Preliminary Draft

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 in the first 20 weeks by 6-10%; and (3) the increases in the duration of job search induced by both programs have no effect on job match quality, as measured by wages or the duration of the next job. We use a job search model to show how these estimates can distinguish between commonly used dynamic models of household behavior, and develop a metric that can be used to calibrate such models to match our empirical findings. Our empirical findings are inconsistent with a simple permanent income model as well as "rule-of-thumb" models with a myopic agent. The results favor a model where agents are forward-looking yet have limited ability to smooth consumption.

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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 set 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 2006a).

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. Conceptually, 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, we show that the effects of cash-in-hand on consumption can be inferred from our estimates of the labor supply responses using a simple job search model.

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 daily employment data are available for the universe of private sector workers, giving us a sample of 650,000 job losers. The sharp discontinuity and large sample allow us to obtain more

[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.

precise estimates of the effects of cash-in-hand than consumption-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 pre-tax salary, or 3,300 Euros at the sample median. This makes our analysis less subject to Browning and Crossley's (2001) criticism that the welfare cost of failing to smooth over small amounts (e.g. the $300-$600 tax rebates in Johnson, Parker, and Souleles 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 possibility 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 discernible and economically significant effect on the duration of unemployment and time to re-employment. The hazard rate of finding a new job during the first 20 weeks of the unemployment spell (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 exhibit 6-10% lower rates of job finding during the first 20 weeks of search. This result shows that individuals anticipate the longer duration of benefits and accordingly reduce search effort *before* the benefit extension takes effect. Hence, this finding provides strong evidence of forward looking behavior, inconsistent with a "rule of thumb"

[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).

model where agents are completely myopic.

Third, we find that neither lump sum severance payments nor extended benefits have any effect on the "quality" of subsequent jobs. Mean wages, the duration of subsequent jobs, occupational mobility, and other measures of job match outcomes are essentially unaffected by eligibility for severance pay or extended benefits. An advantage of our results relative to prior studies of match quality is that our estimates are sufficiently precise to rule out fairly small match quality gains. For example, the additional search induced by the severance payment or benefit extension is estimated to raise the mean subsequent wage by less than 1% at the upper bound of the 95% confidence interval. Thus, severance pay and extended benefits appear to extend durations primarily 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 range of models (from the PIH to complete myopia), we test between various commonly used dynamic models of household behavior. We first show that two widely applied benchmark cases – a simple version of the PIH where agents smooth relative to permanent income and a credit-constrained model where agents set consumption equal to income – are rejected with $p<0.001$. We then develop a simple metric to characterize the types of models that can match the data. Our estimates suggest that deviations from the PIH are substantial: a typical job searcher behaves as if they are roughly halfway between the PIH and fully credit-constrained benchmarks. We conclude that models such as buffer-stock behavior with variable job search intensity, incomplete consumption smoothing, and a forward-looking agent fit the data. This finding implies that temporary income support and tax rebate policies can have substantial economic effects. Perhaps more importantly, the estimated moment can be easily matched when calibrating dynamic models to analyze such policies 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 (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 nondurable 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.

[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 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.

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 that the entire response of search behavior to UI benefits is due to moral hazard (a substitution effect) rather than wealth effects. Chetty (2006b) 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 a substantial portion of the UI benefit effect is a wealth effect, consistent with our results 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 (1976) 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, Chetty 2006a, Shimer and Werning 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

We begin our analysis by presenting a simple job search model that provides a structural framework for interpreting our empirical findings. The model we analyze nests a range of dynamic models commonly used in the literatures on consumption and search, such as the standard permanent income model, buffer stock models, and "rule of thumb" behavior where the agent is myopic and sets consumption equal to income. We use the model for two purposes. First, we derive a set of comparative statics predictions on the effect of assets and UI benefits on search behavior to test between certain benchmark models. Second, we construct a metric based on the relative effects of severance pay and benefit extensions that can be used in calibrating models to match the data.

Model Setup. The model is closely based on Lentz and Tranaes (2004), who incorporate intertemporal consumption and savings decisions into a standard job search model. Consider a discrete-time setting where an individual has a finite planning horizon and a subjective rate of time discounting of δ. Let r denote the fixed interest rate in the economy. 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

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

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 agent chooses s_t to equate the marginal cost of search effort with the marginal value of search effort, which is given by 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 dynamic models of household behavior, because higher unemployment benefits increase durations regardless of the degree of intertemporal consumption smoothing. 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 therefore 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 smooth 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 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 (2006b). 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.[5]

Prediction 2: Extended Benefits. Next, 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:

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

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 $1 + \delta$. A completely myopic "rule of thumb" agent places no value on the future and has $\delta = \infty$. 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 respond more to benefit extensions.

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 these three predictions and shows how they distinguish between four potential models of household behavior.

[5] 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.

A Metric for Calibration. Combining equations (6) and (8), we obtain a simple metric that can be used to quantitatively identify the class of models that fit the data. In particular, consider the ratio of the effect of a 1 euro increase in assets and a 1 euro increase in the value of future benefits on search effort:

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

where

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

The moment $\widetilde{m}$ can be easily simulated in dynamic models of household behavior because it requires knowledge only of the utility function and consumption drop ($\frac{c_e - c_u}{c_e}$) caused by unemployment. In particular, the ψ function can be left unspecified, permitting a calibration that is relatively robust to the way in which the the search process is modelled. The empirical value of $\widetilde{m}$ can be obtained from the ratio of the severance pay and benefit extension effects estimated in our empirical analysis.

Figure 1 illustrates the idea underlying the calibration by ordering a set of models on a continuum by their implied value of $\widetilde{m}$. 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 search behavior to cash-in-hand. At the left extreme of the continuum is the full insurance model, where consumption is perfectly smooth across states and time, and temporary income shocks have no effect on behavior, implying $D = \widetilde{m} = 0$. 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, severance pay affects search effort substantially but benefit extensions have no effect, implying $\widetilde{m} = \infty$. The interior of the continuum includes models that have intermediate values of $\widetilde{m} \in (0, \infty)$, such as the simple permanent income hypothesis without complete markets, buffer stock models (Deaton 1991; Carroll 1992), and credit-constrained models where agents are forward looking but face a binding asset constraint.

In section VIII, we calibrate $\widetilde{m}$ for some benchmark models and compare these numbers with the corresponding value estimated from the data. This exercise can be viewed as a means of identifying the "location" of the representative household in the data on the continuum in Figure 1. More precisely, $\widetilde{m}$ identifies a plane within the space of parameters defined by preferences and financial technologies (e.g. the asset limit, insurance market completeness, discount rate, risk aversion). Of course, a single moment is insufficient to pin down all of these parameters, but it does turn out to be sufficient to distinguish between certain benchmark cases. The value of $\widetilde{m}$ is also of direct interest for assessing the optimal level of unemployment insurance, since D is a sufficient statistic for determining the marginal benefits of social insurance in a general class of dynamic models (Chetty 2006a).

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 and to give at least 6 weeks notice of a pending mass layoff (Winter-Ebmer 2002). Despite these features, rates of job turnover 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. However, rates of employment for younger workers are higher and are comparable to the U.S.[6] 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 collar workers in 1921 and was expanded to include all other workers in 1979. Severance payments are made by firms according to a fixed schedule legislated by the government. In particular, workers outside of the construction industry who are laid off after 3 years of service must be given a severance payment equal to 2 months of their previous salary.[7] Payments are generally made within one month of the job termination, and are not taxable.

Job losers with 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

[6] 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).

[7] 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 (outside our dataset).

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. 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 30 weeks of benefits (which we term "extended 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. Importantly, however, UA benefits are reduced euro-for-euro by the amount of any other family income. As a result, the average UA replacement rate is 38% of the UI benefit level in the population.[8]

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 one discontinuous policy change.

Figure 2a 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 80% of individuals laid off with 35 months of job tenure satisfy this condition, there is a 20 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 (20 percentage point) effect of extended benefits. A similar double discontinuity arises at the threshold for EB, as shown in Figure 2b. The fraction of individuals receiving severance pay jumps discontinuously by approximately 20% at 36 months worked. Hence changes in behavior around 36 months worked are likely to be caused

[8]See section VIII for details on this computation.

primarily by EB, but could also be partly attributed to severance pay. We account for the double discontinuity in our empirical analysis using two independent methods described below.

