Cash-on-Hand and the Duration of Job Search: Quasi-Experimental Evidence from 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 seven percentage points. Wealth and demographic data enable us to verify that the effect is decreasing in prior wealth and absent for women. This favors an interpretation as liquidity constraints over one of mental accounting. Finding liquidity constraints in Norway, despite its equitable wealth distribution and generous welfare state, means they likely exist also in other countries.

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

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, or is some alternative mechanism at play? As a possible alternative we suggest *mental accounting*, whereby households do have enough resources of their own, or could borrow them from financial institutions, but after job loss are less willing to spend prior savings than to spend severance pay money.

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

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[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.

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 constraints, or the need for mental accounting, 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 6-7 percentage points, which corresponds to a relative reduction of about 12 percent. Thus, severance pay effects do not seem to be specific to countries with relatively short maximum UI durations.

Second, we investigate whether this effect does indeed reflect liquidity constraints, as put forward in Card et al. (2007a) and Chetty (2008). In particular, we discuss the alternative interpretation of mental accounting in the spirit of Shefrin and Thaler (1988). In this scenario, even households with enough other financial resources prolong their job search only if they receive severance payments, because they hesitate to tap into the other resources for the purpose of longer job search. We can discriminate between the two scenarios by investigating whether the severance pay effect is decreasing in prior wealth, if mental accounting is thought to be invariant to prior wealth. As we show, this assumption is indeed supported by the data. Thus 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. While the effect does not vary significantly with total wealth, which includes the house, it is found to be decreasing in both measures of liquid wealth, financial wealth and deposits. As another test, we investigate the effect separately for females, whose husbands will typically earn higher incomes that they can tap into to resolve potential liquidity constraints during unemployment. No significant severance pay effect is found for that sample. 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 statistically identical along the relevant dimensions. Furthermore, the mechanism of the pay-outs, which are made by a joint fund financed by firms in a not experience-rated way, ensures that, as we verify in the data, there is no selective lay-off 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 addresses theoretically and empirically the possibility of mental accounting behavior. Section 6 concludes.

2   Empirical Strategy

The challenge in identifying the causal effect of severance payments in most empirical setups is that eligibility or amounts typically 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.

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. 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, 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 the 15 years for which we have data, 1995-2010, the assigned amount of severance pay varied along three dimensions: By job tenure, by age, and across 4 periods. Firstly individuals were required to have at least 10 years of tenure in their current plant or at least 15 years of tenure in a combination of participating plants. In our data we observe any job start date after 1992. Therefore we know exact tenure for those who started their last job in or after 1992. By contrast for someone who started his last job in, say, 1990 and quit in 1998, we will only know that he must have started before 1992 and hence have at least 6 years of tenure, but we do then not know whether or not his tenure does also exceed the 10 years required for severance pay eligibility. Therefore we are not able to exploit tenure as a RDD assignment variable, and we restrict our sample to those *known* to have had at least 10 years of tenure, so that everyone in our sample did satisfy the tenure requirement for severance pay.

The second dimension and the one we exploit is age. As Figure 1 shows, severance pay amounts increased from zero to NOK 18,000 at age 50.[4] This provides a setup for RDD analysis. There are also further increases at ages

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

[4]At the 2004 exchange rate of 6.7 NOK per USD, this corresponds to about $2,700.

52, 54, 56, 58, 59 and 60, as well as annual decreases after age 60. However the other increases until and including the one at age 59 are rather small, and at and above 60 other simultaneous discontinuities apply, in particular in access to early retirement, thus violating the exclusion restriction required for identification. Therefore we focus on the discontinuity at age 50. 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.

Finally, within our period of observation the precise amount paid out at age 50 was adjusted twice. It amounted to NOK 12,000 until September 1995, NOK 14,400 until July 2002, and NOK 18,000 thereafter. Most of our observations come from the last period, and so the average amount individuals in our sample were eligible for if aged between 50 and 52 was NOK 16,924 or $2,500 at 2004 exchange rates.[5] It is worth noting that these amounts do not depend on prior earnings, so we may expect the same amount to have a larger effect on those with lower previous incomes than on those with higher incomes. Median monthly earnings after taxes (the relevant point of reference, since severance payments are not being taxed) amounted to $ 2,158 (see Table 1), so the payments amounted to about 1.2 monthly after-tax incomes for the median earner. It would thus have allowed him to "top up" from the 62% UI replacement rate to 100% of his former income for about 3 months, and top up to lower replacement rates correspondingly longer.

For those aged between 48 and 52 and known to have had 10 or more years of tenure, we estimate the following equation for different outcome measures y:

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

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

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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,[6] and we allow the effect of age to differ on the two sides of the discontinuity. The specification 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 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.[7]

3   Data

We use administrative data from the *FD-Trygd* events database of Statistics Norway, covering the universe of Norwegian residents. We start with information on all job separations by male employees occurring between 1995 and 2010.[8] We then merge in information obtained from the LO-NHO office on which plants were participating in the agreement and restrict to those that were.[9] Furthermore, we add information from *FD-trygd* on exact age at the

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

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

[8]We focus on males as even in Norway females earn significantly less than their husbands and they typically work part time.

[9]General employment information is available from 1992 onward, but it is only from 1995 onward that we know plant identifiers.

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 (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 start a new job just the day after the separation or 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.

Since severance pay eligibility requires at least 10 years of plant tenure, we restrict the sample accordingly. We drop individuals who started their last job before 1992 (for whom we cannot observe the exact start date) *and* who are separated from it before 2002 since we are unable to know whether their full tenure was above or below 10 years. 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 claim forms are sent to the LO-NHO office by

the employer together with the layoff notification, we can expect compliance to be rather high, and so our ITT estimates are expected to be not much below the corresponding Wald 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.[10]

Our first and most natural outcome measure then 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 December 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. However, for someone who has not returned to work after 18 months we do not know whether his complete non-employment duration is 19 months or 24 or 40, yet we *do* know that he was not back in work after 12 months. This suggests as sensible outcome variables the fractions re-employed after respectively 12, 15, and 18 months.[11]

Taking this idea further, we also 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 so far. This allows

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[10]An additional reason in our case is that, as becomes clear from Bratsberg et al. (2010) and Kostøl and Mogstad (2012), 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 analyses excluding any household ever receiving disability pension in our observation window, our main results remain unchanged.

[11]We have also looked at shorter and longer horizons. Effects there go in the same direction, but tend to be smaller. Likely this is the case because at shorter horizons constraints are not yet binding, whereas at longer horizons only a smaller and more selected sample of individuals are still without a job.

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.[12] Given that we find the largest effect of severance pay on the fractions re-employed after 15 and 18 months, we censor the Cox regression for non-employment spells at 15 months. The point estimate we get when censoring after 18 is very similar, and for censoring after 12 or 24 months slightly lower in absolute values.

A final data issue to be discussed is wealth. The exisiting 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. Therefore 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 plants 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-employment duration among those for whom the next job start is observed in the sample (corresponding figure for the placebo sample in parentheses)

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

is about 9 (10.5) months at the mean and 2 (3) at the median. About 40 (46) percent have less than high-school education, 25 (30) percent have a high school degree, and 35 (23) percent have a college degree. Average annual income before taxes is about US$ 43,000 and household financial wealth about US$ 40,000 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 wider bandwidth has significantly smaller standard errors due to the larger sample size. For both panels, we use a simple rectangular kernel, assigning each observation the same weight, which can be implemented by estimating Equation 1 by Ordinary Least Squares. 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 – more robust to job returns not or not yet observed – the fractions re-employed after respectively 12, 15 and 18. Since we find the largest effect after 15 and 18 months, column 5 finally uses as outcome the logarithm of the propensity to start a new job on any given day within the first 15 months after job loss.[13]

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

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[13]When censoring after 12, 18 or 24 months the point estimate is between 0 and 6 percentage points lower in absolute terms, and slightly less significant.

duration by between 45 and 57 days. In line with this, amongst those eligible the fraction re-employed after 12 months is found to be between 6 and 7 percentage points lower, that re-employed after 15 months 8 percentage points lower, and that re-employed after 18 months between 7 and 8 percentage points lower. The same effects can also be seen visually in Figures 2 through 5, which plot respectively 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. Effects for shorter and longer horizons, not displayed, go in the same direction, but are smaller. Thus the relationship between elapsed non-employment duration and the size of the severance pay effect is inversely U-shaped, peaking after about 15 months.

