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# Early retirement provision for elderly displaced workers

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Herman Kruse and Andreas Myhre

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*Herman Kruse and Andreas Myhre*

# Early retirement provision for elderly displaced workers

**Abstract:**
This paper examines the economic consequences of losing eligibility for early retirement (ER) benefits due to involuntary job displacement. In Norway, job displacement before a certain age cutoff results in the loss of eligibility for ER benefits between ages 62 and 67. This allows us to employ a credible regression discontinuity design to identify causal effects. We utilize detailed Norwegian matched employer-employee data containing information on bankruptcy dates to identify job displacements, along with data on individual income, wealth, pensions, and social security benefits. We do not detect any differences in re-employment rates or labor market earnings between workers who retain eligibility for ER benefits and those who do not, with point estimates being close to zero and statistically insignificant. Meanwhile, those who lose ER eligibility substitute 69 percent of their lost benefits through the uptake of other social security benefits, with 51 percentage points attributed to disability insurance and 13 percentage points to unemployment insurance. Applying the Baily-Chetty formula for optimal social security provision, we show that a very high level of risk aversion would be required for the provision of ER benefits to elderly involuntary displaced workers to be an overall improvement for social welfare.

**Keywords:** early retirement, job displacement, labor supply, benefit substitution, social security

**JEL classification:** H55, I38, J14, J26, J65

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**Address:** Akersveien 26, Statistics Norway, Research Department. E-mail: herman.kruse@ssb.no

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## Sammendrag

I denne artikkelen undersøker vi de økonomiske effektene på sysselsetting og bruk av trygdeordninger blant eldre personer som mister muligheten for å ta tidlig alderspensjon (AFP) som følge av at de eksogent mister jobben.

Vi bruker detaljerte norske registerdata som har informasjon om konkursdatoer og individdata på formue, inntekt, pensjoner og trygde- og pensjonsordninger. I hovedanalysen benytter vi et diskontinuerlig regresjonsdesign (RD). Vi kan bruke denne metoden fordi konkurs like før aldersgrensen for uttak av tidligpensjon fra AFP-ordningen resulterte i tap av retten til AFP mellom 62–67 år. Dermed kan vi sammenlikne en behandlingsgruppe som mistet jobben akkurat «sent nok» til fortsatt å kunne benytte AFP-ordningen, med en kontrollgruppe som mistet jobben akkurat «litt for tidlig» slik at de mistet retten til AFP.

Vi finner ingen signifikant forskjell på sysselsettingen blant personer som mister jobben like etter grensen for uttak av AFP og personer som mister jobben akkurat før grensen. På den andre siden finner vi at de som mister jobben akkurat før grensen innhenter om lag 69 prosent av de tapte ytelsene fra AFP gjennom andre trygdeordninger. Av dette er om lag 51 prosentpoeng uføretrygd og 13 prosentpoeng arbeidsledighetstrygd, mens de resterende 5 prosentpoengene er andre ytelser.

Videre benytter vi funnene til å undersøke hvorvidt personer som mister jobben nær pensjonsalder burde kunne gå av med tidligpensjon. For å finne ut om dette er samfunnsøkonomisk gunstig benytter vi Baily-Chetty-rammeverket for optimal størrelse på trygdeytelser. Vi finner at personer må være svært lite tilbøyelige for risiko for at en slik ordning skal gi en velferdsgevinst for samfunnet.

# 1 Introduction

Should elderly displaced workers be able to retire early? Late-career job loss often has significant negative economic consequences for individuals, especially since older workers are typically less desirable to employers.[1] In the United States alone, more than one million workers aged 55 or older lose their jobs each year, and roughly a quarter of them never find reemployment (Merkurieva, 2019). In several OECD countries, late-career job displacement can lead to individuals losing their eligibility to retire early or receive equivalent benefits compared to those who have completed full careers.[2] To mitigate financial hardship, many nations have implemented costly policy measures such as extended unemployment benefits or more lenient criteria for disability insurance targeting older workers.[3] However, less attention has been given to options that allow individuals to exit the labor market through early retirement schemes after experiencing involuntary job loss. Although introducing a generous early retirement option could offer economic relief to individuals, the social desirability of such a program hinges on a trade-off between potential negative effects on job-seeking efforts, the likelihood of individuals substituting the program for other forms of financial support, and the individual insurance value.

The main contribution of this paper is to assess the economic implications of providing early retirement (ER) benefits for elderly involuntarily displaced workers. Specifically, we examine (i) the adverse effects on re-employment rates, (ii) the substitution onto other social security benefits and (iii) the broader policy and welfare implications. Our study is facilitated by a sharp eligibility criterion in the Norwegian ER program.[4] Before 2011, workers in private sector firms covered by the ER scheme could claim benefits from age 62. However, eligibility required current employment with the firm at the time of claiming. A job displacement prior to an individual cutoff date led to ineligibility for ER benefits provided between ages 62 and 67. This setup enables us to employ a regression discontinuity design to study the causal effects of ER provision by comparing workers who just missed eligibility due to job loss with those who just remained eligible. Our data on bankruptcies among Norwegian private sector firms from 2001 to 2010 assists in identifying involuntary job displacements, thereby mitigating potential endogeneity issues arising from voluntary departures from firms.[5] Our use of high-quality data on matched employer-employee relationships and the uptake of diverse social security benefits from tax registers enables us to analyze the effects of ER eligibility on re-employment rates, earnings, and the substitution of benefits between ages 62 and 67. We pay particular attention to the shift toward disability insurance (DI) and unemployment insurance (UI) benefits. Additionally, we explore welfare implications and the financial costs for the state. Employing the sufficient statistics approach proposed by Baily (1979) and Chetty (2006), we assess whether introducing an early retirement option for elderly involuntarily displaced workers leads to improvements in welfare.

Our main empirical findings can be summarized by the following conclusions. First, we find clear evidence of program substitution, with a notable increase in enrollment onto the DI program among individuals who are ineligible for ER benefits. Just below the eligibility cut-off, approximately 48 percent enroll in DI, compared to around 12 percent among those who just retain ER eligibility. This 36-percentage-point increase in DI enrollment among the ineligible individuals is statistically significant. We estimate that out of the \$61,600 in lost ER benefits for the ineligible group, about \$42,500 is replaced by non-pension public transfers. Of this amount, roughly \$31,400 corresponds to higher take-up of DI benefits and \$7,700 corresponds to higher take-up of UI benefits. This is translates to a replacement rate of 69 percent of the lost benefits by other (non-pension) social security benefits, with

[1]E.g. Heyma *et al.* (2014); Vigtel (2018).

[2]Examples of these countries include Austria, Estonia, Hungary, Israel, Korea, Norway, the Slovak Republic, Sweden, Chile, Mexico and Germany (OECD, 2015).

[3]This approach has been adopted by e.g. Denmark, Sweden, Finland, Germany, Netherlands, Austria and Belgium.

[4]The ER program in Norway is known by its acronym AFP from Norwegian "Avtalefestet pensjon".

[5]For job displacements attributed to bankruptcies, an additional rule, referred to as the "52-week-rule", adjusted this threshold to 61 years of age plus the standard notice period, typically ranging from 1–6 months based on tenure and age. This makes the relevant cutoff for workers experiencing a bankruptcy individual-specific and within the range of 60 years and 6 months to 60 years and 11 months. Further details are outlined in Section 2.

approximately half being substituted with DI benefits.[6] Second, substantial heterogeneity exists in benefit substitution behavior among workers within our sample. The extent of benefit substitution is most pronounced for workers with lower earnings, those with limited educational attainment, and those employed in the manufacturing industry. Third, we do not find any evidence of ER eligibility having an impact on labor supply. While our point estimate indicates that re-employment rates for workers displaced just before becoming eligible for ER being only 2 percentage points higher than those for workers displaced just after becoming eligible, our estimate do suffer from limited statistical power, making us unable to draw any firm conclusions. Similarly, our point estimate for labor market earnings also remains small and statistically insignificant, accompanied by a considerable standard error. Fourth, due to the high degree of benefit substitution, our findings indicate that the provision of ER benefits had little impact on individual welfare and net public expenditures. While somewhat imprecisely estimated, our point estimate indicates that the individual consumption gain is \$11,000 per individual over a five-year period, or approximately \$2,200 annually. Similarly, the estimated increase in net public expenditures is more than three times larger amounting to approximately \$7,400 annually. Fifth, when evaluating the social desirability of offering ER benefits, the utilization of the Baily-Chetty formula reveals that a high level of risk aversion of 5.9 would be required for the ER option to result in welfare improvement for involuntary displaced elderly workers. We further show that a very low share of ineligible individuals are significantly worse off because of failing to qualify for ER. This implies that the provision of ER benefits likely is suboptimal in our context.

Since our estimation sample consists exclusively of workers from bankruptcy firms, we undertake measures to ensure that our estimates possess external validity and can be generalized to a more extensive range of workers who experience job loss, including scenarios like "mass layoffs". Although using workers who depart from firms during "mass layoffs" could introduce endogeneity concerns in our context, we demonstrate that our estimation sample is comparable to workers employed in firms that experience significant plant downsizing. Consequently, our empirical findings can be interpreted as having broader implications, and shedding light on the wider pool of elderly workers who become involuntarily displaced.

We believe our analysis is of general interest for three main reasons. First, economic hardship throughout the OECD may lead to a rise in job displacements and decreased labor demand, particularly impacting elderly workers who are typically less desirable hires. Second, numerous countries have implemented ER schemes to offer greater flexibility in labor market withdrawal and to reduce enrollment onto other social security programs. However, the implementation of such programs has proven to be financially burdensome. Our study offers insights into a substantial shock to ER entitlements, given that losing eligibility results in the complete forfeiture of the ER option, equivalent to the benefits over a span of five years. Moreover, our analysis encompasses the entire ER period, allowing us to comprehensively assess effects related to employment, program substitution, and public costs. Third, many countries are engaged in discussions regarding potential reforms in the social security system to ensure fiscal sustainability. This deliberation could lead to strategies like tightening eligibility criteria or reducing benefit amounts. Our research provides evidence that can inform policymakers about the potential advantages and disadvantages of such strategies.

Given our paper's primary focus on elderly displaced workers, its main relationship lies with the literature that examines the impact of extended unemployment insurance (UI) for elderly workers. Among closely related studies, a few have demonstrated that prolonged UI benefits can discourage active job searching, leading to longer unemployment durations. These benefits might even serve as a bridge to retirement.[7] Inderbitzin *et al.* (2016) showed that extended UI has strong effects on labor market exit through ER, and increased exit through the DI channel. Kyyrä & Ollikainen (2008) used a reform in

[6]It is worth noting that while the substitution onto DI is notably high, we emphasize that around 23 percent of the population, on average, claims DI between ages 62 and 67. This proportion increases to about 48 percent for those who experience late-career job displacement and concurrently do not meet ER eligibility. DI take-up is thus high in Norway in general, likely as a result of quite generous benefit levels.

[7]While this literature is often interested in the push and pull factors of UI systems for older workers into unemployment (e.g. Tuit & van Ours, 2010 and Baugelin & Remillon, 2014), we do not explore this margin in our paper.

Finland which increased the eligibility age for extended UI from 53 to 55 and later in Kyyrä & Pesola (2020) from 55 to 57 to study the effects on ER and labor supply, respectively. Kyyrä & Ollikainen (2008) documented a decrease in ER from the first increase in access age, while Kyyrä & Pesola (2020) documented increased employment over the remainder of the working career, and no substitution onto other programs. In contrast to the extended UI literature, our study centers on workers who are much closer to the normal retirement age. Our contribution to this body of work is through the examination of the effects of having the option to retire early, leading to a complete exit from the labor market. This distinct focus could yield implications that fundamentally differ from those associated with extended UI periods.

Our paper is also related to the extensive literature on ER programs and alterations to the early retirement age's impact on labor supply and program substitution (e.g. Geyer & Welteke, 2021; Manoli & Weber, 2016; Atalay & Barrett, 2015; Stefan Staubli and Josef Zweimüller, 2013 among others). A consistent finding within this body of work is that raising the retirement age leads to increased employment, yet evidence regarding program substitution outcomes is mixed. In the context of Norway, Hernæs *et al.* (2016) analyzed a recent reform of the Norwegian pension system that introduced greater flexibility in withdrawal options, while Johnsen *et al.* (2021) used the rollout of the Norwegian ER program, effectively studying a reduction in the normal retirement age. Both studies converged on the finding that workers tend to reduce their uptake of disability insurance benefits in response to enhanced retirement program flexibility. Vigtel (2018) presented a labor demand perspective, demonstrating that the reduction of the early retirement age for a specific subset of workers in Norway prompted risk-averse firms to become more inclined to hire senior employees. Zwick *et al.* (2022) explored the potential adverse consequences of eliminating the ER option, discovering similar labor supply responses for women across both high- and low-demanding jobs, with no observed substitution effects onto other programs. While many of these studies concentrate on the interplay between two programs or their impacts on employment, our research delves into the spillover effect onto the entire spectrum of social security programs for which elderly workers could qualify. In this regard, we contribute to the literature by broadening the scope of our investigation into program substitution. Additionally, our paper aligns with the literature that focuses on the ramifications of tightening eligibility policies for social benefits more broadly.[8]

Finally, our paper is related to an extensive literature on the effects of job displacement (e.g. Jacobson *et al.*, 1993; Davis & Von Wachter, 2011; Lassus *et al.*, 2015; Marmora & Ritter, 2015; Ichino *et al.*, 2017; Huttunen *et al.*, 2018 among others). Across these studies, a common thread is the identification of substantial adverse effects on earnings and employment, both in the short and long term. Of particular relevance to our study is Bratsberg *et al.* (2013), who utilized data on Norwegian bankruptcies to reveal a significant correlation between job displacements and subsequent claims for disability insurance (DI). They found that changes in employment opportunities driven by exogenous factors significantly influence non-participation in the labor market. Meanwhile, Marmora & Ritter (2015) found that unemployment late in workers' careers influences retirement timing, with the effect being more pronounced once workers qualify for social security benefits. More recently, Ichino *et al.* (2017) demonstrated that both older and younger workers bear comparable substantial displacement costs in terms of long-term employment consequences. However, the immediate losses experienced by older workers are more pronounced but tend to recover over time. Although our paper does not center on the direct examination of job displacement effects, we do illustrate how an external alternative for displaced workers impacts re-employment rates and enrollment in various social security programs.

The remainder of this paper is organized as follows. First, we offer an overview of the Norwegian ER program,coupled with a brief account of the corresponding public transfer systems in Section 2. Then, in Section 3, we introduce the administrative data utilized in our analysis. In Section 4 we outline our empirical strategy. In Section 5 we present our main results. In Section 6 we assess the implications of our findings for both policy and welfare. Finally, we conclude in Section 7.

[8]Borghans *et al.* (2014); Karlström *et al.* (2008); Staubli (2011) documented significant spillover effects onto other programs resulting from stricter DI eligibility criteria in the Netherlands, Sweden and Austria, respectively.

## 2 Institutional setting

Our primary focus revolves around elderly workers in private sector firms covered by the ER scheme who experience an involuntary job displacement due to bankruptcy of the firm they work in. Consequently, the institutional background information comprises an outline of the ER program along with its eligibility criteria, which encompass specific regulations pertaining to job displacements. Additionally, we provide a brief overview of other social security programs that workers may be eligible for, with special emphasis on the disability insurance (DI) program and the unemployment insurance (UI) program.

### 2.1 Early retirement program (AFP)

The ER program, denoted by its acronym AFP ("Avtalefestet pensjon"), was introduced in 1988. While full coverage was established for public sector workers upon its inception, approximately half of private sector workers were covered, with this rate experiencing a gradual increase over time. In private sector firms, membership in the program is voluntary and necessitates a collectively negotiated collective pay agreement. Workers within member firms are enrolled irrespective of their individual union affiliations. The ER program's funding is a combination of governmental contributions and payments from participating firms.Until November 2010, the ER program granted enrolled workers the option to claim a full pension starting from the age of 62, in contrast to the normal retirement age set at 67 years through the National Insurance Scheme.[9]

**Eligibility criteria** The ER scheme featured a distinct lock-in (or lock-out) mechanism. Workers were mandated to have been employed in *the same firm* covered by the ER scheme for the preceding three years before becoming eligible to claim benefits. Alternatively, eligibility could be attained by having been employed in *any firm in the same sector* covered by ER for the last five years, with the last two years being in within a single firm.[10] Furthermore, the firm had to employ a minimum of two workers, excluding the firm's owner. On the day of claim initiation, the worker was required to be actively employed by the firm, and the earliest feasible time for claiming benefits was the start of the month when the worker turned 62 years of age. To be counted as "actively employed", the worker's salary had to be at least equivalent to approximately $\$10,000$ (in 2015 dollars) in annual earnings, with this firm being the worker's main employer. Lastly, the ability to claim ER benefits was precluded if a worker was simultaneously seeking DI benefits.