IV.A Data and Sample Definition

We use data from the Austrian social security registry, which covers the universe of private sector employees, spanning 1980-2001. The dataset includes daily information on employment and registered unemployment 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).

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, where the fraction eligible jumps from 0 to 1 at the discontinuity (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 include only individuals who take up UI benefits within 28 days of job loss, thereby eliminating voluntary quitters (who are ineligible for severance pay and have a 28 day waiting period for UI eligibility). 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 1 and 5 years of the past 5 years. Consequently, everyone in the sample is eligible for UI benefits (though not all are eligible for extended benefits), and everyone in the sample is eligible for either 2 months of severance pay, or none. Finally, we drop individuals who were recalled to their prior firm in order to eliminate temporary layoffs who may not be searching for a job. These restrictions leave us with a sample of 650,922 unemployment spells.

Table 2 shows summary statistics for the full sample. Sixty percent of the sample has additional schooling beyond the compulsory level – most members 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 in Austria.[9] 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 numbers of months worked is 41.2. Roughly one-fifth of the sample is eligible for severance pay, while 66% are eligible for extended UI benefits. The mean wage is 17,034 Euros per year in year 2000 Euros. Wages are top-coded at the social security tax cap in the dataset. This cap binds for very few individuals in our sample: less than 2% of the observations have censored wages.

There are two measures of unemployment durations 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 job training and job search assistance services offered by the agency. This measure corresponds to the official definition of "unemployment" in government statistics, and 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 issue. Spells of registered unemployment are relatively short: the median spell length is less than 3 months; 64% of spells end within 20 weeks; and 94% end within a year.

The second measure of the duration of job search, which we label "nonemployment duration," is the amount of time that elapses from the end of the previous job to the start of the next job. Although 92% of the sample is 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.[10] As a result, the median nonemployment duration is 4.3 months while the mean is nearly 17 months. 51% of individuals find a new job within 20 weeks, and 77% find a new job within one year.

We use the nonemployment duration measure as our primary measure of duration of joblessness in our analysis for two reasons. First, the nonemployment measure captures actual transitions from a lost job to a new job, which corresponds most directly to the notion of "labor supply" in our job search model. Second, the unemployment measure suffers from being mechanically affected

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

[10] These individuals may take a job in the public sector (which is not covered by our dataset), leave the country (to work in Germany or Switzerland), or simply drop out of the labor force.

by the program's parameter. For example, the extension of benefits from 20 weeks to 30 weeks can mechanically raise unemployment durations even if it has no impact on job finding rates. A drawback of the nonemployment duration measure is that it assumes that all nonemployed individuals are still searching for a job, whereas some of these individuals may have exited the sample or the labor force temporarily (e.g. to receive training). To evaluate the robustness of our findings, we replicate our analysis using the unemployment duration measure, coding the duration as censored if there is a gap between the end of the registered unemployment spell and the start of the next job. All of our results hold with this alternative measure – which is agnostic about what individuals between the end of a registered unemployment spell and the start of a new job – indicating that the findings are robust to the way in which duration is measured.

The last row of the table summarizes the change in log (real) wage between the old and new jobs. The median wage growth rate is -0.7%, while the mean is -3.4%. There is substantial dispersion in the wage growth distribution (standard deviation = 51%).[11] This suggests that there is considerable scope for a given worker to earn higher or lower wages within the Austrian economy, a point relevant in evaluating the search outcome results in section VII.

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

[11] The wage at a given employer is defined as total earnings from that employer over the calendar year divided by days worked at that employer during the calendar year, multiplied by 365. The earnings growth measure thus adjusts for differences in days worked across jobs, but does not adjust for differences in hours worked per day. Therefore, part of the dispersion in earnings growth may be due to variation in hours worked per day.

between the duration of unemployment experienced by a job loser (y) and a dummy variable S which is equal to 1 if he or she receives severance pay and 0 otherwise:

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

The parameter of interest is the coefficient β_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:

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

where $\nu \equiv \varepsilon - E[\varepsilon|JT]$ is mean independent of S. Moreover, 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 third or fourth 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

Selection Around the Discontinuities. 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 3 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 4a plots average age in each tenure-month cell by job tenure, and shows that there is no evidence of selection on age. Figure 4b 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, note that the magnitude of the discontinuity is small: the jump in the best-fit lines shown in Figure 4b is approximately 300 Euros/year, or about 1.6% 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 insignificant

(2006) argue that in situations like the present case, where the running variable is discrete (measured in days), it 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 probability also evolves smoothly through the 36 month threshold.

effects or small but significant discontinuities – for other observables such as education, industry, occupation, previous firm size, duration of last job, last nonemployment duration, 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 effect of these covariates on unemployment durations. Intuitively, unless the correlation between wages and durations is very large, a small discontinuity in wages cannot lead to much bias in the estimated effect of severance pay on unemployment durations. To quantify the potential 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 "baseline" hazard), and X denotes the following set of observed characteristics: log of the previous wage and its square, age and its square, gender, education, marital status, Austrian nationality, "blue collar" status, previous firm size, total work experience and its square, indicator for prior job, last job duration, last nonemployment duration, total number of spells of nonemployment in career, last job "blue collar" status, and dummies for dummies for industry, region of residence, month of job loss, and year of job loss. We then predict the relative hazard for each observation i, $\widehat{r_i} = \exp(X\widehat{\phi})$, using the estimated $\widehat{\phi}$ vector. Finally, we compute the means of the predicted relative hazards by month of job tenure, $\mathrm{E}[\widehat{r_i}|JT]$ 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 4c shows the results of this exercise. The predicted relative hazards trend downward across the chart, indicating that individuals with higher job tenure have observable characteristics associated with longer durations. The predicted hazards are smooth through the 36 month threshold, showing that the small discontinuities in the individual covariates have little net impact on job findings hazards. Moreover, insofar as the endogenous variables such as prior work history and wages are likely to pick up any significant unobservable differences across individuals, this result indicates that the individuals to the left and right of the discontinuity are fairly similar in terms of unobservable determinants of unemployment durations as well. 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 facilitates 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. In the interest of space, we do not report these results here. We find that 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 4b, there is no significant jump in mean wages nor the other covariates around the 36 month threshold in months worked. Overall, we conclude that the patterns in the data are 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.

Identification with Double Discontinuity. As noted above, although severance pay and EB status depend on different running variables, there is a 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 the resulting "double discontinuity" problem. The first is to analyze a subsample in which the two discontinuities are not overlapping. To implement this approach, we consider the "restricted sample" of individuals who worked for one month or more within the past five years at a firm different from the one from which they were just laid off. Everyone is the restricted sample who has $JT \geq 35$ months has $MW \geq 36$ and thus qualifies for EB. Thus, only severance pay eligibility shifts at $JT = 36$ in the restricted sample (the fraction eligible for EB remains constant at 100% around $JT = 36$). Conversely, as months worked approaches 36 months, no one in the subsample has yet worked 36 months at the same employer. Thus, only EB status shifts at $MW = 36$ in the restricted sample. The separation of the two discontinuities permits the use of conventional single-variable RD methods to identify the severance and EB effects in the restricted sample.

An alternative approach to separating the two effects, which can be applied in the full sample, is to explicitly model the joint effects of severance pay and extended benefits. 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.[14] 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. Under this identification assumption, 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 as above 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. We begin our analysis in Figure 5a by plotting mean nonemployment durations by tenure-month category to give a preliminary sense of how the severance pay discontinuity affects search behavior. For simplicity, we ignore censoring (effectively treating all measured durations as

[14] Note that (12) does not include an interaction effect between S and EB. While in principle we would like to allow for an interaction between the two policies, in practice everyone with $S = 1$ has $EB = 1$. Hence the interaction cannot be identified.

complete), and exclude all observations with a nonemployment duration of more than two years to eliminate the long right tail of the distribution. Figure 5a shows that there is a clearly discernible jump in the average nonemployment duration of approximately 10 days around the severance pay discontinuity.

We cannot attribute the entire gap in Figure 5a to the effect of the severance payment because of the double discontinuity problem – the fraction of individuals receiving EB also jumps (by 20 percentage points) at the cutoff. 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 pure effect of severance pay using simple graphical methods, we focus on the "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 month at another firm within the past five years before joining the firm from which they were laid off. The restricted sample includes 83% of the observations in the full sample. 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. Figure 5b replicates Figure 5a for this subsample. This figure shows a jump in the mean nonemployment duration of approximately 8 days at the 36 month cutoff, confirming that most of the 10 day gap in Figure 5a is indeed caused by the severance payment.

