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Does Cash-in-Hand Matter?
New Evidence from the Labor Market

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ABSTRACT

This paper provides new evidence on the effects of cash-in-hand on household behavior. Using sharp discontinuities in eligibility for severance pay and extended unemployment benefits in Austria, combined with data on over one-half million job losers, we reach three main findings: (1) A lump-sum severance payment equal to two months of wages lowers the rate of new job finding by 8-12% on average; (2) An extension of the potential duration of UI benefits from 20 weeks to 30 weeks lowers job-finding rates by a similar amount; and (3) the increases in the duration of job search induced by either program have no effect on job quality, as measured by wages or the duration of the next job. We use a simple job search model with savings to show how the these estimates can distinguish between commonly used models of household behavior, and provide a simple metric that can be used to calibrate such models to match our empirical findings. The relatively large magnitude of the severance pay effect suggests that job searchers are unable to smooth consumption as much as predicted by a simple permanent income model.

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I Introduction

Does disposable income ("cash-in-hand") affect household behavior? The answer to this basic question has implications for a range of economic issues. In macroeconomics, the answer distinguishes between a continuum of widely used models of household behavior, ranging from the benchmark permanent income hypothesis with complete markets (where changes in disposable income have small effects on consumption) to "rule of thumb" models (where consumption rises dollar-for-dollar with income). In public finance, the answer is relevant for optimal tax and social insurance policies. Temporary tax cuts can only be effective as a fiscal stimulus if households are sensitive to cash-in-hand. Similarly, the benefits of temporary income support programs such as unemployment insurance and welfare are determined by the extent to which individuals can smooth short-term income fluctuations on their own (Baily 1978, Chetty 2006).

The effects of cash-in-hand have been studied for several decades in the macroeconomics literature, where researchers have estimated the effect of windfall cash grants such as tax rebates on non-durable household consumption (see section II for a brief summary of this literature). However, there is still no firm consensus on whether individuals can smooth intertemporally, are fully cash constrained, or fall somewhere in between.[1]

In this paper, we provide new evidence on the effects of cash-in-hand from the labor market. In particular, we study whether lump-sum severance payments and unemployment benefit extensions for job losers in Austria affect search behavior and subsequent job outcomes. At a conceptual level, our analysis is analogous to existing studies, and simply uses a different measure of "consumption" (labor/leisure instead of goods). Excess sensitivity of labor supply to cash-in-hand distinguishes between the permanent income hypothesis (PIH) and other dynamic models in the same way as excess sensitivity of consumption. Indeed, the effects of cash-in-hand on consumption can be inferred from estimates of the labor supply responses, using a simple job search model with savings.

Our labor market approach is a useful complement to existing consumption-based studies for three reasons. First, eligibility for severance pay in Austria is based on a simple discontinuous rule that applies to all private sector workers outside the construction sector: people with over 3 years of job tenure are eligible, whereas those with shorter tenures are not. In addition, administrative wage and employment data are available for over 500,000 job losers. The sharp discontinuity and large sample allow us to obtain more precise estimates of the effects of cash-in-hand than consumption-

[1] The lack of consensus is underscored in the review by Browning and Lusardi (1996), who note that they personally disagree on importance of liquidity constraints.

based studies, which are often limited by small sample sizes and noise in consumption measures. Second, the severance payment is generous – equivalent to two months of salary, or about 2500 Euros at the sample mean. This makes our analysis less subject to Browning and Crossley's (2001) criticism that the welfare costs of failing to smooth over small amounts (e.g. the $300-$600 tax rebates in Johnson et. al. 2006) is negligible. Third, the panel structure of our data allows us to examine the long-term effects of cash grants, in particular subsequent job quality. This allows us to further distinguish between dynamic models of search behavior and provide new evidence on subsequent job quality effects, an issue of independent interest in the literature on job search.

We exploit the quasi-experiment created by the discontinuous Austrian severance pay law using a regression discontinuity (RD) design, essentially comparing the search behavior of individuals laid off just before and after the 36 month cutoff for eligibility. The key threat to a causal interpretation of our estimates is that firms may manipulate their firing decisions to avoid paying severance, leading to non-random selection around the discontinuity and invaliding the "experiment." We evaluate this issue by comparing the number of layoffs at each level of job tenure, and by examining the characteristics of job losers with just under and just over 3 years of tenure. We find no systematic evidence of selection on observables around the discontinuity – a result that is consistent with relatively restrictive firing regulations in Austria and laws against the strategic timing of layoffs.[2] This suggests that any discontinuities in search behavior around the 36 month cutoff can be attributed to the causal effect of severance pay.

Our empirical analysis leads to three main findings. First, lump sum severance pay has a clearly discernable and economically significant effect on the duration of unemployment and time to re-employment. The hazard rate of moving to a new job during the first 20 weeks of search (the period of eligibility for regular UI benefits in Austria) is 8-12% percent lower for those who are just barely eligible for severance pay than for those who are just barely ineligible. Second, using a parallel analysis of a discontinuity in the UI benefit system, we find that job seekers who are eligible for 10 extra weeks of unemployment benefits also exhibit lower rates of job finding in the period before the extension. This result provides strong evidence of forward looking behavior, inconsistent with pure "rule of thumb" models with completely myopic agents. The magnitude of the effect is similar or slightly smaller than the effect of severance pay, i.e., about a 7-10% effect on the hazard. Third, neither lump sum severance payments nor extended benefits have any effect on

[2] As we discuss in more detail in Section IV, the Austrian labor market is characterized by relatively high rates of job mobility and low unemployment (an average rate of 4.8% over the 1992-2002 period). Nevertheless, firms face significant regulations governing layoffs. See Winter-Ebmer (2002).

the "quality" of subsequent jobs. In particular, mean wages and the duration of subsequent jobs are essentially unaffected by eligibility for severance pay or extended benefits. Thus, the behavioral impacts of severance pay and extended benefits on durations appear to work through the margin of search intensity, rather than through a shift in the reservation wages of job seekers.

Combining these findings with predictions from a job search model that nests a continuum of cases (from the PIH to complete myopia), we conclude that simple model with variable job search intensity and incomplete consumption smoothing fits the data. We develop a simple metric to measure how far between the PIH and fully cash-constrained behavior the representative agent in our data lies. Our estimates suggest that deviations from the PIH are substantial: a typical job searcher behaves as if they are half-way between the PIH and cash-constrained benchmarks, implying that temporary income support and tax rebate policies could indeed have substantial economic effects. Perhaps more importantly, this empirical estimate – which is roughly consistent with some of the most recent quasi-experimental studies of consumption behavior (e.g. Johnson, Parker, and Souleles 2006) – provides a moment that can be matched when calibrating dynamic models in subsequent work.

The remainder of the paper proceeds as follows. Section II discusses related literature. Section III presents our theoretical model. Section IV describes the institutional background and data. Section V outlines our estimation strategy and identification assumptions. Section VI presents the empirical results on unemployment durations, and Section VII presents results on search outcomes. Section VIII uses the empirical estimates to calibrate the model. Section IX concludes.

II Related Literature

Our analysis builds on insights and methods from several literatures in macroeconomics, labor economics, and public finance. The first is a set of studies that measures the effects of transitory income shocks on consumption.[3] Bodkin (1959) and Bird and Bodkin (1965) estimated that households spent 40-70% of a one-time windfall payment issued to World War II veterans in the year of the rebate on nondurable consumption.[4] Subsequent studies of tax rebates using aggregate data

[3] Many other strands of the micro consumption literature are also related, including the tests for liquidity constraints developed by Zeldes (1989), and the "excess sensitivity" results in Hall and Mishkin (1982) and Altonji and Siow (1987). See Deaton (1992) for a summary and thoughtful interpretation of much of the literature up the early 1990s, and Browning and Lusardi (1996) for a more recent survey.

[4] The payment was based on an actuarial adjustment to the life insurance provided to all veterans, and averaged $175. A key feature of the rebate was that it was unexpected - according to Bodkin the payments were announced in November 1949 and mailed in the first few months of 1950. In a benchmark permanent income model, the predicted

(e.g., Blinder, 1981; Blinder and Deaton, 1985) also found relatively large impacts on nondurable consumption in the quarter of receipt. More recent microdata-based studies of pre-announced tax cuts and rebates include Parker (1999) and Souleles (1999), both of which find that current non-durable spending absorbs 30-65 percent of the change in after-tax current income. In contrast, however, Hsieh (2003) finds no relation between spending and the timing of recurring payments to Alaska residents from the Alaska State fund. Finally, Johnson, Parker, and Souleles (2006) analyze the 2001 federal tax rebates, exploiting the fact that checks were mailed at different dates to different families. They report estimates of the effect on non-durable consumption in the quarter of the rebate (relative to the preceding quarter) centering on 35-40 cents per dollar.

A second related literature focuses on estimating consumption-income sensitivity for unemployed individuals using variation in unemployment benefits. Gruber (1997) relates the change in food consumption for families with a recently unemployed head to the generosity of the UI benefits potentially available to the head. He estimates that a 10% increase in the UI benefit level leads to a 2.5-3.3 percent increase in food consumption by the unemployed. Subsequent analyses conducted by Browning and Crossley (1999) and Bloemen and Stancanelli (2005) on samples of longer-term job losers in Canada and the U.K. find smaller effects of UI benefits on total expenditures in the aggregate, but larger effects among job losers with low assets prior to job loss. Interestingly, Bloemen and Stancanelli report that job losers who received a severance benefit have higher consumption while unemployed, a result consistent with our findings below.

Outside the consumption literature, our analysis is closely related to studies of the effects of unemployment benefits and assets on job search effort and the duration of unemployment. On the theoretical side, conventional job search models imply that higher unemployment benefits and longer potential eligibility for benefits will raise the average duration of unemployment (e.g., Mortensen, 1977; Mortensen, 1986). Most search models assume risk neutrality and ignore savings, and thus do not study wealth effects. However, a few studies have incorporated these features and show that under certain conditions increases in wealth lower search intensity (Danforth, 1979; Lentz and Tranaes, 2001). On the empirical side, a number of well-known studies have shown that the duration of unemployment is affected by the generosity and potential duration of UI benefits (e.g., Meyer, 1990; Katz and Meyer, 1990; Lalive and Zweimuller, 2004). These studies have generally assumed

effect of a transitory income shock on current non-durable consumption is roughly proportional to the discount rate (e.g., 5-10%). Carroll (2001) has argued against the relevance of this benchmark and in favor of a model with precautionary savings that suggests a larger effect. Both the benchmark model and Carroll's alternative imply a negligible effect of an anticipated income shock on the change in consumption.

that the entire response of search behavior to UI benefits is due to moral hazard (a substitution effect) rather than wealth effects. Chetty (2006) points out that the wealth effects of UI benefits may be non-trivial when agents have limited liquidity. He decomposes the UI benefit elasticity into a wealth effect and substitution effect by examining the heterogeneity of duration-benefit elasticities across liquidity constrained and unconstrained groups in the U.S. He finds that 2/3 of the UI benefit effect is a wealth effect, consistent with our estimates here. The key advantages of the present study relative to the existing literature are the isolation of a credibly exogenous source of variation in wealth and the use of this variation to distinguish between dynamic models of household behavior.

Our analysis also contributes to the literature on match quality gains from job search. Ehrenberg and Oaxaca (1968) found that increases in UI benefits led to small increases in wages at the next job. Subsequent studies have found mixed, fragile results; see Burtless (1990) and Cox and Oaxaca (1990) for reviews and Addison and Blackburn (2000) and Centeno (2004) for more recent analysis. This literature remains quite controversial largely because of the lack of compelling variation in benefit policies. Our analysis yields substantially more precise estimates of match quality gains than earlier studies because of the large sample and RD research design.

Finally, our study is related to the extensive literature on optimal social insurance (e.g. Baily 1978, Flemming 1978, Hansen and Imrohoroglu 1992, Wang and Williamson 1996, Werning 2002, Shimer and Werning 2005, Chetty 2006). Although we do not explicitly consider an optimal social insurance problem here, our findings bear on the problem by empirically identifying the extent to which households can smooth consumption, a central parameter in these calculations. Our findings can be applied in subsequent studies of optimal social insurance by calibrating models to match the moments estimated below.

III A Job Search Model

In this section, we present a simple job search model that provides a structural framework for interpreting our empirical findings. The model we analyze nests the continuum of dynamic models commonly used in the literatures on consumption and search, illustrated in Figure 0. The models on the left side of the continuum assume a higher degree of intertemporal smoothing by households, and thus predict a lower sensitivity of behavior to cash-in-hand. At the left extreme of the continuum is the benchmark permanent income hypothesis with complete markets, where consumption is perfectly smooth across states and time, and temporary income shocks have no effect on behavior (no excess sensitivity). At the right extreme is a "complete myopia" model where households do not smooth intertemporally at all, and simply set current consumption equal to current income. In this model, consumption rises 1-1 with income and thus exhibits a high degree of excess sensitivity. The interior of the continuum includes models that have an intermediate degree of forward-looking behavior, and thus have intermediate predictions for the sensitivity of consumption to income (e.g. the buffer stock models of Deaton, 1991 and Carroll 1992, and models with forward-looking but cash-constrained agents).

