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

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April 5, 2011

Abstract

We investigate the causal effect of lump-sum severance payments on job search duration in Norway. Identification is achieved by exploiting discontinuities in the available amounts along the age and time dimensions. The results can be interpreted as evidence of liquidity constraints, thus suggesting that in the spirit of the Baily-Chetty model of Optimal Unemployment Insurance current benefits are too low. Since Norway has one of the most equitable wealth distributions and one of the most generous UI systems of all OECD economies, this suggests that liquidity constraints are likely to matter also in other OECD economies.

Keywords: Unemployment, Optimal Unemployment Benefits, Liquidity Constraints, Mental Accounting, Duration Analysis, Regression Discontinuity Design

JEL Codes: C41, E21, E24, J65

1 Introduction

Should the generosity of unemployment insurance be increased, to allow individuals to search longer and find a better-fitting job? Or should it be reduced, so as to decrease the severity of Moral Hazard? These questions continue to ignite heated political debates across OECD economies. Much of the academic literature for a long time seemed to support the latter contention: Papers like Katz and Meyer [1990] or Lalive et al. [2006] had shown that more generous unemployment insurance (UI) did prolong unemployment duration, and the common interpretation was one of pure Moral Hazard: Individuals take longer to find

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[†] Christoph Basten would like to thank Statistics Norway for their great hospitality during the work on this project. We are indebted for helpful guidance to Erzo Luttmer, Luigi Guiso and Andrea Ichino, and grateful to seminar audiences at Harvard, the EUI, Statistics Norway, Zurich, and the 2010 Christmas Meeting of German Economists Abroad. The usual disclaimer applies.

a new job because someone else is paying. However, Chetty [2008] and Card et al. [2007a] ingeniously pointed out that part of the effect of UI on job search duration could be an income rather than a price effect: Individuals would like to choose a longer search duration also if they had to entirely finance it themselves – but they cannot do so because financial markets fail to lend them the necessary liquidity. For a given severity of Moral Hazard, greater liquidity constraints call for higher UI in order to correct that market failure[1]. To investigate the empirical relevance of this consideration for Austria, Card et al. [2007a] estimate the effect on job search duration of severance payments which by definition have no price effect because they are granted lump-sum: They find it to be positive and interpret this as evidence that liquidity effects do indeed matter, with the highly relevant implication that the generosity of the unemployment benefits paid in Austria during the sample period was below its optimal level. The finding is argued to be indicative also for the US, since the two countries share many relevant labor market characteristics, including relatively low UI with a maximum duration (in normal times) of six months and a replacement rate of around 50%.

What however does this imply for most other OECD economies, where prevailing benefit levels and durations are significantly more generous? Does the finding of liquidity constrained job losers apply there as well, or is UI there too generous, as many politicians and citizens would suspect? The present paper seeks to make a step in the direction of answering this question by investigating whether a causal effect of lump-sum severance payments on job search duration can be found also in Norway, whose UI generosity is near the upper bound of all OECD economies – with replacement rates of 62.4% and maximum benefit durations of at least two years for most job losers – while at the same time the average household holds comparatively high levels of wealth relative to annual income[2]. If we find an effect of cash-on-hand on job search duration even here, then it seems plausible that similar findings would be made also in many other OECD economies.

To identify the causal effect of lump-sum severance payments, we exploit discontinuities in the amount individuals are eligible for along the age and time dimensions, as agreed upon between the Confederation of Norwegian Enterprise, "Næringslivets Hovedorganisasjon" (NHO), and the Norwegian Confederation of Trade Unions, "Landsorganisasjonen i Norge" (LO). Henceforth we shall refer to these agreements as the "(LO-NHO) scheme". Under these schemes, the amount of severance pay granted, displayed in Table 1, becomes non-zero from age 50 onward, after which it increases every two years until age 58 and every single year thereafter until age 62, and then decreases each year until the final potential labor market year at age 66. In addition, within our period of observation, 1993-2008, the amounts for different age groups were discontinuously increased three times. The agreements cover all individuals who are involuntarily separated

[1] This is under the assumption that the government is not able to cure the market failure more directly by providing the necessary lending.

[2] For details on this, see Basten et al. [2011].

from one of the agreement's member plants after 10 or more years of tenure. While the variable tenure, which would otherwise also have been interesting for use as the running variable for a Regression Discontinuity Design, is not observed with sufficient precision, we fortunately do observe both layoff date and age precise to the day. This allows us to flexibly control for both of these running variables per se, thus identifying the causal effect of different amounts of severance pay eligibility only off of the discontinuities listed in Table 1 and displayed graphically for the age dimension (averaged across the 4 periods) in Figure 1. To fully exploit the duration structure of our data, we combine this identification strategy with a Cox Proportional Hazard model.

We find that on average each NOK 1,000 of severance pay lowers the job starting probability on any day within the first 2 years by about 1.4% relative to the baseline group aged just below 50 and who are hence not eligible for any severance pay. For the group aged just above 50 who in the most recent period received NOK 18,000, this implies a 25% lower job finding hazard. Similar results are obtained when we focus on the discontinuity around age 50 alone, and are also confirmed with Censored Normal Regressions using completed job search duration as the dependent variable. The results are robust to many different kinds of control function as well as different ways of censoring the job search duration, and the functioning of our control function is confirmed by a number of suitable placebo regressions.

In a complementary section, we discuss the possibility that part of the reduced-form effect of severance pay, rather than being evidence of liquidity constraints, may result from Mental Accounting behavior: Households might have a higher propensity to consume goods or leisure out of money mentally classified as "income" than out of money classified as "wealth". If they then classify the severance payments – which they receive when regular labor income drops – as income, we may see a causal effect of such payments on job search duration even amongst households who are not formally liquidity-constrained. To empirically test the plausibility of the liquidity interpretation, we interact the severance pay amount with indicators for the different terciles of households' wealth distribution, on a wide range of different wealth measures, and test whether the size of the effect is decreasing in prior wealth. While we do find the effect to be slightly decreasing in a number of wealth measures, the patterns are not statistically significant. Unfortunately we lack the necessary precision to make a conclusive statement on this issue and need to postpone this to future research. In practice though, we argue that the policy implications of Mental Accounting behavior, which can be interpreted as "internal liquidity constraints" are likely the same as those of external Liquidity Constraints.

The remainder of the paper is structured as follows: Section 2 explains our Empirical Strategy and Section 4 introduces the data we use to implement it. Section 4 presents the general results on the effect of lump-sum severance payments on job search duration. Section 5 addresses theoretically and empirically the possibility of Mental Accounting behavior, and Section 6 concludes.

2 Empirical Strategy

To identify the causal effect of lump-sum severance payments on job-search duration, we exploit agreements first established in 1966 between Norway's Confederation of Trade Unions, "Landsorganisasjonen i Norge" (LO), and the Confederation of Norwegian Enterprise, "Næringslivets Hovedorganisasjon" (NHO) on severance payments ("Sluttvederlag", SLV) to workers losing their jobs. Under these agreements, employees are eligible for severance pay if and only if they are aged above 50 and have at least 10 years of tenure in their current plant or at least 15 years in several member plants. Furthermore, as Table 1 shows, the amount changes discontinuously, i.e. from one day to the next, at several other age thresholds as well as across four different time periods. A simple regression of job search duration on the theoretical severance pay amounts would in this setup not allow us to identify the causal effect of severance pay, because for instance in the age range up until age 62 severance pay amounts are increasing in age, but age is likely to be correlated with search duration also through channels other than the severance pay amounts. Furthermore, nominal amounts are higher in later years, so that differences across years in the tightness of the labor market or in the price level could lead our estimates to be biased.

2.1 The Control Function

However, since we are able to observe the day of layoff and each individual's age precise to the day, we are able to deal with this issue by controlling flexibly for any other factors correlated with age or time per se, and identify the causal effect of severance pay eligibility off of the discontinuous variation at the various age thresholds. In essence, we estimate the following equation for job search duration $dur_{i,t}$:

$$dur_{i,t} = \alpha + \beta * SP_{i,t}^{assigned} + f(age_{i,t}, time_t) + \varepsilon_{i,t} \quad (1)$$

To ensure that our control function does indeed fully take out the effects of any factors correlated with age or time other than the severance payments, we repeat our analyses with a wide range of different control functions for age, and always include also a complete set of calendar year fixed effects. We then test the functioning of our control functions on a placebo sample with similar age and tenure structure but coming from employers that were not participating in the severance pay scheme. As we discuss in more detail in the Section 4 below, we do find an "effect of severance payments" in our placebo sample *before* adding the control function, confirming the need for the latter, but not afterwards. We interpret this as suggesting that our control functions are indeed doing a good job.

In particular, we use specifications with six different control functions. Following Card et al. [2007a] and the papers cited therein, we start with a third-order polynomial in age, but for our most basic specification we use only a single third-order polynomial in age for all intervals shown in Table 1 and for all four periods with different SP amounts. Next, we expand this to four separate polynomials, one for each of the four periods. In a third step, we replace the third-order polynomial with a Linear Spline (LS) in age: A separate linear function with a separate slope is then estimated for each age interval with a different severance pay amount. In a fourth step, we use instead a Restricted Cubic Spline (RCS), which allows the slopes to be nonlinear even within each of the intervals. The exact formulas of both kinds of spline are given in the appendix. Finally, in a fifth and sixth step we allow respectively for four separate Linear Splines and for four separate Restricted Cubic Splines, so as to take account of the different severance pay regimes listed in Table 1. Table 4, which we discuss in further detail in Section 4 below, shows that our estimator of interest is relatively robust to which control function we use. Therefore our main discussion of results will focus on the most basic specification, controlling only for the third-order polynomial in age along with the set of calendar year fixed effects.

2.2 Cox duration analysis and Censored Normal Regression

We combine the identification strategy discussed above with a Cox Proportional Hazards Model (see Cox [1972]). This is based on the concept of the hazard rate, which for any given period is defined as the number of individuals starting a new job divided by the current number of individuals not holding one. It is intended to reflect the propensity to start a new job in a given period, conditional on not having done so until that point in time. Using this hazard rate, the Cox analysis then estimates the following equation

$$h(t|\mathbf{x}_i) = h_0 \exp(\mathbf{x}_i \boldsymbol{\beta}_x) \tag{2}$$

or equivalently in logs

$$ln(h_t|\mathbf{x}_i) = \log h_0 + \beta X \tag{3}$$

where h_0 is the baseline hazard, i.e. the propensity to start a new job conditional on not having done so up until now, for some baseline reference group with $x_i = 0$. The key assumption behind this specification is that the effect of covariates is proportional across all time periods (days in our case): $\frac{d \log h_t}{dX} = \beta = \frac{d \log h_s}{dX} \forall s, t$. Our non-parametric Figures 7 and 8 where this assumption has not been imposed but curves for individuals with different values of the covariates are nonetheless mostly parallel, confirm that this is a valid approximation of reality. At the same time, use of the Cox model has the advantage that we can leave the baseline hazard unspecified. Hence our results will be independent of whether the latter is increasing, decreasing or constant over time, i.e. whether there is positive, negative or no "duration dependence".[3]

[3] For an introduction to Cox Proportional Hazard models and alternative methods of survival analysis, see for instance Cleves et al. [2008].