This finding is also reflected in the upper panel of Figure 6: It plots the hazard rate, i.e. the propensity to start a new job on a given day, against the days elapsed since job loss, separately for those with and those 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. Firstly, 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. Secondly, the line for those eligible is almost always below that for those 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 smaller initial size of the effect may be seen to reflect that in their initial months of unemployment almost all households can and do still draw on prior savings[14], as well as several months of delay between job loss and payment of the sever-

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[14]Indeed, Basten et al. (2012) show that Norwegian households do some, albeit limited, additional saving before and dis-saving after job loss.

ance payment.[15] At durations of and above 1.5 years, by contrast, most of the severance pay will have been spent, and furthermore the composition of those still out of work has changed.

The effect of severance pay on the job starting hazard, as visualized in Figure 6, 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 (after which we censor, since afterwards not everyone is still eligible for unemployment benefits). 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 – an 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 17-18 percent the job finding propensities displayed in Figure 6. The shortcomings in the fit of the Cox model are reflected in the somewhat larger standard errors at bandwidth 2 (upper panel), which do however shrink when when we use the Imbens-Kalyanaraman Optimal Bandwidth of 3 (bottom panel).

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 on average 7 percentage points, corresponding to a relative decline of about 12%, as the average fraction reemployed after 12 to 18 months is about 0.6 (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

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[15]See http://www.sluttvederlag.no/

other percentage above the UI replacement rate), then any given severance pay amount will "last longer" the greater the fraction already covered by UI.[16]

4.2 Sensitivity Checks

The first possible concern that may arise about the credibility of our 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 4 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 at 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.

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 60 and 62, 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 plants 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 5, as well as in the bottom panel of Figure 6. Indeed, no significant effect of being aged above 50 is found here, supporting the view that the exclusion restriction is indeed satisfied.

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[16]By the Paradigm of Revealed Preferences, the fact that households choose to use some of the severance pay money for longer search durations implies that the availability of the payment makes them better off. To see if the severance pay results in a better subsequent job, we have followed Card et al. (2007a) and performed the analysis on wage growth from previous to new job. Like them, however, we find no significant effects. Unfortunately, we are not able to analyze duration on the next job (a common measure of non-pecuniary job satisfaction) as most of the subsequent jobs have only just started by the end of our panel.

As in any Regression Discontinuity Design, we need to explore whether there could have been selection around the threshold. As mentioned above, severance payments 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 -0.018, with a standard error of 0.134, so we fail to reject the null hypothesis of no difference. In line with this, we see no discontinuity at 50 in Figure 7, 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 6 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 8 through 14, lending further support to the view that our main findings can be given a causal interpretation.

Another concern that always arises in a Regression Discontinuity Design 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 choice yields a relatively narrow range, and correspondingly limited precision, compared to previous papers in the literature. Card et al. (2007a), for instance, choose a bandwidth of 3 years per side. This said, Table 7 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 somewhat more significant. In general, they confirm our main results.

5 Liquidity Constraints vs. Mental Accounting

5.1 Mental Accounting as an alternative interpretation

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, or whether it could reflect another mechanism. In particular, we suggest that conceivably

households who could financially afford longer search durations also absent the severance payments would nonetheless be unwilling to do so (and hence respond to severance payments) because they have "earmarked" their savings for other purposes.[17]

Such behavior could be interpreted as an instance of mental accounting in the spirit of Shefrin and Thaler (1988). There individuals behave *as if* there coexisted two selves: A myopic "doer self" concerned only with the current period, and a "planner self" concerned with maximizing a function of lifetime doer utilities. If the choices of consumption each period were left to the "doer self", too much would be consumed in early periods, leading to a sub-optimal lifetime path of consumption. Restricting current consumption to a level below what is available in any given period however costs willpower. To address this problem, the "planner self" is then assumed to place constraints on future consumption choices already in advance, either through external commitment devices like pension plans or internal ones like rules-of-thumb. One such rule is mental accounting: Rather than considering all money as fungible, households mentally assign all funds to different "Mental Accounts". The simplest version contains one account for "Current Income" (C), one for "Current Assets" (A) and one for "Future Income" (F). The rule-of-thumb then has the marginal propensity to consume (MPC) – the fraction of each additional dollar consumed right away – be highest for money classified as "Current Income", lower for "Assets", and lowest for "Future Income".[18] In the words of Shefrin and Thaler (1988), "households treat components of their wealth as non-fungible, even in the absence of credit rationing" (p. 609).

There are important parallels between mental accounting and standard

[17]Furthermore, Basten et al. (2012) find that some Norwegian households do indeed prepare for unemployment by increasing their savings rate in the years before job loss, and do tap into some, but by no means all of their savings in the years after job loss.

[18]In practice, households are likely to have more than just those three accounts, and different households will have different accounts. Furthermore, exactly which consumption choices this classification results in will depend on the exact "framing", i.e. on which categories each account is defined to include and over which horizon each account is to be balanced. This categorization into three main accounts however is thought to be a good first approximation for the average household.

liquidity constraints. In both cases households would have the necessary (lifetime) wealth to increase spending now, yet cannot do so because the wealth is not available at that specific point in time or for that specific purpose. The difference is first that mental accounting arises through constraints that are internal rather than external, and second that – given the individual's temptation to spend excessively absent any commitment devices – the internal constraints can be optimal as a second-best solution. Such mental accounting could be relevant also in the present context of job loss and severance payments, because such payments, received when households lose their jobs and see regular income drop, would likely be classified as "Current Income" and thus attract a higher marginal propensity to consume than prior savings.

5.2 Empirical Evidence

So if the severance pay effect identified above could also reflect mental accounting rather than liquidity constraints, it is worthwhile to investigate which interpretation finds greater support in the data. To do so, we first make use of our information on prior wealth. Clearly, if the correct interpretation is one of liquidity constraints, then the same 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. The relationship between mental accounting and prior wealth is less clear: On the one hand one might think that those who do mental accounting have best avoided wasteful spending and will thus be richest on the day of job loss. In this case mental accounting would show up as a larger severance pay effect amongst the rich than amongst the poor, the opposite of what we should see under liquidity constraints. On the other hand, one might reason that only those who are in general tempted to overspend use mental accounting to discipline themselves, whereas the others do always behave rationally. In this case we should expect the less rational group with fewer savings on the day of job loss to do more mental accounting, and if the severance pay effect did reflect mental accounting it should be decreasing in prior wealth. To see whether this is a plausible

scenario, Table 8 investigates first whether the severance pay effect varies with whether or not individuals have higher education, the best available proxy for whether they would be in the "rational" or "irrational" group. We find that education has no bearin on the severance pay effect, neither when we interact only with education, nor when we interact in addition with any of our wealth measures. This suggests that an interaction of severance pay eligibility with our different wealth measures can provide not conclusive, but very useful evidence for a discrimination between liquidity constraints and mental accounting.

Table 9 provide that evidence, interacting severance pay eligibility with an indicator for whether wealth exceeded the sample median. We use both all financial wealth, including stocks, mutual fund shares, bonds and deposits, and deposits only. Furthermore, we use both the absolute amounts of wealth, and wealth relative to annual income in the last year before job loss. Results with the scaled measures are slightly more noisy than with the unscaled ones, but across all measures we see that the interaction term goes in the opposite direction of the main effect and does pretty much cancel out the main effect, implying that on average no severance pay effect exists for those with above-median wealth. Our main results, estimated for the sample as a whole, are thus the average of very strong effects for those with below-median wealth and zero effects for those with above-median wealth. Given our above results that the size of the effect is not decreasing in education, this provides first evidence in favor of a liquidity constraints interpretation of the severance pay effect.