We use the model for two purposes: (1) To derive a set of comparative statics predictions relating job search behavior to increases in assets and the availability of unemployment benefits. (2) To show how estimates of the relative effects of severance pay and benefit extensions can be used to locate the average behavior of job searchers on the continuum in Figure 0.

The setup of the model, which is closely based on Lentz and Tranaes (2004), is as follows. Consider a discrete-time setting where an individual has a finite planning horizon and faces a fixed interest rate r equal to his subjective rate of time discounting. Suppose the individual enters period t unemployed. He can control his unemployment duration only by varying his level of search intensity, s_t. The agent chooses search intensity at the beginning of period t, and immediately learns if he has obtained a job (which starts in period t itself). Normalize s_t to equal the probability of finding a job in the current period. The disutility of supplying s_t units of search effort is given by a strictly convex function $\psi(s_t)$.

If the agent is successful in job search and finds a new job, he earns a fixed real wage w indefinitely and faces no further uncertainty. For simplicity, we ignore any variability in wage offers, eliminating reservation-wage choices. We discuss how relaxing this assumption would affect our results below. Let c_t^e denote the employed agent's consumption in period t if job search is successful in that period.

If the agent fails to find a job in period t, he receives an unemployment benefit b_t and sets consumption to c_t^u. The agent then enters period $t+1$ unemployed, when he chooses search effort s_{t+1} and the problem repeats.

The agent has a within-period utility over consumption (c_t) given by a strictly concave function $u(c_t)$. To allow for borrowing constraints, assume there is a lower bound L on assets which may or may not be binding.

The value function for an individual who finds a job at the beginning of period t, conditional on beginning the period with assets A_t is

$$V_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - A_{t+1}/(1+r) + w) + \frac{1}{1+r} V_{t+1}(A_{t+1}). \tag{1}$$

The value function for an individual who fails to find a job at the beginning of period t and remains unemployed is:

$$U_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - A_{t+1}/(1+r) + b_t) + \frac{1}{1+r} J(A_{t+1}) \tag{2}$$

where $J(A_{t+1})$ is the value of entering the next period unemployed. It is easy to show that V_t is concave because the agent faces a deterministic pie-eating problem once re-employed. The function U_t, however, can be convex. Lentz and Tranaes (2004) address this problem by introducing a wealth lottery that can be played prior to the choice of search intensity whenever U is non-concave, although they note that in simulations of the model, non-concavity never arises. We shall simply assume that U is concave.

The agent chooses s_t to maximize expected utility at the beginning of period t, taking into account the cost of search:

$$J(A_t) = \max_{s_t} s_t V_t(A_t) + (1 - s_t) U_t(A_t) - \psi(s_t) \tag{3}$$

The first order condition for optimal search intensity is

$$\psi'(s_t^*) = V_t(A_t) - U_t(A_t) \tag{4}$$

reflecting the fact that the value of additional search effort is just the difference between the optimized values of employment and unemployment.

Our testable predictions and empirical analysis all follow from the comparative statics of equation (4). First consider the effect of the UI benefit level on search effort. Differentiating equation (4) and using the envelope theorem, we obtain:

$$\partial s_t^* / \partial b_t = -u'(c_t^u)/\psi''(s_t^*) < 0 \tag{5}$$

Equation (5) is the standard result that higher unemployment benefits reduce search effort, thereby extending unemployment durations. This prediction does not distinguish between the models in the continuum in Figure 1, because regardless of the degree of intertemporal consumption smoothing, higher unemployment benefits always increase durations. Consistent with this result, many well-known studies have found that increases in UI benefits raise the duration of joblessness. To distinguish between the models of interest, we turn to other comparative static implications of (4).

Prediction 1: Severance Pay. The effect of an exogenous cash grant, such as a severance payment, on search effort is given by:

$$\partial s_t^*/\partial A_t = \{u'(c_t^e) - u'(c_t^u)\}/\psi''(s_t^*) \leq 0 \tag{6}$$

Equation (6) shows that the effect of a cash grant on search intensity is determined by the gap in marginal utilities between employed and unemployed states, which is proportional to the size of consumption drop $c_t^e - c_t^u$. Intuitively, when consumption is smoothed across states, a cash grant increases the value of being employed and unemployed by a similar amount, and thus does not affect search behavior much. In contrast, if consumption is substantially lower when unemployed, the cash grant raises the value of being unemployed relative to the value of being employed, leading to a reduction in search effort

It is well known that if an agent has access to complete state-contingent insurance markets (full insurance), $c_t^u = c_t^e$. A PIH model with complete markets therefore predicts that $\partial s_t^*/\partial A_t = 0$. In this (extreme) case a lump sum severance payment has no effect on search behavior, a prediction that we test in our empirical analysis. More generally, if c_t^u is close to c_t^e, as would be expected if individuals can freely borrow and have a high probability of finding a job relatively quickly, the asset effect is small. In contrast, if individuals face asset constraints or have to consume only their net income while unemployed, the asset effect will be relatively large. Thus, there is a direct connection between the degree of consumption smoothing achieved by job searchers (the location on the continuum), and the responsiveness of search intensity to an increase in wealth.

An estimate of $\partial s_t^*/\partial A_t$ is also useful in assessing the degree of moral hazard caused by temporary income support programs, as shown by Chetty (2006). To see this in our model, note that:

$$\partial s_t^*/\partial w_t = u'(c_t^e)/\psi''(s_t^*) > 0$$

and hence

$$\partial s_t^*/\partial b_t = \partial s_t^*/\partial A_t - \partial s_t^*/\partial w_t \tag{7}$$

Equation (7) shows that the response of search intensity to an increase in unemployment benefits can be written as the sum of a wealth effect and a price (or substitution) effect The former has no direct efficiency costs, whereas the latter represents a "moral hazard" response to the price distortion induced by subsidizing unemployment.

Many empirical studies of unemployment insurance ignore the asset effect by assuming that unemployment durations depend on the ratio of benefits to wages. These studies implicitly assume that the PIH with complete markets model applies. To the extent that job seekers have lower consumption when unemployed, however, one should expect benefits to have a larger impact (in absolute value) than wages. Interestingly, this pattern is present in the well-known study by Meyer (1990), whose estimates imply that the effect of UI benefits on the hazard rate of leaving unemployment is about 1.8 times larger than the effect of weekly earnings.

Prediction 2: Extended Benefits. To derive a test of the "complete myopia" model, we examine how search intensity in period t is affected by the level of future benefits, b_{t+1}. Using equations (3) and (2) we obtain:

$$\partial s_t^*/\partial b_{t+1} = -E_t[(1 - s_{t+1}^*)u'(c_{t+1}^u)]/[(1 + r)\psi''(s_t^*)] \leq 0 \tag{8}$$

This equation implies that a rise in the future benefit rate lowers search intensity in the current period, but only by the discounted value of those benefits, which depends on $(1 - s_{t+1}^*)$ and $1 + r$. A completely myopic agent places no value on the future and has $r = \infty$. The complete myopia model therefore predicts that $\partial s_t^*/\partial b_{t+1} = 0$. In this model, extending the potential duration of UI benefits has no effect on search behavior prior to the extension, which is the second prediction that we test in our empirical analysis. More generally, the effect of the benefit extension on pre-extension search behavior provides a measure of how forward-looking agents are; agents who place more weight on the future (as on the left end of the continuum) will in general respond more to benefit extensions.

Combining equations (6) and (8), we obtain the following relationship between the predicted effects of a 1-euro increase in severance pay and a 1-euro increase in the value of future benefits:

$$\frac{\partial s_t^*/\partial A_t}{\partial s_t^*/\partial b_{t+1}} = D \times \frac{1 + r}{1 - s_{t+1}^*}, \tag{9}$$

where

$$D = \frac{u'(c_t^u) - u'(c_t^e)}{E_t[u'(c_{t+1}^u)]}.$$

In models such as the PIH with complete markets where consumption is not very sensitive to temporary shocks, D is close to zero and the relative effect of severance pay is small. In a model with incomplete consumption smoothing, on the other hand, $u'(c_t^u)$ can be much larger than $u'(c_t^e)$, implying a larger relative effect of severance pay. Finally, in a model with completely myopic agents, the relative effect of severance pay approaches infinity. Thus the ratio of the relative effects of severance pay and future UI benefits provides a metric for the agent's location along the continuum. We calibrate this metric for various models and in our data in section VIII.

Prediction 3: Search Outcomes. A final prediction that is useful in distinguishing between models of search behavior is the effect of an increase in assets or future unemployment benefits on subsequent job quality. This prediction cannot be derived from the model here because we have assumed that wages are fixed and agents only control search intensity. However, in a more general model with a non-degenerate distribution of wages or job qualities, one would expect an increase in assets or future benefits to lead to a rise in the quality of the next job (Danforth, 1979; Mortensen, 1977).

Table 1 summarizes the predictions that distinguish between four potential models of household behaviour.

IV Institutional Background and Data

The Austrian labor market is characterized by an unusual combination of institutional regulation and labor market flexibility. Virtually all private sector jobs are covered by collective bargaining agreements, negotiated by unions and employer associations at the industry level (EIRO, 2006). Firms are also required to consult with their works councils in the event of a layoff (Winter-Ebmer, 2002), and to give at least 6 weeks notice of a pending mass layoff. Despite these features, rates of job turnover and and overall employment are relatively high, whereas unemployment is low. Winter-Ebmer (2002), for example, shows that rates of "job creation" and "job destruction" for the overall economy and for most sectors are comparable to those in the U.S. The overall employment-population rate of 15-64 year olds during the 1990s averaged 68% - higher than in Germany or France but below the rates in the U.K. or the U.S. [5] The average unemployment rate over the 1993-2004 period was among the lowest in Europe at 4.1%.

A key aspect of the firing regulations in Austria is severance pay, which was introduced for white

[5] Austria has among the lowest employment rates in Europe for people over 55: 42% for people age 55-59 and only 12% for those 60-64 (EIRO, 2005). Rates for younger workers are relatively high, and comparable to the U.S.

collar workers in 1921, and was expanded to include other workers in 1979. Severance payments are made by firms according to a fixed schedule. In particular, workers outside of the construction industry who are laid off after 3 years of service are entitled to receive a payment equal to 2 months of their previous salary.[6] Payments are generally made within a few weeks of the job termination, and are not taxable.

Job losers with a sufficient work history are also eligible for unemployment benefits. Specifically, individuals who have worked for 12 months or more over the past two years can receive a unemployment benefit (UI) that replaces approximately 55% of their prior net wage, subject to a minimum and maximum (though only a small fraction of individuals are at maximum). Workers who are laid off by their employer are immediately eligible for benefits, while those who quit (or are fired for cause) have a four week waiting period. The maximum duration of regular unemployment benefits is a discontinuous function of the total number of months that the individual worked (at any firm) within the past five years. In particular, individuals with less than 36 months of previous employment receive 20 weeks of benefits, while those who have worked for 36 months or more receive an additional 10 weeks of benefits. Job losers who exhaust their regular unemployment benefits can move to a means-tested secondary benefit, known as "unemployment assistance," (UA) which pays a lower level of benefits indefinitely.[7] Importantly, however, UA benefits are reduced euro-for-euro by the amount of any other family income. As a result, only 13% of individuals who exhaust regular UI in our data receive UA.

Our empirical analysis exploits the discontinuities in the severance pay and benefit duration laws to identify the causal effects of these two entitlements on the duration of unemployment and the time to a new job. The effects of the two policies can be independently identified because they are discontinuous functions of different running variables: previous job tenure in the case of severance pay, and previous weeks of work in the case of extended benefits for regular UI Nevertheless, there is a subset of individuals – those who did not work in the two years prior to the current job – for whom the severance pay and extended UI benefit discontinuities overlap. This creates a "double discontinuity" that complicates the empirical analysis relative to the standard regression discontinuity design proposed by Thistlewaite and Campbell (1960), where there is only

[6] The severance amount rises to 3 months of pay for workers with 5 years of service, 4 months after 10 years, and up to 12 months after 25 years of service. Employees who quit or are fired for cause are not eligible for severance pay. Workers in the construction industry are covered by a different law. The law governing severance pay was changed in January 2003, leading us to limit our sample to the 1980-2002 period.

[7] UA benefits are taxable whereas regular benefits are not. Taking this into consideration, the meximum level of unemployment assistance benefits is 78% of regular benefits (Winter-Ebmer, 2003).

one discontinuous policy change.

