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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 severance payment worth 1.2 months' earnings at the median lowers the fraction re-employed after a year by seven percentage points. Data on household wealth enable us to verify that the effect is decreasing in prior wealth, which favors an interpretation as liquidity constraints over the alternative of mental accounting. Finding liquidity constraints in Norway, despite its equitable wealth distribution and generous welfare state, means they are likely to exist also in other countries.

JEL-Code: C410, E210, E240, J650.

Keywords: unemployment, optimal unemployment insurance, liquidity constraints, mental accounting, severance pay, regression discontinuity design.

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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 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 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 fractions 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

[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.

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 the other resources for the purpose of longer job search. Under the assumption that the strength of potential mental accounting is invariant to prior wealth[3] we can discriminate between the two scenarios, because in a world of liquidity constraints the severance pay effect will clearly be decreasing in prior (liquid) wealth. Since, in contrast to Card et al. (2007a), we are able to observe various measures of household wealth, we can test this, and we find that the effect is indeed decreasing in prior wealth. In fact, no statistically significant effect is found for those with above-median wealth. This evidence favors an interpretation of the severance pay effect as reflecting liquidity constraints rather than mental accounting.

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

[3] We investigate the plausibility of this assumption in Section 5.

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.[4]

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.[5] This provides a setup for RDD analysis. There are also further increases at ages 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.

[4]For further information on LO, NHO, and their joint scheme, see http://www.lo-nho-ordningene.no/

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

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.[6] 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:

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

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,[7] 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.[8]

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

[7] Our point estimates change very little if we instead control for age using a second order polynomial.

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

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.[9] 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.[10] 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 (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.

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

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

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.[11]

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.[12]

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 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.[13] Given that we find the largest effect of severance pay on the reemployment fractions 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 the measure of wealth. In view of the previous literature on liquidity constraints of households (Gruber (2001), Chetty and Szeidl (2007)), the most suitable definition of wealth should be financial wealth – including deposits, bonds, stocks and mutual funds, but not real estate – and measured at the household rather than the individual level, i.e. adding in also the wealth, if any, of the spouse. Nonetheless it is conceivable that transaction costs for stocks

[11] An additional reason in our case is that, as maintained for instance by Bratsberg et al. (2010), 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.

[12] 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.

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

and bonds are so high that households use only deposits, or that transaction costs for real estate are so low that households can swap their house to finance their job search, or that many married individuals keep their budgets sufficiently separate that individual holdings matter more than a household's total holdings. Fortunately, our data set is comprehensive enough that we can use total wealth, financial wealth and deposits alone, and each of these both at the individual and at the household level, thus allowing us to see how robust findings are to the use of different measures.[14]

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. On these grounds we have also repeated our analyses using not absolute wealth, but wealth relative to average income (across 3 years) before the job separation. This yields results similar to those based on absolute wealth.

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) 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

[14] All wealth measures are recorded at the end of the last calendar year before the one of the job separation. The quality of the real estate values in the data set is highly questionable, and it is thus reassuring that our results do not depend on one particular measure of wealth.

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.[15]

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 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. This inversely U-shaped relationship between elapsed non-employment duration and the size of the severance pay effect, with a maximum effect near 15 months, can be rationalized as presumably sufficient other liquidity is still available at very short horizons, whereas at longer horizons the job-finding propensity does generally decline due to skill depreciation and sample selection. This is indeed what we see in the bottom panel of Figure 6, plotting the raw propensity to find a new job against the time elapsed since job loss. 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 between day 200 and day 400. The average difference between those two lines is also captured in the results of the Cox regression, displayed in the last column of the table: Those eligible for the payment have, on average a 17% lower re-employment propensity than those not eligible.

[15]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.

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 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. 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 Regression Discontinuity Design, we need to explore whether there could have been

[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.

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, does not reveal any discontinuities at the age 50 threshold. This is also illustrated visually in Figures 9 through 8, 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 default choice of 2 years on each side has been motivated by choosing the widest-possible bandwidth under which our estimates do not get biased by effects of 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 (this added significance might be related to the small next policy discontinuity at age 52). We see these results as confirming 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

[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, although the use of these savings after job loss is rather limited.