Table 10 shows the result of repeating our main regression for the sub-sample of married females. Since they typically earn lower incomes than their husbands and could thus more easily tap into their total household income resolve liquidity constraints, they are a group which we would expect to be less liquidity constrained than those in our main sample. By contrast, we are not aware of any evidence whereby females would do less mental accounting than males. Our results in Table 10 now find no significant evidence of a severance pay effect for females. They can thus be taken as a second piece of evidence

in favor of a liquidity constraints interpretation.

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, or whether it could reflect another mechanism. In particular, it is conceivable that households who could financially afford longer search durations also absent the severance payments would nonetheless be unwilling to do so (and hence respond to severance payments) because they have "earmarked" their savings for other purposes. We have therefore proceeded to discuss whether the severance pay effect should indeed be interpreted as evidence of liquidity constraints, as in the previous literature, or alternatively as evidence of mental accounting behavior. To discriminate empirically between the two scenarios, we have investigated how the size of the severance pay effect varies with prior wealth and find it to be decreasing therein. Under the assumption that mental accounting, if any, would not vary with prior wealth, and which is supported by the data, this provides not conclusive, but useful evidence in favor of an interpretation as liquidity constraints. At the same time, we find that the severance pay effect exists for males, who typically contribute the larger share of household income, but not for females, who a priori can more easily tap into their husband's income to resolve any liquidity constraints. This provides a second piece of evidence in favor of the liquidity constraints interpretation.

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. An efficient way to improve their situation would be to lend them additional resources, as this policy response would not come at 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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___ **and T. Lemieux**, "Regression Discontinuity Designs: A Guide to Practice," *Journal of Econometrics*, 2008, *142*, 615–635.

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McCrary, J., "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test," *Journal of Econometrics*, 2008.

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Figures and Tables

Figure 1: **Severance Pay Amounts in USD by Age**
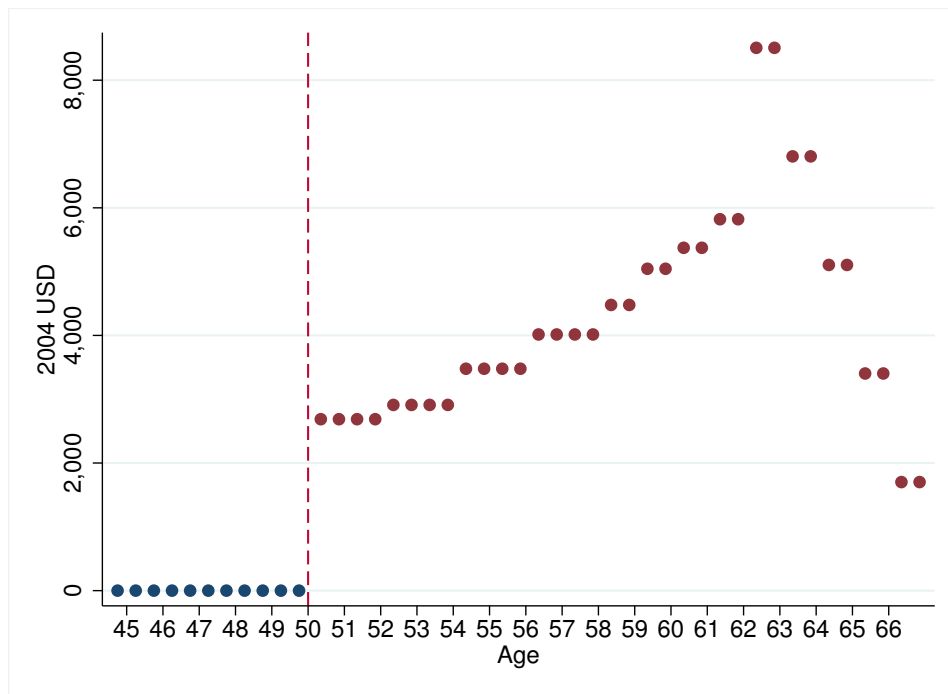
Note: The figure plots the Severance Pay Amount an eligible worker would have received if laid off between 2002 and 2009, for each 6-month bin of age. Amounts have been converted to USD at the average exchange rate prevalent in 2004.

Figure 2: **Non-Employment Duration After Job Loss**
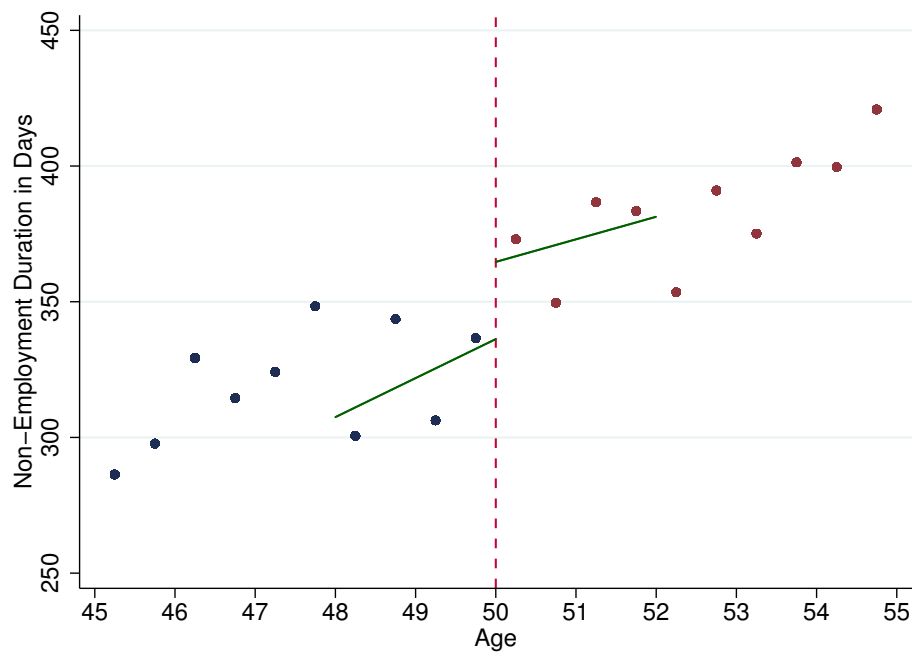
Note: The figure plots the average duration from job loss until the next regular job against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 3: **Fraction Re-Employed 12 Months After Job Loss**
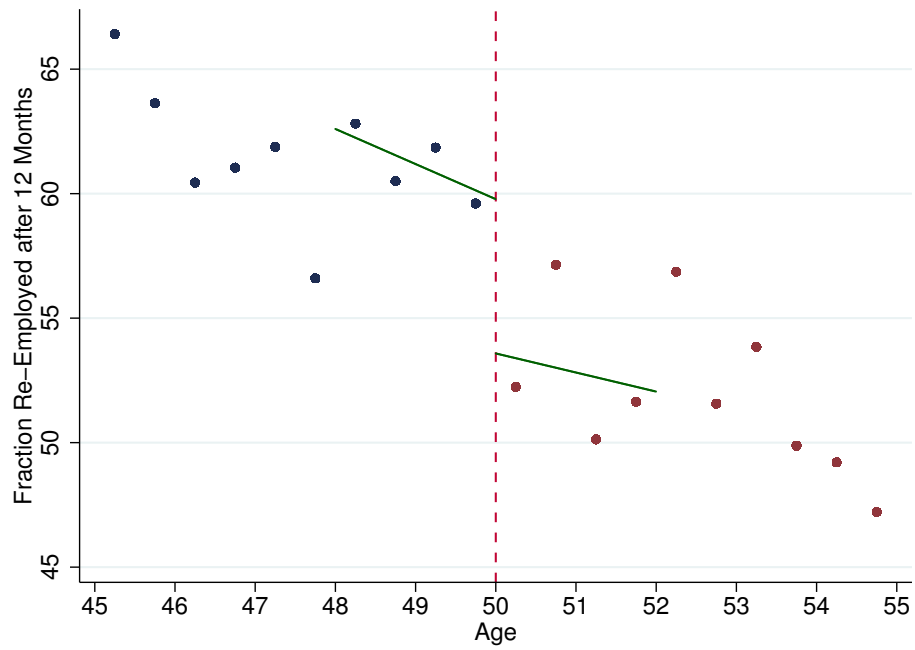
Note: The figure plots the fraction re-employed after 12 months against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 4: **Fraction Re-Employed 15 Months After Job Loss**
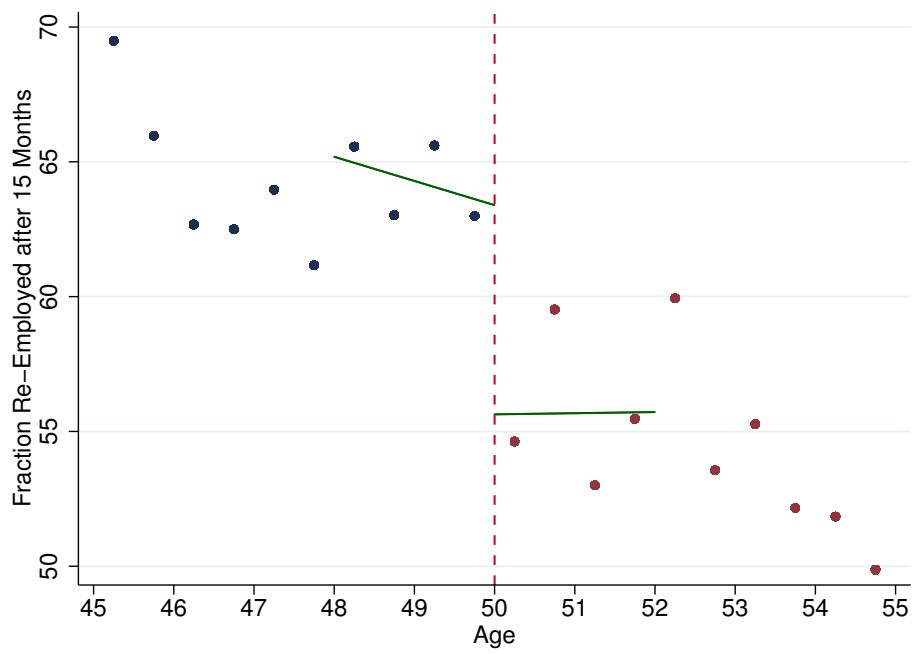
Note: The figure plots the fraction re-employed after 15 months against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