Figure 1a illustrates the problem by plotting the fraction of individuals in our data who receive an extended unemployment benefit (EB) as a function of months of job tenure. Individuals who have 36 or more months of job tenure necessarily have worked for more than 3 of the last 5 years; hence the fraction receiving EB is 100% on the right side of the severance pay discontinuity. Individuals who have 35 months of job tenure receive EB if they worked for one month or more at another firm within the past five years. Since only 85% of individuals laid off with 35 months of job tenure satisfy this condition, there is a 15 percentage point jump the fraction receiving EB at 36 months of job tenure. Consequently, any discontinuous change in behavior at 36 months of job tenure is mainly due to severance pay, but includes a small (15 percentage point) effect of extended benefits. A similar double discontinuity arises at the threshold for EB benefits, as shown in Figure 1b. The fraction of individuals receiving severance pay jumps discontinuously by 18% at 36 months worked. Hence changes in behavior around 36 months worked are likely to be caused primarily by EB, but could also be partly attributed to severance pay. We account for the double discontinuity in our empirical analysis using two alternative methods described below. One method is to limit the samples to groups who are only subject to a single discontinuity. The second is to extend the conventional regression discontinuity method to incorporate the possibility of two discontinuous "treatments" that depend on separate running variables..

IV.A Data and Sample Definition

We use 1980-2002 data from the Austrian social security registry, which gives information on employment and earnings for all private sector employees.. The dataset includes daily information on employment and registered unemployent status, total wages received from each employer in a calendar year, and information on workers' and firms' characteristics (e.g. age, education, gender, marital status, industry, and firm size).

Note that we do not have any information on actual severance payments, or the amount of UI benefits paid in this dataset. Hence, we cannot construct "first-stage" estimates of the effect of the discontinuous policies on actual payments received. Compliance with the severance pay law is believed to be nearly universal, in part because of the monitoring effort of works councils and legal penalties for violations (CESifo, 2004; Baker and Tilly, 2005). Given our data source, we also believe we have accurately captured the eligibility rules for extended benefits. Consequently, we believe that the eligibility rules for both severance pay and EB's create so-called "sharp" regression discontinuity designs (Hahn, Todd, and van der Klaauw, 2001).

We make four restrictions on the original data to arrive at our primarily analysis sample. First, we include only non-construction workers between the ages of 20 and 50 at the time of the job termination, to avoid complications with the retirement system and the differential treatment of construction workers. Second, we exclude voluntary quitters (who are ineligible for severance pay and have a waiting period for UI). Third, we focus on individuals around the discontinuities of interest by only including individuals who worked at their previous firm for between 1 and 5 years, and who worked between 12 and 59 months in the past 5 years.[8] Consequently, everyone in the sample is eligible for UI benefits (though not all are eligible for EB's), and everyone in the sample is eligible for either 2 months of severance pay, or none. Finally, we drop individuals who were recalled to prior firm in order to eliminate temporary layoffs who many not be searching for a job. These restrictions leave us with a sample of 609,546 unemployment spells.

Table 2 shows summary statistics for the full sample. The mean age of sample members is 31; just over one-half are women. Some 60 percent have additional schooling beyond the compulsory level – most of this group have an apprenticeship, equivalent to a "some college" level of education in the U.S. Only 44 percent of the sample are married, reflecting relatively the relatively low age of the sample and the prevalence of non-marital cohabitation.[9] Around 12 percent are non-citizens. Owing to our sample requirement that people have worked between 1 and 5 years at their last job, average tenure is relatively short (26.5 months). However, most people have worked at other jobs in the past 5 years: the mean of months worked is around 42. Roughly one-fifth of the sample is eligible for severance pay, while 66% are eligible for extended UI benefits (i.e., 30 weeks instead of 20). The mean wage is 1206 Euros per month (in year 2000 Euros) equivalent to 16,884 Euros per year (since Austrians recieve 14 "monthly" salaries per year).

There are two measures of "unemployment duration" that can be constructed in the data. The first is the total number of days that an individual is registered with the unemployment agency. Individuals are required to register while they are receiving benefits, and can remain registered even when their benefits are exhausted in order to take advantage of services offered by the agency.[10] This measure corresponds to the official definition of "unemployment" in government statistics, and

[8] People who worked continuously over the past 5 years but have less than 5 years of job tenure with their longest employer are somewhat unusual, since they had a job transition with no intervening spell of unemployment. We therefore exclude this group.

[9] Kiernan (2001) estimates that among all women age 25-29 in Austria in 1996, 45% were married and 8% were cohabitating.

[10] The employment agency offers job training and job search assistance.

we therefore refer to it below simply as the individual's "unemployment duration." Only a small fraction (0.5 percent) of people in the sample have censored spells of unemployment: the summary statistics in Table 2 ignore this. Spells of registered unemployment are relatively short (mean of 4.9 months, median of 3): 64% end within 20 weeks, and 94% end within a year. The second measure of the duration of job search, which we label "time to next job," is the amount of time elapsing from the end of the previous job to the start of the next job. Although over 90% of the sample are observed in a next job, some people lose a job and never return to the data set, leading to a tail of extremely long censored durations.[11] The summary measures of time to the next job in Table 2 exclude censored observations, but even with this exclusion, it is clear that the distribution has a long upper tail: the mean is 9.34 months versus a mean of 4 months. Finally, the last row of the table shows the mean, median, and standard deviation of the change in log (real) monthly earnings between the old and new jobs. The median wage loss is very close to 0, while the mean is -5%. However, there is substantial dispersion, due in part to our inability to control for changes in monthly hours of work.

V Estimation Strategy and Identification Assumptions

Our identification strategy is to exploit the quasi-experiment created by the Austrian severance pay and extended benefit laws using a regression discontinuity (RD) approach. We begin by describing the approach for identifying the causal effect of severance pay on durations, ignoring extended benefits. Intuitively, we compare the unemployment durations of individuals laid off just prior to 36 months of job tenure, who are ineligible for severance pay, with the durations of those laid off just after 36 months of job tenure, who receive a severance payment. As in other regression-discontinuity designs (e.g. Thistlewaite and Campbell 1960, Angrist and Lavy 1999, DiNardo and Lee 2005), we attribute evidence of a discontinuous relation between job tenure and duration at 36 months to the causal impact of a severance payment. We then extend the analysis to incorporate the joint effects of severance pay and extended benefits, addressing the problem noted earlier that some people become eligible for both programs at exactly the same point.

Focusing on severance pay only for the moment, consider the following model of the relationship between the duration of unemployment experienced by a job loser (y) and a dummy variable S

[11] Some of these individuals may leave the country (to work in Germany or Switzerland), or simply drop out of the labor force.

which is equal to 1 if he or she recieves severance pay and 0 otherwise:

$$y = \alpha \ + \ S\beta_s + \varepsilon \ . \tag{10}$$

The parameter of interest is the coefficient $\beta_s$which measures the causal effect of severance pay on y. The problem for inference is that eligibility for severance pay is non-random. In particular, workers who are more likely to have a long enough job tenure to be eligible for severance pay may have other unobserved characteristics that also affect their unemployment duration:

$$E[\varepsilon|JT] \neq 0.$$

Since S is a function of JT, this can lead to a bias in the direct estimation of β_s in equation (10). This bias can be overcome if

$$\lim_{\Delta\to 0^+} E[\varepsilon|JT = 36 + \Delta] = \lim_{\Delta\to 0^+} E[\varepsilon|JT = 36 - \Delta],$$

i.e., if the distribution of unobserved characteristics of people with job tenure just slightly under 36 months is the same as the distribution among those with tenure just slightly over 36 months. In this case, the control function $f(JT)$ defined by

$$E[\varepsilon|JT] = f(JT),$$

is continuous at $JT = 36$. Thus, one can augment equation (10) with the control function, leading to:

$$y = \alpha + S\beta_s + f(JT) \quad + \ \nu \tag{11}$$

where $\nu \equiv \varepsilon - E[\varepsilon|JT]$ is mean independent of S. Morever, since S is a discontinuous function of job tenure, whereas the control function is by assumption continuous at 36 months, the coefficient β_s is identified. In practice, $f(JT)$ is unknown and has to be approximated by some smooth flexible function, such as a low-order polynomial (e.g., Dinardo and Lee, 2005). We follow this approach and use a second or third order polynomial, allowing the linear and higher order terms to be interacted with a dummy for tenure over 36 months.[12]

[12] The fact that the control function is unknown introduces the possibility of specification error. Lee and Card (2006) argue that in situations like the present case, where the running variable is discrete (measured in days), it

The key assumption of the RD approach is that individuals on either side of the 36 month threshold have the same distribution of unobserved characteristics. One may be concerned about the validity of this assumption because firms have an incentive to fire workers prior to the 36 month cutoff in order to avoid the cost of the severance payment. Such selective firing could invalidate the RD research design by creating discontinuous differences in workers' characteristics to the left and right of the 36 month cutoff.

Although the continuity assumption cannot be fully tested, its validity can be evaluated by checking whether the frequency of layoffs and the means of observable characteristics trend smoothly with job tenure through the 36 month threshold (Lee, 2006). As a first check, Figure 2 shows the number of job losers entering unemployment, by months of job tenure. There is no evidence of a spike in layoffs at 35 months, nor of a relative shortfall in the number of people who are laid off just after the threshold, suggesting that employers do not in fact selectively time their firing decisions to avoid the costs of severance pay. Given that such strategic behavior is illegal, and the fact that layoffs have to be vetted by the works council, this is perhaps not too surprising.[13]

Despite the absence of any discontinuity in the number of laid off workers entering unemployment by months of tenure, there could still be differences in the types of workers who are laid off just before and just after the severance eligibility threshold. To assess the importance of such selection, we examine how average sample characteristics vary with job tenure. Figure 3a plots average age in each tenure-month cell by job tenure, and shows that there is no evidence of selection on age. Figure 3b conducts a similar analysis on the mean wages of those laid off at different tenures. In this case there is a small but statistically significant jump in mean wages at the discontinuity, indicating that higher-wage employees are relatively more likely to be laid off just after 36 months than just before. While this is potentially worrisome for our research design, we note that the magnitude of the discontinuity is small: the jump in the best-fit lines shown in Figure 3b is 15.6 Euros/month, or about 1.2% of the mean wage for people with 35 months of tenure. This small discontinuity is only statistically detectable because of the size of our data set and the relatively precise wage measures available to us. We find similar results – either statistically significant effects or small significant effects – for other observables such as education, industry, occuption, previous

is advisable to "cluster" the standard errors of the regression model by values of the running variable. This assures that the average error in the approximating control function is incorporated in the estimated sampling error of the RD effect.

[13] Some fraction of people who are laid off move directly to another job without an intervening spell of unemployment. We have also examined the frequency distribution of the total number of layoffs at each value of previous job tenure, and found no evidence of a spike at 36 months. Finally, we examined the probability that a laid off person filed for UI (and thus appears in our data set). This also appears to vary smoothly through the 36 month threshold.

firm size, and month/year of job loss.

The degree of potential bias from the small amount of selection on wages and other characteristics can be assessed by estimating the average effect of these covariates on unemployment durations. Intuitively, unless the effect of wages on durations is large, a small discontinuity in wages cannot lead to much bias in the estimated effect of severance pay on unemployment durations. To quantify the bias we estimate the effect of wages and other covariates on unemployment exit hazards using the following Cox proportional-hazards specification:

$$h_d = \alpha_d \exp(X\phi)$$

where h_d denotes the unemployment exit hazard on day d for a given individual, α_d is an unrestricted "day effect" (the so-called baseline hazard), and X denotes the following set of observed characteristics: the log of the previous wage and its square, age and its square, gender, “blue collar” status, Austrian nativity, previous firm size, and dummies for industry, region of residence, month of job loss, and year of job loss. We then predict the relative hazard for each observation, $\widehat{r} = \exp(X\widehat{\phi})$, using the estimated $\widehat{\phi}$ vector. Finally, we compute the means of the predicted relative hazards by month of job tenure, $\mathrm{E}[\widehat{r}|JT]$, and plot this function, looking for any indication that the average characteristics of those laid off with 36 months of tenure are much different from those laid off with 35 months of tenure.

Figure 3c shows the results of this exercise. The predicted hazards trend downward across the chart, indicating that individuals with higher job tenure have observable characteristics associated with longer durations. The trend through the 36 month threshold is quite smooth, suggesting that any discontinuities in the individual covariates tend to "cancel out." We conclude that taking the vector of covariates as a whole individuals are "nearly randomized" around $JT = 36$, implying that any significant discontinuity in durations at this point can be attributed to severance pay.

Our identification strategy for estimating the effect of the UI benefit extension on durations is conceptually similar to the strategy for severance pay. Formally, we replace the indicator for severance pay in equation (11) with an indicator E for extended benefit status, and replace job tenure with a measure of months worked (MW) in the five years before the job termination. Again, the potential problem with a simple regression of unemployment duration on EB status is that people with a longer work history may be more (or less) likely to find a job quickly. And, as in equation (10), the key assumption that faciliates an RD approach is that the expected value of unobserved characteristics is the same for people with MW just under 36 months and just over 36 months. We evaluate this assumption by plotting the frequency of layoffs, the average values of various observable covariates, and the predicted unemployment exit hazards against MW. For space reasons we do not report the results here. In summary, however, there are no discontinuities in the relative number of layoffs, nor in the predicted relative hazard at $MW = 36$. Moreover, in contrast to the situation in Figure 3b, there is no significant jump in mean wages around the 36 month threshold in months worked. Overall, we conclude that the patterns in the data are quite consistent with the assumption that EB status is "as good as randomly assigned" among people with values of MW on either side of the 36 month threshold.

