Cash-on-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market

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David Card
UC-Berkeley
and NBER

Raj Chetty
UC-Berkeley
and NBER

Andrea Weber
IHS-Vienna
and UC-Berkeley

ABSTRACT

This paper presents new tests of the permanent income hypothesis and other widely used models of household behavior using data from the labor market. We estimate the "excess sensitivity" of job search behavior to cash-on-hand using sharp discontinuities in eligibility for severance pay and extended unemployment insurance (UI) benefits in Austria. Analyzing data for over one-half million job losers, we obtain three empirical results: (1) a lump-sum severance payment equal to two months of earnings reduces the job-finding rate by 8-12% on average; (2) an extension of the potential duration of UI benefits from 20 weeks to 30 weeks similarly lowers job-finding rates in the first 20 weeks of search by 5-9%; and (3) increases in the duration of search induced by the two programs have little or no effect on subsequent job match quality. Using a search theoretic model, we show that estimates of the relative effect of severance pay and extended benefits can be used to calibrate and test a wide set of intertemporal models. Our estimates of this ratio are inconsistent with the predictions of a simple permanent income model, as well as naive "rule of thumb" behavior. The representative job searcher in our data is 70% of the way between the permanent income benchmark and credit-constrained behavior in terms of sensitivity to cash-on-hand.

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I Introduction

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

The effects of cash-on-hand have been studied since the 1950s in the macroeconomics literature, where researchers have estimated the effects of windfall cash grants on consumption.[1] However, there is still no firm consensus on the extent to which individuals can smooth consumption, due in part to limitations of the available data. As a result, the issue of which model best describes household behavior remains controversial.

In this paper, we provide new evidence on the validity of alternative dynamic models by estimating the effects of cash-on-hand on labor market behavior. In particular, we study whether lump-sum severance payments made to job losers in Austria affect unemployment durations and subsequent job outcomes. Conceptually, our analysis is similar to existing studies of sensitivity to cash-on-hand. We simply use a different measure of "consumption" – search intensity instead of purchased goods. Excess sensitivity of search intensity to cash-on-hand distinguishes between the permanent income hypothesis (PIH) and other dynamic models in the same way as excess sensitivity of consumption. Indeed, using a simple job search model, we show that the effects of cash-on-hand on consumption can be inferred from its effects on search behavior.

Our labor market approach complements existing consumption-based studies in three ways. First, the institutional features of the labor market we study allow a sharper research design. Eligibility for severance pay in Austria is based on a simple discontinuous rule that applies to nearly all workers: people with 3 or more years of job tenure are eligible, whereas those with

[1] Examples include Bodkin (1959), Hall and Mishkin (1982), Gruber (1997), Browning and Collado (2001), Hsieh (2003), and Johnson, Parker, and Souleles (2006). See Deaton (1992) for a summary and thoughtful interpretation of much of the literature up the early 1990s, and Browning and Crossley (2001) for a more recent survey. A detailed discussion of this and other related literatures is available in the NBER working paper version of this paper (Card, Chetty, and Weber 2006).

shorter tenures are not. In addition, administrative wage and employment data are available for the universe of private sector workers, providing a sample of 650,000 job losers. The sharp discontinuity and large sample size allow us to obtain more precise estimates of the effects of cash-on-hand than consumption-based studies, which are often constrained by small samples and difficulties in measurement of nondurable consumption.[2] Second, the severance payment is generous – equivalent to two months of pre-tax salary, or 2,300 Euros at the sample mean. This overcomes 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.[3] Third, the panel structure of our data set allows us to measure the long-term effects of cash grants on subsequent job quality. The size of match quality effects is an important unresolved issue of independent interest in the job search literature.[4]

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 who were laid off just before and just after the 36 month cutoff for eligibility. The key threat to a causal interpretation of our estimates is that firms may alter their firing decisions to avoid paying severance, leading to non-random selection around the discontinuity and invaliding the "experiment." We evaluate this possibility in several ways: testing whether the frequency of layoffs and observable characteristics of job losers evolve smoothly around the discontinuity, examining subsamples in which selective firing is less plausible (such as group layoffs), and conducting "placebo tests" of the effect of tenure in earlier jobs. None of these tests points to evidence of selective firing that would invalidate the RD design, a result that is consistent with relatively restrictive firing regulations in Austria and laws against the strategic timing of layoffs.

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 joblessness. The hazard rate of finding a new job during the first 20 weeks of the unemployment spell (the period of eligibility for regular unemployment 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. This sensitivity to cash-on-hand is inconsistent with a model where agents are able to smooth consumption perfectly. Second,

[2] For example, the 95 percent confidence intervals for the estimates reported by Johnson, Parker,and Souleles (2006) cover a range from 5 to 65 cents per dollar. Earlier studies have similar levels of precision.

[3] While this amount is non-negligible in terms of welfare costs, it is nevertheless "small" relative to lifetime wealth. As we show in section VII, a simple PIH model predicts a very small change in search behavior from such a grant.

[4] See Cox and Oaxaca (1990) for a review of this literature, and Addison and Blackburn (2000) and Centeno (2004) for more recent analysis.

using a parallel analysis of a discontinuity in the unemployment insurance (UI) benefit system, we find that job seekers who are eligible for 30 weeks of benefits exhibit 5-9% lower rates of job finding during the first 20 weeks of search than those who are eligible for only 20 weeks of benefits. This result shows that individuals anticipate the longer duration of benefits and reduce their search effort *before* the benefit extension takes effect. Such forward-looking behavior is inconsistent with a naive "rule of thumb" model where agents are completely myopic.

Third, we find that neither lump sum severance payments nor extended benefits affect the "match quality" of subsequent jobs, as measured e.g. by mean wages or the duration of subsequent jobs. An advantage of our approach relative to earlier studies is that we have enough precision to rule out fairly small job 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. This result suggests that search intensity may be a more important determinant of unemployment duration than reservation wages.

We interpret our reduced-form findings through a job search model that nests several commonly used models of household behavior. In particular, we construct a sample moment based on the relative effects of severance pay and benefit extensions that can be used to calibrate and test between these models. We then simulate the values of this moment from a simple version of the PIH model with unrestricted borrowing and a fully credit-constrained model. Comparing the predicted moments with our empirical estimates, we find that the PIH model is rejected by the data with $p < 0.01$, even with high discount rates or risk aversion. Our estimates suggest that deviations from the PIH benchmark are substantial: typical job searchers behave as if they are located 70% of the way between the PIH with unrestricted borrowing and the fully credit-constrained case (see Figure 1). We conclude that models with forward-looking behavior but limited consumption smoothing – such as Deaton's (1991) buffer-stock model – are most likely to fit the data.[5]

An important caveat to this characterization is that our analysis is restricted to job losers, whose behavior may not be representative of the average individual in Austria. For instance, job losers tend to have lower wages than a typical worker. We find that reweighting the data so that the sample is representative of the overall population in terms of such observables does not affect the estimated sample moment significantly. While this suggests that a more representative

[5] The extent of consumption-smoothing by individuals will generally depend on a variety of institutional factors and market conditions. Austria's unemployment insurance system and labor market characteristics (turnover rates and unemployment rates) are broadly similar to that in the US. This suggests that similar results may apply to households in the US, but more work is clearly needed to draw this conclusion.

sample would exhibit similar intertemporal behavior, our conclusions are nevertheless limited to the set of individuals who are selected into unemployment. If individuals with lower intertemporal smoothing capacity are more likely to be unemployed, the re-weighted estimates will remain biased against the PIH. In future work, it would be interesting to assess the generality of our conclusions about intertemporal behavior by examining the "excess sensitivity" of labor supply in other groups of the population, e.g. by studying choices such as retirement behavior.