A more precise approach to examining the effect of severance payments on search behavior is to examine how job finding hazard rates change around the discontinuity. Figure 6 presents such an analysis. In this figure, we include all nonemployment durations and adjust the hazards for censoring. We focus on the hazard rates over the first 20 weeks – the period of interest from the perspective of testing between models since it includes only the time until the benefit extension – by censoring all observations at 140 days. To examine how average hazard rates vary across tenure-month categories, we fit a Cox proportional-hazards model with dummies for the tenure-month groups. We estimate the model in the full sample while adjusting for the double discontinuity problem due to the EB effect by including cubic polynomials in months worked and a dummy for

EB eligibility:

$$h_d = \alpha_d \exp\{\theta_{13} I(j=13) + \theta_{14} I(j=14) + ... + \theta_{34} I(j=34) \tag{13}$$
$$+ \theta_{36} I(j=36) + ... + \theta_{58} I(j=58)$$
$$+ \gamma X + \beta EB + \beta_1 MW + \beta_2 MW^2 + \beta_3 MW^3$$
$$+ \beta_1^E EB \times MW + \beta_2^E EB \times MW^2 + \beta_3^E EB \times MW^3\}$$

In this specification, α_d denotes the baseline hazard on day d of the spell, EB denotes an indicator variable for extended-benefits eligibility, MW denotes the total number of months worked in the past five years, and X denotes a set of covariates. The key coefficients of interest are the θ_js, which can be interpreted as percentage difference between average hazard in group j and the average hazard in $j = 35$ (the omitted group). Note that this method of estimating the θ_js assumes a proportional shift in the hazard rates across the tenure-month categories. We have also implemented a less parametric approach of stratifying the baseline hazards by tenure-month category (permitting α_{jd} to vary freely across tenure-month categories j), and then examining how the average estimated α_{jd} varies with j. This approach yields very similar results to the ones reported below.

Figure 6a plots the estimated θ_js without any additional controls (no X). Consistent with the results in Figure 5, this figure shows that there is a discontinuous drop of approximately 10% in the hazard rate at the severance pay discontinuity. Since the estimated relative hazards in this figure are adjusted for the EB effect, the entire jump in this figure can be attributed to the effect of severance pay in the full sample.

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.[15] As in a classical experimental design, however, the addition of controls may 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. Figure 6b replicates Figure 6a adjusting for a set of observable covariates by including the following set of controls in the X vector: female, "blue collar" status, marital status, Austrian nationality, age and its square, log wage and its

[15] Note that in our presentation of the RD method we exclude controls. One can think of the ε term as including the effect of all the characteristics that vary across the sample, including potentially observable as well as unobserved characteristics.

square, dummies for month and year of job loss. As in Figure 6a, the job finding hazard shows about a 10% 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 4c that the relationship between the hazards and observable covariates is smooth, and validates the assumptions of the RD approach.

A potential concern in Figures 6a and 6b is that there is some seasonality in the hazard rates associated with job tenure. In particular, the hazard rates in the last few months of each tenure-year (e.g. months 21-23, 33-35, etc.) are approximately 2.5% higher than the hazards in the remainder of the tenure-year. One explanation for this pattern is that individuals who leave a firm shortly after completion of a full year of service are different from those who leave just before completion of a full year. Such differences may arise e.g. because of vesting rules for pension plans that take effect at integer thresholds or because planned terminations are more likely to take place after a full year of service is complete. Since the severance pay cutoff falls at an integer threshold (three years of tenure), one may be concerned that the seasonality in hazards biases the RD estimate of the severance pay effect. To gauge the size of the potential bias, we adjust for tenure seasonality by estimating a parametric RD model with an "end of tenure year" indicator which equals 1 in the three months before the end of each tenure year (21-23, 33-35, 45-47, and 57-59). We then adjust the average hazards in Figure 6a for seasonality by subtracting the estimated end of tenure year effect from the hazard rates at the end of each tenure year.[16] Figure 6c plots the resulting seasonality-adjusted hazards. This figure shows that the seasonality adjustment fully eliminates the potentially worrisome patterns in Figures 6a and 6b. With this adjustment, the average hazard rate falls by approximately 8% at the severance pay cutoff, indicating that the results are essentially robust to the seasonality concern.

As noted above, the causal interpretation of our results relies on the identification assumption that there would be no systematic differences in nonemployment durations between individuals laid off just after three years of tenure and those laid off just before three years absent the discontinuous severance pay treatment. The rich panel structure of our dataset allows a simple "placebo" test to further evaluate the validity of this assumption. In Figure 7, we examine how job tenure at the job *before* the one just lost – which is smoothly related to severance pay eligibility for the current layoff – is related to the current duration of nonemployment. We restrict attention to the individuals who

[16] More precisely, we estimate specification (3) in Table 3 with the end of tenure dummy and subtract the coefficient estimate from hazard rates at the end of each tenure year in Figure 6a.

have job tenure of between 1 and 5 years at a job prior to the one they just lost. This figure shows that mean nonemployment durations evolve smoothly through the 36 month cutoff, supporting our identification assumption. This result indicates that any alternative omitted-variable explanation of our findings would require that individual's unobserved characteristics change over time such that a discontinuity in nonemployment durations at 36 months emerges only in the current job.

Thus far we have summarized the effect of severance pay on search behavior in a single statistic, either mean durations or the average job finding hazard over the first twenty weeks of the spell. We now explore how severance pay affects search behavior as the spell elapses. Figure 8a plots average weekly job finding hazards for individuals laid off in tenure-months 33-35 (no severance) and those laid off in months 36-38 (who receive severance). The figure shows that the gap between the hazard rates in the two groups emerges after week 5 of the spell, and gradually narrows starting around week 25. 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.

We interpret Figures 5-8 as showing that the provision of cash-in-hand through a lump sum severance payment substantially lowers job finding rates. This evidence rejects the full insurance (PIH with complete markets) model based on prediction 1 in Table 1.

Extended Benefits. We now replicate the preceding analysis for the extended benefit policy. Figure 9a plots the relationship between average nonemployment durations and months worked (MW) in the past five years in the full sample. As in Figure 5a, we ignore censoring and exclude observations with a nonemployment duration of more than 2 years. Figure 9a shows that there is a clearly discernible jump in the average nonemployment duration of approximately 7 days around the EB discontinuity. Figure 9b replicates Figure 9a in the restricted sample, where the entire discontinuity at $MW = 36$ can be attributed to EB. This figure confirms that the discontinuity in EB eligibility is in fact responsible for the jump in nonemployment durations in Figure 9a, since the fraction receiving severance pay evolves smoothly through $MW = 36$ in this subsample.

In Figure 9c, we examine how the average hazard rates over the first twenty weeks of the spell vary around the EB discontinuity. We estimate a proportional hazard model analogous to that in (13), with group dummies for months worked instead of job tenure. We include a cubic polynomial in job tenure and a dummy for severance pay eligibility to identify the pure effect of the EB policy on hazard rates in the full sample. Consistent with the results in Figures 9a-b, this figure shows that there is a discontinuous drop of approximately 7% in the average hazard rate prior to week at the EB discontinuity.

In Figure 8b, we explore how extending UI benefits affects search behavior as the spell elapses, comparing the weekly job finding hazards for individuals in the three months to the left and right of the $MW = 36$ discontinuity. This figure shows that the benefit extension has a large effect on behavior after week 20, but also has a substantial effect on 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 finding rejects the complete myopia (rule of thumb) model based on prediction 2 in Table 1.

In the next section, we estimate the severance pay and EB 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 estimate a series of proportional hazards models for the risks of finding a new job or exiting unemployment. 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) that allow the derivative of the control function to change discontinuously at the eligibility cutoffs:

$$
\begin{aligned}
h_d = \alpha_d \exp\{\beta_s S \ &+ \beta_e EB + \gamma X + \\
&+ \mu_1 JT + \mu_2 JT + \mu_3 JT^3 \\
&+ \mu_1^S S \times JT + \mu_2^S S \times JT^2 + \mu_3^S S \times JT^3 \\
&+ \beta_1 MW + \beta_2 MW^2 + \beta_3 MW^3 \\
&+ \beta_1^E EB \times MW + \beta_2^E EB \times MW^2 + \beta_3^E EB \times MW^3\}
\end{aligned} \tag{14}
$$

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.

