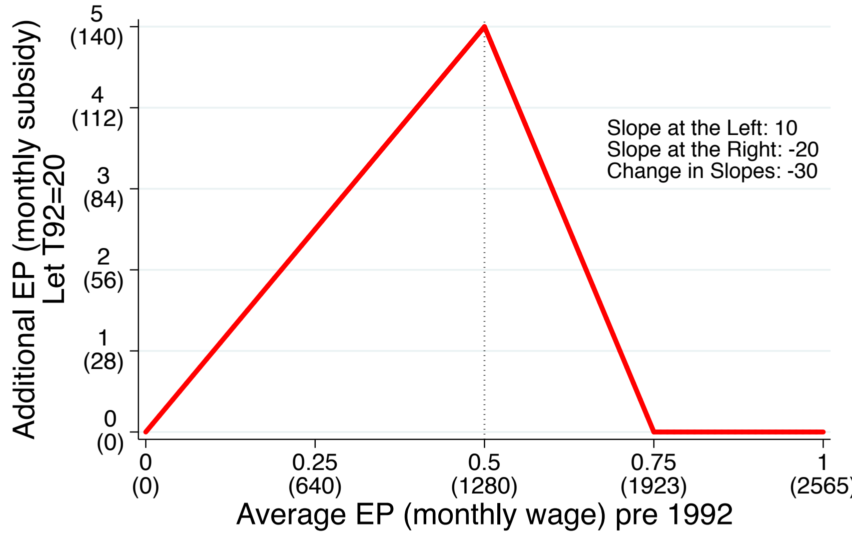
1.8 Conclusion

Facing population aging and financial challenges from the public pension system, many governments have reformed their public pension schemes to encourage longer working life. Those pension reforms, however, typically affect low-income workers disproportionately and widen pension income gap. As the issue of old age poverty surfaces, policymakers start to consider income support programs for the lowest wage earners to ensure adequate income in old age. This paper provides a clear and transparent setting to study the effect of additional pension benefits for low wage female workers. I explore a specific feature of the German pension system, which allows me to identify the effect of additional pension benefits on retirement decisions in an environment in which the statutory pension eligibility age is unchanged.

Using administrative pension insurance records from Germany, I investigate the impact of the impact of a pension subsidy program for low pay workers, implemented in 1992, on retirement decisions of the female recipients. I exploit the very sharp kink in the schedule of benefits to implement a regression kink design. I found that a €100 increase of monthly pension benefit induces workers to retire earlier by about 10 months and the hazard rate to claim a pension at age 60 increases by 17%. The impact on age of exiting employment have the similar magnitude but is noisier. The hazard rate to exit employment at age 60 increases by 14%. Additionally, conditional on participation in regular employment at time t-1, €100 additional monthly pension benefits increases the probability to transition from jobs to unemployment insurance by 0.84%. And it increases the conditional probability to transition from unemployment to non-activities between age 50 to 59 by 0.25%. Moreover, I look at months spent in marginal jobs after exiting regular jobs until claiming a pension. A €100 increase of pension benefit per month reduces the time spent in marginal jobs by about 4 months. The main policy implication of this paper is that while an income transfer to pension benefit to low-income workers induces early pension claim, it has little impacts on the probability to exit regular jobs, which have mandatory social security contribution obligations. Therefore, this pension subsidy program I study in this paper has little impacts on contributions to the public pension system. A back-of-the-envelope calculation suggests the ratio of behavioral cost to mechanical cost of this subsidy program is 0.3, which is smaller than other anti-poverty programs such as extending unemployment benefits and progressive taxation.

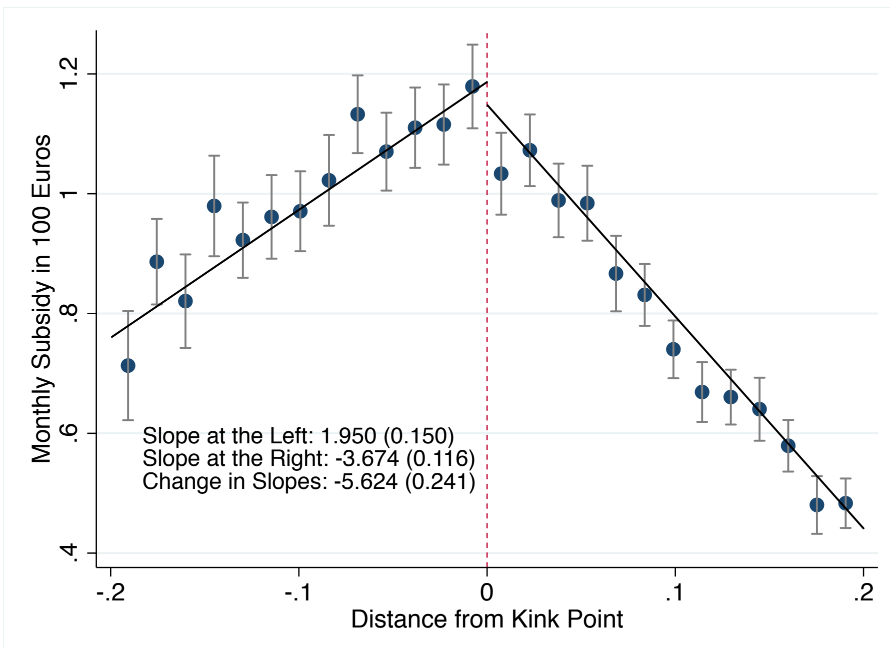
Although the findings of this paper are relevant to understanding the impact of pension benefits on retirement timing, the policy solutions to old age poverty require more studies. As this pension subsidy for low workers phases out, will the potential recipients be better off or worse off under the new parental pension credits policy? Could the existing means-tested basic income program independent of age be a sufficient safety net for low-income workers? Those questions are beyond the scope of this paper. Additionally, it would be interesting to investigate the impact of additional pension benefits on the mortality rate or the private saving patterns. However, the data set does not allow me to explore the consequences regarding those outcomes. Nonetheless, the causal estimates in this paper will help policymakers to evaluate recommendations aiming at alleviating old age poverty.

Figure 1·1: Subsidy Size as a Function of Average Monthly Earning Points before 1992



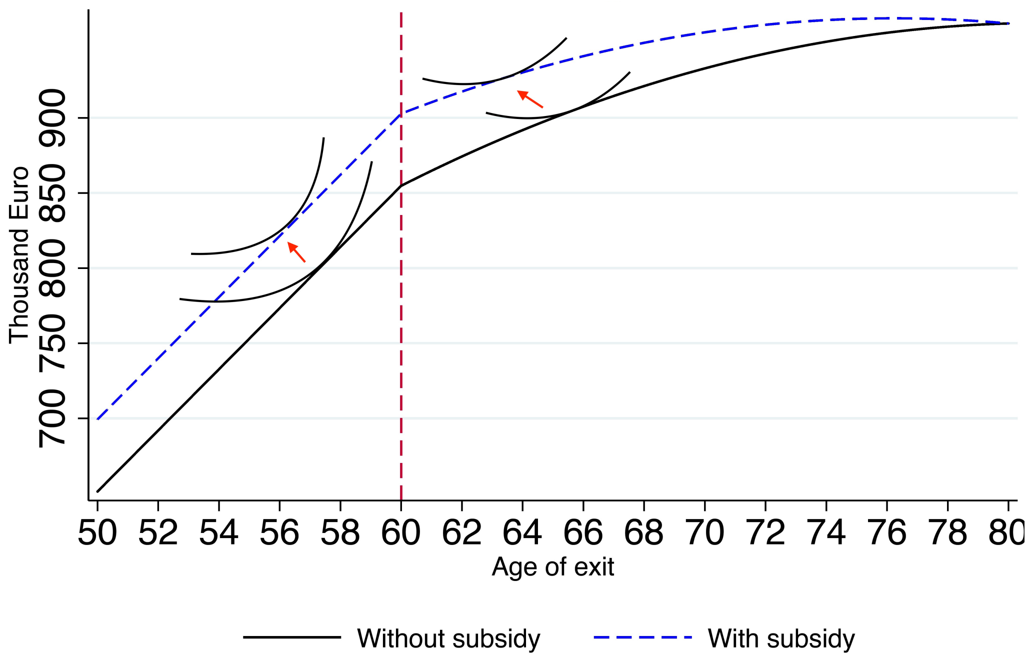
Notes: Figure 1 plots the subsidy size for recipients who have contributed for 20 years before 1992. The subsidy size is measured in earnings points. The average year worked before 1992 of the baseline sample years is 20 years. The theoretical slope of total subsidy measured changes from 10 to -20.
Source: Author's own construction according to SGB VI § 262

Figure 1·2: First Stage: Observed Subsidy Schedule



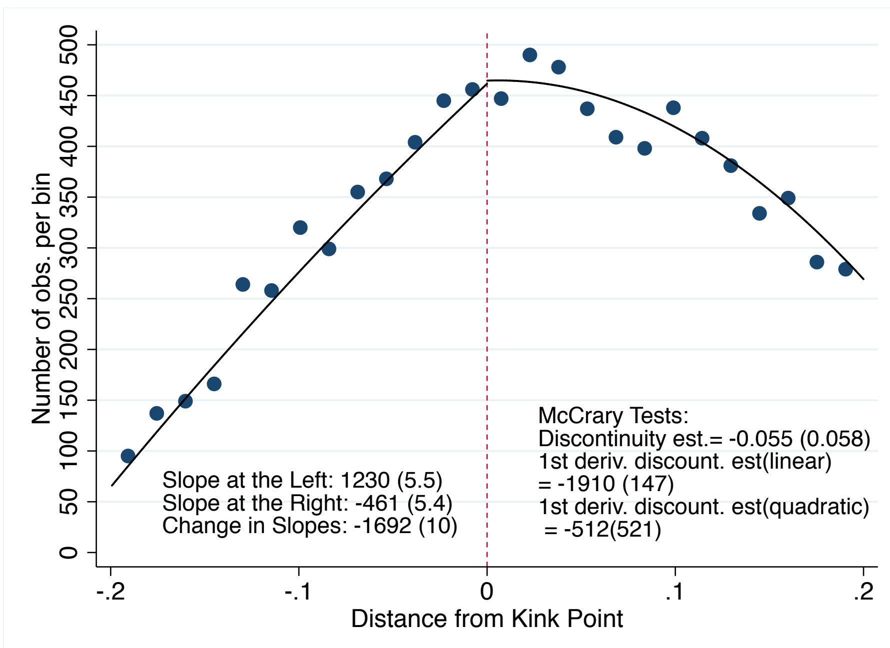
Notes: Figure 2 plots the observed subsidy size for the recipients. It shows that the relationship between aep_{92} and subsidy size is consistent with the policy schedule in Equation 1. The monthly subsidy is measured in 100 euros. The reduced form regression without controls reports an estimated change in slopes of subsidy around the kink of -5.6. The corresponding slope change when subsidy is measured in earnings points is -19.9, from 6.9 to -12.9.

Figure 1·3: Illustration of lifetime budget constrain



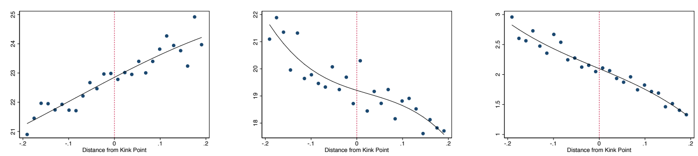
Notes: Figure 3 plots the lifetime budget constraint. The black solid line is the lifetime budget constraint of workers who are not subsidy recipients. The blue dashed line is that of recipients. Here I make the assumption that if workers exit employment before age 60, they will claim a pension immediately when they are 60.
Source: Author's own construction

Figure 1·4: Density around the Kink

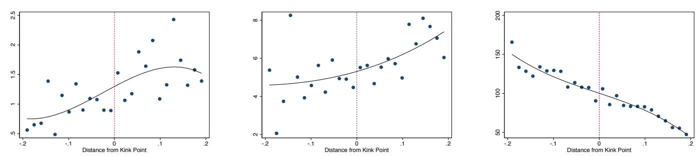


Notes: Figure 4 shows the density bin plot of aep_{92} in 0.05125 ($\sim$ 40 €in 2010) bins as a function of distance to the observed kink point.

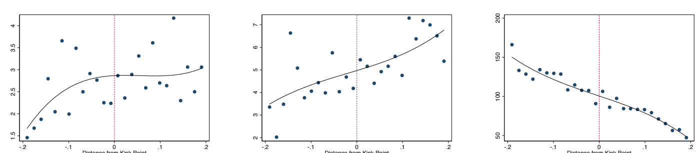
Figure 1·5: Predetermined Covariates around the Kink



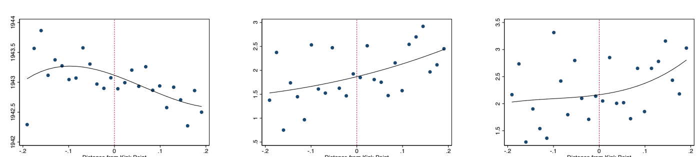
(a) Age of first birth (b) Age of first employment (c) Number of children



(d) Months of UI before 92 (e) Months of UA before 92 (f) Months of childcare before 92



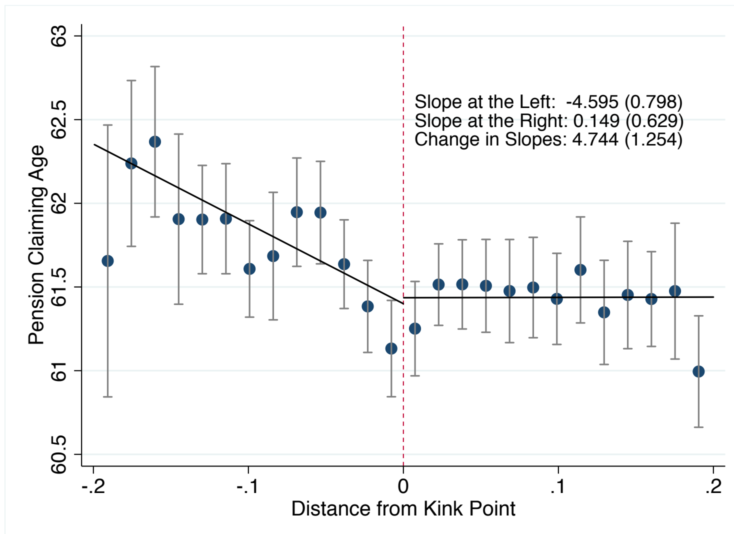
(g) Months of UI before age 50 (h) Months of UA before age 50 (i) Months of childcare before age 50



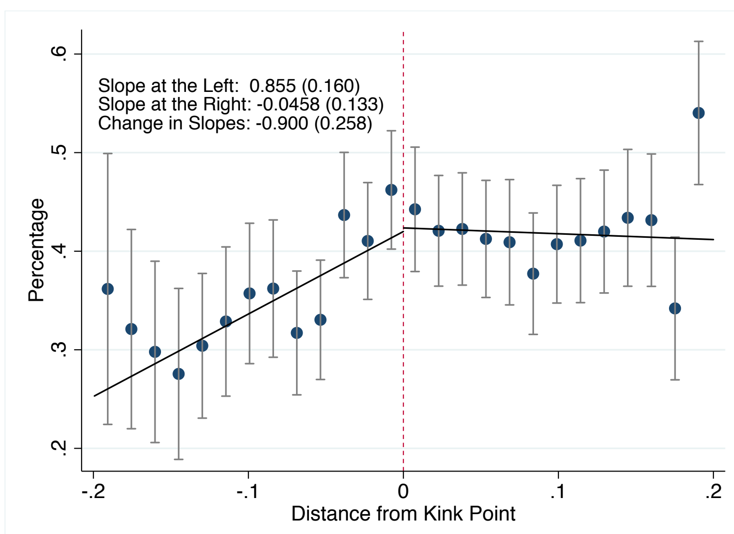
(j) Birth cohort (k) Months of sickness before 92 (l) Months of sickness before age 50

Notes: Figure 5 shows the scatter bin plots of aep_{92} in 0.05125 ($\sim$ 40 €in 2010) bins as a function of distance to the observed kink point for the predetermined covariates. These distributions are smooth around the kink. Table 2 has listed the p-values for changes in slopes of covariates around the kink.

Figure 1·6: Scatter Plots of Age of Claiming Pension around the Kink



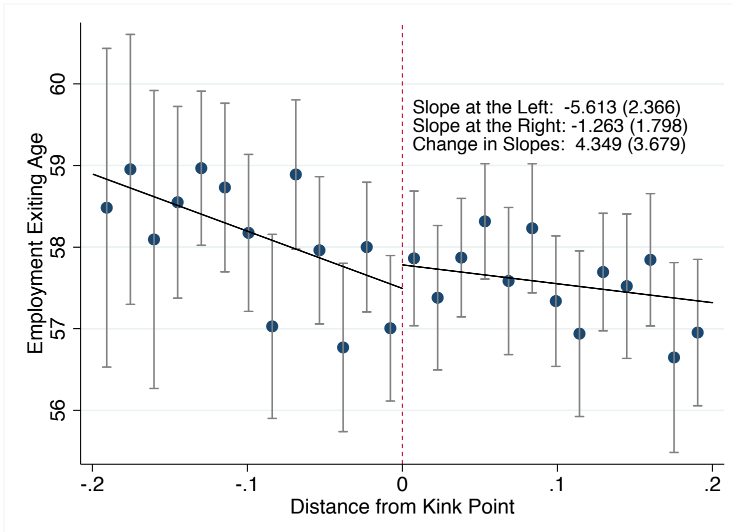
(a) Bin plots: Age of claiming pension



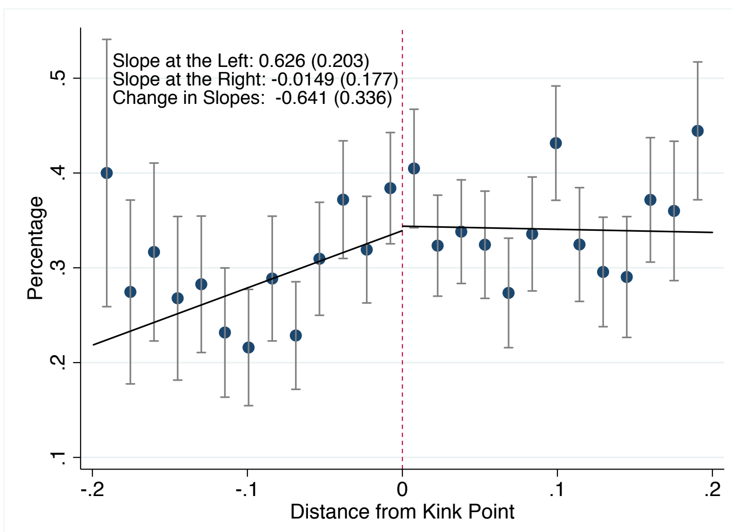
(b) Bin plots: hazard to claim pension at age 60

Notes: Figure 6 shows the scatter bin plots of aep_{92} in 0.05125 ($\sim$ 40 €in 2010) bins as a function of distance to the observed kink point for the main outcome variables: age of claiming a pension and the hazard rate to claim a pension at age 60. The black solid lines are the linear fitted lines. The reduced-form regression results without any controls are reported in the figure.

Figure 1·7: Scatter Plots of Age of Exiting Employment around the Kink



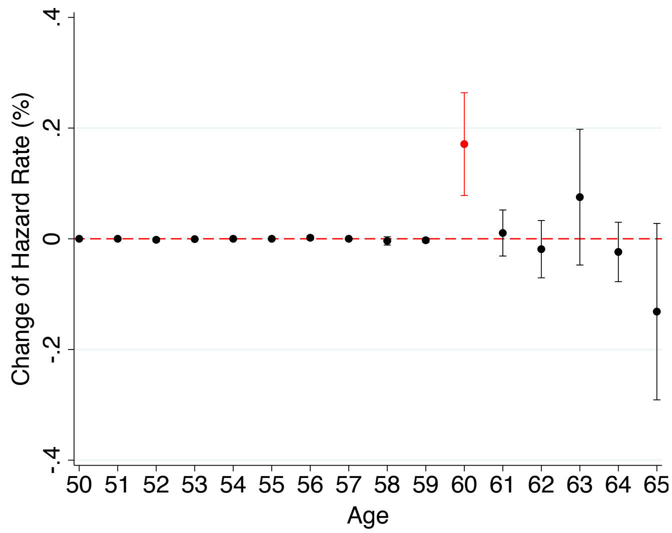
(a) Bin plots: age of exiting employment



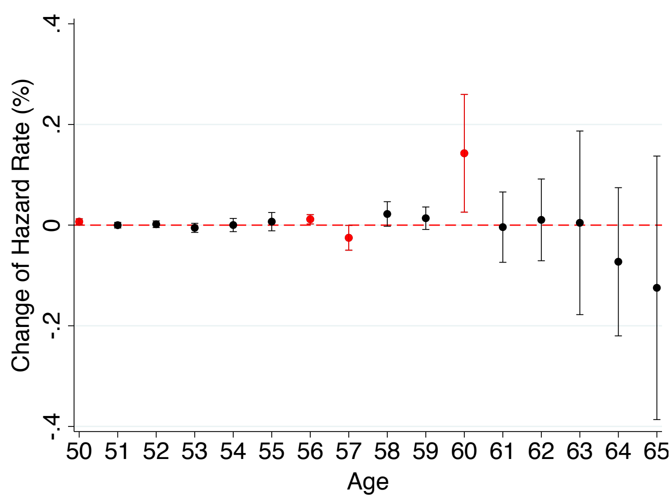
(b) Bin plots: hazard to exit employment at age 60

Notes: Figure 7 shows the scatter bin plots of aep_{92} in 0.05125 ($\sim$ 40 €in 2010) bins as a function of distance to the observed kink point for the main outcome variables: age of exiting employment and the hazard rate to exit employment at age 60. The black solid lines are the linear fitted lines. The reduced-form regression results without any controls are reported in the figure.

Figure 1·8: Hazard analysis from Age 50 to age 65



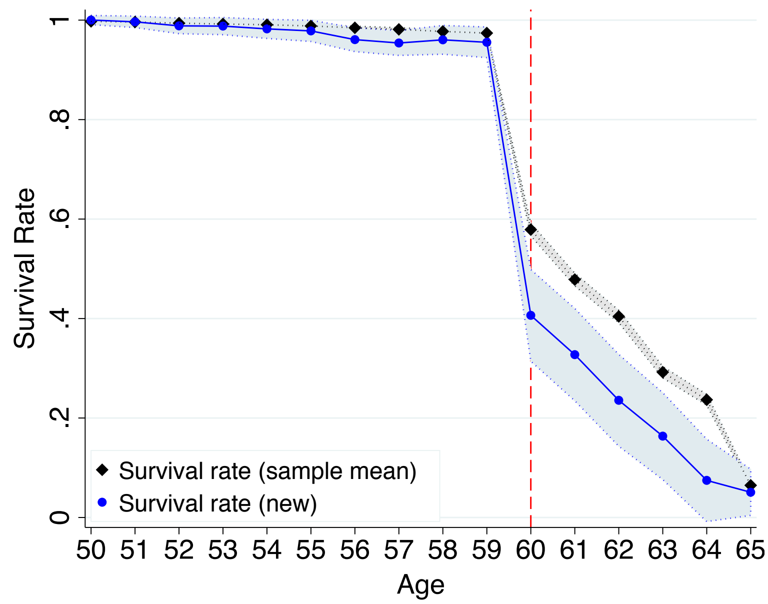
(a) Change of hazard to claim pension with 100 euro pension subsidies



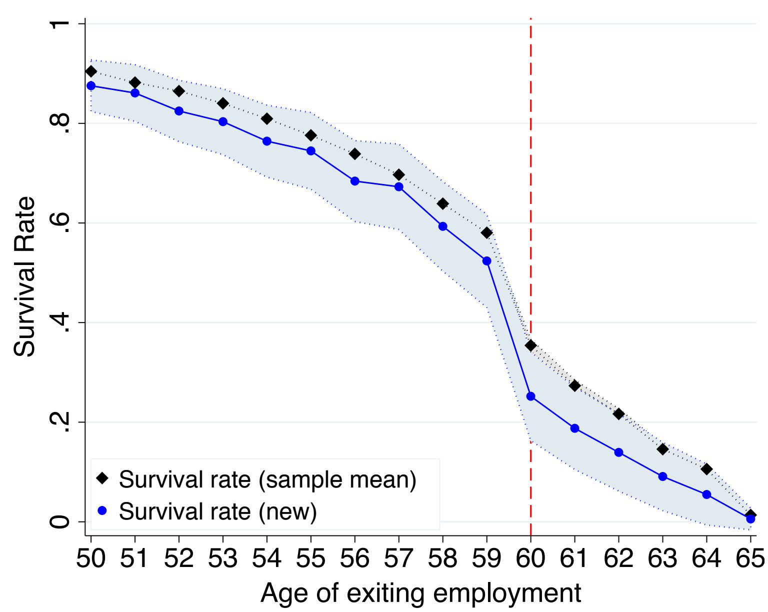
(b) Change of hazard to exit employment with 100 euro pension subsidies

Notes: Figure 8 shows the estimated percentage change of hazard rate to claim a pension and the estimated change of hazard rate to exit employment at ages from 50 to 65 when there is an increase of pension benefit by €100 per month.

Figure 1·9: Survive analysis from Age 50 to age 65



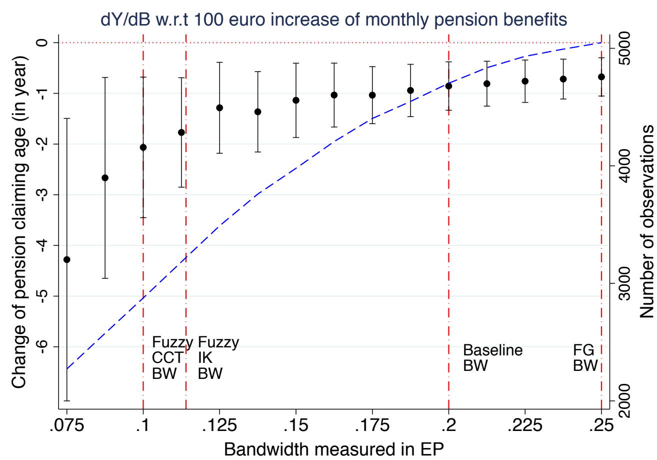
(a) Change in survirval rate in terms of age of claiming pension



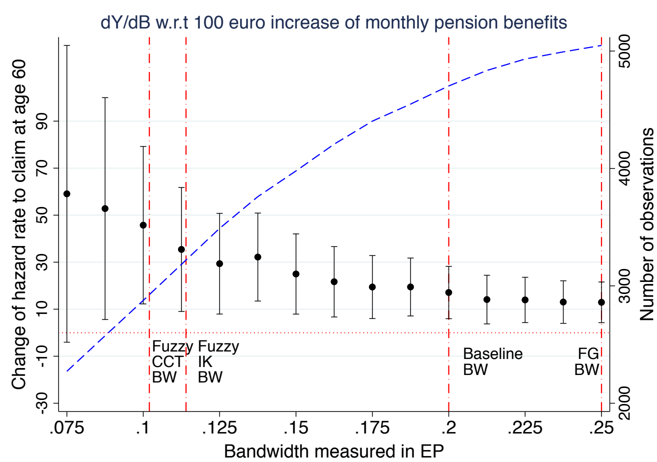
(b) Change in survirval rate in terms of age of exiting employment

Notes: Figure 9 shows the estimated percentage change of hazard rate to claim a pension and the estimated change of hazard rate to exit employment at ages from 50 to 65 when there is an increase of pension benefit by €100 per month.