In addition to distinguishing between models, our findings shed light on normative issues in public finance, in particular the efficiency costs of social insurance programs. Several well-known studies have shown that the duration of unemployment increases when the duration or generosity of UI benefits is increased (e.g., Meyer 1990, Lalive et. al. 2006). Most analysts have assumed that these responses are due to moral hazard (a distortionary substitution effect) rather than *wealth* effects. Chetty (2006b) points out that the wealth effects of UI benefits may be non-negligible when agents have limited liquidity. Consistent with Chetty's empirical findings in U.S. data, our evidence indicates that a substantial share of the behavioral response to longer UI benefits is attributable to a wealth (or liquidity) effect. This implies that the efficiency cost of temporary income support programs such as UI may be significantly lower than previously thought.

The paper proceeds as follows. Section II presents a search model and derives the moment for calibration. Section III describes the institutional background and data. Section IV outlines our estimation strategy and identification assumptions. Section V presents the empirical results on unemployment durations, and Section VI presents results on search outcomes. Section VII uses the empirical estimates to test between models. Section VIII concludes.

II A Job Search Model

We begin by presenting a simple job search model to organize our empirical analysis and provide a structural interpretation of the estimates. The model is closely based on Lentz and Tranaes (2005), who incorporate savings decisions in a job search model with variable search intensity. We make a few assumptions to simplify the analysis. First, we assume that all jobs last indefinitely once found (i.e. there is no subsequent job destruction). Second, anticipating our empirical findings, we assume that wages are exogenously fixed, eliminating reservation-wage choices. Third, we assume that utility is separable in consumption and search effort. We discuss how these assumptions affect our results in the context of calibrating and testing between models in section VII.

Model Setup. Consider a discrete-time setting where individuals have a finite planning horizon and a subjective time discount rate of δ. Let r denote the fixed interest rate in the economy. Flow utility in period t is given by $u(c_t) - \psi(s_t)$, where c_t represents consumption in the period, s_t denotes search effort, and the functions u and ψ are strictly concave and convex, respectively. Normalize s_t to equal the probability of finding a job in the current period.

Assume that the agent becomes unemployed at $t = 0$. An agent who enters a period t without a job first chooses search intensity s_t, and immediately learns if he or she has obtained a job. If search is successful, the agent begins working in that period at a fixed real wage w that persists indefinitely. Let c_t^e denote the agent's optimal consumption choice in period t if a job is found 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 and the problem repeats.

Optimal Search Intensity. 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

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

where L is a lower bound on assets that may or may not be binding. The value function for an individual who fails to find a job at the beginning of period t and remains unemployed is:

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

where

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

is the expected value of entering period t without a job with assets A_t. 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 (2005) 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.

An unemployed agent chooses s_t to maximize expected utility at the beginning of period t, given by (3). The resulting first order condition for optimal search intensity is

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

Intuitively, s_t is chosen 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 follow from the comparative statics of equation (4).

Prediction 1: Severance Pay. First consider the effect of an exogenous cash grant, such as a severance payment, on search effort:

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

Equation (5) shows that the effect of a cash grant on search intensity is determined by the difference in marginal utilities between employed and unemployed states, which varies with the magnitude of the consumption differential $(c_t^e - c_t^u)$. In a model with perfect consumption smoothing $(c_t^u = c_t^e)$, $\partial s_t^*/\partial A_t = 0$, because a cash grant raises $V_t(A_t)$ and $U_t(A_t)$ by the same amount. Thus, testing whether lump-sum severance pay has an effect on unemployment durations constitutes a test of whether agents can smooth consumption perfectly. More generally, if c_t^u is close to c_t^e, as in a permanent income model with unrestricted borrowing, the asset effect is small. In contrast, if individuals face asset constraints or voluntarily reduce c_t^u to maintain a buffer stock of savings, $\partial s_t^*/\partial A_t$ will be larger. Thus, there is a direct connection between the responsiveness of search intensity to a cash grant and the amount of consumption smoothing implied by an intertemporal model.

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

$$\begin{aligned} \partial s_t^*/\partial w &= u'(c_t^e)/\psi''(s_t^*) > 0 \\ \partial s_t^*/\partial b_t &= -u'(c_t^u)/\psi''(s_t^*) \\ \Rightarrow \partial s_t^*/\partial b_t &= \partial s_t^*/\partial A_t - \partial s_t^*/\partial w. \end{aligned} \tag{6}$$

Equation (6) shows that the response of search intensity to an increase in unemployment benefits can be written as the sum of a pure 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. Hence, by combining estimates of $\partial s_t^*/\partial b_t$ and $\partial s_t^*/\partial A_t$, one can infer the extent to which unemployment insurance generates a deadweight cost by distorting marginal incentives.

Prediction 2: Extended Benefits. Next, we examine how search intensity in period t is affected by the level of future benefits, b_{t+j}. Using equations (2) and (3) we obtain:

$$\partial s_t^*/\partial b_{t+j} = -p_{j,t}^* E_t[u'(c_{t+j}^u)]/[(1+\delta)^j \psi''(s_t^*)] \leq 0 \tag{7}$$

where $p_{j,t}^* = (1-s_{t+1}^*)(1-s_{t+2}^*)...(1-s_{t+j}^*)$ is the probability that an individual is still unemployed in period $t+j$ (conditional on being unemployed at t). This equation implies that a rise in the future benefit rate lowers search intensity in the current period, with a magnitude that varies inversely with the discount factor $(1+\delta)^j$. For a completely myopic agent, $\delta = \infty$, and hence equation (7) implies that $\partial s_t^*/\partial b_{t+j} = 0$. Thus, testing whether future benefit levels affect current search behavior constitutes a test of the "rule of thumb" (complete myopia) model.

Prediction 3: Future Job Quality. A final set of predictions that are useful in distinguishing between alternative models concern the effects of assets and unemployment benefits on the expected quality of the next job. The model presented here makes no predictions about job match quality because we have assumed that wages are fixed and agents only control search intensity. In a more general model with a non-degenerate distribution of wages or job qualities, an increase in assets or future benefits can potentially lead to a rise in the reservation wage and an increase in the average quality of the next job (Danforth 1979, Classen 1979). In addition to distinguishing between alternative search models, testing this prediction sheds light on whether improvements in future job outcomes provide a rationale for temporary income support programs.

A Moment for Calibration. We now combine equations (5) and (7) to form a predicted moment that can be used to calibrate and test a broad set of intertemporal models. In particular, consider the ratio of the effects of assets and future unemployment benefits on search intensity at the beginning of a spell (period 0). To simplify notation, let $p_j^* = p_{j,0}^*$ denote the probability that an individual is still unemployed j periods after job loss. Since the present expected value of UI benefits j periods in the future is proportional to the probability that an individual actually receives those benefits (p_j^*), it is convenient to re-scale the effect of an increase in future benefits by this probability – that is, consider $\frac{1}{p_j^*}\partial s_0^*/\partial b_j$ instead of $\partial s_0^*/\partial b_j$. Define the moment

$$m_j \equiv \frac{\partial s_0^*/\partial A_0}{\frac{1}{p_j^*}\partial s_0^*/\partial b_j} = D \times Z_j \times (1+\delta)^j \tag{8}$$

where

$$\begin{aligned} D &= \frac{u'(c_0^u) - u'(c_0^e)}{u'(c_0^u)} \\ Z_j &= \frac{u'(c_0^u)}{E_t[u'(c_j^u)]}. \end{aligned}$$

The moment m_j can be simulated in a model of household behavior because it requires knowledge only of the utility function (u and δ), the initial consumption drop ($\frac{c_0^u - c_0^e}{c_0^u}$) caused by unemployment, and the rate of decline in consumption over the spell ($\frac{c_j^u}{c_0^u}$). Importantly, the value of m_j does not depend on ψ, which cancels out in the division. If the path of consumption is flat during unemployment – as is approximately true for the PIH – then $Z_j = 1$, and only the initial consumption drop has to be calculated. The value of m_j is also of direct interest from a normative perspective because the ratio D is a sufficient statistic for determining the marginal benefits of unemployment insurance in a wide class of dynamic models (Chetty 2006a, Shimer and Werning 2007).