Tables 3a and 3b present the estimated coefficients β_s and β_e from a set of alternative samples and specifications. We begin in columns 1 and 2 by estimating the effects of the severance pay and EB policies separately in the restricted sample without any controls. The coefficient estimates indicate that both severance pay and EB reduce job finding hazards substantially, consistent with the visual evidence documented above. In column 3, we estimate the two effects jointly in the full sample using the "double RD" control function approach in (14). These estimates corroborate that the results in the full sample are similar to those in the restricted sample.

We evaluate the robustness of these results by including additional covariates in the double RD specification with the full sample in specifications 4 and 5. In specification 4, we control for the worker's gender, marital status, Austrian nationality, "blue collar" occupation indicator, age and its square, log previous wage and its square, as well as dummies for the month and year of the job termination. In specification 5, we add to this set of controls the "end of tenure year" dummy defined above to correct for tenure seasonality. The estimates indicate the results are robust to these changes in the covariate set.

Table 3b presents additional robustness checks and placebo tests. In specification 6, we expand the set of covariates in specification 4 of Table 3a to include the following additional covariates that capture the individual's work history: education, total number of employees at firm from which the work was laid off, total years of work experience and its square, "blue collar" status at job prior to the one lost, indicator for having a job before the one just lost, total number of nonemployment spells in the career, a dummy for being recalled to the job before the one just lost, the duration of the job before the one just lost, the last nonemployment duration before the current spell, and dummies for prior industry and region of residence. The inclusion of this rich set of endogenous covariates does not change the estimates, which perhaps helps mitigate concerns about bias due to selection.

In specification 7, we take an alternative approach to dealing with the tenure seasonality problem described above by excluding all observations in the last three months of each tenure year (tenure months 21-23, 33-35, etc.) and replicating specification 3 of Table 3a. In specification 8, we examine the robustness of our results to measuring durations by time registered as unemployed instead of time between jobs. We code observations where the end of registered unemployment does not coincide with the start of a new job as censored and replicate specification 3 of Table 3b with this alternative duration measure. The estimates indicate that the results are robust to these specification checks.

The last three columns of Table 3b report estimates of the "placebo test" corresponding to

Figure 7 by examining how tenure at the previous job and months worked prior to the relevant period for EB eligibility affects job finding hazards. To obtain a sample consistent with the one we use for our main analysis, we restrict attention to the individuals who (a) have job tenure of between 1 and 5 years at a job prior to the one they just lost and (b) worked for between 1 and 5 years of the five years preceding the last five years (i.e., years -10 to -5 before the current job loss date). We then estimate the effect of the severance pay and EB placebos – defined as having more than 36 months of job tenure at the job before the one just lost and having more than 36 months of work experience between years -10 and -5, respectively. Specification 9 reports these estimates for a model analogous to specification 3 without any controls, while specification 10 replicates 9 with the control set used in specification 4. Both sets of estimates confirm that there is no placebo effect, consistent with Figure 7. Finally, in specification 11, we also include the true severance pay and EB eligibility indicators, along with the placebo indicators and cubic polynomials for all four running variables. The true severance pay and EB variables still are estimated to reduce job finding hazards substantially (though the standard errors rise because the sample is one-fifth the original size). The hypothesis that the placebo and true severance pay effects is rejected with $p = 0.06$.

We have fit a wide variety of other specifications in an effort to probe the robustness of the results in Table 3. We find that estimates from alternative models – especially the ratio of the severance pay and EB effect – are quite stable. For example, replacing the third-order polynomials with fourth order models leads to estimated severance pay and EB effects that are a little bigger in magnitude than those reported in Table 3. Trimming outliers, dropping those recalled at the job prior to the one just lost, and estimating the effects of the two policies on average hazards over the first six months or year of the spell also yields similar results.

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 leads to any differences in the nature of the jobs obtained through the search process. Models with a non-degenerate distribution of potential job qualities 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 10a 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 indicates 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 10b 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 11 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 10 and 11 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 add 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, as noted in section IV, the variation in wage changes experienced by job losers is relatively wide ($\sigma(\Delta \log w) = 0.51$) and is 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 (exempt from most taxes) on the exit hazard in the first 140 days of job search. Since we use a proportional hazards specification, $\beta_s \approx \partial \log s_t^* / \partial A_t \times 2w(1-.06)/(1-\tau)$, where w is the net (after-tax) monthly wage, τ is the average tax rate on earned income (approximately 30%), and 6% is the approximate tax rate that applies on severance payments. [17] 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 $2.5w\rho(1 - UA/UI)$, where ρ is the replacement rate of regular UI benefits (relative to net salary w), and UA/UI represents the ratio of unemployment assistance (UA) benefits to UI benefits for a typical worker who would exhaust regular UI after 20 weeks in the absence of the benefit extension. The statutory replacement rate for UI benefits is 55%. However, most workers receive supplementary UI benefits for their children: on average we estimate that this raises the replacement rate to 64%. [18] Offsetting this is the fact that workers in Austria receive 14 "monthly" salaries per year – 12 monthly salaries plus 2 bonus months – whereas UI benefits are monthly. Thus the average effective replacement rate is $\rho = 0.64 \times 12/14 = 0.55$. Benefits for UA are based on a formula: $UA = 0.92UI - F + A$, where F represents other family member's earnings and A represents dependent allowances (currently 423 Euros per month for a partner and 213 Euros per month for each dependent child). 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. This suggests that in our sample F is approximately equal to w. Assuming a typical job loser has a partner and 2 children, and that the ratio of family allowances to average net monthly income is constant, we estimate that $UA/UI = 0.38$. Combining these pieces, the coefficient β_e from our RD models provides an estimate of $\partial \log s_t^* / \partial b_{t+1} \times 2.5w \times 0.55 \times (1 - 0.38)$.[19] The predicted value for the

[17] 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.

[18] Unfortunately, our data source does not contain information on the dependent allowances available to an unemployed worker. We used a variety of sources to compare actual average UI benefits to average net salaries.

[19] $1.2w$ = 2.5 months $\times$ 55% replacement rate $\times$ 87% change of not getting UA benefits.

ratio of the coefficients $\beta_s$and β_e is therefore:

$$\begin{aligned}\frac{\beta_s}{\beta_e} &= \frac{\partial s_t^*/\partial A_t}{\partial s_t^*/\partial b_{t+1}} \times \frac{2w(1-.06)/(1-0.3)}{2.5w \times 0.55 \times (1-0.38)} = 3.15 \times \frac{\partial s_t^*/\partial A_t}{\partial s_t^*/\partial b_{t+1}} \\ &= 3.15 \times D \times \frac{1+r}{1-s_{t+1}^*}.\end{aligned}$$

Given a value of D,we need to multiply by $3.15(1+r)$ 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 not finding a job within 20 weeks, which is approximately 35 percent. Treating r as small (as it only applies over a 20 week period) gives:

$$\frac{\beta_s}{\beta_e} = 9D.$$

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 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 credit constrained benchmark.[20]

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,

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

[20] 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.

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. We assume that the coefficient of relative risk aversion is 2 based on the bound given in Chetty (2006c). Assuming that $w \approx F$, $\sigma = 0.50$. Plugging in these values and setting $\rho_t = \rho_{t+1} = 0.55$, a fully credit-constrained job seeker will have a value of $D = 0.47$, implying a predicted value for β_s/β_e of about 3.

The calculation of D in the PIH case is more complicated. Since D should intuitively be small if people can borrow and lend freely, we proceed by deriving an upper bound on D. We 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 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 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 $t+1$.[21] Assuming that a job seeker can always find a job within T months, this implies:

$$\begin{aligned} u'(c_t^u) &= E_t[s_{t+1}^* u'(c_{t+1}^e) + (1 - s_{t+1}^*)s_{t+2}^* u'(c_{t+2}^e) + (1 - s_{t+1}^*)(1 - s_{t+2}^*)s_{t+3}^* u'(c_{t+3}^e) + ...] \quad (15) \\ &= \sum\nolimits_{j=1}^{T} p_{t+j}^* u'(c_{t+j}^e) \end{aligned}$$

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

$$c_{t+1}^e = w + F + r(A_t + F + b_t - c_t^u) \geq w + F + r(A_t + F + b_t - c_t^e),$$

since $c_t^u \leq c_t^e$. 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 (15). 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

[21] 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 $U'_{t+1}(A_{t+1}) = u'(c_{t+1}^u)$.