An exception was established in the situations involving mass-layoffs or bankruptcy. n cases where the work relationship was terminated due to either of these events, the worker's eligibility for ER was upheld for 52 weeks following the occurrence plus the duration of the standard notice period. The Norwegian Work Environment Act determines this standard notice period, which is determined by the worker's tenure and age, ranging from a minimum of 1 month to a maximum of 6 months.[11] Essentially, this implies that workers who experienced job loss resulting from bankruptcy or mass-layoffs could retain ER eligibility for up to 18 months after the incident. Regardless of eligibility for ER, elderly workers at firms with ER coverage could receive a lump sum severance payment of about $8,000 if being displaced due to a mass-layoff or bankruptcy.[12]

**Benefits levels** The ER benefit level corresponded exactly to the old-age pension benefit that a worker would receive from the National Insurance Scheme if they claimed the pension at the age of 67, plus

[9] Although the program was initially introduced with a minimum claiming age of 66 years, this threshold underwent a series of reductions, culminating in 1998. This final reduction established the minimum legal claiming age for ER at 62 years. Notably, the structure of the ER program underwent modifications in 2011; however, these reforms do not apply to our sample, given that workers within our cohorts spanning from 1939 to 1948 were entirely governed by the preceding rules.

[10] For instance, switching jobs between private and public sector firms just before retirement would lead to loss of eligibility for ER benefits, even if both the private and the public firm were covered by ER.

[11] The precise relationship between age, tenure and the notice period is detailed in Equation (2) in Section 4.

[12] For our sample, severance payments amount to $7,778 for individuals aged 59 years, $8,333 for individuals aged 60 years and $8,889 for individuals aged 61 years.

an additional flat-rate “top-up” of about $2,300.[13] The old-age pension benefit level received at age 67 was unaffected by claiming ER benefits. We provide a detailed overview of how old-age benefits were calculated in Appendix 7. Claimants were subject to a pro-rata earnings test on continued work that exceeded a minimal tolerance level. This effectively resulted in a near 100 percent marginal tax rate on continued work for those claiming ER benefits. On average, annual benefits amounted to roughly $24,000 in 2001 and approximately $27,000 in 2010.

## 2.2 Other social security benefits

**Disability insurance** Individuals classified as having a permanent reduction in earnings capacity due to illness or injury are eligible for disability insurance (DI) benefits. The extent of this benefit may be partial, contingent on the residual earnings capacity. To qualify for disability benefits, an individual must fall between the ages of 18 and 67, and they must have maintained membership in the National Insurance Scheme within the three years prior to their incapacitation. The primary reason for reduced earnings capacity must be illness or injury, adequate vocational rehabilitation measures must have been undertaken, and the earnings capacity should be permanently diminished by at least 50 percent.[14] During the time frame under consideration in this paper, the benefit level corresponded to the old-age pension benefit, thereby making it nearly equivalent to ER benefits (the sole distinction being the $2,300 annual ER benefit "top-up"). Individuals receiving DI were subject to an earnings test that translated into a marginal tax rate of around 60 percent in cases where earnings exceeded approximately $10,000.[15]

**Unemployment benefits** In order to qualify for unemployment benefits, an individual must be registered as a job-seeker with the Norwegian Labour and Welfare Administration. Essential criteria include having one’s working hours reduced by at least half, being genuinely in search of employment, being a member of the National Insurance Scheme, possessing legal residency, and having earned at least $15,000 in income in the previous calendar year, or a combined total of $30,000 over the past three calendar years.If the individual’s income prior to unemployment exceeded $20,000, they may receive unemployment benefits for a maximum of 104 weeks. Conversely, if their income was below $20,000, the maximum period is reduced to 52 weeks. A recipient of unemployment benefits is entitled to 62.4 percent of their previous earnings. The computation of these past earnings encompasses either the last 12 months before unemployment or the annual average of the last 36 months, should the latter surpass the former.

**Other public transfers** In addition to disability and unemployment insurance, workers within our sample could potentially qualify for various other social security benefits. One notably pertinent program for elderly workers is sickness benefits, which serves to compensate for income loss stemming from short-term sickness (up to one year) among employed individuals who are members of the National Insurance Scheme. A complete sickness benefit fully covers earnings within the past year, up to approximately $60,000. While of lesser relevance for elderly individuals in comparison to those in their prime working years, workers within our sample might also be eligible for temporary disability insurance (DI) benefits. The temporary DI program underwent several modifications during our sample period, but it essentially offers financial support during periods when a person is unwell or injured and is attempting to reintegrate into the workforce. Temporary DI benefits were extended for a duration of 1 to 4 years for most individuals within our sample.[16] Additionally, individuals in our sample may also

[13] In 2015 dollars. Throughout the paper, we measure monetary values in 2015 dollars given an average exchange rate of NOK/USD = 9.

[14] Under specific conditions, DI benefits might be granted even if the earnings capacity reduction is below 50 percent. For instance, if the individual is currently enrolled in the Work Assessment Allowance program, 40 percent reduction suffices, and for cases where the decreased earnings capacity arises from an approved occupational illness or injury, a 30 percent reduction is adequate.

[15] For earnings surpassing this threshold, each dollar earned was subjected to the earnings test. Post-2005, only earnings exceeding the threshold were subject to the earnings test if the individual had been granted DI benefits in 2003 or earlier.

[16] Prior to 2010, the temporary DI program encompassed three distinct components: Rehabilitation benefits (lasting up to 1 or 2 years), occupational rehabilitation benefits (no upper time constraint), and time-constrained DI benefits (lasting up to

be eligible for a few less relevant benefit programs such as social assistance and child support.

# 3 Data and sample selection

In our empirical analysis, we utilize data from two primary sources that can be connected through unique and anonymized identifiers assigned to each resident individual and employer. The primary data originates from Statistics Norway (SSB) and provides comprehensive information about individual attributes and employer-employee connections, including precise dates for each employment relationship. This allows us to create monthly records of earnings and employment for each individual and company. Within the employer-employee data, details about firm characteristics are included, such as 5-digit industry codes (NACE) and the precise date of bankruptcy (if applicable). As a result, we can identify individuals employed in firms that undergo bankruptcy. Our second data source is supplied by *Fellesordningen for AFP*, encompassing information about the specific dates when each firm is associated with the ER-scheme.[17] This data permits us to establish whether individuals are qualified for ER based on their employment connection, which is a critical aspect of our analysis. For our key outcome variables, we rely on annual data concerning individual earnings, social security benefits, and assets from reported tax records at SSB. The data span the years from 1999 to 2014.

The administrative nature of our data notably diminishes the extent of measurement errors concerning income variables and employment connections. Given that individual employment affiliations and income details are reported by third parties (namely, employers and tax authorities), their coverage and reliability receive high marks in international quality assessments (see e.g. Atkinson *et al.*, 1995). Since administrative data are a matter of public record, there is no attrition due to non-response or non-consent by individuals or firms, and individuals can only exit these data sets due to natural attrition (death or out-migration).

## 3.1 Sample selection

In our empirical analysis, our primary estimation sample encompasses workers aged 59 to 61 years at the point when their employing firm undergoes bankruptcy. This upper age limit is instituted to mitigate the risk of selection bias. By setting this threshold, we ensure that individuals within our estimation sample have not yet finalized their decision regarding early retirement, as workers in firms encompassed by the ER scheme become eligible for ER benefits beginning at the age of 62. The lower age restriction serves to provide a 12-month buffer on both sides of the eligibility cutoff within our Regression Discontinuity (RD) analysis. Two potential concerns arise in our context. Firstly, firms might lay off workers before the actual occurrence of bankruptcy. Secondly, workers could anticipate potential job loss and decide to leave preemptively. In order to counteract the selection of workers based on these concerns, our main estimation sample encompasses workers who were employed in a firm covered by the ER scheme 24 months prior to the firm's bankruptcy date. Consequently, we predetermine both worker and firm characteristics up to this initial time point when workers are aged 57 to 59. While our estimates exhibit remarkable stability across various specifications of when we predetermine employment affiliation, we assess alternative worker samples with pre-determined affiliations established 12 months and 1 month before the firms' bankruptcy dates as part of robustness checks.

We further apply the following sample restrictions in accordance with the eligibility criteria of the ER program, as outlined in Section 2.1. One of these criteria stipulates that individuals must have worked for a minimum of 3 consecutive years in the same firm with ER coverage. To be eligible for ER benefits at the age of 62, we thus mandate that individuals initiated their employment relationship before turning 59 years old. Additionally, we necessitate that the specific employment relationship was the individual's primary employer (i.e., the one with the highest earnings) if the individual had multiple employers, as only the primary employment relationship was considered for eligibility. Third, we require

4 years). These were subsequently replaced by the Work Assessment Allowance program in March 2010, which provided benefits for up to 4 years as a general rule.

[17] Fellesordningen for AFP stands as the dominant private sector organization for ER schemes and covers nearly the entire market ($\approx 99\%$).

that individuals were not participants in the DI program, as recipients of this program were ineligible for ER benefits. Fourth, we require that firms have at least 2 employees, as workers were deemed ineligible if they were the sole employee at the firm. Finally, we require that individuals worked at least 20 percent of a full-time position, which corresponds to approximately $10,000 in annual earnings and satisfies the final eligibility criterion for ER.

Although our data provides information on registered firm bankruptcies, some of these firms may undergo changes in ownership and retain a portion of their workforce, resulting in few or no job displacements despite the initial bankruptcy declaration. Given our focus on workers who genuinely experience job displacements, we implement a criterion related to the fraction of workers (which includes younger workers not within our estimation sample) who continue to work in the same firm post-bankruptcy. In our baseline specification, we set our threshold to 1/3, meaning that if more than 1/3 of all workers in the bankrupt firm (excluding "self"[18]) are still employed in the *same firm* 12 months after the bankruptcy event, it is categorized a "spurious bankruptcy" and the entire firm is excluded from our main estimation sample. However, we do incorporate these firms in an alternative sample as a part of a robustness check.

As our data begins in 1999, our main estimation sample includes bankruptcies in private sector ER-firms during January 2001 to November 2010 and individuals in cohorts 1939 to 1948.[19] This means that for firms becoming bankrupt in 2001, our workers must have been employed by the firm in 1999. As our data ends in 2014, we are able to follow individuals during the entire potential ER period until they reach the normal retirement age of 67 years.

While we use firm bankruptcies to identify displacements in order to avoid selection in leaving, workers may also be displaced due to e.g. mass layoffs which is commonly used in the literature to investigate effects of job loss (e.g. Jacobson *et al.*, 1993; Rege *et al.*, 2009; Huttunen *et al.*, 2011; Davis & Von Wachter, 2011 among others). This raises the question whether our main estimation sample are representative for the wider pool of displaced workers. We therefore include a sample of workers who are displaced following a plant downsizing as a comparison sample in our analyses. For this sample, we include firms with at least 10 employees which downsize its labor force (from one month to the next) by at least 30 percent, excluding bankruptcy firms. We otherwise impose the same sample restrictions as our main sample. A particular concern using mass layoffs in this case is selection in leaving, as workers who would retain their eligibility for ER may leave voluntarily and accept a severance payment. The opposite may also be the case; workers who are below the threshold may be kept by the firm in order to ensure that ER eligibility is not lost. We investigate this issue by looking for manipulation in ER eligibility reported in Appendix Figure A.1b. Following Frandsen (2017), a formal test rejects the null of no manipulation around the eligibility cutoff. Although this invalidates our RD design, we use this sample as a comparison sample to investigate external validity of our main findings.

### 3.2 Descriptive statistics

In Table 1 we present summary statistics for elderly displaced workers (within 12 months) around the ER eligibility cutoff. The first column includes our main estimation sample of individuals who worked in a private sector firm with ER coverage 24 months before the bankruptcy. The second column includes workers in bankruptcy firms without ER coverage which we use as a placebo sample in our empirical analysis. The third column combines the two bankruptcy samples. The fourth, fifth and sixth columns include individuals who were displaced as the firm downsized its labor force by at least 30 percent (excluding bankruptcies) which we use as a comparison sample to investigate external validity of our main findings.

Overall, the main estimation sample is fairly comparable to workers who are displaced following

[18] For instance, if a firm with 10 employees goes bankrupt, it is considered a "spurious bankruptcy" if $n > (10-1)/3$ workers remain in the same firm a year after the bankruptcy. Here, the subtraction of one represents the worker themselves.

[19] We restrict our attention to bankruptcies occurring before the 2011 Norwegian pension reform for two reasons: the reform changed the rules regarding eligibility and work incentives for individuals claiming ER benefits. While workers in our sample could in principle become eligible for ER benefits under the new scheme following the reform, individuals in our sample had to postpone claiming after the initial claiming month when turning 62 years, and had to be re-employed in a firm with ER coverage to satisfy the new eligibility criteria. Only 3% of our sample claim ER benefits under the new scheme, compared to 37% claiming before the reform.

a plant downsizing with a few small differences. Individuals in our main estimation sample are more likely to be male and have slightly lower earnings on average. They are also more likely to receive sickness benefits, which may suggest they have poorer health. By construction, firms are also smaller on average in terms of the number of employees, as the comparison samples consist of firms with at least 10 employees to identify mass layoffs. Along the industry dimension, the estimation sample is also fairly comparable to the full sample of downsizing firms. Note that firms with ER coverage are far more likely to be in the manufacturing industry than firms without ER coverage. Thus, firms in the estimation sample are slightly more likely to be in the manufacturing industry compared to the full comparison sample. They are also somewhat more likely to be firms in the construction industry but less likely to be firms in other industries.

**Table 1:** Summary statistics of elderly displaced private sector workers

| | Bankruptcy samples | | | Downsizing samples | | |
|---|---|---|---|---|---|---|
| | **Est. sample:** | **Placebo:** | | **Comparison samples:** | | |
| Firm covered by ER scheme: | **Yes** | **No** | **All** | **Yes** | **No** | **All** |
| *Individual characteristics:* | mean | mean | mean | mean | mean | mean |
| Age | 58.3 | 58.2 | 58.2 | 58.2 | 58.3 | 58.2 |
| Fraction females | .22 | .27 | .25 | .29 | .39 | .33 |
| Fraction married | .73 | .72 | .73 | .73 | .71 | .72 |
| Years of education | 10.9 | 11.1 | 11.0 | 11.0 | 11.1 | 11.0 |
| Number of children | 2.1 | 2.3 | 2.2 | 2.1 | 2.2 | 2.1 |
| Wealth ($1,000) | 92 | 91 | 91 | 88 | 105 | 94 |
| *Labor market characteristics:* | | | | | | |
| Monthly earnings ($1,000) | 4.1 | 3.8 | 3.9 | 4.5 | 4.4 | 4.5 |
| Fraction full time employment | .93 | .86 | .88 | .92 | .86 | .90 |
| Tenure (years) | 8.9 | 6.2 | 7.1 | 10.4 | 6.4 | 9.0 |
| Number of employees | 89 | 12 | 39 | 202 | 50 | 150 |
| Fraction receiving sickness benefits | .14 | .10 | .12 | .10 | .08 | .09 |
| Local DI rate | .10 | .10 | .10 | .11 | .10 | .10 |
| Local unemployment rate | .02 | .02 | .02 | .02 | .02 | .02 |
| *Industry (%):* | | | | | | |
| Primary sector | 1.3 | 3.1 | 2.5 | 1.1 | 3.6 | 2.0 |
| Manufacturing | 66.8 | 18.5 | 35.3 | 67.5 | 30.8 | 54.7 |
| Construction | 10.3 | 14.4 | 13.0 | 4.4 | 4.1 | 4.3 |
| Wholesale retail and trade | 13.9 | 37.6 | 29.4 | 10.1 | 25.6 | 15.5 |
| Transportation and storage | 1.3 | 8.2 | 5.8 | 3.6 | 12.3 | 6.6 |
| Scientific and legal activities | 1.3 | 5.3 | 3.9 | 2.7 | 2.6 | 2.7 |
| Other | 4.9 | 12.9 | 10.2 | 10.7 | 21.0 | 14.3 |
| Number of firms | 127 | 372 | 499 | 206 | 164 | 370 |
| Number of individuals | 223 | 417 | 640 | 366 | 195 | 561 |

*Notes:* Bankruptcy samples include individuals who work in a private sector firm 24 months before the firm's bankruptcy date. Downsizing samples include individuals who worked in private sector firms with at least 10 employees who were displaced as the firm downsized its labor force by at least 30 percent (excluding bankruptcies). Variables are measured 24 months prior to downsizing. All samples include individuals who satisfy the initial ER eligibility criteria and whose age is within 12 months of the minimum age for being eligible for ER in case of a mass layoff (see section 2.1). "Spurious" displacements are excluded, defined as at least 1/3 of employees (excluding "self") working in the same firm 12 months after displacement. Local DI and unemployment are measured at the municipality level. Earnings and wealth are measured in 2015 dollars (NOK/USD = 9).