Figure 1·10: RKD estimates by bandwidth



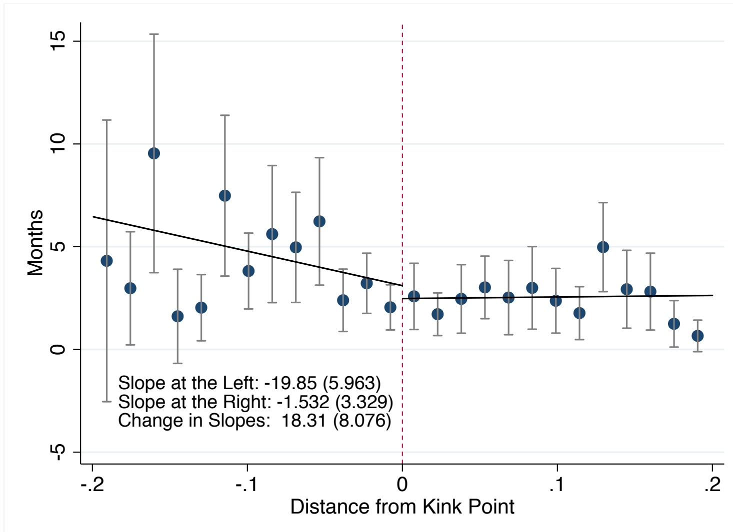
(a) Retirement age



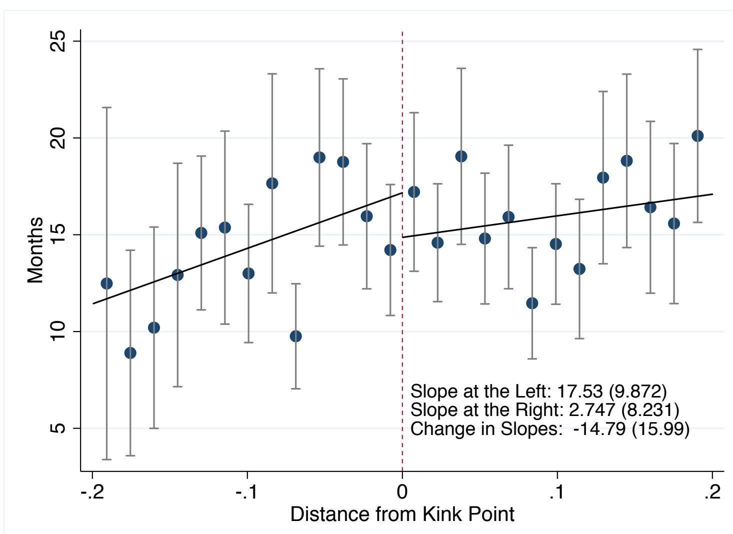
(b) Hazard to claim pension at age 60

Notes: Figure 10 shows the point estimates and 95 percent confidence intervals (on the y-axis) for the impact of a €100 increase in monthly pension benefits on age claiming pension and hazard rate to claim a pension at age 60. The estimations are obtained using linear specifications with controls and cohort fixed effect. The four red vertical dash-dot lines correspond to four different bandwidth selections: the Imbens and Kalyanaraman [2012] bandwidth for fuzzy RKD (Fuzzy IK), the bias-corrected estimates per Calonico et al. [2014] (Fuzzy CCT), the "rule-of-thumb" bandwidth based on Fan et al. [1996] (FG), and the one used in the baseline analysis. Those four bandwidths are 0.114, 0.102, 0.28 and 0.2, respectively. They correspond to 260, 295, 517, 647 euros per month. The number of observations is shown by the blue dotted line. The figures suggest that the point estimator becomes robust to bandwidth selection when the number of observation exceeds 3000.

Figure 1·11: Scatter Plots of Bridge Activities around the Kink



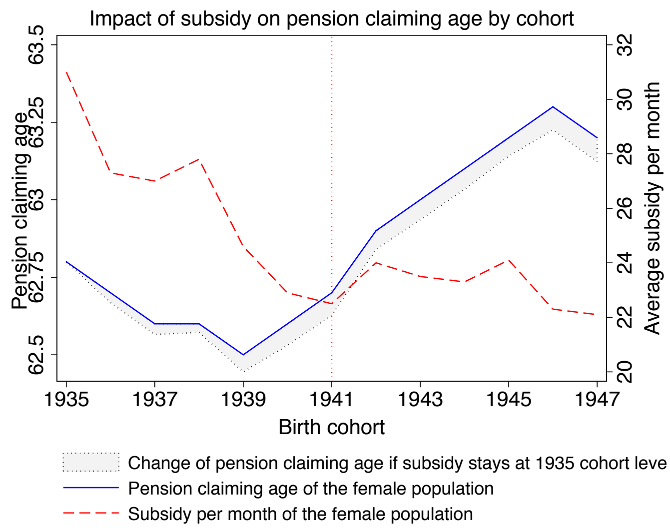
(a) Bin plots: months in marginal employment



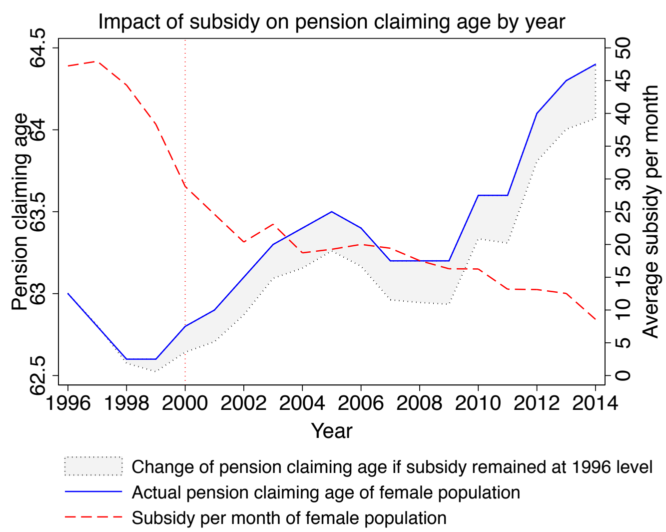
(b) Bin plots: months in unemployment

Notes: Figure 11 shows the scatter bin plots of aep_{92} in 0.05125 ($\sim$ 40 €in 2010) bins as a function of distance to the observed kink point for the outcome variables: months spent in marginal employment and months spent in unemployment during the bridge years. The black solid lines are the linear fitted lines. The reduced-form regression results without any controls are reported in the figure.

Figure 1·12: Policy Implications



(a) Change of pension claiming age by cohort



(b) Change of pension claiming age by year

Notes: Figure 12 shows the counter-factual retirement age for female workers if subsidy size remained at the same level as the 1935 cohort and in 1996, respectively. The average subsidy size is calculated by the author using VSKT data. The pension claim ages for female workers in West Germany by cohort and by year are obtained from the report "Rentenversicherung in Zeitreihen (Pension insurance in time series)"

Table 1.1: Summary Statistics

	Baseline sample			Around kink		
Variables	Mean	s.d.	N	Mean	s.d.	N
Subsidy related characteristics						
Subsidy in EP	3.19	1.77	5218	3.84	1.92	1720
Subsidy in Euro/Month	90.29	50.31	4994	108.74	54.26	1643
Subsidy Share	17%	1%	5218	20%	1%	1720
Years worked before 92	19.74	6.65	5218	19.43	6.46	1720
Mean annual EP	0.55	0.11	5218	0.53	0.08	1720
Mean annual EP pre92	0.47	0.1	5218	0.45	0.03	1720
Mean wage pre92	1228	255	5218	1169	75	1720
Wage before pension claim	899	768	4416	872	723	1456
Mean wage 1 year before last regular employment	1350	543	4787	1302	496	1580
Pension related characteristics						
Total Pension benefits	673	189	4994	665	179	1643
Total EP	24.11	6.75	5218	23.83	6.41	1720
EP from contribution periods	16.58	5.24	5218	15.76	4.35	1720
EP from contribution periods pre92	9.58	4.22	5218	8.80	3.03	1720
EP from consideration periods	20.91	5.74	5218	20.73	5.32	1720
Pension years	41.64	3.8	5218	41.64	3.82	1720
Contribution years	32.4	6.44	5218	32.09	6.26	1720
Consideration years	6.28	4.53	5218	6.57	4.36	1720
Yrs of full-value contribution	30.34	7.42	5218	30.12	7.3	1720
Yrs of full-value contribution pre92	19.43	6.46	5218	19.08	6.37	17207
Outcome variables						
Age of claiming pension	61.57	2.33	4994	61.47	2.29	1643
p60	0.38	0.48	5218	0.40	0.49	1720
p63	0.06	0.23	5218	0.06	0.24	1720
p65	0.19	0.39	3886	0.18	0.38	1259
Age of exiting regular employment	56.83	7.45	5218	56.71	7.58	1720
Age of exiting employment	56.83	7.45	5218	56.71	7.58	1720
Individual characteristics						
Number of kids	2.03	1.08	5218	2.1	0.99	1720
Age of first employment	19.18	5.51	5218	19.25	5.57	1720
Age of first birth	22.95	3.72	4912	22.83	3.61	1720
Birth Cohort	1943	3.75	5218	1943	3.74	1720

Table 1.2: Changes in Slopes of Covariates around the Kink

Covariates	Coeffi.	s.d.	p-values	mean at kink	s.d.
Fixed Characteristics					
Number of kids	-0.551	(0.470)	0.241	2.00	(1.06)
Age when having 1^{st} child	-0.450	1.712	0.793	22.90	(3.73)
Age of first employment	4.990	2.570	0.052	19.03	(5.37)
Years of consideration periods	-0.803	(2.008)	0.689	6.21	(4.51)
Durations of SES before 1992					
Months of UI	1.493	(1.724)	0.387	1.311	(4.027)
Months of UA	10.39	(6.176)	0.093	5.726	(13.68))
Months of Childcare	-4.097	(28.03)	0.884	94.62	(62.99)
Months of Sickness	-0.375	(2.131)	0.860	1.849	(4.686)
As a share of total years before 1992					
Share on Employment	-0.140	(0.076)	0.065	0.590	(0.187)
Share on UI	0.004	(0.005)	0.401	0.003	(0.010)
Share on UA	0.028	(0.016)	0.083	0.015	(0.035)
Share on Childcare	0.0127	(0.073)	0.861	0.242	(-0.164)
Share on Sickness	-0.002	(0.005)	0.745	0.005	(0.011)
Characteristics before age 50					
Months of UI	3.339	(3.906)	0.393	3.102	(8.481)
Months of UA	7.507	(5.975)	0.209	5.456	(13.46)
Months of Childcare	-3.775	(28.16)	0.893	95.05	(63.22)
Months of Sickness	0.177	(2.429)	0.942	2.269	(5.333)

Table 1.3: Impacts of pension subsidies on pension claiming age

	Pension claiming age			Hazard rate at 60		
	(1)	(2)	(3)	(4)	(5)	(6)
First-stage						
$\Delta \frac{dB}{dr}$ (1)	-5.6240***	-5.6296***	-5.3798***	-5.6240***	-5.6296***	-5.3798***
	(0.2940)	(0.2808)	(0.1993)	(0.2940)	(0.2808)	(0.1993)
Reduce-Form						
$\Delta \frac{dY}{dr}$ (2)	4.7437***	5.0352***	4.6032***	-0.9004***	-0.9833***	-0.9202***
	(1.3559)	(1.3287)	(1.3416)	(0.3158)	(0.3053)	(0.3104)
RKD estimator						
$\frac{dY}{dB}$ $\frac{(2)}{(1)}$	-0.8435***	-0.8944***	**-0.8556***	0.1601***	0.1747***	**0.1710** ***
	(0.2361)	(0.2323)	**(0.2436)**	(0.0548)	(0.0533)	**(0.0567)**
Means at the kink						
Subsidy size	108.74	108.74	108.74	108.74	108.74	108.74
Outcome variable	61.47	61.47	61.47	0.42	0.42	0.42
Sample means						
Subsidy size	90.29	90.29	90.29	90.29	90.29	90.29
Outcome variable	61.57	61.57	61.57	0.39	0.39	0.39
Controls	No	No	Yes	No	No	Yes
Cohort Fixed Effect	No	Yes	Yes	No	Yes	Yes
Observations	4994	4994	4703	5218	5218	4912
R^2	0.0092	0.0363	0.0569	0.0071	0.0696	0.0958

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Subsidies are measured in €100. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification. The standard error for RKD estimator is obtained from delta method.

Table 1.4: Impacts of pension subsidies on employment exiting age

	Employment exiting age			Hazard to exit at 60		
	(1)	(2)	(3)	(4)	(5)	(6)
First-stage						
$\Delta\frac{dB}{dr}$ (1)	-5.6240***	-5.6296***	-5.3798***	-5.6240***	-5.6296***	-5.3798***
	(0.2940)	(0.2808)	(0.1993)	(0.2940)	(0.2808)	(0.1993)
Reduce-Form						
$\Delta\frac{dY}{dr}$ (2)	4.3494	4.2372	4.8460	-0.6410	-0.7022*	-0.7679*
	(4.4284)	(4.4111)	(4.1229)	(0.4095)	(0.3894)	(0.3880)
RKD estimator						
$\frac{dY}{dB}$ $\frac{(2)}{(1)}$	-0.7734	0.7527	**-0.9001**	0.1139	0.1247	**0.1427***
	(0.7928)	(0.7894)	**(0.7718)**	(0.0719)	(0.0686)	**(0.0717)**
Means at the kink						
Subsidy size	108.74	108.74	108.74	108.74	108.74	108.74
Outcome variable	57.53	57.53	57.53	34.83%	34.83%	34.83%
Sample means						
Subsidy size	90.29	90.29	90.29	90.29	90.29	90.29
Outcome variable	57.75	57.75	57.75	32.03%	32.03%	32.03%
Controls	No	No	Yes	No	No	Yes
Cohort Fixed Effect	No	Yes	Yes	No	Yes	Yes
Observations	5218	5218	4912	5218	5218	4912
R^2	0.0023	0.0120	0.1183	0.0046	0.1284	0.1558

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.Subsidies are measured in €100. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification. The standard error for RKD estimator is obtained from delta method.

Table 1.5: Effect on transition from regular employment and unemployment

Status at t	Regular Employment (1)	Marginal Employment (2)	Unemployment (3)	Others Activities (4)
Conditional on participation in regular employment at t-1				
RKD estimator				
$\frac{dY}{dB}$	-0.0061	-0.0002	$0.0084^{\dagger}$	-0.0020
	(0.0086)	(0.0002)	(0.0046)	(0.0069)
Sample means	0.966	0.0002	0.014	0.019
Observations	473,287			
Individuals	5,527			
Conditional on participation in unemployment at t-1				
RKD estimator				
$\frac{dY}{dB}$	0.0120	0.0011	-0.0158	0.0025^{*}
	(0.0127)	(0.0024)	(0.0150)	(0.0011)
Sample means	0.030	0.0029	0.948	0.0008
Observations	86,765			
Individuals	2,622			
Conditional on participation in marginal employment at t-1				
RKD estimator				
$\frac{dY}{dB}$	0.0006	-0.0051	-	-0.0126
	(0.008)	(0.025)	-	(0.0235)
Sample means	0.007	0.965	-	0.0190
Observations	15,586			
Individuals	556			
Controls	Yes	Yes	Yes	Yes
Cohort Fixed Effect	Yes	Yes	Yes	Yes

Standard errors in parentheses * $p < 0.10^{**}$ $p < 0.05$. Subsidies are measured in €100. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification. The standard error for RKD estimator is obtained from delta method. The sample consists of female recipients from age 50 to 59 in West Germany.

Table 1.6: Impacts on months spend in other activities between last regular employment and pension claiming

RKD estimator	Marginal Employment (1)	Unemployment (2)
$\frac{dY}{dB}$	-3.6100*	4.3650
	(1.7785)	(3.3544)
Controls	Yes	Yes
Cohort Fixed Effect	Yes	Yes
Observations	4912	4912

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Subsidies are measured in €100. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification. The standard error for RKD estimator is obtained from delta method. Time spent in unemployment include months spent in both UI and UA.

Chapter 2

The Labor Supply Effects of Unemployment Insurance for Older Workers

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2.1 Introduction

Unemployment Insurance (UI) benefits are an important policy tool for helping workers smooth their consumption after job-loss. A large literature has studied the effects of UI extensions on labor supply using quasi experimental methods (see Schmieder and Von Wachter [2016] for a review). This literature has typically found that UI extensions have sizable effects on the non-employment duration of individuals who become unemployed - the intensive margin, while not having an effect on the inflow rates into unemployment - the extensive margin. This can be most clearly seen in papers based on regression discontinuity designs around age or experience thresholds, where a standard validity check is to show that the density of inflows into UI does not change at the threshold [Card et al., 2007a, Centeno and Novo, 2009, Schmieder et al., 2012a, Lalive et al., 2015]. However, this literature is largely based on relatively young workers in their 30s, 40s and early 50s, who are highly attached to the labor force. Older workers, in their late 50s and onwards, are much closer to retirement and may use UI as a stepping stone into retirement. This may be reinforced by firms that seek to reduce employment in response to a negative shock, by laying off or even buying out workers with relatively high outside options, thanks to the possibility of going into early retirement via an intermittent UI spell. Understanding the labor supply behavior of older workers is particularly important given the common goal of extending the work life of the elderly and reducing the burden on the social security system.

In this paper, we study the labor supply effects of UI extensions for older workers in Germany using social security data from 1975 to 2013. Numerous reforms to Germany's UI and retirement system over this period altered both the payoffs to entering UI at different age thresholds and the search incentives of the unemployed. Workers in their late 50s responded sharply to these policy changes. We observe increases in inflows to UI at various age thresholds where maximum UI duration eligibility increases, as well as sharp bunching of UI inflows at precisely the age that allows workers to claim their pension immediately after UI expiration. UI inflows respond as expected to a series of UI extensions and pension rule changes.

These extensive margin responses to UI policies are quantitatively meaningful. The age at which workers can enter unemployment and subsequently receive a pension without any uninsured period can be thought of as a kink in a lifetime budget set relating income to exit age. We quantify the bunching in UI inflows at the bridge-to-retirement kink under several different policy regimes. The bunching in inflows is large and yields estimates of the elasticity of exit age with respect to the net return to work that are comparable to other settings in which individuals choose when to retire without having to go through UI [Brown, 2013]. Furthermore, we quantify the *intensive-margin* effect of UI extensions at 12 different age cutoffs that discontinuously extend UI for workers in their 40s and 50s using regression discontinuity designs. Our evidence suggests that the intensive margin effect is at least as large for workers in their early and late 50s as it is for workers in their 40s. Using a simple, back of the envelope calculation, we show that ignoring the extensive margin effect can lead to significantly downward biased estimates of the non-employment effects of UI extensions for workers in their 50s – possibly less than half of the true effect.

We emphasize that credible estimates of the total non-employment effect of UI extensions for older workers cannot be simply derived from any combination of the preceding bunching and regression discontinuity estimates without implausibly strong assumptions. Instead, we argue that one needs to specify and fit a dynamic labor supply model that captures transitions between employment, unemployment with search, and unemployment as retirement. Such a model is beyond the scope of this chapter, which we conclude with a short discussion of our future plans to make progress on this structural front.

Germany provides a particularly interesting context for studying UI extensions for older workers, since there has been a tremendous amount of policy variation over the past decades. In the early 1980s, the maximum potential benefit duration (PBD) of UI was capped at 12 months regardless of age. Throughout the 1980s, maximum PBDs increased dramatically for older workers, reaching up to 32 months of UI benefits for the oldest group. Between 1999 and 2007, Germany reversed track. Maximum PBDs were reduced for older workers and Germany began the process of eliminating early retirement at age 60 following unemployment. This increase (and later decrease) in UI generosity is matched by a sharp increase (decrease) in the unemployment rate among older workers. Previous authors, such as Buchholz et al. [2013], have attributed this to a variety of policy changes aimed at reducing the labor supply of older workers, but these papers have not attempted to isolate the impact of UI.

While Germany provides many compelling advantages for studying the effects of

UI for workers it also offers a number of challenges. The main complication is that in addition to UI there are a large number of other policies that changed over the past decades and that may affect inflows into UI and unemployment durations. Some of these changes are about regular and early retirement rules and are relatively easy to understand, but there are also many rules based on collective labor agreements (CLAs) that are on the sectoral level or even specific to individual firms. Such CLAs may themselves take policy induced age discontinuities into account, for example by encouraging workers to exit firms at those age thresholds with severance packages. In this case one can view CLAs as a mechanism of how age discontinuities lead to extensive margin responses. On the other hand, CLAs may also lead to bunching at age thresholds that are not directly related to retirement or UI institutions. This complicates our setting and we consider a variety of approaches to obtain meaningful estimates in light of such confounding.

Our setting also raises interesting methodological issues. While several papers have estimated regression discontinuity designs in the presence of manipulation of the forcing variable [see for e.g. Card and Giuliano, 2014, Gerard et al., 2015, Barreca et al., 2016, Hoxby and Bulman, 2016], this manipulation has typically been treated as a nuisance, with researchers attempting to avoid bias using techniques like excluding observations close to the threshold (donut-hole regressions). However, whether and when to enter UI is itself an important outcome and in practice individuals (together with firms) can influence this decision. When UI is used as a pathway to retirement, it essentially constitutes a labor supply decision in the face of a budget set defined by wage rates, the UI system and retirement rules. The UI system create kinks in this budget set and individuals choosing to enter UI as a step towards retirement should bunch at these kink points. We could thus use bunching techniques to back out labor supply elasticities for these workers, based on the amount of bunching around such kinks [Saez, 2010, Kleven, 2016].[1]

While bunching can help recover extensive margin decisions, it complicates identification of intensive margin effects. Ideally, we would use the discrete changes in potential benefit duration at the age thresholds to estimate intensive margin responses. Yet, extensive margin responses at or around these thresholds lead to direct violation of the RD assumption that there is no manipulation of the running variable and individuals on both sides of the cutoff are therefore comparable. We use three approaches to circumvent this challenge and obtain plausible estimates of intensive margin responses: First, we use donut-hole regressions to exclude the range where most of the bunching occurs. This is most credible when the bunching is not too extreme and there does not appear to be an overall shift in the density outside of a sharp window around the threshold. Second, we include a series of individual level controls to help absorb selection effects. Third, we estimate intensive margin responses at slightly younger age thresholds, where bunching is less of an issue, in particular a threshold at age 54 during the 1990s.[2]

Our paper is related to a large literature on retirement decisions. Several methodologically related papers have analyzed bunching in retirement age to derive labor supply elasticities, for example Brown [2013] looks at bunching at the regular retirement age for teachers and Manoli and Weber [2014] analyze permanent exits from the labor force around tenure thresholds in Austria that lead to discrete increases in severance payments. Unlike these papers we look specifically at entry into UI, rather

[1]Note that not all bunching around UI age discontinuities is necessarily related to early retirement. It may also be that firms postpone lay-offs or workers postpone claiming UI benefits until they reach the threshold. This is likely to be most important at ages further away from the retirement age, such as the age threshold at age 54 in the 1990s and the threshold at age 55 in the 2000s.

[2]We can also estimate intensive margin responses on a sample of individuals who later return to the labor market, which likely obtains a lower bound of the intensive margin response for these workers. Finally, we can follow the approach in Gerard et al. [2015], who explicitly provide a framework to estimate bounds in RD settings in the presence of sorting.

than exits from the labor force.

A handful of papers examine the effects of UI extensions on older workers [for example Kyyrä and Ollikainen, 2008, Bennmarker et al., 2013]. Riphahn and Schrader [2017] and Dlugosz et al. [2014] show that the shortened UI benefits for older German workers following a 2006 reform increased employment. A small literature explicitly examines interactions of the UI system with retirement decisions. For example, Lalive [2008a] analyzes the effect of UI extensions for older workers around a discontinuity at age 50 in the Austrian UI system as well as at a border discontinuity and finds relatively large disincentive effects, especially for women. He also shows that women seem to respond on the extensive margin to the change in UI generosity. Using partially the same variation as Lalive, Inderbitzin et al. [2016a] show that much of this was due to early retirement responses. Kyyrä and Pesola [2017] show that postponing eligibility by two years for a retirement-via-UI pathway in Finland increases employment by 7 months. Similarly, Kyyrä and Wilke [2007] show that increasing the age threshold of early retirement via UI benefits from 53 to 57 in Finland significantly reduced unemployment durations. Hairault et al. [2010] provide some evidence based on French survey data, that job search behavior of the unemployed depends on the distance to retirement age.[3] Several papers analyze the interaction between various retirement rules and labor supply in Germany [see Giesecke and Kind, 2013, Boersch-Supan et al., 2004, Boersch-Supan and Hendrik, 2011, among others]. We focus on quantifying the overall effect of UI extensions on labor supply for older workers, accounting for both extensive and, the well documented [Card et al., 2007a, Schmieder et al., 2012a], intensive margin behavior.[4]

[3] Coile and Levine [2007] find that UI generosity has little impact on retirement in the U.S.

[4] While our focus is to quantify the overall effect of UI extensions rather than discussing optimal policy, our analysis can be viewed as an important input into welfare computations. For papers on the optimal design of UI for older workers see, for example: Hairault et al. [2012], Michelacci and Ruffo [2015], and Inderbitzin et al. [2016a].

Finally, our work suggests that German firms play a role in regulating how worker inflows into UI respond to UI extensions. Jaeger et al. [2017] study job destruction following improvement in workers' outside options using variation in UI benefits in Austria, finding that low surplus jobs are destroyed. A few studies have estimated the sensitivity of layoffs of older workers to monetary incentives with some finding little sensitivity [Behaghel et al., 2008, Johnston, 2017], but not others [Schnalzenberger and Winter-Ebmer, 2009].

This paper proceeds in four steps. We first provide a very general decomposition of the effect of UI extensions on time out of work in the presence of intensive and extensive margin labor supply responses, which highlights the importance of imposing additional structure to fully estimate these responses. In Section 2.3, we present the institutional background and describe the core features of the German unemployment insurance and retirement institutions. In Section 2.4, we present graphical evidence of bunching in UI inflows at the bridge-to-retirement kink and at the age cutoffs that discontinuously increase PBDs. In Section 2.5 we present reduced form evidence of both intensive and extensive margin responses. To do so, we estimate RDs at all the older age cutoffs available to us, using various approaches to handle sorting at these cutoffs. We also estimate bunching masses and age-of-exit elasticities at all kinks in the budget set for older workers entering UI, as if every worker were indeed choosing their exit age strategically. Under strong assumptions, we perform a back of the envelope calculation showing that ignoring the extensive margin effects of UI extensions on workers aged 50-60 produces downward biased estimates of the non-employment effect of UI extensions. Section 2.6 concludes by offering a path forward towards more credible estimates that properly account for the document extensive margin effect of UI extensions.

2.2 The Effect of UI Extensions on Total Time out of Work

To fix ideas and terminology, we present a simple framework that describes how potential UI benefit duration affects time out of work in the presence of extensive margin responses. There is a mass of workers N, who enter the workforce at age 1 and reach a mandatory retirement age at T^R. Let potential UI duration be P. In practice this can be a function of the age of entry into UI, but for simplicity of exposition we take P to be constant. In each period t (meaning at each age), a worker is either working or not working. We do not distinguish between unemployment and non-employment and use the terms interchangeably. The fraction of workers entering unemployment at age t is denoted as $g_t(P)$. If an individual becomes unemployed, the duration of non-employment is defined as the time between entering unemployment and either starting a job again or when the individual retires. We denote the expected non-employment duration of individuals becoming unemployed at age t as $D_t(P)$. The expected total time out of work (T^u) for an individual is given by $T^u(P) = \sum_{t=1}^{T^R} g_t(P) D_t(P)$.

Without specifying micro-foundations of this labor market, we can write the relationship between inflows and durations on benefit durations as reduced form functions and decompose the effects of an increase in potential benefit into intensive and extensive margin components. A change in P can thus be decomposed into:

$$\frac{dT^u}{dP} = \underbrace{\sum_{t=1}^{T^R} g_t \frac{\partial D_t}{\partial P}}_{\text{Intensive Margin}} + \underbrace{\sum_{t=1}^{T^R} D_t \frac{\partial g_t}{\partial P}}_{\text{Extensive Margin}} \tag{2.1}$$

The first term represents the standard intensive margin effect of UI extensions on non-employment durations that most of the UI literature has estimated. The second term represents the changes in inflows into unemployment.

The central question of this paper is how to credibly estimate this total effect for older workers. This is challenging to estimate using purely reduced form techniques. Note that $\frac{\partial g_t}{\partial P}$ is never likely to be 0. In practice, if P increases D, employment falls, changing the pool of people at risk of becoming unemployment. Hence, future g_t might decrease, violating $\frac{\partial g_t}{\partial P} = 0$ for some t.

It is instructive to consider two simple cases. Let us assume that such effects on the pool of at-risk people are negligible and that we are focused on younger workers. Schmieder et al. [2012a] show that younger workers do not significantly alter their entry probabilities into UI in response to changes in P. In this case $\frac{\partial g_t}{\partial P} \approx 0$. In such cases $\frac{dT^u}{dP}$ can be recovered from RD estimates of the intensive margin effect at age cutoffs in P [as in Schmieder et al., 2012a].[5]

Now consider a case at the other extreme, with extensive but no intensive margin effects. Suppose older workers only use UI as a bridge-to-retirement, and never become unemployed except by their own choice. Once they exit they stay non-employed until the age at which they can claim their pension (T^R). So at each age, the expected non-employment duration is fixed and not dependent on P: $\frac{\partial D_t}{\partial P} = 0$. However, suppose that these workers time their exit date (into UI) and that this responds to P. For example, suppose there is a mass $\tilde{N}$ of workers that time their entry to maximize unemployment coverage before retirement by entering UI at age $T^R - P$.[6] Suppose that the mass of entries at all ages is otherwise constant. Then an increase in P to $\hat{P}$ would decrease total non-employment duration by $D_{T^R - P} \times \frac{\tilde{N}}{N}$ at age $T^R - P$ and

[5] Note also that if one is interested in more complicated changes in potential benefit durations (that is not just an increase at a single age level k), then it is still relatively straightforward to estimate the intensive margin effect by aggregating estimates of $\frac{\partial D_k}{\partial P_k}$ at different age levels.