Figure 1 shows predicted values of m_2 for a set of commonly used models. The models on the left side of the continuum assume a higher degree of intertemporal smoothing by households, and therefore predict a lower sensitivity of search behavior to cash-on-hand. At the left extreme is the perfect consumption smoothing model, where transitory income shocks have no effect on behavior (i.e., $m_j = 0$). At the extreme right is a "complete myopia" model where households do not smooth intertemporally at all, and benefit extensions have no effect on current search behavior, implying $m_j = \infty$. The interior of the continuum includes models that have intermediate values of $m_j \in (0, \infty)$: the PIH with unrestricted borrowing but no insurance, buffer stock models (Deaton 1991; Carroll 1997), and a credit-constraint model where agents are forward looking but face a binding asset constraint.

In section VII, we calculate predicted values of m_2 for a simplified version of the PIH and the credit-constrained case. We then compare these values with an empirical estimate of m_2 to identify the "location" of the representative household on the continuum of models. We turn now to constructing this empirical estimate of m_2 by estimating the effects of cash-on-hand and future unemployment benefits on search behavior for a sample of job losers in Austria.

III Institutional Background and Data

The Austrian labor market is characterized by an unusual combination of institutional regulation and flexibility. Virtually all private sector jobs are covered by collective bargaining agreements, negotiated by unions and employer associations at the region and industry level (EIRO 2001). Firms with more than 5 employees 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 layoff (Stiglbauer et al. 2003). Despite these features, rates of job turnover are relatively high and the unemployment rate is relatively low. Stiglbauer et al. (2003), for example, show that rates of "job creation" and "job destruction" for most sectors and the overall economy are comparable to those in the U.S. 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 expanded to all other workers in 1979. Severance payments are made by firms according to a fixed schedule set by the government. In particular, firms outside of the construction industry must pay workers who are laid off after 3 years of service a lump sum severance payment equal to 2 months of their salary.[6] Payments are generally made within one month of the job termination, and are exempt from social security taxes.

Job losers with sufficient work history are also eligible for unemployment benefits. Individuals who have worked for 12 months or more in the two years preceding job loss are eligible for UI benefits that replace approximately 55% of their prior net wage, subject to a minimum and maximum (though only a small fraction of individuals are at maximum). Workers who are laid off by their employer are immediately eligible for benefits, while those who quit or are fired for cause have a four week waiting period. The maximum duration of regular unemployment benefits is a discontinuous function of the total number of months that the individual worked (at any firm) within the past five years. Individuals with less than 36 months of employment in the past 5 years 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").[7] 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. 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

[6] The severance amount rises to 3 months of pay for workers with 5 years of job tenure, 4 months after 10 years, and up to 12 months after 25 years. Employees who quit or are fired for cause are not eligible for severance pay.

[7] Starting in 1989, job losers over the age of 40 who worked at least 6 years in the past 10 years were eligible for 39 weeks of benefits.

the population (see the appendix for details of this calculation). The UI and UA systems are not experience-rated, and receipt of severance pay does not affect the unemployment benefit amount.

III.A Data and Sample Definition

We use data from the Austrian social security registry, which covers all workers except for the self-employed and civil servants. Approximately 85% of the Austrian workforce is covered by the dataset. We consider all layoff events that resulted in a UI claim between 1981 and 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. Further details on the database are given in the appendix.

The dataset does not have information on actual severance payments or the amount of UI benefits paid. However, 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 Tilly International, 2005). Given our data source, we also believe we have accurately captured the eligibility rules for extended benefits. Consequently, the eligibility rules for severance pay and EB's create essentially "sharp" regression discontinuity designs, where the fraction eligible jumps from 0 to 1 at the discontinuity (Hahn, Todd, and van der Klaauw 2001).[8]

Starting from the universe of claims, we make a number of additional sample restrictions. First, we drop people younger than 20 years of age or over 50 at the time of job termination to avoid complications with the retirement system and special UI regulations for older workers (Winter-Ebmer 2003). We eliminate people who were employed less than a year on their last job, to ensure that everyone is eligible for at least 20 weeks of UI benefits. We also exclude individuals who take up UI benefits more than 28 days after the date of job loss, thus eliminating voluntary quitters (who are ineligible for severance pay and have a 28 day waiting period for UI eligibility). From this broader sample of about 1.4 million job losers we drop construction workers (who are covered by a different set of severance pay regulations) and individuals who were recalled to their prior firm (to eliminate people on temporary layoff who may not be searching for a job). Lastly, we focus on observations around the discontinuities of interest by only including individuals who worked at

[8] As noted above, there are a few individuals in the sample who are eligible for 39 weeks of UI benefits. The fraction of individuals eligible for 39 weeks of benefits evolves smoothly around the EB discontinuity we focus on, and accounts for roughly 3% of the sample for the group around the discontinuity. Consequently, the average eligibility for UI benefits rises by ten weeks at the EB threshold. Introducing an additional control function and indicator for 39 weeks of eligibility into the hazard models estimated below does not lead to any change in the estimates of the severance pay or EB coefficients.

their previous firm for strictly between 1 and 5 years, and who worked strictly between 1 and 5 years of the past 5 years. The final sample includes 650,922 job losses. Note that individuals can appear in our sample of job losses multiple times: we observe two or more job losses for 16% of the individuals in the sample.

Table 1 presents summary statistics for three groups: a random sample of all workers between age 20-50 in Austria in one year (column 1), the broad sample of all 20-50 year old job losers in the dataset (column 2), and our final analysis sample (column 3). Since some characteristics are only recorded when people file a UI claim, information on the overall workforce is limited. The final analysis sample is slightly younger, more likely to be female, and a little less likely to hold Austrian citizenship than the overall workforce. Job losers also earn lower wages than workers as a whole.

Owing to our requirement that people have worked between 1 and 5 years at their last job, average tenure in our analysis sample is shorter than for job losers as a whole (26.5 months versus 44.4 months). However, many have worked at other employers and the gap in months of work over the past 5 years is smaller (41.1 months versus 47 months). One-fifth of the analysis sample is eligible for severance pay, while 66% are eligible for extended UI benefits. The mean gross (pre-tax) wage is 17,034 Euros per year in year 2000 Euros.[9] Overall, the characteristics of the job losers in our analysis sample are fairly similar to those of the broader set of job losers, suggesting that our empirical results are likely to be representative of the population of job losers.

We measure the duration of job search by the number of days that elapse from the end of the previous job to the start of the next job, which we term the individual's "nonemployment duration."[10] Most spells of nonemployment in Austria are relatively short: over one-half of job losers find a new job within 20 weeks and over three quarters within a year. Despite the very high fractions of people who are observed in a subsequent job, some job losers do not return to the data set, leading to a tail of extremely long censored durations.[11] The mean nonemployment duration in our analysis sample is thus nearly 17 months (not adjusting for censoring).

The final rows of the table summarize the change in log (real) wage between the old and new jobs. On average job losers suffer modest wage losses, with an mean change of -3.4%. However,

[9] Wages are top-coded at the social security tax cap in the dataset. However, this cap binds for less than 2% of the individuals in our sample.

[10] Card, Chetty, and Weber (2006, 2007) argue that time to next job is a better measure of search duration than another commonly used measure, the number of days that an individual is registered as unemployed (Lalive et. al. 2007), because it is not mechanically affected by program parameters. Nevertheless, our empirical estimate of m_2 is similar under both measures of spell length (Table 3a, column 4 in working paper).

[11] These individuals may take a job as a civil servant or become self-employed (occupations not covered by our dataset) or leave the country (to work in Germany or Switzerland). Since we restrict our sample to those who take up UI, permanent labor force leavers should in principle be excluded.

there is substantial dispersion in the wage growth distribution (standard deviation = 51%).[12] 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 VI.