# 4 Empirical framework

This section first presents the assignment rule that creates local random variation in eligibility for the ER program. We then present the regression discontinuity design that we use to identify effects of ER eligibility and discuss threats to identification.

**Assignment variable** As our proxy for job displacements comes from bankruptcies, our assignment variable is based on the age of individual $i$ at the time of the bankruptcy of the firm. As explained in Section 2.1, individuals are, in normal cases, eligible for ER from the age of 62. However, in the case of bankruptcies, workers are granted an additional 52 weeks plus the individual notice period. Hence, our assignment variable (measured in months) is defined as:

$$a_i = age_i - (61 - NP_i) \tag{1}$$

where $age_i$ is individual $i$'s age at the bankruptcy date and $NP_i$ is the *notice period* (in months) of individual $i$, which is governed by the Norwegian Work Environment Act, according to:

$$NP_i = 1 + T_{i,5} + T_{i,10}(1 + \mathbb{I}_{i,50} + \mathbb{I}_{i,55} + \mathbb{I}_{i,60}) \tag{2}$$

where $T_{i,y}$ is a dummy equal to one if individual $i$ has at least $y$ years of tenure and $\mathbb{I}_{i,\bar{a}}$ is a dummy equal one if individual $i$ is at least as old as age $\bar{a}$. The relationship implies that individuals in our sample have a notice period of 1–6 months depending on age and tenure. If $a_i$ is positive (negative), then the firm went bankrupt sufficiently late (too early) and individual $i$ is initially eligible (ineligible) for ER benefits. We emphasize that the eligibility cut-off is specific to displaced workers whose age will be 60 years and 6–11 months depending on their individual notice period. As the standard eligibility cut-off is 62 years, there is no discontinuity in the counterfactual scenario where workers are not laid off.

## 4.1 Regression discontinuity (RD) design

As discussed in Section 3.1 we pre-determine our main estimation sample 24 months prior to bankruptcy. While we do this to mitigate endogeneity issues due to e.g. selective leaving, some workers may not be eligible for ER at the time of bankruptcy even though they were initially eligible based on their status 24 months prior. This could be due to workers either anticipating bankruptcy or the firm laying off workers before the actual bankruptcy. Additionally, some workers may not be displaced at all as new owners may keep a share of the workforce in the event of a takeover, while other workers may become re-employed by a different employer. These individuals may become eligible for ER at a later stage despite being initially ineligible.

Figure 1 shows the monthly employment rates for our main estimation sample of ER-workers around the bankruptcy of the firm. In panel 1a, we plot the fraction of workers employed by the bankruptcy firm. While everyone was employed 24 months prior by construction, just over 60 percent of workers were still employed by the firm in the month of bankruptcy. This indicates that a significant share of workers either left early or that the actual lay-off occurred before the bankruptcy date. There were very few who were still employed by the firm after bankruptcy. In panel 1b, we plot the fraction of workers who were employed by any firm around the bankruptcy date of the original firm. Around 80 percent of workers were still employed in the month of the bankruptcy, while around 25 percent were employed in the month after the bankruptcy. This suggests that a substantial share of workers were re-employed either by new owners of the bankruptcy firm or by a different firm, and may regain eligibility for ER despite being initially ineligible.

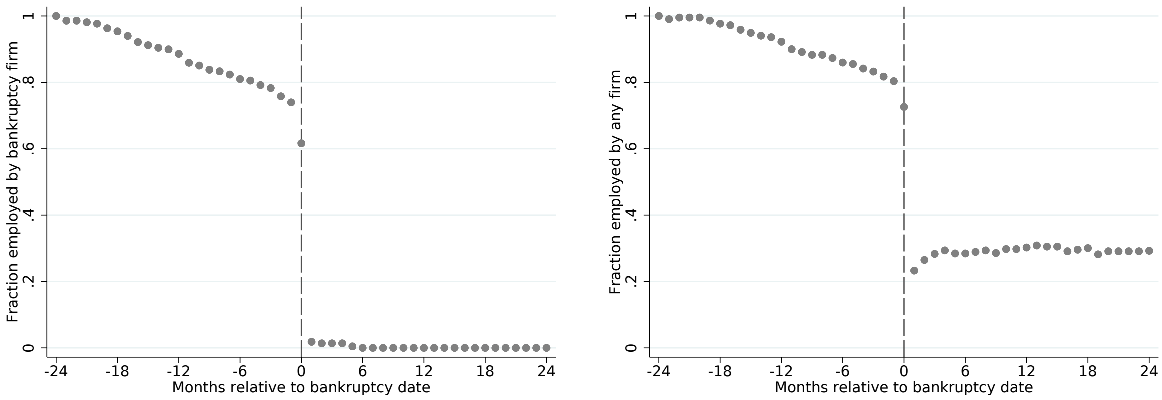
Because of the setting described above, we choose two approaches to estimate effects of ER eligibility. Our first approach incorporates a reduced form RD model using each individuals' initial eligibility status 24 months prior to bankruptcy as the measure of our assignment variable. The estimated effects of this model yields intention to treat (ITT) effects based on individuals initial eligibility status and can be considered as lower bound estimates of optional ER. In an attempt to quantify the "true" effects of optional ER, we also use a fuzzy RD model for our main estimates where we use the assignment variable as an instrument for ER eligibility. Under certain assumptions, this approach yields the local average treatment effect (LATE), that is the average effect of having the option to retire early for compliers in our sample (Imbens & Angrist, 1994).[20] While we are concerned about measurement errors in our treatment

[20] In our setting, the compliers are the workers who become eligible for ER because their age is above the eligibility cut-off.

variable in particular, this approach is useful for better understanding the effects of having an ER option. As we will explain more thoroughly when outlining the model and assumptions, the IV estimates can be interpreted as upper bound estimates of optional ER.

**Figure 1:** Employment around bankruptcy date

**(a)** Fraction employed by bankruptcy firm  **(b)** Fraction employed by any firm




**Reduced form model (ITT)** In our reduced form RD model, assignment to eligibility is a deterministic function of the assignment variable $a$, the age at bankruptcy including each individual's notice period as defined in Equations (1) and (2). Individuals are initially eligible for the ER program if $a \geq 0$. The model can be summarized by the following equation:

$$y_{it} = \alpha + f(a_i) + \beta I_{a_i \geq 0} + \delta X_{it} + \varepsilon_{it} \tag{3}$$

where $y_{it}$ denotes the outcome of individual $i$ at time $t$, $X_{it}$ is a set of covariates, $\varepsilon_{it}$ is the error term and $f$ is an unknown functional form of the assignment variable. The reduced form RD estimate is given by $\beta$, i.e. the coefficient on $I_{a_i > 0}$ which is an indicator variable equal to 1 above the cut-off and 0 otherwise.

In our baseline specification, we follow Lee & Lemieux (2010) and use a local linear regression with separate linear trends and a rectangular kernel density on each side of the cut-off. While we consider multiple outcome variables in our analyses, we keep our bandwidth fixed in our baseline specification. Although different outcomes have different optimal bandwidths, we choose a bandwidth of 12 months (of age) which is in the neighborhood of the optimal bandwidth suggested by Imbens & Kalyanaraman (2012) for two of our key outcome variables ER benefits and (total) non-pension social security benefits. We also show that our estimates are relatively robust to bandwidth selection in Section 5.4.

**Instrumental variable model (IV)** In our fuzzy RD design, the empirical model can be summarized by the following two equations:

$$E_i = \alpha_0 + \alpha_1 \mathbb{Z}_{a_i \geq 0} + f(a_i) + \delta X_{it} + \varepsilon_{it} \tag{4}$$

$$y_{it} = \beta_0 + \beta_1 E_i + f(a_i) + \delta X_{it} + \varepsilon_{it} \tag{5}$$

where $E_i$ takes the value one if individual $i$ is eligible for ER and zero otherwise, $X_{it}$ is a set of covariates and $y_{it}$ is the outcome of interest for individual $i$ at time $t$, $\varepsilon_{it}$ is the error term and $f$ is an unknown functional form of the assignment variable. The indicator variable $\mathbb{Z}_{a_i \geq 0}$ is the instrumental variable, where $a_i$ is defined as in Equation (1), meaning that if individuals' age at the bankruptcy date is above the threshold, the instrument takes the value one, and zero otherwise. It is crucial that $\mathbb{Z}$ is uncorrelated with potential measurement error in $E$. While we are able to construct a fairly accurate measure for eligibility by determining who are eligible based on the criteria outlined in Section 2.1, we cannot observe eligibility directly. Because of this, it is possible that our treatment variable is measured with some errors.[21] While measurement error in the treatment variable in an IV setting creates a bias in the estimator (see e.g. Lewbel, 2007; Jiang & Ding, 2020; Yanagi, 2019), Ura (2018) and Yanagi (2019) showed that under the assumption that the instrumental variable is uncorrelated with the measurement error in the treatment variable (i.e. the probability of misclassification of treatment), the Wald estimator gives an upper bound estimate in absolute value of the true coefficient.

Additionally, it is not clear how to define the treatment in our setting as individuals' eligibility status could change depending on employment status and the various other criteria for ER eligibility. Therefore, some non-treated or treated individuals could be partially treated. We decide to define treatment as eligible for ER at some point between ages 62–67 years as most partially treated individuals will regain eligibility shortly after the earliest point of withdrawal (e.g. at ages 62 or 63). In practice, this means that our estimates will serve as upper bound estimates as some individuals we define as treated will be partially treated. As potential measurement errors in our treatment variable will also contribute to overestimate the true effects, we therefore emphasize that the IV estimates should be interpreted as upper bound estimates of optional ER. However, we argue that the IV estimates are useful for scaling and interpretation of the effects of our main outcomes in particular.

A key identifying assumption for the IV to be valid is the exclusion restriction, i.e. the instrument must be conditionally independent of potential outcomes. We argue that the exclusion restriction holds in our case as just being above the cut-off age in itself does not affect employment or take-up of other social security benefits. It is only because this affects eligibility and in turn make some individuals claim social security benefits. As a further argument for this claim, our placebo estimates of non-ER workers reported in Table A.4 in the appendix indicate that outcomes of ineligible individuals are indeed similar around the age-threshold. Another key identifying assumption is monotonicity in responses. We consider "defiers" highly unlikely in our setting as this would imply that some individuals become eligible because their age is just below the threshold but would not have become eligible otherwise. Finally, the instrument must be relevant, i.e. just reaching the individual age-threshold must affect eligibility. Appendix Figure A.2 illustrates a highly significant jump in the fraction of individuals who satisfy the eligibility criteria at the threshold, from about 20 percent of individuals just below the cut-off to about 90 percent of individuals just above the cut-off.

### 4.2 Threats to identification

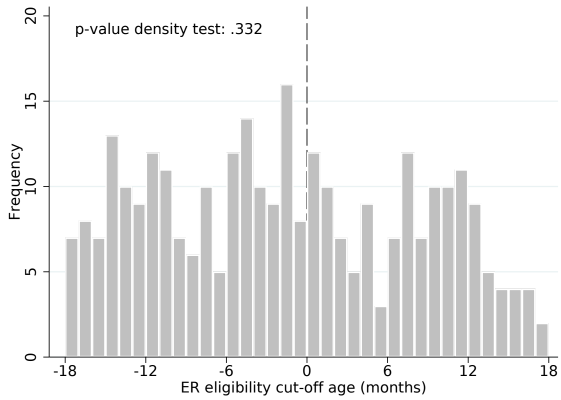
The validity of our identification requires that individuals are not able to precisely manipulate the assignment variable, which in our setting is their age at the bankruptcy date. As individuals cannot manipulate their age, the only possible way to manipulate the assignment variable is by manipulating the bankruptcy date itself. While we consider this highly implausible, we carry out the standard validity checks for RD designs. Figure 2 shows the distribution of the assignment variable around the cut-off. Because our assignment variable is discrete, we follow Frandsen (2017) and perform a formal statistical test for bunching on either side of the cut-off. Reassuringly, the test results do not allow us to reject the null hypothesis of no bunching.

If individuals are unable to manipulate the assignment variable, any pre-determined covariate should have the same distribution on either side, close to the cut-off. As a formal test, we run reduced form regressions with our baseline specifications on worker characteristics as the dependent variable, each measured 24 months prior to the bankruptcy. The point estimates and standard errors are reported in Appendix Table A.1. We also present these results graphically in Appendix Figure A.3. Reassuringly, key covariates such as monthly earnings, tenure, and the number of employees in each firm appear smooth around the cut-off and are insignificant at all conventional test levels. One exception is the local DI rate (measured at the municipality level) which is significant at the 5% level. However, based on the large number of covariates that we consider, the probability of observing changes in one covariate around the cut-off is quite large. Additionally, the correlations between the local DI rate and the outcome

[21] Out of the 199 individuals we classified as ineligible following the standard criteria, 4 individuals in our sample or around 2 percent were observed with actual take-up of ER benefits. Unfortunately, we are unable to provide a measure of the number of individuals we classify as eligible whose true status are in fact ineligible as we cannot distinguish these individuals from never-takers of ER benefits.

variables we consider are very small and close to zero. When we perform a joint test for all covariates, we cannot reject the null of no manipulation at any conventional level as reported in Appendix Table A.1.

**Figure 2:** Distribution of eligibility age around cut-off




*Notes:* The figure shows the distribution of age (in months; defined as in Equation (1)) around the individual ER eligibility cut-off. P-value is calculated using the discrete density test of Frandsen (2017). The sample consists of individuals employed by a firm with ER coverage 24 months before the firm's bankruptcy date who satisfied the initial ER eligibility criteria (see details in Section 3.1).

# 5 Main results

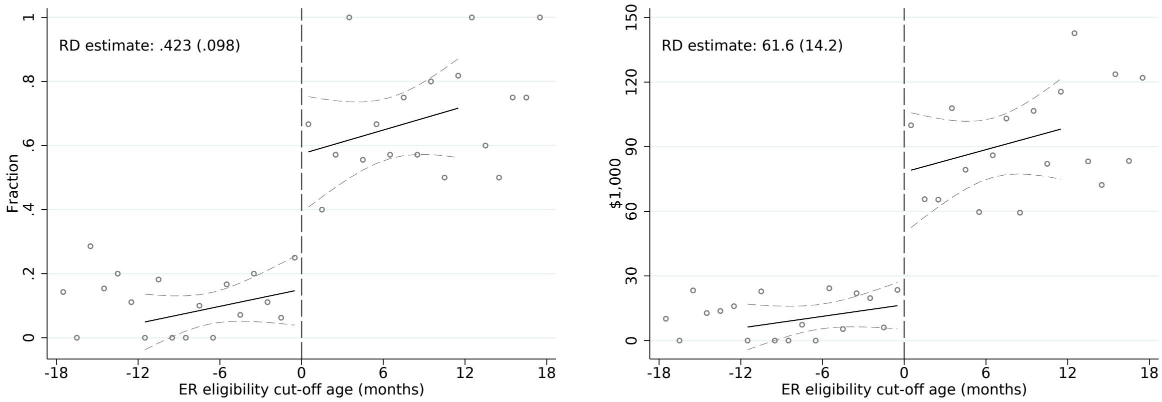
We now turn to our main results. First, we present the direct effect on take-up of ER benefits from reaching the individual cut-off date before the bankruptcy occurs. We then investigate the effects on subsequent employment and explore whether the loss of eligibility for ER benefits induces benefit substitution toward other public transfer programs.