[6] This could be the case if we think of workers as maximizing lifetime utility over consumption and leisure subject to their budget constraints. Depending on institutional parameters, such a lifetime budget constraint might exhibit a kink at the 'bridge-to-retirement-via-UI' age of $T^R - P$, as we will show is the case in Germany. Extending P moves this kink, and hence moves UI exit mass. If this were the correct model, we could calibrate its key parameters using bunching techniques in a manner similar to Brown [2013] and then simulate $\frac{dT^u}{dP}$.

increase non-employment duration by $D_{T^R-\hat{P}} \times \frac{\tilde{N}}{N}$ at age $T^R - \hat{P}$. The total non-employment effect of the increase in P in this pure, extensive margin setting would thus be given by $\frac{dT^u}{dP} = (D_{T^R-\hat{P}} - D_{T^R-P})\frac{\tilde{N}}{N} = (\hat{P} - P)\frac{\tilde{N}}{N}$.

In practice, neither of these cases fully captures the complexity of reality. Older workers are likely to still have strong intensive margin responses to changes in P, and some older workers, even at later ages, will find themselves unemployed not by their own choice. Note further, that a simple two type model (one representing each case above) is problematic in its artificiality – in practice workers may choose to transition between states as a function of P. We opt here to present reduced form evidence on each margin and, in future work, to estimate a dynamic life cycle model by matching it to these reduced-form generated moments.

2.3 Institutional Background and Data

2.3.1 Unemployment Insurance

The German unemployment insurance system provides income replacement to eligible workers who lose their job. Prior to 1985, eligible workers were entitled to at most 12 months of benefits. Replacement rates for UI were relatively stable over the period of study (1980–2015) (67-68% for an individual with children and 63-60% for an individual without children).[7] Beginning in 1985, numerous reforms changed PBDs in a manner that tied the maximum PBD to recipients' exact age at the beginning of their UI spell.[8]

[7]Individuals who exhausted UI benefits prior to 2005 and whose net liquid wealth fell below a certain threshold were eligible for unemployment assistance (UA) benefits with an effective average replacement rate of around 30%. In principle, replacement rates were between 50% and 57% but lower in practice due to deductions like spousal income. See Schmieder et al. [2012a] for a discussion. From 2005 on, UA was replaced by unemployment insurance benefits 2 (UIB II), a completely means tested program. Both UA and UIB II are unlimited in duration.

[8]See Hunt [1995] and Fitzenberger and Wilke [2010] for an analysis and discussion of these reforms.

Reforms in 1985 and 1987 increased maximum PBDs for workers above age 42. The most generous PBD – up to 32 months – became available to workers aged 54 and above following the 1987 reform. Reforms in 1999 and 2006 gradually decreased the generosity of the system. In 1999, age thresholds were increased, and then, beginning 2006, maximum PBD was reduced from 32 to 18 months for workers above age 55, while everyone else could only receive 12 months. There was a modest reversal of this trend in 2008 when workers above 58 could attain a maximum PBD of 24 months.

Figure 2·1 plots maximum PBD by age for older workers in each different institutional regime.[9] Appendix Table B.1 provides details about each reform. These policy changes provide highly useful empirical variation, both at the age thresholds, and by changing incentives on when to enter unemployment if using unemployment as a bridge-to-retirement, as we elaborate on in the next section.

2.3.2 Pension System and Early Retirement Via Unemployment

Germany has a generous pay-as-you-go public pension insurance with high effective replacement rates. Participation is mandatory, with the exception of civil servants and the self-employed, which are not covered by our data. Pension benefits depend on workers' earnings, years of contributions, an adjustment factor, and the type of pension claimed. Benefits are roughly proportional to lifetime income at an average replacement rate of 50% [Deutsche Rentenversicherung, 2016].

The statutory retirement age (SRA) for a regular old age pension remained at 65 throughout our sample period, with the only prerequisite being 5 years of contributions. Several alternate pathways made receiving a pension before 65 an option. The five main pathways to retirement were regular old-age pensions, old-age pensions for long-term insured, old-age pensions for women, old-age pensions due to unemploy-

[9]We omit the short 1985 regime in the interest of brevity and because it appears that some individuals who entered UI in 1985 retroactively benefited from the UI extensions in later years.

ment (and, later, part-time work) and old-age pensions for severely disabled persons [see for e.g. Boersch-Supan and Wilke, 2005]. Appendix Table B.2 documents the earliest possible retirement age for each of these pathways over the past 4 decades, while Appendix Table B.3 documents all relevant reforms. We focus primarily on the pathway into retirement via unemployment.[10]

The unemployment pathway (UI pathway) provided eligible workers with an option to retire at the age of 60. The eligibility requirements for this pathway were: 1) at least 15 years of contributions, at least 8 of which must have occurred in the past 10 years, and 2) being unemployed for at least 1 year after the age of 58 and a half. The generosity of UI benefits, combined with lenient job search requirements for older workers, made old-age pensions due to unemployment attractive. Workers 58 and older could receive unemployment benefits without actively looking for a job or other obligations.[11] For the first 3 cohorts we will focus on, the unpenalized/normal retirement age (NRA) as well as the earliest possible retirement age (ERA) via the UI pathway was age 60. This means persons satisfying the requirements could retire at 60 with no penalty other than the loss of additional years of pension contributions.

This system incentivizes workers considering early retirement to time their entry it to UI around the age that allows workers to transition directly from UI to pension, without any uncovered period. We note that entering UI voluntarily is highly feasible in Germany and at most lightly penalized.[12] Put differently, the possibility of using UI as a bridge-to-retirement introduces a kink in a lifetime budget constraint relating

[10] While early retirement due to disability is also quantitatively important, Riphahn [1997] argues that in practice they are not close substitutes and that retirement due to disability is in fact usually associated with a health shock.

[11] This so-called "58er-Regelung" was formally introduced end of 1985 and in place until end of 2007.

[12] A worker may be sanctioned if he or she quits a job voluntarily. These sanctions take the form of losing the first few weeks of benefits and vary from a 4-12 week penalty over the study period. These sanctions, which are not always applied, are insufficient to offset the appeal of using UI as a pathway into retirement.

lifetime income to year of exit into UI. Individuals retiring before $60 - P$, with P being the maximum UI PBD, are forced to spend time reliant on a spouse or on unemployment assistance (UA/UIB 2) before their pension, whereas individuals who leave at or after $60 - P$ can take the full UI duration and transfer directly into pensions. This reduces the value of an extra year of work after the kink, decreasing the slope of the budget constraint. In general, the size of the kink is exacerbated by the generosity of the UI system, the size of the drop comparing UI to UA/UIB 2, and how generously time on UI is counted towards pension contributions.[13] We will show that UI entries react to this kink at age $60 - P$.

The NRA and ERA via the UI pathway remained at 60 until a 1992 reform. Cohorts born between January 1937 and December 1941 saw their NRA increase in steps by birth month from 60 to 65. While they could continue to retire at the ERA of 60, they now faced an actuarial adjustment in the form of a 0.3% pension reduction per each month they retired in advance of the NRA.

Cohorts born in or after 1946 saw their ERA for the UI-pathway increase in steps by birth month from 60 to 63, ending with cohorts born in December 1948. This meant that these cohorts could no longer claim their pensions at age 60, even with a penalty. Cohorts born after 1952 (after our sample) saw this pathway into retirement via UI entirely abolished.

Figure 2·2 plots the evolution of stylized lifetime budget constraints for select cohorts experiencing different UI and pension regimes. Appendix **??** contains detailed descriptions of how these budget sets are constructed. We assume workers earn a constant after tax wage of €30,000 and live 80 years. For simplicity, we assume that the max PBD is fixed over time for each cohort at the level that prevailed when they

[13]In practice, unemployment counts as an 80% contribution year calculated on pre-unemployment wages.

were close to the kink.[14] In panels (a)-(c), representing the 1924, 1929, and 1935 cohort respectively, the the NRA and ERA for retirement via unemployment was age 60, but maximum PBD varied. In panel (d), representing the 1941 cohort, the ERA remained at 60 but the un-penalized NRA was increased to around 64, with slight variation by month of birth. This amounted to a financial penalty for retiring at age 60 of approximately 18% of gross lifetime pension benefits. In panel (e), representing the 1949 cohort, the ERA was increased to 63 and the NRA was 65.[15] The penalty for retiring at age 63 via unemployment was 7.2%. In panel (f), representing the 1952 cohort, the pathway into retirement via unemployment was abolished, leaving the earliest possible retirement age as 63 for long-term insured workers with over 35 years of qualified contributions. The un-penalized NRA for long-term insured is 65.5, making the financial penalty for retiring at 63 9%.

Note further that the large, discontinuous increases in PBD at the various age cut-offs in the PBD duration schedule (see Figure 2·1) could also induce selection into UI. This could occur for both people who only plan to temporarily be on unemployment and among people planning to retire.

Throughout the rest of this paper, we focus primarily on the first kink induced by using UI as a bridge-to-retirement, but we also discuss the notches at the PBD age cutoffs. In practice, agents might also use UI as a bridge to the long-term contribution retirement age of 63 or the regular retirement age of 65.[16] Since we cannot credibly calculate whether or not a person is eligible for the long-term contribution rate, examining bunching at these kinks is challenging. Note also, that changes in other pathways may create alternative substitutes for workers aiming to retire early.

[14]The dashed line shows the realized lifetime budget set that takes into account all the UI policy-induced changes and age cutoffs.

[15]Retiring at 63 via the long-term insured pathway is slightly more costly.

[16]Individuals cannot receive UI past 65, and cannot receive UI and pensions simultaneously.

Appendix Table B.3 summarizes the reforms for all of the different pathways over our study period.

2.3.3 Data

We use rich administrative data from the German Social Security system, assembled by the Institute for Employment Research (IAB) into the Integrated Employment Biographies data file (IEB) [see also Card et al., 2013, Jäger, 2016, Schmieder et al., 2012a]. This data contains information on all employment periods covered by social security and on all periods of UI receipt between the years 1975 and 2013. The employment information covers approximately 80% of the regular workforce, with the self-employed and civil servants being the most common exceptions. The data on UI receipt stem from administrative UI records and contain information on the exact duration of UI-receipt and the amount of daily benefits.

This version of the paper focuses on men, since early retirement rules differ for men and women. Results for women are available upon request and yield a qualitatively similar picture. We select all male UI-entries between 1980 and 2010 who qualify for their age-specific maximum PBD based on their working histories. This leaves a five year window before the first year in the data (1975) and a three year window after the last (2013), allowing us to calculate UI eligibility for all individuals and unemployment durations for up to three years after UI entry. Since some of the requirements for maximum PBD eligibility, such as the duration over which claims could be accumulated, changed over the study period, the restrictions set on this duration differ slightly over time. We summarize these restrictions in Appendix Table B.4. Additionally, we exclude mining and steel construction from our analysis, since both sectors are known to have specific early-retirement rules for at least some of the periods. For other specific subgroups which face some, but less clear or pronounced early retirement rules we do not exclude cases a priori, but address them throughout the analysis. For the selected individuals, we construct detailed biographical information such as experience tenure or past exposure to unemployment.

2.4 Graphical Evidence

This Section documents the behavior of older individuals entering UI over three decades. We present evidence of sizable extensive margin UI responses at the bridge-to-retirement kink and show that UI inflows react to UI and retirement policy changes. Specifically, we document spike in UI inflows at each bridge-to-retirement age: at 59 when the ERA was 60 and maximum PBD was 1, at 58 when maximum PBD was extended to 2, and at age 57 and 4 months when maximum PBD was extended to 32 months. As the NRA increases this bunching is reduced, and eventually as the ERA increases it dissipates. The next Section quantifies the bunching mass and estimates regression discontinuities at each of the PBD age cutoffs to quantify responses on the intensive margin.

We will also see evidence of clear bunching at various other thresholds, not all of which corresponds to kinks or notches in our stylized budget sets. For example, beginning with the 1929 cohort, we see bunching into UI entries at age 55. While some of this could be round number bunching or bunching at reference points, much of this is driven by specific collective labor agreements at the firm or sectoral level that specified retirement packages and ages. Indeed, this type of bunching is almost entirely absent in the years leading up to and including 1982, consistent with the timing of the first major CLAs specifying retirement ages (see Trampusch et al. [2010]). Our sample drops the mining and steel sectors which have clearly defined CLAs, but inevitably picks up other sectors and firms with CLAs. During Germany's high unemployment years, many firms reduced employment through CLAs that bought out older workers. Age 55, and to a lesser extent age 56, was a common cutoff used in these CLAs. The importance of these CLAs fades throughout the late 90s and early 2000s. In robustness exercises, we consider alternate samples and ways to address any confounding. Generally, the bunching at the kink into retirement exceeds bunching at these alternative thresholds. Nevertheless, the data points to an active role for firms, together with workers, in governing responses to UI extensions. Regardless of the source, it will be clear that changes in UI durations generate extensive margin responses that should be taken into account when designing policy.

Figure 2·3 shows the number of individuals entering UI by age for 6 select cohorts in our sample, each chosen to represent a different institutional regime. We opt to display these annual cohort-level graphs to keep retirement rules constant within-figure. In practice the retirement rules vary by month of birth (see Appendix Table B.3), but fixing year of birth is a good approximation and increases sample sizes. When constructing cohort-by-cohort figures, the state of the economy is not fixed at one point in time, so we also plot the prevailing unemployment rate at the time for reference. Furthermore, since UI rules changed over time (and not by cohort) UI entrants at different ages in the same cohort can have different PBDs (see Appendix Table B.1).[17] Graphs of UI entrants by calendar year offer different trade-offs but ultimately yield a similar picture and are available upon request. Figures 2·4 and 2·5 complement Figure 2·3 by plotting mean UI benefit receipt duration and mean non-employment duration (capped at 36 months) by age for each cohort. We now

[17] From the perspective of a single cohort, UI can change for two reasons. It can change at known age cutoffs (represented by the dashed red lines in Figures 2·3 – 2·5), for example the 1941 cohort would have turned 54 in 1995, amid a UI policy regime that had maximum PBD of 26 months for workers entering UI below age 54 and 32 for workers above age 54. These age-cutoffs would be known to the individual years before turning 54. Alternatively, UI can change for workers above a certain age in a cohort due to a policy change in the future. These policy changes would not necessarily be know the the individual in advance.

discuss each cohort in turn.

Benchmark: 1924 Cohort. Figure 2·3 Panel (a) shows UI inflows for the 1924 cohort. Note that UI entries pre-age 59 track the official West German male unemployment rate at the time (the dashed line).[18] When this cohort was less than 61 years old, their PBD was 12 months.[19] Cohorts born before 1937, including this cohort, could retire early and without penalties at age 60 following a year of unemployment insurance. Since the maximum PBD was 12 months for this cohort, the 'bridge-to-retirement' pathway, in which individuals will be covered by UI or pensions without gaps, has individuals entering unemployment at age 59. This is indicated by the red and blue shaded areas under the figure (see also Figure 2·2 panel (a)).

We observe clear bunching in UI entries at age 59, precisely the age at which individuals can transition into retirement immediately following UI expiration. There is no comparable bunching elsewhere. Figure 2·4 panel (a) shows average UI benefit duration for the individuals in Figure 2·3 Panel (a). The average UI benefit receipt of 11.8 months around age 59 is very close to the maximum PBD of 12 months, supporting the idea that entrants are predominantly using UI as a bridge-to-retirement. UI durations increase at older ages, in step with the UI reforms in those years, and exhibit declining patterns before the retirement age for the long-term insured at 63 and the standard retirement age at 65. Figure 2·5 panel (a) plots average non-employment duration (capped at 36 months) for the individuals in Figure 2·3 Panel (a). This peaks at age 59, averaging 35.4 months, again supporting the idea that most entrants at this age retire. Together, this is clear evidence of sizable, extensive margin responses

[18] We seasonally adjust reported UI rates using X-13 ARIMA - SEATS.

[19] On January 1 1985, UI was extended to 18 months. This means that when the person born on Dec 31st 1924 turns 60 and a day, they would be eligible for 18 months of PBD. By age 61, everyone in the 1924 cohort is eligible for 18 months of PBD. This 'entire-cohort eligibility' point is indicated by the change in the lower, grey-shaded bars, which also show the later UI reforms.

to UI policy. This view is reinforced below, where we examine UI entries for later cohorts facing longer PBDs and hence kinks at different, earlier ages.

1929 Cohort. Figure 2·3 Panel (b) shows UI entries for the 1929 cohort. This cohort faces the same potential retirement ages as the 1924 cohort, but has longer PBDs in their late 50s. Specifically, those who enter UI at age 58 have 24 months maximum PBD. This shifts the 'bridge-to-retirement' age to 58, and indeed, we see extensive bunching at around age 58, while we continue to note some excess mass at 59.

This figure also clearly shows bunching in UI entries at other, non-kink points, particularly at age 55 and 57. As discussed, these likely represent firm-specific collective bargaining agreements to release or buy out workers once they turn 55. This also suggests that the bunching at the bridge-to-retirement age is driven by joint decisions between firms and workers.

Panel (b) of Figures 2·4 and 2·5 show average UI benefit receipt and average non-employment durations for this cohort. Figures 2·4 (b) reveals that average UI duration at the kink is 23.0 months, very close to the full 24 around the 58 cutoff. There are also clear spikes in UI durations at age 55 and 57, mirroring bunching in UI entries at those ages. Figure 2·4 Panel (c) shows that average non-employment duration at the bridge-to-retirement age reaches 34.3 months.

1935 Cohort. The 1935 cohort continues to face the same potential retirement ages as the prior cohorts, but even more generous UI. Workers entering UI at or after age 54 had a maximum PBD of 32 months. Accordingly, Figure 2·3 Panel (c) shows that UI entries exhibit strong bunching at precisely age 57 and 4 months, or 32 months before the early retirement age of 60. We continues to see some excess bunching at age 58 and 59, as well as some at 55 and 56. Panel (c) of Figures 2·4 and 2·5 confirm once again that people entering at the bridge-to-retirement age take UI for close to the maximum duration (29.7 months) and have a 35.3 month average capped non-employment duration. These figures also show discrete jumps at age 54 and 55 in average UI duration and non-employment duration. The jump at age 54 is consistent with the July 1987 reform that extended maximum PBD from 24 to 32 months (26 months) for workers above 54 (between 49 and 54). The jump at 55 in both figures continues to reflect the fact that layoffs after age 55 differ in composition and likely reflect firm-level CLAs.

1941 Cohort. This is the first cohort for which retirement rules change. The 1941 Cohort could still retire at age 60 following a year of unemployment, but a 1992 reform introduced actuarial adjustments for retirement before age 65. These were introduced gradually by month and year of birth for cohorts born between January 1937 and December 1942, resulting in an approximate 18% penalty for anyone in the 1941 cohort retiring at 60. The maximum PBD remained at 32 months for workers above age 54. Figure 2·3 Panel (d) reveals that we continue to see bunching at age 57 and 4 months, but it is now more muted relative to entries below this age. Moreover, consistent with the larger penalties, we see in Figure 2·4 panel (d) that average UI benefit duration no longer reaches 32 months at this bridge-to-retirement age, but instead averages just 25.3 months. Similarly, average non-employment durations are also lower, around 30.5 months, suggesting that some workers are returning to work instead of retiring at the penalized ERA. Spikes at age 55 and 56 continue to be visible in entries, UI receipt, and duration. Interestingly, this figure displays what looks very much like a discrete jump in the level of UI entries after age 55.

Additionally, this cohort faced a stable PBD schedule in their 50s, with a known age cutoff at 54 (for which maximum PBD jumped from 26 to 32 months). We see some bunching at this cutoff, which could arise from people expecting long unemployment timing their entry into UI or from those considering very early transitions to retirement. The sorting around this age cutoff poses a challenge to standard RD estimates of the effects of PBD extensions on non-employment duration, as we discuss further below.

1949 Cohort. The 1949 cohort faced both reduced PBD if retiring at later ages and a stricter retirement law. Individuals born in 1949 could no longer retire early via unemployment at 60, but instead could only draw pensions at age 63 at the earliest. They had to wait until age 65 to draw pensions without actuarial adjustments (7.2% for retiring at 63). Figure 2·3 Panel (e) shows some bunching at 61, consistent with an early retirement age of 63 and the 2 years maximum allowable PBD, but it is not extensive. Importantly, now that the bridge-to-retirement at 60 has been removed, we now no longer see bunching between ages 57 and 59. We continue to see some age-55 bunching. Panel (e) in Figures 2·4 and 2·5 shows that average UI durations at the new bridge-to-retirement reach 13.7 months, well below 24, and non-employment durations average 33.2 months.

1952 Cohort. This cohort is no longer allowed to retire early via unemployment, although if they are eligible for the long term old age pension, they could retire at age 63. Unfortunately, we run out of data past age 59 (as we need 3 years post-2010 to calculate non-employment durations). Nevertheless, the distribution of UI entries continues to look relatively smooth. The 1952 cohort would have known about the age 58 PBD cutoff extending maximum PBD from 18 months to 24 months starting in 2008 (i.e. when they turn 56). As with the 1941 cohort at age 54, we see evidence of sorting into UI to take advantage of this UI extension. We continue to see some bunching at age 55.

Overall, we observe clear bunching into UI at the bridge-to-retirement age. The bunching mass responds to UI extensions. We have also seen evidence of sorting into UI at earlier age cutoffs where PBDs are extended discontinuously, including age 54 and 58. Bunching at other points in the distribution related to CLAs, suggest that firms play an important role. While we cannot easily identify the extent to which responses come from workers or firms, it is clear that a full accounting of the effects of UI extensions on non-employment need to take into account this extensive margin to avoid downward bias. In the next section, we take a first pass at understanding the potential magnitude of this bias by quantifying the bunching mass and by comparing it to RD estimates of the intensive margin effect.

2.5 Quantifying Intensive and Extensive Margin Responses: A Back of The Envelope Calculation

We have seen that PBD extensions alter inflows into in UI, which implies that the total effect of PBD extensions on time out of employment for older workers cannot be estimated solely from intensive-margin estimates (recall Equation 2.1). Yet, the extent of any such bias remains unclear. In this section we quantify the bunching mass at each kink and show that it is quantitatively meaningful. Next, we use Regression Discontinuity Designs to estimate the intensive margin effects of PBD extensions for older workers. We perform a simple, 'back of the envelope' calculation to show that the bias from ignoring the extensive margin is likely severe. We discuss the drawbacks and assumptions behind this calculation and how future work can improve upon this.

2.5.1 Estimating Extensive Margin Responses using Bunching Estimators

In this subsection, we estimate the amount of bunching at each retirement-via-UI kink in Figure 2·3.[20] The amount of bunching can be viewed as a reduced form parameter, while converting it into a labor supply elasticity requires additional structure.

In order to estimate bunching mass at each kink we need to fit a counter-factual to the data. We use two approaches. First, we use the polynomial approach suggested in Chetty et al. [2011]. In this approach, we exclude a region around the kink and fit a seventh degree polynomial to the data, including dummies for the excluded bins. The normalized bunching mass is then given by the difference between the actual density and the counter-factual distribution, divided by the average of the counter-factual over this bunching region.[21] Column (1) of Table 2.2 shows this normalized bunching mass; the footnote lists the chosen excluded regions.[22] As one would expect, the normalized bunching mass is largest for the 1935 cohort and smallest for the 1949 cohort. Since this counter-factual approach relies heavily on the shape of the counter-factual [Blomquist and Newey, 2017], we also use an alternative approach. For this second approach, we take advantage of the fact that UI entries closely track the male unemployment rate (UR).We scale the UR by the ratio of the mean number of UI entries between age 49 and the kink to the mean UR in this area, and use this re-scaled UR as a counter-factual. This approach yields a similar qualitative picture,

[20]There are 5 estimates because our data does not extend far enough yet to observe the kink for the 1952 cohort.

[21]We choose to count the bunching mass as the mass between the cutoff and the right excluded point. Including the imprecision on the left of the cutoff is also an option and would lead to larger estimates.

[22]The exclusion region is chosen visually. For the first three cohorts we simply exclude a region slightly to the left of the cutoff up to age 60. The most difficult and potentially arbitrary choice is for the 1941 cohort, which displays a discrete jump in UI inflows at age 55. We opt to exclude all of the region post-age 55, but only count entries post-age 57.33 in the bunching mass.

but suggests less bunching for the 1929 cohort and significantly more in 1935 and 1941 cohorts (see Table 2.2 column (2)).

To get a sense of how these magnitudes compare to other contexts, we make the strong assumption that all workers around this age behave as if they were following a simple lifetime labor supply model, as in Brown [2013]. That is, we assume workers choose their entry date strategically to maximize utility over lifetime consumption and leisure subject to a lifetime budget set. As long as workers abilities are drawn from a continuous distribution, the distribution of UI entries will be smooth. The introduction of a kink in the budget set results in bunching at the kink point, and the amount of bunching allows estimation of a labor supply elasticity, using by now standard techniques [see e.g. Saez, 2010, Kleven, 2016]. We refer the reader to Appendix **??** for the details and to Appendix **??** for how we construct the budget set. Under this model, the normalized bunching mass allows us to recover the elasticity of exit age with respect to the net return to an extra year of work. These estimates are contained in Table 2.2. The elasticity estimates for the first four cohorts range from 0.026–0.069 (0.026–0.101 under the UR-counterfactual). While sensitive to the exact specification of the counter-factual, these fall squarely in the range of Brown [2013]'s estimates for Californian teachers timing their retirement as a function of pension benefits.[23] It is striking that we are seeing retirement-via-UI responses that are comparable to standard retirement decisions that do not pass through UI. This should be understood in context, and is likely a function of the lack of serious penalties for entering voluntarily and the relatively low requirements whilst on UI as an older worker.[24]

[23] Brown [2013]'s preferred estimate is 0.04. She examines the retirement behavior of Californian teachers whose normal retirement age is 60 and whose average pension replacement rate is 59%.

[24] It also lines up with findings in other contexts, such as Kyyrä and Pesola [2017].

2.5.2 Estimating Intensive Margin Responses using Regression Discontinuity Estimators

In order to understand how these large bunching responses might bias our standard, intensive-margin estimates of the non-employment effects of UI extensions, it is helpful to have estimates of the intensive-margin effect in this context. Fortunately, the PBD step schedules shown in Figure 2·1 allow clean Regression Discontinuity (RD) estimates of this effect [as in Schmieder et al., 2012a]. These estimates require that there is no sorting into UI around the age cutoffs. This is satisfied at younger ages, but not at the oldest ages.

Here we estimate RDs pooling all years under each UI regime, starting with the 1987-1999 period (see Appendix Table B.1).[25] This gives us 12 age cutoffs at which we can estimate the non-employment effect of UI extensions. In practice the age cutoffs provide between an extra 3–6 months of PBD, and we will divide each estimate by the number of months PBD was extended to get the marginal non-employment effects of an extra month of PBD.

At each age cutoff we estimate the following RDD specification:

$$y_i = \delta\, \mathbf{1}(a_i \geq A)\, \Delta PBD + f(a_i) + X_i\beta + \varepsilon_i \tag{2.2}$$

y_i is non-employment duration (capped at 36) for individual i, a_i is the age at time of UI entry (measured on the daily level) and $\mathbf{1}(a_i \geq A)$ is an indicator function which equals one when individuals age is above the age cutoff A where benefits are extended discontinuously by ΔPBD months. In this specification, δ measures the effect of a one month increase in PBD. The function $f(a_i)$ is set to be a linear function with different slopes on each side of the cutoff in the baseline specification. X_i is a

[25] We omit the period 1986-1987 due to it being a short transition period. There is no first stage in this period.

vector of additional controls. We use a local polynomial regression with rectangular kernel and cluster standard errors on the daily level. We set the bandwidth to two years, but restrict it to one year on the right hand side for the 49 and 54 years cutoff during the 1987-1999 period due to other discontinuities at 50 and 55.

This specification is well-identified for the age cutoffs where UI entries and other pre-determined outcomes are smooth around the cutoff. This is case for all of the younger ages [Schmieder et al., 2012a]. Sorting at the cutoff is a concern for some of the older cutoffs (this can be seen, for example, at the age 54 cutoff in Figure 2·3 panel (d)). The degree of sorting varies between cutoffs and is usually most pronounced within the first 1 to 2 months around the cutoff. We apply an imperfect solution to address this concern by excluding 2 months on each side of the cutoff – the donut hole – in all our regressions. Second, we add detailed individual controls such as education, tenure and other pre-unemployment characteristics to help address some of this selection.[26]

The results of each RD estimation are depicted in Figure 2·6 and reported in Table 2.1. The results at the cutoffs at or below age 50 are relatively insensitive to the inclusion of controls and average 0.089 meaning an extra month of PBD results in an extra 0.089 months – or about 3 days – of non-employment [as in Schmieder et al., 2012a].[27] We obtain a similar estimate, if slightly higher, at age 52 and 54. Results for the oldest cutoffs are biased upwards by sorting. Given the sorting it is difficult to draw strong conclusions, but the evidence suggests that the intensive-margin, non-employment effect of UI extensions may increase slightly for workers in their mid-50s relative to workers in their 40s. While these patterns alone are of interest, the policy

[26]We are also exploring using a method due to Gerard et al. [2015], that allows explicitly to apply RDDs in situations where the running variable is manipulated.

[27]Note that there are differences in sample restrictions between this paper and that in that here we restrict to men who tend to be less responsive to UI, omit some industries, and have different pre-unemployment sample restrictions.

relevance of these results is compounded by the presence of extensive margin effects.