IV 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. Consider the following model of the relationship between the duration of unemployment (y) and a dummy variable S which is equal to 1 if he or she receives severance pay and 0 otherwise:

$$y = \alpha + S\beta_{sp} + \varepsilon. \tag{9}$$

The parameter of interest is the coefficient β_{sp}, 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 (JT) 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_{sp} in equation (9) using OLS. This bias can be overcome 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:

$$\lim_{\Delta \to 0^+} E[\varepsilon|JT = 36 + \Delta] = \lim_{\Delta \to 0^+} E[\varepsilon|JT = 36 - \Delta]. \tag{10}$$

In this case, the control function $f(JT)$ defined by $f(JT) = E[\varepsilon|JT]$ is continuous at $JT = 36$. Thus, one can augment equation (9) with the control function:

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

[12] 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.

where the error $\nu \equiv \varepsilon - E[\varepsilon|JT]$ is now mean independent of S. Since S is a discontinuous function of job tenure, whereas the control function is by assumption continuous at 36 months, the coefficient β_{sp} is identified. Intuitively, any discontinuous relation between job tenure and duration at 36 months can be attributed to the causal impact of a severance payment under the identification assumption in (10).

In practice, the control function $f(JT)$ is unknown. We therefore approximate $f(JT)$ using a third-order polynomial (as in Angrist and Lavy 1999 or Dinardo and Lee 2004), interacting the linear and higher-order terms with a dummy for tenure over 36 months.

Selection Around the Discontinuity. One may be concerned about the validity of the identification assumption in (10) because firms have an incentive to fire workers prior to the 36 month cutoff to avoid 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 cutoff.

Although the continuity assumption cannot be fully tested, its validity can be evaluated by checking whether the frequency of layoffs and the means of observable characteristics trend smoothly with job tenure through the 36 month threshold (Lee 2006). As a first check, Figure 2 shows the number of job losers entering unemployment, by months of job tenure.[13] 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 selectively time their firing decisions to avoid severance pay. Given that such strategic behavior is illegal, and the fact that layoffs at firms with more than 5 workers must be approved by the Works Council, this is perhaps unsurprising. Moreover, firms that continually fire workers just before the eligibility threshold would presumably pay a price through reputation effects. Cases in which firms are perceived to have deliberately fired employees to avoid paying severance have led to lawsuits and coverage in the media.

Next, we check for potential differences in sample composition around the 36 month threshold by examining how observable characteristics vary with job tenure. Figure 3a plots the average number of jobs (defined as the number of continuous employment spells since the start of the data) held by job losers in each tenure-month category. This figure shows no discontinuity at 36 months of tenure, indicating that prior work histories are similar for individuals laid off just

[13] In this and all other figures, we define a "month" as a period of 31 days. We define the months starting from the discontinuity (3 years = 1096 days), counting 31 day intervals on the left and the right. Because of this counting convention and our sample restriction of having between 1 and 5 years of job tenure and months worked, the month groups 12 and 59 contain less than 10 days. Therefore, we exclude these points from the figures and only plot values for months 13 to 58. In the regression analysis, all time variables are analyzed at a daily level, and the small number of observations that fall into months 12 and 59 are included as well.

before and after the cutoff. Figure 3b conducts a similar analysis on the mean wages of those laid off at different tenures. In this case there is a small but statistically significant jump in mean wages at the discontinuity, indicating that higher-wage employees are slightly more likely to be laid off just after 36 months than just before. While this is potentially worrisome for our research design, it is important to distinguish between economic and statistical significance in a dataset of this size. The jump in the best-fit lines shown in Figure 3b is approximately 300 Euros/year, or about 1.6% of the mean wage for people with 35 months of tenure.[14] This small discontinuity is only statistically detectable because of the sample size and the relatively precise wage measures available in our data. We find similar results – either statistically insignificant effects or small but significant discontinuities – for other observables (age, education, industry, occupation, previous firm size, duration of job before the one just lost, 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 nonemployment durations. Intuitively, unless the correlation between wages and nonemployment durations is very large, a small discontinuity in wages – or any unobservable characteristic correlated with wages – cannot lead to much bias in the estimated effect of severance pay on search durations. To quantify the potential bias, we estimate the effect of wages and other covariates on re-employment hazards using a Cox proportional-hazards specification for nonemployment durations: $h_d = \alpha_d \exp(X\phi)$, where h_d denotes the re-employment hazard on day d of the spell for a given individual, α_d is the "baseline" hazard, and X denotes a rich set of observed characteristics, including demographics, previous work history and wages, and region and time effects (see the notes to Figure 4 for the complete list of regressors). 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 predicted hazard is different for those laid off just before or after the eligibility threshold.

The predicted relative hazards for different tenure groups are plotted in Figure 4. The downward trend indicates that people with longer job tenure have observable characteristics that on average are associated with longer durations. The predicted hazards are smooth through the 36 month threshold, however, implying that any small discontinuities in the observable characteristics

[14] Note that higher wage workers have shorter unemployment durations in our data. This small amount of selection should therefore, if anything, work *against* finding a positive effect of severance pay on durations.

have little net impact on nonemployment durations. One may be concerned that differences in *unobserved* characteristics (such as motivation or ability) could also violate our key identification assumption. While this can never be ruled out entirely, many of the $X's$ included in the construction of Figure 4 are "endogenous" outcomes, such as the number of previous jobs, the duration of the most recent spell of non-employment, and wages. Unobserved attributes that affect the duration of job search are likely to be highly correlated with these observed variables. Hence, if there were important differences in unobserved attributes between those laid off just before or just after the threshold, we would expect a jump in the predicted relative hazard at $JT = 36$. Since there is no such jump in Figure 4, we conclude that individuals are "nearly randomized" around $JT = 36$, implying that any discontinuity in search behavior at this point can be attributed to the causal effect of 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 S 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. As in equation (9), 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 reemployment 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 3b, there is no significant jump in mean wages or any other covariate around $MW = 36$. Overall, we conclude 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. The effects of severance pay and EB can be independently identified using RD designs because they are discontinuous functions of different running variables: job tenure in the case of severance pay, and months worked in the past 5 years in the case of extended benefits. Nevertheless, there is a subset of individuals (roughly 20% of the sample) – those who did not work in the two years prior to their 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). In particular, the fraction eligible for extended benefits jumps from 80% to 100% at 36 months of job tenure, the threshold for severance pay eligibility. 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 issue arises at the threshold for extended benefits eligibility, where there is a 20% jump in the fraction eligible for severance pay.

To see how the two effects can be separated, consider the extended model

$$y = \alpha + S\beta_{sp} + E\beta_{eb} + \varepsilon \tag{12}$$

where S and E are indicators for severance pay and EB eligibility, respectively. As in the single discontinuity case, the problem for inference is that the unobserved determinants of y 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 identification assumption 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 assumption, we can augment equation (12) with the control function

$$y = \alpha + S\beta_{sp} + E\beta_{eb} + g(JT, MW) + \nu$$

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 $\beta_{sp}$and β_{eb} are identified. We implement this "double discontinuity" model by assuming as above that g can be approximated by a low order polynomial of JT and MW.

An alternative method of separating the EB and severance effects is to analyze a subsample in which the two thresholds never coincide. Specifically, consider the subsample of people who worked at least one month in the past 5 years at a firm different from the one from which they were just laid off. In this subsample, the fraction eligible for EB reaches 100% when job tenure equals 35 months, eliminating the overlapping thresholds at $JT = 36$. We obtain similar estimates for the EB and severance pay effects using conventional RD methods on this "restricted" subsample.

V Effects of Cash-on-Hand and Benefit Extensions on Durations

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

V.A Graphical Results

Severance Pay. We begin our analysis in Figure 5 by plotting mean nonemployment durations vs. months of job tenure. For simplicity, in this figure we ignore censoring (effectively treating all measured durations as complete), and exclude observations with a nonemployment duration of more than two years to eliminate the long right tail of the distribution. For visual reference, we superimpose a quadratic regression model fit separately to points on the right and left of the eligibility threshold. The figure shows a clearly discernible jump of about 10 days in the average nonemployment duration at the threshold for severance pay eligibility. Note that the graph is smooth away from the $JT = 36$ threshold, implying that the average search durations are similar for people with similar job tenures in the absence of the discontinuous severance pay rule. The actual shape of the graph away from the discontinuity reflects the correlation between job tenure and the (observed and unobserved) characteristics that drive the duration of search, and has no causal interpretation.