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

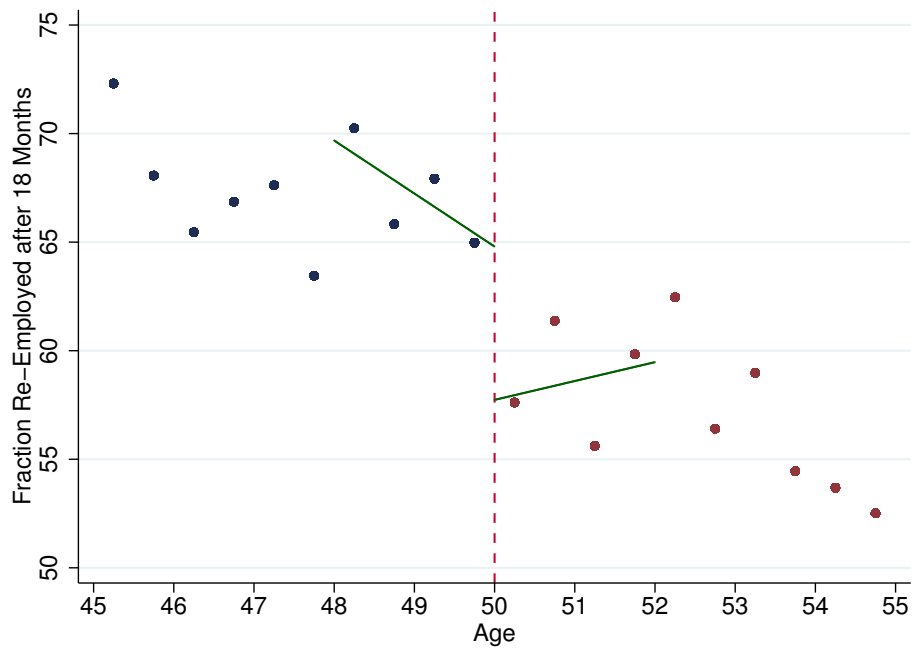*Note*: The figure plots the fraction re-employed after 18 months against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 6: **Hazard Rates in Main and Placebo sample**
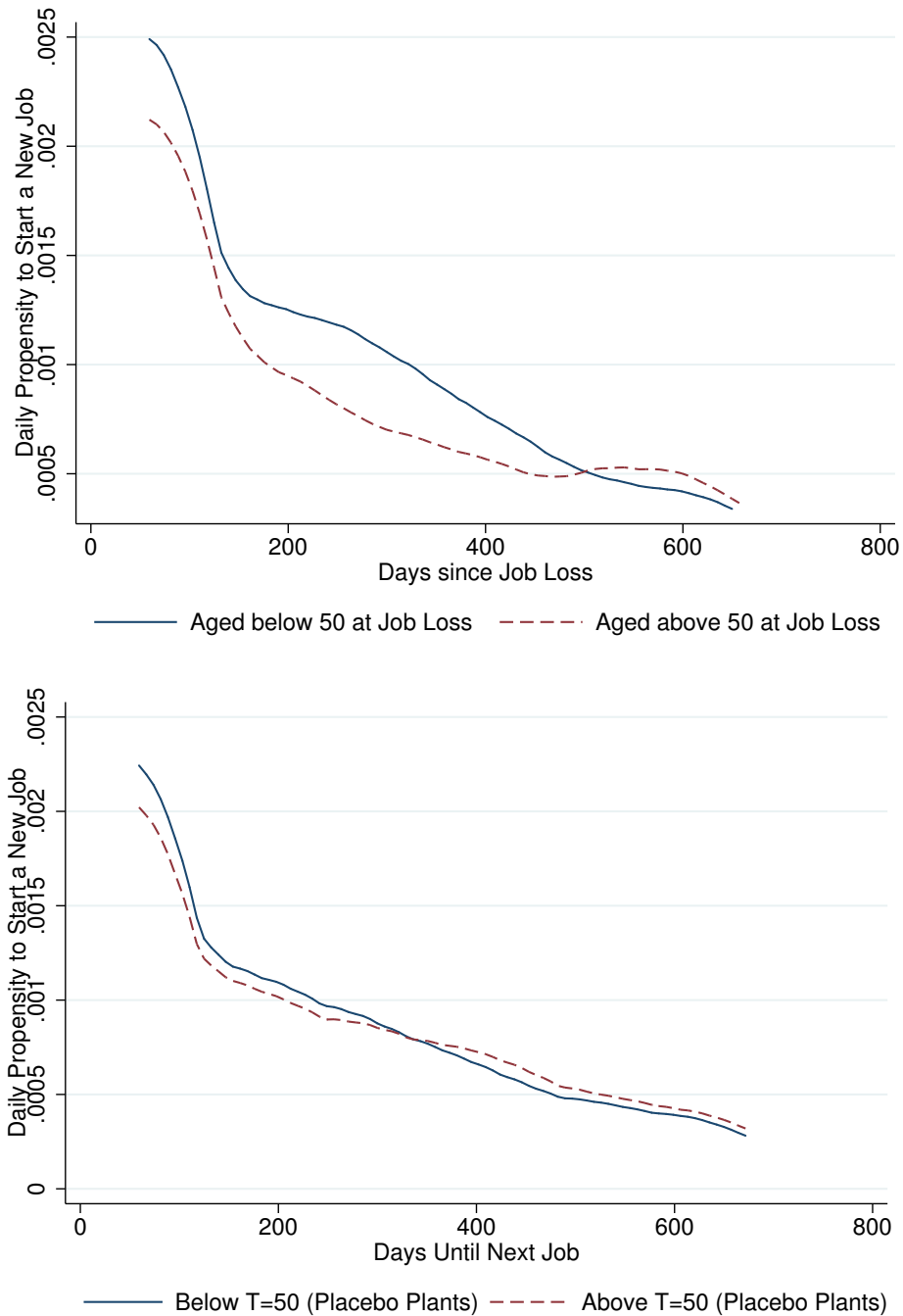
Note: In the upper part, we plot the hazard rates, i.e. the daily propensity to start a new job, against the number of days elaped since job loss in our main sample. That hazard is almost always higher for those aged below 50 and hence not eligible for a severance payment at age 50. The difference in hazards is biggest after about a year, suggesting that then the effect of the payments is strongest. In the lower part we plot the hazard rates of finding new jobs for workers coming out of a job in the placebo plant sample.