## 5.1 Direct effects on early retirement benefits

Figure 3 illustrates two measures of the magnitude of the direct treatment effect: ER benefit claiming (panel 3a), which is a binary variable equal to 1 if individuals have claimed ER benefits at some point between ages 62 and 67, and ER benefits (panel 3b), which is the total take-up of benefits between ages 62 and 67 (in \$1,000). The left-hand side observations consist of individuals whose employer becomes bankrupt before reaching the individual eligibility cut-off. However, they might recover their ER option by extending their working career or by leaving the firm early and finding a new job. Those on the right-hand side are certain to fulfill the eligibility criteria if they are still employed by the firm when the bankruptcy occurs. The closer to the cut-off, the shorter the time period for which individuals may make their initial ER benefit claim. Just above the cut-off, individuals who are still employed by the firm just meet the eligibility criteria at the date of bankruptcy.

**Figure 3:** Graphical evidence of ER benefit take-up between 62–67 years of age

**(a)** ER benefit claiming **(b)** ER benefit take-up ($1,000)




*Notes:* The figures show the fraction of individuals with some ER benefit take-up (a) and ER benefit take-up in $1,000 (b) between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. Benefits are measured in 2015 dollars (NOK/USD = 9).

The figures show a visually clear discontinuity at the threshold for the two measures of the magnitude of the direct treatment. Using our reduced form RD strategy, we estimate a highly statistically significant increase in ER benefit claiming of about 42 percentage points. We estimate that these individuals claim about $61,600 more total ER benefits. The estimates suggest that our treatment had a significant impact on the displaced workers' ability to retire early with an ER benefit.

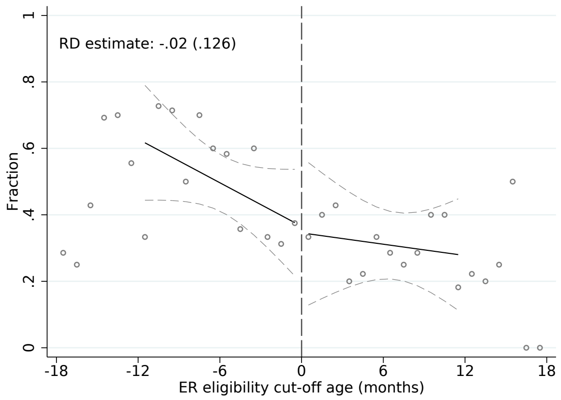
## 5.2 Effect on subsequent employment

We now investigate whether the initial ER eligibility status had an impact on re-employment rates and labor market earnings. Theoretically, those who lose eligibility should be induced to extend their working careers to redeem some of the lost benefits at the expense of foregone leisure, which becomes costlier. At the same time, individuals may have a hard time finding a new job as they are relatively close to the normal retirement age of 67 years. Local labor demand could also be an important factor.

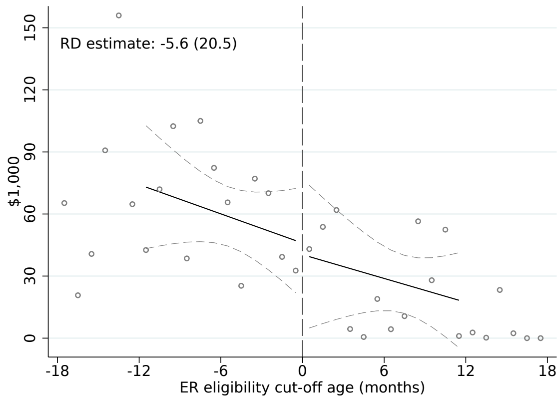
Visually, Figure 4a shows that we are unable to detect a discontinuity around the cut-off for being employed at some point between ages 62–67. Our point estimate suggests that individuals who just reached the age-threshold are about 2 percentage points less likely to reengage in the labor market compared to those just below the cut-off. This effect is, however, somewhat imprecisely estimated due to our small sample size. At the 95 percent confidence level, we can rule out a negative effect on re-employment of about 27 percentage points or more. Similarly, Figure 4b shows that we cannot distinguish between labor market earnings of individuals on either side of the cut-off, with a negligible point estimate of $5,600 corresponding to about $1,100 in annual earnings. We observe a downward slope in both figures, consistent with the fact that those who are further to the right are more likely to be eligible for ER benefits and are older at the time of the bankruptcy and thus closer to the normal retirement age.

**Figure 4:** Graphical evidence of employment and labor market earnings between 62–67 years of age

**(a)** Ever employed




**(b)** Labor market earnings ($1,000)




*Notes:* The figures show the fraction of individuals ever engaging in employment (a) and the unrestricted means for each age-bin of labor market earnings in $1,000 (b) between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level and are robust to heteroskedasticity.

We report regression results for the two outcomes in Table 2. The first two columns report results using our reduced form model where the second specification includes the pre-determined covariates in Appendix Table A.1 as control variables and year fixed-effects. This barely moves our estimates which is reassuring as the pre-determined covariates should have the same distribution on either side of the cut-off. As some individuals to the left of the cut-off may regain eligibility at a later stage, and some individuals to the right of the cut-off may lose ER eligibility due to e.g. leaving the firm before the actual bankruptcy, the reduced form estimates yields intention to treat (ITT) effects. These estimates can therefore be interpreted as lower bound estimates of access to ER benefits. The third and fourth column report results of our fuzzy RD model using the assignment variable as an instrument for ER eligibility. Due to some individuals may being partially treated and potential measurement errors in our eligibility measure which will bias estimates upwards, these estimates can be interpreted as upper bound estimates of the effect of ER eligibility. Our first stage estimate is 0.69 (0.67 including control variables) and is highly significant with a standard error of 0.09. This means that just being above the cut-off increases the likelihood of being eligible for ER benefits by 69 (67) percentage points compared to individuals just below the cut-off where about 20 percent of individuals are eligible. Thus, the IV estimates scales our reduced form estimates by a factor of 1.4–1.5. Additionally, we report means and standard deviations of the initially ineligible workers (i.e. the workers to the left of the cut-off) and of our comparison sample of private sector workers who experience a job-loss due to plant downsizing (to the left of the cut-off) in columns 5 and 6, respectively.

While re-employment rates are slightly higher for the downsizing sample, labor market earnings between ages 62–67 are almost identical. Although effects are imprecisely estimated, our results indicate that many workers who lose eligibility for ER benefits because of job displacement are either unwilling to, or possibly unable to redeem parts of the lost benefits through re-engaging in the labor market. While this may be surprising from a theoretical point of view, a possible explanation could be that workers could offset some of the lost benefits by other social security benefits such as DI benefits before they reach the normal retirement age. We investigate this hypothesis in the next section.

**Table 2:** Effect of ER eligibility on employment and labor market earnings between 62–67 years of age

| | Reduced form (ITT): | | IV estimate (2SLS): | | Mean [SD] Initially ineligible | Mean [SD] Downsizing sample |
|---|---|---|---|---|---|---|
| Ever employed | -.020 | -.018 | -.029 | -.027 | .492 | .576 |
| | (.126) | (.136) | (.183) | (.205) | | |
| Labor market earnings ($1,000) | -5.6 | -4.1 | -8.1 | -6.2 | 59.5 | 61.7 |
| | (20.5) | (20.3) | (29.7) | (30.6) | [89.0] | [92.8] |
| Controls | NO | YES | NO | YES | | |
| Number of firms | 127 | 127 | 127 | 127 | 82 | 214 |
| Number of individuals | 223 | 223 | 223 | 223 | 120 | 283 |

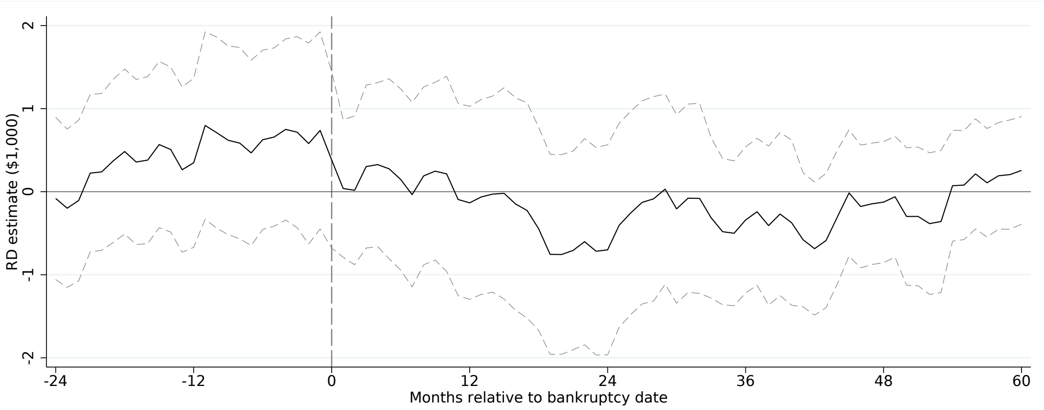
*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
*Notes:* The table shows results of local linear reduced form RD regressions and the corresponding 2SLS estimates using ER eligibility as the treatment variable. Both specifications use local linear regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome. Controls in the alternative specification include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Initially ineligible are defined as the estimation sample to the left of the cut-off. The downsizing sample only includes individuals to the left of the eligibility cut-off. Earnings are measured in 2015 dollars (NOK/USD = 9).

One might argue that the employment effect can be affected by timing of when the bankruptcy occurs, perhaps due to anticipation in the pre-period and increasing job-searching effort in the post-period. Therefore, we explore whether the RD effect is stable over time relative to the bankruptcy date. This also serves partly as a robustness check of our main result. We compute separate reduced form estimates for each month $m$ in the time span $m \in (-24, 60)$ for labor market earnings. The results are presented in Figure 5.

We observe that the ITT estimate on labor market earnings is very close to zero in our initial time period 24 months before bankruptcy, and then increases somewhat during the months leading up to bankruptcy. While the effect is not significant for either of these months, we observe a sizable drop in the month after the bankruptcy for which the effect remains roughly stable around zero. This might suggest that we are unable to detect an effect on labor supply because of selective leaving before bankruptcy. We investigate whether selective departure has been taken place in Appendix Figure A.5. While there are more "early leavers" among the initially ineligible, there does not appear to be a sharp discontinuity at the cut-off, suggesting that individuals were unable to predict the bankruptcy date and leave early. As an additional robustness check, we repeat the exercise for a sample of workers who were employed by the bankruptcy firm 12 months before and 1 month before bankruptcy, shown in Appendix Figure A.4. As the figures show, we are still unable to find a significant labor market earnings effect, with point estimates very stable around zero. This suggests that the additional "early leavers" in our initial estimation sample do not affect our conclusions.

**Figure 5:** Labor supply effects over time

**(a)** Earnings ($1,000)




*Notes:* The figures show separate ITT estimates of labor market earnings (in $1,000) for each month relative to bankruptcy date. The ITT effects are estimated by local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. Point estimates are represented by the black solid line, and the dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. Earnings are measured in 2015 dollars (NOK/USD = 9).

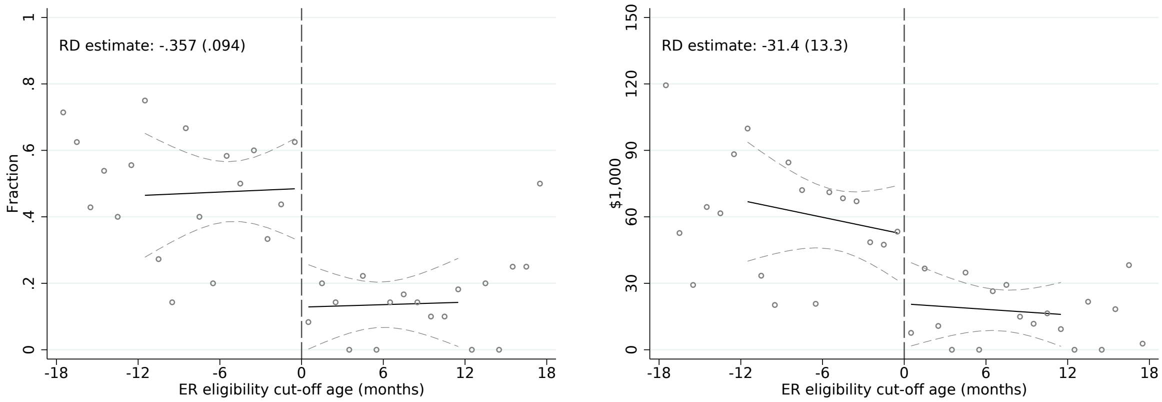
### 5.3 Benefit substitution

As reported in the previous section, we were unable to detect any effects ER eligibility on re-employment. A possible explanation for this could be that workers were able to offset some of the lost benefits through take-up of other social security benefits depending on eligibility. In particular, Bratsberg *et al.* (2013) show that a large share of DI claims in Norway can be attributed to job displacements. They argue that discretionary judgment by the social security administration is an important explanatory factor for the high rollover to DI among displaced workers, as work capacity is assessed relative to realistic employment opportunities. We therefore start our analysis by investigating benefit substitution toward DI benefits. As explained in Section 2, the DI benefit in our sample period was essentially equivalent to ER benefits, meaning that workers should be more or less financially indifferent between the two benefit programs all else equal.

**Disability insurance (DI)** Figure 6 shows the fraction of individuals who claim DI benefits at some point between ages 62–67 (panel 6a) and the cumulative DI benefit take-up between ages 62–67 (panel 6b) around the cut-off. From panel 6a, we observe a clear discontinuity in the likelihood of claiming DI benefits depending on initial ER eligibility. Our reduced form RD-estimate indicates that DI claiming is about 36 percentage points lower among individuals who worked in firms where the bankruptcy occurred just after they reached the individual age threshold. As about half of those who were just initially ineligible for ER claim DI benefits, the effect of reaching the threshold translates to a reduction in DI claiming by about 75 percent. Panel 6b shows the corresponding effect on cumulative DI benefit take-up (in $1,000). Workers just to the right of the eligibility cut-off claim about $31,400 less DI benefits between ages 62–67, or about half of the DI benefits that individuals just to the left of the cut-off.

**Figure 6:** Graphical evidence of benefit substitution towards DI between 62–67 years of age

**(a)** Ever on DI **(b)** DI benefits ($1,000)




*Notes:* The figures show the fraction of individuals ever on DI (a) and the unrestricted means for each age-bin of cumulative DI take-up in $1,000 (b) between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. Benefits are measured in 2015 dollars (NOK/USD = 9).

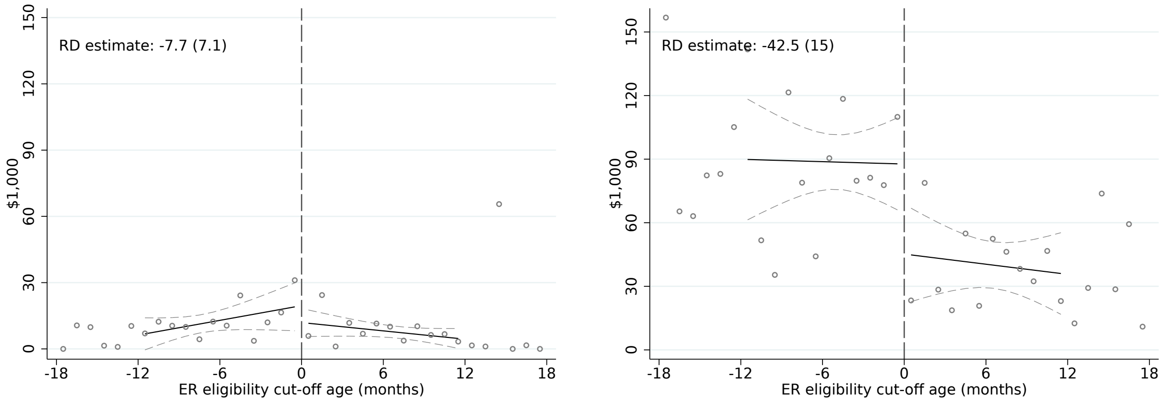
The reduced form estimate for ER benefits in panel 3b suggested that those who were just too young to reach their individual eligibility age before the bankruptcy date claim about $61,600 less ER benefits than those who just reached their eligibility age. Our results thus indicate that about half of the lost benefits are replaced by DI benefits. The estimates are highly significant, and we interpret this as clear evidence of program substitution toward DI benefits.