Combining this with our discontinuity design then requires us to include in the set of regressors X both the assigned amount of severance pay S_i and our control function in age and time per. Doing so then gives an equation of the following form:

$$h(t|\mathbf{x}_i) = h_0 \exp[\beta_T S_i + f(age, time)] \quad (4)$$

In addition to our Cox analyses, we also estimate the effect of severance pay on completed job search durations. To take into account that these durations have been censored, an issue we discuss in more detail below, we estimate these equations by Censored Normal Regression instead of the more standard Ordinary Least Squares. In contrast to Ordinary Least Squares estimates, the CNR ones prove relatively robust to how we censor the durations, albeit not as much as the Cox estimates, which confirms our choice of focusing on the latter.

2.3 Testing for Manipulation of the Threshold

When seeking to identify a causal effect off of one or several discontinuities, two major challenges do typically arise. The first is potential manipulation of the threshold: In our context, employers or employees might systematically try to make the separation happen just before or just after the employee reaches a new integer age at which the severance pay amount changes, and if that is possible for some individuals then one has to worry that in those cases in which it works individuals may be either more or less keen to return to work than in the cases where it does not work. In considering whether this is an issue for us, a qualitative investigation of the context makes such "gaming" appear relatively unlikely: As severance payments under the LO-NHO scheme are made by the LO-NHO fund rather than the individual employer, and since employers' future dues to that fund are not "experience-rated", i.e. do not depend on the fund's past payouts to the company's ex-employees, the employer has no particular incentive to lay off individuals just before they turn 50, 52, and so on. At the same time, the administration of the fund will work to ensure that employers do not systematically lay off their employees just after the respective thresholds.

This said, we do of course wish to search empirically for any evidence of potential threshold manipulation To do so, Figures 2, 3 and 4 plot the frequency of separations in our final sample (as defined in Section 3) by age, using respectively quarterly, monthly and weekly age bins. Figures 5 and 6 then repeat the same exercise, with respectively monthly and weekly bins, focusing around the age 50 discontinuity only, which we analyze in more detail below. As can be seen, there is no evidence of separations spiking either just before or just after severance payments increase or decrease, confirming the more qualitative picture given above.

2.4 Other Discontinuities and Censoring

The second major threat to identification is the possibility that other things also change at the thresholds. In general our research has not detected any such changes at the discontinuities under consideration, except however for two of our three larger discontinuities at respectively ages 60 and 62: Norwegian employees who lose their job at or after age 62 are then eligible to receive unemployment benefits continuously until the official retirement age of 67, rather than just for the usual maximum duration of 2 years. As a consequence, someone losing his job at or after age 60 can first fully exhaust the standard maximum duration of 2 years and then make use of the other rule. A possible way to deal with this is to censor job search durations before those critical thresholds: Then individuals separating from their jobs only after reaching age 60 or 62 will not be part of the analysis at all, and those who separate say N days before but are still without a job when reaching the threshold will only be used to compute the hazard rate, i.e. the number of job finders relative to the number currently without a job, only on his first N days but not thereafter. Table 5 shows the results of estimating the most basic specification of our Cox regressions censoring at respectively the formal retirement age of 67 and the potentially problematic ages 62 and 60. It also varies the job search duration after which we censor between 2 years, the maximum duration for which everyone in our sample can receive UI benefits, and 6 months, the maximum duration for which individuals can receive UI in Austria and hence the censoring duration used in Card et al. [2007a]. It turns out that the effect of severance payment in our sample is economically and statistically significant under all possible censoring choices. To be on the safe side however, we henceforth make the most conservative choice and censor already at age 60. The finding that our results do not hinge on other things changing at thresholds 60 and 62 is also confirmed in the results section when we conduct a simpler Regression Discontinuity Design exploiting only the third larger discontinuity, at age 50.[4]

3 Data and Measurement

3.1 Data Sources and Sample Definition

We use administrative data from Norwegian tax registers that cover the universe of Norwegian taxpayers. Our records on job separations are taken from Norwegian social security registers and we start with all job separations occurring between 1993 and 2008 out of plants that were party to the severance pay agreement drawn up between the labor union, LO, and the employer organization, NHO. For the placebo sample we employ all the same restrictions, but take only individuals from plants that did not participate in the agreement. In both cases, we retain those aged between 45 and 66 at the day of leaving their job (although our main results are only based on individuals aged below 60, see

[4] In addition to age 60 and a duration of 2 years, search time is naturally censored at the end of our panel, on 31 December 2008.

Section 2), because eligibility for any severance payment starts only at age 50, and because 67 is the regular retirement age. We drop all those entering early retirement. Early retirement is possibly endogenous and therefore might lead to bias if our results depended on individuals eligible for early retirement, but our choice of censoring durations at age 60 takes care of this. As indicated above, individuals are eligible for severance pay only if the separation is declared to be involuntary, a status which we cannot observe directly. We drop individuals who are observed to start a new job just on the subsequent calendar day. Many of these job "changes" are purely administrative records of individuals who merely switch to a different position within the same firm. Some are changes to a new employer and in principle a subset of these might be cases in which the individual was laid off from the previous job and able to secure a directly following new job already within the 3-month notice period. Such an individual might be eligible for severance pay. However, it seems reasonable to assume that the majority of such cases would take more than one day before starting a new job, and that the set of those starting a new job already the next day will therefore almost entirely be composed of individuals leaving their old job voluntarily precisely because they wish to start a new job. We have however also tried our main regressions on the larger sample still including those who start a new job just the next day, and the results are qualitatively unchanged. Furthermore, for our main sample we drop those registered as being out of their job for precisely one specific calendar *year* and which we think are likely to be data errors, but again the results hold also when we do not impose this rule. Finally, while Card et al. [2007a] drop those returning to their old firm, on the grounds that these are likely to know about this return already at the point of leaving and will thus likely not be truly searching, we retain them in our sample for our main results as we think that the sample of individuals allowed to return will be endogenous, and since those whose firm offers them a subsequent job lose their severance pay eligibility only if this new job starts already within 3 months of separating from the old job. But in the results sections below we will also display the results obtained when excluding returners in addition; we find that the estimated effects become very slightly smaller then. Furthermore, we require individuals to have earned at least the (very low) "minimum amount" (Grunnbeløpet) required for eligibility for at least two years of UI, so that our sample is homogeneously eligible for the same UI. Finally, the results displayed in Tables 4 ff. are for males only. In the subsample of females the effect is also found, but the average effect is much weaker and not statistically significant, and so we focus on the sample of males so as to obtain more precise point estimates, also for our subsequent analyses for which we stratify the sample by tercile of wealth.

3.2 Restricting the Sample by measured Tenure

As we have pointed out above, eligibility for LO-NHO severance pay requires a continuous tenure of at least 10 years in the current plant, or a combined tenure of at least 15 years in plants that were all members of the scheme. Hence in order to correctly estimate the size of the severance pay effect, we would like to include in our sample all those who satisfy this requirement and exclude all those who do not. This is complicated somewhat by the fact that we do not observe everyone's past job history for 15 or more years, and that we do not observe which tenure interruptions –such as maternity leave and some specific types of sick leave – were for severance pay eligibility purposes counted as continuation of the previous job. This is also why we cannot implement a Regression Discontinuity Design with tenure as the running variable, as we might otherwise have wanted to do. For our identification strategy pointed out above it means that we are able to identify a subsample of individuals for whom we know that they satisfied the tenure requirement. By contrast, amongst those for whom we do not know this some will and some will not have fulfilled the requirement. To get our point estimates of the effect of severance pay right, we make the conservative choice of using only those individuals for whom tenure is known to be at least 10 years.[5] This restriction shrinks our sample size to about 10% and so the question arises to what extent the sample on which our analyses are based is still representative for the larger population without the tenure restriction. To answer this question, Table 2 presents summary statistics first for the full sample and then for the subsample used. We find that our subsample is on average about 9 years older, more educated and has higher income and wealth. A priori, one would expect these factors to lead to lower liquidity constraints, and so it may be expected that our results must be interpreted as lower bounds on the effect that would be found if our quasi-experiment and data allowed us to identify the severance pay effect also for those with lower measured tenure.

3.3 Intention-to-Treat vs. Wald Estimators

A last restriction from our data is that, since severance payments are not taxable, 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, i.e. to instrument actual with hypothetical payments. Instead, like Card et al. [2007a], we can only estimate the Reduced-Form or Intention-to-Treat effect of severance pay eligibility, which constitutes again a lower bound on the effect of actual severance pay. We can however expect compliance amongst those eligible to be very high, since the claim forms are automatically sent to the LO-NHO joint office by the employer, together with the layoff notification.

3.4 Defining the duration of Job Search

The previous literature analyzing the duration of unemployment or job search duration has commonly considered two measures thereof. The first is simply the period for which an individual is officially registered as unemployed, the second is the time between lay-off and the start of a new regular job, and which Card et al. [2007a] denote "non-employment duration". Based on the findings in Card et al. [2007b], they argue for the former, on the grounds that in their

[5] Using the full sample results in estimates of the severance pay effect that are about half as high as thoe we display here.

Austrian sample many individuals will leave the unemployment register after six months not because they have actually found a job, but only because their benefits expire so that they no longer have an incentive to keep registering at the agency.

In our setting this is less of a problem, because all employees in our sample are eligible for up to two years of benefits. Yet there are good reasons for us to also focus on the latter measure: As Bratsberg et al. [2010] and some of the papers cited therein point out, Norway has four times as many individuals receiving disability insurance as individuals receiving unemployment insurance, and many of the former would be assigned the latter label in other countries. At the same time, Autor and Duggan [2007] show that liquidity effects are as relevant an issue for those on disability insurance as for those on unemployment insurance. Furthermore, the Mental Accounting scenario considered in Section 5 also applies also to those on disability insurance. Hence we use as outcome measure the duration from lay-off until the start of a new job, thus capturing both individuals receiving unemployment benefits and those receiving disability benefits. An added benefit of doing so is that it provides us with a larger sample size and hence ceteris paribus allows for greater statistical precision.

