Cash-on-Hand and the Duration of Job Search: Quasi-Experimental Evidence from Norway*

Christoph Basten (KOF Swiss Economic Institute and FINMA)
Andreas Fagereng (Statistics Norway)†
Kjetil Telle (Statistics Norway)

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Abstract

We identify the causal effect of lump-sum severance payments on non-employment duration in Norway by exploiting a discontinuity in eligibility at age 50. We find that a payment worth 1.2 months' earnings at the median lowers the fraction re-employed after a year by about eight percentage points. This is what we would expect if liquidity constraints force unemployed workers to accept a job offer earlier than would be optimal. As further support for the liquidity story, we use data on wealth before unemployment to show that the effect of non-employment duration occurs for the non-wealthy only.

Keywords: Unemployment, Optimal Unemployment Insurance, Liquidity Constraints, Severance Pay, Regression Discontinuity Design.

JEL: D91, E6, H5, J6

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†Corresponding author: Andreas Fagereng, Statistics Norway, Research Department, Postboks 8131 Dep, NO-0033 Oslo, Norway. Email: andreas.fagereng@ssb.no

1 Introduction

Are unemployed households liquidity-constrained, so that they have to accept a job offer earlier than would be optimal? This is the argument implied by Card et al. (2007a), based on evidence that Austrian job losers eligible for lump-sum severance payments take more time until their next job than do their non-eligible counterparts. Together with Chetty (2008), which shows theoretically how liquidity constraints can affect job search duration and finds longer durations for those with (possibly endogenously) greater financial resources in the United States, this has transformed the unemployment duration literature, which hitherto had assumed that unemployment insurance (UI) prolonged search duration exclusively by distorting the relative price of being unemployed rather than employed ("moral hazard").[1]

Yet two questions remain: First, how generalizable are these findings from Austria and the United States to other countries? The question arises because both countries grant UI only for a relatively short period, maximally 6 months in normal times,[2] and because especially the United States has a more unequal wealth distribution than the majority of OECD economies. Hence, one might think that smaller or no liquidity constraints will exist in most other OECD economies. Second, does the reduced-form effect of severance payments indeed reflect liquidity constraints in the sense that households are unable to spend more resources while out of work? If so, we would not expect an effect of severance payments on non-employment duration of the wealthy, only an effect on the non-wealthy.

The present paper addresses both of these questions. First, we investigate whether severance payments prolong job search in Norway, which has one of the world's most generous UI systems, replacing 62% of prior income for up to 2 years, and also has one of the rich world's most equitable wealth distributions. Despite these circumstances, which may be thought to render liquidity

[1] For examples, see Katz and Meyer (1990) or Lalive et al. (2006).

[2] After that period, households can still receive "unemployment assistance", which is however lower and means-tested.

constraints less likely, we find clear evidence of a causal severance pay effect. The severance pay amounts to about 1.2 months of net-of-tax median earnings, which allow the job-seeker to "top up" from the 62% replacement rate provided by the UI system to 100% of his prior income for about 3.2 months. These payments are found to increase average non-employment duration by about a month, and to reduce the fraction re-employed after 12 months by 8 percentage points, which corresponds to a relative reduction of 14 percent. Thus, severance pay effects do not seem to be specific to countries with relatively short maximum UI durations.

Second, we investigate whether the effect on job search duration reflects liquidity constraints, as put forward in Card et al. (2007a) and Chetty (2008). Our data includes pre-unemployment wealth measures, which enable us to use the quasi-experimental variation in severance pay to investigate whether the severance pay effect differs between the wealthy and the non-wealthy. Moreover, we exploit the fact that we observe various measures of household wealth, both absolute and scaled by prior annual income, and interact these measures with severance pay eligibility. Across measures of financial wealth and bank deposits, our findings confirm that the sensitivity to cash-on-hand is mostly present for the presumably liquidity constrained. The response in non-employment duration is substantial for those with below-median wealth, and small or non-existing for those with above-median wealth. For the below-median households we find that the severance payments reduce the fraction re-employed after 12 months by 13 to 17 percentage points. These pieces of evidence lend additional support to an interpretation of the severance pay effect as liquidity constraints, which due to data limitations the existing literature was not able to provide.

Our identification exploits the fact that in severance pay agreements concluded between the Confederation of Norwegian Enterprise and the Norwegian Confederation of Trade Unions, only those aged above 50 on the day of their job separation are eligible for payments. This allows us to implement a regression discontinuity design (RDD), comparing those aged just above 50 to those

aged just below. A number of tests verify that the two groups are otherwise statistically identical along the relevant dimensions. Furthermore, the mechanism of the pay-outs, which are made by a joint fund financed by employers in a not experience-rated way, ensures that (as we verify in the data) there is no selective layoff behavior.

The remainder of the paper is structured as follows: Section 2 outlines the Norwegian severance pay program and discusses our empirical strategy. Section 3 introduces the data. Section 4 presents the general results on the effect of lump-sum severance payments on job search duration, and Section 5 provides separate effect estimates for the wealthy and the non-wealthy. Section 6 concludes.

2 Empirical Strategy and Institutional Details

The challenge in identifying the causal effect of severance payments in most empirical setups is that eligibility or amounts depend on factors like age, tenure or prior earnings which, however, are likely to be correlated with non-employment duration also through other channels. To address this problem, we exploit a rule under which employees separated from their job just before the age of 50 are not eligible for severance pay, whereas those aged just above 50 are. In the immediate neighborhood of the discontinuity all other factors that might influence our outcomes of interest can be expected to be statistically identical, so that any discontinuity in outcomes can be attributed credibly to the discontinuity in severance pay. In this section we first describe important features of the severance pay agreement between the Confederation of Norwegian Enterprise and the Norwegian Confederation of Trade Unions, as well as some relevant aspects of the Norwegian welfare system in general. Then we discuss our empirical strategy.

2.1 Institutional Details

While many firms in Norway have heterogeneous severance pay rules at the firm level, those who are members of Norway's Confederation of Trade Unions, "Landsorganisasjonen i Norge" (LO) and the Confederation of Norwegian Enterprise, "Næringslivets Hovedorganisasjon" (NHO), have agreed on common rules about eligibility and amounts of severance pay ("Sluttvederlag", SLV) paid to employees who are involuntarily separated from their jobs. The LO is Norway's largest and most influential workers' organization, covering about 850,000 Norwegian employees, or one-third of the Norwegian labor force.

For our time period 2002 to 2009 the magnitudes of the severance pay from the program are plotted by age in Figure 1.[3] It shows that the severance pay amounts increased from zero to NOK 18,000 (USD 2,700) at age 50, the discontinuity we employ.[4] The severance pay from the program is tax exempt. In our sample, median monthly earnings after taxes were $ 2,271 (see Table 1), so the severance payment amounted to about 1.2 monthly after-tax incomes for the median earner. For workers in this earnings range, the Norwegian UI replaces 62% of previous earnings, meaning that the severance pay would allow the worker to "top up" from the 62% UI replacement rate to 100% of his former earnings for about 3 months, and top up to lower replacement rates correspondingly longer. As is clear from this description, the size of this lump sum amount does not depend on prior earnings.

A key advantage of the LO-NHO agreement for our identification is that actual payments are made not by firms, but by a fund to which firms contribute each month according to their number of full-time employees, and not according to past layoffs. As our sensitivity tests verify (Section 4.2), this ensures that there is no manipulation of the threshold in the sense of firms trying to systematically lay off workers just below or just above age 50.

[3]For an overview of the exact severance pay amounts by period and age, see Table 2.

[4]There are also further increases at ages 52, 54, 56, 58, 59 and 60, as well as annual decreases after age 60. Worth noting however, is that the other increments until and including the one at age 59 are rather small, and above 60 other simultaneous discontinuities apply, in particular in access to early retirement and extended UI, thus violating the exclusion restriction required for identification (see Section 2.2).

Apart from the requirement that a worker needs to be above 50 years of age at the day of separation, there are two other main requirements that need to be met for the worker to be eligible for the severance pay. First, the worker needs tenure in a firm participating in the LO-NHO agreement. At the day of separation the laid off worker is required to have at least 10 years of continuous tenure in the firm. Alternatively, the worker also meets the tenure requirement if he has at least 15 years of continuous tenure in a combination of participating firms, or if he has worked - possibly in several spells - in the same firm for at least 20 years and his spell at the time of separation has lasted for at least three years.

Second, only workers who are separated from their jobs *involuntarily* are eligible for the severance pay. Involuntary layoffs typically occur in association with plant downsizing, including closure, or financially necessary restructuring which makes the worker's qualifications redundant.[5] The involuntary-requirement applied for the severance pay is similar to the one applied for UI, but there is no rule that the worker needs to file for UI to receive the severance pay.

A worker is typically required to be given notice of a layoff between three and six months in advance. At the time of notification the employer submits an application to the LO-NHO fund for the laid off workers, with a copy of the resignation letter. Our contacts at the LO-NHO office has confirmed that in the majority of cases the severance pay is payed out to the workers between 2 and 8 weeks after the last day of work. Since this is a one-time transaction between the fund and the individual, details about a workers bank account are not collected. Instead the payment is sent by mail in form of a *cash check*, which the worker then deposits in the bank.[6]

[5]Workers in Norway are in general protected by relatively strict rules when it comes to layoffs related to individual (mis)behavior (Addison and Teixeira, 2003). However a firm may lay off workers if its demand for labor is permanently reduced, and the firm can verify that other relevant work elsewhere in the firm cannot be found for each of the laid off workers.