As noted above, although severance pay and EB status depend on different running variables, there is a small group of people in our sample – those with only 1 employer in the past 5 years – who reach the 36 month eligibility thresholds for severance pay and EB's at the same point. There are two ways to handle this problem. The first is to choose a subsample that avoids any "double discountinuity". Specifically, consider people who worked for some minimum period (e.g. 1 week) for at least two employers in the past five years. As tenure with the longest employer approaches 36 months in this subsample, everyone will have already passed the 36 month threshold in total months of work. Thus, at 36 months of job tenure only severance pay eligibility is affected. Likewise, as weeks worked reaches 36 months, no one in the subsample has yet worked 36 months at the same employer. Thus at the 36 month threshold in MW, only EB status shifts.

A second approach is to explicitly model the joint effects of severance pay and extended benefits. Specifically, consider the extended model:

$$y = \alpha \ + \ S\beta_s + E\beta_e + \varepsilon \ , \tag{12}$$

where S and E are indicators for severance pay and EB eligibility, respectively. Notice that we are not including an interaction effect. While in principle we would like to allow this, in practice everyone with $S = 1$ has $EB = 1$: hence we cannot identify such an effect. As in the single discontinuity case, the problem for inference based on this model is that the unobserved determinants of unemployment duration may be correlated with JT and/or MW. Define the control function $g(JT, MW)$ as

$$E[\varepsilon | JT, MW] = g(JT, MW).$$

The key assumption needed is that $g(JT, MW)$ is continuous at $JT = 36$ for all values of MW, and continuous at $MW = 36$ for all values of JT. Assuming this is true, we can augment equation (12) with the control function

$$y = \alpha \ + \ S\beta_s + E\beta_e + g(JT, MW) \ + \ \nu$$

where $\nu \equiv \varepsilon - E[\varepsilon|JT, MW]$ is mean independent of E and S. Since S and E jump discontinuously at $JT = 36$ and $MW = 36$, respectively, and JT and MW are imperfectly correlated, the coefficients $\beta_s$and β_e are identified controlling for g. To implement this model, we assume that g can be approximated by a low order polynomial of JT and MW.

VI Effects of Cash-In-Hand and Benefit Extensions on Durations

This section presents the main results on the effect of severance pay and UI benefit extensions on durations. We begin with a non-parametric graphical overview and then estimate a set of hazard models to obtain numerical measures of the elasticities of interest.

VI.A Graphical Results

Severance Pay. Figure 4a plots the relationship between average unemployment exit hazards and previous job tenure. We construct the average unemployment exit hazards non-parametrically as follows. Let h_{jd} denote the unemployment exit hazard on day d of the spell for individuals with j months of previous job tenure. Note that h_{jd} is simply the number of unemployment durations that end on day d in group j divided by the total number who were still unemployed at day $d-1$. Let h_j denote the average daily hazard over the first six months of the spell in group j (i.e., $h_j = \sum_{d=1}^{180} h_{jd}/180$). Figure 4a plots h_j versus j, along with a "best fitting" linear control function (allowing for separate slopes on either side of the 36 month threshold. The graph shows that the average unemployment exit hazard drops discontinuously by about 14% at the 36 month cutoff (from 0.0073 to 0.0064), consistent with the hypothesis that severance payments lengthen durations.

As noted by Lee and Card (2006), one of the appealing features of a regression discontinuity approach is that estimates of the discontinuity should be invariant to the presence or absence of control variables.[14] As in a classical experimental design, however, the addition of controls may

[14] Note that in our presentation of the RD method we exclude controls. One can think of the ε term as including

lead to some gain in precision. Moreover, a comparison of the estimated discontinuities with and without controls provides an informal specification test that the underlying smoothness assumptions required for an RD approach are valid. In Figure 4b, we show average hazards for each level of previous job tenure, adjusted for a vector of observed characteristics of job-losers. To construct this graph we fit a stratified Cox hazard model:

$$h_{jd} = \alpha_{jd} \exp(X\phi)$$

where X includes the same vector of covariates used in the construction of Figure 3c. We then recovered the estimated tenure-group-specific baseline hazards from this specification, $\{\alpha_{jd}\}$ and took the average of the regression-adjusted baselines over the first 180 days for each tenure group j:

$$\alpha_j = \sum_{d=1}^{180} \alpha_{jd}/180$$

Figure 4b plots α_j versus j. As in Figure 4a, the unemployment exit hazard shows about a 13% drop at the 36 month threshold for receiving severance pay. The similarity of the discontinuities in the estimated hazards with and without other controls is consistent with the result in Figure 3c that the relationship between the hazards and observable covariates is smooth, and validates the assumptions of the RD approach. It is also perhaps reassuring that once the observed controls are added, the trends in the hazards with job tenure are similar on the left and right sides of the discontinuity.

We interpret Figures 4a and 4b as showing that job losers who are eligible for severance pay have substantially lower exit rates from unemployment than those who are not. However, we cannot attribute the entire gap in this figure to the severance payment because there are two treatments applied at the cutoff: a severance pay treatment applied to 100% of the sample and an EB treatment applied to about 15% (the "double discontinuity" problem). Given that the change in the EB policy applies to a small group of individuals, one would expect that most of the discontinuity in durations at 36 months of job tenure is due to severance pay.

To isolate the effect of severance pay using simple graphical methods, we focus on a restricted subsample where the two discontinuities are not overlapping. In particular, as described above, we consider the subset of individuals who have worked for at least one day at another firm within

the effect of all the characteristics that vary across the sample, including potentially observable as well as unobserved characteristics.

the past five years, before joining the firm from which they were laid off. Since eligibility for EB is determined by total months worked within the past five years, the fraction of individuals receiving EB in the restricted sample converges smoothly to 100% before the 36 month job tenure cutoff. Figures 5a and 5b replicate Figures 4a and 4b for this restricted subsample, which includes 83% of the observations in the overall data set. Examination of these figures suggest that exit rates from unemployment fall by about 12-14% as individuals reach the threshold for eligibility for severance pay. Since the entire discontinuity in unemployment exit hazards in these figures can be attributed to the severance payment, the results confirm that severance pay has a significant effect on job search. This evidence rejects the full insurance PIH model based on prediction 1 in Table 1.

So far we have summarized the effect of severance pay on search behavior in a single statistic, the average unemployment exit hazard over the first six months of the spell. We now explore how severance pay affects search behavior as the spell elapses. To isolate the effect of severance pay, we continue to analyze the restricted subsample with at least some employment at another employer.

Figure 6a plots average weekly unemployment exit hazards for individuals laid off in tenure-months 33-35 (no severance) and those laid off in months 36-38 (who receive severance). Figure 6b replicates 6a by plotting average weekly job finding hazards ("time to next job") for the same two groups. These figures show that the gap between the hazard rates in the two groups emerges only after week 5, and gradually disappears starting around week 30. This delayed, temporary effect of severance pay on search behavior may be consistent with a buffer stock model where agents become increasingly liquidity constrained as the spell elapses, making cash grants more relevant later in the spell. Insofar as the "complete myopia" model would predict a change in search effort as soon as the agent knows he will receive the cash grant, this time pattern is less consistent with that model.

Extended Benefits. Figure 7a plots the relationship between average unemployment exit hazards and months worked (MW) in the past five years in the restricted sample. In this figure, we compute average unemployment exit hazards for each months-worked group using the same methodology as in Figure 4a. Figure 7b adjusts the hazards for covariates using the methodology described above for Figure 4b. In both figures, the average unemployment exit hazard drops discontinuously by approximately 10% at the 36 month cutoff, implying that extending the duration of UI benefits from 20 weeks to 30 weeks lowers unemployment exit hazards. Note that the entire discontinuity here is attributable to the EB policy because the fraction receiving severance pay is smooth through $MW = 36$ in the restricted sample.

In Figure 8, we explore how extending UI benefits affects search behavior as the spell elapses, comparing the weekly hazards for individuals in the three months to the left and right of the $MW = 36$ discontinuity. Figure 8a shows unemployment exit hazards, while 8b shows job finding hazards. In relation to the predictions in Table 1, the key lesson of these figures is that extending UI benefits affects search behavior prior to week 20, i.e. *before* the agent actually receives any additional income. This provides clear evidence that at least some individuals are forward-looking, in that they take into account their future expected income stream when searching in the early weeks of the spell. This evidence rejects the complete myopia model based on prediction 2 in Table 1.

In summary, the visual evidence indicates that both severance payments and extending UI benefits have substantial effects on search behavior early in the unemployment spell. These findings are inconsistent with the two extremes of the continuum of models described in Section III, and point to an intermediate model with forward-looking behavior but incomplete consumption smoothing. In the next section, we estimate the two effects using parametric hazard models, and confirm the visual evidence.

VI.B Hazard Model Estimates

To more precisely quantify the effects of severance pay and extended benefits on the duration of job search we estimated a series of proportional hazards models for the risks of exiting unemployment or starting a new job. These models include unrestricted daily baseline hazards, indicators for eligibility for severance pay and extended benefits (S and E, respectively), and third-order polynomials in job tenure (JT) and and months of work in the previous 5 years (MW).[15] In all cases we censor the spells at 139 days in order to isolate the effects of the policy variables in the first 20 weeks of job search, prior to the point at which extended benefits become available. Thus, the estimated effect of extended benefits can be interpreted as the effect of future benefits on current search activity.

Table 3 presents the estimated coefficients β_s and β_e from a selection of alternative samples and specifications. The estimates in columns 1-3 describe the exit rate from unemployment, while the esimates in columns 4-7 describe the exit rate to a new job. To begin, the models in columns 1-2 and 4-5 are fit to the restricted subsample of people who had at least one other job in the past three

[15] The polynomial terms are interacted with the indicators S and MW, allowing the derivatives of the control function to change discontinuously at $JT = 36$ and $MW = 36$.

years. In this subsample there is no "double discontinuity" problem, and thus we can estimate the effects of severance pay and EB's separately. The models in columns 3, 6, and 7 are fit to the full sample of job losers, and estimate the two effects jointly, using our "double RD" control function approach. With the exception of the model in column 7, none of the specifications include other control variables. The specification in column 7 adds controls for the worker's gender, occupation, age, previous wage, and previous firm size, as well as dummies for the month and year of the job termination.

Examination of the estimates suggests that the both severance pay and extended benefits exert a significant negative effect on search intensity, as measured by either the exit rate from unemployment or the exit rate to a new job. The magnitudes of the effect of extended benefits are similar in the models for the duration of unemployment and time to a new job, ranging from -7 to -10 percent. The magnitudes of the effect of severance pay are somewhat smaller in the models for unemployment than in the models for the time to a new job, ranging from a low of -4.3% to a high of -12%.

We have fit a wide variety of other specifications in an effort to probe the robustness of the results in Table 3. In general, we find that estimates from alternative models for the time to a new job are quite stable, while the estimates for models of the duration of unemployment are more sensitive to the parameterization of the control function and the selection of the sample. For example, replacing the third-order polynomials with fourth order models leads to estimated severance pay and EB effects on the duration of unemployment that are a little bigger in magnitude, and closer to the corresponding estimates for the time to a new job. In view of this, we believe that more weight should be placed on the estimates of the effects of the policy variables on the time to a new job.

VII Search Outcomes

Having found that severance pay and extended benefits increase the duration of job search, it is interesting to ask whether this increased duration is associated with any differences in the nature of the jobs obtained through the search process. The pure search intensity model outlined in Section III ignores this possibility. More general models with a non-degenerate distribution of potential job qualities, however, predict that job searchers with more assets and longer benefits will raise their reservation job quality threshold, leading to a rise in the average quality of accepted jobs.

As above, we begin with a graphical overview of the main findings and then present regression estimates.

VII.A Graphical Results

The first measure of job quality we examine is the wage on the next job. Define $g_i = \log(w_i^n) - \log(w_i^p)$ where w_i^n is individual i's wage in the first year at the next job and w_i^p is his wage in the final year at the previous job. Note that g_i is missing for 15% of the sample, most of which is accounted for by individuals who do not find a new job before the end of the sampling period. Figure 9a plots the average value of g_i, excluding outliers where $|g_i| > 2$ (which account for 0.65% of observations with non-missing g_i), in each tenure-month cell. The smoothness of wage growth rates through the 36 month discontinuity suggests that the increased duration of search induced by severance payments does not yield any improvements in ex-post wages.

Even if there are no benefits in terms of wages, it is possible that individuals could jobs with higher quality in other dimensions. One convenient summary statistic for the quality of subsequent jobs is their duration (e.g., Jovanovic 1979). Figure 9b shows the relationship between job tenure on the previous job and the average hazard of leaving the next job over the first two years at the job. We calculate the average job leaving hazard by first computing the monthly hazard rates of exiting the next job within each tenure-month category. We then take an unweighted average of these monthly hazards over the first two years at the next job to arrive at the values plotted in the figure. The job-leaving hazards are smooth through the discontinuity, indicating that workers who received severance pay do not stay at their next jobs any longer than those who did not receive severance pay.