Figure 7: **Frequency of Job Separations**
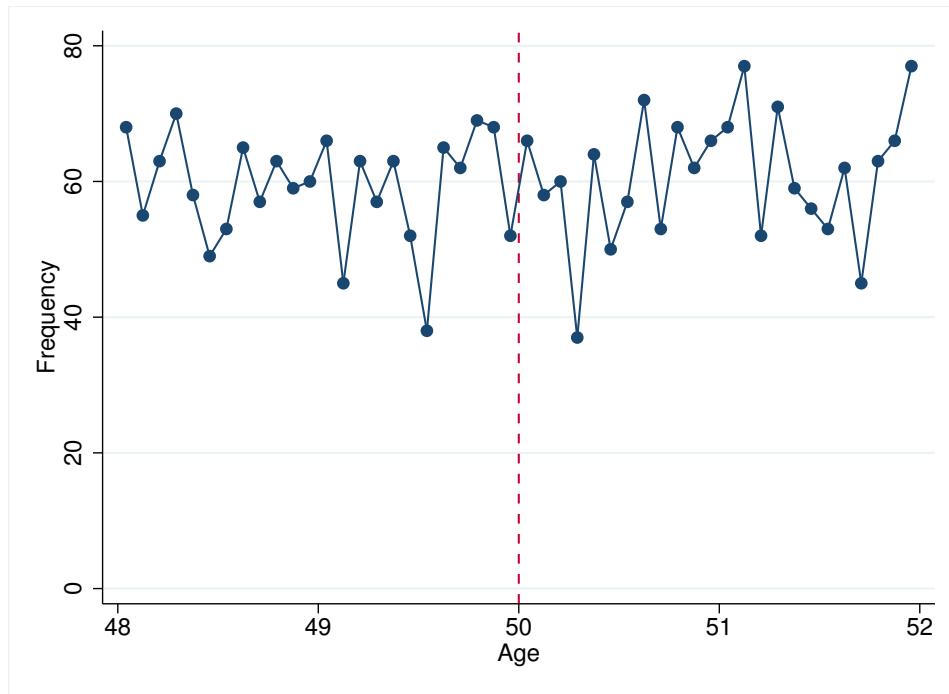
Note: Frequency plots of Job Separations around the threshold at age 50. Monthly bins. Corresponding to the visual impression, an estimation of the density of observations, following McCrary (2008), yields a coefficient of -0.018 and a standard error of 0.134, thus failing to reject the null hypothesis of no difference in densities.

Figure 8: **Household Wealth in the Year before Job Loss**
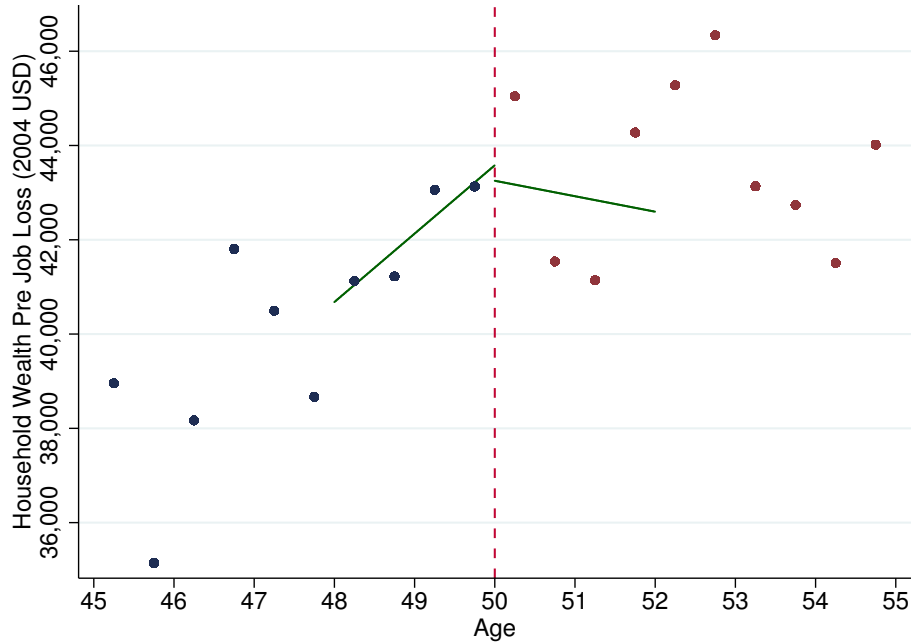
Figure 9: **Household Financial Wealth in the Year before Job Loss**
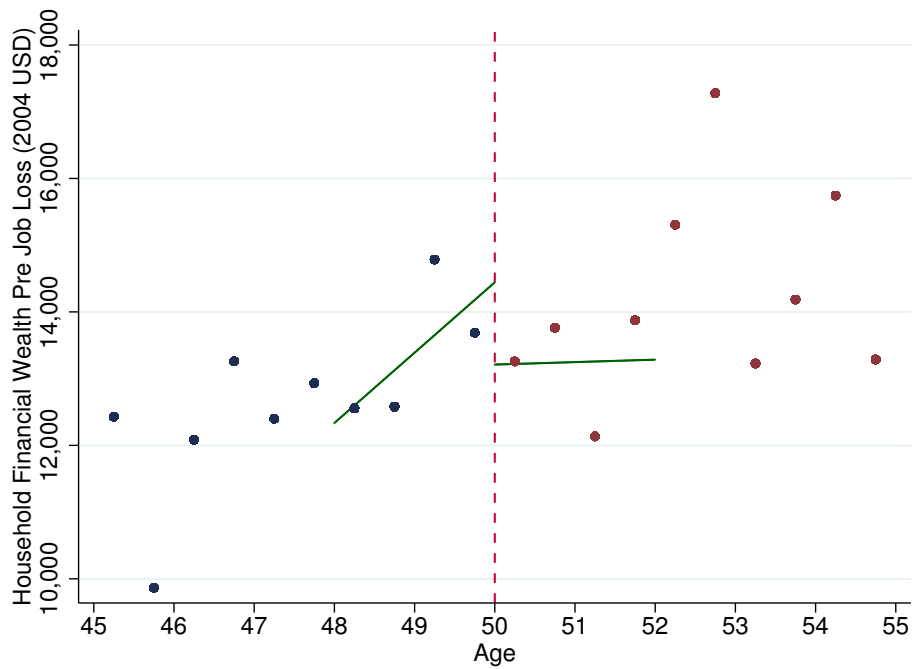
Note: The figures plot respectively households' total wealth (upper) and financial wealth (lower) against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 10: **Household Deposits in the Year before Job Loss**
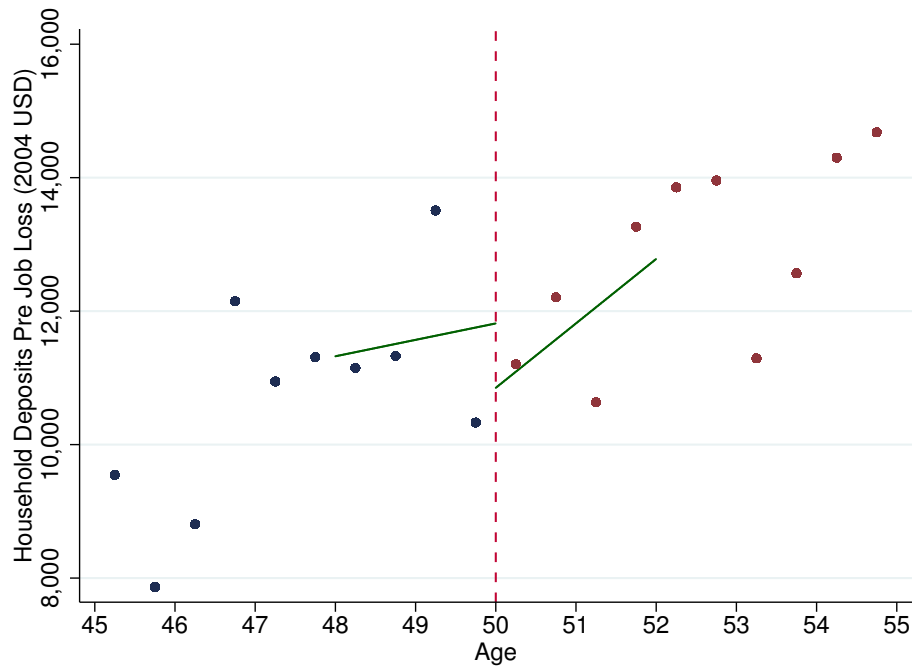
Figure 11: **Household Income in the Year before Job Loss**
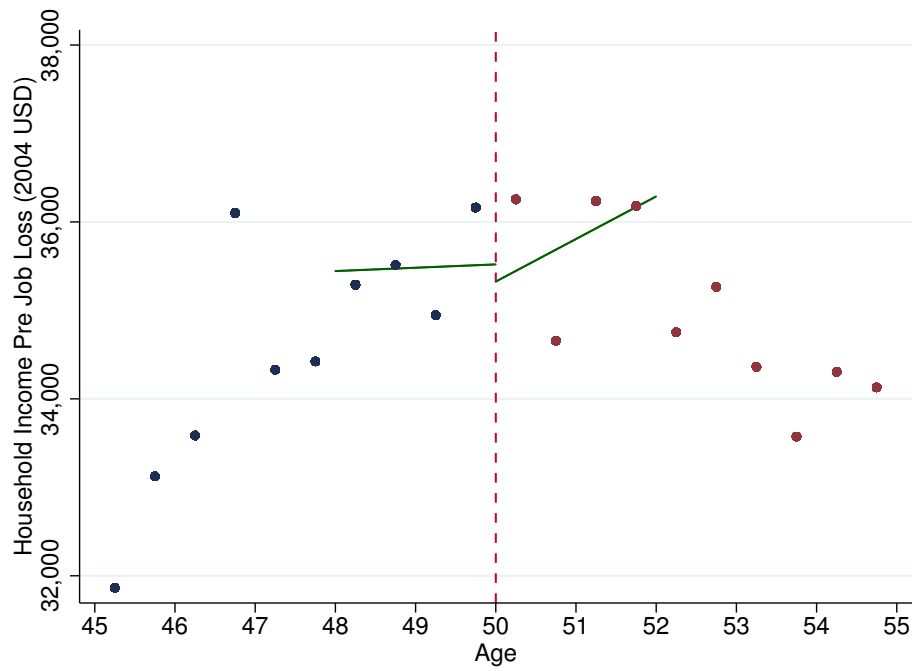
Note: The figures plot respectively households' deposits (upper) and income (lower) against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