[6]For further information on LO, NHO, and their joint scheme, see http://www.sluttvederlag.no/

The welfare system in Norway is considered to be generous (Kautto, 2001) and the participation in welfare programs is compulsory for residents. To be eligible for UI, the individual must have earned a minimum amount in the year prior to unemployment. This amount is adjusted in accordance to wage inflation, and in 2007, for instance, the amount was about NOK 100,000 or USD 16,000.[7] Virtually any full-time worker will meet this requirement, and will then receive 62% of the pre-unemployment earnings for earnings up to a relatively high ceiling (about NOK 400,000, or USD 64,000, in 2007). The maximum duration of UI is 2 years, but unemployed workers above 64 years can receive UI until the retirement age of 67.[8]

While being on UI or when UI has expired, the resident is entitled to means-tested social assistance to cover basic subsistence cost (no maximum duration), or possibly to sick money or disability pension. Workers eligible for UI would typically also meet the requirements for sick money given that a medical doctor certifies that they cannot work (or seek work) because of sickness or injury. The maximum duration of sick money is one year, and the replacement rate is 100% (up to the same ceiling as for UI). When sick money has expired, and medical or vocational training has proven unsuccessful, disability pension is a common alternative (Markussen et al., 2012, 2011). The disability insurance covers every person from the age of 18 to 67 and the replacement rate would typically be about 67% (again up to the above mentioned ceiling). The main requirement is that the person is incapable of supporting himself because of a permanent disability, which needs to be medically certified. Though layoffs or spells of unemployment are formally irrelevant when evaluating eligibility for disability pension, several studies have documented that exposure to plant downsizing or economic incentives affect individuals' drawing of disability pension (Rege et al., 2009; Kostøl and Mogstad, 2013). From the age of 67 every resident is entitled to elderly pension, normally of the same magnitude as the disability pension. We observe that about 70% of our sample end up finding

[7]Further, a worker may also be eligible for benefits if he or she over the last three full calendar years has earned twice this amount.

[8]The official retirement age in Norway is 67, but in practice, early retirement schemes are available for a majority of workers in Norway from the age of 62 (Vestad, 2013).

new work. Some of the remaining 30% will find work after we stop observing them, whereas the rest will remain out of the labor force.[9]

2.2 Empirical Strategy

Our empirical strategy relies on the fact that employees separated from their job just before the age of 50 are not eligible for severance pay, whereas those aged just above 50 are.[10] For those aged between 48 and 52[11] and known to have had 10 or more years of tenure, we estimate the following equation for different outcome measures y:

$$y_i = \alpha + \beta T_i + \gamma z_i + \delta T_i z_i + \varepsilon_i \tag{1}$$

Here T is an indicator for being aged above 50, z is the forcing variable (age-50), and ε is a mean-zero error term. So essentially we estimate the effect of being aged above 50, while controlling for the effect of age *per se*. Since we can make the interval small, we rely on a linear control for age,[12] and we allow the effect of age to differ on the two sides of the discontinuity. The specification

[9] Card et al. (2007a) report that 85% of their sample of Austrian workers is observed with a new job before the end of their sampling period. The difference is likely due to the considerably higher mean age among the Norwegian workers, since our study applies the discontinuity in eligibilty to severance pay at the age of 50. The mean age in our sample is 50, compared with 31 in the sample of Card et al. (2007a).

[10] Unfortunately, our data do not allow us to exploit tenure as a RDD assignment variable. While we *can* identify those with tenure above 10 years, our lack of information from before 1992 disables us from verifying that a worker does *not* meet at least one of the tenure requirements (e.g. the rule of 20 years in same firm). See Sections 2.1 and 3 for details.

[11] With a view to the next, albeit small discontinuity at age 52, our baseline specification uses a bandwidth of only 2 years, but using the Imbens and Kalyanaraman (2012) optimal bandwidth of, in our case, 3 years turns out to produce quantitatively very similar estimates, at greater statistical precision due to the larger sample size (see Tables 3 and 8).

[12] Our point estimates change very little if we instead control for age using a 2nd order polynomial.

does also allow us to add an interaction of T with different measures of wealth when we investigate how the severance pay effect varies with prior wealth. To maximize transparency and facilitate interaction of the treatment indicator with further covariates, our baseline specification uses a rectangular kernel, thus weighting each observation equally. This can be implemented by simply estimating Equation 1 by ordinary least squares. The sensitivity checks (see Table 8) reveal that our results are robust to the alternative use of a triangular kernel, which assigns greater weight to observations closer to the threshold and which Fan and Gijbels (1996) showed in general to be preferable for RDD purposes.[13]

3 Data

Administrative data from the *FD-Trygd* events database of Statistics Norway are used, and they cover the universe of Norwegian residents for the period 1992 to 2010. We start with information on all job separations by male employees occurring on or before December 31 2008.[14] This ensures that we can observe job search for all workers separated from their jobs for at least two years, which is also the maximum duration of unemployment benefits. We then merge in information obtained from the LO-NHO office on which firms were participating in the agreement and restrict to those that were. Furthermore, we add information from *FD-trygd* on exact age at the day of the job separation, and we restrict the main sample to those aged between 48 (inclusive) and 52 (exclusive) on the day of their job separation.

Since we do not explicitly observe which of the job separations are involuntary (another requirement for receiving severance pay), we exclude cases

[13]For background papers on the RDD approach, see Trochim (1984), Imbens and Lemieux (2008) and Lee and Lemieux (2010).

[14]We focus on males as even in Norway females earn significantly less than their husbands and they typically work part time. We have also investigated the effect separately for women. Since wives typically earn less than husbands, the household may be better able to tap into the husband's higher incomes to resolve potential liquidity constraints during female unemployment. In line with this, no significant severance pay effect is found for the women.

(using information from *FD-Trygd*) in which the job separation is likely to occur because of some other event, after which individuals are likely not to be searching for a new job. These are, first, separators receiving disability pension in the year of their job separation, second, those on parental leave (given the gender and age range of the sample, these are very few), and third, those who return to the same firm within 3 months. All these restrictions will reduce the fraction of voluntary quitters, but they may also introduce bias due to endogenous sample selection. Luckily, however, we find that our point estimates change very little when we lift any or all of these restrictions.[15]

As severance pay eligibility requires at least 10 years of firm tenure, we restrict the sample accordingly (see Section 2.1). Since we cannot reliably observe tenure before 1992, we can only be sure that the tenure requirement is met for individuals who are in the job for at least 10 years after 1992. This forces us to drop individuals who are separated from their job before 2002. Among the remaining individuals, we keep only those with observed tenure of at least 10 years at the time of job loss. This reduces the sample size significantly, but it guarantees that everyone in our sample does satisfy the tenure requirement for severance pay, so that the discontinuity at the age threshold reflects as closely as possible the full treatment effect of the payment.

A last restriction from our data is that we do not observe the amounts actually received, as would be necessary to compute the Wald estimate of the effect of actual severance pay on job search duration. Instead, like Card et al. (2007a), we can only estimate the reduced-form or intention-to-treat (ITT) effect of severance pay eligibility, which constitutes a lower bound on the effect of actual severance pay. But with the other sample restrictions in place, as explained above, and since the severance pay applications are sent to the LO-NHO office by the employer together with the layoff notification (see Section 2.1), we can expect compliance to be rather high, and so our ITT estimates are expected to be not much below the corresponding Wald

[15]We have also performed our analysis excluding workers who start a new job the day after the separation. Again, results are unchanged.

estimates.

We follow Card et al. (2007a) in using as outcome variable "non-employment duration", defined as the number of days from layoff until the start of a new job, as opposed to the duration of registered unemployment. Their argument, based on the findings in Card et al. (2007b), is that people may cease to register as unemployed once their benefit eligibility runs out.[16]

Our first outcome measure is the completed duration of job search. One drawback of this measure is that we observe it only for those who start a new job by 2010. Furthermore, this measure is somewhat sensitive to the choice of the duration after which we censor. Card et al. (2007a) censor after 6 months, on the grounds that this is the maximum UI duration in their sample. In our case the same argument speaks for censoring after 2 years, which we implement throughout the analysis. We further define as outcome variables the fractions re-employed after 12, 15 and 18 months, and we also plot the severance pay effect estimates on reemployment fractions for all durations of non-employment up to 24 months.

Complementing this, we estimate a Cox regression in which the dependent variable is (the logarithm of) the hazard rate, i.e. a person's propensity to start a new job given that he has not yet done so. This allows us to estimate the effect of severance pay on the hazard in any given day since job loss without having to specify whether in general the hazard is increasing, decreasing or flat in the time elapsed, however it does require us to assume that the effect is the same at all stages of the spell.[17]

[16] An additional reason in our case is that, as becomes clear from Bratsberg et al. (2010) and Kostøl and Mogstad (2013), many individuals who would be labeled as unemployed in other countries draw on disability insurance instead of unemployment insurance in Norway. Similar considerations about moral hazard vs. liquidity constraints apply to those on disability pension as to those on regular unemployment insurance (see for instance Autor and Duggan, 2007). In any case, when we perform the analysis excluding any household ever receiving disability pension in our observation window, our main results remain unchanged.

[17] See Cox (1972) for the original outline of the Cox Proportional Hazard model, or Card et al. (2007a) for another recent application.

Given that the severance pay results in longer search durations (as we will show), the match quality of the new job may also be affected. Following Card et al. (2007a) we proxy new-job quality by the change in earnings between the old and the new job. We measure the growth in log earnings from the calendar year before job loss to the calendar year after starting the new job. In addition, we also proxy new job quality by looking at how long the worker stays with the the new employer, captured by duration (again censored after two years) and by the fractions who are still in the new job two years after employment.[18]

A final data issue to be discussed is wealth. The existing literature on liquidity constraints of households as well as on the illiquidity of real estate during unemployment (Chetty and Szeidl, 2007) suggests to ignore real estate and focus instead only on financial wealth (deposits, bonds, stocks and mutual funds), or alternatively on deposits only. It also suggests to use wealth at the household rather than at the individual level. Of course how long someone can sustain the household with a given amount of savings will depend on the monthly expenditures such as monthly rent, insurance payments etc, which in turn will be highly correlated with prior income. The severance pay is also constant and does not vary with prior income. Hence, we use both absolute financial wealth and deposits, *and* both measures scaled by average annual income across the last three years before the year of job loss.