We replicate the same analysis for the EB policy in Figure 10 by changing the running variable on the x-axis to months worked in the past five years. Again, we find that both wages and subsequent job-leaving hazards are smooth through the EB discontinuity, indicating that there is no evidence of match quality gains from extending durations through provision of extended benefits either.

We also checked for match effects by replicating Figures 9 and 10 for several other measures: the probability of moving across regions of Austria, the probability of switching industries, the probability of switching from a "blue collar" to a "white collar" occupation, the mean log wage in the five years following the unemployment spell, and the size of the next firm. In addition, we examined percentiles of the wage distribution to check if there are gains in the tails of the distribution. We found no discontinuity in any of these measures for either the EB or severance pay policy. We also split the data into subgroups (e.g. by age, gender, wage, education) and found no evidence of match effects in any of the subgroups.

VII.B Regression Estimates

To provide a more formal summary of the job quality effects associated with severance pay and extended benefits, we modelled the impacts of these variables on the change in log wages (from the old job to the new job) and on the hazard rate of leaving the new job within the first two years, using double RD specifications similar to the ones in Table 3.

Table 4 reports the results of this analysis. Specification 1 examines the effect of severance pay and EB on wage growth without any controls. Specification 2 addd the full set of observables described above to this regression. Specification 3 reports coefficient estimates from a hazard model for the duration of the new job without controls, while specification 4 replicates this estimation with controls. The results of the analysis are consistent with the figures: there is no evidence on match quality gains in any of the specifications.

An important distinction between the present analysis of job quality effects and some earlier studies that have failed to detect evidence of quality gains is the precision of the estimates. The regression estimates and figures show that even a small improvement in wages or subsequent job tenure (e.g. 1%) would be detectable in our analysis. Hence, our evidence suggests that the job quality gains from extending unemployment durations are not merely statistically insignificant, but small in magnitude.

One caveat is that the Austrian labor market is characterized by nearly 100% coverage under collective bargaining agreements. Relative to other less regulated labor markets, the range of variation in the quality of jobs available to a given worker may be somewhat compressed. Nevertheless, the variation in wage changes experienced by job losers is relatively wide ($\sigma(\Delta \log w) = 0.51$) and comparable to variability measured among displaced workers in the U.S.

VIII Calibration: Location on the Continuum

In this section we use the model outlined in Section III to interpret our main empirical findings. For convenience we restate the key prediction (equation (9)) giving the relative effects of severance pay and extended benefits on the optimal choice of search intensity (s_t^*,the probability of finding a job):

$$\frac{\partial s_t^*/\partial A_t}{\partial s_t^*/\partial b_{t+1}} = D \times \frac{1+r}{1-s_{t+1}^*}$$

where

$$D = \frac{u\prime(c_t^u) - u\prime(c_t^e)}{u\prime(c_{t+1}^u)}.$$

Note that the coefficient β_s from our RD model gives the effect of a severance payment equal to 2 months of salary on the exit hazard in the first 140 days of job search. Since we use a proportional hazards specification, $\beta_s \approx \partial \log s_t^*/\partial A_t \times 2w$, where w is the monthly wage.[16] Likewise, the coefficient β_e from our RD models gives the effect of eligibility for 10 additional weeks (or 2.5 months) of regular UI benefits. The net income from extended benefits is approximately $2.5w\rho(1-UA)$, where ρ is the replacement rate and UA is the probability of receiving unemployment assistance once regular UI runs out. Assuming that the replacement rate is 55%, and that $UA = 0.13$ (the average fraction of people in our sample who receive unemployment assistance after exhausting regular benefits at 20 weeks), the coefficient β_e from our RD models provides an estimate of $\partial \log s_t^*/\partial b_{t+1} \times 1.2w$.[17] The predicted value for the ratio of the coefficients $\beta_s$and β_e is therefore:

$$\frac{\beta_s}{\beta_e} = \frac{\partial s_t^*/\partial A_t}{\partial s_t^*/\partial b_{t+1}} \times \frac{2w}{1.2w} = D \times \frac{1+r}{1-s_{t+1}^*} \times 1.67.$$

Given a value of D,we need to multiply by $1.67(1+r)$ and divide by $(1-s_{t+1}^*)$ to obtain a prediction for β_s/β_e . The latter term adjusts future unemployment benefits for the probability they will be received. Interpreting period t as the first 20 weeks of job search, this adjustment factor is just the probability of exhausting regular UI benefits, which is approximately 36 percent. Treating r as small (since it only applies over a 20 week period) we then predict:

$$\frac{\beta_s}{\beta_e} = 4.64D.$$

Two Benchmarks.

As we noted in Section III, our theoretical model is sufficiently general to nest a wide range of preferences and financial environments faced by job seekers. Different degrees of risk aversion and different abilities to borrow and lend over the course of a spell of unemployment will predict

[16]The fraction of people who find a job in under N days is approximately the product of the daily hazard rates up to day N (assuming the daily hazard is small). Thus, the effect of some variable on the log of the probability of finding a job within N days is approximately equal to the effect on the log hazard rate.

[17]$1.2w$ = 2.5 months × 55% replacement rate × 87% change of not getting UA benefits.

different values for the ratio D, and lead to different predictions for the relative magnitude of the severance pay and EB effects in our models. Here we consider two cases that represent useful "bounds": the case where individuals have unrestricted access to credit at a fixed interest rate – the "permanent income hypothesis" (PIH) benchmark; and the case where individuals set consumption equal to income in each period – the "cash constrained" benchmark.[18]

Assume that the family income of a job seeker includes his or her earnings or UI benefits, and other sources that total F (euros per month). Let $\sigma = w/(w+F)$ denote the share of the job-seeker's earnings in total family income. In the cash-constrained case, $c_t^e = w + F$, and $c_t^u = b_t + F = \rho_t w + F$, where ρ_t is the replacement rate in period t. Assuming that $u(c)$ is in the constant relative risk aversion class,

$$\begin{aligned} D &= \frac{u\prime(b_t + F) - u\prime(w + F)}{u\prime(b_{t+1} + F)} \\ &= \frac{u\prime((\sigma\rho_t + (1-\sigma)) - u\prime(1)}{u\prime((\sigma\rho_{t+1} + (1-\sigma))} \end{aligned}$$

Note that if $\rho_t = 1$, or if $\sigma \approx 0$, then $D = 0$. Otherwise, the predicted value of D is greater, the larger is σ, the smaller is ρ_t, and the more elastic is $u\prime(c)$, i.e., the greater is the coefficient of relative risk aversion. Data from the 2004 Survey of Income and Living Conditions show that the average wage earner between the ages of 20 and 49 in Austria contributes just under one-half of his/her family income. Assuming that $\sigma = 0.50$ and that $\rho_t = \rho_{t+1} = 0.55$, a cash-constrained job seeker with a risk aversion coefficient of 2.5 will have a value of $D = 0.47$, implying a predicted value for β_s/β_e of about 2.2.[19]

The calculation of D in the PIH case is more complicated. Clearly, however, D will be relatively small if people can borrow and lend relatively freely. We therefore proceed by deriving an upper bound on D. To begin, assume that individuals have a relatively long work life/planning horizon, so that the annuity income from any asset amount A is approximately $r/(1+r)A$. An individual who finds a job in period t, with asset income A_t, will then set $c_t^e = w + F + r/(1+r)A_t$. The first order condition for optimal consumption for an individual who does not find a job at the beginning

[18] Given the evidence that unemployment responds to severance pay, we rule out the "full insurance" case. Likewise, given that eligibility for EB's affects the exit rate from unemployment in the first 20 weeks of unemployment, we rule out the "fully myopic" case.

[19] The predicted ratio falls to 1.5 if the coefficient of relative risk aversion is 1.5.

of period t and for whom the lower bound on assets is not binding can be written as:

$$u'(c_t^u) = E_t[s_{t+1}^* u'(c_{t+1}^e) + (1 - s_{t+1}^*)u'(c_{t+1}^u)]$$

where s_{t+1}^* is the optimal level of search intensity in period $t+1$.[20] Assuming that a job seeker can always find a job within T months, this implies:

$$\begin{aligned} u'(c_t^u) &= E_t[s_{t+1}^* u'(c_{t+1}^e) + (1 - s_{t+1}^*)s_{t+2}^* u'(c_{t+2}^e) + (1 - s_{t+1}^*)(1 - s_{t+2}^*)s_{t+3}^* u'(c_{t+3}^e) + ...] \quad (13) \\ &= \sum_{j=1}^{T} p_{t+j}^* u'(c_{t+j}^e) \end{aligned}$$

where p_{t+j}^* is the probability of finding a job j months after the start of a spell of unemployment in period t. A lower bound on the optimal path of c_{t+j}^e can be determined by noting that $c_{t+1}^e \geq c_t^e - r(w - b_t)$, since the dissaving rate during a spell of unemployment is never bigger than $w - b_t$ (euros per period). Given p_{t+j}, the replacement rate, the interest rate, initial assets, and the job loser's share of family income, it is then straightforward to construct an upper bound on $u'(c_t^u)$ using equation (13). The denominator of D is $u\prime(c_{t+1}^u)$. Note however that $c_{t+1}^u \leq c_t^u$ (since an unemployed individual runs down wealth): hence $u\prime(c_{t+1}^u) \geq u\prime(c_t^u)$. We can therefore derive an upper bound on D by constructing an upper bound for $D^* = (u\prime(c_t^u) - u\prime(c_t^e))/u\prime(c_t^u) \geq D$.

As we noted in the discussion of Table 2, over 95% of the job losers in our sample have left unemployment within a year. The fraction who remain unemployed longer than 18 months is only 3%. For purposes of calibrating the PIH benchmark, we set T=18, and use the observed distribution of waiting times to a new job in our sample to estimate p_{t+j}^*.[21] A potential problem is that even after 18 months, some 18% of the sample have not returned to work. We believe that most of the non-returners have either left the labor force, taken a job in the sectors that are not covered by our data set (mainly self employment and the government sector), or left the country.[22] We therefore calculate the distribution of times to re-employment ignoring those who are censored after 18 months.[23]

[20] This is derived by using the first order condition for A_{t+1} in equation (2), and the results that $V'_{t+1}(A_{t+1}) = u'(c_{t+1}^e)$, $J'_{t+1}(A_{t+1}) = s_{t+1}^* V'_{t+1}(A_{t+1}) + (1 - s_{t+1}^*)U'(c_{t+1}^u)$, and $U'_{t+1}(A_{t+1}) = u'(c_{t+1}^u)$.

[21] Note that as written our model implies that search intensity (which is also the probability or returning to work) rises over the spell of unemployment, since assets are decreasing (see also Lenz and Tranaes, 2004). More realistically, the marginal cost of search will rise over time as people exhaust their best leads for finding a new job.

[22] The fraction of job losers who do not find a job after 3 years is nearly as high – XX% – consistent with the view that they are no longer looking for work.

[23] The resulting distribution has a 17% probability of re-employment within a month, 45% within 3 months, 71% within 6 months, and 93% within a year.

The term in the numerator of D (or D^*) reflects the reaction of job losers to the increase in assets when they receive severance pay. Since everyone who is eligible for severance pay receives EB's, we assume that $\rho_t = 0.55$, for the first 30 weeks of unemployment, and that $\rho_t = 0.55 \times UA$ thereafter, where UA is the probability of receiving unemployment assistance, which we set to 13%. We also assume that a typical job loser contributes 50% of his or her family income, and that at the time of job loss assets are 0. Assuming that the coefficient of relative risk aversion is 2.5, and that individuals face an annual interest rate of 10%, we obtain the prediction $D^* = 0.029$. Raising the interest rate to 15% leads to a predicted value $D^* = 0.043$.[24] We believe this is a generous upper bound on D for the PIH benchmark. Multiplying by the scaling factor of 4.64, the PIH benchmark suggests that the ratio of the severance pay effect to the EB effect should be no larger than 0.20.

Comparing the Estimates to the Benchmarks

How do our estimates of the relative effects of severance pay and extended benefits compare to these benchmarks? For purposes of this exercise, we focus initially on the double discontinuity specification in column 7 of Table 3, which includes a broad set of controls. This model yields an estimate of $\beta_s = -0.083$ (standard error=0.018) and an estimate of $\beta_e = -0.067$ (standard error=0.016). Thus our estimated ratio is $\beta_s/\beta_e = 1.24$, with a standard error of 0.30.[25] This estimate is just over half-way between the prediction from a simple PIH model and the prediction from a simple cash-constrained model. Moreover, the estimate is precise enough to rule out a value below 0.60 or above 1.9 at conventional significance levels.

One useful thought experiment is to ask: what interest rate would we have to assume in the PIH model to generate a predicted value for the ratio β_s/β_e at the lower bound of the confidence interval? The answer is a 42% annual interest rate. Even with a 33% interest rate – the rate suggested by Carroll (2001) to capture precautionary savings motives given his preferred income generating model – the predicted value of β_s/β_e is only 0.45. Thus, we interpret the relative magnitide of the estimates from the models of the time to a new job in Table 3 as providing fairly strong evidence against the PIH benchmark.