[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

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 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 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 than on those with lower prior wealth. We can exploit this fact to discriminate between liquidity constraints and mental accounting if and only if plausibly the degree of mental accounting does not covary with wealth. It is however conceivable that education or some personality trait correlated with education, such as discipline, will affect both the degree of mental accounting and the amount of prior wealth held on the day of the job separation. However, none of our results do significantly change when we control for different measures of education. Moreover, further results suggest that the size of the severance pay effect does not vary across individuals holding and not holding a university degree. This suggests that plausibly the severance pay effect should be invariant to prior wealth under mental accounting, and that hence any such variation would speak in favor of liquidity constraints.

To proceed with our test, Table 8 augments the baseline regressions from Table 3 with continuous measures of income (column 1), total wealth (column 2), financial wealth (column 3), and deposits (column 4) – all measured in the year prior to the job separation. We find that the effect on all 3 re-employment fractions is clearly decreasing in both total and financial wealth, whereas the interaction

to be balanced. This categorization into three main accounts however is thought to be a good first approximation for the average household.

with deposits is not statistically significant.[19] In Table 9 we interact instead with indicators for whether someone's value of the different wealth measures exceeds the respective sample median. The table displays for each outcome variable and each interaction variable the main effect, T, which is now the effect for only those below the median, then the coefficient on the interaction between T and the dummy for being above the median, and finally the sum of those two. Consistent with the results from the interactions with the continuous measures, we find that the effect is always smaller for those above than for those below the median and in fact we always fail to reject at the 90% confidence level the hypothesis that the effect is zero for those above the median. These results do lend additional support to the view expressed in Card et al. (2007a) that the severance pay effect should indeed be interpreted as evidence of liquidity constraints.

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. This lends additional support to the view expressed by Card et al. (2007a) that the observed severance pay effect does indeed reflect liquidity constraints.

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

[19]The fact that we find significant interaction effects for total and financial wealth, but not for deposits (which account for only a limited fraction of households' assets) suggests that assets other than deposits either are not as illiquid for our sample as one might have thought, or that those households who do have them are able to borrow against 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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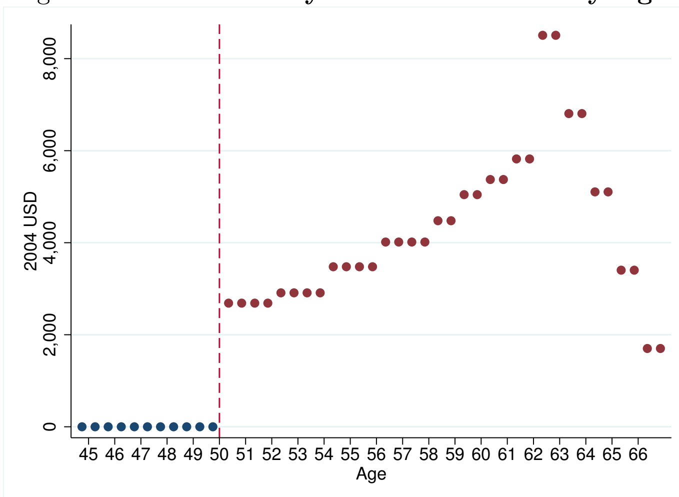
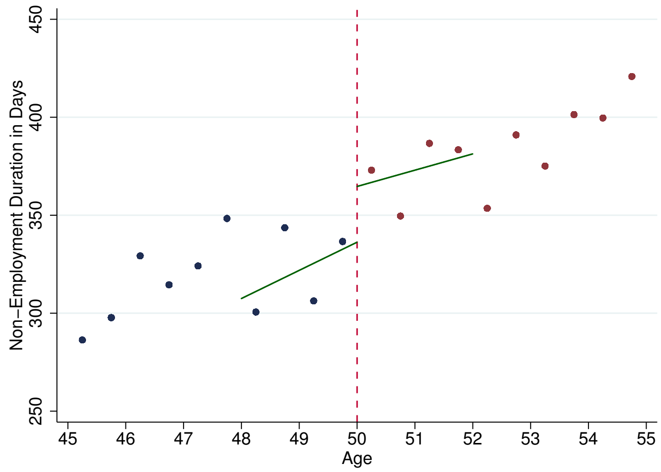
Figures and Tables