3.5 Measuring Household Wealth

In view of the previous literature on liquidity constraints of households, 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. Nonetheless it is conceivable that transaction costs for stocks and bonds are so high that households use only deposits, or that transaction costs for real estate are so low that they can also use their real estate, or that many married individuals keep their budgets sufficiently separate that individual holdings matter more than a household's total holdings. Fortunately, our dataset 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.[6] 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 largely depend on prior income. On these grounds we subsequently repeat the same analyses stratifying the sample by the terciles of different "preparedness" measures, for which we scale the different wealth measures by the average income earned in the last three years before job loss.

[6] Here we add to the holdings of the individual under consideration those of the spouse, if any.

4 Results

4.1 Non- and Semi-Parametric Graphical Analysis

Before implementing the Cox model, which assumes that the treatment effect is the same in each stage of the job search duration, we start with non-parametric Kaplan-Meier survival curves, plotting the fraction of individuals still without a new job against the days elapsed since the separation. Figure 7 plots these for individuals eligible for different amounts of severance pay and shows that on any day since the separation those eligible for higher amounts were indeed more likely to still be without a job than those eligible for lower amounts. Now this might of course be entirely due to other factors correlated with age and hence also correlated with the severance pay amounts. To inquire whether this is the case, Figure 8 continues with a semi-parametric analysis, plotting the same curves after controlling for our basic control function with a third-order polynomial in age and a complete set of calendar year dummies. To obtain these curves, we have first estimated a Cox model on the control function only, and have then used the resulting baseline hazard to plot Kaplan-Meier curves adjusted for the control function. We can clearly see that the pattern with higher "survival" probabilities for those eligible for higher severance pay persists also after adjusting for the control function. Now one may of course suspect that the control function is simply not comprehensive enough. To test this, we now repeat the same two analyses for our placebo sample (more on which below). The results of doing so, first without and then with control function, are displayed respectively in Figures 9 and 10. Interestingly we see the qualitatively the same pattern as in our main sample before adding the control function, confirming the need for the latter. After adding it however, the survival rates are no longer monotonously increasing in the severance pay amounts, indicating that our control function is working. The more formal analysis provided below will confirm this. Also noteworthy is the fact that the curves are not always parallel, but are so most of the time, thus vindicating our subsequent use of the Proportional Hazards model.

4.2 Cox Proportional Hazard Analysis

Table 4 shows the results of our main regressions for the different control functions discussed above. We find that for each NOK 1,000 of severance pay the propensity to find a job the next day is between 1.2 and 1.82% lower than for the comparison group of those aged just below 50 and who are therefore not eligible to receive any severance pay. All estimates are statistically significant at the 5% level or lower, except for the one obtained with four separate Linear Splines, which is significant at 6.5%. Focusing on our most basic control function, we obtain a point estimate of 1.44%, which implies a total effect of about 25% for the NOK 18,000 (after-tax) payment available to those individuals aged just above 50 in the most recent period.

To confirm that this coefficient is not just reflecting other, correlated factors not taken out by our control function, we repeat our analysis on the placebo sample. This has been obtained by imposing all the same restrictions as for our main sample, except that we now take only those plants that were not part of the LO-NHO scheme. Table 3 shows summary statistics for the two samples, which reveal that the individuals in the placebo sample have practically the same age, but tend to be somewhat more educated. Income and wealth differences however do are not significantly different economically. When we repeat the Cox regressions for this sample, displayed in Table 6, we find either a positive (instead of negative, as in the main sample), or statistically insignificant effect for all control functions, confirming the validity of our estimation strategy.

4.3 Focusing on the Discontinuity around Age 50

In addition to our main sample, which now focuses on individuals aged between 45 and 60, we have also conducted a more standard Regression Discontinuity analysis focusing only on those aged between 48 and 52, where the two-year bandwidth has been chosen with a view to the fact that, as discussed above, the severance pay amount increases again from age 52 onwards. The results for the resulting subsample are displayed in Table 9: Column (1), based on regressions without control function, implies an effect of 25%, which corresponds to what we have computed on the basis of the full sample above. When we add year fixed effects and respectively one 3rd-order polynomial in age for all 4 years or 2 separate ones for respectively those aged below and those aged above 50 on the day of job separation, this estimate increases to 46%, as displayed in columns (2) and (3). Columns (4) through (6) then repeat the same three analyses in a placebo setup in which we use individuals aged between 48 and 50, who are hence all ineligible for any severance pay, and estimate the effect of being aged 49 or higher. None of the three specifications finds an effect that is statistically significant at any reasonable level. Finally, columns (7) through (9) repeat again the same analysis now for a sample with the same age restrictions as columns (1) through (3), but now again for the placebo sample of individuals separating from non-LONHO plants. Again, no significant effect is found here, confirming the validity of the findings in our main sample.

4.4 Analyses of Completed Durations

Table 7 presents the results of our Censored Normal Regressions with completed job search duration as the dependent variable. For comparability, sample and control functions are exactly the same as in the Cox regressions reported in Table 4 above. Depending on the control function used, we find an effect of between 4 and 5.6 days. The point estimate obtained with our most basic specification, 4.2, implies a total effect of 75 days or about 2.5 months for the NOK 18,000 payment received at age 50. Table 8 explores how robust these findings are to how we censor the job search duration and shows that durations are still fairly robust to the maximum age but not to whether we censor after 6 months or 2 years of completed duration, with estimates obtained with early censoring amounting to only 1.5-1.8 instead of 4.0-5.6 days. That suggests that the point estimates obtained with Cox regressions are more reliable, even if the direction and statistical significance of the effect remain robust also here.

5 Mental Accounting instead of Liquidity Constraints?

In the previous sections 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 robustly present also in Norway, making it plausible that the finding applies also to other OECD economies. To this point we have simply adopted their interpretation of this reduced-form effect as evidence of liquidity constraints, but in this complementary section we shall discuss also a possible alternative interpretation based on Mental Accounting.

Since this alternative hypothesis is an adaptation to the context of job loss and severance payments of the ideas advanced in Shefrin and Thaler [1988], we start by summarizing the latter. At the core of that paper is the idea that individuals behave *as if* there coexisted two selves: A myopic "doer self" that is always 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 C, lower for A, and lowest for F. In practice, households are likely to have more than just those three accounts, and different households will have different accounts, for their kids' education or other purposes. 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.

Building on this categorization, we suggest that if households do indeed classify money into I, A or F and are more willing to consume out of I than out of A or F, then the consumption of job-search time is likely to respond to severance pay even for households who are not formally, or externally, liquidity-constrained, because the severance pay, in contrast to prior savings, is likely to be classified as "Current Income", seeing that it is paid out after lay-off precisely when regular income drops to the UI replacement rate. Put differently, households may implicitly understand the severance payments as specifically intended to be used for maintaining consumption while searching for an adequate new job, whereas prior savings are instead understood to be reserved for different purposes such as retirement or children's education. In the words of Shefrin and Thaler [1988]: "households treat components of their wealth as nonfungible, even in the absence of credit rationing." It is worth noting that conceptually Mental Accounting is in fact quite similar to standard liquidity constraints in that 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 firstly that Mental Accounting arises through constraints that are internal rather than external, and secondly that – given the individual's temptation to spend excessively absent any commitment devices – the internal constraints are optimal as a second-best solution.[7]

Just like the problem of liquidity constraints has a first-best policy solution, lending, and a possible second-best, increasing UI, there are several possible policy responses to the phenomenon of Mental Accounting. The first-best, given individuals' risk of myopia, would be to provide an external commitment device that still constrains myopic spending but does a better job at allowing higher spending if and only if that can be expected to increase lifetime utility, for instance by allowing the individual to find a financially or otherwise better subsequent job. But if, plausibly, such a policy is not possible, we are back with the same policy options as for Liquidity Constraints. So the policy responses to the finding of severance pay effects are overall similar, nonetheless a test of whether this effect stems mostly from Liquidity Constraints or mostly from Mental Accounting can improve our understanding of what exactly is happening within the affected households.

To do so, we have stratified our sample by terciles of the wealth distribution, where the terciles are defined separately within each calendar year. We stratified in turn by last year's income, total wealth, financial wealth (total wealth net of real estate holdings), and deposits, always first at the level of the husband only and then at the level of the total household, adding the assets of the wife, if any. All measures have first been winsorized at the 99th percentile to

[7] There is also a third possible interpretation of a reduced-form effect of severance pay on job-search duration: If the payment makes households richer, and risk aversion is decreasing in wealth, then it could make the recipients more willing to reject a merely moderately good early offer in order to wait for the next one, which might either be better or worse than the one received early on. In our context, where the payment amounts only to 1 month's wages for the typical household, an effect through lifetime wealth seems less likely to be the correct interpretation though.

take care of outliers. Table 10 shows a summary of the wealth measures by their respective terciles. Then we re-estimate our previous Cox regressions, but interact the severance pay variable with indicators for being in the second or third terciles, and also control for the main effect of being in those terciles. The interaction coefficient can then be read to inform us whether the size of the severance pay effect varies across the three terciles. The results of these analyses are displayed in Tables **??** and .12. We see that the size of the severance pay effect is monotonously decreasing in wealth, household wealth, household financial wealth, but not in individual financial wealth, deposits, household deposits or any of these measures scaled by previous income. Also, none of the interaction coefficients is statistically significant at the conventional levels, except for that for scaled household financial wealth, where however the pattern is not monotonous. Overall, the findings do if anything provide some slight support for the Liquidity Constraints interpretation, but no conclusive conclusions on the issue are possible. The same finding is reached when we stratify into halves or quartiles instead of terciles, or when we interact our severance pay variable with continuous measures of wealth. Hence, while we have robustly shown an effect of lump-sum severance payments, further research is needed on which of the interpretations discussed here is the most appropriate.

6 Conclusion

We have shown a clear 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 an effect, 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. We have then discussed how this can be interpreted either as evidence of liquidity constraints, as in the previous literature, or alternatively as evidence of Mental Accounting behavior. No definitive conclusion could be reached on the which of these is the more appropriate interpretation, so that this issue will require further research in the future. Nonetheless, we can infer that slightly more generous unemployment benefits are likely to solve the problems faced by the households affected, although it may not be the first-best policy response.