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, most of the people in our sample of job losers who are observed returning to work within 5 years have returned by 18 months. The re-employment hazard rate after 18 months is extremely low (less than 1% per month), suggesting that many of those who are not back to work within 18 months have taken either a temporary break (e.g., a maternity leave) or have found a job outside the country or in the public sector or in self-employment. 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}^*.[22] We calculate the distribution of times to re-employment ignoring those who are censored after 18 months.[23]

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 $\rho_t = 0.55 \times UA/UI$ thereafter, using the same calculations as above to set $UA/UI = 0.38$. 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, and that individuals face an annual interest rate of 5%, we obtain the prediction $D^* = 0.010$. Raising the interest rate to 10% leads to a predicted value $D^* = 0.028$.[24] Taking 5% as a reference, the PIH benchmark suggests that the ratio of the severance pay effect to the EB effect should be roughly 0.1.

Comparing the Empirical Estimate 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 1 of Table 3b, which includes a broad set of controls. This model yields an estimate of $\beta_s = -0.094$ (standard error=0.019) and an estimate of $\beta_e = -0.064$ (standard error=0.018). Thus our estimated ratio is $\beta_s/\beta_e = 1.47$, with a standard error of 0.30.[25] This estimate is roughly halfway between the prediction from the simple PIH model and the prediction from the credit-constrained model. Moreover, the estimate is precise enough to rule out a value below 0.8 or above 2.1 at conventional significance levels.

[22] 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.

[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.

[24] Reducing the coefficient of relative risk aversion to 1.5, we get the prediction $D^* = 0.008$ if r=5% and $D^* = 0.017$ if r=10%.

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

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 magnitude of the estimates of β_s and β_e in Table 3 as providing fairly strong evidence against the PIH benchmark.

IX Conclusion

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 little or 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 based on the ratio of the severance pay and EB effects 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, 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 2006b). Analyzing these issues formally in dynamic models calibrated to match the evidence documented here is an interesting direction for further research.

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TABLE 1

Testable Predictions

Prediction	A. PIH with complete mkts.	B1. Buffer stock w/match quality	B2. Buffer stock w/search intensity	C. Rule of thumb: complete myopia
	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	31.20	30.00	7.96
Female	0.52	1.00	0.50
Post-compulsory Schooling	0.60	1.00	0.49
Married	0.43	0.00	0.50
Austrian Citizen	0.88	1.00	0.33
Blue Collar Occupation	0.58	1.00	0.49
Previous Job/Employment:			
Months of Tenure	25.61	21.90	11.93
Months Worked in Past 5 Years	41.11	44.10	13.79
Eligible for Severance Pay	0.21	0.00	0.41
Eligible for Extended UI	0.66	1.00	0.47
Previous Wage (Euros/yr)	17033.69	15949.75	7587.71
Wage Top-Coded	0.02	0.00	0.14
Post-Layoff:			
Duration of Unemployment	4.75	2.94	8.37
Unemployed < 20 Weeks	0.64	1.00	0.48
Unemployed < 52 Weeks	0.94	1.00	0.24
Duration of Nonemployment	16.93	4.29	38.19
Nonemployed < 20 Weeks	0.51	1.00	0.50
Nonemployed < 52 Weeks	0.77	1.00	0.42
Observed in New Job	0.92	1.00	0.27
Among those with New Job:			
Months to Re-employment	8.96	3.84	17.71
Change in Log Wage	-0.03	-0.01	0.51

Note: Based on sample of 650,922 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 individuals losing a job in construction are excluded. Wages are expressed in real (year 2000) Euros.

TABLE 3a

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

	(1) Restricted sample	(2) Restricted sample	(3) Full sample	(4) With controls	(5) Tenure Seasonality
Dependent Var:	Job Finding Hazard				
Severance pay	-0.127		-0.125	-0.115	-0.076
	(0.019)		(0.017)	(0.018)	(0.019)
Extended benefits		-0.084	-0.093	-0.064	-0.065
		(0.018)	(0.016)	(0.017)	(0.017)
Sample size	512,767	512,767	650,922	565,835	565,835

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 3b

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

	(6) Saturated controls	(7) Excluding end of year	(8) Unemp. censored	(9) Placebo no ctrls	(10) Placebo controls	(11) Placebo comparison
Severance pay	-0.094	-0.095	-0.099			-0.112
	(0.019)	(0.033)	(0.022)			(0.038)
Extended benefits	-0.064	-0.059	-0.078			-0.073
	(0.018)	(0.019)	(0.021)			(0.058)
Sev. pay placebo				-0.017	-0.016	-0.016
				(0.037)	(0.039)	(0.037)
EB placebo				-0.003	0.009	-0.007
				(0.024)	(0.026)	(0.024)
Sample size	509,355	457,783	650,922	122,848	108,383	122,848

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

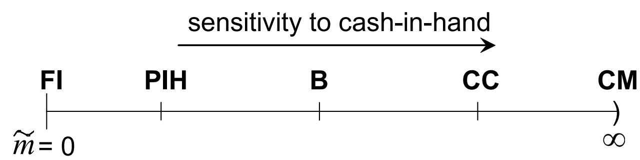
TABLE 4

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	-0.011	-0.004	-0.023	-0.004
	(0.006)	(0.006)	(0.015)	(0.017)
Extended benefits	-0.005	-0.008	-0.011	0.004
	(0.005)	(0.005)	(0.014)	(0.015)

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 1
Dynamic Models Ordered by Sensitivity to Cash-In-Hand