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



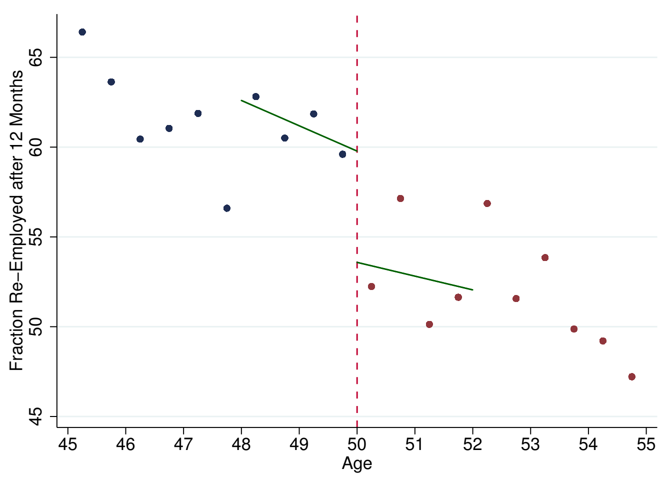
Note: The figure plots the Severance Pay Amount an eligible worker would have received if laid off between 2002 and 2009, for each 6-month bin of age. Amounts have been converted to USD at the average exchange rate prevalent in 2004.

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



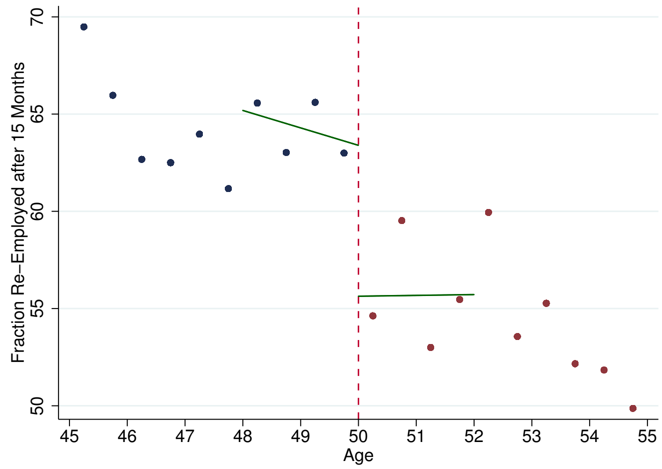
Note: The figure plots the average duration from job loss until the next regular job against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

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



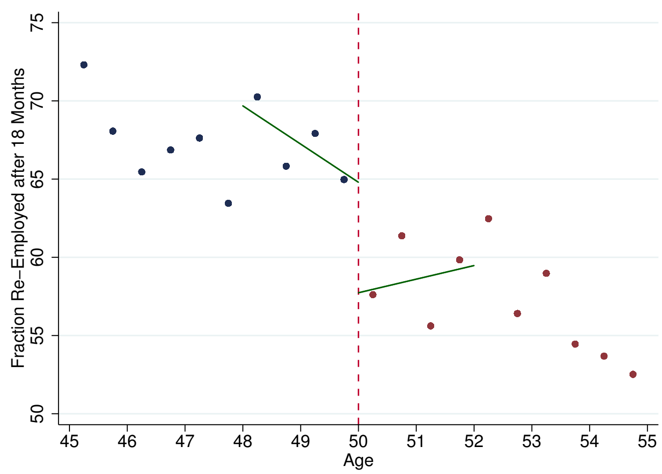
Note: The figure plots the fraction re-employed after 12 months against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