We cannot attribute the entire jump in Figure 5 to the effect of severance pay because the fraction of individuals receiving EB also jumps at the cutoff. In Figure 6, we adjust for this "double discontinuity" and correct for the censoring of nonemployment spells by examining how the re-employment hazard rate changes at the severance pay eligibility threshold. In constructing this figure, we include all spells and focus on the re-employment hazard in the first 20 weeks – the period of interest from the perspective of testing between models since it includes only the time before the benefit extension – by censoring all observations at 140 days. To obtain an estimate of the average re-employment hazard for people with different months of previous job tenure, we fit a Cox proportional-hazards model with dummies for each tenure group. We adjust for the double discontinuity problem by including cubic polynomials in months worked, a dummy for extended benefit eligibility, and their interaction:

$$
\begin{aligned}
h_d = \alpha_d \exp\{&\theta_{13} I(JT=13) + ... + \theta_{34} I(JT=34) + \theta_{36} I(JT=36) + ... + \theta_{58} I(JT=58) \\
&+ \beta \times E + \beta_1 MW + \beta_2 MW^2 + \beta_3 MW^3 \\
&+ \beta_1^E E \times (MW-36) + \beta_2^E E \times (MW-36)^2 + \beta_3^E E \times (MW-36)^3\}.
\end{aligned} \tag{13}
$$

The key coefficients of interest in this specification are the θ_{JT}'s, which measure the percentage difference between average daily hazard for people with JT months of previous job tenure and those with 35 months of tenure (the omitted group). Figure 6a plots the estimated θ_{JT}'s from equation (13). Consistent with the results in Figure 5, there is a discontinuous drop of approximately 10% in the average 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.

A potential concern in Figure 6a is that there is some cyclicality in the hazard rates associated with job tenure. Close examination of the graph suggests that 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 before completion of a full year of service are different from those who leave just after. Such differences may arise because planned terminations are more likely to take place after a full year of service is complete, or because of features such as employer-provided pensions that vest after integer numbers of years of service. To gauge whether this seasonality pattern affects the severance pay estimate, we estimate 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.[15] Figure 6b shows that the seasonality adjustment fully eliminates the potentially worrisome patterns in Figure 6a but does not affect the drop in hazards at the severance pay cutoff significantly, indicating that the results are not substantially affected by the cyclical pattern.

As noted above, the causal interpretation of our results relies on the identifying assumption

[15] More precisely, we estimated specification 1 in Table 2, adding the "end of tenure year" indicator. We then subtracted the coefficient estimate on the indicator from the hazard rates shown in Figure 6a for the last three months of each tenure-year category (i.e., 21-23, 33-35, etc). Adjusting for additional covariates in these figures does not affect the results (Figure 6b in the working paper).

that in the absence of severance pay there would be no systematic differences in nonemployment durations between individuals laid off on either side of the 36 month eligibility threshold. The panel structure of our dataset allows a simple "placebo" test of this assumption, using the 16% of our sample who we observe with more than one job termination. In particular, if people who are laid off after 35 months of tenure are systematically different than those laid off after 36 months, one would expect a discontinuous effect of job tenure at the job *before* the one just lost on the *current* duration of nonemployment. Figure 7 examines this relationship, and shows that current nonemployment durations evolve smoothly through the 36 month cutoff for lagged job tenure, supporting our identification assumption. Any omitted-variables explanation of our findings would therefore require that people'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). To eliminate any double discontinuity effects, this figure is drawn using the "restricted" subsample of individuals with at least one month of work at another employer in the past 5 years; hence, the differences between the job finding hazards in this figure can be attributed entirely to severance pay (and not EB). The figure shows that severance pay lowers job finding hazards throughout the spell. The gap between the hazard rates in the two groups expands after week 5 of the spell, and gradually narrows starting around week 25. This pattern is consistent with a model where agents become increasingly sensitive to cash-on-hand as the spell elapses, but eventually deplete the initial cash grant.

We interpret Figures 5-8 as showing that a shock to cash-on-hand has substantial effects on behavior, rejecting a model of perfect consumption smoothing.

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. As in Figure 5, this figure ignores censoring and excludes observations with a nonemployment duration of more than 2 years. There is a clearly discernible jump in the average duration of joblessness of approximately 7 days around the EB discontinuity. In Figure 9b, 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 dummies for months of work in the previous 5 years instead of job tenure. To eliminate the double discontinuity problem, we include a cubic polynomial in job tenure and a dummy for severance pay eligibility (plus their interactions), analogous to Figure 6b. There is a discontinuous drop of approximately 7% in the average hazard rate at the cutoff for EB eligibility.

In Figure 8b, we examine 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. As in Figure 8a, we again use the "restricted" subsample defined above to eliminate the overlapping discontinuities. This figure shows that the benefit extension has a large effect on behavior after week 20, when the additional income is received. However, people eligible for extended benefits also have substantially lower job finding hazards than those ineligible for EB *prior to* week 20, i.e. before they actually receive any additional income. This result (consistent with Figure 9b) provides clear evidence that at least some individuals are forward-looking, and take into account their future expected income stream when choosing search behavior in the early weeks of the spell.[16] This finding rejects a model of completely myopic behavior.

V.B Hazard Model Estimates

To quantify the effects of severance pay and extended benefits on the duration of job search more precisely, we estimate a series of proportional hazards models for the risk of finding a new job. These models include unrestricted daily baseline hazards, a set of covariates (X), indicators for eligibility for severance pay and extended benefits (S and E, respectively), and third-order polynomials in job tenure (JT) 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:

$$\begin{aligned} h_d = \alpha_d \exp\{\beta_{sp}S \ + \beta_{eb}E + \phi X & \\ + \mu_1 JT + \mu_2 JT + \mu_3 JT^3 & \\ + \mu_1^S S \times (JT - 36) + \mu_2^S S \times (JT - 36)^2 + \mu_3^S S \times (JT - 36)^3 & \\ + \beta_1 MW + \beta_2 MW^2 + \beta_3 MW^3 & \\ + \beta_1^E E \times (MW - 36) + \beta_2^E E \times (MW - 36)^2 + \beta_3^E E \times (MW - 36)^3\}. & \end{aligned} \tag{14}$$

[16] This behavioral response is consistent with but conceptually distinct from Katz and Meyer's (1990) well known finding that unemployment exit hazards rise in the weeks immediately before the date of benefit exhaustion. We show that the benefit exhaustion date affects search behavior early in the spell as well.

In all models, we censor the spells at 140 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 coefficient β_{eb} captures purely the effect of *future* benefits on current search activity, i.e. the forward-looking behavior documented in Figures 8b and 9b.

Table 2 presents estimates of β_{sp} and β_{eb} from a set of alternative samples and specifications. In this and all subsequent tables, we adjust for potential correlation in errors across spells by clustering the standard errors by person. In column 1, we estimate the severance pay and EB effects using (14) without any additional controls. These estimates indicate that eligibility for severance pay reduces job finding hazards in the first 20 weeks by 12.5%, while eligibility for EB reduces the hazard rate by 9.3%. Both coefficient estimates are highly statistically significant, with a t-statistic of 7.4 for severance pay and 5.8 for EB.

In columns 2 and 3, we assess the robustness of the estimates to the inclusion of covariates. Specification 2 includes a set of basic covariates – gender, marital status, Austrian nationality, "blue collar" occupation indicator, age and its square, log previous wage and its square, and dummies for month and year of job termination. Specification 3 adds the full set of worker and firm characteristics used in Figure 4 (see the notes to Table 2 for the full list). Many of these covariates – such as the duration of the previous spells of employment and nonemployment – are correlated with the unobserved skills and tastes of job seekers, and could not be included in a conventional causal model for the effects of severance pay and extended benefits. In a regression discontinuity design, however, adding such "endogenous" control variables does not affect the consistency of the RD estimates. The estimates of β_{sp} and β_{eb} remain stable and precisely estimated as the set of covariates is expanded. The robustness of the estimates to the inclusion of this rich set of covariates helps mitigate concerns that our results are driven by selection around the discontinuity (Lee and Card 2006).