Table 1 shows in the left panel the summary statistics for the sample on which our main (bandwidth 2) results are based, and in the right panel those for a placebo sample. Individuals in the latter sample, used for some of the sensitivity checks below, satisfy all the same requirements as those in the main sample, except that they come from firms not participating in the severance pay agreement. Both samples have mean and median ages of about 50, and tenure of about 16 years at the mean and 14 at the median. Uncensored non-

[18]This requires data on new jobs for the workers also after 2010, which are not yet incorporated in our main data source (*FD-trygd*). To keep our sample intact, for 2011 and 2012 we have made use of records from the Employer-Employee registry, which is the data source used by *FD-trygd.*

employment duration among those for whom the next job start is observed in the sample (corresponding figure for the placebo sample in parentheses) is about 10 (11) months at the mean and 2 (3) at the median. About 47 (40) percent have less than high-school education, 29 (24) percent have a high school degree, and 24 (36) percent have a college degree. Average annual income before taxes is US$ 44,885 (44,701) and household financial wealth about US$ 41,310 (40,916) at the mean.

4 Results

4.1 Main Results

Our main results are displayed in Table 3 and Figures 2 through 5. The table reports the coefficients from estimating Equation 1: With the conservative baseline of 2 years in the upper panel, and with the Imbens and Kalyanaraman (2012) optimal bandwidth of 3 years in the lower panel. The two bandwidths yield very similar point estimates, but the results from applying the wider bandwidth has higher precision due to the larger sample size. T denotes the indicator for being aged above 50, while z and Tz are the controls for a linear effect of (age-50), allowing it to differ on the left and right side of the discontinuity. In column 1 the dependent variable is the completed duration until re-employment, censored after 2 years, whereas the outcomes in columns 2-4 are the fractions re-employed after respectively 12, 15 and 18 months.

Depending on the bandwidth, eligibility for the severance payment, worth 1.2 months' after-tax salaries at the median, is found to prolong non-employment duration by between 37 and 41 days. In line with this, amongst those eligible the fraction re-employed after 12, 15 and 18 months is found to be about 8 percentage points lower. The same effects can also be seen visually in Figures 2 through 5, plottng the completed non-employment duration and the three fractions against 6-month bins of age, along with a fitted linear curve of length 2 on each side of the threshold. The graphs show that duration is indeed increasing and re-employment probability decreasing in age, confirming the need

for a quasi-experimental design. At the same time, despite the remaining noise, the fractions re-employed exhibit a clear jump at age 50. To investigate the timing of the effect further we have also looked at the reemployment fraction at all monthly stages during the first two years after job loss. Figure 6 shows the effect estimates from these 24 regressions on the fraction re-employed after each month separately. We see that severance pay does not affect the fraction employed in the first months after layoff, but it lowers re-employment by around 8 percentage points from 11 to 19 months after layoff.

This finding is also reflected in the upper panel of Figure 7, where we plot the hazard rate against the days elapsed since job loss, separately for those with and without severance pay eligibility. The continuous line represents those aged below 50 at job loss and hence ineligible for severance payments, whereas the broken line represents those eligible. The figure shows three interesting findings. First, both lines are almost monotonously downward-sloping, implying that the propensity to start a new job given that none has been found so far is declining over time. Second, the line for those eligible is almost always below that for those not eligible, implying a lower job finding hazard for the former on most days. And finally, the difference between the two curves is largest after about a year, consistent with the above finding that the difference in the fractions that have already started a new job peaks a bit after one year out of work. The corresponding survivor functions for the two samples are plotted in the upper panel of Figure 8, where again the continuous line represents those aged below 50 at job loss. The gap between the groups reaches the maximum after about 1.5 years and then starts to close.

The effect of severance pay on the job starting hazard, as visualized in Figure 7, can also be investigated by means of a Cox regression, in which the log hazard rate is regressed on the covariates of interest. It allows us to estimate the effect of severance pay eligibility on the hazard, averaged across all days within the first two years. This has the advantage of leaving unspecified how the hazard rate changes over time, but it requires the assumption that the effect of severance pay is the same on any day within those first two years – a

simplification which, as we have seen, does somewhat differ from the pattern reflected in our data. The results of this analysis, displayed in the last column of Table 3, tell us that on average the payment reduces by 14-16 percent the job-finding hazards displayed in Figure 7.

How does the size of the effect compare to the one Card et al. (2007a) found for Austria? In their case a payment worth 2 months' wages lowered the re-employment probability by 8-12% on average over the first 20 weeks after job loss. In our case, a payment worth 1.2 months' wages at the median lowers the re-employment probability by about 8 percentage points, corresponding to a relative decline of about 14%, as the average fraction reemployed after 12 to 18 months is about 0.55 (see Table 1). Hence relative to the size of the payment our effects appear somewhat larger. One likely reason for this is the fact that we measure the effect at later points in the spell, where many of the Austrian job losers are presumably already back in a new job. Another is the more generous UI: If households are willing to remain unemployed as long as they can maintain consumption at say 80% of previous income (or any other percentage above the UI replacement rate), then any given severance pay amount will "last longer" the greater the fraction already covered by UI.

With the result that non-employment durations are indeed sensitive to a lump sum severance pay, we may also expect it to affect later job outcomes. The fact that households choose to use some of the severance pay money for longer search durations suggests that the severance pay makes them better off. To see if the severance pay results in a better subsequent job, we have attempted to follow Card et al. (2007a) and performed the analysis on wage growth from previous to new job, and we have also looked at the duration in the new job. The results are displayed in Table 4, where we also have estimated the effect of severance pay on the fraction that remains in their job after 2 years. The results indicate that the severance pay may have a slightly positive impact on the duration of the new job, and the fraction that remains after 2 years, but a slightly negative effect on the wage growth. However, like Card et al. (2007a) none of these effect estimates are significant at conventional

levels.

4.2 Sensitivity Checks

The first possible concern that may arise about the credibility of our effect estimates is that our controls for the effect of age may not suffice. After all, an effect of age per se is apparent from the Figures 2 through 5 and is also reflected in the coefficients on z and Tz in Table 3. To test this, Table 5 displays the discontinuities in our outcomes of interest for different placebo age thresholds, going in half-year intervals from age 47 all the way until age 51, after which the small discontinuity in severance pay at age 52 will come into play. The table shows that indeed the only age threshold at which we observe significant discontinuities in our outcomes of interest is that at age 50.[19]

The exclusion restriction represents another possible concern. What if other policies that are correlated with non-employment duration do also change at age 50? While there are discontinuities in early retirement access at ages above 60, we are not aware of other policy discontinuities at age 50. One may worry that some policy discontinuities do nonetheless exist. To explore this, we repeat our analysis on a placebo sample of individuals who satisfy all the same requirements as those in our main sample, except that they are separated from firms which were not affiliated with LO-NHO and hence did not participate in the severance pay agreements. The results of this test are displayed in Table 6. Indeed, no significant effect of being aged above 50 is found here, supporting the view that the exclusion restriction is indeed satisfied.

As in any RDD, we need to explore whether there could have been selection around the threshold. As discussed in Section 2.1 above, severance payments

[19]Note that as we approach the actual cut-off at 50, the bandwidth of two years implies that the treatment group starts to include those actually treated (>50). Thus, a non-minor (but insignificant) point estimate at age 49.5, for example, is not surprising since here the bandwidth of two years implies that the treatment group (49.5 to 51.5) does in fact include many of the men actually treated.

under the LO-NHO agreement are made by a joint fund and financed in a not experience-related way, thus alleviating concerns that firms might choose to lay off (a selected group of) individuals just before they turn 50. By contrast the fund has an incentive to ensure that firms and employees do not collude to systematically postpone layoffs until after age 50, but how well does it enforce this in practice? A first check is to test for discontinuities at the threshold in the density of observations, following McCrary (2008). In the present case, this test yields a coefficient for the log difference in density of -0.129, with a standard error of 0.130, so we fail to reject the null hypothesis of no difference. In line with this, we see no discontinuity at 50 in Figure 9, which plots the frequency of observations in our sample for each 1-month bin between age 48 and age 52, and the same story emerges for different bin sizes.

While this suggests that there is no systematic selection of the number of individuals to either side of the threshold, one may still worry that the individuals on each side differ in *type*. To check this, Table 7 reports the results of repeating our main regressions on a set of variables of which the values should be predetermined at the time of the job separation. Here we look in particular at the financial variables also used to investigate the plausibility of the liquidity constraints explanation, as well as indicators for respectively higher education (other education categories were also tried and yielded similar results), receipt of sickness benefits in year before job loss, and the share of cases working in manufacturing (again, the result of no discontinuity holds also for other sectors). These analyses, using the exact same methodology as for our main outcome variables, do not reveal any discontinuities at the age 50 threshold. This is also illustrated visually in Figures 10 through 16, lending further support to the view that our main findings can be given a causal interpretation.

Another concern that always arises in a RDD is how sensitive the results are to the choice of different bandwidths or kernels. In general the trade-off is between limited precision at very narrow bandwidths and potential bias at too wide bandwidths. Our rather conservative default choice of 2 years on each

side has been motivated by the desire to avoid any bias from the next, albeit small discontinuity in severance pay amounts at age 52 (cf. Figure 1). This said, Table 8 displays the results of varying the bandwidth. The four columns show these for the same four outcomes (completed duration, and fractions re-employed after 12, 15 and 18 months). The top panel provides the results from varying the bandwidth but keeping the rectangular kernel. The bottom panel provides results using a triangular kernel. In both panels we show first the results obtained under the Imbens and Kalyanaraman (2012) "optimal bandwidth", which varies a bit across outcome variables, but is around 3 years in the top and around 4 years in the bottom panel. Then we show results obtained when using half the optimal bandwidth. The point estimates are slightly larger than with our conservative 2-year bandwidth choice and are also more significant. In general, they confirm our main results.

5 Liquidity Constraints?

In the previous section we have shown that the causal effect of lump-sum severance payments on job search duration which Card et al. (2007a) found for Austria is also present in Norway, making it plausible that the finding applies also to other OECD economies. But given that Norway has both a more egalitarian wealth distribution and a more generous welfare state than for instance Austria or the United States, the question arises whether the severance pay effect does indeed reflect liquidity constraints. Clearly, if the correct interpretation is one of liquidity constraints, then the same severance payment should have a smaller effect on those with higher prior wealth, especially when scaled by prior income, as they should have enough other resources to tap into. Further, if we interpret previous income as a proxy for previous expenditures, households with a high wealth level relative to previous income will be able to sustain previous expenditure levels longer after job loss. Thus, it would seem plausible that households with little wealth relative to prior income could run into liquidity constraints faster, and for these households the effect of severance pay could be more pronounced.