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



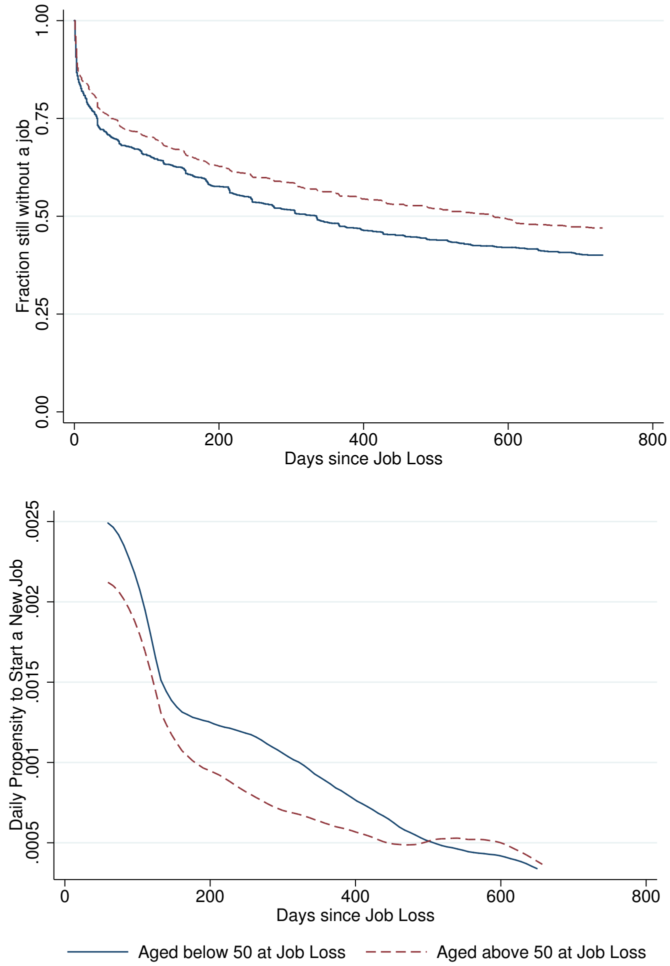
Note: The figure plots the fraction re-employed after 15 months against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

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



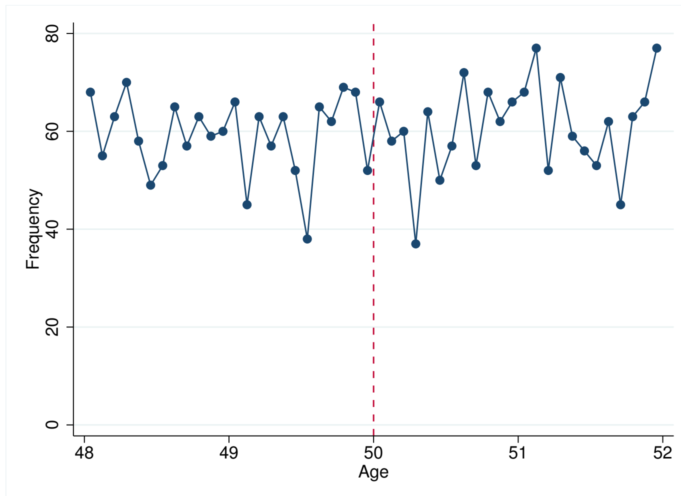
Note: The figure plots the fraction re-employed after 18 months against 6-month bins of age at job loss. Linear curves are fitted separately on each side of the age 50 discontinuity, for our default bandwidth of 2 years.

Figure 6: **Kaplan-Meier Survival Curves and Hazard Rates**



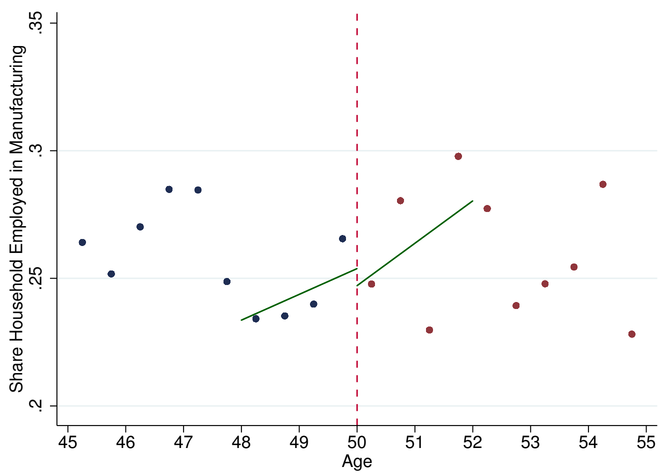
Note: In the upper part, Kaplan-Meier Survival Curves plot the fraction still without a new job against the number of days elapsed since job loss. It is always higher for those who lost their job only after turning 50 and who were hence eligible for the severance payment. In the lower part, we plot the hazard rates, i.e. the daily propensity to start a new job, against the number of days elaped since job loss. 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.