FI. Full insurance: PIH with complete markets

PIH. Standard PIH with unrestricted borrowing and lending

B. Buffer stock models (Deaton 1991; Carroll 1992)

CC. Credit constrained: binding asset limit but forward looking

CM. Complete myopia “rule of thumb” with consumption = income

Figure 2a

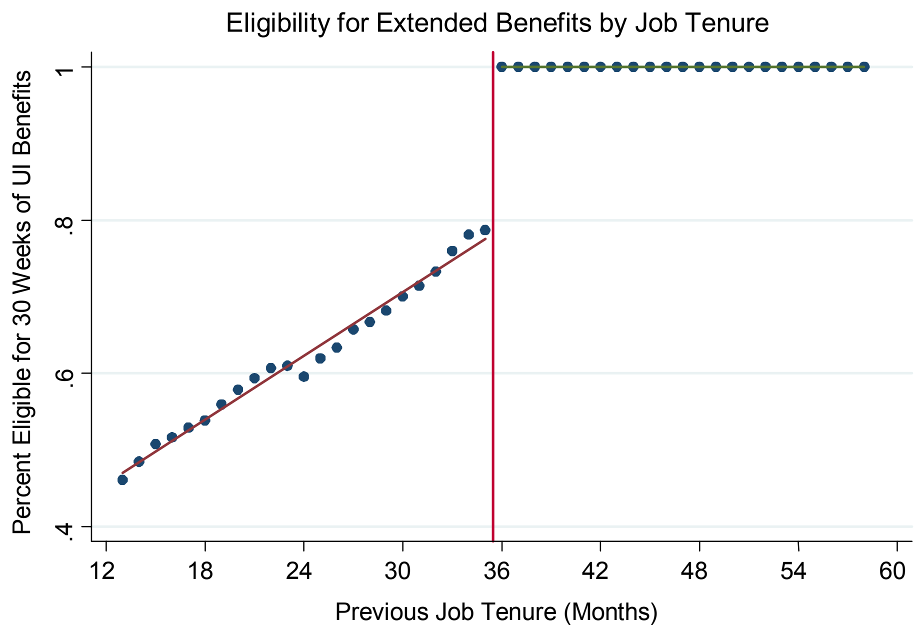


Figure 2b

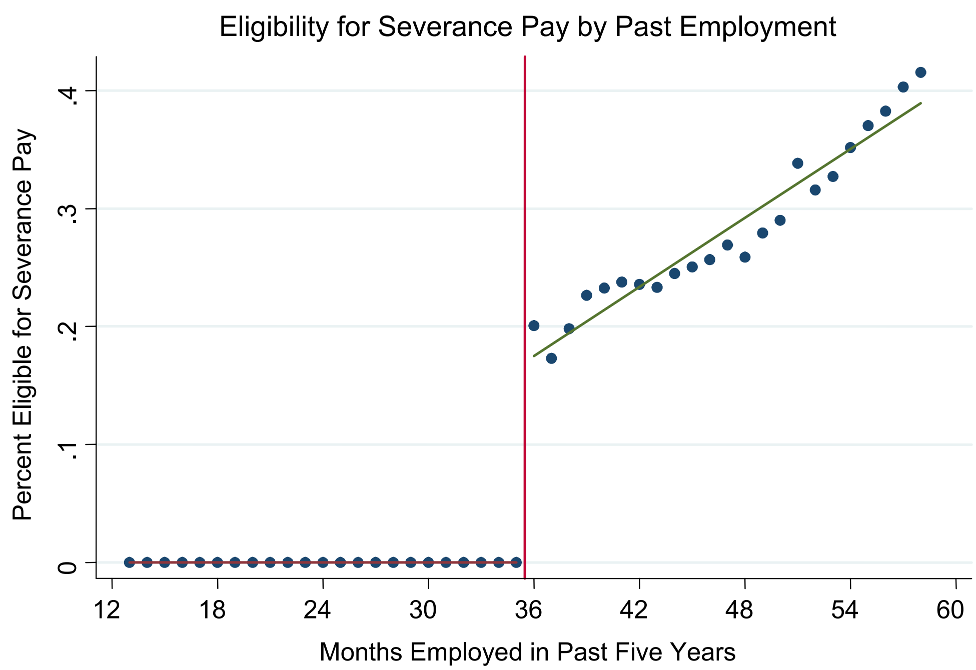
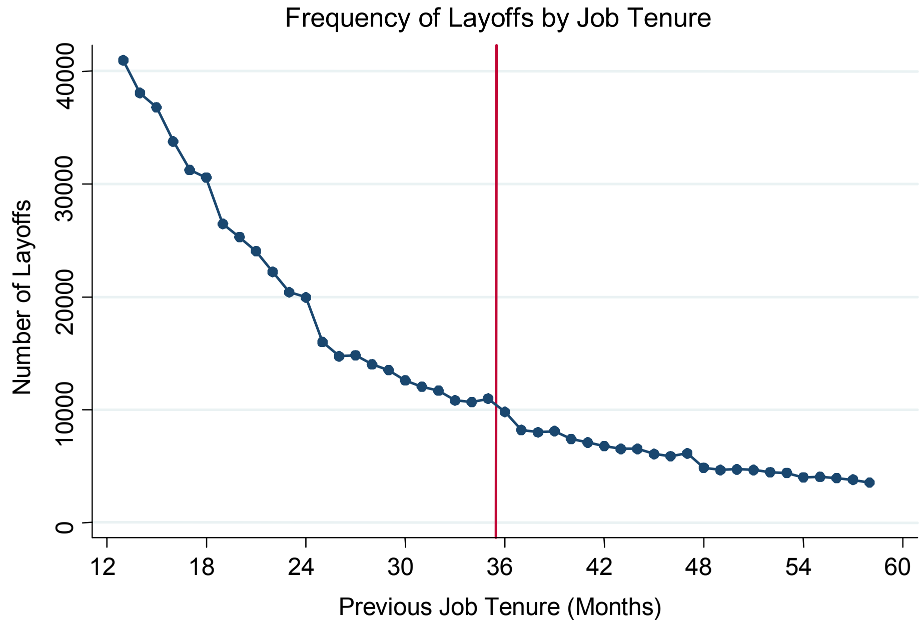


Figure 3



Frequency of Layoffs by Job Tenure
Number of Layoffs
0
10000
20000
30000
40000
12
18
24
30
36
42
48
54
60
Previous Job Tenure (Months)