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7 Appendix I: Figures and Tables

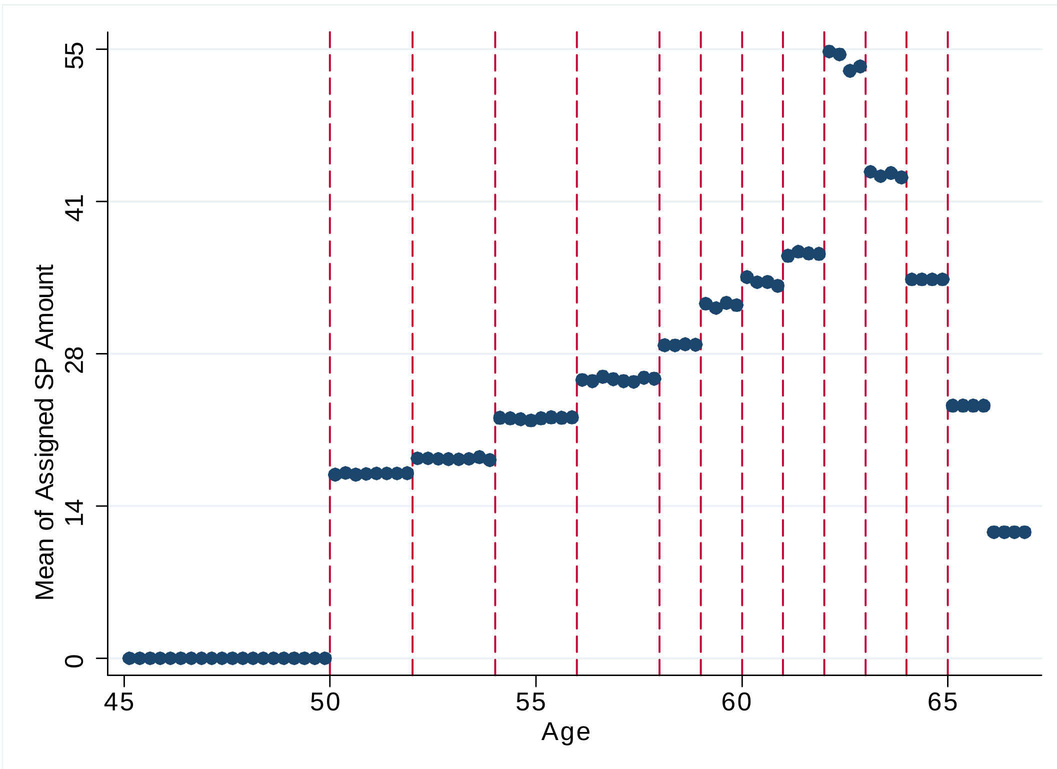

Figure 1: Average (across the 4 periods) amounts individuals of different ages were eligible for, following Table 1, in NOK 1,000.

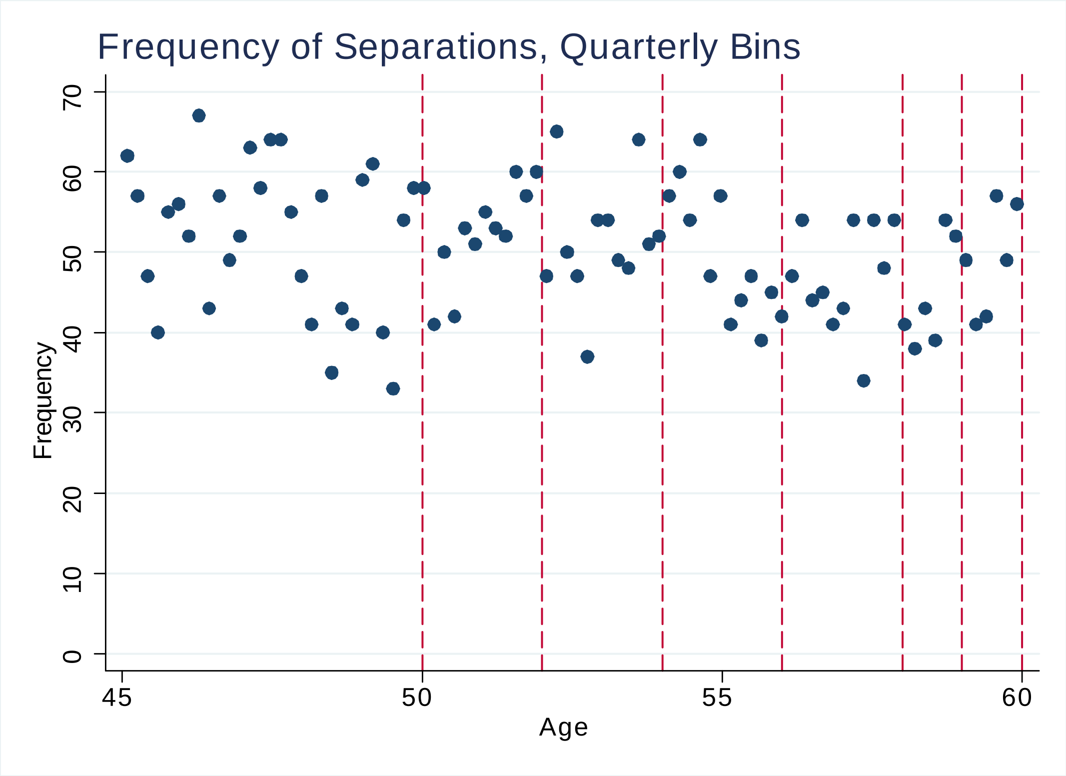

Figure 2: Frequency of job separations in the sample used for our main analyses, male and with at least 10 years of tenure. Bin size 3 months.

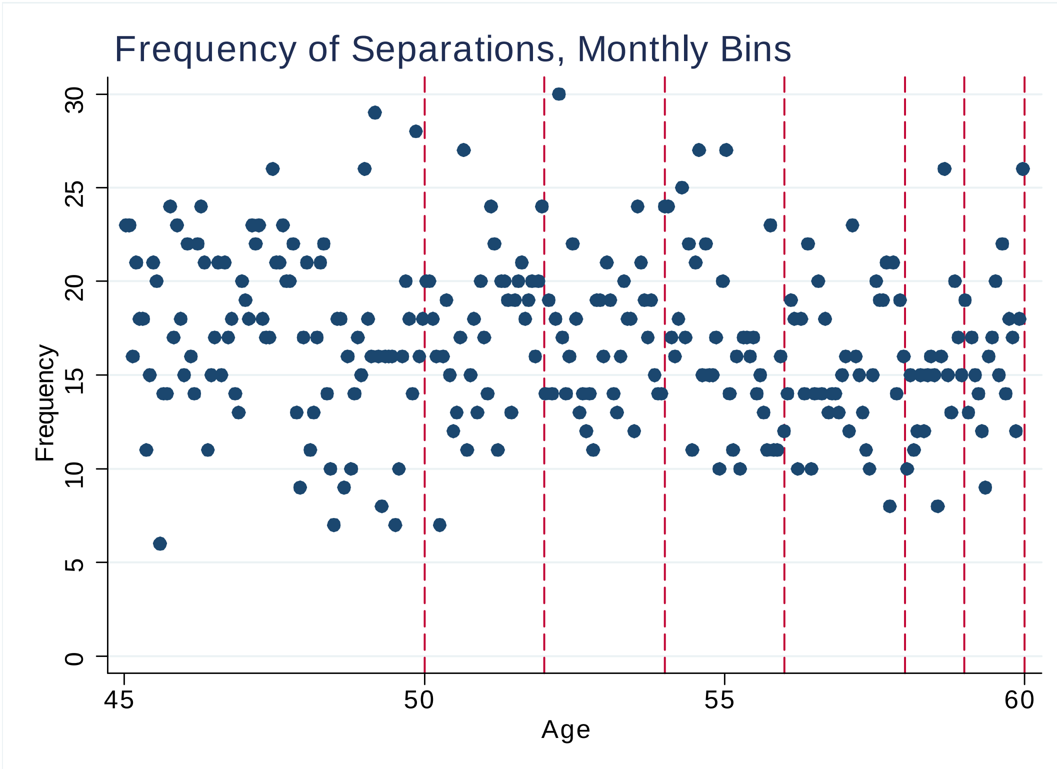

Figure 3: Frequency of job separations in the sample used for our main analyses, male and with at least 10 years of tenure. Bin size 1 month.

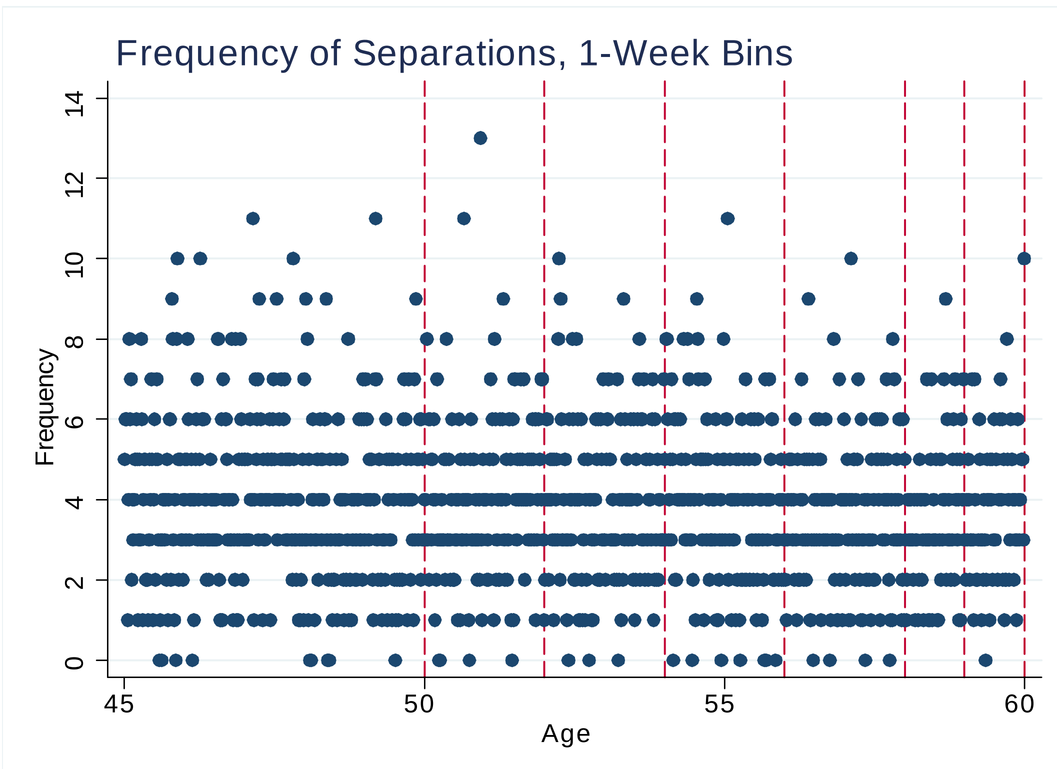

Figure 4: Frequency of job separations in the sample used for our main analyses, male and with at least 10 years of tenure. Bin size 1 week.