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: **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.
\clearpage

Figure 9: **Household Wealth in the Year before Job Loss**

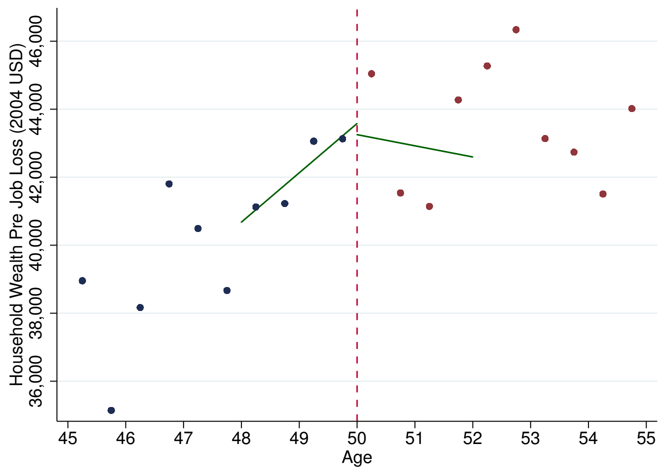
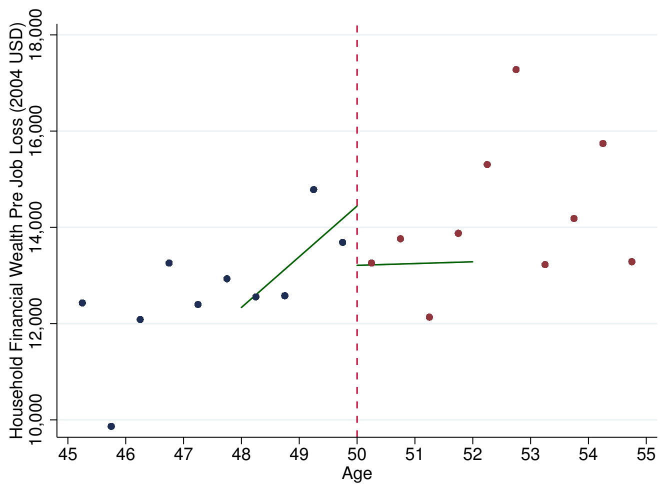


Figure 10: **Household Financial Wealth in the Year before Job Loss**



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 11: **Household Deposits in the Year before Job Loss**

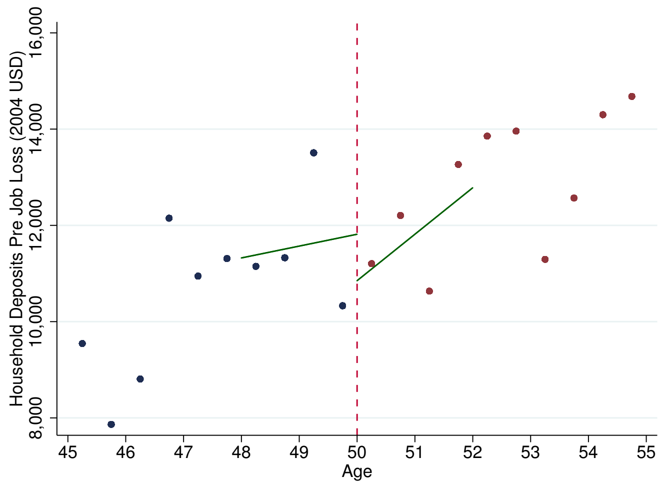
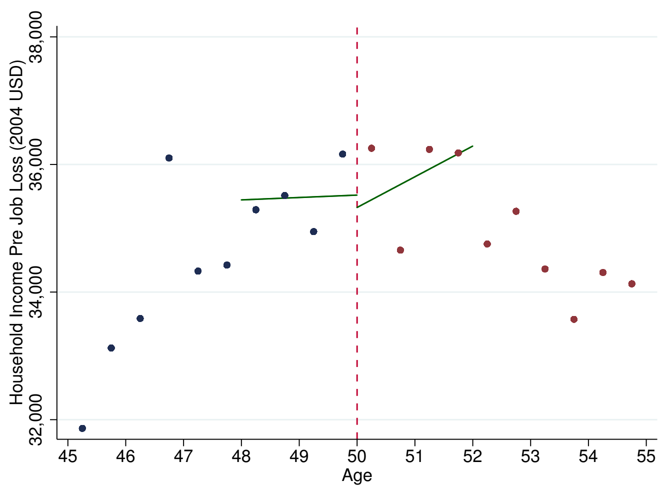