**Unemployment insurance (UI) and other public transfers** We now investigate whether individuals who were initially ineligible offset some of the lost ER benefits through take-up of unemployment insurance. Additionally, we pool all public transfers (excluding ER benefits and old-age pensions) in order to estimate benefit substitution toward all relevant parts of the social security system. Figure 7 shows the cumulative take-up of UI benefits (panel 7a) and total public transfers (panel 7b) between ages 62–67 years (in $1,000). Although we estimate that individuals who were just below the cut-off claimed less UI benefits, this effect is not significant at conventional levels. However, workers in our sample are only eligible for UI benefits for up to 2 years. As most individuals close to the cut-off are just a few months shy of turning 61 years when bankruptcy occurs, most individuals would have exhausted their UI spell before turning 62 years.[22] Panel 7b shows that initially ineligible individuals claimed significantly more non-pension public transfers. Our reduced form point estimate indicates that they claim about $42,500 more between ages 62–67, where we estimated that $31,400 is DI benefits and $7,700 is UI benefits. This suggests that a negligible $3,400 is replaced by other social security benefits.

[22] We consider program complementarity between ER and UI highly unlikely between ages 62–67 years as eligible individuals can claim ER benefits from age 62.

**Figure 7:** Graphical evidence of unemployment insurance and social insurance benefit take-up ($1,000) between 62–67 years of age

**(a)** UI benefits **(b)** Non-Pension public transfers




*Notes:* The figures show unrestricted means for each age-bin of cumulative UI take-up and total non-pension social insurance benefit take-up in $1,000 between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. Benefits are measured in 2015 dollars (NOK/USD = 9).

**Table 3:** Effect of ER eligibility on cumulative social insurance benefit take-up ($1,000) between 62–67 years of age

| | | | | | Mean [SD] | |
|---|---|---|---|---|---|---|
| *Outcome:* | Reduced form (ITT): | | IV estimate (2SLS): | | Initially ineligible | Downsizing sample |
| ER benefits | 61.6*** | 57.6*** | 89.2*** | 86.6*** | 11.4 | 22.4 |
| | (14.2) | (15.7) | (20.0) | (22.5) | [35.0] | [49.9] |
| *Program substitution:* | | | | | | |
| Non-pension public transfers | -42.5*** | -36.2** | -61.5*** | -54.5** | 88.8 | 93.6 |
| | (15.0) | (16.1) | (21.8) | (23.4) | [74.5] | [94.0] |
| DI benefits | -31.4** | -24.6* | -45.5** | -37.0* | 59.5 | 42.2 |
| | (13.3) | (14.5) | (20.1) | (22.1) | [72.1] | [63.7] |
| Unemployment benefits | -7.7 | -9.5 | -11.1 | -14.2 | 13.2 | 11.8 |
| | (7.1) | (7.2) | (9.9) | (10.2) | [24.9] | [29.1] |
| Controls | NO | YES | NO | YES | | |
| Number of firms | 127 | 127 | 127 | 127 | 82 | 214 |
| Number of individuals | 223 | 223 | 223 | 223 | 120 | 283 |

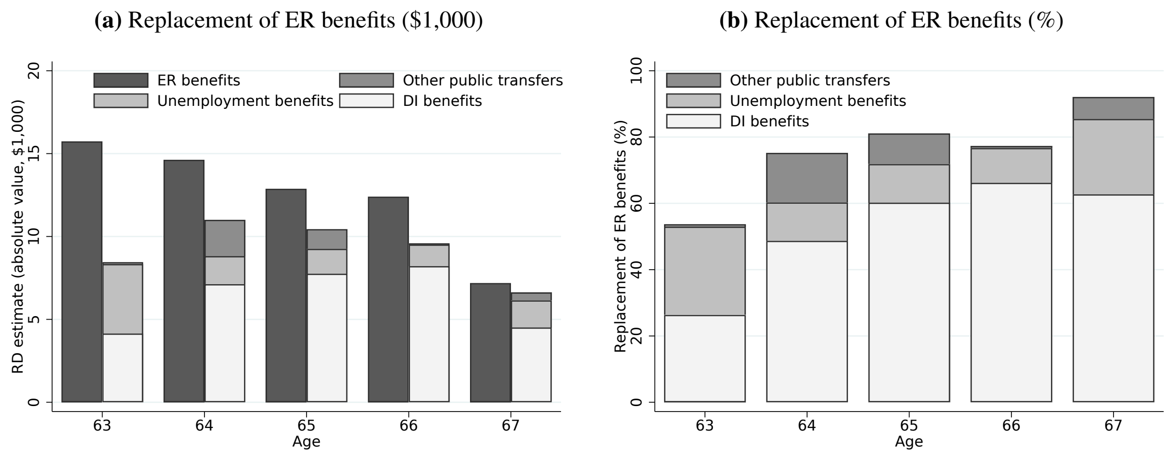
*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
*Notes:* The table shows results of local linear reduced form RD regressions and the corresponding 2SLS estimates using ER eligibility as the treatment variable. Both specifications use local linear regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000). Controls in the alternative specification include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Initially ineligible are defined as the estimation sample to the left of the cut-off. The downsizing sample only includes individuals to the left of the eligibility cut-off. Benefits are measured in 2015 dollars (NOK/USD = 9).

Table 3 reports reduced form and IV estimates of ER benefits and program substitution toward social insurance benefits. While total program substitution effects are slightly lower if we include control variables, estimates are qualitatively similar. Our estimates indicate that individuals who were just initially eligible for ER claim about half of non-pension social security benefits compared to those who were initially ineligible. While ER benefit take-up is $61,600 higher among workers who retain eligibility, about $42,500 are replaced with other social security benefits among those who are initially ineligible, equivalent to a replacement rate of about 69 percent. Of those, about 51 percentage points are DI benefits and 13 percentage points are UI benefits. We interpret this as substantial benefit substitution as those who are initially ineligible due to the job displacement substantially increase take-up of other social transfers, and primarily disability insurance.

To investigate whether our findings have external validity to the wider pool of displaced workers, we investigate relevant outcomes for individuals who were displaced due to a plant downsizing. Table 3 reports means and standard deviations for the initially ineligible (i.e. those to the left of the cut-off) for this sample as well as our main estimation sample. Compared to the downsizing sample, our estimation sample is more likely to receive DI benefits and less likely to retain ER benefits. This may suggest that the scope for program substitution towards DI benefits is somewhat smaller for the full sample of displaced workers. However, the downsizing sample receives about the same amount in total non-pension public transfers which may induce more substitution towards other social security benefits.

**Effects for each age** To further investigate how lost ER benefits are redeemed in terms of take-up of other public transfers, we run separate reduced-form regressions for each age. The point estimates are reported in Appendix Table A.2, while Figure 8 illustrates the effects graphically. In panel 8a, the darkest bar is the ITT estimate on ER benefit take-up for each age, i.e., just reaching the individual threshold implies increased take-up of ER benefits by just over \$15,000 at age 63. The three lighter stacked bars illustrate the effects on DI benefits, UI benefits, and other public transfers for each age. We observe that the effect on ER benefits is decreasing with age, while the effect on take-up of other social security benefits is fairly stable across the age groups. DI benefits are by far the largest substitute, and the degree of substitution is increasing with age. This is further illustrated in panel 8b, showing the effects on take-up of each benefit relative to the effect on take-up of ER benefits. We observe that the increased replacement rate is mainly driven by increased replacement through the take-up of DI benefits.

**Figure 8:** Graphical illustration of program substitution




*Notes:* Panel (a) illustrates the ITT effects for each outcome and each age (in \$1,000). The effect on ER benefits is illustrated in absolute value, while the effects on public transfers are illustrated cumulatively. Panel (b) illustrates the cumulative ITT effects on public transfers, but relative to of the ITT effect of ER benefit take-up. The effects are estimated by local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. Benefits are measured in 2015 dollars (NOK/USD = 9).

## 5.4 Robustness analysis

To verify the validity of our main results, we conduct a series of robustness checks. In Appendix Table A.3 we present nine alternative specifications for our reduced form model in addition to our main specification which uses a rectangular kernel and 12 months of bandwidth on each side of the cut-off. We observe that in all our robustness checks, estimates remain fairly close to our baseline specification. Take-up of ER benefits are positive and significant for all specifications, with the point estimates being quite stable across specifications. For non-pension public transfers and DI benefit take-up, we observe that the point estimates are negative for all our specifications and are close in magnitude. For non-pension public transfers, all specifications yield significant results at the 10% level.

The first specification in Table A.3 is our baseline reduced form estimates of outcomes between ages 62–67. The second row adds control variables which include the pre-determined variables we use for balancing (see Table A.1 in the appendix) and year fixed-effects, which we observe have little impact on our estimates. Next, following Gelman & Imbens (2019) we use quadratic trends on each side of the cut-off instead of linear trends. We observe that estimates are less precisely estimated and a magnitude larger. In specifications (iv) and (v) we check whether a local linear specification is appropriate when we deviate from the baseline choice of bandwidth. Particularly, we report estimates reducing the bandwidth by 50 percent (from 12 to 6 months) and increasing the bandwidth by 50 percent (18 months). We observe that the point estimates are very similar to what obtained in the baseline specification. In Figure A.6 in the appendix, we extend this exercise by plotting the RD estimates with confidence intervals for each outcome. Combining the evidence from specifications (iv) and (v) with the graphical evidence in Appendix Figure A.6, we conclude that the estimates are very robust to the choice of bandwidth when using linear trends suggesting linearity is a reasonable approximation to the trends around the cut-off. In specification (vi) we use a triangular kernel (rather than rectangular) which has negligible impact on our estimates. For our main estimation sample, the minimum firm size is 2 employees. In such small firms, delaying bankruptcy to meet the eligibility criteria might be a possibility. As a robustness check, we therefore only include firms with at least 10 employees in specification (vii). The effects are still highly persistent. Specifications (viii) and (ix) change the pre-determination of employment status in bankruptcy firms from 24 months before bankruptcy to 12 months before and 1 month before, respectively. Reassuringly, the point estimates are quite similar to our main specification although estimates in the latter specification are less precise due to a smaller sample size. Finally, specification (x) includes bankruptcies where at least 1/3 of (all) employees switched to the same firm which we deemed as "spurious" bankruptcies. As expected, the estimated effects are smaller in magnitude when we include these firms as a larger share of workers did not experience a job displacement as firms were likely taken over by new owners.

We also perform a placebo test by using private sector bankruptcy firms *without* ER coverage in an otherwise similar setup to our baseline sample. As the "cut-off" for these workers does not involve the loss (or gain) of ER eligibility, our main outcomes should have the same distribution just before and just after the hypothetical cut-off. Firms without ER coverage differ in some dimensions from those with ER coverage, and in particular, they are far less likely to be manufacturing firms. We therefore provide industry-weighted estimates to better resemble firms with ER coverage.[23]

The estimated effects are reported in Appendix Table A.4. For the baseline estimate using firms without ER coverage, we are unable to reject the null of no difference between workers on each side of the cut-off for any of our main outcomes. Results are also shown graphically in Appendix Figure A.7. There is, as expected, a close-to-zero effect on ER benefit take-up, as the only way for these individuals to become eligible for ER benefits is to switch workplace to a firm with ER coverage and acquire at least three years of tenure. While the point estimate for labor market earnings is positive, and the point estimates for public transfers and DI benefits are negative, the estimates are roughly within one standard error. Using weights to account for differences in industries barely move the estimates. If anything, the industry weighted placebo estimates are closer to zero.

## 5.5 Heterogeneity

As workers in our estimation sample differ slightly from the average displaced worker in terms of observable characteristics, we further investigate the driving forces behind the main responses. Particularly, Table 1 revealed that workers in our sample are typically male workers in the manufacturing sector. Workers' wages and education may also be important; workers with high wages are likely eligible for

[23] Specifically, firms without ER coverage are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of having ER coverage and $P(I_i=1|x_i)$ is estimated with a logit model using industry fixed effects as control variables.

a higher ER benefit as the benefit is linked to past earnings, which may result in loss of eligibility for ER being a larger shock to individuals with higher wages. However, workers with high wages may also have better outside options in the labor market, and may have lower search costs when unemployed.[24]

In Table 4 we report reduced form estimates of our main outcomes for different subsets of workers corresponding to differences in gender, pre-bankruptcy earnings, educational attainment and industry. The estimated coefficients for men indicate that they exhibit similar properties as the full estimation sample. For women, the point estimates are smaller, but also more imprecise mainly due to the small sample size. While we lack precision to provide definitive answers, the estimates indicate that men are more likely to respond to the incentive to claim ER benefits and reduce take-up of other social benefits, while women to a larger extent claim other social security benefits regardless of having the option to retire early.[25]

To explore heterogeneous effects in wages, we split our sample on earnings (24 months) prior to bankruptcy. As expected, compared to high earnings workers, the effect on ER benefit take-up is smaller for workers with low earnings (smaller than or equal to the median). This difference is likely somewhat mechanical as low earnings workers have lower accrual of ER benefits on average. However, we observe that low earnings workers replace almost the entire loss of ER benefits with other social security benefits, while high earnings workers replace a significantly lower share. In fact, the estimated coefficients for high earnings workers on our social security outcomes are not significantly different from zero at conventional significance levels.

When we split our sample on educational attainment (high education is defined as completed high school or higher and low otherwise), we find a quite similar pattern as when we split our sample on earnings prior to bankruptcy, although with one notable exception; the point estimate on ER benefits is large and highly significant for workers with low education, but rather low and insignificant for workers with high education. The point estimates on our social security outcomes are significantly larger for low-education workers and give clear evidence of responses being driven by workers with low education.

[24] Similarly, education may be correlated with better outside options, as education is highly correlated with earnings.

[25] When exploring gender differences, we would ideally also want to explore spousal spillover effects. We estimated the effect on spousal outcomes and were unable to detect significant effects on employment or take-up of any social security benefits for the spouse. We emphasize that this should be interpreted with caution due to our small sample size, although most point estimates are close to zero.

**Table 4:** Subsample analysis of labor market outcomes and social insurance benefit take-up ($1,000) between 62–67 years of age

| | | | | Program substitution: | | | |
|---|---|---|---|---|---|---|---|
| | Ever employed | Labor market earnings | ER benefits | Non-pension public transfers | DI benefits | Unemployment benefits | Obs <Firms> |
| *Column:* | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Full sample | -.020 | -5.6 | 61.6*** | -42.5*** | -31.4** | -7.7 | 223 |
| | (.126) | (20.5) | (14.2) | (15.0) | (13.3) | (7.1) | <127> |
| | [.492] | [59.6] | [11.4] | [88.8] | [59.5] | [13.2] | |
| Males | -.015 | -8.2 | 70.7*** | -44.9** | -39.1** | -3.1 | 173 |
| | (.143) | (24.8) | (14.8) | (17.4) | (16.2) | (5.7) | <101> |
| | [.522] | [66.6] | [11.2] | [89.9] | [62.7] | [11.2] | |
| Females | -.135 | -12.5 | 29.8 | -30.0 | -7.9 | -17.4 | 50 |
| | (.230) | (29.3) | (37.7) | (32.8) | (25.3) | (20.5) | <42> |
| | [.393] | [36.4] | [12.1] | [85.1] | [49.2] | [19.5] | |
| High earnings | -.009 | -3.5 | 75.0*** | -32.7 | -28.9 | -6.5 | 108 |
| | (.185) | (34.0) | (23.9) | (24.8) | (21.2) | (10.0) | <65> |
| | [.534] | [74.2] | [13.8] | [91.4] | [57.1] | [14.4] | |
| Low earnings | -.028 | -4.9 | 51.1*** | -50.4** | -32.7* | -9.2 | 115 |
| | (.161) | (22.6) | (18.3) | (19.9) | (18.3) | (9.2) | <87> |
| | [.452] | [45.8] | [9.2] | [86.3] | [61.8] | [12.1] | |
| High education | -.037 | -7.5 | 21.9 | -5.2 | -5.1 | -2.1 | 75 |
| | (.188) | (41.4) | (27.7) | (34.8) | (27.8) | (6.1) | <50> |
| | [.558] | [89.7] | [18.7] | [68.9] | [42.6] | [7.9] | |
| Low education | .000 | .8 | 81.7*** | -63.2*** | -46.3*** | -11.5 | 148 |
| | (.163) | (21.1) | (16.4) | (17.3) | (16.1) | (9.3) | <99> |
| | [.455] | [42.7] | [7.4] | [99.9] | [69.0] | [16.1] | |
| Manufacturing | .091 | 18.7 | 47.8*** | -46.9** | -49.4*** | 4.1 | 149 |
| | (.162) | (22.5) | (16.6) | (19.3) | (17.1) | (6.9) | <72> |
| | [.468] | [58.5] | [14.3] | [93.7] | [63.9] | [11.9] | |
| Other industries | -.235 | -53.1 | 96.6*** | -42.8* | -6.6 | -29.7** | 74 |
| | (.197) | (37.9) | (25.7) | (24.7) | (19.0) | (12.0) | <55> |
| | [.535] | [61.4] | [6.4] | [80.0] | [51.6] | [15.5] | |

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity. Independent means of initially ineligible (the sample to the left of cut-off) in brackets.
*Notes:* The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome and each subgroup. High earnings are defined as larger than median 24 months before bankruptcy date, and low earnings otherwise. High education is defined as completed high school or more, and low education otherwise. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

In our estimation sample, around 67 percent of workers are employed in the manufacturing industry which is somewhat higher than for workers who are displaced following a plant downsizing (about 55 percent). This motivates us to study manufacturing separately. We observe that point estimates on ER benefits are smaller for workers in the manufacturing industry. However, this is not because of differences in wages; in fact, workers in the manufacturing have comparable earnings to workers in other industries prior to bankruptcy. While the point estimates of total public transfers are similar between the two subgroups, manufacturing workers replace a much larger share of the lost ER benefits with other social security benefits compared to other workers. In fact, the point estimates suggest that manufacturing workers replace the entire lost ER benefits with DI benefits. This may suggest that workers in more physically demanding jobs are more inclined to be eligible and possibly apply for DI benefits. There is no evidence for substitution toward DI benefits for workers in other industries. Rather, there is clear evidence of workers in other industries replacing some of the lost ER benefits with unemployment benefits, with an estimated coefficient significant at the 5% significance level. Interestingly, the point estimate of labor market earnings is negative and relatively large for workers in non-manufacturing industries compared to manufacturing workers. While not significant at conventional levels, it may seem that the lack of a labor supply response for our main estimation sample could be driven by manufacturing workers. A possible explanation for this could be because of low local labor demand, and in particular for workers with specific occupational skills, as a relatively large share of the manufacturing firms in our sample were relatively large firms located in small towns.