Table 9 provides results from the RDD where we have interacted severance pay eligibility with the indicator for whether household holdings of financial wealth and deposits exceeded the sample median (before job loss). We use both the absolute holdings, and holdings relative to annual income in the last year before job loss. For example, in the first column of Table 9 the interaction term is decided by the level of household pre-unemployment holdings of financial wealth. For the outcome measure *Re-Employed After 15 Months,* we find that the severance pay reduces the likelihood of re-employment after 15 months by 17.39 percentage points for the households with financial wealth below the median. For the above-median households, the effect is virtually zero and clearly not significantly different from zero at any conventional level. Our main results, estimated on the whole sample in Table 3, are thus the average of the fairly strong effects for those with below-median holdings and the zero effects for those with above-median holdings. This picture is confirmed for the other liquidity proxies in the table. Hence, we provide first evidence in favor of a liquidity constraints interpretation in a setting with quasi-experimental assignment of severance pay.

6 Conclusion

We have documented a causal effect of lump-sum severance payments on the duration of job search in Norway. To our knowledge, this is only the second paper in the literature to find such a causal effect (after Card et al. 2007a), and the first to find it in a Scandinavian-type welfare state. This makes it likely that such effects hold also in other OECD economies.

But given that Norway has both a more egalitarian wealth distribution and a more generous welfare state than for instance Austria or the United States, the question arises whether the severance pay effect does indeed reflect liquidity constraints. We have investigated how the size of the severance pay effect varies with prior wealth, and we find no effect for those with above-median wealth and large effects for those with below-median wealth. This provides

the first piece of evidence in favor of the liquidity constraints interpretation in a quasi-experimental setting.

The implication of this finding is that in most OECD economies there exists a subset of job losers who, with no or insufficiently generous unemployment insurance, have to accept a new job offer earlier than would be optimal. A possibly efficient way to improve their situation would be to lend them additional resources, as this policy response could come without the cost of increased moral hazard. Where such lending is not possible, for instance for political reasons, the choice of the optimal generosity of unemployment insurance must still weigh the effects of the liquidity constraints against those of potential moral hazard.

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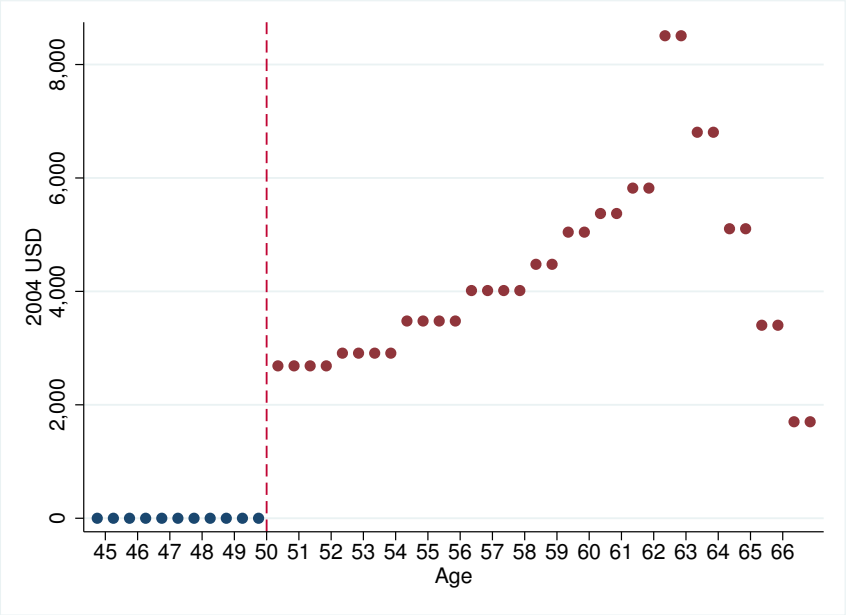
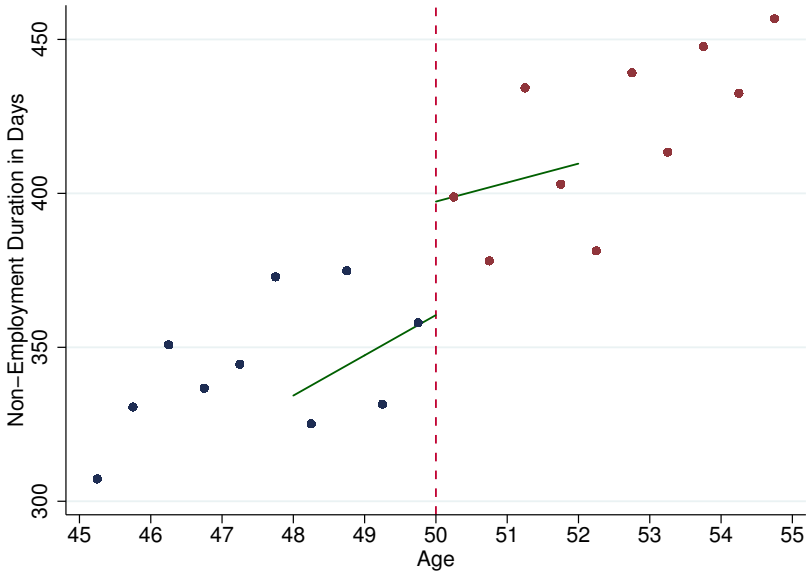
Figures and Tables

Figure 1: **Severance Pay Amounts in USD by Age**



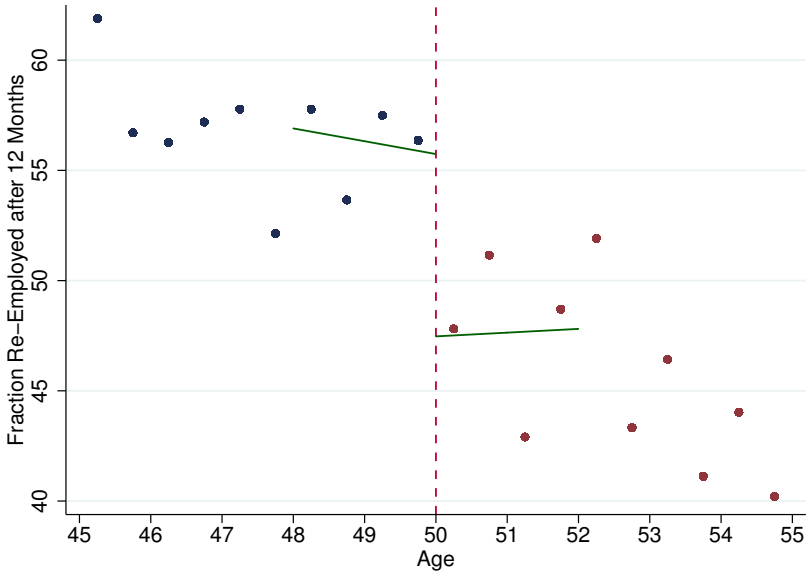
Note: The figure plots the aeverance pay amount that an eligible worker would have received if laid off between 2002 and 2009, for each 6-month bin of age. Amounts have been converted to USD at the average exchange rate prevalent in 2004.

Figure 2: **Non-Employment Duration After Job Loss**



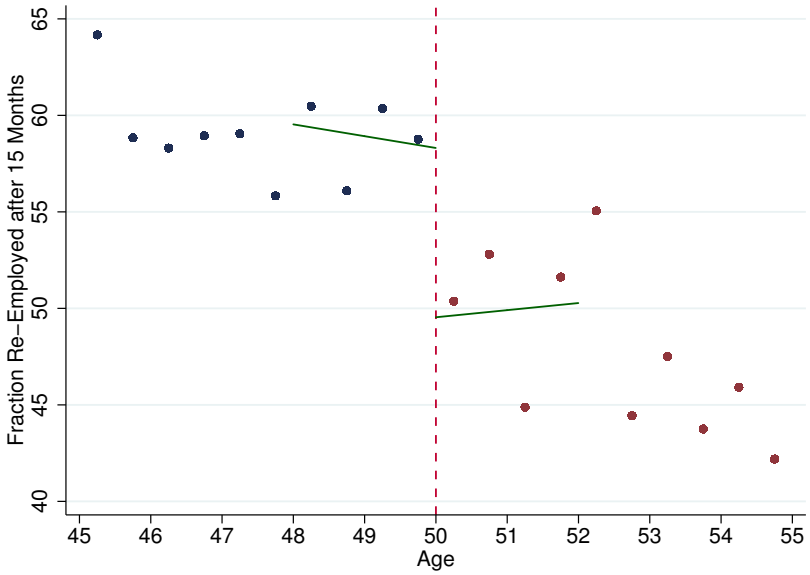
Note: The figure shows the average duration from job loss until the next regular job plotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 3: **Fraction Re-Employed 12 Months After Job Loss**



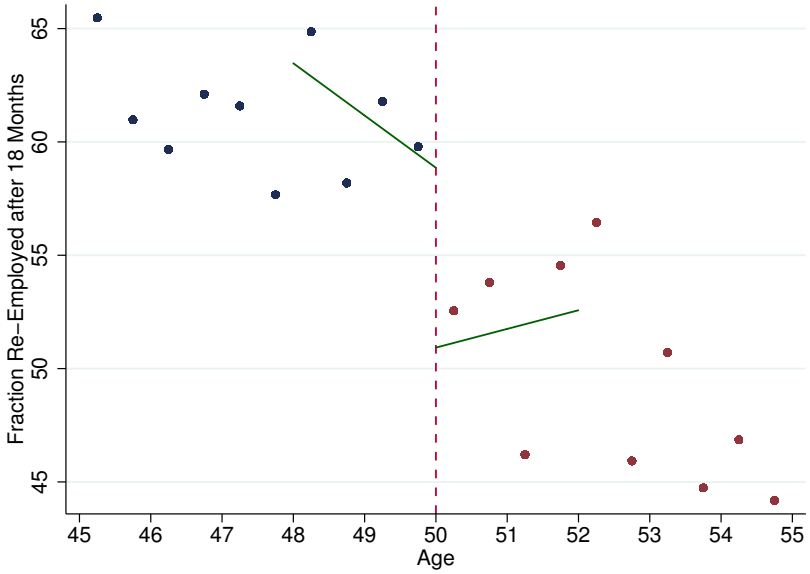
Note: The figure shows the fraction re-employed after 12 months, plotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 4: **Fraction Re-Employed 15 Months After Job Loss**