Figure 12: **Household Income in the Year before Job Loss**



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.

Figure 13: **Share of Households with Higher Education**

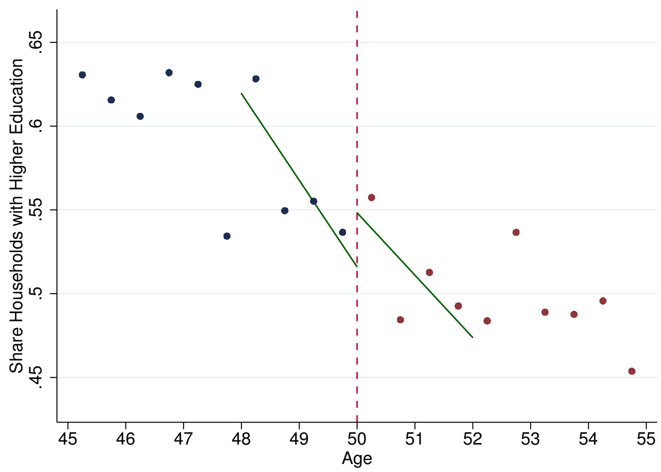
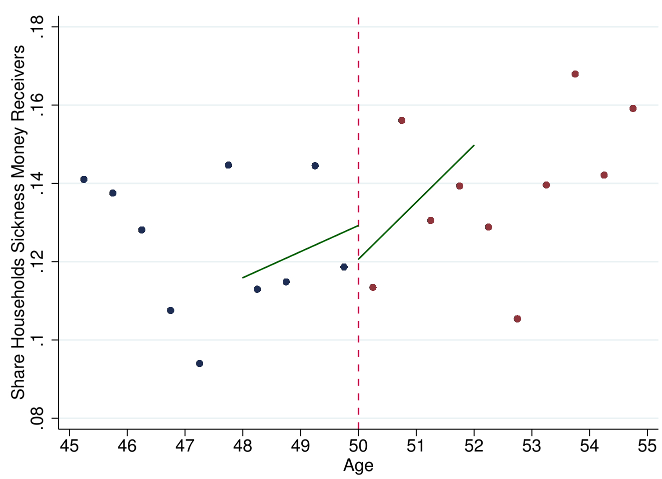


Figure 14: **Share of Households receiving Sickness Money**



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.

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 By Age And Period

Age	Oct 1993-	Oct 1995-	Mar 1998-	Aug 2002-
$\leqslant 49$	0	0	0	0
50	12,000	14,400	14,400	18,000
51	12,000	14,400	14,400	18,000
52	13,000	15,600	15,600	19,500
53	13,000	15,600	15,600	19,500
54	15,500	18,600	18,600	23,300
55	15,500	18,600	18,600	23,300
56	18,000	21,500	21,500	26,900
57	18,000	21,500	21,500	26,900
58	20,000	24,000	24,000	30,000
59	22,500	27,000	27,000	33,800
60	24,000	28,800	28,800	36,000
61	26,000	31,200	31,200	39,000
62	28,500	34,200	57,000	57,000
63	28,500	34,200	45,600	45,600
64	34,200	34,200	34,200	34,200
65	22,800	22,800	22,800	22,800
66	11,400	11,400	11,400	11,400

Note: The table displays predicted Severance Pay in NOK 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.