2.5.3 Back of the Envelope Calculation

Estimating the total non-employment effect of UI on older workers requires accounting for both the intensive and extensive margin. A credible estimate of this effect needs to allow for individuals to transition between states of job search and unemployment without search, as well as between entering and not entering UI, as a function of PBDs. There is no simple way to credibly combine bunching and regression discontinuity estimates to arrive at this number. If one were to, for example, specify a two-type model where one type always times their entry into retirement via UI and the other type never does but can become unemployed randomly, one could make progress. But the assumption that individuals can be so easily categorized is unrealistic. Moreover, modeling this behavior purely from the individual perspective is at odds with the evident role that firms play in both generating un-desired layoffs and agreed-upon exits. A fully fledged dynamic model of labor supply, combined with a role for firms, is well beyond the scope of this chapter. However, it is the subject of ongoing work.

Here, we instead perform a simple calculation to highlight the importance of the extensive margin under admittedly stark assumptions. Taking the 1935 cohort as a baseline, we consider the effect of reducing maximum unemployment duration by 8 months from 32 months to 24 months for workers aged 50 to 60. We assume that this would shift the entire bunching mass at the 57.33 kink rightward to age 58, without otherwise affecting UI entries. This is somewhat consistent with the fact that the bunching mass is relatively similar for the 1929 and 1935 cohorts (under the polynomial counter-factual). We consider two scenarios for how the bunching mass might respond to this change: in the first, we assume each individual belonging to the entire bunching mass from 57.33 to 60 delays entry by the full 8 months, in the second we assume only individuals in the reduced bunching mass between 57.33 and 58 delay entry, and the rest do not move at all.[28]

We imagine that all non-bunchers (aged 50 to 60) are fired involuntarily and opt to search for jobs. We ignore the idea that involuntary exits closer to the kink might switch from searching to not-searching. We take the counter-factual polynomial estimate to represent the number of people aged 50 to 60 who respond to this intensive margin estimate. Further, we assume that, conditional on unemployment, all these workers respond to a 1 month UI extension by increasing their non-employment durations by 0.089, the average RD estimate for workers aged 40-50.

Under this set up, the intensive margin effect acting on workers aged 50 to 60 is a reduction of 0.712 months of non-employment. The counter-factual accounts for 65% of workers, so the expected intensive margin effect is -0.463 months. The full bunching mass accounts for 35% of workers, so the expected extensive margin effect is -2.795 months. The more conservative bunching mass estimate (in which only those between 57.33 and 58 respond to the change) accounts for 16% of workers, making the expected extensive margin effect -1.277 months. The total effect of time out of work on this 8 month reduction is thus -3.258, or using the conservative version, -1.740. That is, the total non-employment effect is between 2.44 and 4.58 times as large in magnitude as the pure intensive margin.

While a number of things would deflate this estimate – including using a larger estimate of the true intensive-margin effect, using the 1929 counter-factual instead of the 1935 one, and assuming that some bunchers respond by less than the full 8

[28] In practice the conservative approach yields a distribution closer to the actual 1929 distribution. It nevertheless over-estimates bunching between 58 and 60. In addition, the 1935 counter-factual under-estimates the mass of people in UI between 52 and 56 for the 1929 cohort. Together, this means that we may be over-estimating the importance of the extensive-margin effect.

months – it is clear that the extensive margin plays a non-negligible role.

2.6 Conclusion

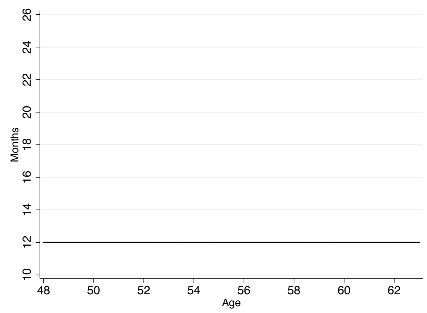
In this paper we document the labor supply effects of UI benefit extensions for workers approaching retirement age. We show that extensive margin responses, that is UI-induced inflows into non-employment, play an important role, and operate in addition to the standard intensive margin UI responses for younger workers that much existing literature has focused on. The combination of intensive and extensive margin responses, as well as voluntary and involuntary inflows into UI, complicates the application of standard non-parameteric estimators such as RD designs and Bunching estimators, but we argue the discontinuities, kinks, and notches induced by the UI and retirement institutions can still be used to learn about labor supply responses.

Our evidence reveals sizable labor supply responses on both the intensive and extensive margin. A naive, back of the envelope calculation suggests that using standard, intensive-margin estimates of the non-employment effects PBD extensions for workers aged 50 to 60 will severely underestimate the non-employment effects of UI. However, such a calculation is also naively simplistic. We drew an unrealistic distinction between types who bunch into UI for retirement and types who only get fired involuntarily and search for a job no matter what. In practice this is too simplistic for many reasons. We have no empirical way of identifying these two types even if they did exist. Moreover, these types are unrealistically separate, with reality surely being more fluid: individuals might voluntarily leave employment but not go into retirement and instead look for a job, and individuals involuntarily fired at later ages might choose to retire after attempting job search.

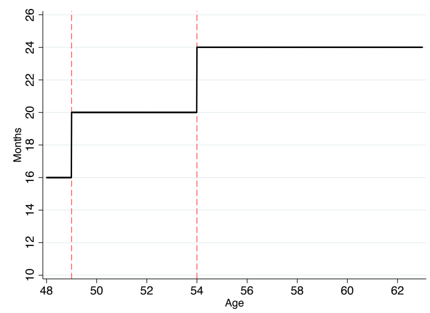
Additionally, we have suggested that firms may play an important role in regulating individuals' inflows into UI after a PBD extension. This does not alter the fact that extensive margin responses need to be taken into account when estimating total non-employment effect, but it does mean that UI entry decisions are not easily separated from firm decisions.

In future work, we plan to obtain a more credible estimate of the total non-employment effect of UI extensions on older workers by specifying and estimating a dynamic labor demand model. This would need to model transitions between employment, unemployment, and retirement and how they are affected by the structure of UI benefits and parameters of the old age pension system. We believe the data in Figures 2·3–2·5 provide compelling moments to match using a simulated method of moments approach. Additionally we will aim to explicitly match our model to our reduced form RD and bunching mass estimates. We will use our 3 decades of policy variation to perform valuable out-of-sample simulations. Rather than striving for maximal realism, we plan to develop a model that allows us to capture the core mechanisms in an internally consistent way, while at the same time abstracting from some other features of the data. For example we will not attempt to model realistic wage evolution, retirement savings decisions, or the role of CLAs, among others, but will instead focus on labor supply decisions of individuals, i.e. whether or not to retire at any given point in time and how hard to search for a job in the case of unemployment. We hope to also model the decision as a joint decision with firms, exploiting firm-level variation in our linked employer-employee data. Model-in-hand, we will be able to answer questions like how much of the stark decrease in employment among older workers in Germany throughout the late 80s and 90s was caused by the UI extensions and how much is likely explained by other factors.

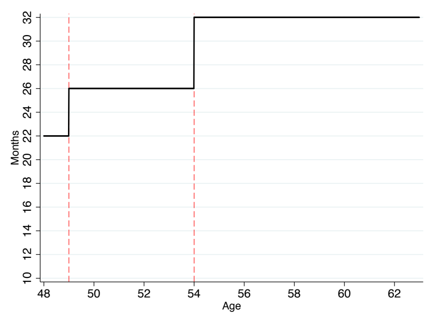
Figure 2·1: Maximum UI PBDs by Age in Different Time Periods in Germany



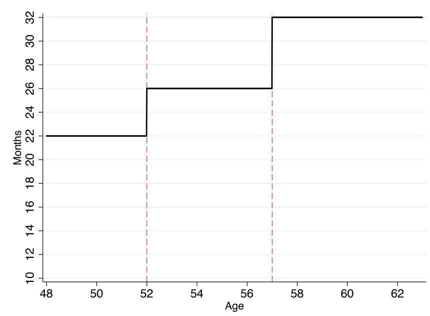
(a) PBD, 1980-1984



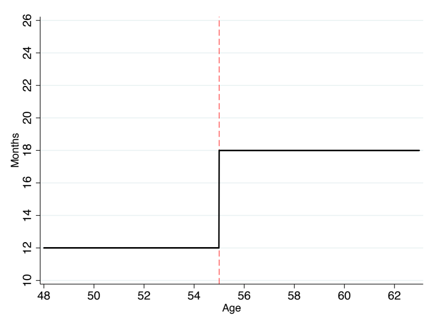
(b) PBD, 1986-June 1987



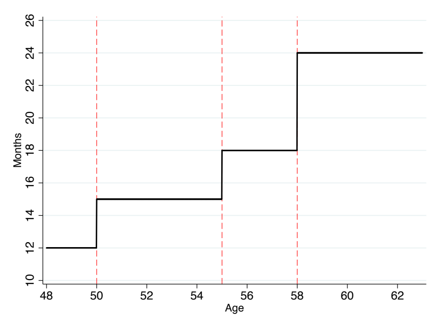
(c) PBD, Jul 1987- Mar 1999



(d) PBD, Apr 1999 - Jan 2006



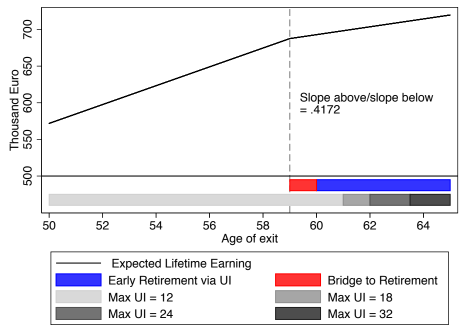
(e) PBD, Feb 2006 - Dec 2007



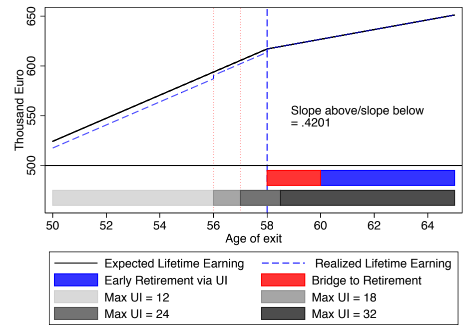
(f) PBD, Jan 2008 - 2010

Notes: This table plots maximum potential benefit durations for unemployment insurance in Germany between 1980 and 2010. We drop the brief 1985 regime in the interest of brevity. Appendix Table B.1 contains more detailed information on each institutional regime, including eligibility requirements and benefit levels.

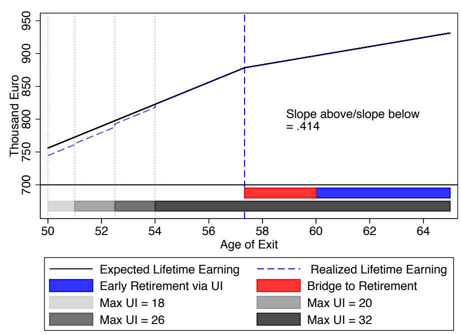
Figure 2·2: Stylized Budget Sets by Exit Age for Different Cohorts



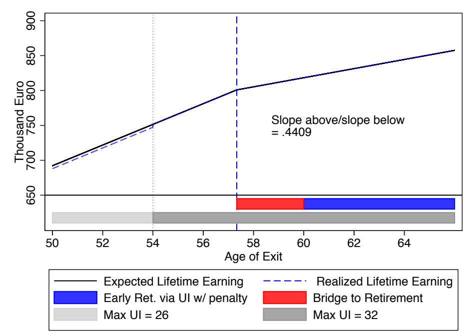
(a) Lifetime Earnings w/ 1 yr PBD, 1924 Cohort



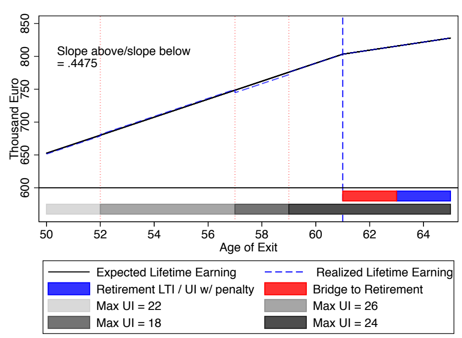
(b) Lifetime Income w/ 2 yr PBD, 1929 Cohort



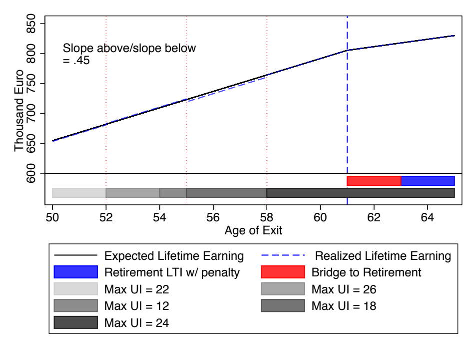
(c) Lifetime Income w/ 32 m PBD, 1935 Cohort



(d) Lifetime Income w/ 32 m PBD, 1941 Cohort



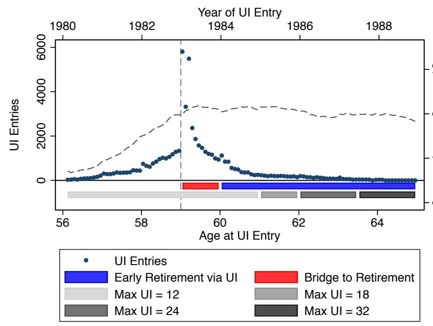
(e) Lifetime Income w/ 2 yr PBD, 1949 Cohort



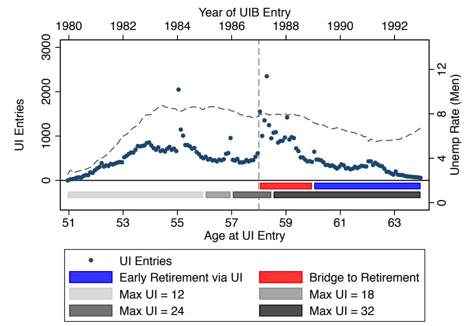
(f) Lifetime Income w/ 2 yr PBD, 1952 Cohort

Notes: These figures contain lifetime budget sets as a function of exit age (into UI and eventually retirement). The ERA is 60 for the first 4 cohorts and 63 for the last two. The NRA (un-penalized retirment age) for retirement via UI is 60 for the first three cohorts, approximately 64 for the second cohort, and 65 for the last cohort. We assume PBD are fixed at 1 year, 2 years, 32 months, 32 months, 2 years, and 2 years respectively.

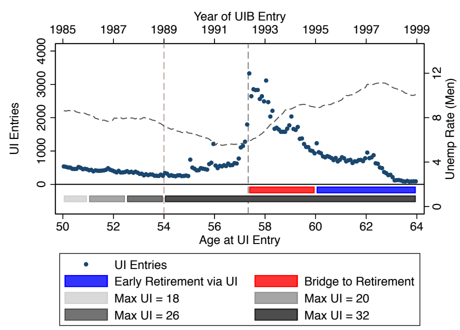
Figure 2·3: UI Inflows by Age for Different Cohorts in Germany, Men



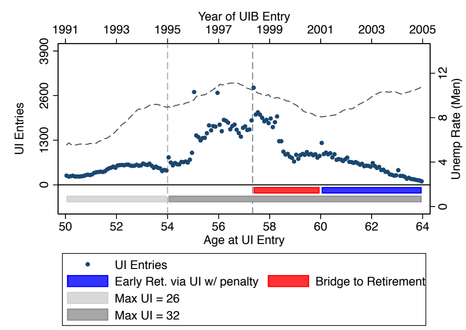
(a) UI Inflows, 1924 Cohort



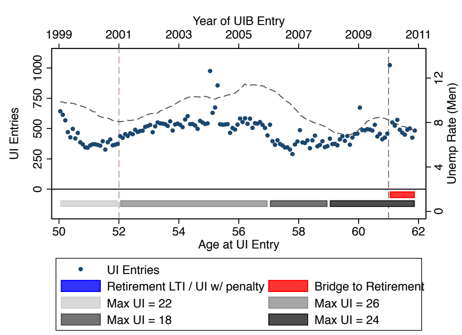
(b) UI Inflows, 1929 Cohort



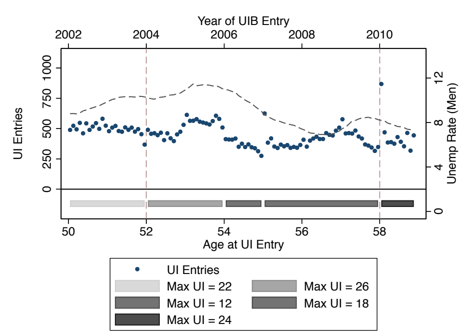
(c) UI Inflows, 1935 Cohort



(d) UI Inflows, 1941 Cohort



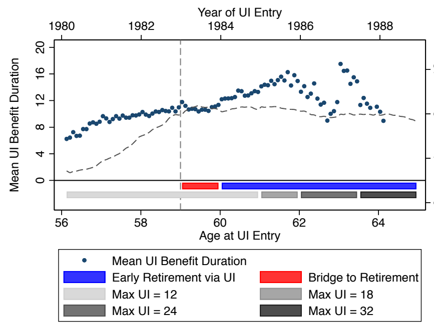
(e) UI Inflows, 1949 Cohort



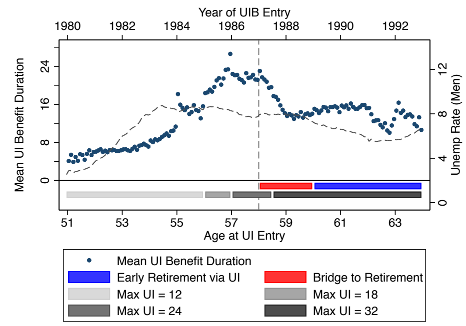
(f) UI Inflows, 1952 Cohort

Notes: These figures plot UI inflows by age for different cohorts of West German Men with full UI eligibility, excluding mining and steal construction. The bin width is 1/12 of a year.

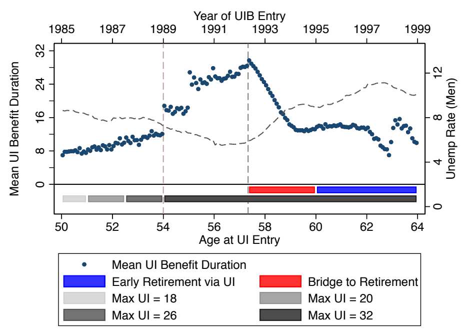
Figure 2·4: Mean UI Benefit Receipt by Age for Different Cohorts in Germany, Men



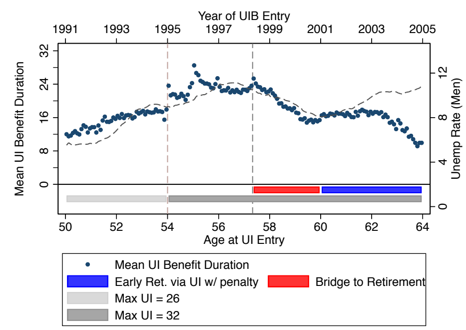
(a) Mean UI Benefit Receipt, 1924 Cohort



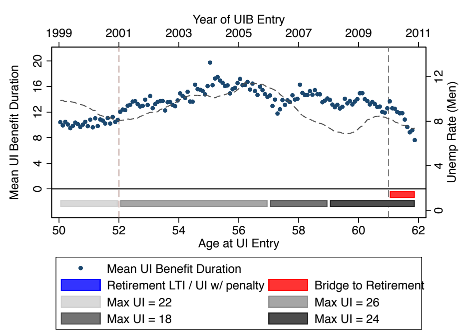
(b) Mean UI Benefit Receipt, 1929 Cohort



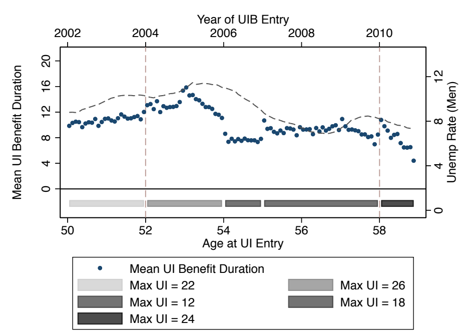
(c) Mean UI Benefit Receipt, 1935 Cohort



(d) Mean UI Benefit Receipt, 1941 Cohort



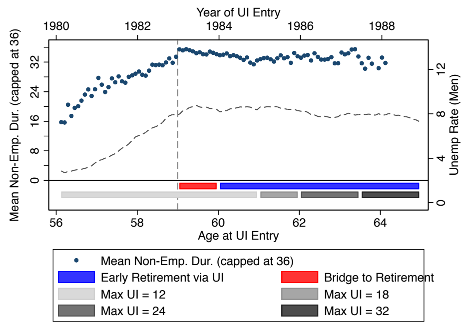
(e) Mean UI Benefit Receipt, 1949 Cohort



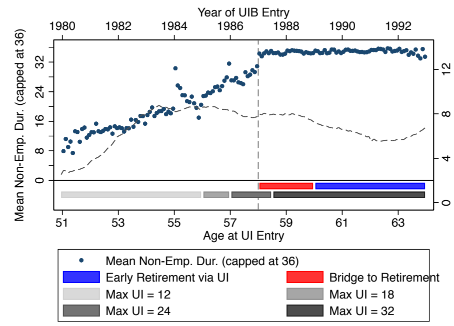
(f) Mean UI Benefit Receipt, 1952 Cohort

Notes: Red dashed lines represent ages at which UI benefit duration increases discontinuously; the black dashed line shows the earliest bridge-to-retirement kink. The red bar under the figure indicates the period over which an individual would receive UI before drawing pension (the blue bar). The different shades of grey represent different maximum PBD eligibility for UI, which can change because of an existing age-cutoff (the red dashed line) or because of an overall UI policy change enacted in that year.

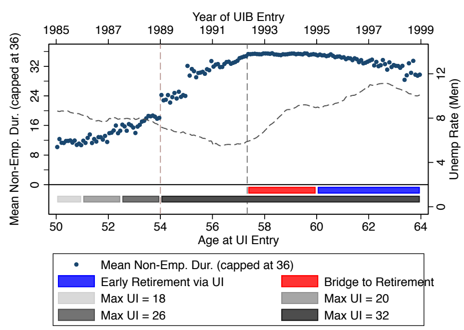
Figure 2·5: Mean Capped Non-Emp. Duration by Age for Different Cohorts in Germany, Men



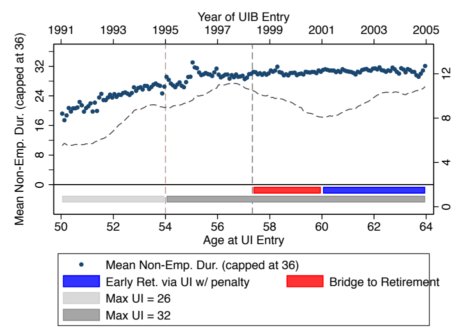
(a) Mean Non-Emp. Duration, 1924 Cohort



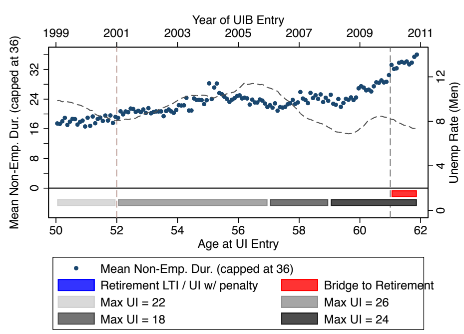
(b) Mean Non-Emp. Duration, 1929 Cohort



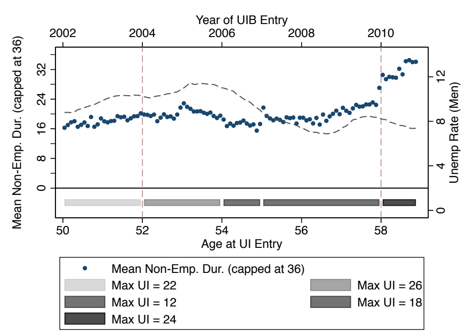
(c) Mean Non-Emp. Duration, 1935 Cohort



(d) Mean Non-Emp. Duration, 1941 Cohort



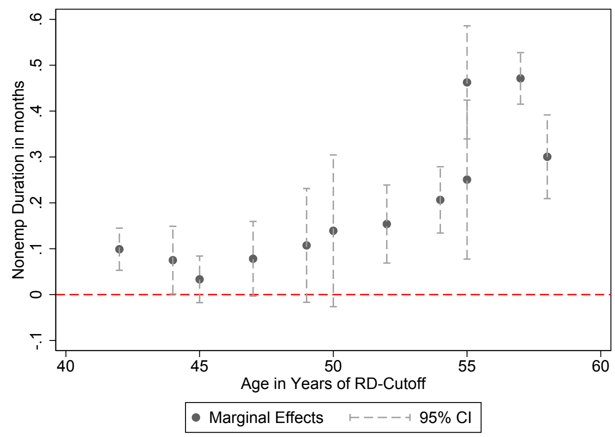
(e) Mean Non-Emp. Duration, 1949 Cohort



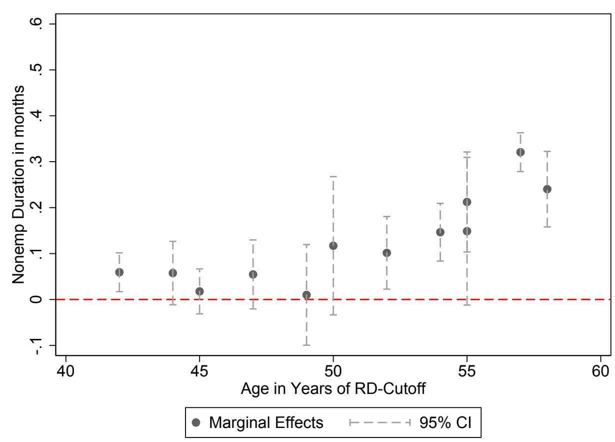
(f) Mean Non-Emp. Duration, 1952 Cohort

Notes: These figures plot mean non-employment duration (capped at 36 months) by age for different cohorts of West German Men with full UI eligibility, excluding mining and steal construction. The bin width is 1/12 of a year.

Figure 2·6: RD Results: 1 month PBD Extension on Non-Employment, Men



(a) Donut-Hole RD Results: Non-Emp. Duration, Men w/out controls



(b) Donut-Hole RD Results: Non-Emp. Duration, Men w/ controls

Notes: These figures contain Regression Discontinuity estimates of the effect of UI potential benefit durations at each age cut-off beginning July 1987. See Table 2.1 for the estimates. We pool all years under the same UI regime. We employ a local polynomial regression with a rectangular kernel and cluster standard errors at the daily level. 95% CI are plotted. All results are divided by the number of months PBD was extended. The bandwidth is 2 years except for the '87-'99 age 49 and 54 cutoffs where it is 1 year on the right due to other discontinuities. We exclude 2 months on each side of the cutoff – the donut hole – to partially address sorting. We also include detailed individual controls. Controls include: pre-unemployment wage, gender, nationality (non-german), experience, wage/occupation/firm-tenure, education, industry (3-digit), firm-size, month and year. Sample Restrictions: West German Men With full eligibility, excluding mining and steal construction.

Table 2.1: The Effect of PBD on Non-employment Durations

		All Exits	
		(1) No Controls	(2) Controls
Jul 1987 - Feb 1999			
Age 42, P: (12-18), ΔP: 6	$\frac{dy}{dP}$	0.099	0.059
		[0.023]***	[0.022]***
	Observations	205478	205478
	Mean of Dep. Var.	15.2	15.2
Age 44, P: (18-22), ΔP: 4	$\frac{dy}{dP}$	0.075	0.058
		[0.038]**	[0.035]
	Observations	200089	200089
	Mean of Dep. Var.	16.1	16.1
Age 49, P: (22-26), ΔP: 4	$\frac{dy}{dP}$	0.107	0.010
		[0.063]*	[0.056]
	Observations	118965	118965
	Mean of Dep. Var.	17.6	17.6
Age 54, P: (26-32), ΔP: 6	$\frac{dy}{dP}$	0.206	0.147
		[0.037]***	[0.032]***
	Observations	150812	150812
	Mean of Dep. Var.	23.9	23.9
Mar 1999- Jan 2006			
Age 45, P: (12-18), ΔP: 6	$\frac{dy}{dP}$	0.033	0.018
		[0.026]	[0.025]
	Observations	181770	181770
	Mean of Dep. Var.	15.2	15.2
Age 47, P: (18-22), ΔP: 4	$\frac{dy}{dP}$	0.078	0.055
		[0.041]*	[0.038]
	Observations	170340	170340
	Mean of Dep. Var.	16.3	16.3
Age 52, P: (22-26), ΔP: 4	$\frac{dy}{dP}$	0.154	0.102
		[0.043]***	[0.040]**
	Observations	149850	149850
	Mean of Dep. Var.	19.9	19.9
Age 57, P: (26-32), ΔP: 6	$\frac{dy}{dP}$	0.471	0.321
		[0.029]***	[0.022]***
	Observations	208831	208831
	Mean of Dep. Var.	28.5	28.5
Feb 2006- Dec 2007			
Age 55, P: (12-18), ΔP: 6	$\frac{dy}{dP}$	0.463	0.212
		[0.063]***	[0.056]***
	Observations	35124	35124
	Mean of Dep. Var.	17.8	17.8
Jan 2008- Dec 2010			
Age 50, P: (12-15), ΔP: 3	$\frac{dy}{dP}$	0.139	0.117
		[0.084]	[0.077]
	Observations	85107	85107
	Mean of Dep. Var.	16.2	16.2
Age 55, P: (15-18), ΔP: 3	$\frac{dy}{dP}$	0.251	0.149
		[0.088]***	[0.082]
	Observations	67199	67199
	Mean of Dep. Var.	19.2	19.2
Age 58, P: (18-24), ΔP: 6	$\frac{dy}{dP}$	0.300	0.240
		[0.047]***	[0.042]***
	Observations	62228	62228
	Mean of Dep. Var.	22.7	22.7

Notes: This table contains Regression Discontinuity estimates of the effect of UI potential benefit durations at each age cut-off beginning July 1987.Standard errors are in brackets and clustered on day level (* P<.1, ** P<.05, *** P<.01)). We pool all years under the same UI regime. We employ a local polynomial regression with a rectangular kernel and cluster standard errors at the daily level. 95% CI are plotted. All results are divided by the number of months PBD was extended. The bandwidth is 2 years except for the '87-'99 age 49 and 54 cutoffs where it is 1 year on the right due to other discontinuities. We exclude 2 months on each side of the cutoff – the donut hole – to partially address sorting. We also include detailed individual controls. Controls include: pre-unemployment wage, gender, nationality (non-german), experience, wage/occupation/firm-tenure, education, industry (3-digit), firm-size, month and year. Sample Restrictions: West German Men With full eligibility, excluding mining and steal construction.