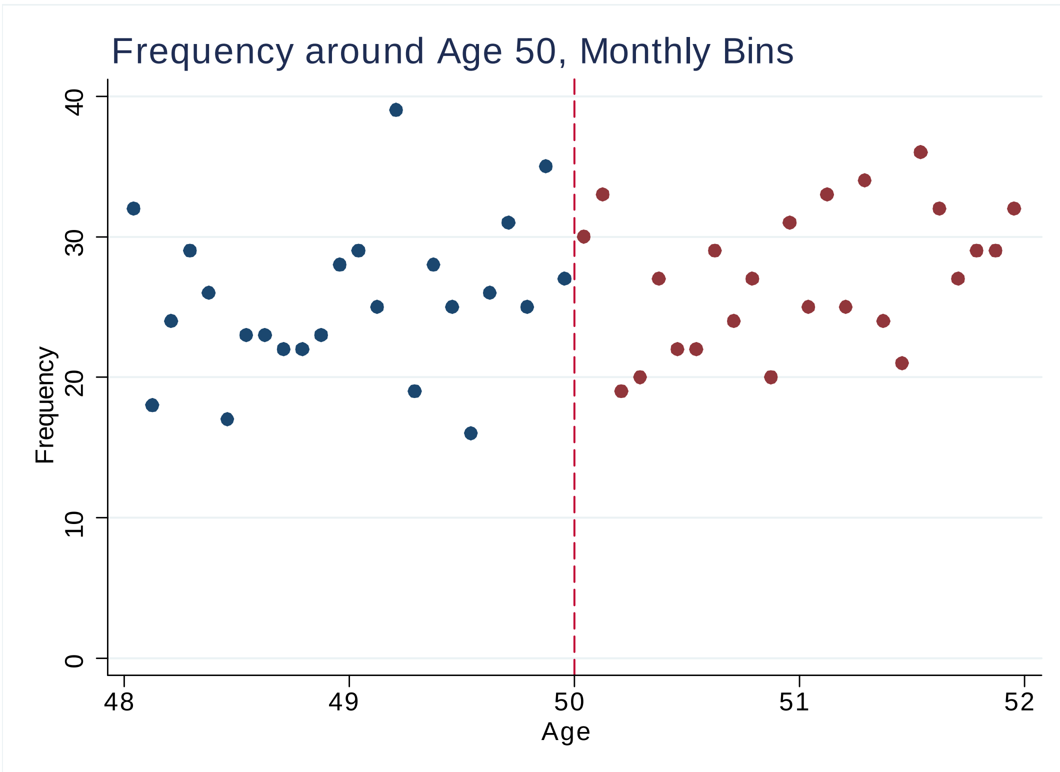

Figure 5: Frequency of job separations in the sample used for our complementary RD analysis around the age 50 threshold. Bin size 1 month.

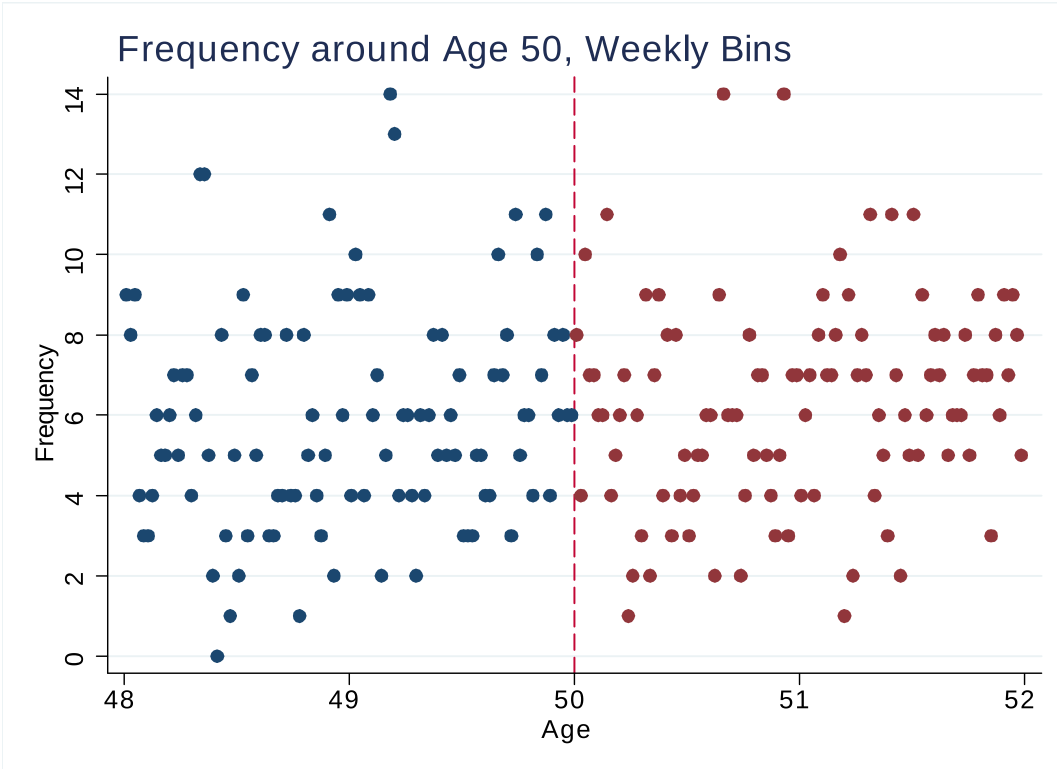

Figure 6: Frequency of job separations in the sample used for our complementary RD analysis around the age 50 threshold. Bin size 1 week.

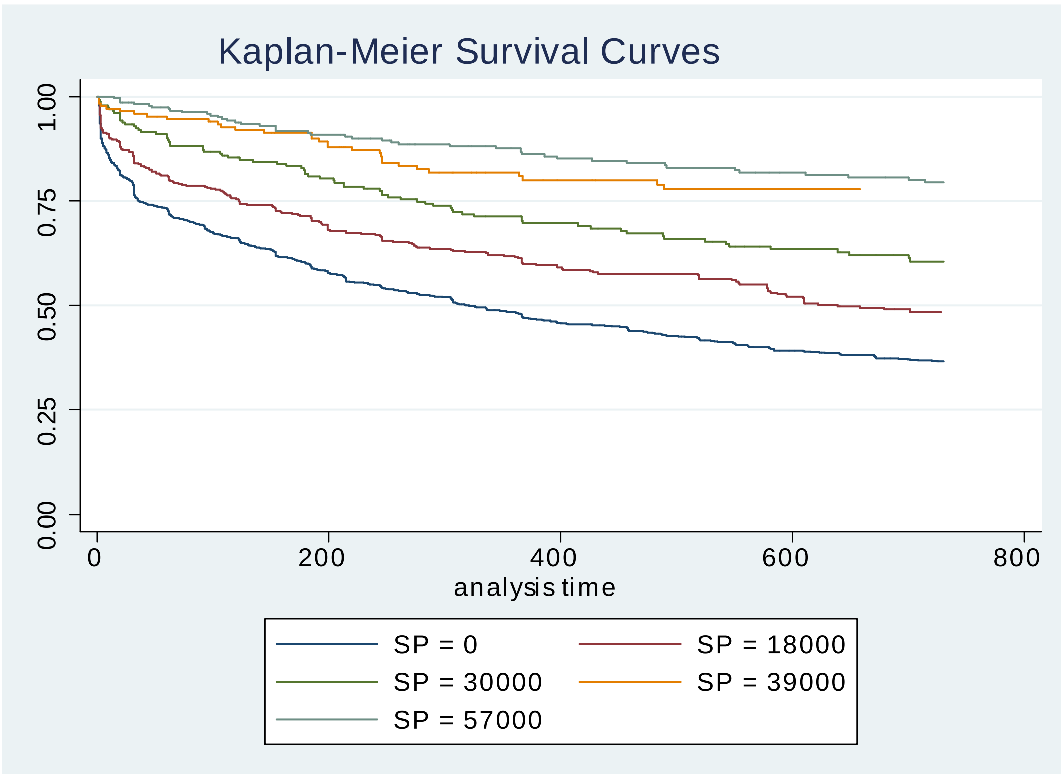

Figure 7: Nonparametric Kaplan-Meier Survival Curves, displaying for each day the fraction of individuals still without a job. Different curves for individuals eligible for different amounts of severance pay in NOK.

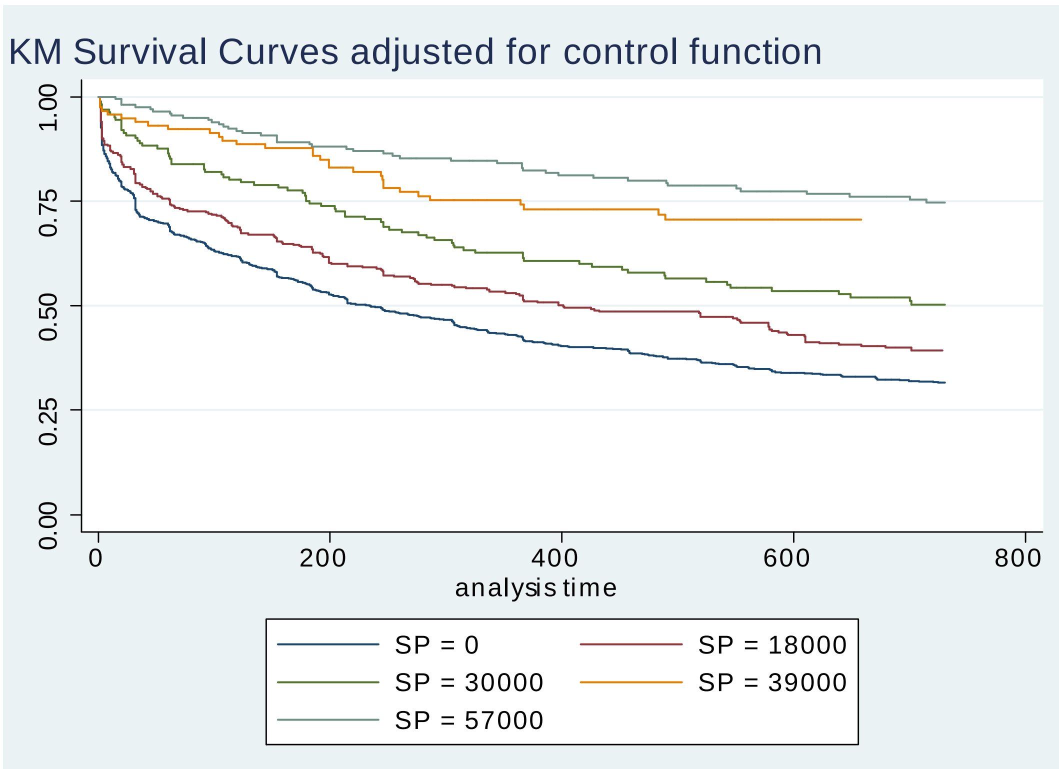

Figure 8: We estimated a Cox Model on a third polynomial in the deviation of age from its sample average of 52 and a set of calendar year fixed effects, with year 2000 as omitted category; Using the baseline hazard from this estimation, we then plot the non-parametric Kaplan-Meier Survival Curves.