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

		Income	Wealth	Fin Wealth	Deposits
Completed	T	44.01	49.02	48.30	45.24
Duration		(33.44)	(33.30)	(33.36)	(33.44)
	T*W	-64.17*	-30.64	-32.75	17.72
		(34.68)	(37.05)	(36.85)	(34.07)
Re-Employed After	T	-5.96	-6.04*	-5.96	-5.84
12 Months:		(3.63)	(3.62)	(3.62)	(3.63)
	T*W	2.90	6.56**	11.89***	-3.65
		(5.06)	(2.68)	(3.60)	(3.29)
Re-Employed After	T	-7.38**	-7.53**	-7.47**	-7.32**
15 Months:		(3.66)	(3.65)	(3.65)	(3.66)
	T*W	4.97	7.16***	11.56***	-2.68
		(5.29)	(2.52)	(3.87)	(3.31)
Re-Employed After	T	-7.07*	-7.24**	-7.17**	-7.03*
18 Months:		(3.62)	(3.60)	(3.60)	(3.61)
	T*W	6.69	7.37***	12.39***	-1.85
		(5.07)	(2.54)	(3.88)	(3.40)
N		2,882	2,882	2,882	2,882
Cox	T	-0.16	-0.17*	-0.17*	-0.17
Regression		(0.10)	(0.10)	(0.10)	(0.10)
	T*W	0.13	0.19*	0.17	-0.01
		(0.10)	(0.11)	(0.11)	(0.11)
N		2,732	2,732	2,732	2,732

Note: This table provides the regression discontinuity estimates of Equation 1, augmented by continuous measures of wealth and income (deflated to 2004 values), as well as their interaction with each of the other regressors. Each column uses a different income or wealth measure as indicated. The top panel uses as outcome variable non-employment duration in days, the following ones use the fraction re-employed after respectively 12, 15 and 18 months and the lower panel the standard Cox Regression. Standard errors, clustered by plant, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

		Income	Wealth	Fin Wealth	Deposits
Completed	T	102.38**	143.45***	68.30	134.89***
Duration		(47.04)	(48.96)	(47.09)	(47.58)
	T*D	-113.69*	-189.23***	-44.75	-172.44**
		(66.94)	(71.24)	(66.55)	(68.20)
	T + T*D	-11.31	-45.78	-23.55	-37.56
	Prob > F(1,2875)	0.81	0.34	0.62	0.43
Re-Employed After	T	-9.71*	-8.25	-14.95***	-15.87***
12 Months:		(5.40)	(5.45)	(5.39)	(5.41)
	T*D	7.87	4.95	18.04**	19.84***
		(7.64)	(7.65)	(7.63)	(7.63)
	T + T*D	-1.83	-3.29	3.09	3.98
	Prob > F(1,2875)	0.73	0.54	0.57	0.46
Re-Employed After	T	-12.20**	-9.35*	-15.09***	-16.41***
15 Months:		(5.34)	(5.40)	(5.36)	(5.38)
	T*D	9.85	4.16	15.50**	17.88**
		(7.56)	(7.57)	(7.56)	(7.55)
	T + T*D	-2.34	-5.19	0.41	1.46
	Prob > F(1,2875)	0.66	0.33	0.94	0.78
Re-Employed After	T	-12.73**	-9.95*	-15.40***	-16.51***
18 Months:		(5.26)	(5.32)	(5.30)	(5.32)
	T*D	11.50	5.95	16.64**	18.58**
		(7.46)	(7.48)	(7.47)	(7.46)
	T + T*D	-1.22	-4.01	1.23	2.07
	Prob > F(1,2875)	0.82	0.45	0.81	0.69
N		2,882	2,882	2,882	2,882
Cox Regression	T	-0.29	-0.40	-0.23	-0.41
		(0.14)	(0.14)	(0.14)	(0.14)
	T*D	0.27	0.47	0.12	0.48
		(0.20)	(0.20)	(0.20)	(0.20)
	T + T*D	-0.03	-0.07	-0.11	0.07
	Prob > Chi2(1)	0.85	0.65	0.45	0.61
N		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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table does also provide the sum of the coefficient on being above the threshold and the coefficient on the interaction of the threshold dummy with the dummy for income or wealth above the median. The p-value for the F-test with the null hypothesis that this sum is zero is reported in the line below. None of these 20 tests rejects this Null hypotheses at the 10% level.