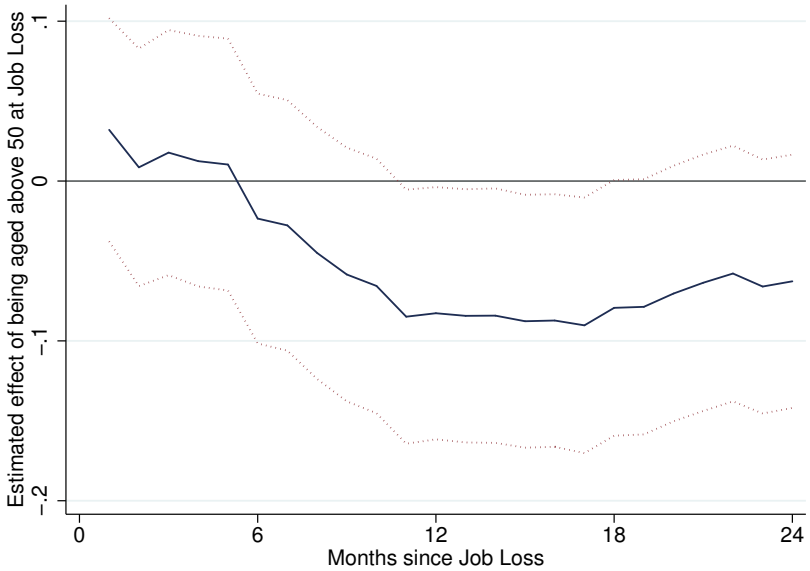
Note: The figure shows the fraction re-employed after 15 months plotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 5: **Fraction Re-Employed 18 Months After Job Loss**



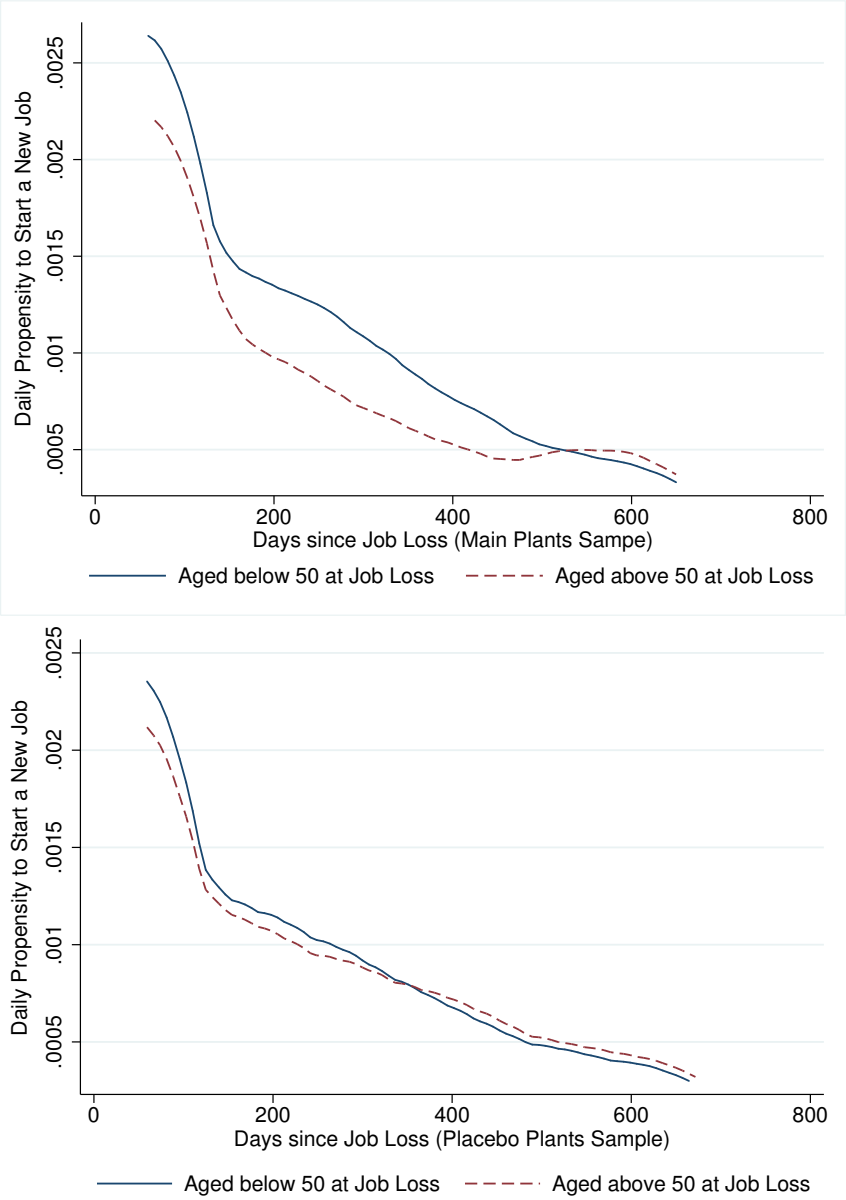
Note: The figure shows the fraction re-employed after 18 months, plotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 6: **Fraction Re-Employed 1-24 months**



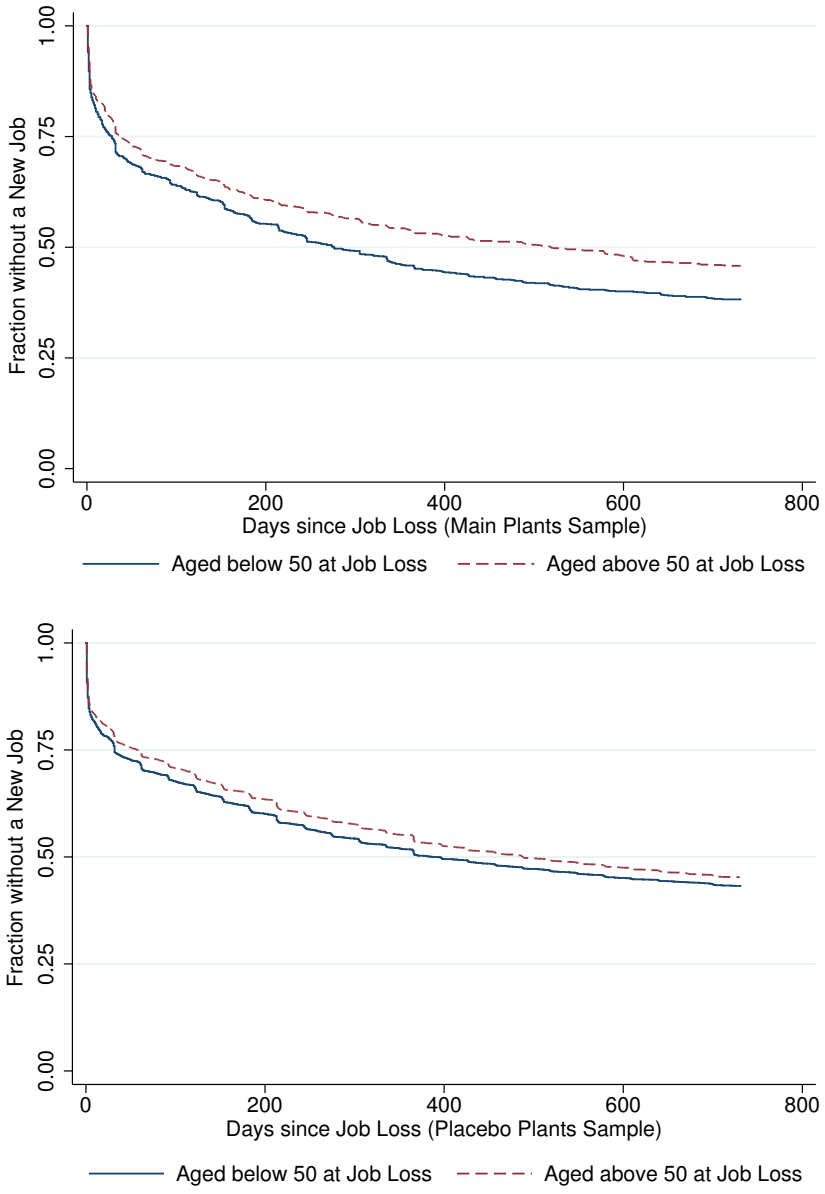
Note: The figure shows the effect estimates from 24 regressions (using baseline specification from Table 3, Panel A) when measuring the outcome variable as the fraction re-employed after each month from the first month up to 24 months after job loss. Stipled lines indicate the 95% point-wise confidence intervals.