Nevertheless, estimates from the models of the duration of unemployment are more favorable to the PIH model. For example, the estimated ratio β_s/β_e from the model in column 3 of Table 3 is 0.55 (with a standard error of 0.30). Clearly, this estimate does not rule out the possibility that

[24] Assuming r=0.15, but reducing the coefficient of relative risk aversion to 1.5, we get the prediction $D^* = 0.026$.

[25] We calculated the standard error using the delta method. The estimates of β_e and β_e are slightly negatively correlated.

job searchers are following the PIH model with an interest rate on the order of 10-15%.

IX Conclusions

In this paper, we used discontinuities in the Austrian unemployment benefit system to distinguish between commonly used models of dynamic household behavior. We reached three main findings: (1) A cash grant equivalent to two months of wages induces substantial changes in search behavior beyond what would be predicted by a benchmark lifecycle model without liquidity constraints; (2) extending UI benefits also affects search behavior early in the spell, providing evidence that households are somewhat forward-looking; and (3) lengthening durations through EB and severance pay policies has no effect on subsequent job match quality. Using a model that nests a continuum of cases from perfect smoothing to complete myopia, we conclude that the evidence favors a model of job search with varying job search intensity and incomplete consumption smoothing. We also provide a metric that can be used to calibrate dynamic models of household behavior to our empirical findings.

These results have several implications for macroeconomics and public finance. At a broad level, they suggest that temporary changes in income have more important economic consequences than traditional models suggest. For example, households' sensitivity to cash-in-hand suggests that temporary fiscal policy changes such as tax cuts could have significant effects on the economy. The evidence of imperfect smoothing also suggests that there may be a significant role for temporary income assistance programs such as unemployment insurance, temporary welfare assistance, and workers compensation. The finding that cash grants change search behavior in a manner similar to UI benefit extensions also suggests that much of the behavioral response to temporary benefit social insurance programs is an "income" or liquidity effect rather than moral hazard caused by distortion in relative prices (Chetty 2006). Analyzing these issues formally in dynamic models calibrated to match the evidence documented here would be an interesting direction for further research.

TABLE 1
Testable Predictions

Prediction	A. PIH with complete mkts.	B1. Buffer stock w/match effects	B2. Buffer stock w/search intensity	C. Rule of thumb "watching TV"
	Model			
1. Sev Pay affects duration?	**N**	Y	Y	Y
2. Sev Pay affects search outcomes?	N	**Y**	N	N
3. Benefit extension affects initial hazards?	Y	Y	Y	**N**

Table 2
Sample Characteristics: Austrian Job Losers, 1980-2002

	Mean	Median	Std. Dev.
Worker Characteristics:			
Age in Years	31.11	30.00	7.98
Female	0.52	1.00	0.50
Post-compulsory Schooling	0.60	1.00	0.49
Married	0.44	0.00	0.50
Austrian Citizen	0.88	1.00	0.33
Blue Collar Occupation	0.57	1.00	0.49
Previous Job/Employment:			
Months of Tenure	26.47	22.77	12.02
Months Worked Past 5 Years	42.01	44.80	13.89
Eligible for Severance Pay	0.20	0.00	0.40
Eligible for Extended UI	0.66	1.00	0.47
Previous Wage (Euros/mo)	1206.04	1129.95	537.01
Post-Layoff Outcomes:			
Duration of Unemployment	4.92	3.07	8.66
Unemployed < 20 Weeks	0.64	1.00	0.48
Unemployed < 52 Weeks	0.94	1.00	0.24
Observed in New Job	0.92	1.00	0.27
Among those with New Job:			
Months to Re-employment	9.34	4.00	18.40
Change in Log Wage	-0.03	-0.01	0.51

Note: Based on sample of 609,546 job losers over the period 1980-2001. Sample includes people age 20-50 who worked at their previous firm between 1 and 5 years. Job quitters and people losing a job in construction are excluded. Wages are expressed in real (year 2000) Euros.

TABLE 3

Hazard Model Estimates: Effects of Severance Pay and EB on Durations

	(1) Restricted sample	(2) Restricted sample	(3) Full sample	(4) Restricted sample	(5) Restricted sample	(6) Full sample	(7) Full samp. w/controls
Dependent Var:	Unemployment Exit Hazard			Job Finding Hazard			
Severance pay	-0.064		-0.043	-0.122		-0.105	-0.083
	(0.018)		(0.016)	(0.020)		(.018)	(.018)
Extended benefits		-0.089	-0.078		-0.096	-0.081	-0.067
		(0.016)	(0.014)		(0.018)	(0.016)	(0.016)
Sample size	491,873	491,873	609,546	491,873	491,873	609,546	579,059

NOTE--All specs are Cox hazard models that include cubic polynomials
with interactions with sevpay and/or extended benefit dummy. See text for details.

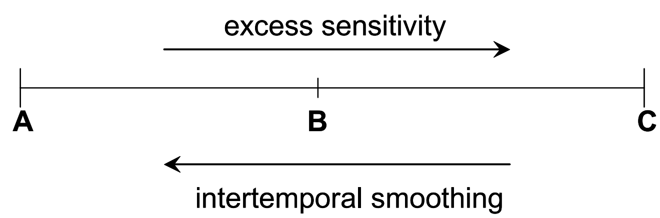
TABLE 4

Effects of Severance Pay and EB on Search Outcomes

	(1) No controls	(2) Full controls	(3) No controls	(4) Full controls
Dependent var:	log wage change	log wage change	job leaving haz.	job leaving haz.
Severance pay	-0.013	-0.003	-0.018	-0.016
	(0.006)	(0.005)	(0.014)	(0.014)
Extended benefits	-0.008	-0.011	-0.004	-0.004
	(0.006)	(0.005)	(0.012)	(0.013)

All specs include cubic polynomials with interactions with sevpay and ebl. Columns (1) and (2) report coefficients from OLS regressions; columns (3) and (4) report Cox hazard model coefficient estimates. See text for details.

Figure 0
Continuum of Dynamic Models



A. Permanent income hypothesis with complete markets

B. Buffer stock models (Deaton/Carroll)

C. Complete myopia (consumption = income)

Figure 1a

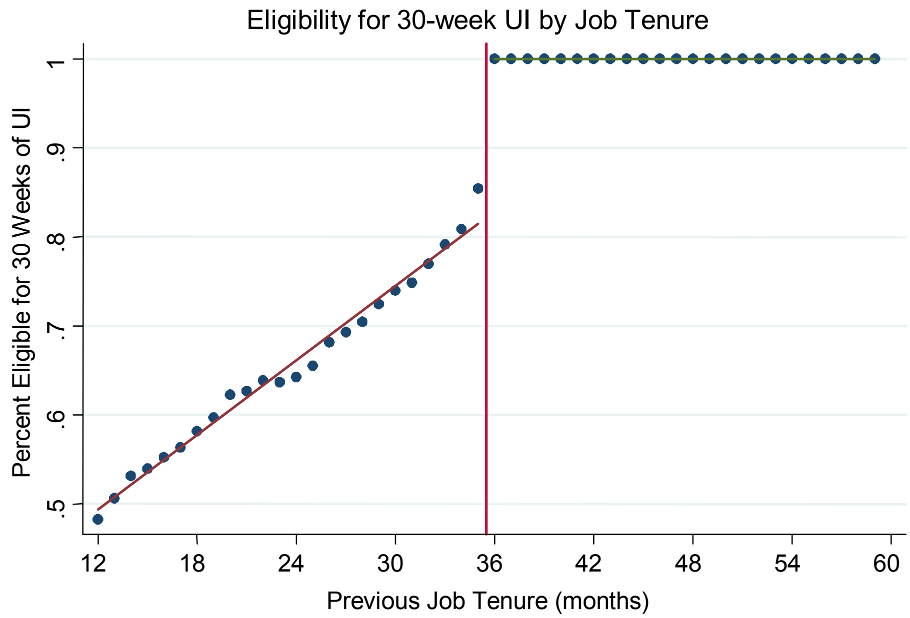
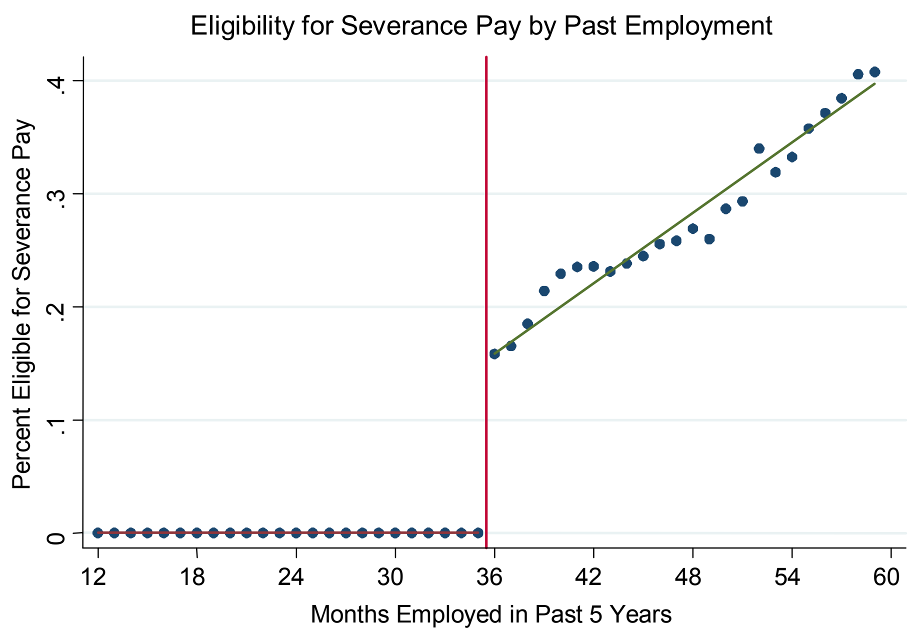


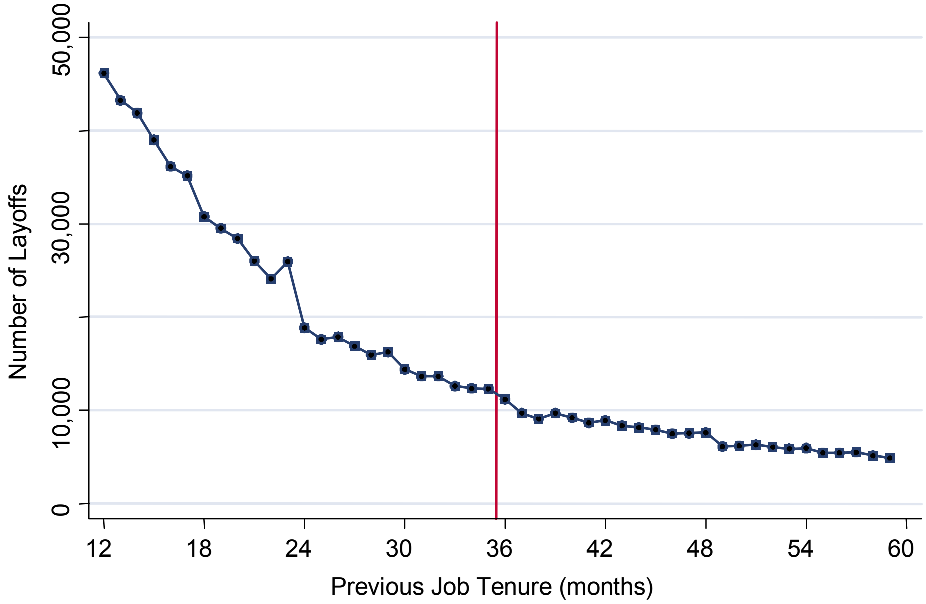
Figure 1b



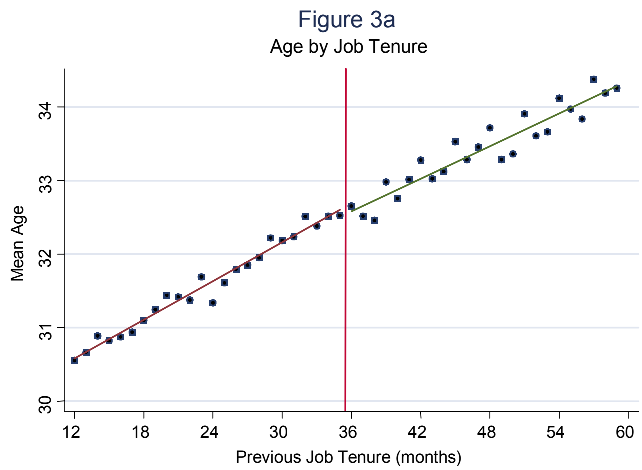
NOTE–These figures illustrate the "double discontinuity" at 36 months. Figure 1a plots fraction of individuals who receive 30 weeks of UI benefits among those laid off after x months at the current job. Figure 1b plots fraction of individuals who receive severance pay in the group of individuals who are laid off with x number of months employed (at any firm) in the past five years. Both figures are drawn on the full sample (see notes to Table 2).