In column 4, we evaluate whether the estimates would differ in a sample whose characteristics are more reflective of the average labor market participant. To obtain estimates for a sample with characteristics representative of the overall population, we reweight the data by age, gender, wage, nationality, and occupation to match the sample means for all workers (see the appendix for details on the construction of the weights). We then replicate specification (2) with these weights. The coefficient estimates do not change significantly, suggesting that a more representative sample would exhibit similar intertemporal behavior.

Robustness Checks. We further examined the importance of selection around the severance pay discontinuity by restricting the analysis to two subgroups for which selective firing is less likely to occur: (1) individuals laid off by small firms (<100 employees) and (2) cases where multiple individuals were laid off together from the same company in the same month. Since workers in smaller firms typically perform more specialized job functions and have fewer close substitutes, we conjecture that these firms will find it harder to lay off one worker instead of another simply to save a severance payment. Similarly, we expect that layoffs involving multiple workers are more likely to be caused by an "exogenous" shock such as financial distress, and thus less likely to involve selective firing. Consistent with this intuition, we find less evidence of a discontinuity in wages in these subsamples than in the full sample. Reassuringly, the point estimates of the severance pay and EB coefficients remain quite stable in samples involving smaller firms or larger group layoffs. A representative set of these estimates is shown in column 5 of Table 2, which replicates specification (2) for the subsample of individuals laid off from a firm that laid off four or more workers within a single month. See Table 3b in the working paper for additional estimates.

We have fit a wide variety of other specifications to further probe the robustness of the results in Table 2. Adjusting for seasonal patterns associated with integer years of tenure does not changed the estimated coefficients significantly. Replacing the third-order polynomials with fourth-order polynomials leads to estimated severance pay and EB effects that are a little bigger in magnitude than those reported in Table 2. Estimating the effects of the two policies on average hazards over a shorter period (e.g. the first 10 weeks) or a longer period (e.g. the first six months or year of the spell) also yield similar results. The estimated severance pay and EB effects are always on the order of -6 to -12 percent, with a ratio of β_{sp}/β_{eb} between 1.2 and 1.8.

Finally, we conducted "placebo tests" analogous to Figure 7 by estimating hazard models with placebo eligibility cutoffs for individuals with two or more unemployment spells in the data. Consistent with the graphical evidence, we find that the severance pay and EB placebos – indicators for having more than 36 months of job tenure on the previous job, and more than 36 months of work in the 5 years before the end of the previous job – have no effect on current nonemployment durations. In a model that includes both the placebo and true severance pay and EB eligibility indicators, the hypothesis that the placebo and true severance pay effects are equal is rejected with $p < 0.01$. These results provide further support for the causal interpretation of our estimates.

Heterogeneity Analysis. One intuitive method of testing between intertemporal models is to split the sample into liquidity constrained and unconstrained groups (as in Zeldes 1989), and compare the estimated effects of severance pay in these groups. Unfortunately, the Austrian

Social Security database does not have proxies for liquidity such as data on the assets or family circumstances of job losers. Cutting the data on variables that are likely to be correlated with liquidity – such as age or wage – is problematic because our treatment effects are likely to vary across these groups for other reasons. For example, higher wage or older workers may have more assets on average, but also receive larger severance pay amounts on average and are less likely to receive unemployment assistance. Thus, we cannot make sharp predictions about heterogeneity across demographic groups. In practice, we found no systematic differences in the estimated effects of severance pay and EB across wage quartiles, age quartiles, gender, blue collar status, education, or region. We also split the sample across time periods (e.g. recessions vs. booms), and found no significant differences in the estimates across these periods.

While we cannot obtain clear evidence on heterogeneity in the Austrian data, such evidence is available in U.S. data. Chetty (2006b) studies the heterogeneity in the effect of UI benefits on unemployment durations. He finds that the effect of UI benefits on durations is largest among groups that are likely to have limited ability to smooth consumption – e.g. those with low assets prior to job loss or a single earner in the household. This finding is consistent with models such as buffer-stock behavior, which also match the results we obtain here.

VI Search Outcomes

Having found that severance pay and extended benefits increase the duration of search, we now explore whether the longer search process leads to improvements in job match quality.

VI.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 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, individuals could potentially find jobs with higher quality in other dimensions. One convenient summary statistic for the match quality of subsequent jobs is their duration: better matches should presumably last longer (see e.g., Jovanovic 1979). We examine the effect of severance pay on the duration of the next job in Figure 10b. This figure plots the average monthly hazard of leaving the next job (over the first 5 years on that job) by tenure at the job that just ended. We construct this figure by fitting a Cox model for the duration of the next job, with dummies for the tenure-month categories (omitting month 35). We then plot the coefficients on the tenure-month categories, which can be interpreted as the percentage difference in the average job-leaving hazard in a given tenure-month group relative to tenure-month 35. The job-leaving hazards are smooth through the discontinuity, indicating that severance pay eligibility has no effect on the duration of the subsequent job.

We have conducted an analogous analysis for extended benefits by changing the running variable on the x-axis to months worked in the past five years (Figure 11 in the working paper). Again, we find that both wages and subsequent job-leaving hazards are smooth through the EB discontinuity. Hence, extending unemployment durations by increasing the maximum potential duration of UI benefits does not appear to yield any match quality gains as measured by wages or subsequent job duration.

VI.B Regression Estimates

To formally identify the match quality impacts of severance pay and extended benefits, we estimate double RD specifications analogous to (14), changing the dependent variable to a measure of job quality. Specification 1 of Table 3 examines the effect of severance pay and EB on wage growth (g_i) using an OLS regression without any controls. Specification 2 adds the full control set used in specification 3 of Table 2 to this regression. Specification 3 reports coefficient estimates from a hazard model for the duration of the new job without controls, censoring next job durations at five years to examine how the policies affect average job-leaving hazards in the first five years. Finally, specification 4 replicates specification 3 with the full control set. The regression estimates in Table 3 are consistent with the figures: there is no evidence of match quality gains in any of the specifications. For example, in the specifications with controls, severance pay is estimated to change wage growth by a statistically insignificant -0.2% and change the hazard rate of leaving the next job by 0.0%.

An important distinction between the present analysis and some earlier studies that have failed to detect evidence of quality gains is the relative precision of our estimates. The standard errors in specifications 1 and 2 of Table 3 show that even a 1% improvement in wages caused by either severance pay or EB would be detectable in our analysis. Hence, our evidence suggests that any job quality gains from extending unemployment durations are quite small in magnitude.

We also checked for match quality effects using analogous regression models and graphical methods for several other measures (estimates available in Table 4b of the working paper): the probability of switching industries or occupations, the probability of moving to a different geographical region, the total number of days employed in each of the five years following the unemployment spell, the mean growth in wages and total earned wage income (at any employer) in each of the five years following the unemployment spell, and the change in the size of the firm (number of employees) at which the individual is employed. In addition, we examined percentiles of the wage distribution to check if there are gains in the tails of the distribution. None of these outcomes shows evidence of discontinuities at the eligibility thresholds for extended benefits or severance pay. 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.

In view of these results, we conclude that the extension of search duration through increases in cash-on-hand or provision of extended UI benefits has little or no effect on subsequent job quality in the short and intermediate run (up to 5 years). There are several potential explanations for the absence of match quality effects. One possibility is that there is limited variation in the quality of jobs available to a given worker because of the high rate of union coverage in Austria. As noted in section III, however, the variation in wage changes experienced by job losers is fairly large ($\sigma(\Delta \log w) = 0.51$), suggesting that there is significant ex-ante uncertainty about job qualities. A second explanation – emphasized by Classen (1979) – is that reservation wages decline over the spell, making the effect of assets and UI benefits on observed match quality theoretically ambiguous. A third possibility is that reservation wages do not rise much in response to severance pay or extended benefits because employed workers can continue to search. Indeed, if search is equally productive on and off the job, the reservation wage only depends on current UI benefits (Lise 2006), and severance pay and extended benefits should have no effect on the quality of the jobs obtained in the first 20 weeks of unemployment. A final explanation is that the arrival rate of job offers is relatively low, so the option value of waiting for a better offer is small and most workers take the first offer they receive. Unfortunately, given the available evidence, we cannot distinguish between these alternative explanations.