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Figure 12: **Share of Households with Higher Education**
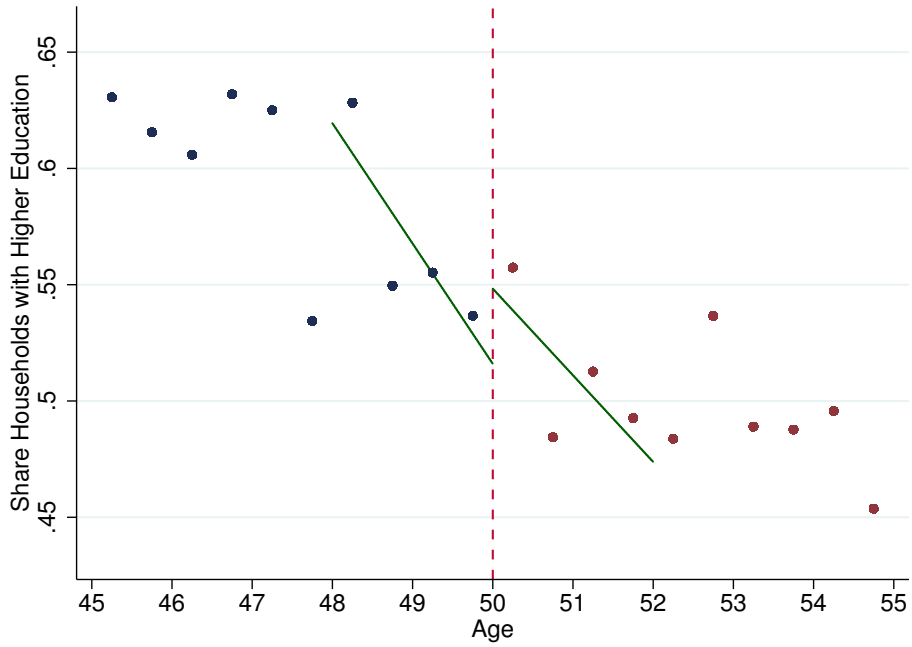
Figure 13: **Share of Households receiving Sickness Money**
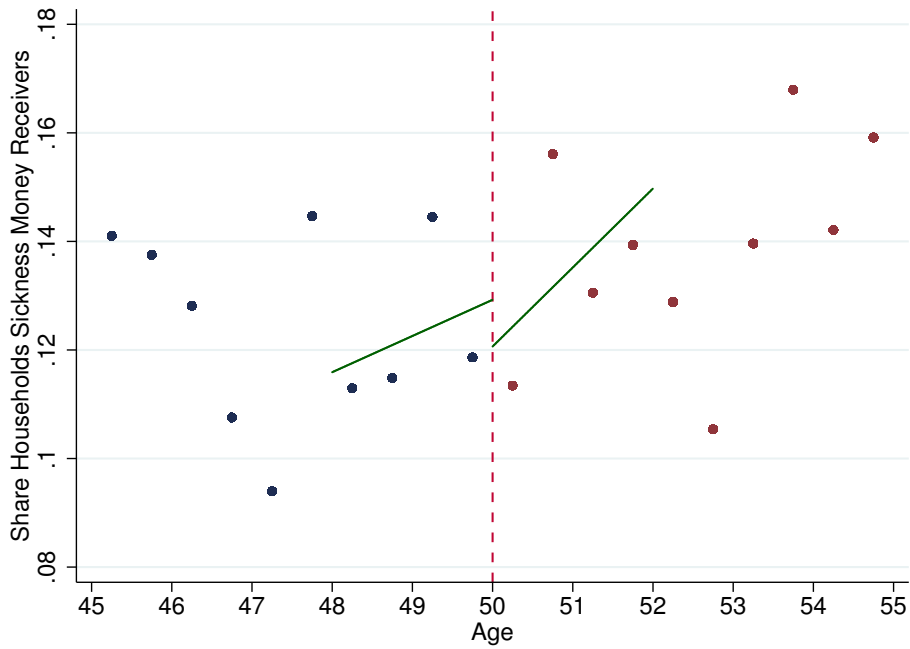
Note: The figures plot respectively the fraction of households in which the husband has higher education (upper) and the fraction receiving sickness money (lower), both against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 14: **Share of Households Employed in Manufacturing**