Table 2.2: Estimates of the Bunching Mass at each Bridge-to-Retirement Kink

		(1) Estimated Counter-Factual	(2) UR as Counter-Factual
		Bunching at the Kink Induced by Early Retirement via UI	
1924 Cohort			
Age 59, P: (12), R: (60)	Kink Size ($\frac{w^{above}}{w^{below}}$)	0.417	0.417
	Normalized Bunching ($\frac{B}{h_o(0)}$)	15.826	16.343
	Elasticity (e)	0.026	0.026
1929 Cohort			
Age 58, P: (24), R: (60)	Kink Size ($\frac{w^{above}}{w^{below}}$)	0.420	0.420
	Normalized Bunching ($\frac{B}{h_o(0)}$)	37.914	22.294
	Elasticity (e)	0.061	0.036
1935 Cohort			
Age 57.33, P: (32), R: (60)^A	Kink Size ($\frac{w^{above}}{w^{below}}$)	0.414	0.414
	Normalized Bunching ($\frac{B}{h_o(0)}$)	43.033	64.288
	Elasticity (e)	0.069	0.101
1941 Cohort			
Age 57.33, P: (32), R: (60)^A	Kink Size ($\frac{w^{above}}{w^{below}}$)	0.441	0.441
	Normalized Bunching ($\frac{B}{h_o(0)}$)	14.540	23.204
	Elasticity (e)	0.026	0.041
1949 Cohort			
Age 61, P: (24), R: (63)^A	Kink Size ($\frac{w^{above}}{w^{below}}$)	0.447	0.447
	Normalized Bunching ($\frac{B}{h_o(0)}$)	0.686	0.872
	Elasticity (e)	0.001	0.001

Notes: This table contains estimates of the bunching mass at the bridge to retirement kink for each of the cohorts depicted in Figures 2·3. The bunching mass is estimated in two ways. First, we fit a 7th degree polynomial to the UI entry data excluding a region around the kink point. For the 1924 cohort we exclude 0.3 years to the left and 1 year to the right of the age 59 cutoff; for the 1929 cohort we exclude 0.3 years to the left and 2 years to the right of the age 58 cutoff; for the 1935 cohort we exclude 0.3 years to the left and 2.66 years to the right of the age 57.33 cutoff; for the 1941 we exclude 2.66 years to the left and 2.66 years to the right of the age 57.33 cutoff; for the 1949 cohort we exclude 0.3 years to the left and 0.66 years to the right of the age 61 cutoff. The normalized bunching mass is given by the difference between observed and counter-factual N between the cutoff and the right exclusion region, divided by the average counter-factual in this region. We also use the unemployment rate as a rough counter-factual by scaling it by the ratio of the mean number of UI entries between age 49 (or lowest available) and the kink to the mean unemployment rate. The normalized bunching mass for this is defined analogously. The bin-width is 1/12 of a year. We also show the point estimate for the elasticity of exit age with respect to the net return to work. This is estimated as described in Appendix **??**.

Chapter 3

The Effects of Advance Notice on Displaced Workers' Labor Market Outcomes: Evidence from the WARN Act

3.1 Introduction

It is well established that job displacement has lasting and negative impacts on workers' earnings (Fallick [1996], Jacobson et al. [1993], Ruhm [1991], Sullivan and Von Wachter [2009]). One support program for displaced workers is the mandated requirement of advance notice. It helps workers to begin job search earlier and avoid risky investments and smooth consumptions.

The Worker Adjustment and Retraining Notification Act (WARN) of 1989 is the federal mandated requirement in the U.S. It requires employers to provide the displaced workers with written notice 60 days in advance of a plant closing or mass layoffs. There have been studies in the 1990s investigating the impact of advance notice on the post-displacement outcomes of the displaced workers, exploring the introduction of the federal WARN Act in 1989. They found that lengthy formal written notice is associated with shorter jobless spell and higher post-displacement earnings (Swaim and Podgursky [1990], Nord and Ting [1991], Addison and Portugal [1992], Ruhm [1994b]).

This paper revisits the question — what is the impact of formal written advance notice on the labor market outcomes of the displaced workers — by exploiting the variation across states in advance notification regulations. Since the 2003, nine states have enacted stricter advance notice laws similar to the WARN Act. Those state mini WARN Acts expand the requirements of the federal WARN Act. They provide protection to workers in smaller firms and at smaller layoff events. Figure 3.1 shows the gradual adoption of stricter WARN acts by states between 1995 and 2015. Between 2003 and 2011, 9 states passed mini WARN Acts. This state level variation can help obtain more convincing identification than previous studies. [1]

My estimation sample comprises 12 waves of the Displaced Worker Supplement (DWS) to the Current Population Surveys, from 1996 to 2018. The DWS survey asks people to recall their job history over the past 3 years and collect information on one job loss for each individual. The survey has a question on whether or not the displaced workers have received advance notice and, if the answer is positive, the worker is asked how long before the job loss he or she received notice. [2] These questions have been widely used in the literature to proxy for the enforcement of the WARN Act.

Using the DWS and a differences-in-difference method, I find that the probability of receiving lengthy advance notice is higher in states and years with stricter WARN Acts. However, the probability of getting any notice, formal or informal, decreases in stricter states. Moreover, I find that the displaced workers affected by the mini WARN Acts are 3% more likely to claim unemployment insurance. Conditional on claiming UI, they are less likely to exhaust UI. In terms of post-displacement earnings in the short run, I find the displaced workers affected by the mini WARN Acts have a slightly higher weekly post-displacement earnings, a lower probability of being employed and

[1] Studies on the impact of federal WARN Act were built on variants of the conditional independence assumption. They assume that the comparison of post-displacement outcomes of the notified and the non-notified workers indicates a causal explanation, after controlling for all observable characteristics of the displaced workers. Those are very strong assumptions.

[2] Appendix 3.2 lists the exact Questionnaire questions

have switched more jobs following the displacement.

This chapter contributes to the literature in three ways. First, the state variations in WARN eligibilities provide an exogenous increase in the probability of receiving a written notice two-months in advance. This natural experiment helps me to address selection issues related to notice provision. For example, Ruhm [1994b] suggests that when firms have discretion over the ordering of the displacements, firms will choose to notify workers with relatively dismal post-displacement job prospects. In the fear of impairing the delivery of production contracts, the firm minimizes the damage by lowering the incidence of early quitters. Therefore, notified displaced workers are the ones who are endogenously disadvantaged. In this chapter, I can use the exogenous adoption of stricter state version WARN Acts as a natural experiment and causally identify the impact.

Second, this chapter explores a new policy environment and evaluates the effectiveness of state version WARN acts. This is because all the previous studies related to advance notice were done in the 1990s. As I mentioned earlier, since the 2000s, many states have implemented stricter laws. From a policy perspective, it is worth evaluating those state reforms.

Lastly, jobs are generally less protected in the U.S. than those in OECD countries. The prevalence of "at-will" employment means that employers can often terminate jobs for any reason and with little notice. For instance, according to the U.S. Bureau of Labor Statistics, from 2013 to 2015, of the 3.2 million long- tenured workers displaced, only 45% of them received written an advance notice of the job loss.

This paper relates closely to the literature on the effect of advance notice. This body of literature typically focuses on two post-displacement outcomes of workers: the unemployment duration (Ruhm [1991],Burgess and Low [1992]) and post-displacement earnings (Ruhm [1994b], Ehrenberg and Jakubson [1988], Nord and

Ting [1991]). Ruhm [1994b] suggests that formal written notice is associated with a 10% wage premium for the notified displaced workers relative to their non-notified counterparts. Other studies found no such premium effect (Swaim and Podgursky [1990]). In terms of unemployment duration, studies have shown advanced notice has little impact on the unemployment duration. For example, Jones and Kuhn [1995] conclude that 6 months of advance notice has little impact on long-term unemployment using Canadian survey data. They suggest that instead of assisting disadvantaged workers to reduce their unemployment durations, the notification only benefits relatively more capable workers. Advance notification helps the more capable ones by avoiding what otherwise would be a very short period of unemployment. Addison and Portugal [1992] also found negligible effects of advanced notice on unemployment spell durations for blue-collar workers.

3.2 Data and Institutions

3.2.1 Data

The primary data source is the Displaced Worker Supplement (DWS) to the Current Population Surveys (CPS). The DWS is a biennial survey conducted either in January or February. The DWS collects retrospective information on job histories of displaced workers, who had lost or left a job due to layoffs or shutdowns within the past 3 years of the survey date. [3] It contains information on weekly earnings at the lost job and current job, unemployment insurance take-up and exhaustion rate and reasons for the job loss. Most importantly, it asked the respondents two questions —"Had you been given written notice informing you that you would lose your job?" and "How long before the job loss did you receive that notice?" (See Appendix C.1).

[3] From 1984 through 1992, displaced workers are the ones had lost a job in the last five years. since 1994, displaced workers are the ones had lost a job in the last three years.

This question has been widely used in the literature to proxy for enforcement of the WARN Act.

The main sample comprises 12 waves of DWS surveys, from 1996 to 2018. It consists of displaced workers aged 20 to 65 at the survey date and who lost/left their jobs from 1995 to 2015. I only looked at displaced workers.[4] Moreover, I exclude workers laid off due to terminations of self-employment and seasonal contracts. Workers who expect to be recalled in the next six months are excluded. Workers in military service are also excluded. Workers with missing pre-displacement characteristics are also dropped. There are 28,791 displaced workers over the sample period. Among them, around 3,300 received written notice more than 2 months before the layoff, and around 9,800 received written notice before the layoff.

3.2.2 The Federal WARN Act

The Worker Adjustment and Retraining Notification (WARN) Act was passed on Feb 4 1998, and came into effect on Feb 4 1989. The WARN Act requires employers to provide notice 60 days in advance of plant closings and mass layoffs. Specifically, employers with 100 or more full-time workers are regulated by the WARN Act. There are two triggering conditions[5]: 1) the shutdown of an establishment will result in an employment loss of 50 or more employees during any 30-day period[6]; 2) A mass layoff event will result in an employment loss of at least 33% of the workforce for 50-499

[4] There are two questions related to DWS sample composition — "did you lose a job or leave one due to your plant or company closed or moved, position or shift was abolished or insufficient work during the last 3 calendar years", and "what is the specific reason that you no longer work at the job". If a worker answered "Yes" to the first question, he or she is in my sample (See Appendix C.1).

[5] The firm size thresholds and layoff events magnitude cutoffs provide identifications for future research. I am in the process to get the Longitudinal Employer-Household Dynamics data where firm size and layoff sizes are precisely measured.

[6] This does not count employees who have worked less than 6 months in the last 12 months or employees who work an average of less than 20 hours a week for that employer. These latter groups, however, are entitled to notice"-U.S. Department of Labor

employees, or 500 or more employees. [7] Figure 3.2a illustrates these two triggering conditions: the y axis is the firm size and the x axis is the size of employment loss. We can see that only firms with more than 100 employers are required to WARN their employees. Also, only relatively larger employment losses are covered. The blue region to the right indicates employment losses with more than 1/3 of the firm size.

From 2013 through 2015, there were 3.2 million workers displaced from jobs they had held for at least 3 years. According to the Mass Layoff Statistics, around 10% of the separations are due to permanent worksite closures. Around 95% of mass layoff events are employment losses of between 50 and 500. This suggests that the WARN Act protects a non-trivial percentage of displaced workers. However, only half of all displaced workers received written advance notice, and only 2/3 of displaced workers from plant closing have received written advance notice. [8]

3.2.3 State mini WARN Acts

The California WARN Act California first enacted its own version of the WARN Act in Jan 2003. Compared with the Federal WARN, the California WARN Act (CA WARN) protects a larger share of displaced workers. It lowers the triggering threshold by extending the mandatory notice to smaller size layoff events and smaller firms. There are two main differences. First, the covered employers are the ones with at least 75 persons within the preceding 12 months, including part-time workers. Second, the threshold of employment loss for a mass layoff is much lower in California. Any event that will result in an employment loss of more than 50 is protected by the WARN Act, regardless of the percentage of workforce.[9] Figure 3.2b illustrates the

[7] "This does not count employees who have worked less than 6 months in the last 12 months or employees who work an average of less than 20 hours a week for that employer."-U.S. Department of Labor

[8] Source:Worker Displacement News Release

[9] Source: California Employment Development Department

two triggering condition in California and Figure 3.2c compares the CA WARN Act with the federal WARN Act. Additionally, the CA WARN Act makes the lawsuits more affordable and accessible for displaced workers. The employer who violates the WARN requirements in California is liable for more severe financial liabilities than in other states.

Mini WARN Acts in other States Since 2003, several states enacted more stringent versions of the WARN Act.[10] Different parameters of the WARN Acts vary by states. For example, New York requires companies with 50 or more employees to provide a 90-day notice. Meanwhile, New Hampshire requires layoffs of 25 or more workers to provide notice; however, the notice period is only one week. Compared to California, New Jersey implements the similar rules but requires a severance payment equals to one week of pay for each full year of employment when the employer violates the act. The Iowa WARN Act applies to employers with 25 or more employees but requires a notice period of 30 days.

Some states, such as Oregon and Minnesota, do not have mini WARN Acts but have additional notice obligations. Regulated employees in these states must also report to other workforce development agencies. Meanwhile, Maryland, Massachusetts and Michigan have state-level guidelines which encourage firms to voluntarily provide advance notice to workers. Depending on compliance, these mini WARN Acts are not necessarily more protective than the federal WARN Act.

There are also some states with more lenient requirements. For example, in South Carolina, a notice period of two weeks is required in a plant shutdown only if the employer requires similar notice from employees in the event of a quit. Tennessee's WARN act only applies to employers of between 50 and 99 employees. In Rhode Island, no notice is required, however an employer that liquidates or merges, sells or

[10] See the Legal Appendix for a complete list.

moves out of state must pay workers their wage within 24 hours of the triggering event.

For the baseline analysis, I define states with mini-WARN Acts as the ones that implement mandatory, stricter WARN acts. There are nine such states. They are California(2003), Wisconsin (2005), Illinois (2005), New Jersey (2007), New York (2009), Maine (2007), Iowa(2010), New Hampshire(2010) and Hawaii(2011). Figure 3.1 shows the gradual adoption of stricter WARN acts by states between 1995 and 2015. Between 2003 and 2011, 9 states passed mini WARN Acts.

The gradual adoption of mini WARN Acts provides state-level variation in the probability of being WARNed. The enforcement of the state legislation increases the share of displaced workers who will be protected by the WARN act. It allows me to identify the impact of the state laws. In an initial analysis, I separate states into two groups — states with mini WARN acts and states without mini WARN acts. We would expect a higher chance of getting notice two-months or more in advance for displaced workers in states with mini WARN acts after 2003. Figure 3.3a shows the probability of receiving notice two months or more in advance for those two groups. I also plot the probability of receiving any notice before job loss in Figure 3.3c. The patterns are very noisy.

3.2.4 Issues with the Displaced Workers Surveys

One concern with using the DWS is the potential bias due to sample "attrition". Specifically, what if the person who leaves a job in anticipation of being displaced is also the one who is not identified as a displaced worker in the DWS. For instance, Farber [2001] points out that the distinction between quits and layoffs is not always clear in the DWS. Those early leavers may not consider themselves to have left or lost a job due to plant closings or mass layoff events. Especially, if the early leavers are systematically more likely to receiving written advance notice, then I face sample selection issues. In this case, the WARNed workers in the DWS might be negatively selected to begin with, in comparison to the unWARNed displaced workers. For example, Schwerdt [2011] finds that early leavers face significantly better labor market prospects than the ultimately displaced workers after a job loss due to a plant closure. This negative selection will attenuate the estimated impact of the advance notice. Additionally, if the early leavers are the ones treated by the mini WARN Act, the first stage impact of the mini WARN Acts on the probability of being WARNed is downward biased. This is because the workers who benefit from the mini WARN Act could be the earlier leavers and therefore not included in the displaced worker sample. Unfortunately, I cannot identify the early leavers in the DWS.

Table C.1 compares the main characteristics of the displaced workers and non-displaced workers who have left/lost a job within the past three calendar years of the survey date. It is not surprising that the displaced workers are more likely to get written advance notice. In particular, 12% of the displaced workers receive written notice two months in advance, while only 5% of the non-displaced workers do. Overall, the displaced workers are older, more likely to be female, to be black, and are less likely to have a college degree than the non-displaced ones. Moreover, in terms of pre-displacement job characteristics, displaced workers are less likely to be union members, much more likely to have lost/left a job due to a plant closing, and have much lower weekly earnings than the non-displaced workers. In summary, the displaced workers tend to be disadvantaged compared to other workers who have lost/left jobs for other reasons.

3.3 Descriptive Analysis

3.3.1 Summary Statistics

Using the DWS, I divide the displaced workers into two groups based on whether they have received notice two months in advance or not. The treatment group consists of displaced workers who have received written notification two months in advance. I will refer to it as the WARNed group. The control group is the unWARNed group. This group includes the workers who have received notice less than two months in advance and the workers who haven't received any notice in advance. On average, only 37% displaced workers were given notice of the job loss. Among them, only one third have received notice more than two months before. This is because the majority of jobs in the U.S. are "at-will" employment.

Table 3.1 compares the observable characteristics for the WARNed and unWARNed workers, such as gender, age, marital status, education and job tenure, etc. Except for the probability of being Black, all observed time-invariant and pre-displacement characteristics of those two groups are statistically different. It is worth noting that WARNed workers are older, more likely to be female and married, and with longer job tenure. Table 3.1 also shows that workers are more likely to be WARNed if they were laid off due to plant closings, if they had health insurance at the lost job, if they were members of the union, and were full-time workers and better paid. The unbalanced pre-displacement characteristics suggest potential selection issues. I also look at post-displacement outcomes for those two groups in Table 3.1. On average, the WARNed workers have higher wages at their new jobs. The unemployment insurance take-up rates are similar between the two groups; however, conditional on claiming UI, the WARNed workers are more likely to exhaust the unemployment insurance.

To further investigate the correlations between receiving advance notice and some of the workers' observables, I plot the kernel density of workers' job tenure, age, and hourly wage at last job for the WARNed and unWARNed workers, respectively. This helps compare the differnce in distirbution of the observables. The bottom panel of Figure C.1 shows that the density curve of the hourly wage. The density of the WARNed and the unWARNed intersect around 10 dollaers per hour. Higher than this threshold, the WARNed workers' wage always dominates their counterparts. In other words, if wage reflects productivity when the hourly wage is higher than $ 10, the WARNed workers are preferred in terms of their expected productivity. In addition, because the proportion of workers who received notice is less than a half, according to the Bayesian rule,[11] the probability of WARNed, conditional on wage is less than $10 per hour, is lower than the unWARNed. That is for low paying jobs, workers are less likely to be WARNed. Similarly, workers with less than 3 years tenure are less likely to be WARNed, and workers younger than 40 are less likely to be WARNed.

3.3.2 Potential Selection Biases

Formally, I investigate the relationship of the observables with the probability of receiving written notice two months in advance using the following naive linear regression equation ,

$$WARNed_{it} = \alpha + \gamma X_{it} + \upsilon_{it} \tag{3.1}$$

I also run a simple OLS for the outcome variables.

$$OUTCOME_{it} = \alpha + \beta WARNed_{it} + \gamma X_{it} + \epsilon_{it} \tag{3.2}$$

where X_{it} is a set of covariates that are observable to the econometricians, $WARNed_{it}$

[11] $\forall w < 10$, $Pr(wage = w|WANRed) < Pr(wage = w|unWARNed) \Rightarrow \frac{Pr(wage=w)*Pr(WARNed|w)}{Pr(WARNed)} < \frac{Pr(wage=w)*Pr(unWARNed|w)}{Pr(unWARNed)}$, because $\frac{Pr(WARNed)}{Pr(unWARNed)} < 1$, therefore, $Pr(WARNed|w) < Pr(unWARNed|w)$, $\forall w < 10$.

is a dummy variable equals to 1 if the displaced workers receive written notice two months in advance, and equals to 0 otherwise. Column 2 of Table C.2 lists the regression results. Similarily to Table 3.1, female, married, senior and full-time workers are more likely to receive advance notice. Workers belong to a union and are laid off due to plant closings are more likely to be WARNed. Workers who had health insurance at the lost job and had higher weekly earnings at the lost job are selected into getting notifications. The $OUTCOME_{it}$ are dummies for UI take-up and UI exhaustion rates.

Table C.2 also lists the impact of WARN from a naive linear regression. It suggests that being WARNed is associated with a smaller probability of taking up UI and a larger probability of exhausting UI. However, both within a firm and across firms selections will bias the estimation. On the one hand, when firms have the discretion over the ordering of the displacements, firms will choose to notify workers with relatively bleak job prospects (Ruhm [1994]). It is incentive compatible because firms can minimize layoff damages by lowering the incidence of early quitters — the "leaving the sinking ship" story (Schwerdt [2011]). This kind of negative selection bias will underestimate the impact reducing unemployment duration. On the other hand, large and mature firms are more likely to be self-regulated in providing separation benefits. Displaced workers at those firms are innately more capable and have better job prospects after the layoffs than their counterparts working at smaller firms. This kind of positive selection bias overestimates the impact of advance notice on reducing unemployment duration. Additionally, workers who are older and more experienced have greater bargaining power. They might be more able to secure better separation benefits.

Moreover, firms provide written notification are also more likely to offer other job search assistance, such as more generous severance payments. The UI literature has shown better unemployment benefits reduce job search incentives, and therefore prolong the unemployment duration. It's difficult to disentangle the impact of written notice from the impact of other job search assistance.

3.4 Empirical Strategy

In this paper, I explore the fact that the stricter mini WARN Acts are adopted in different states and years. My main empirical strategy is to examine the labor market outcomes of displaced workers in state and year under the regime of stricter WARN regulation, relative to outcomes in years prior to the enforcement of the mini WARNAct in that state, and relative to the other states without mini WARN Acts during the same period. I estimate the differences-in-difference of the following form:

$$OUTCOME_{ist} = \alpha + \beta \times REGIME_{st} + \gamma X_{ist} + \mu_t + \eta_s + \epsilon_{ist} \tag{3.3}$$

In this equation, $OUTCOME$ is one of the measures of labor market outcomes that might be affected by the enforcement of mini WARN Acts. $REGIME$ is an indicator variable for the mini WARN Acts. It equals one if a stricter WARN Act is enacted in a given state and year and zero otherwise. X_{ist} is the characteristics of displaced worker i in state s in year t. I also include a year fixed effect μ_t and a state fixed effect η_s.

The primary advantage of this research design is that the gradual adoption of the mini WARN Acts provides exogenous variation across states and year. Additionally, I use state fixed effects to capture the responses while holding the time-invariant state labor market situation constant. However, due to the lack of firm size information in the DWS, this design cannot measure the within-state variation in the WARN treatment. Ideally, if I had information on the employment loss of each layoff event and the firm/establishment size, I could measure the changes in outcome within a state and across the WARN eligibility cutoffs.[12]

The identifying assumption of a differences-in-difference setting is that the only reason for changing outcomes in the state year with stricter WARN acts, relative to other states in the same year, is the enforcement of stricter WARN act. In this setting, I can rule out concerns about fixed differences between the treated states and the untreated states through the state fixed effects; and I can rule out the differential time trends by state by controlling the interaction of state dummy and year. There are a couple of potential concerns. First, there might be other concurrent reforms/changes. For example, it could be that states with high unemployment rates are more likely to implement stricter WARN acts to provide additional protections to displaced workers. Poor labor market conditions will prolong displaced workers' unemployment duration. In the regression, I control for the state-level unemployment rate to account for this potential endogeneity.

Second, if the states that reformed their WARN acts are systematically better at enforcement, the estimated impact might be downward biased. For example, the firms in California could have already voluntarily de-facto enforced stricter notification norm before the legal term was enacted. Similarly, states without mini WARN Acts are not necessarily the ones provide fewer separation protections. Firms could have already voluntarily de-facto notify the displaced workers two-months in advance of the job loss. One way to partially account for this concern is to carefully investigate the dynamics in the probability of WARNed and the outcomes before the enforcement of the mini WARN Act to see if there is any evidence of de-facto enforcement preceding the law change. Nevertheless, I cannot fully rule out this "measurement" error.

[12] I am in the process of applying for the Longitudinal Employer-Household Dynamics data from the U.S. census bureau. The LEHD contains detailed firm side information and precise measure of the unemployment duration.

Lastly, the state-specific labor protection laws may also cause migration from state to state. Workers might migrate to states with better labor protection laws to avoid the adversity of unprepared layoffs. In this paper, I ignore this second-order response.

3.5 Main Findings

3.5.1 The Impact on the Probability of Receiving Written Notice in Advance of Layoffs

Since 2003, some states start to implement stricter WARN requirements. Figure 3.3 plots the probability of receiving written notice two months, at least one month in advance and receiving any notice for workers laid off in years from 1995 to 2015. The blue triangle dots are the average probability for states with mini-WARN Acts and the red diamond dots are the means for states without mini WARN Acts. The dash-dotted lines are the 95% confidence intervals.

We can visually see an increase of the probability of getting two-months advance notice starting from 2003 and the gap disappears around 2013. There is no visual difference in terms of getting written notice 1-2 months in advance. The probability of getting notice less than 1 month in advance or no notice at all are lower in mini-WARN Act states, but the difference is not significant.

I formally estimate the impact of the mini-WARN Acts on the probability of receiving written notice, using the following equation:

$$WARNed_{ist} = \alpha + \beta REGIME_{st} + \gamma X_{ist} + \mu_t + \eta_s + \epsilon ist \tag{3.4}$$

I predict the probability of receiving written notice in advance for individual i in state s and laid off in year t. The estimation results can be found in Table 3.2. Each presents a linear model of receiving written notice more than 2 months in advance, at least one month in advance and receiving written advance notice on the state and year dummies, controls (in even-numbered columns) and the REGIME dummy. I find that the state WARN Acts increases the probability of getting notice more than two months in advance by 0.08%, with a standard error of 0.008.

Counterintuitively, the displaced workers in states and years with stricter WARN Acts are less likely to receive advance written notice. In other words, the probability of receiving any advance notice decreases by 2%. This result is puzzling. One possible explanation is that the early leavers who are not observed in the DWS are the ones who benefit from the mini WARN Acts. Therefore, the states and years affected by the mini WARN Acts have smaller percentages of workers receiving advance notice due to sample "attrition". Another possible story is that firms under a stricter regime manipulate the layoff size and avoid losses that are large enough to trigger the advanced warning condition. Therefore, there are fewer displaced workers protected by WARN. I cannot test the plausibility of this story in the DWS, because firm sizes and layoff sizes are not observed.

I further investigate the validity of the first stage results by adding leads and lags into equation 3.4. Figure 3.5a plots the estimated coefficients and confidence bands on the probability of being WARNed and each of the 11 dummy variables which indicate 5 years before the reform, the year of the reform, and 5 years after the reform. The coefficients for dummy variables for three years before the registration of mini WARN are significantly positive. The coefficients for the lagged years are larger but also noisier. Figure 3.5b plots the regression estimates for the outcome variable, receiving any written notice. The coefficients for the lead years are slightly above zero but not significant. Overall, the first stage results are very noisy using the fixed effect regression.[13]

[13] Ideally, I would like to investigate the variation in the enforcement of WARN act at the requirement threshold, such as firms with more than 100 employees. This threshold setting allows me to analysis the within state variations and are potentially more interesting and have more power.