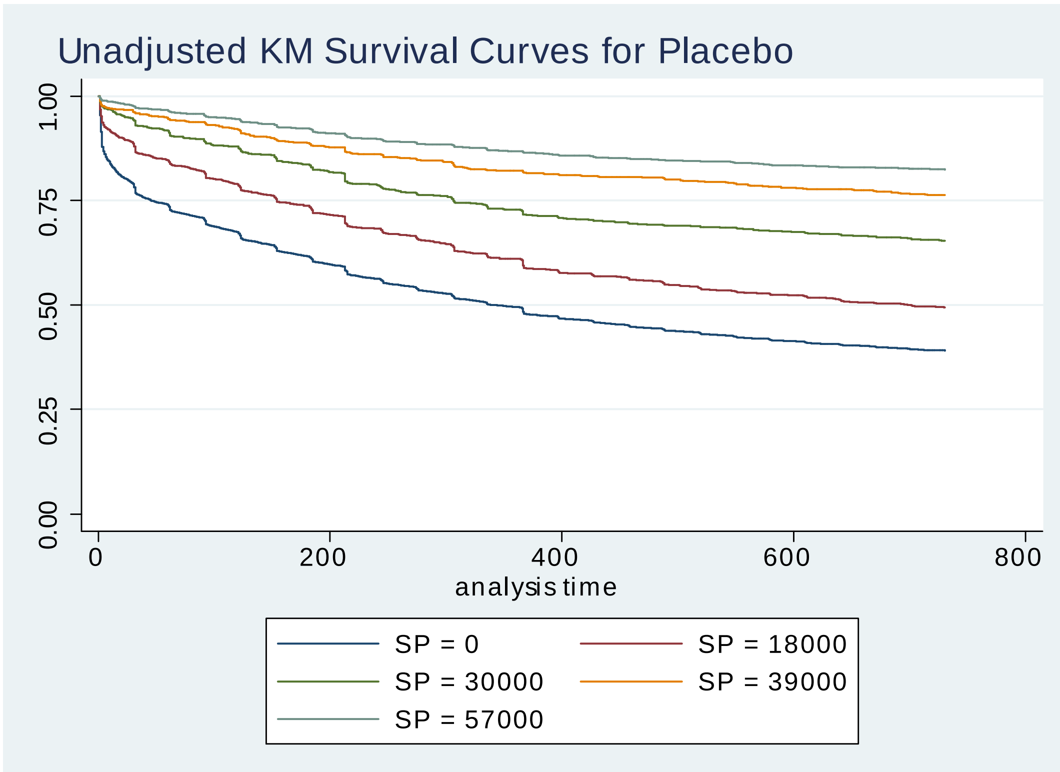

Figure 9: Non-parametric Kaplan-Meier Curves, as in Figure 7, but now for the placebo sample of individuals separated from plants that were not part of the severance pay agreement.

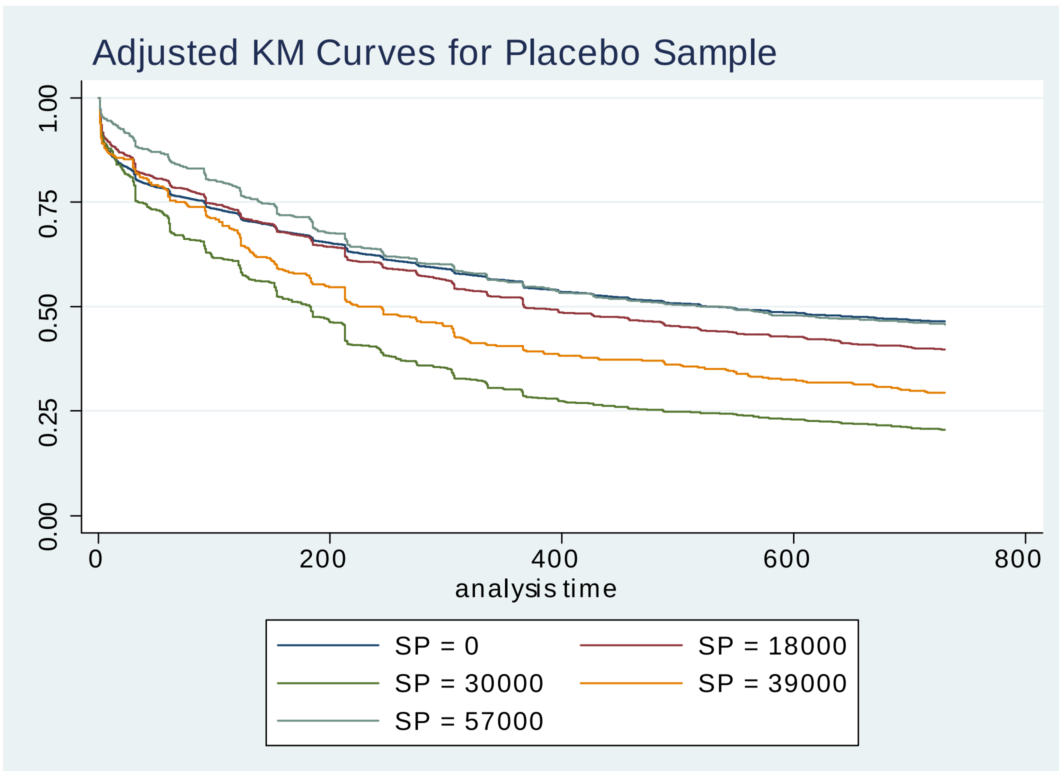

Figure 10: Semi-parametric Survival Curves, as in Figure 8, but now for the placebo sample of individuals separated from plants that were not part of the severance pay agreement.

Table 1: Severance Pay Amounts by Age and Period in NOK

Age	01oct1993-30sep1995	1oct1995-28feb1998	01mar1998-31jul2002	01aug2002 ff
<= 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

Table 2: Summary Statistics: Full Sample vs. Those known to have at least 10 years of Tenure

	All					Tenure>=10					Compared
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	T
Age	34,111	51.444	4.215	45	59.99726	4,362	52.320	4.320	45	59.99726	-12.65
Year 1994	34,111	0.014	0.116	0	1	4,362	0.000	0.000	0	0	
Year 1995	34,111	0.021	0.144	0	1	4,362	0.009	0.093	0	1	
Year 1996	34,111	0.028	0.165	0	1	4,362	0.011	0.105	0	1	
Year 1997	34,111	0.036	0.185	0	1	4,362	0.018	0.133	0	1	
Year 1998	34,111	0.045	0.207	0	1	4,362	0.027	0.163	0	1	
Year 1999	34,111	0.051	0.220	0	1	4,362	0.027	0.163	0	1	
Year 2000	34,111	0.061	0.238	0	1	4,362	0.038	0.190	0	1	
Year 2001	34,111	0.062	0.241	0	1	4,362	0.039	0.193	0	1	
Year 2002	34,111	0.086	0.280	0	1	4,362	0.153	0.360	0	1	
Year 2003	34,111	0.088	0.284	0	1	4,362	0.091	0.287	0	1	
Year 2004	34,111	0.090	0.287	0	1	4,362	0.089	0.284	0	1	
Year 2005	34,111	0.093	0.291	0	1	4,362	0.096	0.294	0	1	
Year 2006	34,111	0.107	0.309	0	1	4,362	0.124	0.330	0	1	
Year 2007	34,111	0.121	0.326	0	1	4,362	0.167	0.373	0	1	
Year 2008	34,111	0.096	0.295	0	1	4,362	0.112	0.315	0	1	
Tenure	34,111	4.660	5.424	0	36.496	4,362	15.863	5.796	10	36.496	-121.06
Mean Inc	32,880	344,840	179,921	15,700	1,105,624	4,324	393,299	176,224	61,100	1,105,624	-16.96
Inc	32,880	362,933	191,269	40,500	1,188,900	4,324	404,975	187,176	56,400	1,188,900	-13.85
HH Inc	32,880	479,832	265,727	40,500	1,554,400	4,324	543,098	261,984	56,400	1,554,400	-14.90
Wealth	32,880	480,186	634,460	0	4,073,107	4,324	662,732	805,354	0	4,073,107	-14.33
HH Wealth	32,880	614,124	807,661	0	5,794,015	4,324	841,997	1,044,172	0	5,794,015	-13.82
FinW	32,880	169,711	398,766	0	2,666,576	4,324	264,024	551,158	0	2,666,576	-10.88
HH FinW	32,880	232,073	531,822	0	4,018,066	4,324	361,737	751,264	0	4,018,066	-10.99
Deposits	32,880	104,945	225,877	0	1,448,589	4,324	145,241	276,454	0	1,448,589	-9.19
HH Deposits	32,880	145,921	289,374	0	1,959,351	4,324	201,553	355,026	0	1,959,351	-9.88
Edu: General	30,953	0.361	0.480	0	1	3,998	0.335	0.472	0	1	
Edu: Humanities	30,953	0.016	0.125	0	1	3,998	0.012	0.108	0	1	
Edu: Teaching	30,953	0.013	0.112	0	1	3,998	0.012	0.109	0	1	
Edu: Social-Legal	30,953	0.009	0.094	0	1	3,998	0.010	0.101	0	1	
Edu: Econ/Adm	30,953	0.086	0.281	0	1	3,998	0.103	0.304	0	1	
Edu: Science/Eng	30,953	0.423	0.494	0	1	3,998	0.444	0.497	0	1	
Edu: Health/Sports	30,953	0.007	0.085	0	1	3,998	0.006	0.076	0	1	
Edu: Primary	30,953	0.033	0.180	0	1	3,998	0.032	0.175	0	1	
Edu: Services	30,953	0.051	0.221	0	1	3,998	0.046	0.210	0	1	
Edu: None	31,195	0.002	0.049	0	1	4,027	0.003	0.057	0	1	
Edu: Primary	31,195	0.002	0.047	0	1	4,027	0.002	0.042	0	1	
Edu: Middle School	31,195	0.270	0.444	0	1	4,027	0.231	0.422	0	1	
Edu: High Sch. 1st y.	31,195	0.243	0.429	0	1	4,027	0.250	0.433	0	1	
Edu: High Sch., full	31,195	0.271	0.444	0	1	4,027	0.296	0.457	0	1	
Edu: High Sch. Plus	31,195	0.048	0.214	0	1	4,027	0.045	0.207	0	1	
Edu: Bachelors	31,195	0.117	0.322	0	1	4,027	0.115	0.320	0	1	
Edu: Masters	31,195	0.041	0.197	0	1	4,027	0.048	0.214	0	1	
Edu: Doctorate	31,195	0.005	0.070	0	1	4,027	0.009	0.095	0	1	
Colleagues	15,386	201	463	1	3,154	2,201	200	480	1	3,154	0.04
Downsizing	15,386	0.312	0.300	0.003	1	2,201	0.309	0.318	0.003	1	0.51