VII Calibration Results for Competing Models of Behavior

In this section, we use the theoretical framework developed in Section II to interpret the implications of our empirical findings for models of intertemporal behavior. In relating our empirical estimates to the search model, we define each "period" as an interval of 10 weeks. Under this timing convention, the benefit extension from 20 to 30 weeks raises the value of UI benefits 2 periods after the period of job loss. By combining the estimated effects of severance pay and the benefit extension on the re-employment hazard, we can obtain an estimate of the moment m_2 defined in equation (8). We then compare the empirical estimate of m_2 to the values predicted by two benchmark models: a simple PIH model and a credit-constraint model where consumption equals current income.

Empirical Estimate of Sample Moment. Our hazard models give the effects of eligibility for 2 months of severance pay or 10 weeks of additional UI benefits on the log of the re-employment hazard rate. To calculate the implied value of m_2, we re-scale the hazard coefficients into estimates of the relative effects of a \$1 increase in cash-on-hand and a \$1 increase in b_2. Letting v_{sp} denote the cash value of severance pay, the effect of eligibility for severance pay on the hazard rate is $\beta_{sp} \approx \partial \log s_0^*/\partial A_0 \times v_{sp}$. Likewise, $\beta_{eb} \approx \partial \log s_0^*/\partial b_2 \times v_{eb}$, where v_{eb} represents the cash value of extended benefits. Given estimates of β_{sp} and β_{eb} the implied estimate of m_2 is therefore:

$$m_2 \equiv \frac{\partial s_0^*/\partial A_0}{\frac{1}{p_2^*}\partial s_0^*/\partial b_2} = \frac{\partial \log s_0^*/\partial A_0}{\partial \log s_0^*/\partial b_2} \times p_2^* = \frac{\beta_{sp}}{\beta_{eb}} \times \frac{v_{eb}}{v_{sp}} \times p_2^*.$$

In the appendix, we show that $v_{sp} \approx 2.69w$, where w is the net (after-tax) monthly wage, and that $v_{eb} \approx 0.85w$. Hence $v_{eb}/v_{sp} \approx 0.32$. To calculate p_2^*, the probability that extended benefits are actually received, first note that 50% of the job losers in our sample are not observed in a new job within 20 weeks. However, this 50% figure overstates the fraction of individuals who are actually out of work for 20 weeks, because some individuals presumably return to work in sectors not covered by our data (self-employment or civil service). Given the low hazard of observed re-employment after two years, we believe that most of the individuals in our sample who are not observed with a job after two years (15% of the sample) have returned to work in uncovered sectors. Assuming that the re-employment rates of these missing individuals are the same as those of other job losers, the implied probability of remaining out of work for 20 weeks or more is $p_2^* = 1 - \frac{0.5}{(1-.15)} = 0.41$.

Combining all these elements, we conclude that $m_2 \approx 0.13\beta_{sp}/\beta_{eb}$. Using the estimates of β_{sp} and β_{eb} reported in column 1 of Table 2, the baseline no-controls specification, we obtain a point estimate of $m_2 = 0.174$ with a standard error (constructed by the delta method) of 0.041. The estimates from column 3 of Table 2, which includes our richest set of controls, imply $m_2 = 0.19$, with a standard error of 0.071.

Predicted Moment for Credit-Constraint Model. Consider a model where individuals are forward looking but set consumption equal to income in each period. We now calculate the value of m_2 predicted by this "fully credit constrained" model by computing the values of D and Z_2 in (8).

We first compute D, the gap in marginal utilities in the period of job loss. Let F represent other family income, which we shall assume is exogenously fixed. Since consumption equals income, $c_0^e = w + F$ and $c_0^u = b_0 + F$. Let $\rho_t = \frac{b_t}{w}$ denote the UI replacement rate in period t of the unemployment spell and $\sigma = w/(w + F)$ denote the share of the job-seeker's earnings in total family income. Assuming that $u(c)$ exhibits constant relative risk aversion ($u(c) = \frac{c^{1-\gamma}}{1-\gamma}$), it follows that

$$D = \frac{u'(\rho_0 w + F) - u'(w + F)}{u'(\rho_0 w + F)} = \frac{[\sigma\rho_0 + (1 - \sigma)]^{-\gamma} - 1}{[\sigma\rho_0 + (1 - \sigma)]^{-\gamma}}$$

As discussed in the appendix, we estimate from survey data that a typical Austrian wage earner in our age range contributes about 1/2 of his or her family income ($\sigma = 0.50$). The average UI replacement rate is $\rho_0 = 0.55$. Using these values of σ and ρ_0, we obtain a simple mapping from the coefficient of relative risk aversion (γ) to D. For example, if $\gamma = 2$, $D = 0.4$.

Next, we compute Z_2, the change in marginal utility over the spell. Note that $c_2^u = \rho_2 w + F$, where ρ_2 represents the replacement rate for income support in the absence of extended benefits, which we estimate to be approximately 0.21. Thus

$$Z_2 = \frac{u'(\rho_0 w + F)}{u'(\rho_2 w + F)} = [\frac{1.55}{1.21}]^{-\gamma}$$

with CRRA utility. For example, with $\gamma = 2$, $Z_2 = 0.61$. Using (8), it follows that $m_2 = 0.4 \times 0.61 \times (1 + \delta)^2$, where δ is the discount rate over a 10 week period. If the annual discount rate is $\delta = 10\%$, $m_2 = 0.253$ when $\gamma = 2$. Values of m_2 predicted by the credit-constraint model for other combinations of risk aversion and annual discount rate are presented in Panel A of Table 4.

Predicted Moment for PIH Model. Now consider a model where individuals have unrestricted access to credit at a fixed interest rate – the permanent income hypothesis (PIH). The calculation of m_2 in this case is more complicated, and in general requires an iterative solution procedure. We instead derive an upper bound for the predicted value of m_2 under three simplifying assumptions.

First, we assume that the rate of time discount equals the interest rate. This implies that once employed, people choose a constant consumption profile. Second, we assume that people have a relatively long work life, so that the annuity income from an asset amount A is approximately $r/(1+r)A$.[17] Together with our assumption that jobs persist indefinitely, these assumptions imply that individuals consume the annuity value of their wealth once re-employed: $c_t^e = w + F + r/(1+r)A_t$. Our third assumption is that individuals can find a job with certainty within T periods. As noted above, 85% of the job losers in our sample are observed in a new job within 2 years, and the remaining 15% are likely to have taken jobs outside the sectors covered by our data. Therefore, we set $T = 10$ (i.e., 10 periods of 10 weeks, or approximately 2 years).

To derive an upper bound for m_2, first observe that consumption will fall over the unemployment spell, implying that $Z_2 = u'(c_0^u)/E_0[u'(c_2^u)] < 1$ and that $m_2 = DZ_2(1+\delta)^2 \leqslant D(1+\delta)^2$. Hence, an upper bound on D yields an upper bound on m_2.

We derive an upper bound for D in a series of steps. The general logic is to bound the size of the consumption drop at the time of job loss ($c_0^e - c_0^u$) by exploiting two facts: (1) an optimizing agent will equate his marginal utility of consumption while unemployed with his expected marginal utility once re-employed, and (2) consumption when re-employed is bounded below by the annuity value of remaining wealth if the agent were to consume his full wage income even while unemployed.