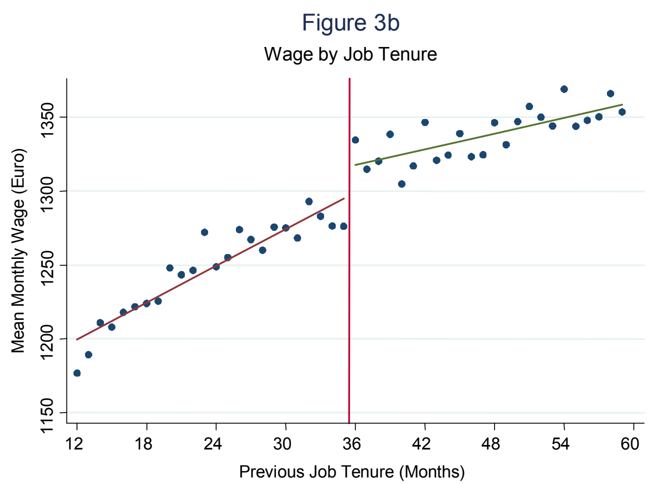
Figure 2

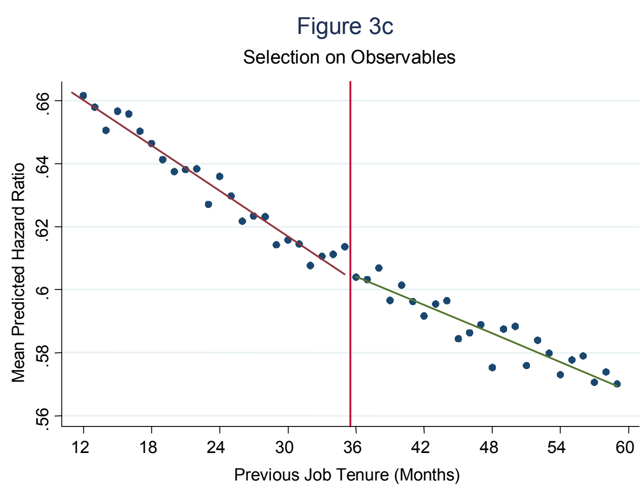
Frequency of Layoffs by Job Tenure



NOTE–This figure shows the total number of layoffs in the full sample at each month of previous job tenure.







NOTE–Figure 3a and 3b show average age and wage in each tenure-month category in the full sample. Figure 3c shows average predicted hazard ratios from fitting a Cox model on observables.

Figure 4a

Effect of Severance Pay on Unemployment Exit Rates

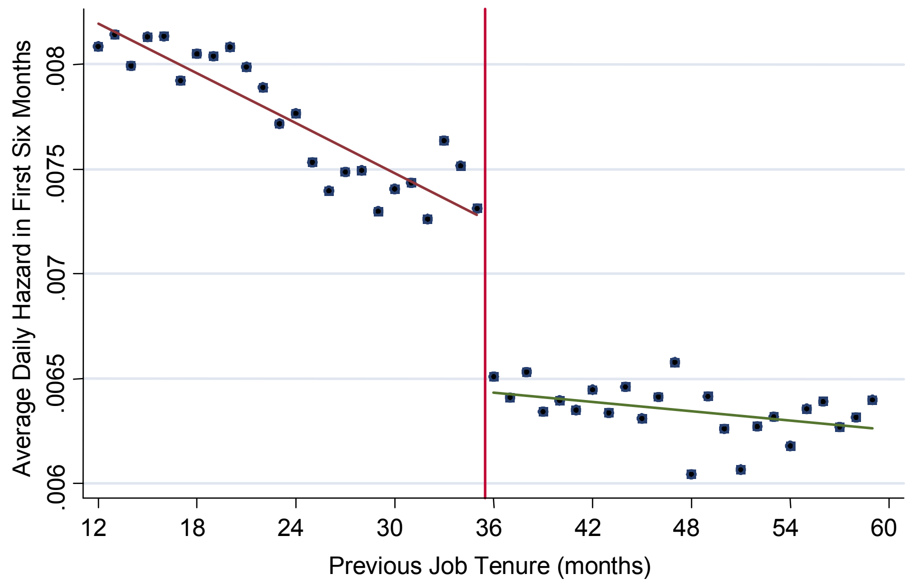
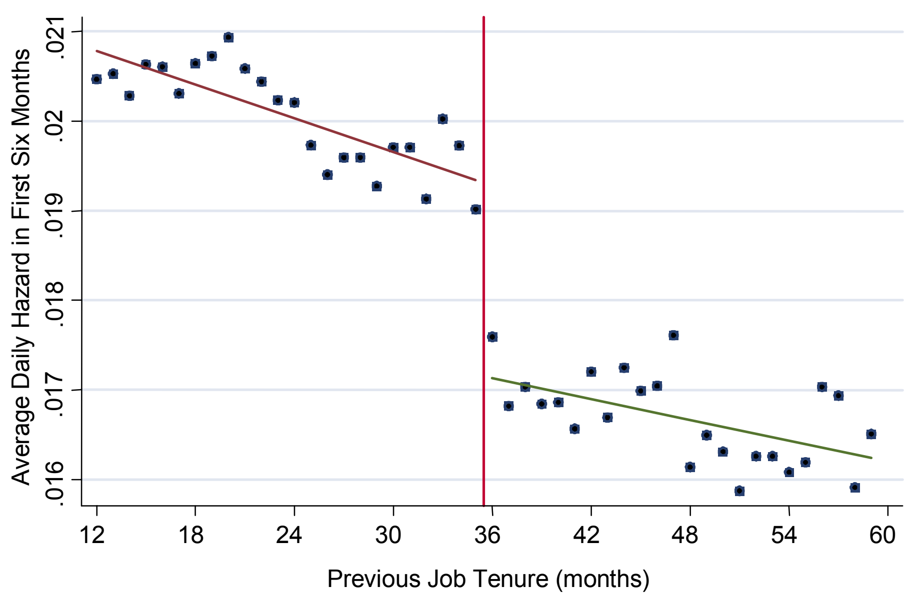


Figure 4b

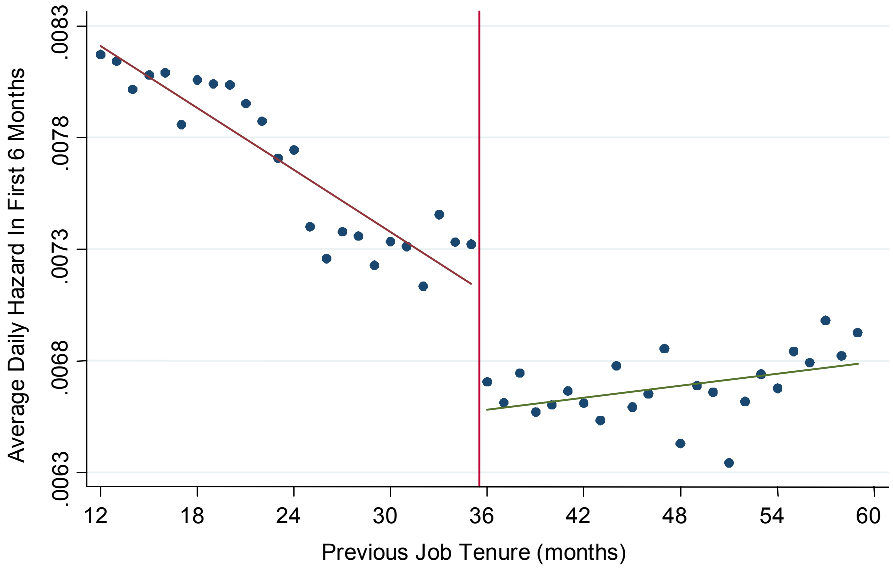
Unemployment Exit Hazards Adjusted for Covariates



NOTE–These figures plot the average daily hazard rate of exiting unemployment in the first six months of the spell for each tenure-month category. Figure 4a plots raw means, while Figure 4b plots the means of the stratified baseline hazards recovered from fitting a Cox hazard model with the set of observables defined in the text.

Figure 5

Effect of Severance Pay on Unemployment Exit Rates (Restricted Sample)



NOTE–This figure replicates Figure 4a in the “restricted sample” of individuals who have had at least one day of employment at another firm within the past five years. In this restricted sample, there is no discontinuity in extended benefits at 36 months of job tenure, and hence the entire discontinuity in this figure can be attributed to severance pay.

Figure 6a

Effect of Severance Pay on Weekly Unemployment Exit Hazards

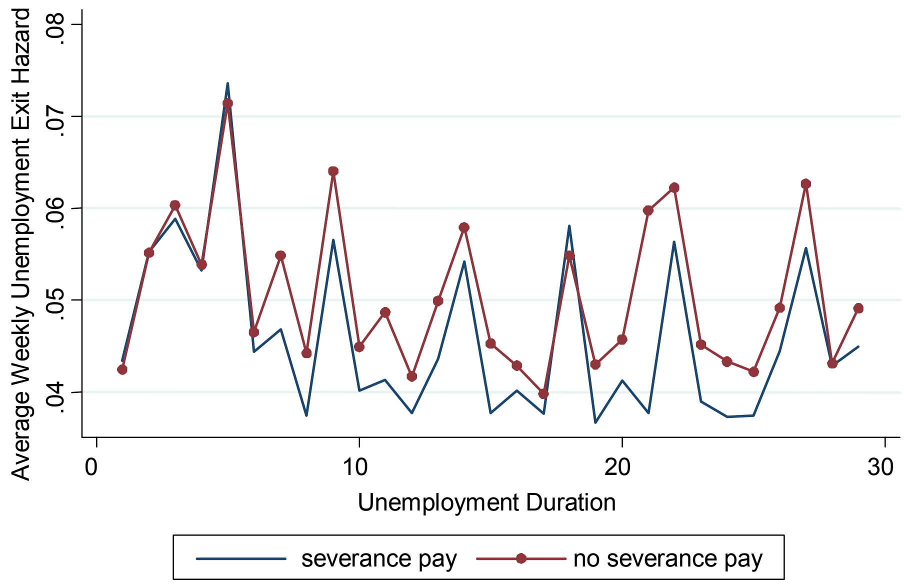
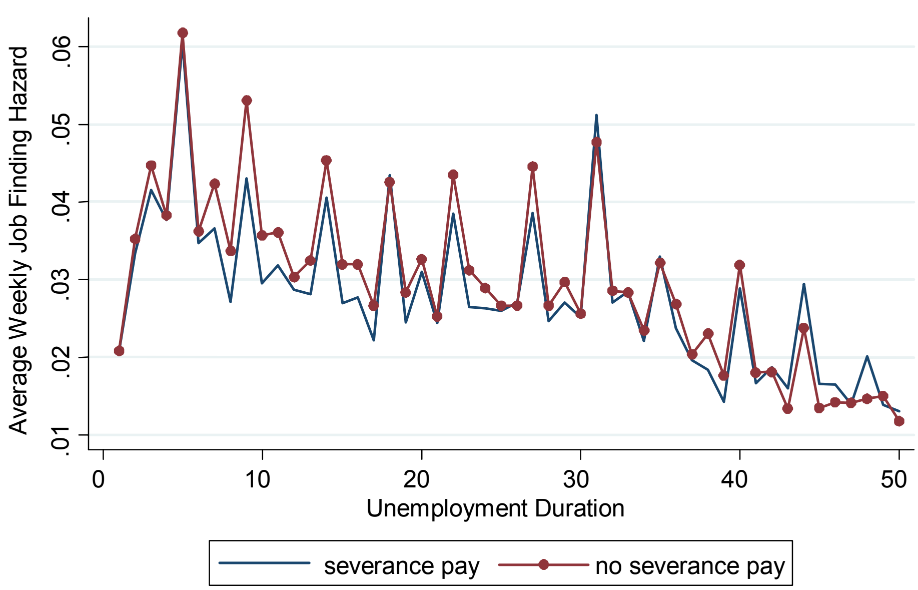


Figure 6b

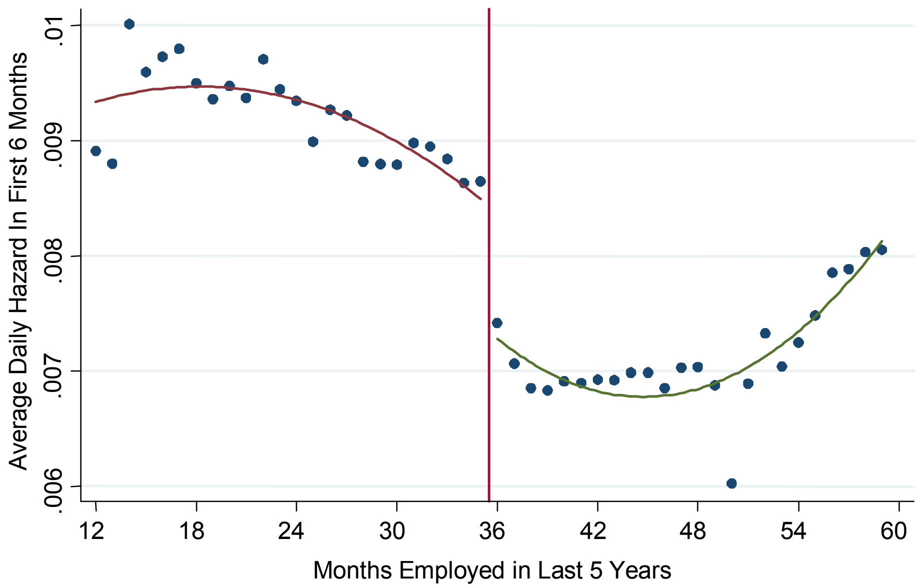
Effect of Severance Pay on Weekly Job Finding Hazards



NOTE–Figure 6a plots the weekly hazard of exiting unemployment for two groups: those laid off with 36-38 months of job tenure (who receive severance pay) and those laid off with 33-35 months of job tenure (who do not receive severance pay). Figure 6b replicates these two curves, plotting the weekly hazard of finding a new job instead. Both figures are drawn using the restricted sample.