Table 3: Summary Statistics: Main vs. Placebo Sample

	Main					Placebo					Compared
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	T
Age	4,362	52.320	4.320	45	59.99726	31,864	52.741	4.389	45.000	59.997	-6.02
Year 1994	4,362	0.000	0.000	0	0	31,864	0	0	0	0	
Year 1995	4,362	0.009	0.093	0	1	31,864	0.034	0.181	0	1	
Year 1996	4,362	0.011	0.105	0	1	31,864	0.042	0.200	0	1	
Year 1997	4,362	0.018	0.133	0	1	31,864	0.056	0.229	0	1	
Year 1998	4,362	0.027	0.163	0	1	31,864	0.041	0.198	0	1	
Year 1999	4,362	0.027	0.163	0	1	31,864	0.046	0.210	0	1	
Year 2000	4,362	0.038	0.190	0	1	31,864	0.048	0.214	0	1	
Year 2001	4,362	0.039	0.193	0	1	31,864	0.054	0.225	0	1	
Year 2002	4,362	0.153	0.360	0	1	31,864	0.116	0.320	0	1	
Year 2003	4,362	0.091	0.287	0	1	31,864	0.093	0.291	0	1	
Year 2004	4,362	0.089	0.284	0	1	31,864	0.096	0.295	0	1	
Year 2005	4,362	0.096	0.294	0	1	31,864	0.081	0.272	0	1	
Year 2006	4,362	0.124	0.330	0	1	31,864	0.095	0.293	0	1	
Year 2007	4,362	0.167	0.373	0	1	31,864	0.119	0.324	0	1	
Year 2008	4,362	0.112	0.315	0	1	31,864	0.080	0.271	0	1	
Tenure	4,362	15.863	5.796	10	36.496	31,864	16.986	6.137	10	40.80835	-11.91
Mean Inc	4,324	393,299	176,224	61,100	1,105,624	30,778	390,163	184,722	20,000	1,024,734	1.09
Inc	4,324	404,975	187,176	56,400	1,188,900	30,778	401,644	195,349	40,700	1,082,600	1.09
HH Inc	4,324	543,098	261,984	56,400	1,554,400	30,778	543,985	279,042	40,700	1,603,600	-0.21
Wealth	4,324	662,732	805,354	0	4,073,107	30,778	684,249	808,408	0	4,145,446	-1.64
HH Wealth	4,324	841,997	1,044,172	0	5,794,015	30,778	895,870	1,152,863	0	7,082,675	-3.13
FinW	4,324	264,024	551,158	0	2,666,576	30,778	287,524	563,482	0	2,800,465	-2.62
HH FinW	4,324	361,737	751,264	0	4,018,066	30,778	402,139	812,366	0	4,876,388	-3.28
Deposits	4,324	145,241	276,454	0	1,448,589	30,778	154,045	285,149	0	1,451,702	-1.95
HH Deposits	4,324	201,553	355,026	0	1,959,351	30,778	217,435	376,121	0	2,108,490	-2.73
Edu: General	3,998	0.335	0.472	0	1	28,240	0.291	0.454	0	1	
Edu: Humanities	3,998	0.012	0.108	0	1	28,240	0.033	0.179	0	1	
Edu: Teaching	3,998	0.012	0.109	0	1	28,240	0.047	0.211	0	1	
Edu: Social-Legal	3,998	0.010	0.101	0	1	28,240	0.019	0.137	0	1	
Edu: Econ/Adm	3,998	0.103	0.304	0	1	28,240	0.116	0.320	0	1	
Edu: Science/Eng	3,998	0.444	0.497	0	1	28,240	0.365	0.481	0	1	
Edu: Health/Sports	3,998	0.006	0.076	0	1	28,240	0.028	0.166	0	1	
Edu: Primary	3,998	0.032	0.175	0	1	28,240	0.019	0.136	0	1	
Edu: Services	3,998	0.046	0.210	0	1	28,240	0.083	0.275	0	1	
Edu: None	4,027	0.003	0.057	0	1	28,401	0.001	0.038	0	1	
Edu: Primary	4,027	0.002	0.042	0	1	28,401	0.002	0.041	0	1	
Edu: Middle School	4,027	0.231	0.422	0	1	28,401	0.182	0.386	0	1	
Edu: High Sch. 1st y.	4,027	0.250	0.433	0	1	28,401	0.248	0.432	0	1	
Edu: High Sch., full	4,027	0.296	0.457	0	1	28,401	0.258	0.437	0	1	
Edu: High Sch. Plus	4,027	0.045	0.207	0	1	28,401	0.041	0.198	0	1	
Edu: Bachelors	4,027	0.115	0.320	0	1	28,401	0.184	0.388	0	1	
Edu: Masters	4,027	0.048	0.214	0	1	28,401	0.078	0.268	0	1	
Edu: Doctorate	4,027	0.009	0.095	0	1	28,401	0.006	0.078	0	1	
Colleagues	2,201	200	480	1	3,154	15,732	424	962	1	6,277	-17.48
Downsizing	2,201	0.309	0.318	0.003	1	15,732	0.329	0.351	0.001	1	-2.79

Table 4: Cox Proportional Hazards Duration Model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1 Polyn.	No Return	By Age	4 Polyn.	LS	RCS	4 LS	4 RCS
SP	-1.44	-1.3	-1.3	-1.82	-1.44	-1.25	-1.41	-1.2
	(0.018)**	(0.041)**	(0.245)	(0.004)***	(0.041)**	(0.031)**	(0.065)*	(0.049)**
Age	-485.3	-397.6	-781.7					
	(0.142)	(0.258)	(0.488)					
Age2	9.63	7.96	15.9					
	(0.131)	(0.241)	-(0.477)					
Age3	-0.0636	-0.0532	-0.108					
	(0.119)	(0.221)	(0.465)					
SP*Aged			0.181					
			(0.524)					
SP*Aged2			0.034					
			(0.719)					
SP*Aged3			-0.00206					
			(0.789)					
N	4,362	4,078	4,362	4,362	4,362	4,362	4,362	4,362

Outcome: Effect of each NOK1,000 of SP on propensity to start new job in 24 months after job loss, in %
All columns control for year effects; (4) has a separate polynomial in age for each of the 4 period displayed in Table 1
(5) - (8) control for a Linear Spline, a Restricted Cubic Spline, 4 Linear Splines, 4 RCS (for details, see text).
P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 5: Baseline regression, censoring at different ages and durations

	67		62		60	
	2y	6m	2y	6m	2y	6m
SP	-1.03	-1.39	-1.22	-1.43	-1.44	-1.57
	(0.015)**	(0.012)**	(0.033)**	(0.046)**	(0.018)**	(0.038)**
Age	-209.8	-207.3	-419.6	-496.9	-485.3	-497.6
	(0.117)	(0.229)	(0.064)*	(0.073)*	(0.142)	(0.209)
Age2	4.23	4.2	8.3	9.77	9.63	9.83
	(0.089)*	(0.192)	(0.054)*	(0.064)*	(0.131)	(0.198)
Age3	-0.0287	-0.0285	-0.0548	-0.0641	-0.0636	-0.0647
	(0.062)*	(0.150)	(0.044)**	(0.054)*	(0.119)	(0.185)
N	5,452	5,452	4,876	4,876	4,362	4,362

Estimation of baseline Equation 4; Duration censored at ages 67/62/60 and after 2 or 0.5 years as indicated
Outcome: Effect of each NOK 1,000 of SP on propensity to start new job in %; All columns control
for year FEs; P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 6: Cox Proportional Hazards Duration Model for the Placebo Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Basic	No Return	By Age	4 Polys	LS	RCS	4 LS	4 RCS
SP	0.539	0.66	1.41	-0.0179	0.335	0.294	-0.0269	0.091
	(0.017)**	(0.007)***	(0.003)***	(0.940)	(0.197)	(0.189)	(0.926)	(0.702)
Age	8.41	66.9	-17.7					
	(0.946)	(0.619)	(0.967)					
Age2	0.0937	-1.03	1.23					
	(0.969)	(0.693)	(0.884)					
Age3	-0.00318	0.00382	-0.0152					
	(0.836)	(0.819)	(0.784)					
SP*Aged			0.39					
			(0.000)***					
SP*Aged2			-0.0343					
			(0.303)					
SP*Aged3			0.00438					
			(0.123)					
N	31,864	29,135	31,864	31,864	31,864	31,864	31,864	31,864

Same regressions as in Table 4, but for the Placebo Sample of individuals separated from planss that were not a member of the severance pay scheme.

Table 7: Completed Durations

	(1) Basic	(2) No Returners	(3) By Age	(4) 4 Polys	(5) LS	(6) RCS
SP	4.20	3.98	4.69	5.35	5.62	4.76
	(0.051)*	(0.091)*	(0.242)	(0.016)**	(0.027)**	(0.022)**
Age	-83.19	-609.30	2680.30			
	(0.939)	(0.614)	(0.487)			
Age2	1.58	11.72	-54.55			
	(0.940)	(0.615)	(0.475)			
Age3	-0.01	-0.07	0.37			
	(0.947)	(0.621)	(0.461)			
SP*Age			-0.57			
			(0.572)			
SP*Age2			0.00			
			(0.993)			
SP*Age3			-0.02			
			(0.443)			
Cons	1760.20	10778.10	-43501.50	364.50	44.89	180.50
	(0.925)	(0.604)	(0.500)	(0.000)***	(0.910)	(0.639)
Sig Cons	453.30	477.90	452.80	451.50	452.70	452.80
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
N	4,362	4,078	4,362	4,362	4,362	4,362

Outcome: Effect of each NOK 1,000 of severance pay on duration until new job in days
Censored Normal Regressions, using the same control functions as in Table 4.
P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 8: Completed Durations Regressions with Different Censorings

	67		62		60	
	2y	6m	2y	6m	2y	6m
SP	4.30	1.49	4.22	1.75	4.43	1.71
	(0.003)***	(0.008)***	(0.034)**	(0.026)**	(0.032)**	(0.038)**
Age	519.10	128.60	444.80	382.50	-127.30	136.80
	(0.256)	(0.460)	(0.556)	(0.194)	(0.903)	(0.745)
Age2	-10.73	-2.74	-9.07	-7.62	2.43	-2.84
	(0.205)	(0.396)	(0.527)	(0.174)	(0.904)	(0.726)
Age3	0.07	0.02	0.06	0.05	-0.01	0.02
	(0.151)	(0.321)	(0.489)	(0.150)	(0.910)	(0.702)
Cons	-7,946.20	-1,831.50	-6,978.80	-6,262.80	2,530.40	-2,029.60
	(0.330)	(0.555)	(0.596)	(0.224)	(0.888)	(0.779)
Sig Cons	478.40	167.00	463.90	165.20	448.50	163.00
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
N	5,870	5,870	5,192	5,192	4,666	4,666

As in Table 7, but censoring at different ages and durations as indicated.