Figure 7: **Hazard Rates in the Main and the Placebo sample**



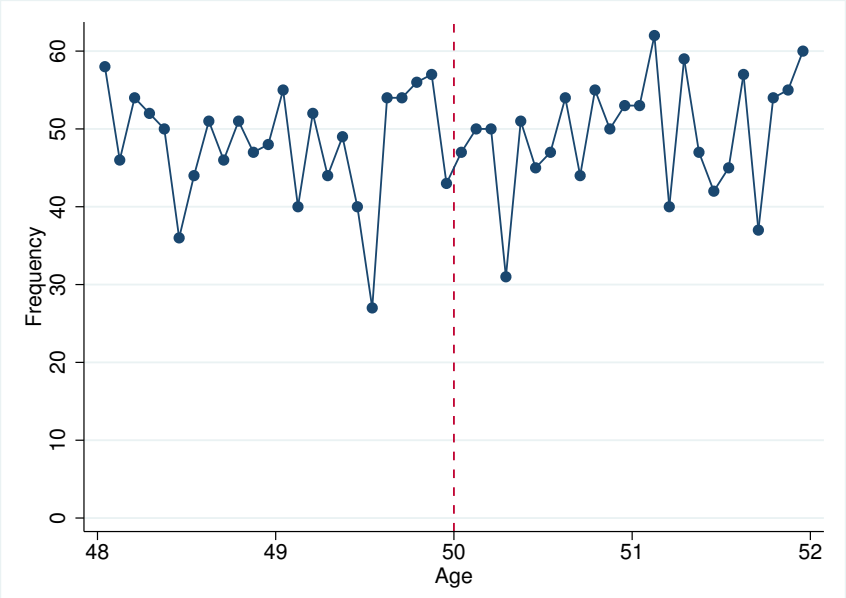
Note: The figure shows in the upper panel the hazard rates against the number of days elaped since job loss in our main sample. The hazard is almost always higher for those aged below 50 and hence not eligible for a severance payment at age 50. In the lower panel, the figure plots the hazard rates of finding new jobs for workers coming out of a job in the placebo firm sample.

Figure 8: **Survivor functions in the Main and the Placebo sample**



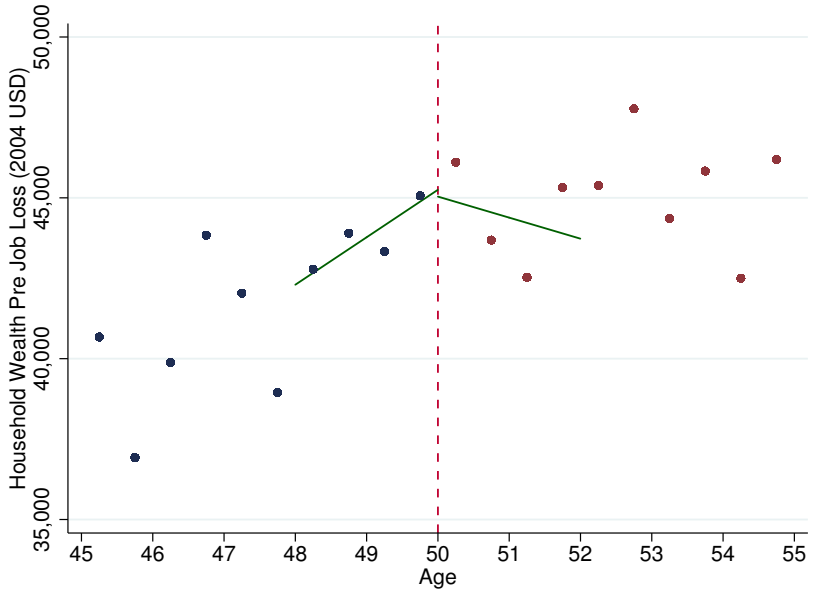
Note: The figure shows in the upper panel the survivor functions, the fraction without a new job, against the number of days elaped since job loss in our main sample split at the age of 50. In the lower panel, the figure plots the survivor function for workers (above and below the age of 50) coming out of a job in the placebo firm sample.

Figure 9: **Frequency of Job Separations**



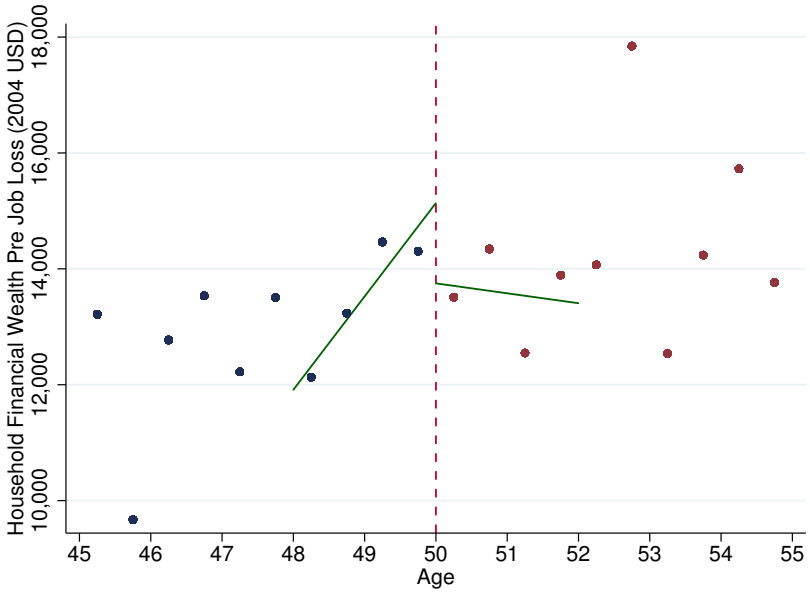
Note: The figure shows the frequencies of job separations around the threshold at age 50, using monthly bins. Corresponding to the visual impression, an estimation of the density of observations, following McCrary (2008), yields a coefficient of -0.129 and a standard error of 0.130, thus failing to reject the null hypothesis of no difference in densities.

Figure 10: **Household Total Wealth in the Year before Job Loss**



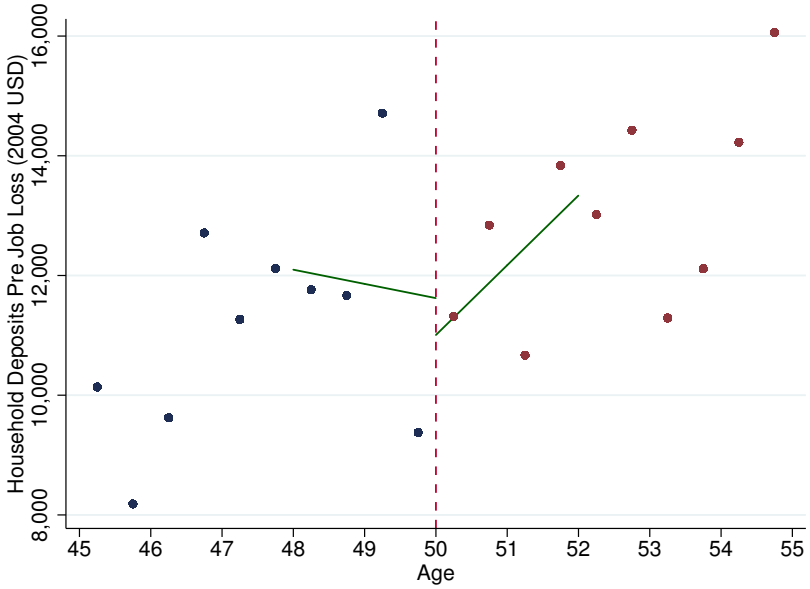
Note: The figure shows total household wealth (financial wealth plus real estate holdings), plotted against 6-month bins of age at job loss. Linear curves are fitted on each side for our default bandwidth of 2 years.

Figure 11: **Household Financial Wealth in the Year before Job Loss**



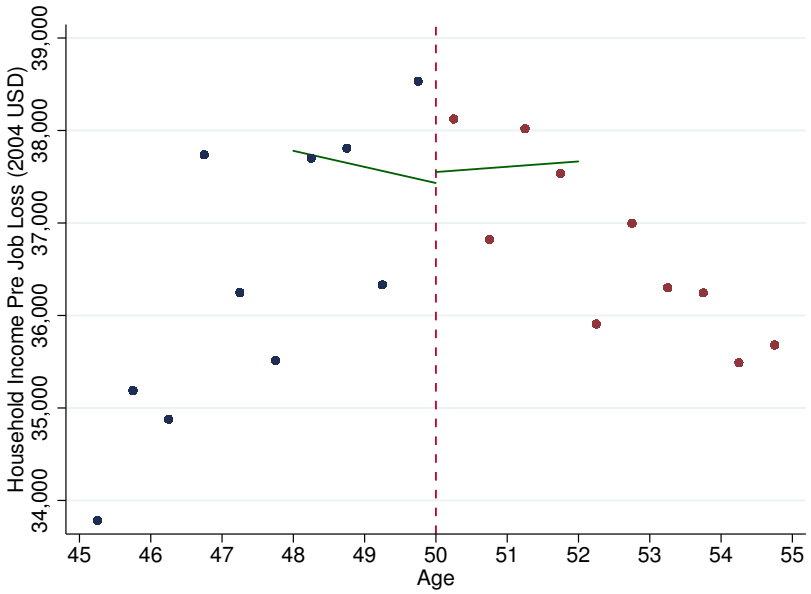
Note: The figure shows the households' financial wealth plotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 12: **Household Deposits in the Year before Job Loss**



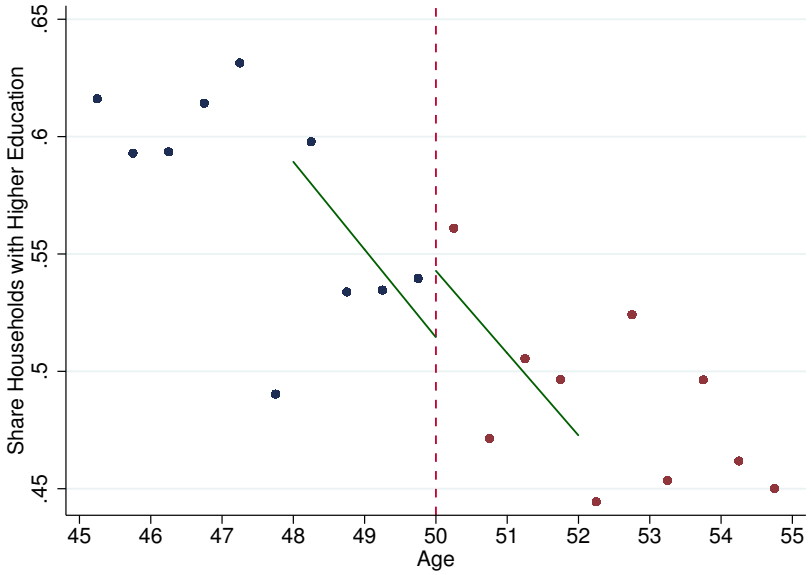
Note: The figure shows the households' deposits plotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 13: **Household Income in the Year before Job Loss**