Figure 4a

Age by Job Tenure

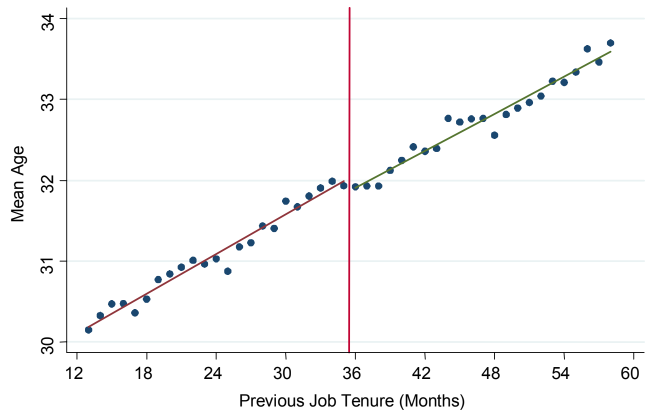


Figure 4b

Wage by Job Tenure

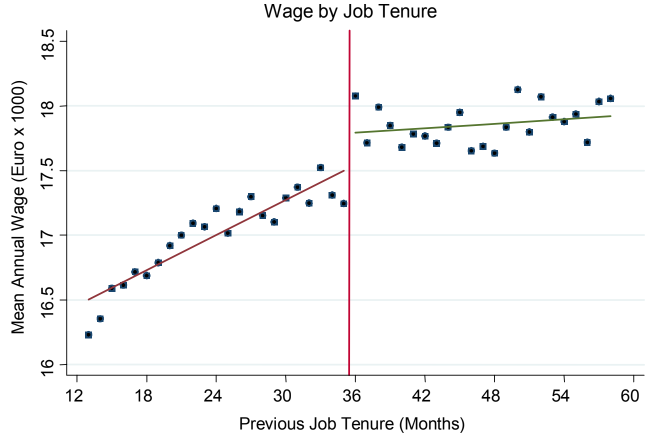


Figure 4c

Selection on Observables

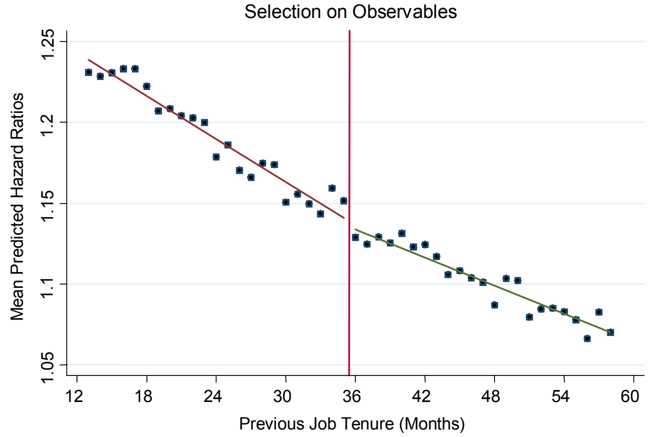


Figure 5a

Effect of Severance Pay on Nonemployment Durations

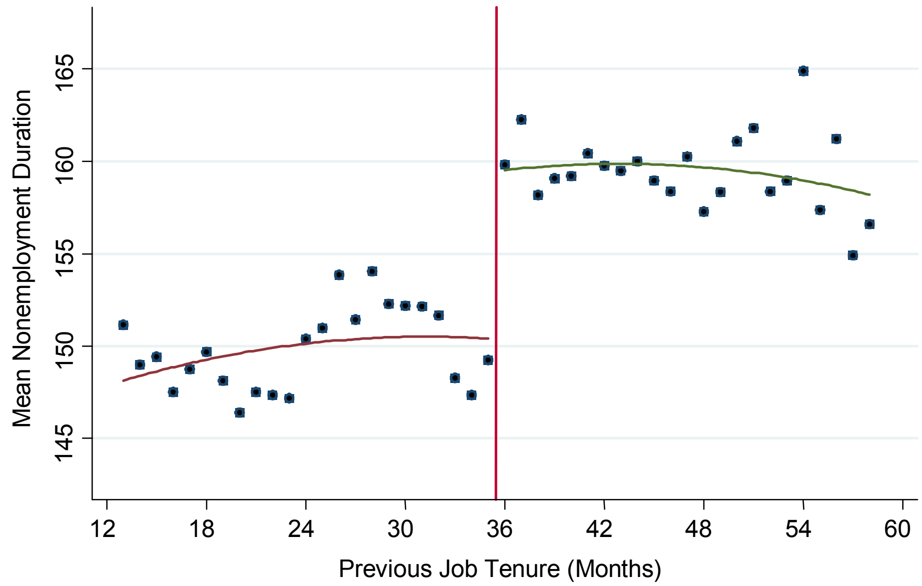


Figure 5b

Effect of Severance Pay in Restricted Sample

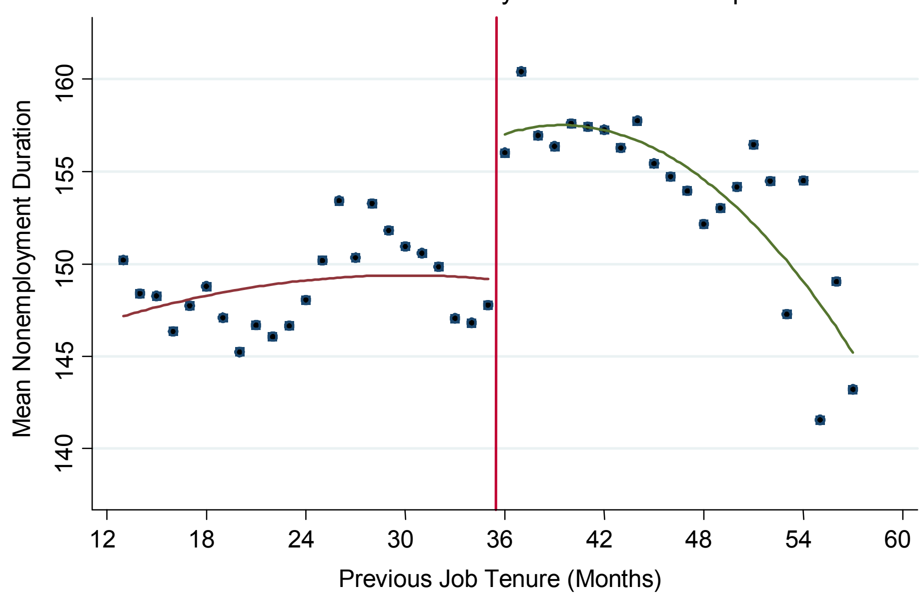


Figure 6a

Effect of Severance Pay on Job Finding Hazards

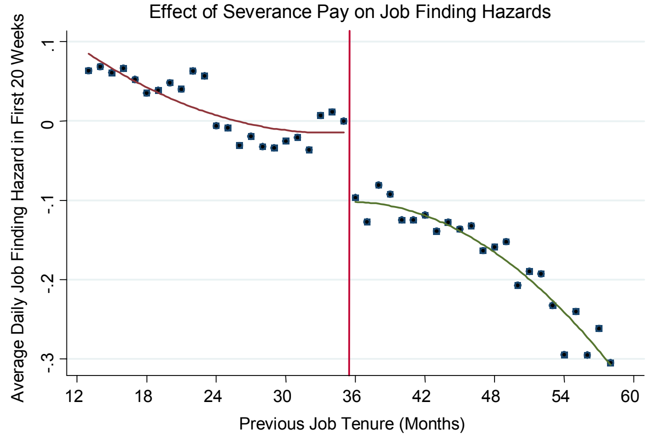


Figure 6b

Job Finding Hazards Adjusted for Covariates

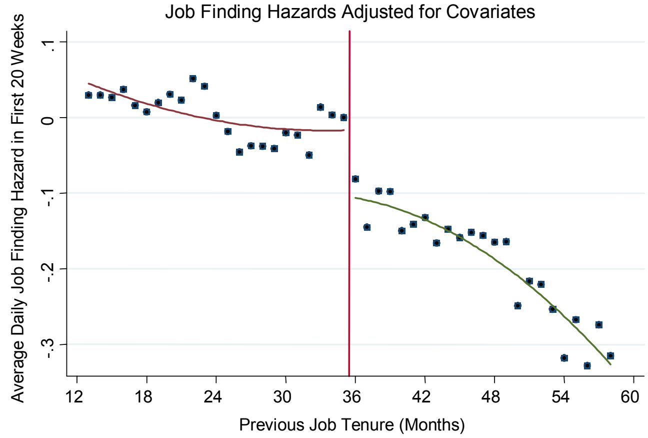


Figure 6c

Effect of Severance Pay Adjusted for Tenure Seasonality

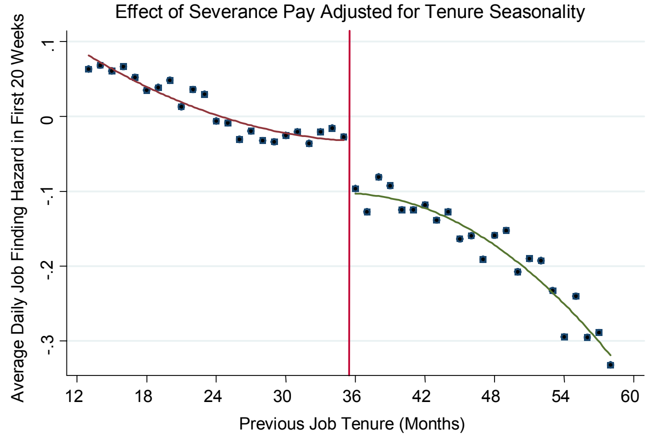


Figure 7