3.5.2 The Impact on Displaced Workers' UI Take-up and Exhaustion Rates

Using Equation 3.3, I estimate the reduced form impact of the mini-WARN Acts on two main outcome variables: UI take-up rate and UI exhaustion rate. Figure 3.4 plots the UI take-up rate and UI exhaustion rates from 1995 to 2015 for states with mini WARN Acts and states without mini WARN Acts. Visually, we cannot see any notable difference between these two groups after 2003.

Table 3.3 displays the regression results for the UI take-up rate and UI exhaustion rate. Each column presents a linear model of the outcome variables on state and year dummies and the $REGIME$ dummy. I find a significant increase in the UI take-up rate associated with being displaced in states with stricter WARN acts. Specifically, the displaced workers affected by the mini WARN Acts are 3% more likely to claim UI. This is intuitive, as the WARN act often also require the firms to notify a Rapid Response team. The Rapid Response team is a government agency which provides a range of services to workers prior to the layoff, such as job search assistance, applying for unemployment insurance, information on health benefits and pensions, etc. [14] Note that the positive relationship of WARN and UI take-up rates is the opposite to the naive association I found in section 3.3.2. However, it is important to keep in mind that the first stage estimates are not very strong.

I also find that conditional on claiming UI, the displaced workers affected by the mini WARN acts are less likely to exhaust UI benefits. However, the impact is not significant. Interestedly, even though the coefficient is not significant, the negative sign is consistent with job search theory. According to standard job search theory, the effect of advance notice on unemployment duration should be either zero or negative.[15]

[14] Rapid Response: Solutions for Business

[15] Table C.2 displays the naive old regression results. The results suggest that receiving a two-month affiance notice is positively associated with UI exhaustion rate among the UI claimers. This

Again, I investigate the dynamic of impacts by adding leads and lags into equation 3.3. Figure 3.6a and Figure 3.6b plot the estimated coefficients and confidence bands on the UI takeup and UI exhaustion rate. Each of the 11 dummy variables which equal one for 5 years before the reform, year of the reform, and 5 years after the reform. The results do not support the parallel trend assumption for the DID setting. In fact, the impacts on UI take-up rates one year and four years before the reform are significantly negative. Conditional on being UI claimers, the impacts on UI exhaustion rate are very noisy. The pattern suggests that one to two years prior to the reform, the displaced UI claimers in mini WARN states are already having better job search outcomes. The negative association of mini WARN with UI exhaustion rate or unemployment durations could be caused by the pre-existing systematically better labor market outcomes of those Mini WARN states.

3.5.3 The Impact on Displaced Workers' Post-displacement outcomes

Table 3.4 and Table 3.5 list the regression results for the five post-displacement labor market outcomes- log weekly earnings at current job, earnings loss at current job, probability of being employed within 3 years of job loss, hours worked at the current job, and number of jobs since the job loss. The impact of mini WARN Acts on log weekly earnings at the current job and earnings loss at the current job are significant. When I look at the probability of being employed within 3 years of job loss, I find a significant negative impact of 3%. The effect on hours worked at the current job is not significant as well. And I find that workers who are protected by

contradicts the standard job search theory. The impact of advance notice on unemployment duration should be either zero or negative. One explanation for the positive sign could be market screening. Workers in the unWARNed pool have less time to search for a job compared to the WANRed ones. More capable workers escape faster from the unemployment pool if the labor market is competitive. Therefore, the less capable ones are left over into the pool of UI claimers. The WARNed UI claimers have been given longer time to search but failed to find a job. Jone and Ruhm (1995) points out that advance notice systematically benefits the workers who are capable of finding a job prior to the layoffs.

stricter WARN acts have higher job turnover. They have worked at more jobs since job displacement.

Table 3.6 and Table 3.7 show the impact on log weekly earnings at the current job and probability of being employed within 3 years of job loss for UI claimers and non-UI claimers, separately. I find a positive and larger impact on log weekly earnings for the UI claimers. This suggests that advance notice affects labor outcomes via the Rapid response team. Displaced workers with access to UI become pickier and have higher reservation wages, and therefore have higher post-displacement earnings.

However, the workers who are protected by stricter WARN Acts are less likely to be employed. The negative impact for UI claimers is larger. They are 5% less likely to be employed within three years of displacement. The impact for non-UI claimers is positive and insignificant. In terms of the number of jobs since displacement, the impact of WARN Acts is twice as large for non-UI claimers as UI claimers. Table 3.8 suggests that among displaced workers who did not claim UI, the ones with advance notice are more likely to switch jobs.

3.6 Conclusion

Many policymakers advocate for employment protections for displaced workers to help them overcome the hardship of unemployment. Advance notification is one of the separation benefits that buffer the shocks of job loss. However, most papers on the impact of advance notice have focused on the effectiveness of the federal WARN Act. Since the early 2000s, some states have started to gradually adopt stricter WARN Acts. I explore the introduction of state WARN Acts and use a differences-in-difference method. I examine the labor market outcomes of displaced workers. In other words, I compare the displaced worker's labor market outcomes in states and years under the regime of stricter WARN regulation, relative to outcomes in years prior to the enforcement of the mini WARN Act in that state and relative to the other states without mini WARN Acts during the same period.

Unfortunately, the state laws suffer from a weak first stage problem. This is possible because I cannot target workers who are from firms and layoff events that are directly protected by the WARN Acts, due to lack of information on firm sizes and layoff sizes. Moreover, because in the DWS I cannot distinguish job loss and job quits. It is plausible that the unobserved early leavers are also the ones who benefit the most from the WARN Act. Therefore, I cannot detect a strong first stage impact in my sample. With these concerns in mind, I find that the displaced workers affected by the mini WARN acts are 3.3% more likely to claim unemployment insurance and conditional on claiming UI, they are less likely to exhaust UI. Moreover, the displaced workers affected by the mini WARN Acts have a lower probability of being employed within 3 years of the job loss and have switched more jobs since the job loss.

In future work, I plan to obtain a more credible estimate of the impact of WARN Acts by exploring the WARN eligibility cutoffs. The firm size thresholds and layoff events magnitude cutoffs in Figure 3.2 provide more precise identifications. This allows me to focus on within state variations and helps rule other confounding factors. I plan to use the Longitudinal Employer-Household Dynamics data (LEHD) to explore this possibility. Moreover, states list all mass layoff events and plant closings with advance notice online. The company names, plant address, number of workers affected and reasons for the event are public information available online. This helps me to verify the enforcement of the WARN Act using the government disclosure information on WARN.

Table 3.1: Comparison of WARNed and unWARNed characteristics

	All Displaced		t-test	UI Claimers		p-value
	WARNed	unWARNed		WARNed	unWARNed	of difference
	(1)	(2)	(3)	(4)	(5)	(6)
Number of observations	3304	25487	-	1487	11482	-
Panel A: Outcome variables						
UI take-up rate	0.46	0.45	0.3325	1	1	
	(0.003)	(0.008)				
UI exhaustion rate				0.42	0.38	0.002
				(0.004)	(0.011)	
Employed at time of survey	0.81	0.72	0.00	0.72	0.65	0.00
	(0.006)	(0.003)		(0.011)	(0.004)	
Weekly earnings at current job	943.6	795	0.00	859.5	852.1	0.76
	(16.03)	(5.27)		(23.8)	(8.50)	
Panel B: Demographics						
Age	42.87	40.25	0.00	44.64	42.89	0.00
	(0.174)	(0.066)		(0.250)	(0.100)	
Female	0.50	0.43	0.00	0.52	0.43	0.00
	(0.007)	(0.003)		(0.011)	(0.004)	
Black	0.11	0.11	0.99	0.12	0.11	0.028
	(0.001)	(0.005)		(0.007)	(0.0026)	
Married	0.59	0.51	0.00	0.61	0.55	0.00
	(0.007)	(0.003)		(0.011)	(0.004)	
Education (in years)	13.73	13.15	0.00	13.4	13.31	0.098
	(0.037)	(0.013)		(0.054)	(0.019)	
High school graduates	0.65	0.57	0.00	0.6	0.59	0.564
	(0.007)	(0.003)		(0.011)	(0.004)	
College graduates	0.33	0.25	0.00	0.28	0.26	0.161
	(0.007)	(0.002)		(0.010)	(0.004)	
Panel C: Pre-displacement variables						
Tenure at lost job	8.40	4.40	0.00	9.60	5.36	0.00
	(0.131)	(0.033)		(0.200)	(0.050)	
Union member (%)	0.14	0.07	0.00	0.18	0.10	0.00
	(0.005)	(0.001)		(0.009)	(0.003)	
Part-time worker (%)	0.12	0.18	0.00	0.06	0.07	0.08
	(0.002)	(0.005)		(0.006)	(0.002)	
Plant closing (%)	0.62	0.31	0.00	0.65	0.30	0.00
	(0.007)	(0.003)		(0.011)	(0.004)	
Had health insurance at lost job (%)	0.71	0.49	0.00	0.80	0.65	0.00
	(0.003)	(0.007)		(0.009)	(0.004)	
Weekly earnings at lost job	1000	849	0.00	1012.6	989.6	0.24
	(12.96)	(4.41)		(18.33	(6.61)	

Note: The sample includes all displaced workers from the survey year 1998 to 2016. Weekly earning at the lost job and current job are inflation-adjusted. Standard errors in parentheses. The p-value for testing the null hypothesis: the WARNed and unWARNed have the same characteristics are reported in column 3 and 6.

Table 3.2: The Estimated Impact of Mini WARN Acts on Receiving Written Notice

Outcome variables	> 2 months (1)	(2)	≥ 1 month (3)	(4)	Any notice (5)	(6)
Mini WARN Act	0.0098	0.0080	0.00465	0.00325	-0.0202*	-0.0229***
	(0.0090)	(0.0084)	(0.0075)	(0.0084)	(0.0095)	(0.0063)
Sample mean	0.11		0.22		0.34	
	(0.32)		(0.42)		(0.47)	
Controls	No	Yes	No	Yes	No	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
State F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28791	28666	28791	28666	28791	28666
R^2	0.009	0.104	0.007	0.139	0.007	0.120

Standard errors clustered at the state level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3.3: The Estimated Impact of Mini WARN Acts on UI Take-up and UI Exhaustion Rate

Outcome variables	UI Take-up rate (1)	(2)	UI Exhaustion rate (3)	(4)
Mini WARN Act	0.0206	0.0303*	-0.00958	-0.00821
	(0.0142)	(0.0115)	(0.0214)	(0.0209)
Sample mean	0.45		0.38	
	(0.50)		0.49	
Controls	No	Yes	No	Yes
Year F.E.	Yes	Yes	Yes	Yes
State F.E.	Yes	Yes	Yes	Yes
Observations	28950	28821	12979	12927
R^2	0.044	0.175	0.030	0.056

Standard errors clustered at the state level. † $p < 0.10$ * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3.4: The Impact of Mini WARN Acts on Earnings at New Job

Outcome variables	Log Weekly Earnings at New Job		Earnings Loss	
	(1)	(2)	(3)	(4)
Mini WARN Act	-0.0167	0.00736	0.0480	0.0178
	(0.0245)	(0.0201)	(0.0326)	(0.0322)
Sample mean	6.35 [812]		0.92 [-78]	
	(1.01) [694]		1.74[560]	
Controls	No	Yes	No	Yes
Year F.E.	Yes	Yes	Yes	Yes
State F.E.	Yes	Yes	Yes	Yes
Observations	19201	19201	29058	29058
R^2	0.050	0.050	0.021	0.021

Standard errors clustered at the state level. † $p < 0.10$ * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3.5: The Estimated Impact of Mini WARN Acts on the Probability of being Emoployed at Survey Date

Outcome variables	Probability of being employed		Hours worked at current job		Number of jobs since job loss	
	(1)	(2)	(3)	(4)	(5)	(6)
Mini WARN Act	-0.0295*	-0.0294**	-0.260	-0.128	0.0885*	0.0921*
	(0.0123)	(0.0101)	(0.510)	(0.538)	(0.0367)	(0.0383)
Sample mean	0.74		35.3		1.04	
	(0.43)		(10.5)		(1.21)	
Controls	No	Yes	No	Yes	No	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
State F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25924	25795	6560	6525	28269	28135
R^2	0.057	0.093	0.030	0.145	0.025	0.040

Standard errors are clustered at the state level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3.6: The Estimated Impact of Mini WARN Acts on Log Weekly Earnings at New Job

Groups	All		UI Claimers		Non-UI Claimers	
	(1)	(2)	(3)	(4)	(5)	(6)
Mini WARN Act	-0.017	0.0075	0.057	0.045	-0.065*	-0.003
	(0.025)	(0.020)	(0.039)	(0.031)	(0.028)	(0.030)
Sample mean	6.35 [812]		6.40 [853]		6.31 [787]	
	(1.01)[694]		(1.02)[697]		(0.99)[692]	
Controls	No	Yes	No	Yes	No	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
State F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19201	19100	7679	7650	11437	11377
R^2	0.050	0.277	0.061	0.228	0.049	0.333

Standard errors are clustered at the state level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3.7: The Estimated Impact of Mini WARN Acts on the Probability of being Employed at Survey Date

Groups	All		UI Claimers		Non-UI Claimers	
	(1)	(2)	(3)	(4)	(5)	(6)
Mini WARN Act	-0.0295*	-0.0294**	-0.0518***	-0.0513***	0.00309	0.00688
	(0.0123)	(0.0101)	(0.0124)	(0.0113)	(0.0152)	(0.0126)
Sample mean	0.74		0.66		0.81	
	(0.44)		(0.47)		(0.40)	
Controls	No	Yes	No	Yes	No	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
State F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25924	25795	11729	11684	14084	14012
R^2	0.057	0.093	0.073	0.103	0.037	0.097

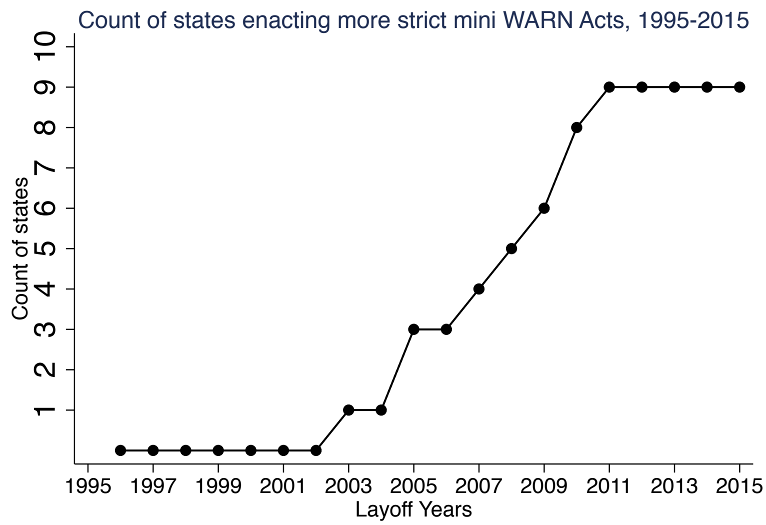
Standard errors are clustered at the state level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3.8: The Estimated Impact of Mini WARN Acts on Number of Jobs since Displacement

Groups	All		UI Claimers		Non-UI Claimers	
	(1)	(2)	(3)	(4)	(5)	(6)
Mini WARN Act	0.0885*	0.0921*	0.0558	0.0657	0.129***	0.129***
	(0.0367)	(0.0383)	(0.0569)	(0.0621)	(0.0346)	(0.0349)
Sample mean	1.05		0.93		1.14	
	(1.21)		(1.1)		(1.27)	
Controls	No	Yes	No	Yes	No	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
State F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28269	28135	12723	12672	15447	15372
R^2	0.025	0.040	0.036	0.053	0.022	0.037

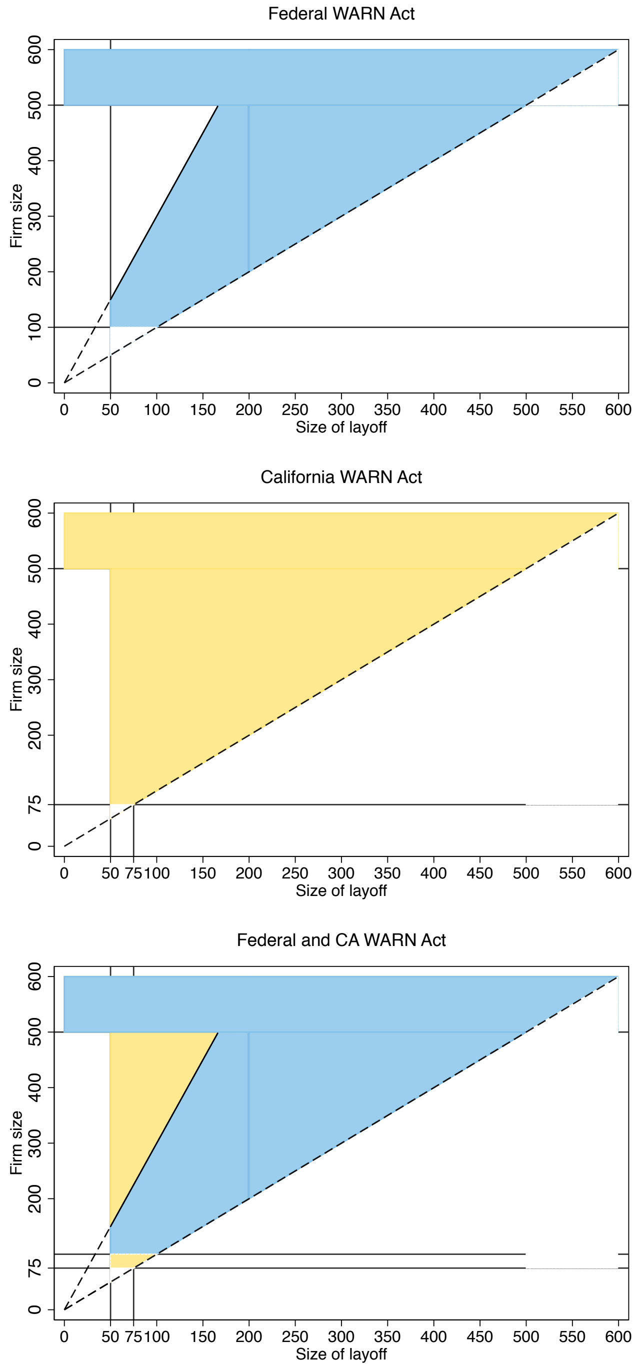
Standard errors are clustered at the state level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure 3·1: Count of states enacting more strict mini WARN Acts, 1995-2015



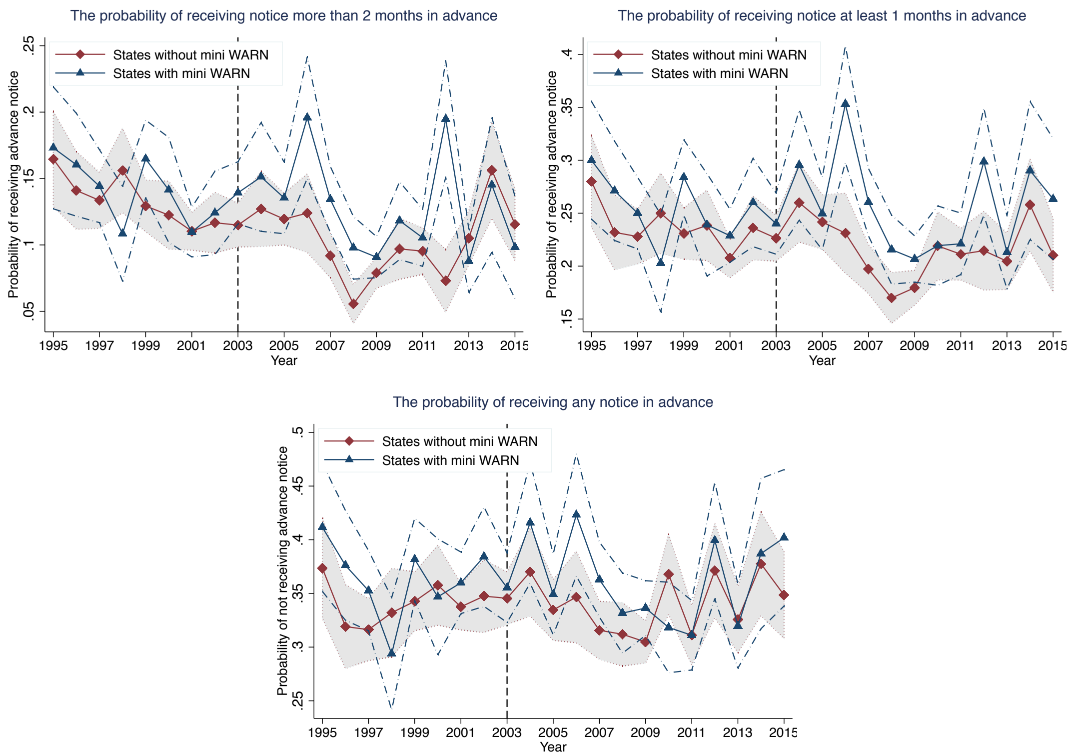
Source: Author's construction.

Figure 3·2: Federal WARN Act and California WARN Act



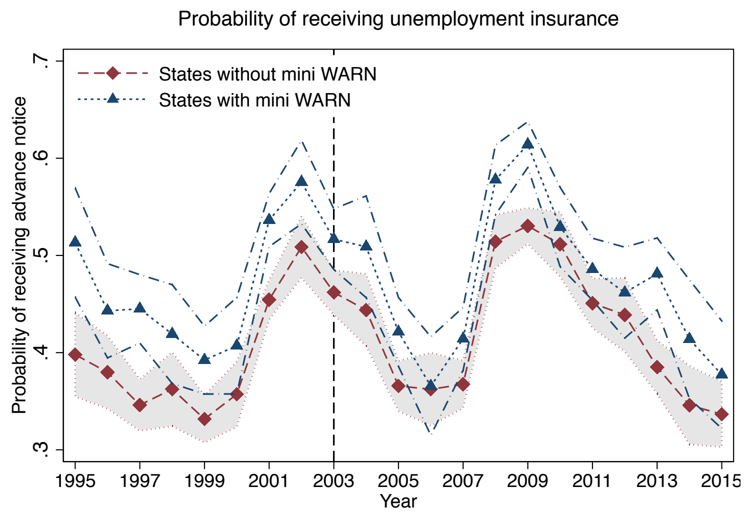
Source: Author's construction.

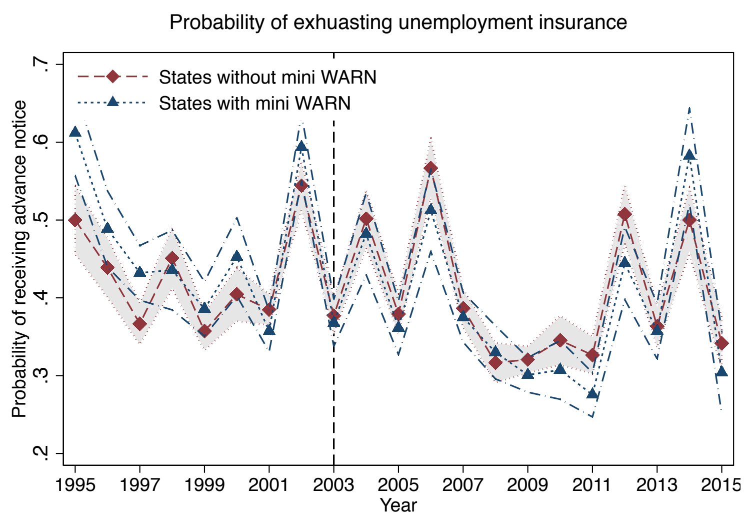
Figure 3·3: Probability of Receiving Advance Notice by Year and by States



Notes: Figure 3.3 plots the probability of receiving notice more than 2 months in advance, at least 1 month in advance, and receiving any notice in advance for states with mini WARN act and states without mini WARN acts. The dashed and dotted lines are the 95% confidence intervals. The black vertical line shows the first year when states started implementing mini WARN Acts.

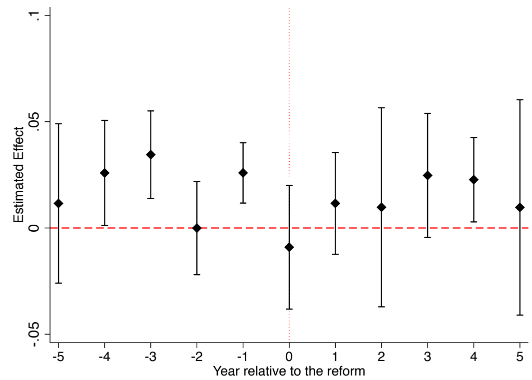
Figure 3·4: UI take-up rate and UI exhaustion rate by Year and by States



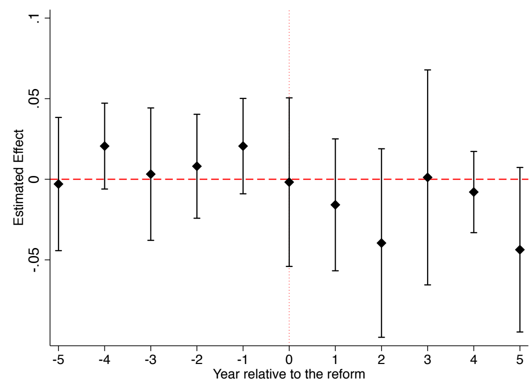


Notes: Figure 3.4 plots the probability of claiming UI for states with mini WARN act and states without mini WARN acts. The dashed and dotted lines are the 95% confidence intervals. The black vertical line shows the first year when states started implementing mini WARN acts.

Figure 3·5: Pre-reform and Post-reform Probability of Receiving Advance Notice



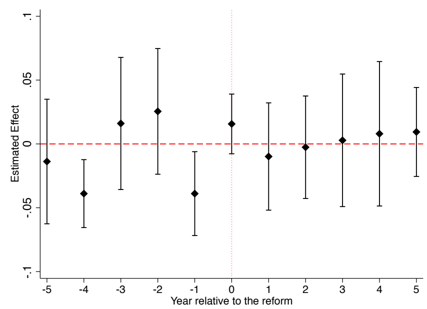
(a) Receiving written notice more than 2 months in advance



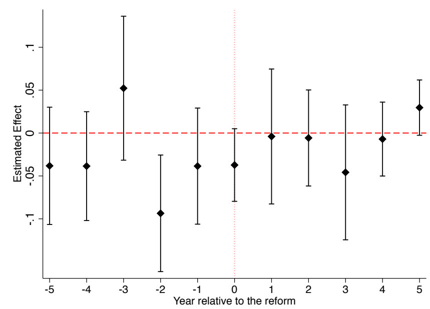
(b) Receiving any written notice in advance

Notes: Figure 3.5 plots the estimated coefficients and confidence bands on the probability of receiving written notice more than 2 months in advance and probability of receiving any written notice in advance and each of the 11 dummy variables which equal one for 5 years before the reform, year of the reform, and 5 years after the reform.

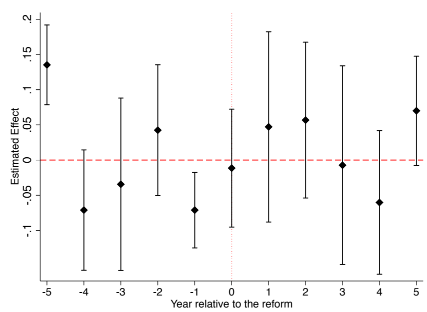
Figure 3·6: Leads and Lags of the Estimated Impact of the Mini WARN Act



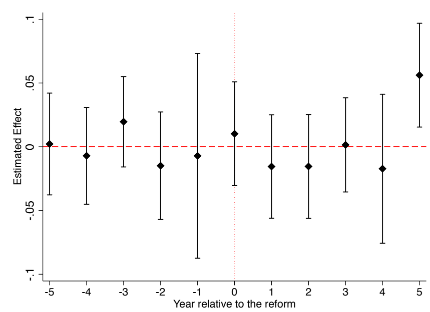
(a) UI take up rates



(b) UI exhuastion rates



(c) Log Weekly Earning Post-displacement



(d) Emploment Rate within 3 years of Displacement

Notes: Figure 3.6 plots the estimated coefficients and confidence bands on the UI take-up rates, UI exhaustion rate, log weekly earnings at current job and employment rate within 3 years after displacement The 11 scatter bins are the coefficient for dummy variables which equal one for 5 years before the reform, year of the reform, and 5 years after the reform.