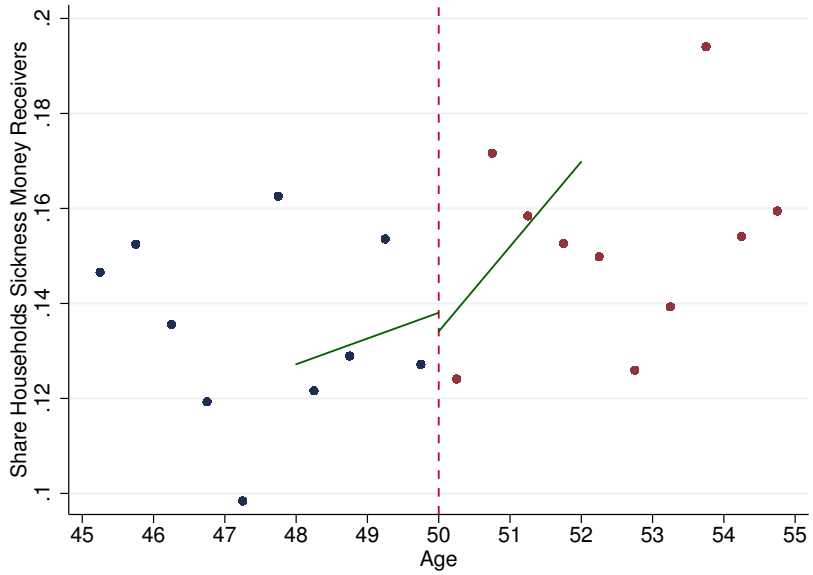
Note: The figure shows household income plotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 14: **Share of Households with Higher Education in the Year before Job Loss**



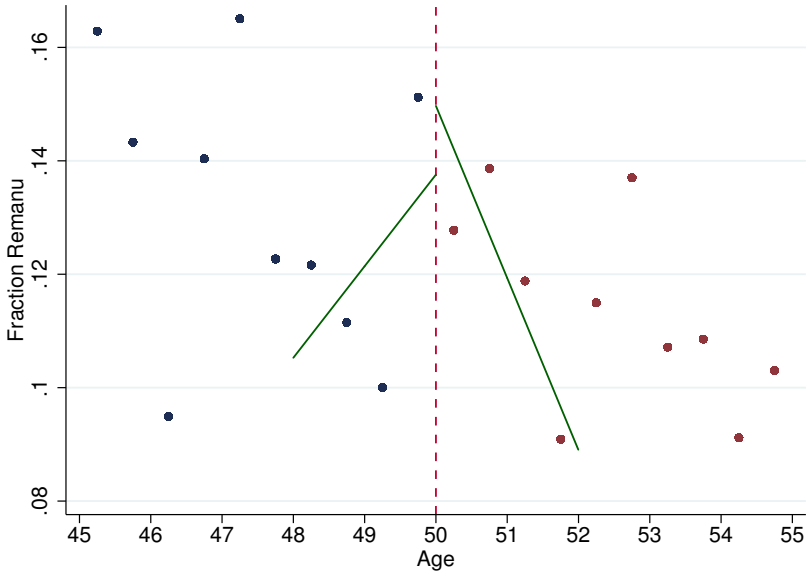
Note: The figure shows the share of households having a higher education, both plotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 15: **Share of Households receiving Sickness Money before Job Loss**



Note: The figure the fraction of households receiving sickness moneyplotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 16: **Share of Households Employed in Manufacturing before Job Loss**



Note: The figure shows the share of households employed in manufacturing plotted against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Table 1: Summary Statistics, Estimation And Placebo Samples, Age 48-52

	Estimation (N=2,342)			Placebo (N=9,340)		
	Mean	Std Dev	Median	Mean	Std Dev	Median
Year	2,003	3.82	2,003	2,003	3.94	2,003
Age	50.02	1.17	50.02	50.00	1.15	50.00
Tenure (in years)	15.979	5.41	14.16	16.02	5.48	14.54
Dur NonEmpl (in days)	293.28	494.82	72.50	337.01	554.47	112.00
Fraction Re-Employed After (in %):						
12 Months	51.90			48.54		
15 Months	54.34			51.87		
18 Months	56.39			54.08		
Education (in %)						
Less than Highschool	47.0			40.1		
High School	28.6			23.9		
College	24.4			36.0		
Education Main Field (in %)						
General	32.0			26.9		
Humanities	1.7			4.6		
Teaching	1.5			6.2		
Econ/Adm	9.8			12.7		
Science/Eng	45.9			33.7		
Health/Sports	1.1			4.4		
Primary	3.5			2.7		
Services	3.6			6.5		
Industry (in %)						
Manufacturing	24.1			27.3		
Construction	8.7			7.9		
Wholesale / Retail	14.8			19.8		
Transport / Communication	10.4			9.8		
Real estate	8.5			10.9		
Health / Social work	6.1			2.4		
Financial Variables (in 2004 USD):						
Annual Earnings	44,885	22,374	38,939	44,701	23,474	39,348
Monthly Earnings After Tax	2,618	1,305	2,271	2,608	1,369	2,295
HH Annual Earnings	60,011	29,599	55,499	60,752	31,480	55,538
Deposits	13,144	28,351	3,497	14,752	30,761	3,748
HH Deposits	17,952	34,957	5,864	19,872	36,505	6,724
Financial Wealth	32,982	94,430	4,919	32,542	82,272	6,058
HH Financial Wealth	41,310	107,126	8,464	40,916	95,201	10,639
Wealth	75,433	121,984	44,437	77,704	112,460	46,685
HH Wealth	92,093	138,035	57,644	95,131	129,129	59,347

Note: This table displays in the left panel summary statistics for the sample of 2,342 households, aged between 48 and 52 satisfying all the criteria described in Section 3. Additionally, summary statistics for the placebo sample of 9,340 households (satisfying all the same criteria except that the firm of separation was participating in the severance pay agreements) are displayed in the right panel. For the duration of non-employment, summary statistics are reported for households who have found jobs within the sample window (before 31 Dec 2008). Smaller Education Fields and Industries are omitted for spatial reasons. Financial variables and income are measured two years before the year of job separation and the values are denoted in 2004 USD.

Table 2: Severance Pay Amounts In NOK and USD By Age And Period

Age	Oct 1993-		Oct 1995-		Mar 1998-		Aug 2002-	
	NOK	USD	NOK	USD	NOK	USD	NOK	USD
$\leqslant$ 49	0	0	0	0	0	0	0	
50 - 51	12,000	1,791	14,400	2,149	14,400	2,149	18,000	2,687
52 - 53	13,000	1,940	15,600	2,328	15,600	2,328	19,500	2,910
54 - 55	15,500	2,313	18,600	2,776	18,600	2,776	23,300	3,478
56 - 57	18,000	2,687	21,500	3,209	21,500	3,209	26,900	4,015
58	20,000	2,985	24,000	3,582	24,000	3,582	30,000	4,478
59	22,500	3,358	27,000	4,030	27,000	4,030	33,800	5,045
60	24,000	3,582	28,800	4,299	28,800	4,299	36,000	5,373
61	26,000	3,881	31,200	4,657	31,200	4,657	39,000	5,821
62	28,500	4,254	34,200	5,104	57,000	8,507	57,000	8,507
63	28,500	4,254	34,200	5,104	45,600	6,806	45,600	6,806
64	34,200	5,104	34,200	5,104	34,200	5,104	34,200	5,104
65	22,800	3,403	22,800	3,403	22,800	3,403	22,800	3,403
66	11,400	1,701	11,400	1,701	11,400	1,701	11,400	1,701

Note: The table displays predicted Severance Pay in NOK (and USD) by age and period, according to the Severance Pay agreements between the Confederation of Norwegian Enterprise (NHO) and the Norwegian Confederation of Trade Unions (LO). For a plot of predicted amounts (in the last period) in 2004 USD, see Figure 1. 6.7 NOK = 1 USD (2004).

Table 3: Baseline Specification, Main Outcomes

	Completed Duration	Fraction Re-Employed After: 12 Months	15 Months	18 Months	Cox Regression
Panel A: Bandwidth = 2:					
T	36.87	-8.27**	-8.77**	-7.93*	-0.14
	(26.23)	(4.03)	(4.04)	(4.08)	(0.11)
z	13.07	-0.58	-0.61	-2.30	-0.07
	(15.66)	(2.43)	(2.44)	(2.39)	(0.06)
Tz	-6.91	0.75	0.98	3.13	0.05
	(37.45)	(3.48)	(3.47)	(3.45)	(0.09)
Constant	360.47***	55.74***	58.31***	58.86***	
	(18.75)	(2.91)	(2.90)	(2.88)	
N	2,342	2,342	2,342	2,342	2,342
Panel B: Bandwidth = 3 (Imbens and Kalyanaraman (2012) optimal):					
T	40.53*	-7.34**	-8.01**	-7.80**	-0.16*
	(21.83)	(3.33)	(3.35)	(3.38)	(0.09)
z	0.32	0.05	0.32	-0.13	-0.01
	(8.49)	(1.32)	(1.31)	(1.30)	(0.03)
Tz	8.37	-0.64	-0.95	-0.53	-0.02
	(12.40)	(1.91)	(1.91)	(1.90)	(0.05)
Constant	351.96***	55.90***	58.87***	60.41***	
	(15.84)	(2.45)	(2.43)	(2.41)	
N	3,540	3,540	3,540	3,540	3,540

Note: The table provides the regression discontinuity estimates based on Equation 1 and using our baseline bandwidth of 2 years on each side in the upper panel, and the bandwidth of 3 years (Imbens and Kalyanaraman (2012) optimal) in the lower. T is the indicator for being aged above 50 and hence eligible for severance pay, z is the age control (age-50) on the left side and Tz allows another age control on the right side of the threshold. Non-employment durations are censored when unemployment benefit expires after 2 years. Standard errors, clustered by firm, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect of Severance Pay on Next Job Quality