Note: The figure plots the fraction employed in the manufacturing sector 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

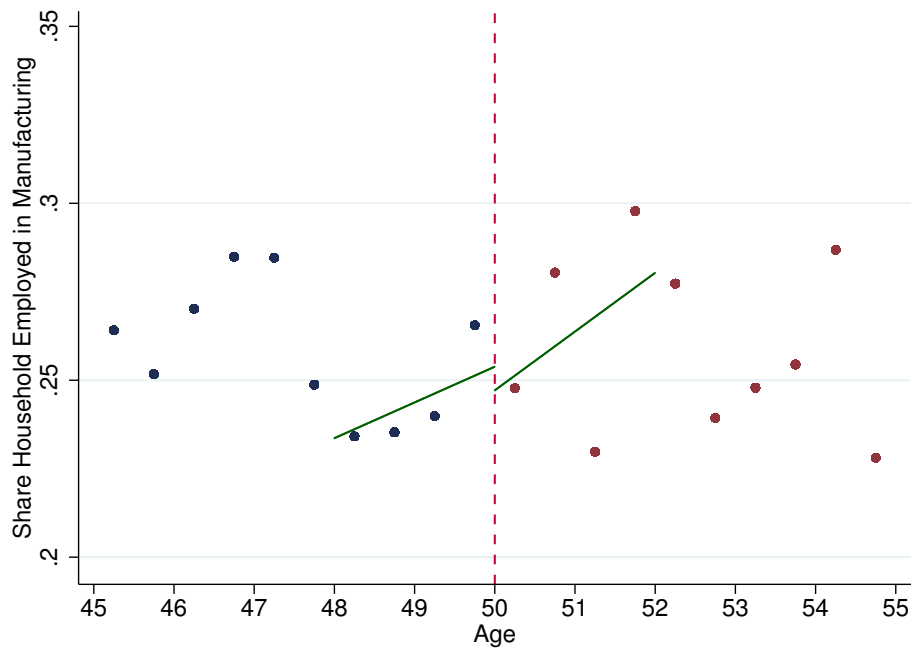|	Estimation (N=2,882)			Placebo (N=11,065)		
	Mean	Std Dev	Median	Mean	Std Dev	Median
Year	2,004	4.25	2,005	2,004	4.37	2,004
Age	50.02	1.17	50.02	50.00	1.16	50.00
Tenure (in years)	15.90	5.48	14.20	16.06	5.49	14.52
Dur NonEmpl (in days)	273.77	473.33	63.00	318.09	537.13	95.00
Fraction Re-Employed After (in %):						
12 Months	56.94			53.66		
15 Months	59.92			57.13		
18 Months	62.87			59.99		
Education (in %)						
Less than Highschool	39.3			46.0		
High School	25.2			30.7		
College	35.4			23.3		
Education Main Field (in %)						
General	28.3			33.2		
Humanities	4.4			1.7		
Teaching	5.7			1.3		
Econ/Adm	12.5			9.3		
Science/Eng	33.9			45.4		
Health/Sports	4.2			1.0		
Services	6.1			3.7		
Industry (in %)						
Manufacturing	14.0			32.9		
Construction	8.7			7.9		
Wholesale / Retail	14.8			19.8		
Transport / Communication	10.4			9.8		
Real estate	8.5			10.9		
Public adm / Defense	12.6			0.2		
Education	8.4			1.0		
Health / Social work	6.1			2.4		
Financial Variables (in 2004 USD):						
Annual Earnings	42,671	22,098	37,001	43,109	23,368	37,965
Monthly Earnings After Tax	2,489	1,289	2,158	2,515	1,363	2,215
HH Annual Earnings	56,933	29,282	52,342	58,360	31,274	52,936
Deposits	12,924	28,210	3,349	14,600	30,780	3,611
HH Deposits	17,461	34,343	5,591	19,530	36,489	6,386
Financial Wealth	31,475	90,124	4,686	32,878	83,586	5,869
HH Financial Wealth	39,446	103,107	8,095	41,053	96,484	10,231
Wealth	72,151	117,529	41,962	76,259	113,280	44,633
HH Wealth	88,287	133,935	54,462	93,457	129,952	56,979

Note: This table displays in the left panel summary statistics for the estimation sample of 2,882 households, aged between 48 and 52 and satisfying all the criteria described in Section 3. Additionally, summary statistics for the placebo sample of 11,065 households (satisfying all the same criteria except that the plant of separation was not 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 2010). Education Fields and Industries with shares smaller than 4% are omitted. 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
⩽ 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 details, see `http://www.sluttvederlag.no/`. 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	45.16	-6.20*	-7.76**	-7.06**	-0.17
	(33.43)	(3.56)	(3.54)	(3.55)	(0.10)
z	20.20	-1.41	-0.90	-2.44	-0.03
	(19.65)	(2.17)	(2.15)	(2.11)	(0.06)
Tz	-10.66	0.64	0.94	3.31	0.02
	(28.98)	(3.16)	(3.12)	(3.07)	(0.09)
Constant	417.10***	59.78***	63.39***	64.80***	
	(24.28)	(2.60)	(2.55)	(2.53)	
N	2,882	2,882	2,882	2,882	2,732
Panel B: Bandwidth = 3 (IK Optimal):					
T	57.57**	-7.07**	-8.09***	-7.82***	-0.18**
	(27.74)	(2.99)	(2.97)	(2.94)	(0.08)
z	0.06	0.06	0.34	-0.10	0.01
	(10.50)	(1.17)	(1.16)	(1.13)	(0.03)
Tz	6.84	-0.22	-0.39	0.26	-0.03
	(15.83)	(1.73)	(1.70)	(1.68)	(0.05)
Constant	402.54***	60.59***	64.20***	66.48***	
	(20.11)	(2.17)	(2.10)	(2.07)	
N	4,367	4,367	4,367	4,367	4,142

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 (IK 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. The effect on non-employment duration in days is estimated with durations censored after 2 years. Standard errors, clustered by plant, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: 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 Duration	-5.26 (31.54)	20.79 (29.89)	-39.01 (31.28)	2.06 (32.24)	-28.42 (33.10)	43.00 (33.43)	45.16 (33.43)	18.08 (34.97)	16.34 (34.84)
Fraction Re-Employed After 12 Months	1.47 (3.64)	-2.94 (3.41)	4.59 (3.55)	2.98 (3.62)	1.03 (3.67)	-4.79 (3.66)	-6.20* (3.56)	-0.56 (3.78)	-3.27 (3.59)
Fraction Re-Employed After 15 Months	2.69 (3.61)	-1.35 (3.32)	2.67 (3.53)	1.80 (3.53)	1.73 (3.61)	-4.68 (3.61)	-7.76** (3.54)	-1.06 (3.77)	-1.75 (3.61)
Fraction Re-Employed After 18 Months	1.99 (3.50)	-2.68 (3.32)	4.26 (3.47)	0.52 (3.50)	0.44 (3.61)	-5.24 (3.55)	-7.06** (3.55)	-1.35 (3.67)	-0.69 (3.59)
N	3,019	2,975	2,900	2,876	2,910	2,910	2,882	2,876	2,870
Cox Regression	0.03 (0.10)	-0.05 (0.10)	0.08 (0.10)	0.03 (0.10)	0.05 (0.10)	-0.13 (0.10)	-0.17 (0.10)	-0.03 (0.10)	-0.07 (0.10)
N	2,841	2,798	2,740	2,718	2,755	2,756	2,732	2,737	2,724

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. Standard errors, clustered by plant, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Placebo Plants: Baseline Specification, Main Outcomes

	Completed Duration	Fraction Re-Employed After:			Cox Regression
		12 Months	15 Months	18 Months	
T	1.265	-0.460	-0.748	-0.799	0.039
	(12.113)	(1.887)	(1.859)	(1.864)	(0.060)
z	10.144	-1.394	-1.167	-0.944	-0.051
	(7.509)	(1.173)	(1.158)	(1.144)	(0.037)
Tz	-1.462	0.387	0.710	0.551	-0.010
	(10.602)	(1.653)	(1.639)	(1.617)	(0.052)
Constant	375.043***	53.702***	57.146***	60.116***	
	(8.652)	(1.537)	(1.513)	(1.477)	
N	11,065	11,065	11,065	11,065	10,569