# 6 Fiscal costs and welfare implications

In this section, we assess policy implications of our findings and welfare for the displaced workers in our sample. While access to ER benefits provides better insurance for elderly workers in the event of a job displacement, it could also increase public expenditures through increased benefit payments and decreased tax revenues. However, as we have shown, decreased benefit payments of other social security programs could offset some of the increased costs. These trade-offs are particularly important in assessing the desirability of access to ER for elderly displaced workers.

## 6.1 Empirical analysis

To assess how access to ER benefits affects public finances, we estimate effects using net public expenditures as the outcome variable, defined as gross benefit payments from all social security benefits subtracted all taxes.[26] As a rough measure of individual welfare, we consider disposable income as an outcome variable, defined as income from labor, business, capital and social security benefits net of taxes. Finally, we proxy individual consumption by generating a measure of individual savings. We define our consumption proxy as disposable income subtracted net savings, defined as the change in individual bank accounts subtracted debt.[27]

In Table 5, we report ITT estimates from our reduced form model as well as IV estimates from our fuzzy RD model. Although somewhat imprecise, our estimates indicate that access to the ER program likely had a small impact on public finances. This is not surprising given our previous findings, where we did not find evidence of an effect on labor supply, but relatively large substitution effects onto other social security programs. Our point estimate using the IV model suggests that the increase in net public expenditures is \$37,100 for compliers in our sample during ages 62–67, or about \$7,400 per annum. Recall that the IV estimate should be interpreted as an upper bound of access to ER, as some individuals might be partially treated and potential measurement errors in our treatment variable will bias our estimates away from zero. This suggests that the increase in net public expenditures is at most \$97,000 between ages 62–67 using a confidence level of 95%.

[26] As reported in individual tax registers reported to the authorities. This includes taxes on labor, business and capital income, taxes on social security benefits and wealth taxes.

[27] This approach follows Fagereng & Halvorsen (2015). Unfortunately, we do not have detailed data on housing, stocks, gifts and inheritances. Therefore, it is possible that consumption is measured with some errors.

**Table 5:** Financial costs and benefits ($1,000)

| | | | | | Mean [SD] | |
|---|---|---|---|---|---|---|
| | | | | | Initially | Downsizing |
| *Outcome:* | Reduced form (ITT): | | IV estimate (2SLS): | | ineligible | sample |
| Net public expenditures | 25.6 | 18.9 | 37.1 | 28.4 | 72.2 | 79.9 |
| | (21.0) | (21.2) | (30.6) | (31.9) | [83.1] | [96.0] |
| Imputed consumption | 7.6 | 24.0 | 11.0 | 36.1 | 181.3 | 192.8 |
| | (18.4) | (17.9) | (26.6) | (27.0) | 86.6 | [93.1] |
| Disposable income | 16.6 | 26.4 | 24.0 | 39.7 | 177.9 | 194.6 |
| | (16.9) | (16.1) | (24.5) | (24.6) | 78.0 | [87.7] |
| Controls | NO | YES | NO | YES | | |
| Number of firms | 127 | 127 | 127 | 127 | 82 | 214 |
| Number of individuals | 223 | 223 | 223 | 223 | 120 | 283 |

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
*Notes:* The table shows results of local linear reduced form RD regressions and the corresponding 2SLS estimates using ER eligibility as the treatment variable. Both specifications use local linear regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000). Controls in the alternative specifications include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Net public expenditures are defined as gross benefit payments from all social security programs subtracting all individual taxes. Disposable income is defined as gross individual income (earnings, benefits and capital income) subtracting all individual taxes. Net savings are defined as the change in individual bank accounts subtracted debt. Consumption is defined as disposable income subtracted net savings. Initially ineligible are defined as the estimation sample to the left of the cut-off. The downsizing sample only includes individuals to the left of the eligibility cut-off. Variables are measured in 2015 dollars (NOK/USD = 9).

Our estimates also indicate that access to the ER program likely had little impact on average welfare for individuals, with point estimates on imputed consumption and disposable income being small and insignificant. However, due to lack of significance, we cannot conclude that access to ER increased average consumption or disposable income for individuals, but we can rule out a large decrease in average welfare for ineligible individuals. At the 95% confidence level, our IV estimate suggests that compliers in our sample would at the most forgo $63,000 in consumption if they did not have access to ER benefits, or about $12,600 annually. Similarly, we estimate that individuals would forgo at most $14,400 in disposable income annually. Taken together, this suggests that the provision of ER benefits had little impact on individual welfare and net public expenditures.

### 6.2 Is early retirement provision for elderly displaced workers welfare improving?

To address whether provision of ER benefits for elderly displaced workers is an optimal policy, we build on the sufficient statistics framework proposed by Baily (1979) and Chetty (2006), later used in e.g. Inderbitzin *et al.* (2016); Haller (2022); Kolsrud *et al.* (2023). The method evaluates the insurance value of, in our case, optional ER benefits against the "moral hazard" costs which in our case is the labor supply distortion. In our context, optimal benefits fulfill the following equation:

$$\frac{u'(C_{\bar{ER}}) - u'(C_{ER})}{u'(C_{ER})} = \frac{\varepsilon_{ER,b}}{1 - ER}, \tag{6}$$

where the left-hand side captures the marginal benefit of consumption smoothing by providing optional ER benefits ($\bar{ER}$ here refers to not having that option). The right-hand side captures the "moral hazard" cost. More precisely, we have that:

$$\varepsilon_{ER,b} = \frac{\Delta ER}{ER} \bigg/ \frac{\Delta b}{b}, \tag{7}$$

where $b$ reflects total social security benefits one would receive in the absence of an ER program. $\Delta b$ is the additional benefits one would receive if eligible for ER benefits. This means that spillovers onto other social security programs are implicitly taken into account.

The right-hand side of (6) is then straightforward to compute using (7), and we obtain that $\varepsilon_{ER,b} = \frac{0.02}{0.508} / \frac{28}{101.9} = 0.142$.[28] To compute the left-hand side of (6), we assume that utility follows the CRRA-class:

$$\frac{u'(C_{\bar{ER}}) - u'(C_{ER})}{u'(C_{ER})} = \left(\frac{C_{\bar{ER}}}{C_{ER}}\right)^{-\gamma} - 1, \tag{8}$$

where $\gamma$ is the coefficient of relative risk aversion. Taking logs on both sides, we are able to directly back out $\gamma$, i.e. the required risk aversion coefficient for provision of ER benefits to be welfare improving:

$$\gamma = -\frac{\ln\left(1 + \frac{\varepsilon_{ER,b}}{1-ER}\right)}{\ln\left(\frac{C_{\bar{ER}}}{C_{ER}}\right)} \tag{9}$$

As an estimate of the insurance value of ER benefits, we our individual consumption proxy outlined in Section 6.1 between ages 62–67 of eligible individuals as an estimate of $C_{ER}$ and those ineligible for $C_{\bar{ER}}$. Doing this we get:

$$\gamma = -\frac{\ln\left(1 + \frac{0.142}{1-0.508}\right)}{\ln\left(\frac{173.7}{181.3}\right)} \approx 5.9 \tag{10}$$

If we instead use disposable income as the consumption proxy, the required risk aversion is $\gamma = 2.6$.[29] This implies that individuals to a large degree are able to self-insure through savings.

While the coefficient of relative risk aversion is quite common in the economic literature, it's value is often context-specific and varies greatly. In Chetty (2006), later reported in Inderbitzin *et al.* (2016), the mean implied risk aversion coefficient is 0.71 with a range from 0.15 to 1.78. Although usually taken as an external parameter, a commonly used value for $\gamma$ in the macroeconomic literature is 1.5 (see e.g. Low & Pistaferri, 2015; Low *et al.*, 2018). Our measure is well above this range, i.e. individuals would be required to have a risk aversion coefficient above 5.9 (or 2.6 if we use disposable income as our consumption proxy) for ER provision to be welfare improving for involuntary displaced workers. This means that ER provision in our context likely is a suboptimal policy.

A potential caveat to this exercise is that the social security benefits excluding ER typically come with specific eligibility criteria related to for instance a health condition (such as DI) or it may be associated with a perceived social stigma cost. While ER is given "no strings attached" to eligible individuals, other social security programs may be more rigorous and involve a more stringent application process with the risk of being rejected. In this case the required risk aversion for the ER program to be welfare improving would be lower than our estimate.

Another caveat is the assumption of a representative agent in the Baily-Chetty framework. In the next section, we show that there is considerable heterogeneity in consumption and disposable income across individuals. Due to decreasing marginal utility, the average utility gain from provisional ER might in practice be higher than the gain for the representative agent. This would imply a lower required level of risk aversion for the program to be welfare improving.

On the other hand, this exercise also assumes that there are no moral hazard costs of providing ER benefits. While we would argue that moral hazard costs are negligible in our context since bankruptcies identify displacements, and thus can broadly be interpreted as exogenous, the same may not be true in other real-life contexts. If offering an ER option incentivizes firms and/or individuals to initiate displacements on their own, program inflow would increase, and the required level of relative risk aversion would likely be higher than our estimate. Therefore, we emphasize that our estimate first and foremost applies to cases where displacements are involuntary. An ER option that includes a broader range of

[28] More specifically, the estimate for $\Delta ER$ is the reduced form estimate of "Ever employed" and the estimate for $ER$ is one minus the mean of the initially ineligible in the same row in Table 2. For the estimate of $\Delta b$ we run the reduced form model on total net social security benefits (including ER) and $b$ the mean for the initially ineligible. Note that we would get the same estimate if we instead used the IV model as the same scaling would apply to both the numerator and denominator and thus cancel out.

[29] With the values being $C_{ER} = 177.9$ and $C_{\bar{ER}} = 161.3$, respectively.

job displacements would lead to an even higher required relative risk aversion for the program to be welfare-improving.

Another feature that might be specific to our context is the very high benefit substitution, especially with regard to DI benefits. In other countries where social security is less generous, the insurance value of ER benefits will likely be higher. On the other hand, so might the labor supply distortions be. Therefore, it is not clear whether the required level of risk aversion would be higher or lower in other countries with less generous social security.

## 6.3 Distributional impacts

In this section we investigate distributional impacts of access to ER by studying the marginal distributions of consumption under different treatment statuses for compliers in a standard Imbens & Rubin (1997) framework. More specifically, we use eligibility age above cut-off ($\mathbb{Z}$) as an instrument for ER eligibility ($E$). The marginal distributions of potential outcomes for compliers $g_e$ where $e$ is treatment status are defined as:

$$g_0(y) = f_{00}(y) \cdot (p_c + p_c)/p_c - f_{10}(y) \cdot p_n/p_c \tag{11}$$

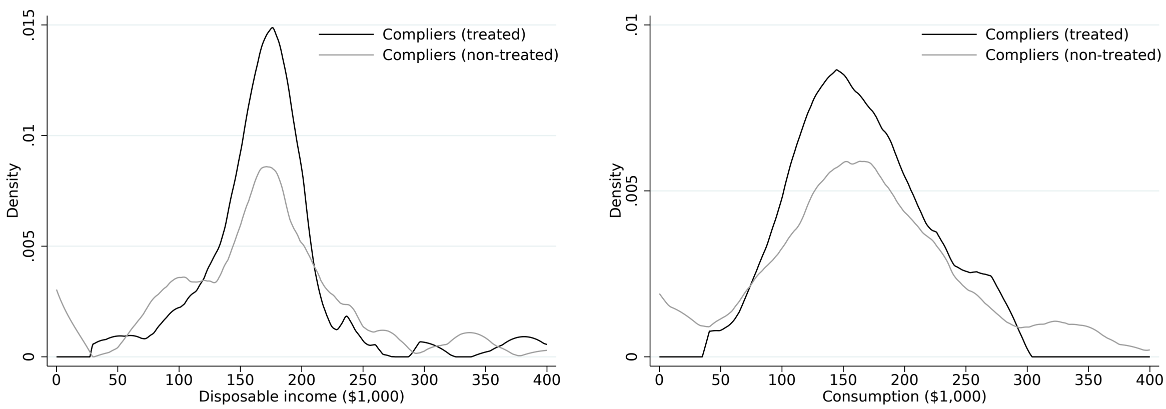
$$g_1(y) = f_{11}(y) \cdot (p_c + p_c)/p_c - f_{01}(y) \cdot p_a/p_c \tag{12}$$

where $f_{ze}$ is the distribution of consumption for individuals with $z$ being equal to 1 if eligibility age is above cut-off and 0 otherwise and treatment status $e = 0,1$. $p_a$ is the proportion of "always-takers", $p_n$ is the proportion of "never-takers" and $p_c$ is the proportion of compliers. We estimate $f$ using an epanechnikov kernel with data-driven bandwidth.

Figure 9 shows the estimated distributions of potential disposable income and imputed consumption for compliers in our sample, that is, the individuals who become eligible for ER because their age is above the eligibility cut-off, but would not have become eligible otherwise. We observe that the distribution for treated compliers is somewhat more concentrated with smaller dispersion as most of these individuals receive similar amounts of ER benefits, while non-treated receive different kinds of social security benefits and for different amounts of time. We observe that there is evidence of some ineligible compliers having very low disposable income and/or consumption. However, this amounts to quite few individuals. This suggests that a very low share of ineligible individuals are significantly worse off because of failing to qualify for ER, as most individuals have some income either through participation in the labor market or receiving some type of social security benefit.

**Figure 9:** Distributional impacts of ER eligibility

**(a)** Disposable income ($1,000) **(b)** Imputed consumption ($1,000)




*Notes:* The figure shows distributions of potential outcomes for compliers as defined by Imbens & Rubin (1997) (see text for details). Densities are estimated using an epanechnikov kernel with a data-driven bandwidth of 18.73. Disposable income is defined as all individual income (earnings, benefits and capital income) net-of-taxes. Consumption is defined as disposable income subtracted net savings (change in individual bank accounts subtracted debt) Both outcomes are summarized between ages 62-67 and measured in 2015 dollars (NOK/USD = 9).

# 7 Conclusion

In this paper, we have examined the impact of early retirement provision on labor supply and benefit substitution among elderly displaced workers. Using register data, the study leveraged the timing of firm bankruptcies as a source of exogenous variation in job loss and eligibility for early retirement (ER) benefits. Employing a regression discontinuity design, we are unable to detect whether ER benefit provision harming labor supply. We do, however, find that a significant portion of lost ER benefits are replaced by other social security benefits, notably disability insurance.