	New Job Duration	Fraction in New Job 2 years	Cox Regression	Change in log Salary
T	22.20	0.05	-0.17	-0.015
	(18.90)	(0.04)	(0.17)	(0.009)
z	-11.17	-0.02	0.08	0.004
	(10.38)	(0.03)	(0.10)	(0.006)
Tz	-8.37	-0.02	0.09	0.000
	(15.73)	(0.04)	(0.14)	(0.007)
Constant	612.54***	0.69***		-0.034***
	(13.03)	(0.03)		(0.007)
N	1,644	1,644	1,644	1,644

Note: The table provides the regression discontinuity estimates of Equation 1 using our baseline bandwidth of 2 years on each side, on the effect of next job quality. Column 1 shows the estimate of the effect on the duration in the new job censored after 2 years (we can only observe durations of new jobs until Dec 31 2012), column 2 on the fraction still in the new job after 2 years, column 3 the estimate from a Cox hazard model for the duration of the next job (censored after 2 years) and column 4 the effect on the change in log annual earnings in the year before job loss to the year after job start for those finding a new job within our sample window. Standard errors, clustered by firm, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Placebo Thresholds, Ages 47-51, Employment Fraction Outcomes

	T=47	T=47.5	T=48	T=48.5	T=49	T=49.5	**T=50**	T=50.5	T=51
Completed	-10.43	18.30	-28.68	3.67	-29.81	18.94	36.87	23.26	17.86
Duration	(26.37)	(24.60)	(25.47)	(26.63)	(26.65)	(26.39)	(26.23)	(26.62)	(26.23)
Fraction Re-Employed	2.49	-4.23	3.22	2.08	4.63	-1.49	-8.27**	-3.93	-2.51
After 12 Months	(4.12)	(3.84)	(3.97)	(4.15)	(4.13)	(4.13)	(4.03)	(4.13)	(3.99)
Fraction Re-Employed	2.32	-2.56	2.39	0.79	4.28	-1.49	-8.77**	-5.23	-1.58
After 15 Months	(4.09)	(3.76)	(3.98)	(4.07)	(4.09)	(4.09)	(4.04)	(4.11)	(4.02)
Fraction Re-Employed	1.87	-2.56	4.05	-1.19	2.29	-2.21	-7.93*	-5.87	-1.13
After 18 Months	(4.00)	(3.79)	(3.92)	(4.06)	(4.10)	(4.05)	(4.08)	(4.04)	(4.02)
Cox	0.04	-0.09	0.11	-0.05	0.11	-0.08	-0.14	-0.14	-0.06
Regression	(0.10)	(0.10)	(0.10)	(0.10)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
N	2,439	2,412	2,375	2,354	2,372	2,360	2,342	2,333	2,316

Note: The table provides the regression discontinuity estimates of Equation 1 around the true Threshold (T) at age 50, as well as around 8 placebo thresholds above and below 50. Above we go until age 51, because at 52 there is the next true discontinuity (see Table 2). The forcing variable z is defined as 'z = age - placebo threshold', and the baseline bandwidth is 2 years. Non-employment durations are censored when unemployment benefit expires after 2 years. Standard errors, clustered by firm, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Placebo Plants: Baseline Specification, Main Outcomes

	Completed	Fraction Re-Employed After:			Cox
	Duration	12 Months	15 Months	18 Months	Regression
T	-4.132	-0.138	-0.392	-0.282	0.009
	(13.026)	(2.040)	(2.031)	(2.051)	(0.054)
z	11.602	-0.893	-0.593	-0.669	-0.040
	(8.337)	(1.286)	(1.278)	(1.271)	(0.033)
Tz	-0.222	-1.257	-1.046	-0.748	0.007
	(11.514)	(1.791)	(1.785)	(1.776)	(0.047)
Constant	398.037***	49.213***	52.576***	54.579***	
	(12.074)	(1.766)	(1.747)	(1.721)	
N	9,340	9,340	9,340	9,340	9,340

Note: The table repeats the main regressions from Table 3 for our placebo sample of individuals separated from firms that were not affiliated with LO-NHO and hence did not participate in the severance pay agreements (see Section 2 for details). As before, we estimate Equation 1, using our baseline bandwidth of 2 years on each side. T is the indicator for being aged above 50 and hence "eligible" for severance pay, z is the control for (age-50) on the left side, and Tz allows for another age control on the right side of the threshold. Non-employment durations are censored when unemployment benefit expires after 2 years. Standard errors, clustered by firm, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Placebo Outcome Variables, Baseline Specification

	Income HH	Wealth HH	Fin Wealth HH	Deposits HH	Higher Edu.	Sickness Ben.	Manuf. Share
T	-532.75	-932.80	-1935.92	-1105.69	0.028	-0.004	0.012
	(1850.47)	(4569.67)	(2913.24)	(2964.75)	(0.043)	(0.028)	(0.027)
z	-279.85	2396.57	2590.12	-57.09	-0.037	0.005	0.016
	(1173.58)	(2570.76)	(1638.31)	(1589.54)	(0.025)	(0.018)	(0.017)
Tz	450.58	-3521.67	-3205.63	1743.68	0.002	0.012	-0.046**
	(1618.40)	(3738.64)	(2263.11)	(2537.19)	0.002	0.012	-0.046**
Constant	55206.09***	67137.54***	22798.11***	17523.99***	0.515***	0.138***	0.138***
	(1347.73)	(3227.38)	(2133.26)	(1951.60)	(0.031)	(0.021)	(0.021)
N	2,342	2,342	2,342	2,342	2,2172	2,342	2,342

Note: This table repeats the main regressions from Table 3 for a set of predetermined outcomes that should not exhibit discontinuities at age 50. These are annual income, total wealth, financial wealth and deposits, all at the household level, as well as an indicator for whether the household has completed high school or a higher degree, whether the household received sickness benefits or not and the share of household employed in manufacturing. Results for financial variables at the individual level or other education categories are not displayed, but do not show discontinuities either. As before, we estimate Equation 1, using our baseline bandwidth of 2 years on each side. T is the indicator for being aged above 50 and hence eligible for severance pay, z is the control for (age-50) on the left side, and Tz allows for another age control on the right side of the threshold. Standard errors, clustered by firm, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Alternative Optimal Bandwidths: Main Outcomes

	Completed	Fraction Re-Employed After		
Rectangular Kernel:	Duration	12 Months	15 Months	18 Months
Optimal Bandwidth	40.24*	-7.14**	-7.27**	-7.41**
	(20.95)	(3.17)	(3.15)	(3.15)
N	3,782	3,866	3,877	4,096
0.5*Opt Bw	37.55	-7.63*	-8.29*	-7.26
	(29.47)	(4.46)	(4.46)	(4.44)
N	1,851	1,911	1,915	2,009
Optimal Bandwidth	3.22	3.31	3.32	3.49
Triangular Kernel:				
Optimal Bandwidth	41.11**	-7.61**	-8.07***	-7.75***
	(20.29)	(3.06)	(3.06)	(3.01)
N	4,843	4,978	5,005	5,300
0.5* Opt Bw	35.73	-8.40**	-8.33*	-7.36*
	(28.29)	(4.28)	(4.27)	(4.22)
N	2,405	2,462	2,465	2,591
Optimal Bandwidth	4.10	4.21	4.23	4.45

Note: This table displays only the coefficients, and in parentheses the standard errors clustered by firm, on being aged above 50, now for different bandwidths and kernels. The top panel follows our main estimates in using a rectangular kernel, with equal weighting of observations. The bottom panel uses a triangular kernel, putting greater weight on observations closer to the threshold. Within each panel, we display first the estimates based on the Imbens and Kalyanaraman (2012) optimal bandwidth and then those based on half the optimal bandwidth. The respective optimal bandwidth itself is displayed at the bottom of each panel. Non-employment durations are censored when unemployment benefit expires after 2 years. Standard errors, clustered by firm, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Stratifying By Wealth Measures: Above Median (D)

		Fin Wealth	Deposits	Finw/Inc	Dep/Inc
Completed	T	97.59***	98.66***	67.95*	73.14**
Duration		(37.76)	(36.30)	(36.92)	(36.09)
	T*D	-118.48**	-120.41**	-63.91	-73.85
		(55.70)	(53.32)	(55.25)	(53.51)
Re-Employed After	T	-16.78***	-16.89***	-12.64**	-13.67**
12 Months:		(5.85)	(5.72)	(5.71)	(5.62)
	T*D	16.64*	16.86**	8.93	11.05
		(8.58)	(8.27)	(8.55)	(8.33)
Re-Employed After	T	-17.39***	-17.33***	-12.69**	-13.64**
15 Months:		(5.82)	(5.69)	(5.68)	(5.58)
	T*D	17.05**	16.89**	8.25	9.98
		(8.57)	(8.23)	(8.50)	(8.21)
Re-Employed After	T	-16.85***	-16.58***	-11.83**	-13.11**
18 Months:		(5.89)	(5.74)	(5.75)	(5.67)
	T*D	17.64**	17.10**	8.27	10.64
		(8.48)	(8.10)	(8.42)	(8.13)
Cox Regression	T	-0.39**	-0.38**	-0.24	-0.27*
		(0.16)	(0.15)	(0.15)	(0.15)
	T*D	0.48**	0.46**	0.22	0.26
		(0.22)	(0.22)	(0.22)	(0.22)
N		2,342	2,342	2,342	2,342

Note: This table provides the regression discontinuity estimates of Equation 1, augmented by an indicator variable for whether the value of different income and wealth measures (all deflated to 2004 values) exceeds the sample median in the year before job loss, as well as interactions between that indicator and the other regressors. For example, in the first column for *Re-Employed After 15 Months* we find that the average household below the median is 17.39% less likely to be re-employed after 15 months. For the above-median households, the effect estimate (the sum of the two coefficients T + T*D, -17.39% + 17.05%) is virtually zero, and not statistically significant at any conventional level, F(1,1552)=0.00, Prob>F=0.96. Non-employment durations are censored when unemployment benefit expires after 2 years. Standard errors, clustered by firm, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.