Table 9: Proportional Hazard Analyses around the Age 50 Discontinuity

	Main Sample			Age 49 Placebo			Non-EP Placebo		
	No Controls	1 Poly	2 Polys	NC	1 Poly	2 Polys	NC	1 Poly	2 Polys
T	-25.3	-46	-46.6	-0.12	16.3	16.3	-6.81	-1.15	2.02
	(0.001)***	(0.031)**	(0.160)	(0.990)	(0.446)	(0.446)	(0.023)**	(0.888)	(0.863)
Aged		25.8	-23.8		-14	-14		-3.38	59.9
		(0.124)	(0.813)		(0.456)	(0.456)		(0.609)	(0.113)
Aged2		2.48	-55.2		2.39	2.39		-0.762	67.8
		(0.452)	(0.636)		(0.873)	(0.873)		(0.555)	(0.122)
Aged3		-7.75	-26		3.85	3.85		0.236	21
		(0.066)*	(0.491)		(0.675)	(0.675)		(0.889)	(0.145)
T*Aged			100.1						-149
			(0.486)						(0.004)***
T*Aged2			-0.0884						29.2
			(1.000)						(0.634)
T*Aged3			36.4						-51.5
			-(0.500)						(0.011)**
N	1,160	1,160	1,160	884	884	884	8,243	8,243	8,243

Effect of being aged >=50 (1st placebo: 49) on propensity to start new job in 2 yrs after job loss, in %; Bandwidth ages 48-52 (placebo: 47-50); "Aged" is deviation of Age from 50 (49); Besides the main sample analyses have been repeated for the placebo sample aged between 47 and 50 and who are hence all too young to get SP, and for a placebo sample of individuals from plants not participating in the SP agreement

For each we have one specification without controls, one with the 3rd-order polynomial in age plus year effects, and one with separate control functions for left and right of the threshold; P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 10: Males' Income and Wealth by Tercile

		N	Mean	SD	Min	Max
Income	All	4,324	404,975	187,176	56,400	1,188,900
	1	1,558	261,452	41,950	56,400	315,800
	2	1,437	364,206	30,965	315,900	424,400
	3	1,329	617,309	199,660	425,000	1,188,900
Wealth	All	4,324	662,732	805,354	0	4,073,107
	1	1,571	172,596	99,010	0	323,394
	2	1,445	456,248	86,432	323,570	628,129
	3	1,308	1,479,531	1,059,094	628,523	4,073,107
HH Wealth	All	4,324	841,997	1,044,172	0	5,794,015
	1	1,605	235,719	127,340	0	422,153
	2	1,419	586,690	110,497	422,304	803,986
	3	1,300	1,869,195	1,419,078	804,742	5,794,015
Financial W.	All	4,324	264,024	551,158	0	2,666,576
	1	1,581	9,725	8,498	0	28,554
	2	1,453	76,019	37,593	28,696	163,204
	3	1,290	787,448	789,786	163,411	2,666,576
HH Fin.W.	All	4,324	361,737	751,264	0	4,018,066
	1	1,604	18,187	14,893	0	51,033
	2	1,455	127,431	57,962	51,186	250,259
	3	1,265	1,066,851	1,102,543	250,552	4,018,066
Deposits	All	4,324	145,241	276,454	0	1,448,589
	1	1,575	6,756	5,977	0	19,578
	2	1,471	51,474	24,157	19,587	106,854
	3	1,278	423,836	382,821	106,876	1,448,589
HH Dep.	All	4,324	201,553	355,026	0	1,959,351
	1	1,598	13,215	10,605	0	36,164
	2	1,483	89,484	40,120	36,219	178,194
	3	1,243	577,388	484,408	178,856	1,959,351
Wealth/Inc	All	4,324	0.710	1.638	0.000	9.903
	1	1,574	0.028	0.024	0.000	0.078
	2	1,455	0.203	0.094	0.078	0.410
	3	1,295	2.107	2.479	0.410	9.903
HH W / Inc	All	4,324	0.989	2.441	0.000	19.422
	1	1,594	0.052	0.041	0.000	0.141
	2	1,443	0.335	0.144	0.141	0.652
	3	1,287	2.884	3.853	0.654	19.422
FinW / Inc	All	4,324	1.833	2.560	0.000	18.030
	1	1,532	0.477	0.266	0.000	0.879
	2	1,434	1.243	0.228	0.879	1.688
	3	1,358	3.988	3.694	1.689	18.030
HH FinW / Inc	All	4,324	2.345	3.725	0.000	37.256
	1	1,562	0.658	0.346	0.000	1.167
	2	1,416	1.590	0.267	1.168	2.113
	3	1,346	5.097	5.735	2.115	37.256
Dep / Inc	All	4,324	0.396	0.866	0.000	6.587
	1	1,553	0.019	0.017	0.000	0.056
	2	1,486	0.134	0.060	0.056	0.272
	3	1,285	1.155	1.302	0.272	6.587
HH Dep / Inc	All	4,324	0.556	1.171	0.000	10.602
	1	1,587	0.038	0.029	0.000	0.098
	2	1,475	0.229	0.097	0.099	0.444
	3	1,262	1.590	1.777	0.444	10.602

Table 11: Proportional Hazard Analyses stratified by Terciles of Wealth

	Inc	Wealth	HH Wealth	FinW	HH FinW	Deposits	HH Dep.
SP	-1.620	-1.760	-1.730	-1.510	-1.860	-1.420	-1.600
	(0.020)**	(0.009)***	(0.010)***	(0.025)**	(0.005)***	(0.035)**	(0.017)**
SP*T2	0.172	0.234	0.303	-0.247	0.180	-0.610	-0.190
	(0.725)	(0.621)	(0.518)	(0.600)	(0.703)	(0.197)	(0.683)
SP*T3	0.190	0.565	0.510	0.305	0.656	0.417	0.419
	(0.699)	(0.265)	(0.309)	(0.544)	(0.193)	(0.403)	(0.409)
Age	-0.033	-0.075	-0.296	-0.170	0.189	-0.029	0.003
	(0.989)	(0.976)	(0.903)	(0.944)	(0.938)	(0.991)	(0.999)
Age2	-0.343	-0.372	-0.354	-0.357	-0.377	-0.355	-0.363
	(0.023)**	(0.014)**	(0.019)**	(0.018)**	(0.013)**	(0.019)**	(0.016)**
Age3	-0.074	-0.074	-0.071	-0.069	-0.074	-0.072	-0.072
	(0.075)*	(0.074)*	(0.084)*	(0.094)*	(0.073)*	(0.084)*	(0.083)*
T2	14.600	18.400	23.500	25.100	18.800	25.200	22.500
	(0.078)*	(0.016)**	(0.002)***	(0.001)***	(0.013)**	(0.001)***	(0.003)***
T3	25.300	-0.829	4.150	2.890	-0.397	-2.050	0.066
	(0.002)***	(0.923)	(0.630)	(0.738)	(0.963)	(0.813)	(0.994)
N	4,324	4,324	4,324	4,324	4,324	4,324	4,324

We repeat the regressions from Table 4, (1), but interact the assigned SP amount with indicators for the 3 terciles (T1-3) of different measures of income and wealth: Annual income, total wealth, financial wealth (stocks, bonds, cash, but not real estate) and deposits, each at the individual and the household level. Aged is the deviation of age from the sample mean. P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 12: Proportional Hazard Analyses stratified by Terciles of Wealth over Income

	Wealth/Inc	HHW/Inc	Finw/Inc	HHFinw/Inc	Dep/Inc	HHDep/Inc
SP	-1.570	-1.990	-1.970	-2.170	-1.580	-1.750
	(0.018)**	(0.003)***	(0.003)***	(0.001)***	(0.018)**	(0.008)***
SP*T2	-0.250	0.646	0.767	1.290	-0.336	0.327
	(0.593)	(0.170)	(0.111)	(0.007)***	(0.475)	(0.485)
SP*T3	0.287	0.546	0.562	0.522	0.578	0.322
	(0.567)	(0.279)	(0.255)	(0.291)	(0.250)	(0.520)
Age	0.132	0.373	-0.045	0.122	0.048	0.113
	(0.957)	(0.878)	(0.985)	(0.960)	(0.984)	(0.963)
Age2	-0.364	-0.380	-0.361	-0.379	-0.367	-0.352
	(0.016)**	(0.012)**	(0.017)**	(0.012)**	(0.015)**	(0.019)**
Age3	-0.070	-0.078	-0.072	-0.077	-0.069	-0.073
	(0.088)*	(0.057)*	(0.082)*	(0.062)*	(0.093)*	(0.077)*
T2	26.400	12.800	1.620	4.660	21.900	9.330
	(0.001)***	(0.094)*	(0.836)	(0.555)	(0.004)***	(0.221)
T3	-0.605	-2.510	-2.370	1.540	-7.640	-2.380
	(0.944)	(0.770)	(0.774)	(0.850)	(0.385)	(0.782)
N	4,324	4,324	4,324	4,324	4,324	4,324

As Table 11, but interacting with the terciles of different wealth measures scaled by annual income.
P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

8 Appendix II: Computation of the Spline Control Functions

The Linear Spline consists of elements $V_{i,}$ for $i = 1, ..., n$ which – given the underlying continuous variable V (i.e. age) and knots $k_i, for\ i = 1, ..., n-1$ at each age threshold with a change in the severance pay amount – are computed as follows:

$\mathrm{V}_1 = \min(V, k_1)$

$\mathrm{V}_i = \max\{\min(V, k_1), k_{i-1}\} - k_{i-1}$, i=2,...,n

The Restricted Cubic Spline consists of elements $V_{i,}$ for $i = 1, ..., n-1$ which – given the underlying continuous variable V (i.e. age) and knots k_i, for $i = 1, ..., n$ at each age threshold with a change in the severance pay amount – are computed as follows:

$\mathrm{V}_1 = V$

$\mathrm{V}_{i+1} = \frac{(V-k_i)_+^3 - (k_n - k_{n-1})^{-1}\{(V-k_{n-1})_+^3(k_n-k_i) - (V-k_n)_+^3(k_{n-1}-k_i)\}}{(k_n-k_1)^2}$ for i=1,...,n-2

where $(\mathrm{u})_+ = u$ if u>0 and $(\mathrm{u})_+ = 0$ if u≤ 0.

See Stata [2009], pp. 1053-1058.