Figure 7

Effect of Extended Benefits on Unemployment Exit Rates



NOTE–This figure plots the average daily hazard rate of exiting unemployment in the first six months of the spell for groups of individuals classified by number of months employed in the past five years. The figure is drawn using the restricted sample.

Figure 8a

Effect of Extended Benefits on Weekly Unemployment Exit Hazards

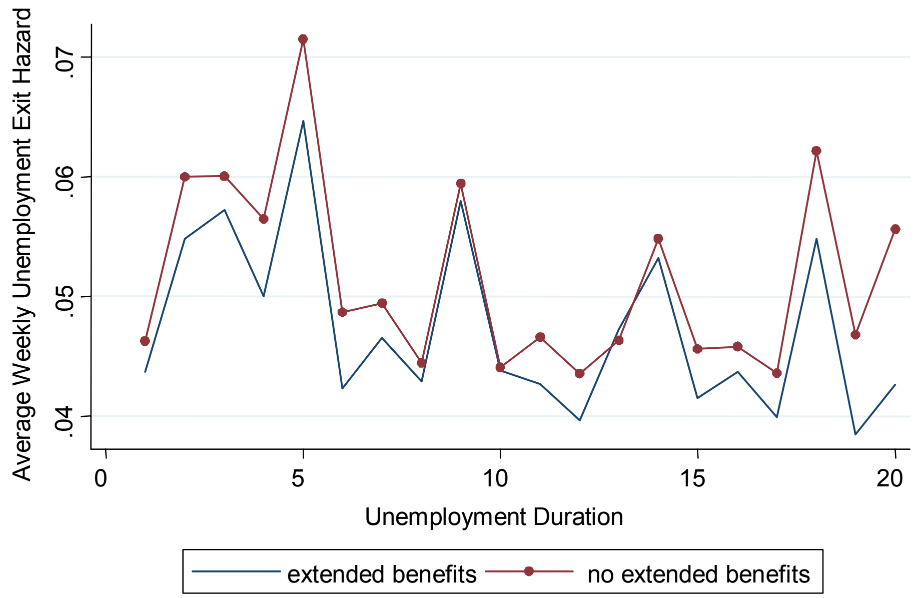
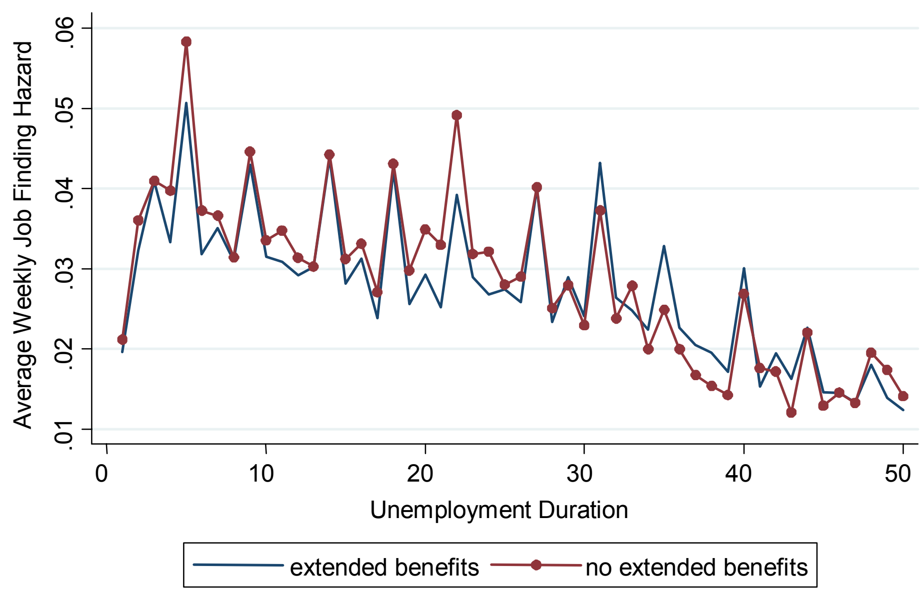


Figure 8b

Effect of Extended Benefits on Job Finding Rates



NOTE–Figure 8a plots the weekly hazard of exiting unemployment for two groups: those laid off with 36-38 months worked in the past five years (who receive 30 weeks of UI benefits) and those laid off with 33-35 months of worked in the past five years (who receive 20 weeks of UI). Figure 8b replicates these two curves, plotting weekly hazard of finding a new job instead. Both figures are drawn using the restricted sample.

Figure 9a

Effect of Severance Pay on Wage in Next Job

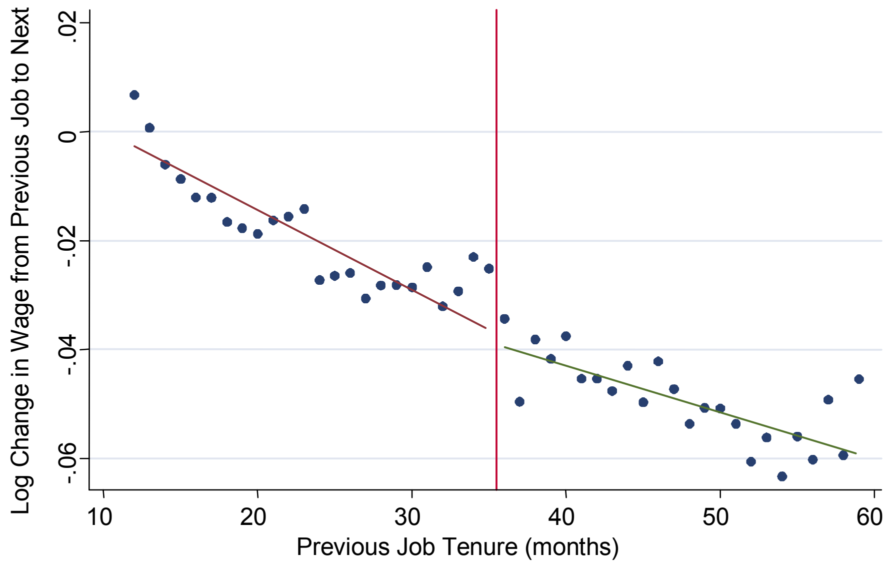
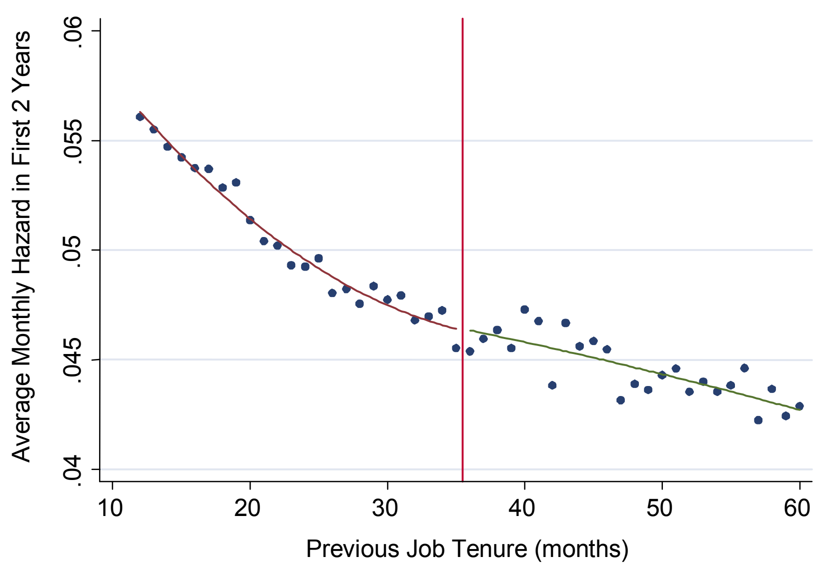


Figure 9b

Effect of Severance Pay on Duration of Next Job



NOTE–Figure 9a plots the average growth rate (log change) of monthly wages from the last year in the previous job to the first year in the next job in each tenure-month category. Figure 9b plots the average monthly hazard of leaving the next job in the first two years after getting it. Both figures are drawn using all individuals in the full sample who find a new job before the end of the sample.

Figure 10a

Effect of Extended Benefits on Wage in Next Job

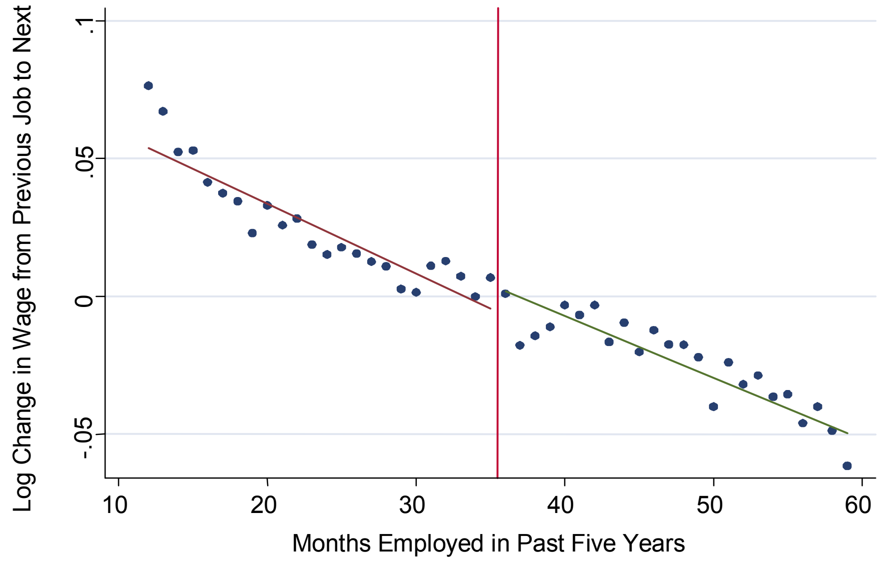
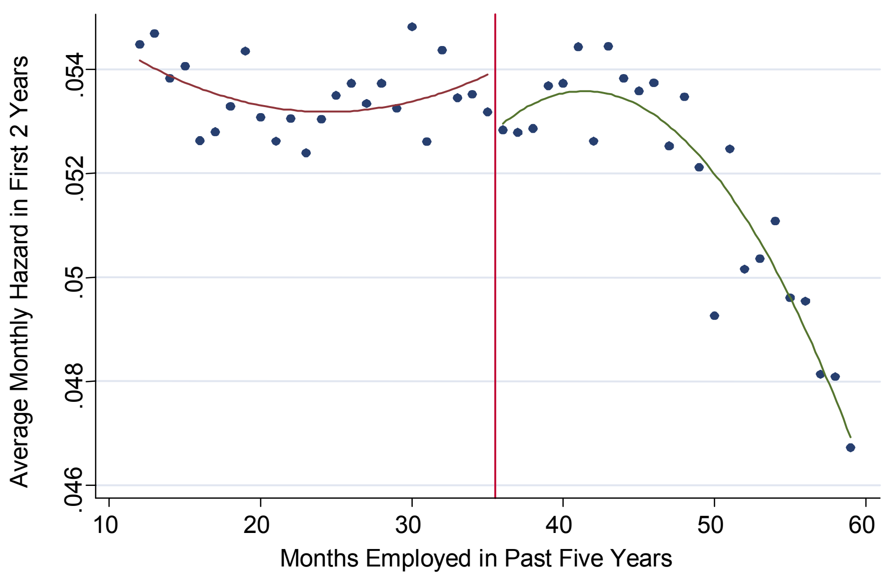


Figure 10b

Effect of Extended Benefits on Duration of Next Job



NOTE–Figure 10a plots the average growth rate (log change) of monthly wages from the last year in the previous job to the first year in the next job for groups of individuals classified by number of months employed in the past five years. Figure 9b plots the average monthly hazard of leaving the next job in the first two years after getting it. Both figures are drawn using all individuals in the full sample who find a new job before the end of the sample.