Appendix A

Appendix — Chapter 1

A.1 Additional details on institution

A.1.1 Example of Pension Benefit and Subsidy Calculation

Below is an example of a hypothetical pensioner who started contributing to the system since 1983 and claimed a pension in 2015. Her contribution period is 33 years. For each year of work, some earnings points are accumulated. For incidence, in 1985, she earned 1200 euros per month, and the average monthly wage of all insured was 1200 as well. Therefore, 1 EP was credited. In 1986, her wage income was half of the average. Therefore, 0.5 EP was credited. The sum of EP between 1983 and 2015 was 18.55. The average annual EP at retirement was 0.56. Pension value in 2015 was 30 euros. Her pension benefits without the subsidies were 556.5 euros per month. The sum of EPs before 1992 was 5.75 EPs. The years contributed before 1992 was 10 years. Her average annual EP before 1992 was 0.575. I also assume this hypothetical pensioner has one child. Therefore, the condition of 35 years credible periods is satisfied. Because both her average annual EP before 92 and average annual EP at retirement were smaller than 0.75, she was entitled to the subsidy for low pay workers. The subsidy size was (0.75-0.575)*10=1.75, which was equivalent to 52.5 euros in 2015. Her total pension benefits was around 600 euros per month.

An Example of Pension Benefit Calculation

Year	1983	1984	1985	1986	1987	1988	1989	1990	1991	**1992**	...	2013	2014	2015
Monthly Wage	500	750	1200	600	400	500	500	500	500	500	500	1000	800	1200
Average Monthly Wage of all Insured	1000	1000	1200	1200	800	1000	1000	1000	1000	1000	1000	1000	1000	1200
EP	0.5	0.75	1	0.5	0.5	0.5	0.5	0.5	0.5	0.5	10	1	0.8	1

Sum of EP	18.55	Sum of EP pre 92	18.55
Mean EP	0.56	Mean EP pre 92	0.56
AR in 2015	30	Subsidy in EP	1.75
Monthly Pension Benefit	**556.5**	AR in 2015	30
Monthly Pension Benefit + Subsidy	**609**	**Monthly Pension Benefit**	**52.5**

A.1.2 Pension Reforms

The statutory retirement age in Germany for a regular old age pension remained at 65 throughout our sample period, with the only prerequisite being 5 years of contributions. Several alternate pathways make retiring before 65 an option. The five main pathways to retirement are regular old-age pensions, old-age pensions for long-term insured, old-age pensions for women, old-age pensions due to unemployment (and, later, part-time work) and old-age pensions for severely disabled persons, see for example Börsch-Supan and Wilke [2004]. We focus on the old-age pensions for women pathway. Eligibility for this pension requires 15 years of contributions of which at least 10 years have to be earned after the age of 40. All recipients in our sample are eligible for this pathway. The early retirement age (ERA) via the women pension pathway stayed at 60 for cohorts born before 1951. The 1992 pension reform has increased the retirement age with full benefit, normal retirement age (NRA), and introduced actuarial adjustment for claiming early. Specifically, for women pension pathway, NRA increases to 65 by monthly step since cohort 1941. In the meanwhile, beginning with cohorts born in January 1941, each year of early claim renders a 3.6%

benefit deduction. The penalty to retire at 60 was phased in gradually in monthly steps, up to 18%. The penalty stabled at 18% for cohort younger than 1945. The 1999 reform abolished the early retirement program for women in cohorts born after 1951. Female workers can no longer retire at age 60. They retire the earliest at age 63 via pension for long-term insured.

A.1.3 Information Revelation

Workers know the expected pension benefits they will get when they retire. It is because letters with detailed pension information were sent to insured individuals every 3 years from age 55 before 2005. Since 2005, letters have been sent annually to workers who are 27 years old and have contributed to the public pension for at least 5 years. Dolls et al. [2018] have shown that those letters inform workers their pension entitlements in a salient fashion. The salience of information helps individuals plan and allows individuals to take into account the additional pension benefits when they make labor supply choices. In detail, the statement is a two-page letter with a summary of the insurance record, including pension service year, full contribution year, accumulated pension points and projected pension entitlement conditional on future contributions. It also indicates warnings and risks, such as shifting of relative income position.

A.1.4 Lifetime budget constraint

Here I describe a simple life cycle model to illustrate how the subsidy plays a role. All individuals maximize lifetime utility subject to their lifetime budget set. I assume individuals earn a constant (after tax and pension contribution) wage w at regular jobs and v at marginal employment and at retirement receive total pension pb. Let T be the last period of life, C be total consumption, Y be lifetime income, T^E be the year of exit from the labor force, T^R be the year claim pension and workers start work from period 0. I assume no discounting and that T is known with certainty. Here I assume T is 80. The lifetime budget constraint takes the following form:

$$C = Y = w \times T^E + v \times (T^R - T^E) + pb \times (T - T^R)$$

, where pb is the pension benefit per year and $pb = \frac{w}{\bar{w}} \times T^E \times AR + b$. b is the subsidy amount. I denote the pension replacement rate for each year of contribution as p, where $p = \frac{AR}{\bar{w}}$. Therefore, $pb = p * w * T^E + b$. I also ignore the adjustment due to early claiming.

For simplicity, I make the two assumptions: 1) If one leaves job before early retirement age 60 ($T^E < 60$), then $T^R = 60$. Worker claims pension immediately as pension become available at early retirement age. In the sample, among the individuals whose leave employment before 60, half retire at 60. 2) If one leaves a job after early retirement age 60, then worker claims pension immediately ($T^E = T^R$). In the sample, among the individuals who exit employment after age 60, 70% claim immediately. The lifetime budget constraint is the following:

$$Y = \begin{cases} w \times T^E + v(60 - T^E) + (p * w * T^E + b)(T - 60) & T^E < 60 \\ w \times T^E + (p * w * T^E + b)(T - T^E) & T^E \geq 60 \end{cases}$$

The slope of the budget constrain is the following:

$$\frac{dY}{dT^E} = \begin{cases} w - v + p * w(T - 60) & T^E < 60 \\ w + p * w(T - T^E) - (p * w * T^E + b) & T^E \geq 60 \end{cases}$$

The implicit tax t on work is

$$t = \frac{w - \frac{dY}{dT^E}}{w}$$

The change of implicit tax rate due to pension subsidy b is zero if one exit employment before age 60 and claim pension at 60. The change of implicit tax rate t due to pension subsidy b is $\frac{b}{w}$ if one exit employment 60 and claim pension immediately afterwards.

$$\frac{dt}{db} = \begin{cases} 0 & T^E < 60 \\ -\frac{b}{w} & T^E \geq 60 \end{cases}$$

If workers bridge to retirement via marginal employment, the change of implicit tax rate due to pension subsidy b is now $-\frac{b}{v}$. Because $|-\frac{b}{v}| > |-\frac{b}{w}|$ and $v < w$, I expect to see the workers bridge to retirement via marginal employment are more affected by the subsidy program.

A.1.5 Parameters in the illustrated budget constraint

The taxable wage income is after social security contribution (SCC) and child allowance. Healthcare insurance is almost always 100% deductible during the sample period. Before 2005, pension contributions were 100% tax-free. As of 2005, to balance the changes in pension income tax, 60% of pension contributions were tax-free, and it increased by 2% each year. In 2025, 100% of contributions will be taxed. For simplicity, I assume all SCC are tax deductible.

The social security contribution (SSC) includes contributions to healthcare insurance, long-term care insurance, unemployment insurance and pension insurance. The average SSC is around 20% of gross wage income. The baseline budget set is constructed for the sample of the married female without dependent children. Given that in the sample, around 90% have non-dependent children, it is representative to construct the lifetime budget constraint for the married couple without children. According to online tax calculator [1], the average tax rate of the married individual with average wage income and whose spouse makes zero income is 0.12.

The public pension benefits are calculated on a complex formula of individual career earnings, average pay, revaluation, and insurance periods. The main determinant of pension payments is the sum of individual accumulated earnings points. Some periods without contribution also count as insurance periods after the age of 17, such as years of further education, time spent in military service, and time spent in raising children. The annual pension wealth of a worker who claims old age pension without financial adjustment and insured for $T^E - s$ years is the following:

$$pb_{gross} = \sum_{t=T^R}^{T} AR_t \times \sum_{\tau=s}^{T^E} \frac{w_\tau}{\bar{w}}$$

, where AR_t is aggregate pension base of year t, w is gross annual individual income τ , $\bar{w}$ is the average income of all insured people in the pension system. If we assume constant wage and take the mean of AR_t, the total pension wealth is

$$PB_{gross} = (T - T^R)\frac{AR}{\bar{w}}(T^E - s) = pw(T^E - s)(T - T^R)$$

, where p is the gross pension replacement rate per year of the pension contribution. The interest portion (Ertragsanteil) of pension is subject to income tax. The taxable portion depends on retirement age. It is 27% if one retires at full retirement age 65. The taxable rate of pension is around 30%. Because the taxable portion of pension on average falls into the zero tax bracket, we assume that pension is not subject to income tax.

[1] The tax rates are obtained from https://www.bmf-steuerrechner.de/ekst

A.2 Additional details on sample construction

A.2.1 Construction of average earning points before 1992

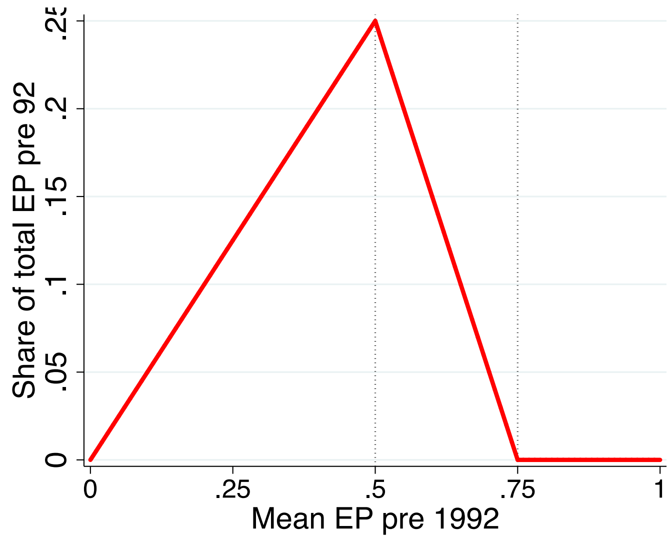
The assignment variable is average monthly pension points accumulated from full-value contribution. In the VSKT dataset, we observe 624 months of pension-related biographies. Respondents enter the data set in January of the year they turn 14 until the December of the year they become 65 years old. I use the birth year and birth month to back out the corresponding year and month when the contribution was made. Additionally, I also observe the socioeconomic status associated with the recorded pension contribution. To calculate average EP from full-value contribution before 1992, I sum up EP and number of months with " gainfully employment with pension contribution obligations." Because in the data, I observe the number of months before 1992 used to calculate the subsidy amount, I compare this variable with the constructed number of months contributed before 1992. This way I can test for the accuracy of the variable construction. I have estimated the regression kink estimates using the policy-defined cutoff 0.5 as the kink point. I find the impacts on pension claim age is around 5 months and on hazard rate to claim at age 60 is around 9%.

A.2.2 Sample construction

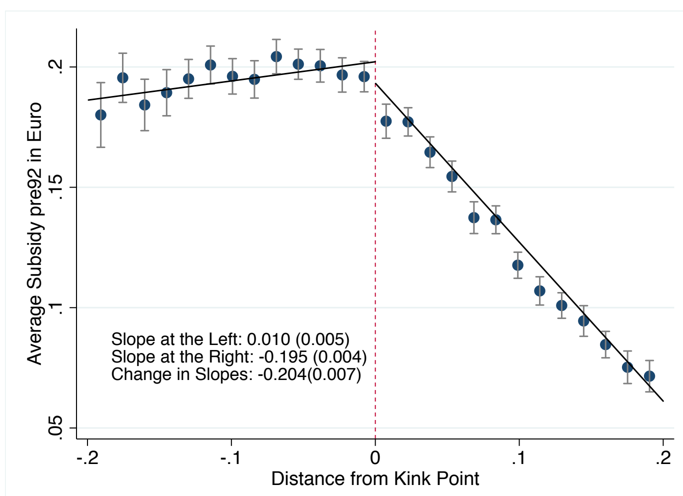
Since the personal identification number varies over time in the VSKT data, I can not guarantee that the same individual won't be surveyed again over different waves of VKST. Following the method used by **?**, For the baseline sample, I take cohorts that are at least as old age 63 from each wave. That corresponds to cohorts 1935, 1936, 1937, 1938 and 1939 from 2002 wave, cohorts from 1937 to 1941 from 2004 wave, 1938 to 1942 from 2005 wave, and so on. I further use time-invariant information, such as kids' birth months, total pension points, pension periods, birth month, etc., to jointly rule out potentially duplicated individuals. I have also compiled samples using individuals who are at least as old as 60, 61, 62 and 64 years old from each wave.

A.3 Additional Figures and Tables

Figure A·1: Average subsidy before 1992 as a function of average monthly earnings points before 1992



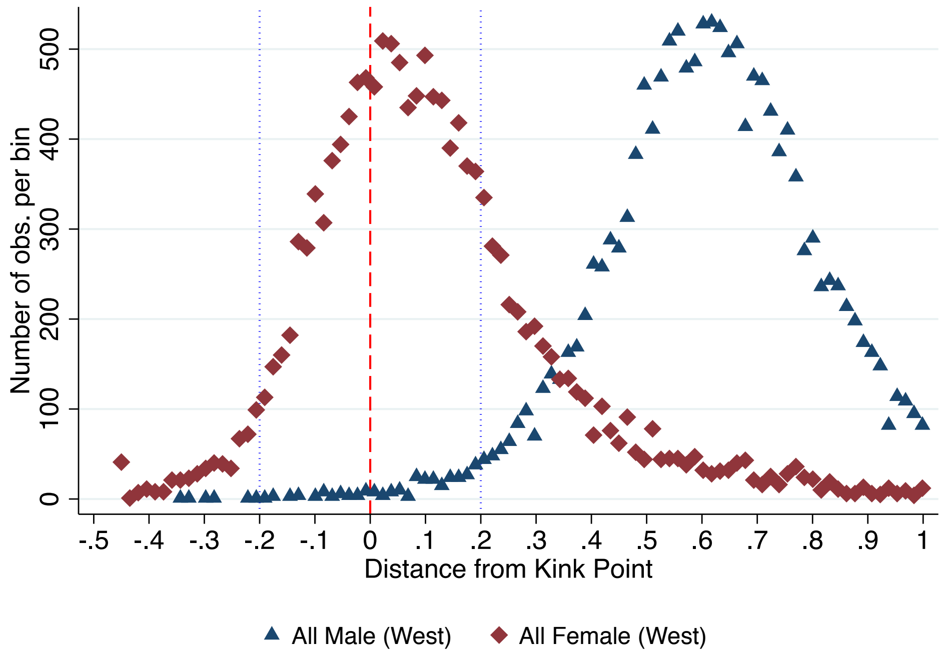
(a) Average subsidy before 1992



(b) Scatter plots: average subsidy per year before 92

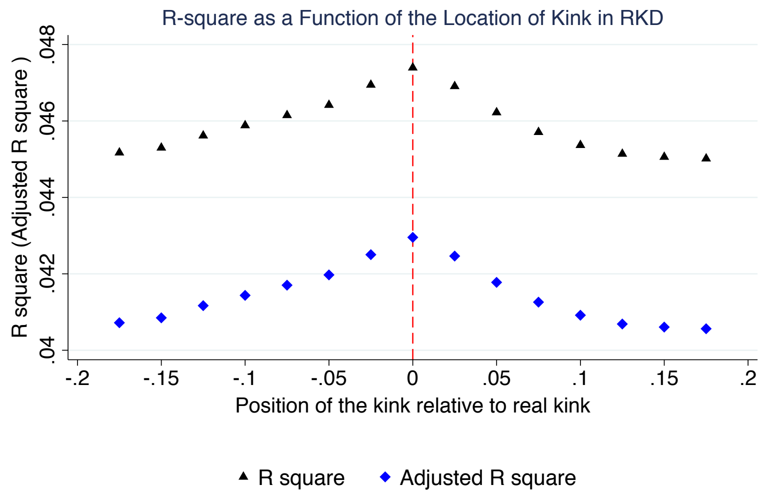
Notes: Figure A1 (a) shows the slope of average subsidy per year before 92 changes from 0.5 to -1 at the kink, as Equation 2 suggests.Figure A1 (b) plots the distribution of average subsidies per year before 1992. It should change from 0.5 to -1 as in Figure A1 (a). However, the slope to the left is smaller than 0.5. Those deviations are measurement errors coming from constructing aep_{92} in the data.

Figure A·2: Density of Female and Male Population

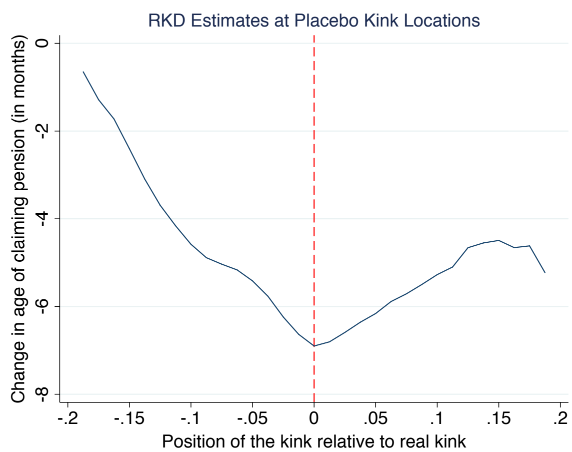


Notes: Figure A2 shows the density of female workers in West Germany and male workers in West Germany.

Figure A·3: Global Kink Points



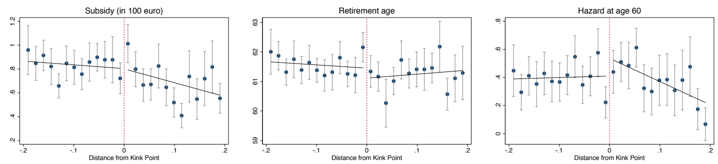
(a) R-squares as a function of Placebo Kinks



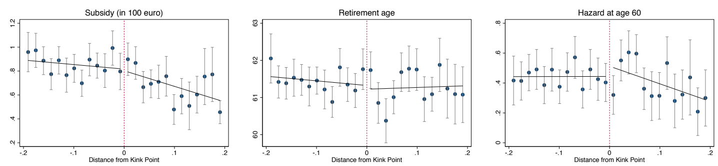
(b) Estimates at Placebo Kinks

Notes: Figure A3a shows the R-squares and adjusted R-squares of the baseline model when the kink is placed at "placebo" locations around the kink. This method follows Landais [2015]. Both the R-squares and adjusted R-squares are maximized at the real kink. Figure A3b shows the estimates as a function of the placebo kinks.

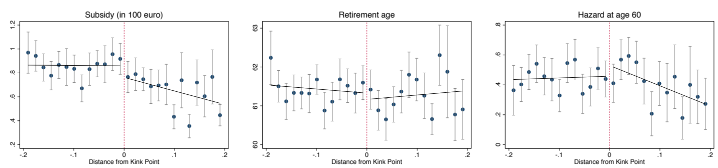
Figure A·4: Scatter plots around the kink using placebo forcing variables



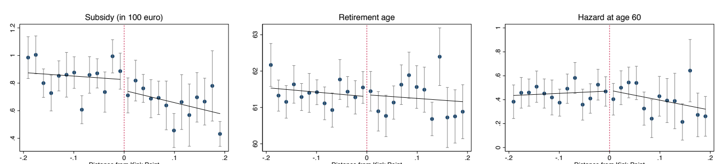
(a) Average EP 1 year after employment



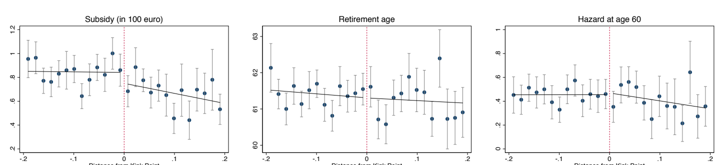
(b) Average EP 2 year after employment



(c) Average EP 3 year after employment



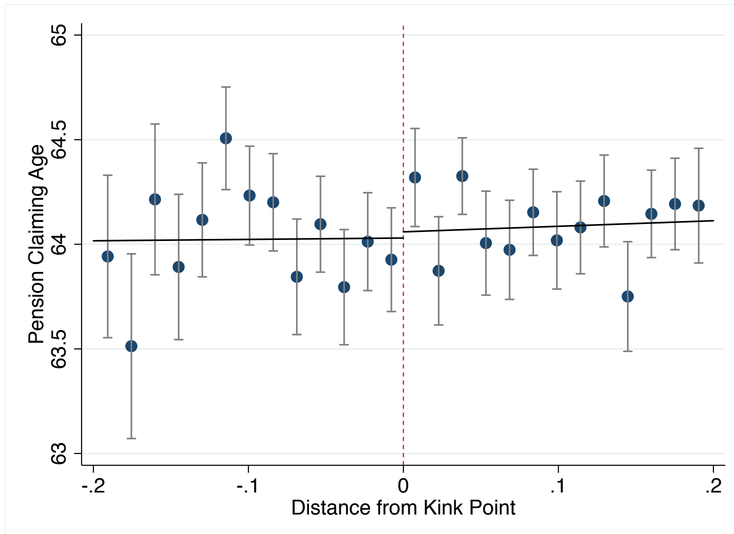
(d) Average EP 4 year after employment



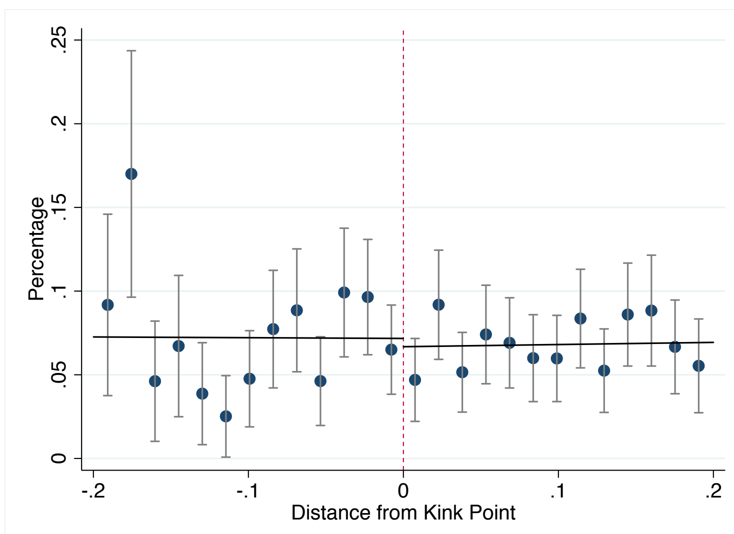
(e) Average EP 5 year after employment

Notes: The figures show bin scatter plots using post-employment EPs as placebo forcing variables.

Figure A·5: Scatter Plots of Age of Claiming Pension around the Kink for Workers with less than 35 credible years



(a) Bin plots: Age of claiming pension



(b) Bin plots: hazard to claim pension at age 60

Notes: Figure A5 shows the relationship of age of claiming pension with average earnings points before 1992 for non-recipients. It shows that the estimated impact on age claiming pension is not caused by the quadratic functional form of age claiming pension.

Table A.1: RKD Estimates of the effect of pension subsidies: by different measure of treatment variables

	Pension claiming age			Hazard rate at 60		
	(1)	(2)	(3)	(4)	(5)	(6)
First-stage	Subsidy Size	Subsidy Share	Total pension	Subsidy Size	Subsidy Share	Total pension
$\Delta \frac{dB}{dr}$ (2)	-521.4***	-0.674***	-525.6***	-521.4***	-0.674***	-525.6***
	(14.99)	(0.0292)	(18.35)	(14.99)	(0.0292)	(18.35)
Means at the kink	112.2	0.20	669.9	112.2	0.20	669.9
Sample means	89.2	0.16	672.4	89.2	0.16	672.4
Reduce-Form						
$\Delta \frac{dY}{dr}$ (1)		4.489***			-0.927***	
		(1.217)			(0.230)	
Means at the kink		60.86			0.43	
Sample means		61.35			0.38	
RKD estimator						
$\frac{dY}{dB}$ $\frac{(1)}{(2)}$	-0.0086***	-6.660***	-0.0100***	0.0018***	1.37***	0.0021***
	(0.0022)	(1.61)	(0.0004)	(0.0022)	(0.30)	(0.0008)
Controls	YES	YES	YES	YES	YES	YES
Cohort Fixed Effect	YES	YES	YES	YES	YES	YES
Observations	5605	5605	5605	5750	5750	5750
R^2	0.048	0.007	0.173	0.001	0.091	0.061

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Treatments are subsidy size measured in 2010 euro, subsidy as a share of total pension and total pension size in 2010 euro. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification. Means at the kink are obtained when aep_{92} is within 0.1 EP around the kink.

Table A.2: Impacts of pension subsidies on employment exiting age

	Age of exiting regular employment			Age of exiting employment		
	(1)	(2)	(3)	(4)	(5)	(6)
First-stage						
$\Delta \frac{dB}{dr}$ (1)	-5.6240***	-5.6296***	-5.3798***	-5.6240***	-5.6296***	-5.3798***
	(0.2940)	(0.2808)	(0.1993)	(0.2940)	(0.2808)	(0.1993)
$\Delta \frac{dY}{dr}$ (2)	-0.4023	-0.3019	0.1865	4.3494	4.2372	4.8460
	(5.001)	(4.9841)	(4.6512)	(4.4284)	(4.4111)	(4.1229)
RKD estimator						
$\frac{dY}{dB}$ $\frac{(2)}{(1)}$	0.0715	0.0536	-0.0347	-0.7734	0.7527	-0.9001
	(0.8886)	(0.8848)	(0.8648)	(0.7928)	(0.7894)	(0.7718)
AIC	35776	35788	32547	31927	34907	34928
BIC	35795	35906	32723	32102	35025	34948
AICc	21020	21021	21021	21021	21021	21020
Means at the kink						
Subsidy size	108.74	108.74	108.74	108.74	108.74	108.74
Outcome variable	56.71	56.71	56.71	57.53	57.53	57.53
Sample means						
Subsidy size	90.29	90.29	90.29	90.29	90.29	90.29
Outcome variable	56.83	56.83	56.83	57.75	57.75	57.75
Controls	No	No	Yes	No	No	Yes
Cohort Fixed Effect	No	Yes	Yes	No	Yes	Yes
Observations	5218	5218	4912	5218	5218	4912
R^2	0.0002	0.0036	0.1683	0.0023	0.0120	0.1183

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.Subsidies are measured in €100. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification. The standard error for RKD estimator is obtained from delta method.

Table A.3: Heterogeneous RKD Estimates

Outcome variables		Age of claiming pension		Hazard to claim pension at 60		
$\Delta B =$ €100		$\frac{dY}{dB}$	p-value	$\frac{dY}{dB}$	p-value	Obs.
Subgroups						
Subsidy Size	High	-0.7172*	0.0971	0.2732***	0.0012	2634
		(0.3441)		(0.0800)		
	Low	0.3225		-0.1178		2269
		(1.444)		(0.3066)		
T_{92}	High	-0.5849*	0.6732	0.1758 ***	0.1069	2312
		(0.2367)		(0.0628)		
	Low	-1.4618		0.2227		2600
		(0.8007)		(0.1667)		
Sick period before age 50	Yes	-1.0307*	0.6036	0.2272 **	0.3603	1869
		(0.4372)		(0.0975)		
	No	-0.7498**		-1.3009		3043
		(0.2971)		(0.0722)		
More than 1 child	Yes	-1.0258***	0.1694	0.1925***	0.1185	3702
		(0.2865)		(0.0664)		
	No	-0.1122		-0.0055		1210
		(0.3610)		(0.0841)		
Cohort Fixed Effects		YES		YES		
Controls		YES		YES		

Standard errors are in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The RKD estimates are the changes in outcome variable in response to an 100 €additional pension income from the subsidy. Subsidies are measured in €100. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification.The high subsidies group are recipients with subsidies above average (82 euro/month). High T_{92} group are recipients who contributed more than 20 years before 1992. I define the healthy group as workers who have never experienced any sick leave before age 50. Lastly, I look at recipients have more than one child. All regressions control for predetermined covariates and cohort fixed effect. The p-values are from a test of the hypothesis that the coefficients are equal within a category.

Table A.4: RKD estimates by polynomial orders

		Pension claiming age			Hazard rate at age 60		
		(1)	(2)	(3)	(4)	(5)	(6)
		Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
$\frac{dB}{dr}$	(1)	-5.3798 **	-3.3285**	-2.6433***	-5.3798 **	-3.3285**	-2.6433***
		(0.1993)	(0.7031)	(1.722)	(0.1993)	(0.7031)	(1.722)
$\frac{dY}{dr}$	(2)	4.6032**	13.1436**	30.5413***	-0.9202***	-2.6966***	-4.1251***
		(1.3416)	(4.9678)	(11.9833)	(0.3104)	(1.1473)	(2.7888)
$\frac{dY}{dB}$	$\frac{(2)}{(1)}$	-0.8556***	-3.9487**	-11.5541	0.1710***	0.8101***	1.5606
		(0.2436)	(1.4978)	(7.7411)	(0.0567)	(0.3489)	(1.2990)
AIC		21020.485	21020.486	21020.9	6188.4008	6188.3959	6191.8902
BIC		21194.796	21207.709	21221.035	6362.769	6375.6802	6392.0907
AICc		21020.785	21020.832	21020.883	6188.7004	6188.7426	6192.2875
Controls		YES	YES	YES	YES	YES	YES
Cohort Fixed Effect		YES	YES	YES	YES	YES	YES
Obs.		4912	4912	4912	4912	4912	4912

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Subsidy is measured in 100 €in this regression. The bandwidth is 0.2 around the kink point 0.45. r stands for $aep_9 2$, the running variable.