Placebo Test: Job Tenure and Subsequent Nonemployment Duration

Figure 8a

Effect of Severance Pay on Weekly Job Finding Hazards

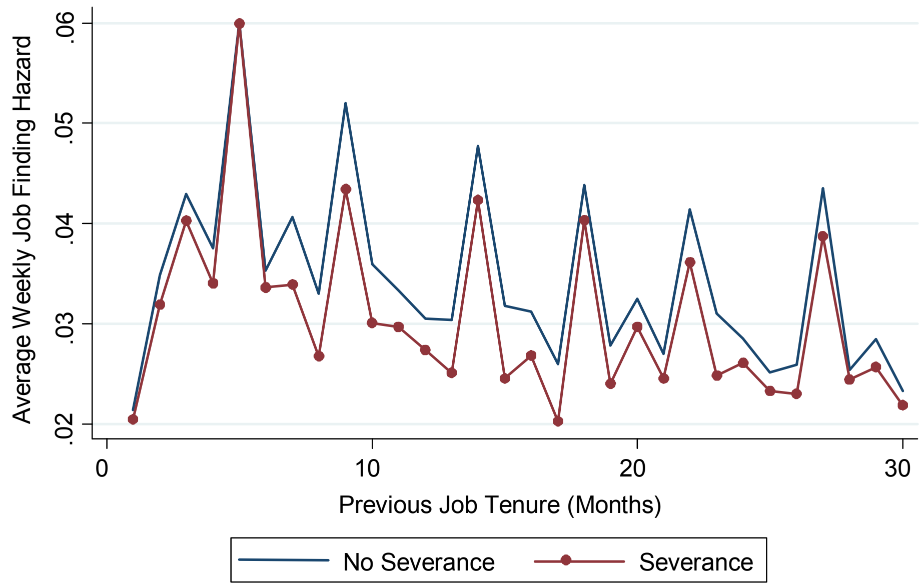


Figure 8b

Effect of Extended Benefits on Weekly Job Finding Hazards

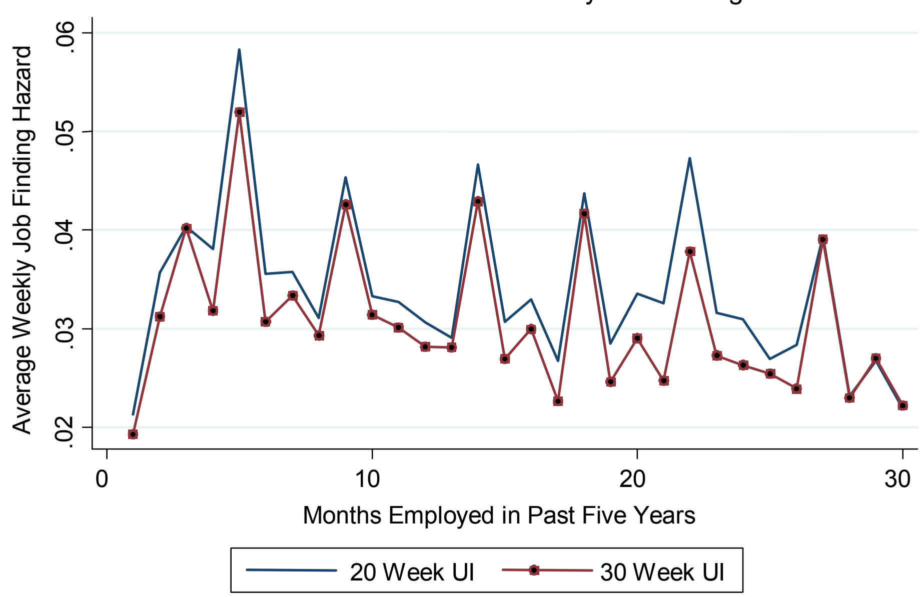


Figure 9a

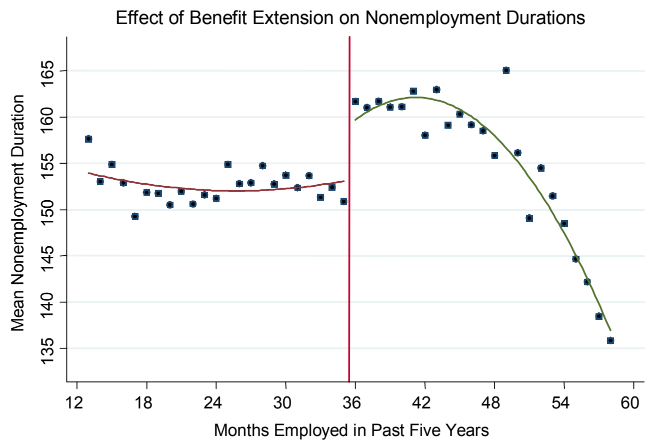


Figure 9b

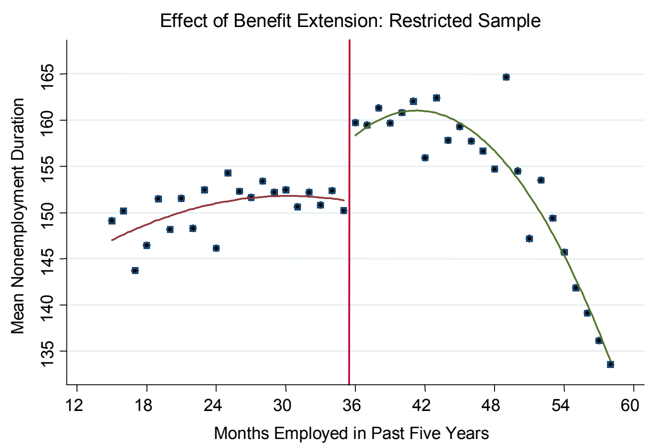


Figure 9c

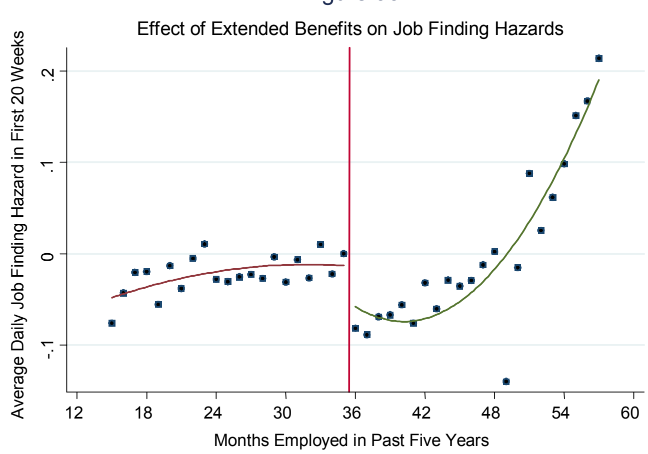


Figure 10a

Effect of Severance Pay on Wage in Next Job

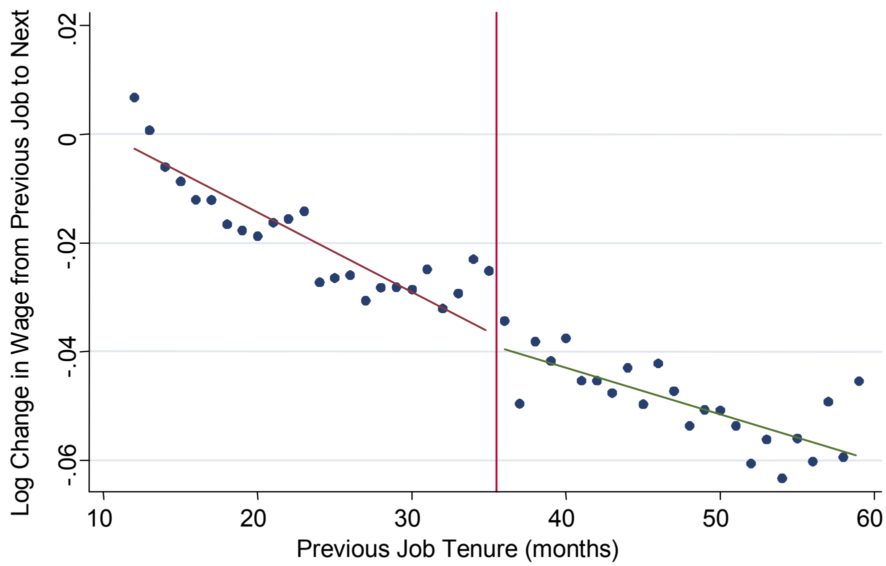


Figure 10b

Effect of Severance Pay on Duration of Next Job

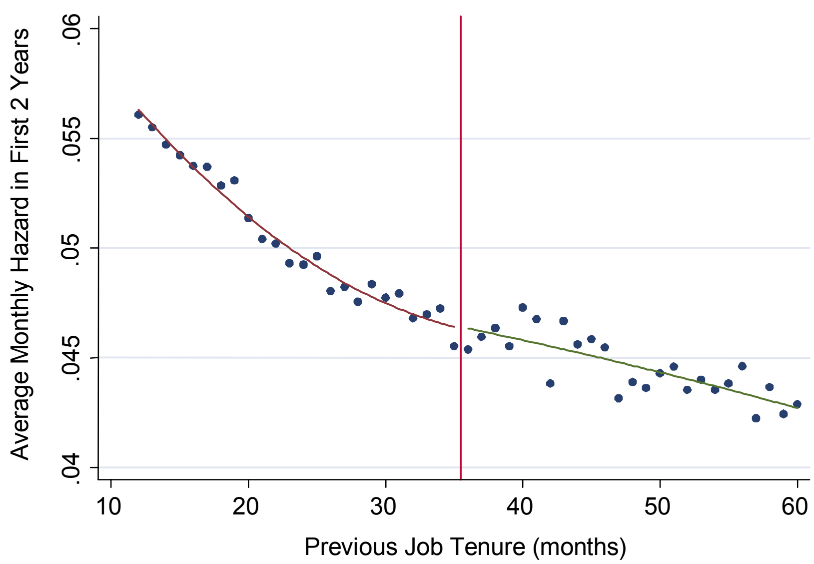


Figure 11a

Effect of Extended Benefits on Wage in Next Job

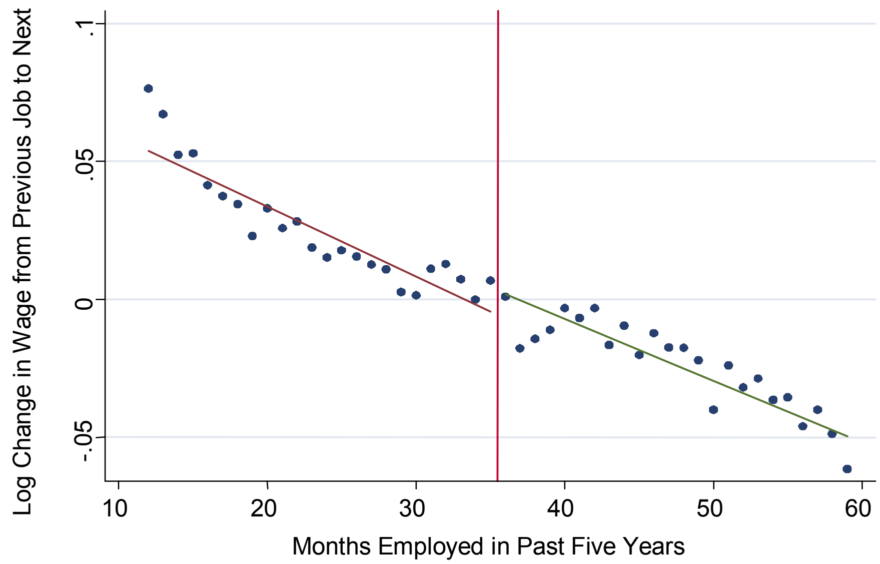


Figure 11b

Effect of Extended Benefits on Duration of Next Job