Note: This table repeats the main regressions from Table 3 for our placebo sample of individuals separated from plants that were not affiliated with LO-NHO and hence did not participate in the severance pay agreements (see Section 3 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. The effect on non-employment duration in days is estimated with durations censored after 2 years, and so is the Cox regression. Standard errors, clustered by plant, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Placebo Outcome Variables, Baseline Specification

	Income HH	Wealth HH	Fin Wealth HH	Deposits HH	Higher Edu.	Sickness Ben.	Manuf. Share
T	-706.8	-548.4	-1611.9	-1741.8	0.0323	-0.00860	-0.00668
	(1718.6)	(4306.1)	(2744.2)	(2847.0)	(0.0383)	(0.0241)	(0.0321)
z	75.20	2164.2	1524.6	790.5	-0.0517**	0.00667	0.0101
	(1099.2)	(2571.9)	(1635.1)	(1586.7)	(0.0228)	(0.0157)	(0.0195)
Tz	622.5	-2976.0	-1769.4	543.5	0.0145	0.00788	0.00651
	(1534.7)	(3652.7)	(2187.1)	(2306.0)	(0.0328)	(0.0223)	(0.0287)
Constant	54629.8***	67360.7***	22540.9***	18802.1***	0.516***	0.129***	0.254***
	(1255.5)	(3113.1)	(2053.0)	(2039.7)	(0.0276)	(0.0188)	(0.0261)
N	2,882	2,882	2,882	2,882	2,701	2,882	2,882

Note: This table repeats the main regressions from Table 3 for a set of outcomes that should not exhibit discontinuities at age 50. Displayed 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 plant, are reported in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. An estimation of the density of observations, following McCrary (2008), yields a coefficient of -0.018 and a standard error of 0.134, thus failing to reject the null hypothesis of no difference in densities.

Table 7: Alternative Optimal Bandwidths: Main Outcomes

| | Completed | Fraction Re-Employed After | | |
Rectangular Kernel:	Duration	12 Months	15 Months	18 Months
Optimal Bandwidth	37.90**	-7.06**	-8.48***	-7.72***
	(18.65)	(2.99)	(3.02)	(2.78)
N	4,391	4,367	4,352	4,796
0.5*Opt Bw	40.58	-7.17*	-7.83*	-5.71
	(26.51)	(4.19)	(4.20)	(4.01)
N	2,172	2,153	2,146	2,363
Optimal Bandwidth	3.02	3.00	2.99	3.32
Triangular Kernel:				
Optimal Bandwidth	39.11**	-7.56***	-8.50***	-7.65***
	(18.05)	(2.88)	(2.88)	(2.70)
N	5,594	5,530	5,456	6,184
0.5* Opt Bw	29.27	-6.53	-7.43*	-6.37*
	(25.62)	(4.06)	(4.09)	(3.84)
N	2,747	2,725	2,684	3,037
Optimal Bandwidth	4.15	3.81	3.76	4.23

Note: This table displays only the coefficients, and in parentheses the standard errors clustered by plant, 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. Instead of the censored regressions from Table 3, we here use the completed duration measure without censoring. 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 optimum bandwidth itself is displayed at the bottom of each panel. Stars denote statistical significance as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Regression on outcome variable: Re-Employment After 15 Months. Stratifying By Education & Above Median Wealth

	Higher Education	Higher Educ & Income	Higher Educ & Fin Wealth	Higher Educ & Deposits	Higher Educ & Finw/Inc	Higher Educ & Dep/Inc
Higher Educ	-2.50	2.77	1.92	-4.74	1.60	-7.44
	(5.85)	(9.85)	(9.00)	(9.17)	(8.35)	(9.07)
T	-8.03*	-10.31*	-18.77***	-18.11***	-15.55***	-15.39***
	(4.12)	(5.60)	(5.67)	(5.86)	(5.75)	(5.85)
Higher Educ*T	0.52	-21.34	-2.36	-0.28	0.75	1.00
	(8.83)	(14.81)	(13.60)	(13.77)	(13.27)	(13.16)
Constant	67.16***	67.14***	70.80***	70.46***	70.14***	70.59***
	(2.97)	(4.08)	(3.90)	(3.99)	(3.97)	(4.01)
N	2,701	2,701	2,701	2,701	2,701	2,701

Note: This table provides the regression discontinuity estimates of Equation 1 for the outcome variable Re-Employment After 15 Months. In the first column the coefficients T (Threshold) and z (forcing variable) are interacted with a dummy for higher education. In columns (2)-(6) T & z are interacted with the education measure, in addition to the five stratification measures in Table 9 accordingly, Income, Wealth, Financial Wealth, Deposits, Financial Wealth/Income & Deposits/Income. Remaining interaction terms with Above Median Wealth and the forcing variable z are omitted. Standard errors, clustered by plant, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

		Income	Fin Wealth	Deposits	Finw/Inc	Dep/Inc
Completed	T	102.38**	143.45***	134.89***	99.57**	109.81**
Duration		(47.04)	(48.96)	(47.58)	(47.96)	(47.77)
	T*D	-113.69*	-189.23***	-172.44**	-107.16	-127.74*
		(66.94)	(71.24)	(68.20)	(69.31)	(66.83)
Re-Employed After	T	-10.82**	-16.61***	-15.98***	-13.11**	-14.30***
12 Months:		(5.18)	(5.17)	(5.12)	(5.09)	(5.07)
	T*D	9.22	20.52***	19.37***	13.79*	16.37**
		(7.24)	(7.52)	(7.33)	(7.41)	(7.21)
Re-Employed After	T	-13.48***	-17.73***	-16.88***	-13.69***	-14.87***
15 Months:		(5.11)	(5.11)	(5.05)	(5.03)	(5.01)
	T*D	11.34	19.72***	18.03**	12.01	14.37**
		(7.24)	(7.57)	(7.32)	(7.44)	(7.23)
Re-Employed After	T	-12.84**	-16.94***	-16.00***	-12.68**	-14.22***
18 Months:		(5.08)	(5.15)	(5.05)	(5.07)	(5.02)
	T*D	11.41	19.56***	17.71**	11.43	14.46**
		(7.08)	(7.46)	(7.15)	(7.27)	(7.01)
N		2,882	2,882	2,882	2,882	2,882
Cox Regression	T	-0.30**	-0.40***	-0.41***	-0.28**	-0.35**
		(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
	T*D	0.27	0.47**	0.48**	0.24	0.37*
		(0.20)	(0.20)	(0.20)	(0.20)	(0.20)
N		2,732	2,732	2,732	2,732	2,732

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, as well as interactions between that indicator and the other regressors. Standard errors, clustered by plant, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. .

Table 10: Baseline Specification, Main Outcomes, Female Sample

	Completed Duration	Fraction Re-Employed After:			Cox Regression
		12 Months	15 Months	18 Months	
T	-15.73	2.43	2.23	0.01	0.08
	(41.39)	(4.38)	(4.34)	(4.31)	(0.12)
z	13.10	-1.71	-1.99	-1.26	-0.06
	(17.71)	(1.86)	(1.86)	(1.85)	(0.05)
Tz	-7.68	0.55	0.99	1.03	0.03
	(23.80)	(2.52)	(2.50)	(2.49)	(0.07)
Constant	496.46***	50.89***	53.94***	58.39***	
	(31.39)	(3.25)	(3.24)	(3.22)	
N	2,050	2,050	2,050	2,050	1,959

Note: The table provides the regression discontinuity estimates based on Equation 1 and using our baseline bandwidth of 2 years on each side of the threshold for females sepearated from their job. 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. The effect on non-employment duration in days is estimated with durations censored after 2 years. Standard errors, clustered by plant, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.