Table A.5: Placebo tests using average EP five years after exiting employment as the forcing variable

	Pension claiming age (1)	Employment exiting age (2)	Hazard rate at age 60 (3)
	Average EP 1 year after employment		
$\frac{dY}{dB}$	-1.0375 (7.9755)	5.4465 (20.0896)	2.1327 (5.2783)
	Average EP 2 year after employment		
$\frac{dY}{dB}$	-0.0808 (2.1288)	-0.0655 (4.2719)	0.5217 (0.6693)
	Average EP 3 year after employment		
$\frac{dY}{dB}$	-1.9119 (2.9564)	-2.5791 (5.7393)	1.0317 (1.0544)
	Average EP 4 year after employment		
$\frac{dY}{dB}$	-2.3722 (3.4679)	-3.8350 (7.3996)	1.4814 (1.4893)
	Average EP 5 year after employment		
$\frac{dY}{dB}$	-2.2762 (3.7713)	-2.0570 (7.2694)	1.2687 (1.4869)

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Subsidy is measured in €100. The bandwidth is 0.2 around the kink point 0.45 with 1st order polynomial. The table explores the robustness of the RKD results by using average EP after exiting employment as placebo forcing variables. Post employment EPs are correlated with post employment wage incomes, thus lifetime earnings but are not correlated to aep_{92} strongly. The results show that there are no effect in these placebo specifications.

Table A.6: RKD Estimates of the effect of pension subsidies by cohort groups

	Pension claiming age			Hazard rate at age 60		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\leq$ 1940	1941-1944	$\geq$ 1945	$\leq$ 1940	1941-1944	$\geq$ 1945
$\frac{dY}{dB}$	-0.5718	-0.6518	-1.2579*	0.1649*	0.1743*	0.1420
	(0.3187)	(0.4108)	(0.5670)	(0.0907)	(0.0986)	(0.1160)
Obs.	1372	1574	1784	1390	1598	1792

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Subsidy is measured in €100. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification. The standard error for RKD estimator is obtained from delta method.

Table A.7: Effect on SES before pension claim

Status before pension claim	Regular Employment (1)	Marginal Employment (2)	Unemployment (UI+UA) (3)
$\frac{dY}{dB}$	-0.004	-0.0224	**0.090**†
	(0.0569)	(0.0261)	(0.052)
Sample means	0.43	0.05	0.29
Observations	924,059		
Individuals	5,763		
Controls	Yes	Yes	Yes
Cohort Fixed Effect	Yes	Yes	Yes

Standard errors in parentheses† $p < 0.10$. Subsidies are measured in €100. The results are from local linear regressions with a bandwidth of 0.2 EP around the kink for the baseline specification. The standard error for RKD estimator is obtained from delta method.

Appendix B

Appendix — Chapter 2

B.1 Data

The Integrated Employment Biographies (IEB) in Germany contains information on all social security reliable employment periods and periods of UI receipt between the years 1975 and 2013. The employment information comes from the employers who are required to report information on all their employees annually, with the exact duration of the employment periods and corresponding individual information. A new employment period starts with a new year, the beginning of a new job or changes at the current job that require notification, such as a switch of the health insurance or from minor employment to social security reliable employment. For each of those periods, individual characteristics such as the birth date, gender and nationality and employment information such as daily gross wage, occupation, educational status and several employer characteristics are reported. The data on UI receipt stems from administrative UI records which are used in the local UI agencies to determine eligibility and to govern the payment process to the UI recipients. It entails information on the exact duration of UI-receipt, the daily benefits.

Due to the daily character of our data, we can exactly determine, whether an individual is regularly employed, on UI benefits, or – when currently not in the data – non-employed. The structure of the data allows furthermore constructing detailed biographical information such as experience or tenure or past exposure to unemployment. We select all UI-entries between 1980 and 2010, which qualify based on their working history for their age-specific maximum PBD. The period-specific calculations are shown in table B.4.

B.2 Life Cycle Model and Budget Set Construction

Life Cycle Model

Here we describe a life cycle model as in Brown [2013] and how it can be used to estimate how UI entries vary with maximum PBD duration P ($\frac{dg_t}{dP}$). We assume that all workers maximize lifetime utility subject to their lifetime budget set. In particular, let T be the last period of life, C be total consumption and E be the year of exit from the labor force (which equals total years of work S plus years of schooling s). We assume no discounting and that T is known with certainty. We assume the lifetime utility function take the following function form:

$$U(C,E) = C - \frac{a}{1+\frac{1}{e}}\left(\frac{E}{a}\right)^{1+\frac{1}{e}}$$

where e is the labor supply elasticity and a is ability. The heterogeneity is captured by a a density distribution $\mu(a)$. This quasi-linear, iso-elastic utility function rules out income effect. This model predicts perfect consumption smoothing over the lifecycle: $c_t = \frac{C}{T}$.

We assume individuals earn a constant (after tax) wage w and at retirement receive total pension payments $y^R(S)$ and UI payments $y^{UI}(S)$ which both will depend on years worked (in potentially discontinuous and non-differentiable ways). Note that this yields a budget constraint: $C = w(E-s) + y^{UI}(E) + y^R(E)$.

Note that we can write the elasticity of exit age with respect to the change in effective net wage of working an additional period is $e = \frac{dE}{E} \times \frac{w^{net}}{dw^{net}}$ where $w^{net} \equiv w + \frac{\partial y^{UI}}{\partial E} + \frac{\partial y^{R}}{\partial E}$. This elasticity will be obtained by a bunching estimator.

The FOC of this problem is given by

$$E = a[w^{net}]^{e}$$

If the distribution of ability $\mu(a)$ is smooth, this implies a smooth distribution of exit age with density $h_0(E)$. We know that a constant potential benefit duration of P induces a convex kink at age $T^R - P$ where the slope of the individual's budget set exhibits a discrete decline from w_H^{net} to w_L^{net}. Bunching at this kink can be used to recover the elasticity e [see e.g. Saez, 2010, Kleven, 2016]. In particular, under the given utility function, we can use the fact that the marginal buncher with ability $a^* + \Delta a^*$ is indifferent between locating at her optimal point under w_{above}^{net} and locating at the kink to get an exact formula for the elasticity of

$$e = -\frac{ln\left(1 + \frac{\Delta E^*}{E^*}\right)}{ln\left(\frac{w_{Above}^{net}}{w_{Below}^{net}}\right)}$$

The total amount of observed bunching B is given by $B = \int_{E^*}^{E^*+\Delta E^*} h_0(E)dE$.

Thus, observed bunching and an estimate of $h_0(E)$ can be used to recover e and $\mu(a)$.

Budget Set Construction

We assume individuals earn a constant (after tax) wage w and at retirement receive total pension payments $y^R(S)$ and UI payments $y^{UI}(S)$. This yields a budget constraint of the form

$$C = w(E - s) + y^{UI}(E) + y^R(E)$$

Here we detail how we compute the budget set. We denote p as the gross pension replacement rate per year of pension contribution[1]. In other words, Each year of work with wage of w will increase pension benefits $y^R(E)$ by pw. We also denote UI provides income support of $0.68w$. Each year spent on UI increases pension benefits $y^R(E)$ by $0.8 \times pw$. We assume individuals take their full UI duration upon exit and then rely on UA retire, this too can be modified. For illustration purpose, here we assume UA provides zero income. In the simulation, we assume UA yields $0.30w$ and workers spend $T^R - E - P$ on UA.

The budget constraint is thus given by:

$$C = w(E-s) + \underbrace{bD + 0.8 \times pwD \times [T - max\{T^R, E-s+T^u]}_{y^{UI}(E)}$$

$$+ \underbrace{pw(E-s) \times [T - max\{T^R, E-s+T^u\}]}_{y^R(E)}$$

Where D is UI duration, T^u is unemployment duration, P is maximum potential UI duration, b is UI benefit level, m is the UA benefit level. The retirement type r, by definition, $T^u = D \geq P$.

Therefore,

$$C = Y = \begin{cases} w(E-s) + bP + pw \times (E-s+0.8P) \times [T - T^R] & \text{if } E < T^R - P \\ w(E-s) + bP + pw \times (E-s+0.8P) \times [T - (E-s+T^u)] & \text{if } E \geq T^R - P \end{cases}$$

The stylized budget sets in Figure 2 make an assumption that worker always retire at the earliest possible retirement age. Lets take as example the 1924 cohort (where $P = 1$ and $T^R = 60$). Therefore, the budget set is

$$C = Y = \begin{cases} w(E-s) + bP + pw \times (E-s+0.8P) \times [T - 60] & \text{if } E < 60 - P \\ w(E-s) + b(60-E) + pw \times (E-s+0.8 * (60-E)) \times [T - 60] & \text{if } E \geq 60 - P \end{cases}$$

$$\frac{dY}{dE} = \begin{cases} w + pw[T - T^R] & \text{if } E < T^R - P \\ w - b + pw(1-0.8)[T - T^R] & \text{if } E \geq T^R - P \end{cases}$$

[1] On average, the net pension replacement rate for an average earner with 45 years of insurance is 70%

Parameters in the budget sets

The baseline busget set by cohort is constructed for the sample of married couple without dependent children. Given that in our sample, around 80% are married and around 15% have dependent children, it is representative to construct the life time budget constrain for married couple without children. We use the following parameters: $s = 20$, $T = 80$, $a = 0.8$ and $B = 0.68w$. The tax rate of married individual with average wage income and whose spouse makes average wage income for cohort 1924, 1929, 1935, 1941, 1949 and 1951 are 0.22 0.24, 0.22, 0.22, 0.22 and 0.18, respectively [2]. The gross average wage are 19456, 17779, 24886, 24886, 22477 and 22423; and the pension replacement rate p also varies by cohorts. Moreover, we use a linear approximation to the curved budget set to measure the changes in slope at the kink point.

The pension replacement rate p

The public pension is calculated on a complex formula of individual career earnings, average pay, revaluation, and insurance periods. The main determinant of pension payments is the sum of individual accumulated earnings points (Entgeltpunkte). One pension earnings point (EP) represents annual pension contributions made by a contributor earns average income. The gross lifetime pension income of a worker who claims old age pension without financial adjustment[3] and insured for $E - s$ years is the following:

$$Y^R_{gross} = \sum_{t=T^R}^{T} AR_t \times \sum_{\tau=s}^{E} \frac{w_\tau}{\bar{w_\tau}}$$

where AR_t is aggregate pension base of year t, w_τ is gross individual income in year τ, $\bar{w_\tau}$ is the average income of all insured people in the pension system. AR_t also

[2]The tax rates are obtained from https://www.bmf-steuerrechner.de/ekst

[3]See section for detailed pension calculation when pension types and financial adjustments are considered.

represents the pension value of one EP[4]. If we assume constant wage and take the mean of AR_t and $\bar{w_\tau}$,

$$Y^R_{gross} = (T - T^R)\frac{AR}{\bar{w}}(E - s)w_{gross} = (T - T^R)(E - s)pw_{gross}$$

where $p = \frac{AR}{\bar{w}}$ is the gross pension replacement rate per year of pension contribution. A person with 45 years of contribution year has a gross pension replacement rate around 50%.

Prior to 1982, gross pension is the same as net pension benefit. After 1982, pension is subject to health care contribution (KVdR). This percentage of contribution ranges between 6.8% and 8.5%.[5]

$$\begin{aligned} Y^R_{net} &= Y^{gross}(1 - KVdR) = (T - T^R)(E - s)p(1 - KVdR)w_{gross} \\ &\simeq (T - T^R)(E - s)p(1 - 8\%)w_{gross} \\ p_{net} &= \frac{AR}{\bar{w}}(1 - KVdR) \end{aligned}$$

Each additional year of S increases life time income by w_{net} and $p(1-KVdR)w_{gross}$. The $p(1-KVdR)$ of married individual with average wage income is 0.01128, 0.01077, 0.01173, 0.00969, 0.00953, 0.00945 of the six cohorts, respectively.

B.3 Additional Tables

[4]Both AR_t and $\bar{w_\tau}$ are public available information. Table lists AR_t and $\bar{w_\tau}$ of our sample period.

[5]This contribution includes health care insurance contribution and long term care contribution. From April 1, 2004 pensioners have to pay the full contribution (1.7%) for long-term care insurance instead of only half of it.

Table B.1: Potential Unemployment Insurance Benefit (UIB) Durations as a Function of Age and Months Worked in Previous 7 Years.

Months Worked in prev. X years	January 1983- December 1984	January 1985- December 1985	January 1986- June 1987	July 1987- March 1997	April 1997* - January 2006	February 2006 - December 2007	January 2008 - today (2015)
12	4	4	4	6	6	6	6
16	4	4	4	8	8	8	8
18	6	6	6	8	8	8	8
20	6	6	6	10	10	10	10
24	8	8	8	12	12	12	12
28	8	8	8	14 (≥42)	14 (≥45)	12	12
30	10	10	10	14 (≥42)	14 (≥45)	15 (≥55)	15 (≥50)
32	10	10	10	16 (≥42)	16 (≥45)	15 (≥55)	15 (≥50)
36	12	12	12	18 (≥42)	18 (≥45)	18 (≥55)	18 (≥55)
40	12	12	12	20 (≥44)	20 (≥47)	18 (≥55)	18 (≥55)
42	12	14 (≥49)	14 (≥44)	20 (≥44)	20 (≥47)	18 (≥55)	18 (≥55)
44	12	14 (≥49)	14 (≥44)	22 (≥44)	22 (≥47)	18 (≥55)	18 (≥55)
48	12	16 (≥49)	16 (≥44)	24 (≥49)	24 (≥52)	18 (≥55)	24 (≥58)
52	12	16 (≥49)	16 (≥44)	26 (≥49)	26 (≥52)	18 (≥55)	24 (≥58)
54	12	18 (≥49)	18 (≥49)	26 (≥49)	26 (≥52)	18 (≥55)	24 (≥58)
56	12	18 (≥49)	18 (≥49)	28 (≥54)	28 (≥57)	18 (≥55)	24 (≥58)
60	12	18 (≥49)	20 (≥49)	30 (≥54)	30 (≥57)	18 (≥55)	24 (≥58)
64	12	18 (≥49)	20 (≥49)	32 (≥54)	32 (≥57)	18 (≥55)	24 (≥58)
66	12	18 (≥49)	22 (≥54)	32 (≥54)	32 (≥57)	18 (≥55)	24 (≥58)
72	12	18 (≥49)	24 (≥54)	32 (≥54)	32 (≥57)	18 (≥55)	24 (≥58)
Rahmenfrist - Min emp dur. for new UI eligibility	12	12	12	12	12	12	12
X - Base Period for P≥12	7	7	7	7	7	5	5
X - Base Period for P<12	4	4	4	3	3	2	2
Replacement Rates on Gross Wages in Percent:							
UI (children)	68	68	68	$67^{\ddagger}$	67	67	67
UI (no children)	$63^{\dagger}$	63	63	$60^{\ddagger}$	60	60	60
UA (children)	58	58	58	$57^{\ddagger}$	57	UIB II	UIB II
UA (no children)	$53^{\dagger}$	53	53	$50^{\ddagger}$	50	UIB II	UIB II

Source: Hunt (1995), Bundesgesetzblatt (1983-2015) and Dlugosz etal (2013).

*The reform in 1997 was phased in gradually: For workers who had worked for more than one year during the three years before April 1997, the old rules applied until March 1999 (See Arntz, Simon Lo, and Wilke 2007).

† UI and UA replacement rates were lowered starting in January 1984. Until December 1983, ALG was 68 percent and ALH 58 percent of the previous gross wage, irrespective of whether the recipient had children.

‡ UI and UA were lowered starting in January of 1994.

Table B.2: The schedule of earliest retirement age (ERA) by different retirement pathways

Pathways		1957 - 2011		2012 - 2020
Regular old age pension (qualifying period of 5 years)		65		from 65 to 67
	1957 - 2005	2006 - 2011	2012 - 2016	2017 till now
Pension due to unemployment (at least 52 weeks unemployed after $58\frac{1}{2}$) (qualifying period of 15 years)	60	60 to 63	63	same as regular old age pension
		1957 - 2016		2017 till now
Pension for women (qualifying period of 15 years)		60		same as regular old age pension
	1957 - 1972	1972 - 2010	2011	2012 - 2025
Pension for long-term insured (qualifying period of 35 years)	65	63	63 to 62	62

Table B.3: Retirement age by retirement pathways from 1957 till now

Pathways	Time of implementation	Affected cohorts	SRA		Reform
Standard old-age pension	1957 - 2011	<1947 Jan	65	-	
(Years of contribution: 5^6)	2012 - 2030	1947 Jan- 1964 Jan	65 to 67	-	2007 Reform
	> 2031	≥1964 Jan	67	-	
			NRA (no penalty)	ERA (earliest possible)	
Old-age pension for long-term insured	1972 - 1999	1909 Jan - 1936 Dec	63	-	1972 Reform ‡
(Years of contribution :35)	2000 - 2003	1937 Jan - 1938 Dec	63 to 65	63	1992 Reform §
	2004 - 2010	1939 Jan - 1947 Dec	65	63	
	2011 - 2030	1949 Jan - 1964 Jan	65 to 67	63	2007 Reform *
Old-age pension due to unemployment	1972 - 1996	< 1937 Jan	60	-	1972 Reform
or part-time work	1997 - 2006	1937 Jan - 1941 Dec	60 to 65	60	1992/99 Reform
(at least 52 weeks unemployed after $58\frac{1}{2}$, or		1942 Jan - 1945 Dec	65	60	
2 years part-time)	2006 - 2011	1946 Jan - 1948 Dec	65	60 to 63	1992 Reform
(Years of contribution: 15(8 in last 10 yrs))	2012 - 2016	1949 Jan - 1951 Dec	65	63	
	> 2017.1	> 1952 Jan	Phased out	-	2007 Reform
Old-age pension for women	1957 - 2000	<1940 Jan	60	-	
(Years of contribution: 15 (10 after age 40))	2000 - 2009	1940 Jan - 1944 Dec	60 to 65	60	1992 Reform
	2010 - 2016	1945 Jan - 1951 Dec	65	60	
	> 2017.1	> 1952 Jan	Phased out	-	2007 Reform
Old-age pension for disabled workers	1972 - 1977	1911 - 1917	62	-	1972 Reform
(Years of contribution: 35)	1978 - 1980	1918 Jan - 1919 Dec	62 to 60	-	1978 Reform
(Loss of at least 50 percent of earnings	1981- 2000	1920 Jan - 1940 Dec	60	-	
capability)	2001 - 2006	1941 Jan - 1943 Dec	60 to 63	60	1992 Reform
	2007 - 2011	1944 Jan - 1951 Dec	63	60	
	2012 - 2025	1952 Jan - 1963 Dec	63 to 65	60 to 62	2007 Reform
	> 2026	> 1964 Jan	65	62	
Old-age pension for especially long-term insured	<2016	< 1953	63	-	
(qualifying period of 45 years)	2016 - 2028	1953 Jan - 1963 Dec	63 to 65	-	2007 Reform
	> 2029.1	> 1964.1.1	65	-	
Disability pension : independent of age					
	<1985	5 years of contribution			
	> 1985	5 yrs with minimum 3 in last 5 yrs			1984 Reform

† The German public pension system distinguishes "old-age pensions" from "disability pensions": old-age pensions for workers aged 60 and older; and disability benefits for workers below age 60, which at the statutory retirement age are converted to old-age pensions at age 65.

‡ The 1972 reform: "flexible retirement" after age 63 with full benefits became possible for long-term insured; retirement at age 60 with full benefits became possible for women, the unemployed, and older disabled workers.

§ The 1992 reform introduced actuarial adjustment. Since then, we distinguish ERA and NRA. It also increased NRA to 65 for all pathways except for disabled workers. It increased ERA for the unemployed to 63 (See SGBVI appendix 19).

* The 2007 reform increases SRA stepwise between 2012 and 2029 from 65 to 67 for both men and women (see SGB VI 235). For cohorts older born in 1952 and after, retirement pathway for women and the unemployed are phased out.

Sources: Sozialgesetzbuch (SGB) Sechstes Buch (VI), Börsch-Supan and Jürges (2012), Börsch-Supan and Wilke (2006), Giesecke and Kind (2013).

Table B.4: Period Specific Restrictions on Working Histories

Periods	New Eligibility: contributions* >= 12 months during ...	Full Eligibility: contributions* >= ... during ...
1980-1984	previous 4 years	60 months, previous 7
1986-1987	previous 4 years	72 months, previous 7
1987-1999	previous 3 years	72 months, previous 7
1999-2006	previous 3 years	64 months, previous 7
2006-2007	previous 2 years	48 months, previous 5 years
2008-2010	previous 2 years	48 months, previous 5 years

*As contribution duration we count all regular social security reliable employment relationships. For simultaneous employment relationships, we take the one with the highest earnings.

Appendix C

Appendix — Chapter 3

C.1 CPS-DWS's Questionnaire form

Displaced workers are workers who lost or left jobs because their plant or company closed or moved, there was insufficient work for them to do, or their position or shift was abolished. Those workers are older than 20 years old and receive wages, salaries, commissions, tips, payment in kind, or piece rates (Displaced Workers Technical Note 2016).

SD1: During the last 3 calendar years, that is, January 2013 through December 2015, did (name/you) lose a job, or leave one because: (your/his/her) plant or company closed or moved, (your/his/her) position or shift was abolished, insufficient work or another similar reason?

- (1) Yes
- (2) No (Skip to ST1LCK)

SD2: Which of these specific reasons describes why (name/you)(is/are) no longer working at that job? If (name/you) lost or left more than one job in the last 3 years, refer to the job (you/he/she) had held the longest when answering this question and the ones that follow.

- (1) Plant or company closed down or moved

- Plant or company operating but lost or left job because of: (2) Insufficient work (3) Position or shift abolished
- (4) Seasonal job completed (Skip to ST1LCK)
- (5) Self-operated business failed (Skip to ST1LCK)
- (6) Some other reason (Skip to ST1LCK)

SD5: Had (name/you) been given written advance notice informing (you/him/her) that (the plant or business would be closed) ((you/he/she) would lose (your/his/her) job)?

- (1) Yes (go to SD6)
- (2) No (go to SD7)

SD6: How long before (name/you)(were/was) to have lost (your/his/her) job did (you/he/she) receive that notice?

- (1) Less than 1 month
- (2) 1 to 2 months
- (3) More than 2 months

C.2 Additional Tables and Figures

Table C.1: Comparison of displaced and non-displaced workers

	Displaced Workers		Nondisplaced Workers		P-value
	Mean (1)	S.D. (2)	Mean (3)	S.D. (4)	(5)
Panel A: Outcome variables					
UI take-up rate	0.45	(0.5)	0.48	(0.5)	0.005
UI exhaustion rate	0.38	(0.49)	0.17	(0.38)	0.00
Employed at time of survey	0.74	(0.44)	0.75	(0.43)	0.002
Weekly earnings at current job	803	(688)	730	(667)	0
Panel B: Recived notice					
Less than 1 month	0.112	(0.31)	0.102	(0.11)	0
1 to 2 months	0.107	(0.31)	0.01	(0.08)	0
more than 2 months	0.117	(0.32)	0.052	(0.22)	0
No notice at all	0.664	(0.47)	0.790	(0.41)	0
Panel C: Demographics					
Age	40.56	(11.94)	39.03	12.26	0
Female	0.44	(0.5)	0.311	(0.5)	0
Black	0.106	(0.31)	0.0873	(0.32)	0.005
Married	0.522	(0.5)	0.505	(0.5)	0
Education (in years)	13.21	(2.44)	12.55	(2.56)	0.008
High school graduates	0.58	(0.49)	0.56	(0.5)	0.003
College graduates	0.255	(0.44)	0.35	(0.43)	0.141
Panel D: Pre-displacement variables					
Tenure at lost job	4.87	(6.41)	3.64	(5.33)	0
Union member (%)	0.08	(0.27)	0.23	(0.42)	0
Part-time worker (%)	0.17	(0.38)	0.18	(0.38)	0.83
Plant closing (%)	0.35	(0.48)	0.03	.(0.17)	0
Had health insurance at lost	0.52	(0.50)	0.40	(0.49)	0
Weekly earnings at lost job	856.2	(706)	833.9	(676)	0
No. of observations	29,978		23,308		

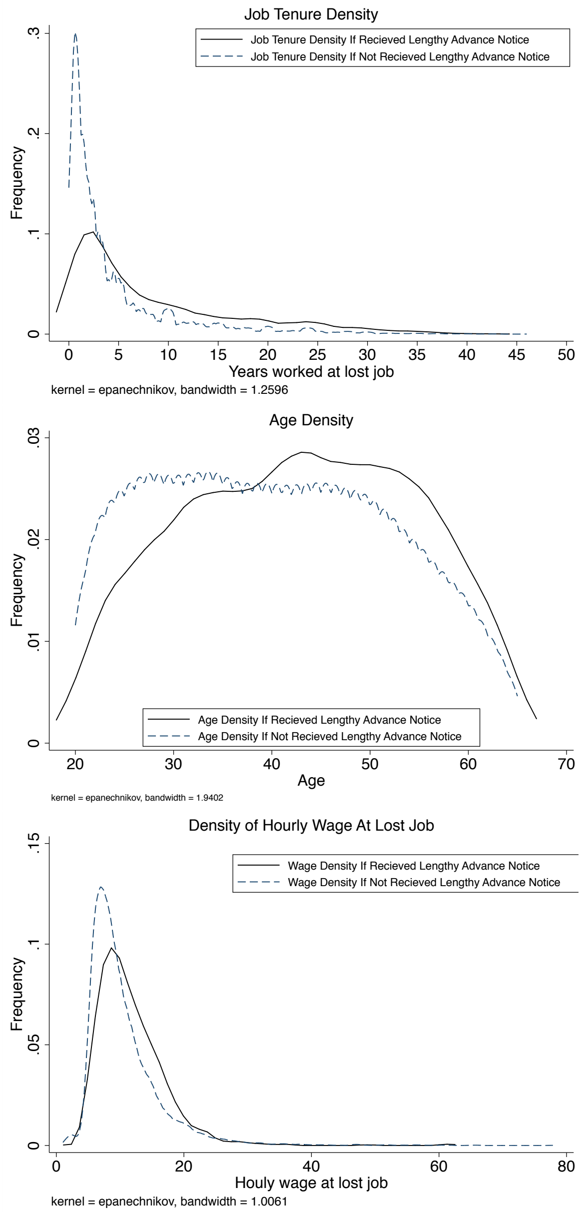
Note: The sample includes all displaced and non-displace workers from survey year 1998 to 2016. Those workers have lost or left their jobs within 3 years of the survey time. Weekly earning at lost job and current job are inflation-adjusted. Standard deviations are in parentheses. The p-value for testing the null hypothesis: the displaced and non-displaced workers have the same characteristics are reported in column 5.

Table C.2: The Impact of Advance Notice - A Naive OLS Results

Outcome Variables	WARNed		UI Take-up		UI Exhaustion Rate (UI Claimers)	
Variables		S.E.		S.E.		S.E.
WARNed	-	-	-0.0580***	[0.0087]	0.0193	[0.0200]
Age	-0.0002	[0.0011]	0.0118***	[0.023]	0.0024	[0.0021]
Female	0.0284***	[0.0034]	0.0745***	[0.0082]	0.02478*	[0.010]
Black	0.0107	[0.0066]	0.0404	[0.0165]	0.1080***	[0.016]
Married	0.0116**	[0.0042]	0.0035	[0.0070]	-0.0284*	[0.0097]
Education(in years)	0.0007***	[0.0001]	-0.0002	[0.0002]	-0.00057	[0.0003]
High school graduates	0.0110	[0.0057]	0.0074	[0.0075]	-/0.0004	[0.0123]
Tenure at lost job	0.0106***	[0.0013]	0.0051*	[0.0020]	0.0039	[0.0024]
Tenure Squared	-0.0001*	[0.0000]	-0.0002*	[0.00006]	-0.00002	[0.00008]
Union member(%)	0.0521***	[0.0111]	0.0659***	[0.0129]	0.0368*	[0.0172]
Part-time worker(%)	0.0014	[0.0054]	-0.1792***	[0.0099]	0.0070	[0.019]
New worker(%)	-0.0115	[0.0044]	-0.1052***	[0.0116]	-0.0209	[0.018]
Plant closing(%)	0.1286***	[0.008]	-0.0208***	[0.0069]	-0.01203	[0.0100]
Had health insurance	0.0448***	[0.0043]	0.1753***	[0.0085]	-0.0204	[0.0149]
Hourly wage at lost job	0.0008	[0.0006]	0.0018	[0.0010]	-0.0033*	[00.0015]
State fixed effect	Yes		Yes		Yes	
Year fixed effect	Yes		Yes		Yes	
No.of observations	26322		26013		11,724	

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure C·1: Kernal Density of Job Tenure, Age, Wage at Lost Job



Notes: Author's construction

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