Additionally, we explored the social desirability of the early retirement program for elderly involuntary displaced workers. We found that, on average, the welfare gain of providing ER is likely low, but so are the associated public costs. We interpret this as a result of the high degree of benefit substitution, making ER provision financially redundant for many individuals in the presence of other social security benefits. Investigating this trade-off by employing the Baily-Chetty framework, we demonstrated that a substantial level of relative risk aversion would be required to support the provision of an early retirement option for displaced workers in Norway. Furthermore, we investigated distributional effects of ER provision and found that a relatively small share of individuals are significantly worse off because of failing to qualify for ER. Taken together, our findings indicate that the provision of ER benefits for elderly displaced workers is suboptimal in our context.

Generally, policymakers must carefully weigh the fiscal costs against the positive welfare effects when considering the adoption of milder eligibility criteria for ER or even eliminating them altogether. When analyzing this trade-off in other contexts, the individual ability to self-insure, alternative social security options, labor supply distortions and costs associated with the program at hand are factors that may affect the policy recommendation. In other countries where options for other social security might be more limited, the benefits of an ER option might be higher, but so might the associated costs be.

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# Appendices

## Additional Tables and Figures

**Table A.1:** Smoothness of predetermined covariates

| | **Main est. sample:** **Workers in ER-covered firms** | | | **Placebo sample:** **Workers in firms w/o coverage** | | |
|---|---|---|---|---|---|---|
| *Dependent variable:* | *coeff.* | *std. error* | *p-value* | *coeff.* | *std. error* | *p-value* |
| Female | -.093 | (.104) | .373 | .050 | (.089) | .576 |
| Married | .015 | (.113) | .893 | -.068 | (.089) | .445 |
| Years of education | -.236 | (.388) | .545 | .324 | (.477) | .497 |
| Tenure | -.85 | (2.22) | .703 | .58 | (1.48) | .695 |
| Number of employees | 16.8 | (28.8) | .561 | -1.5 | (2.7) | .573 |
| Monthly earnings ($1,000) | -.155 | (.498) | .757 | .387 | (.411) | .347 |
| Manufacturing | .073 | (.130) | .577 | -.177** | (.077) | .022 |
| Full time employment | .077 | (.065) | .239 | -.121** | (.052) | .019 |
| Local DI rate | .014** | (.007) | .030 | .005 | (.005) | .305 |
| Local unemp. rate | -.001 | (.002) | .647 | .000 | (.002) | .869 |
| Share senior workers | .026 | (.030) | .400 | .028 | (.040) | .488 |
| Wealth ($1,000) | -27.4 | (25.3) | .282 | .2 | (20.4) | .993 |
| Sickness benefits | -.043 | (.091) | .638 | -.002 | (.065) | .975 |
| Joint test | | | .263 | | | .182 |
| Number of individuals (firms) | 223 | (127) | | 417 | (372) | |

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
*Notes:* The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each pre-determined covariate. Each covariate is measured 24 months before bankruptcy date for each employee. Local DI rate and unemployment rates are measured at the municipality level. The share of senior workers is defined as the share of (all) coworkers above 57 years (excluding self). Earnings and wealth are measured in 2015 dollars (NOK/USD = 9).

**Table A.2:** Reduced form effects on labor market outcomes and social insurance benefit take-up ($1,000) by age

| | | | | Program substitution: | | | |
|---|---|---|---|---|---|---|---|
| | Ever employed | Labor market earnings | ER benefits | Non-pension public transfers | DI benefits | Unemployment benefits | Obs <Firms> |
| *Column:* | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Total effect | -.020 | -5.6 | 61.6*** | -42.5*** | -31.4** | -7.7 | 223 |
| 62-67 years | (.126) | (20.5) | (14.2) | (15.0) | (13.3) | (7.1) | <127> |
| *Effect by age:* | | | | | | | |
| 62 years | -.027 | -2.3 | 7.3*** | -1.3 | -3.5 | 1.8 | 216 |
| | (.138) | (8.0) | (2.0) | (3.9) | (2.8) | (3.3) | <124> |
| 63 years | -.094 | -1.3 | 15.7*** | -8.5** | -4.1 | -4.2** | 214 |
| | (.122) | (7.7) | (3.2) | (3.5) | (3.1) | (1.9) | <124> |
| 64 years | -.069 | -1.9 | 14.6*** | -11.0*** | -7.1** | -1.7 | 212 |
| | (.109) | (5.4) | (3.3) | (3.3) | (3.0) | (1.5) | <123> |
| 65 years | .008 | 1.1 | 12.9*** | -10.4*** | -7.8*** | -1.5 | 211 |
| | (.102) | (4.2) | (3.0) | (3.3) | (3.0) | (1.2) | <123> |
| 66 years | .019 | 1.2 | 12.4*** | -9.6*** | -8.2*** | -1.3 | 208 |
| | (.093) | (4.3) | (3.0) | (3.1) | (2.8) | (1.1) | <122> |
| 67 years | -.068 | -3.7 | 7.2*** | -6.6*** | -4.5** | -1.6 | 163 |
| | (.097) | (3.5) | (2.3) | (2.3) | (2.1) | (1.0) | <91> |

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
*Notes:* The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

**Table A.3:** Specification checks

| | | | | Program substitution: | | | |
|---|---|---|---|---|---|---|---|
| | Ever employed | Labor market earnings | ER benefits | Non-pension public transfers | DI benefits | Unemployment benefits | Obs <Firms> |
| *Column:* | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| i: Baseline RD est. | -.020 | -5.6 | 61.6*** | -42.5*** | -31.4** | -7.7 | 223 |
| | (.126) | (20.5) | (14.2) | (15.0) | (13.3) | (7.1) | <127> |
| ii: With controls | -.018 | -4.1 | 57.6*** | -36.2** | -24.6* | -9.5 | 223 |
| | (.136) | (20.3) | (15.7) | (16.1) | (14.5) | (7.2) | <127> |
| iii: Quadratic trends | .169 | 30.2 | 73.5*** | -67.0*** | -40.4** | -12.6 | 223 |
| | (.215) | (34.8) | (19.2) | (23.0) | (20.2) | (11.8) | <127> |
| iv: Bandwidth: | .103 | 19.3 | 75.9*** | -43.6* | -26.9 | -9.9 | 115 |
| 50% lower | (.203) | (33.2) | (20.3) | (22.2) | (18.6) | (11.0) | <70> |
| v: Bandwidth: | -.083 | -8.8 | 64.4*** | -42.5*** | -32.3*** | -9.0 | 305 |
| 50% higher | (.109) | (17.5) | (12.6) | (13.7) | (12.3) | (5.9) | <160> |
| vi: Triangular kernel | .054 | 8.4 | 66.3*** | -52.0*** | -35.0** | -9.6 | 223 |
| | (.150) | (24.5) | (14.4) | (16.4) | (13.8) | (8.6) | <127> |
| vii: Workers 12 months | -.067 | .4 | 60.6*** | -44.9*** | -34.0** | -5.7 | 213 |
| pre-bankruptcy | (.139) | (21.7) | (14.9) | (17.0) | (15.1) | (7.1) | <124> |
| viii: Workers 1 month | -.013 | .4 | 62.9*** | -37.0* | -27.9 | -5.6 | 163 |
| pre-bankruptcy | (.178) | (27.5) | (16.2) | (20.0) | (17.5) | (8.9) | <96> |
| ix: >=10 employees | .019 | -2.2 | 69.7*** | -55.5*** | -47.7*** | -3.5 | 189 |
| | (.136) | (22.2) | (15.3) | (15.8) | (13.5) | (7.7) | <94> |
| x: With "spurious" | -.015 | -2.3 | 49.0*** | -26.5* | -18.4 | -3.0 | 290 |
| bankruptcies | (.108) | (22.5) | (13.2) | (14.5) | (12.2) | (6.6) | <161> |

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.

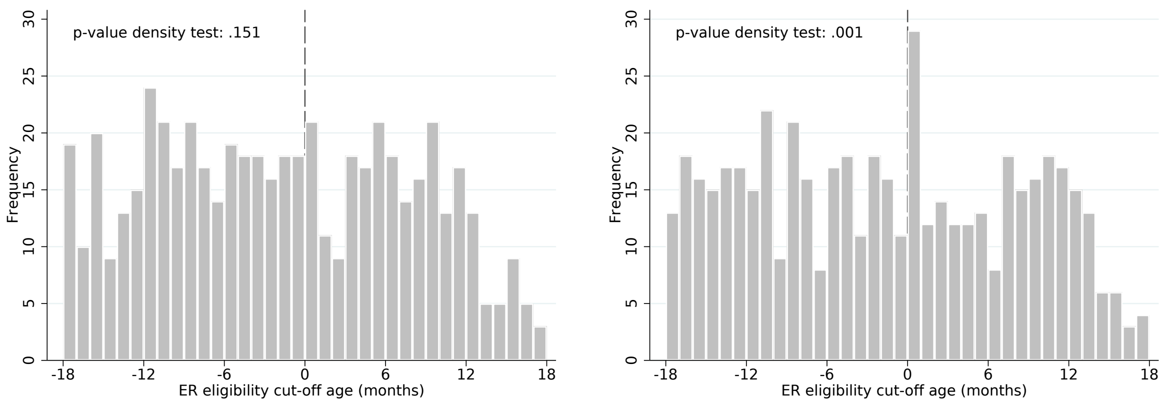
*Notes:* The table shows results of local RD regressions for each outcome and each respective specification. All specifications use linear separate linear trends except specification (iii) which uses separate quadratic trends. Main specification (i) uses a rectangular kernel and 12 months of bandwidth. Controls in specification (ii) include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Specification (vii) and (viii) includes workers who worked in bankruptcy firm 12 and 1 month respectively before the bankruptcy date (all other specifications include individuals who worked in firm 24 months before bankruptcy). Specification (ix) excludes individuals who worked at firms with less than 10 employees 24 months prior to bankruptcy. Specification (x) includes bankruptcies where at least 1/3 of (all) employees switched to the same firm. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

**Table A.4:** Placebo estimates: Firm bankruptcies without ER coverage

| | Ever employed | Labor market earnings | ER benefits | Program substitution: Non-pension public transfers | DI benefits | Unemployment benefits | Obs <Firms> |
|---|---|---|---|---|---|---|---|
| *Column:* | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Workers in firms without ER coverage: | | | | | | | |
| i: Baseline RD estimate | .002 | 20 | .8 | -6.1 | -15.2 | 3.1 | 417 |
| | (.099) | (23.5) | (2.1) | (16.2) | (13.7) | (5.7) | <372> |
| | [.579] | [79] | [4.4] | [76.7] | [40] | [10.9] | |
| ii: With controls | .004 | 19.3 | 1.8 | -3.3 | -11 | 5.6 | 417 |
| | (.093) | (21.4) | (2.1) | (15.7) | (12.8) | (6.0) | <372> |
| | [.579] | [79] | [4.4] | [76.7] | [40] | [10.9] | |
| iii: Industry weighted | -.076 | -14.1 | 1.3 | -3 | -6.8 | 3.9 | 640 |
| | (.165) | (31.2) | (3.3) | (28.9) | (25.6) | (10.5) | <499> |
| | [.648] | [80.2] | [6.8] | [79.7] | [42.3] | [13.4] | |
| Workers in firms with ER coverage: | | | | | | | |
| Mean (initially ineligible) | [.492] | [59.5] | [11.4] | [88.8] | [59.5] | [13.2] | 120 |

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity. Independent means of initially ineligible (the sample to the left of cut-off) in brackets.
*Notes:* The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome. Controls in specification (ii) include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. In specification (iii), individuals are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being employed in a firm with ER coverage and $P(I_i=1|x_i)$ is estimated with a logit model using industry fixed effects as control variables. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

**Figure A.1:** Distribution of eligibility age around cut-off for alternative samples

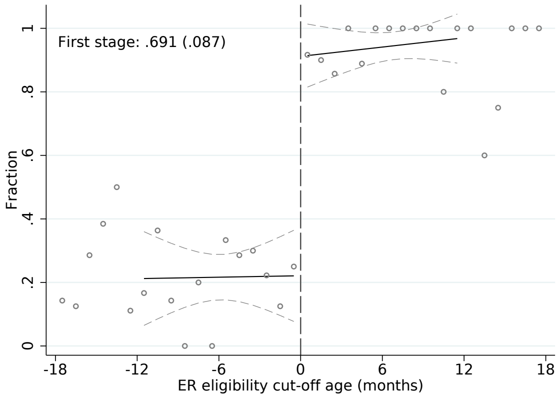



**(a)** Placebo sample: Bankruptcies in firms without ER coverage

**(b)** Comparison sample: Plant downsizings in firms with ER coverage

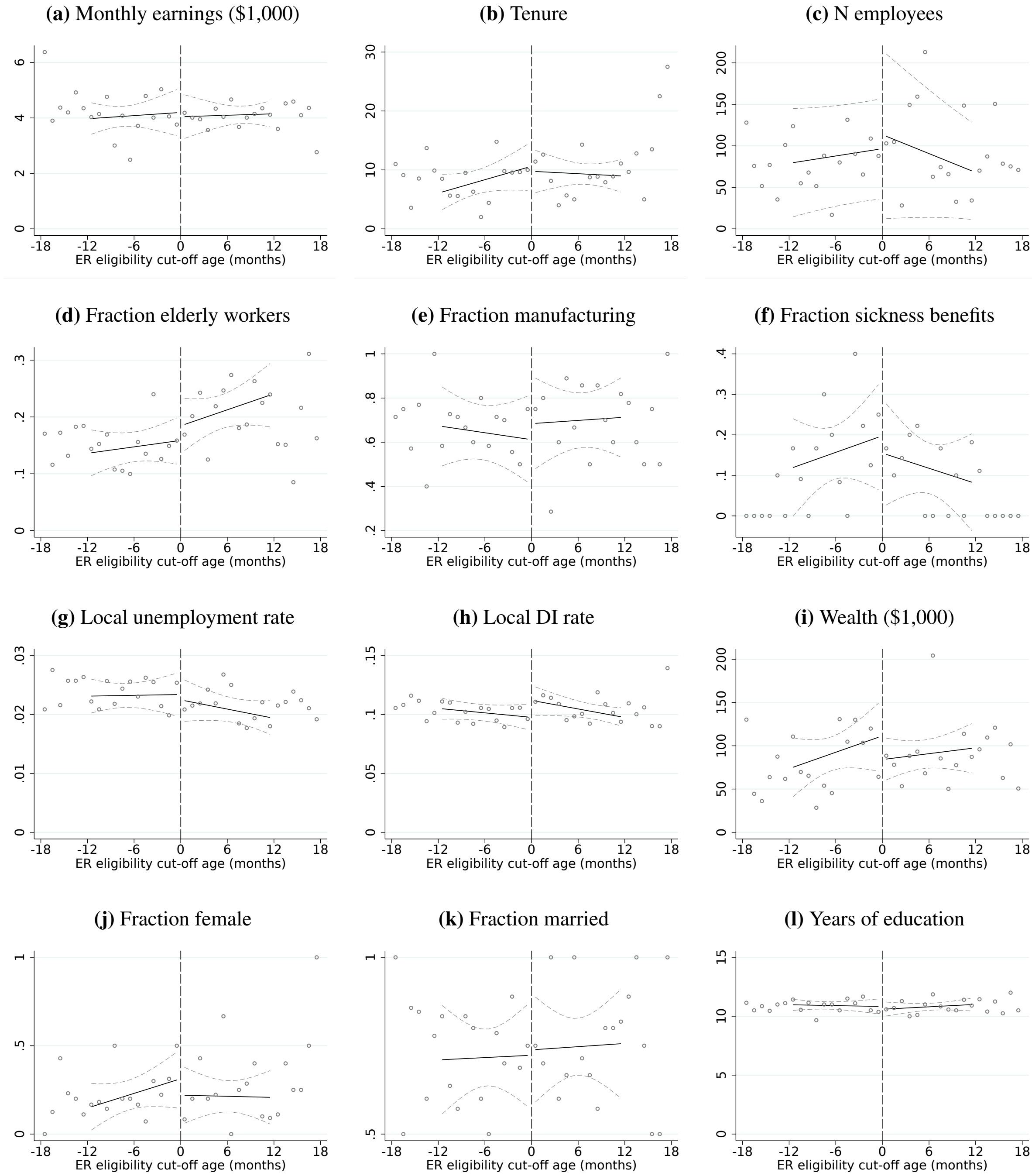
*Notes:* The figures show the distribution of age (in months; defined as in equation 1) around the individual eligibility cut-off. P-value is calculated using the discrete density test of Frandsen (2017). In figure (a), the sample consists of individuals employed by a firm without ER coverage 24 months before the firm's bankruptcy date, but otherwise satisfied the initial ER eligibility criteria (see details in Section 3.1). In figure (b), the sample consists of individuals who work in a private sector firm with ER coverage and at least 10 employees one month prior to the firm downsizing employees by at least 30 percent (excluding bankruptcies), but otherwise satisfied the initial ER eligibility criteria.

**Figure A.2:** First stage: ER benefit eligibility




*Notes:* The figure show the fraction of individuals satisfying the eligibility criteria for ER benefits at some point between ages 62–67 for each age-bin, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level.

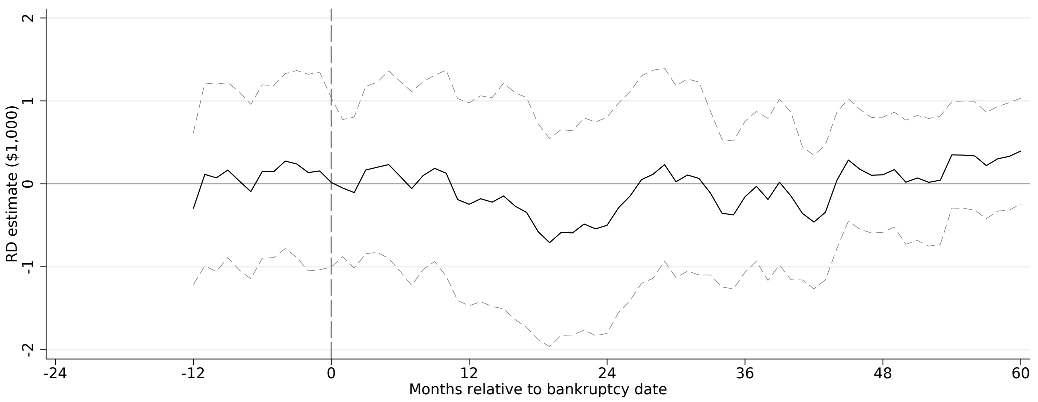
**Figure A.3:** Characteristics around cut-off



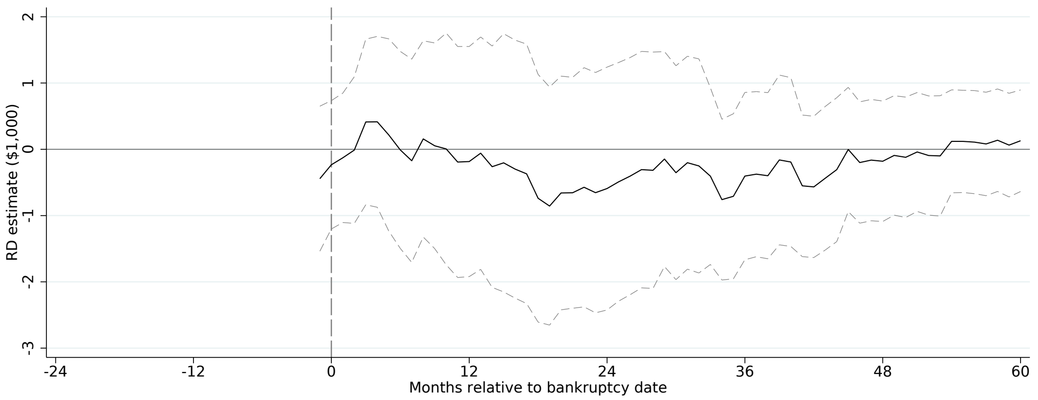

*Notes:* The figures show the unconditional means of each pre-determined covariate for each monthly age-bin relative to cut-off. Each covariate is measured 24 months before bankruptcy date. The black solid lines illustrate results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level and are robust to heteroskedasticity. Local DI rate and unemployment rates are measured at the municipality level. The share of senior workers are defined as the share of (all) coworkers above 57 years (excluding self). Earnings and wealth are measured in 2015 dollars (NOK/USD = 9).

**Figure A.4:** Labor market earnings effects over time (in $1,000) for alternative pre-determination of employment status

**(a)** Workers employed 12 months before bankruptcy

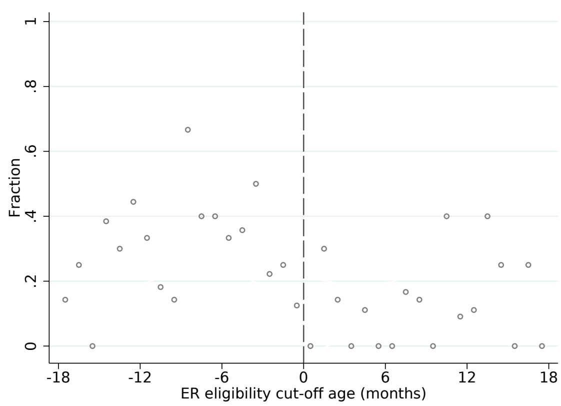



**(b)** Workers employed 1 month before bankruptcy




*Notes:* The figures show separate ITT estimates of labor market earnings (in $1,000) for each month relative to bankruptcy date for the sample of workers employed 12 months before bankruptcy (a) and 1 month before bankruptcy (b). The ITT effects are estimated by local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. Point estimates are represented by the black solid line, and the dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. Earnings are measured in 2015 dollars (NOK/USD = 9).

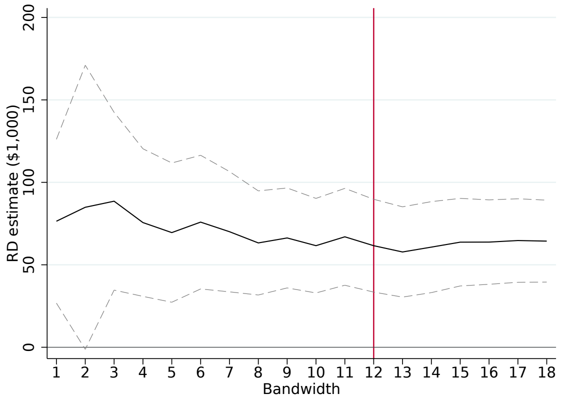
**Figure A.5:** Fraction of individuals who leave the firm before the bankruptcy date



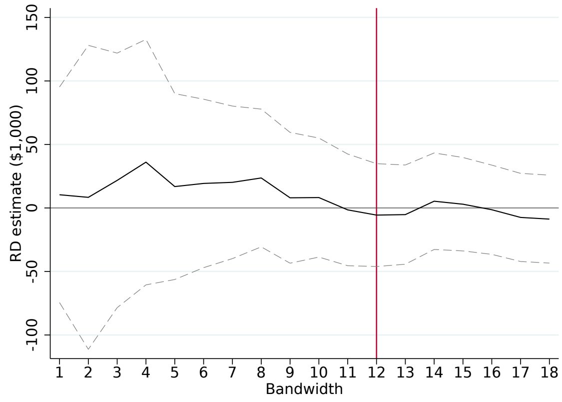

*Notes:* The outcome is defined as the individual exact date of resignation being less than the bankruptcy date.

**Figure A.6:** RD estimates and bandwidth selection: Cumulative outcomes

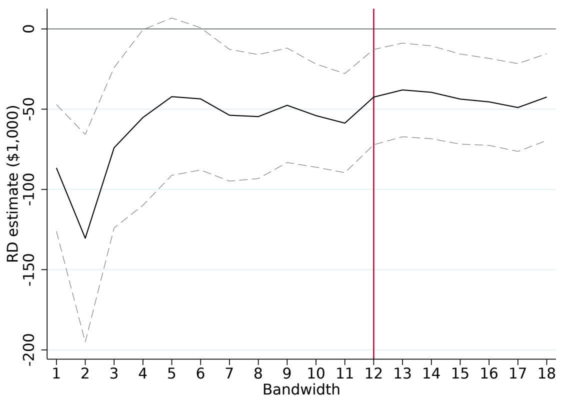
**(a)** ER benefit take-up



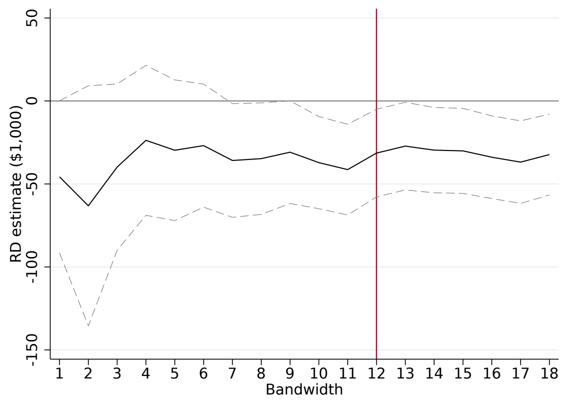

**(b)** Labor market earnings




**(c)** Non-Pension Public transfers

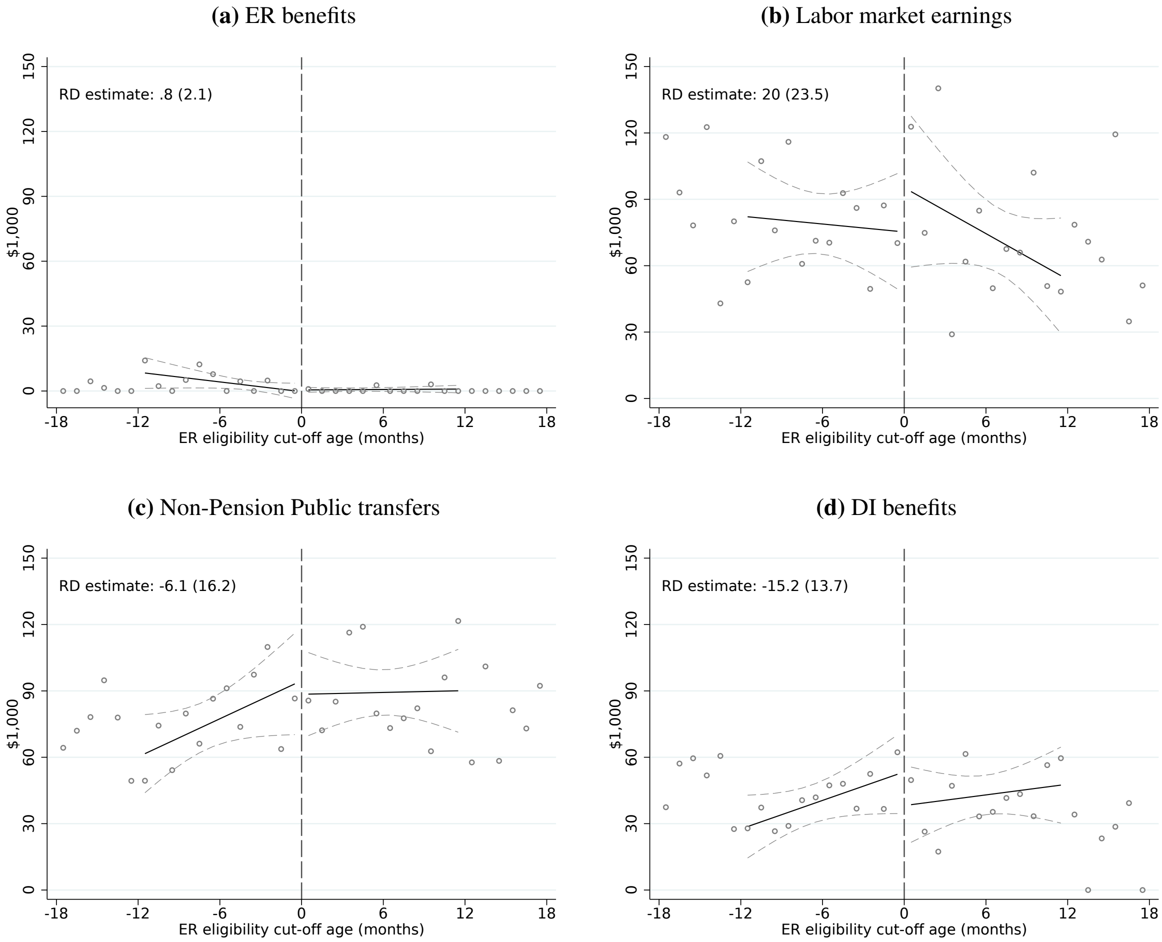



**(d)** DI benefits




*Notes:* The figures illustrate the estimated ITT effect for each outcome (in $1,000) for each choice of bandwidth (indicated on the horizontal axis). The ITT effects are estimated by RD regressions using a local linear regression and a rectangular kernel on each side of the cut-off. The red vertical line represents the baseline bandwidth choice of 12 months. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level and are robust to heteroskedasticity. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

**Figure A.7:** Graphical evidence of placebo estimates of cumulative outcomes (in $1,000): Bankruptcies in firms without ER coverage




*Notes:* The figures show unrestricted means for each age-bin of labor market earnings and social insurance benefit take-up in $1,000 between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

## Old-age pension benefit calculation

In this appendix, we outlay the details of how the old-age pension benefit levels are calculated in the Norwegian pension system. Except for the flat "top-up" of about $2,300, the ER benefit calculation was equivalent to this calculation. The old-age pension benefits consist of three main pillars: a guarantee pension, an income-related pension and a defined-contribution employer-provided pension plan.

**Guarantee pension** Individuals who had resided in Norway for at least three years between ages 16–66 were entitled to the minimum *guarantee pension*. However, the guarantee pension was *pro-rata* cut with years of residence succeeding 40 years. A full guarantee pension in 2015 was approximately $15,500, and the guarantee pension is indexed annually.[30]

**Income pension** The income pension was a mapping based on the 20 best years of income after the introduction of *Folketrygden* in 1967.[31] The mapping was based on a *base level* that we denote $G$, which is set by the government and indexed annually. In 2015, $1G$ was approximately $10,000. Essentially, accrual in a year was calculated as the income exceeding $1G$. For instance, a person earning $5G$ accrued 4 in that year. Only years where the accrual exceeded the average of the 20 best years up until that year would adjust the accrued level. The income pension on accrual was capped at $12G$ which implied, in combination with a decreasing accrual rate for income exceeding a certain threshold, that the replacement rate from the old-age pension system declined with income.[32] In the years between 1967–1991, the accrual rate of pension benefits was 45 percent of the resulting accrued number calculated as above, while in the years 1992–2011, the accrual rate was 42 percent. The average of the 20 years with the highest accrual numbers constituted the final number (*sluttpoengtallet*), which was multiplied by the accrual rate for the number of years of accrual pre-1992 and post-1992, and finally the base amount $G$, to determine the income pension level. As a minimum, the income pension yielded $1G$, given 40 years of residence (with similar pro-rata cut as the guarantee pension).[33]

**Defined-contribution pension plan** After 2006, employers had to make a mandatory minimum contribution of 2 percent of earnings of their employees to a *defined contribution* pension plan. A defined benefit scheme was allowed as an alternative, however the defined benefit plan had to be on at least the same level as the expected benefits under the defined contribution plan. Contributions were mandatory for income levels between $1G$–$12G$. Benefits were paid out as life-long annuities from claiming age.

[30] Exchange rate NOK/USD=9. There were different levels depending on marital status and the labor market status of the spouse.

[31] Folketrygden is the Norwegian law governing the social security system, known as the National Insurance Scheme. All residents are automatically member of the National Insurance Scheme.

[32] For the years 1967–1992, years with income exceeding $8G$ only gave one-third accrual for the income exceeding $8G$. For instance, a person earning $9G$ would accrue 7.33 that year $((8-1)+1\times 0.33)$. After 1992, income exceeding $6G$ would only give one third accrual. A person earning $9G$ would then get $(6-1)+3\times 0.33 = 6$.

[33] As an example, say an unmarried individual worked for 40 years, where 25 of those years were pre-1992. The person had a smooth income for all those years equal to $6G$, meaning that the average of the 20 best years gives an accrual of 5. The person claimed old-age pension in 2015, giving approximately:

$$\$10,000 + (0.45\times 5\times 25/40\times \$10,000) + (0.42\times 5\times 15/40\times \$10,000) = \$32,000$$

This benefit would be *upward adjusted* if it was lower than the minimum guarantee pension, which for 2015 was about $16,200 (at the regular level for married couples with one spouse claiming benefits and the other working or claiming DI).