The first step in bounding D is to calculate a lower bound on the optimal path of c_t^e. Since consumption is always lower when unemployed than employed ($c_t^u \leq c_t^e$), the rate of decline in assets over a spell of unemployment can be bounded.[18] This upper bound on the rate of decline in assets yields a lower bound on consumption if the agent finds a job in period t:

$$c_t^e \geqslant c_0^e - r \sum_{k=1}^{t} (w - b_k) \tag{15}$$

where $c_0^e = w + F + r/(1+r)A_0$. Next, we use this bound on c_t^e to derive an upper bound on $u'(c_0^u)$, the marginal utility of consumption in the first period of the unemployment spell. Consider the consumption Euler equation for an individual who does not find a job at the beginning of period 0:

$$u'(c_0^u) = E_0[s_1^* u'(c_1^e) + (1 - s_1^*) u'(c_1^u)] \tag{16}$$

[17] This is a reasonable approximation for our case, since we focus on people under age 50, and the Austrian pension system is quite generous (replacing about 75% of wages).

[18] Specifically, if a person is still unemployed in period $t-1$, $A_t = (1+r)(A_{t-1} + F + b_{t-1} - c_{t-1}^u)$. Using the fact that $c_{t-1}^u \leqslant c_{t-1}^e$ and the equation for c_{t-1}^e, this implies that $A_t \geqslant A_{t-1} - (1+r)(w - b_{t-1})$, and thus $A_t \geqslant A_0 - (1+r)\sum_{k=0}^{t-1}(w - b_k)$.

where s_1^* is the optimal level of search intensity in period 1.[19] Iterating forward, if the job seeker can always find a job within T periods, (16) implies that

$$u'(c_0^u) = \sum\nolimits_{t=1}^{T} q_t^* u'(c_t^e), \tag{17}$$

where $q_t^* = (1 - s_1^*)(1 - s_2^*)...(1 - s_{t-1}^*)s_t^*$ represents the probability of obtaining a job in period t, conditional on unsuccessful search in period 0. The intuition underlying (17) is that an optimal consumption path must equate the marginal utility when unemployed with the expected marginal utilities in subsequent periods after re-employment. We use the empirical distribution of waiting times to a new job in our sample to estimate q_t^*.[20] Finally, plugging in the lower bound on c_t^e in (15) and the empirical values of q_t^* into equation (17), we obtain an upper bound on $u'(c_0^u)$. This translates directly into an upper bound on $D = \frac{u'(c_0^u) - u'(c_0^e)}{u'(c_0^u)}$ since c_0^e is fixed.

Obtaining a numerical value for D through this procedure requires specification of several parameters related to the income path and preferences. We assume that $\rho_t = 0.55$ for the first 30 weeks (3 periods) of joblessness, and that $\rho_t = 0.21$ thereafter, reflecting the safety net of unemployment assistance. We also assume that a typical job loser contributes $\sigma = 50\%$ of his or her family income, assets at the time of job loss $A_0 = 0$, and utility exhibits CRRA.

The free parameters in calibrating the PIH model are the interest rate (r, assumed to be equal to the rate of time discounting δ) and the coefficient of relative risk aversion (γ). We present the implied upper bounds $D(1 + \delta)^2 \geqslant m_2$ for various combinations of the annual interest rate and risk aversion in Panel B of Table 4. Note that alternative parameter combinations can lead to approximately the same prediction for m_2. For example, a model with $\gamma = 1, r = 10\%$ yields an upper bound for $D(1 + \delta)^2 = 0.013$, the same value implied by a model with $\gamma = 2, r = 5\%$.

Comparing the Empirical Estimate to the Benchmarks. How does the empirical value of the sample moment compare with the values predicted by the two benchmark models? Panel C of Table 4 shows the empirical values of m_2 implied by the no-controls and full-controls hazard model estimates for comparison to the values predicted by the two models.

The data appear to be clearly inconsistent with the simple PIH model, using values for r and γ in a conventional range. For example, the lower bound of the 95% confidence interval for the estimate of m_2 based on the baseline hazard model without controls is 0.115. Comparing this

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

[20] In calculating q_t^*, we ignore those who are not observed in a new job within $T = 10$ periods (100 weeks), again assuming that this group finds jobs in sectors not covered by our dataset at the same rate as the rest of the sample.

lower bound to the predictions in Panel B of Table 4, one could reject any parameter combination with $r < 30\%$ or $\gamma < 3$. Hence, unlike most consumption-based studies which find evidence of "excess sensitivity," the estimates here are sufficiently precise to rule out the PIH even with fairly extreme assumptions about risk aversion and the interest rate.

The estimates of m_2 are closer to the fully credit-constrained model, which predicts $m_2 = 0.253$ if $\gamma = 2$ and $\delta = 10\%$. This prediction is above our point estimates of m_2, but lies within the 95% confidence interval of the estimates.

Summary. Figure 1 summarizes our calibration results by showing where the representative agent in the data lies on the continuum of dynamic models ordered by sensitivity to cash-on-hand. The predicted values of m_2 for the PIH and credit-constraint models in this figure assume $r = 5\%$ and $\gamma = 2$, which are typical parameter choices for the interest rate and risk aversion in the literature (see e.g., Carroll 2004, Chetty 2006c). Our empirical estimate of $m_2 \simeq 0.17$ is about 70% of the way between the values predicted by the PIH and credit-constraint models. A model with heterogeneous agents, some of whom behave as the PIH predicts and some of whom set consumption equal to income (as in Campbell and Mankiw 1989), could therefore fit the data.

We considered the PIH and credit-constraint models primarily for illustrative purposes: a similar exercise could be performed for many other models. It is important to keep in mind that a wide range of models could potentially predict values for m_2 that are consistent with the data. Specifying the value of m_2 identifies a plane within the space of parameters defined by preferences and financial technologies (e.g. asset limits, discount rates, risk aversion, prudence) but does not uniquely identify any one model. One well-known model that is likely to be consistent with our estimates is Deaton's (1991) buffer-stock model, which assumes forward-looking behavior but an asset limit that eventually constrains borrowing. In this model, the optimal level of consumption while unemployed can be substantially lower than in the PIH, leading to a higher predicted value for m_2. A similar consumption profile is predicted by Carroll's (1997, 2004) intertemporal consumption model, which does not impose an exogenous asset limit. However, a key assumption of the Carroll model – that income can fall to 0 – is less attractive for Austria, where unemployment assistance constitutes a lower bound on income.[21]

It is worth underscoring some of the limitations of our calibration exercise. A key assumption is the existence of a single "representative agent." While this is a convenient simplification, it ignores

[21] See Michaelides (2003) for a more detailed discussion of how the availability of social insurance can be used to distinguish between the Deaton and Carroll models.

heterogeneity in the value of UA benefits, other family member's incomes, and assets. If data were available, it would be preferable to calibrate the model separately for different subgroups and construct an average predicted value for m_2. We have also calibrated a particularly simple version of the PIH that assumes separability between consumption and leisure and ignores differences in the length of work life and the risk of future job separations. Finally, our theoretical framework focuses on search intensity and ignores the choice of reservation wages. We believe our qualitative conclusions would hold if these assumptions were relaxed, particularly in view of the evidence of small match-quality effects. Nevertheless, it would be useful to re-evaluate our conclusions about intertemporal behavior using a richer model in future work.

VIII Conclusion

The general objective of this paper has been to bring methods and data from the labor economics literature to bear on a question of longstanding interest in macroeconomics and public finance: how does cash-on-hand affect household behavior? Our empirical findings – that cash-on-hand has relatively large effects on search behavior relative to unemployment benefit extensions – imply that the behavior of job searchers is best described by a model such as buffer-stock savings, where agents have limited capacity to smooth income fluctuations.

This characterization of household behavior has several implications for public finance. The evidence of imperfect smoothing suggests that temporary tax changes could have significant economic effects. In addition, there may be a substantial role for temporary income support programs such as unemployment insurance and short-term welfare. The finding that cash grants change search behavior in a manner similar to UI benefit extensions implies 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 incentives. Finally, the finding that the provision of temporary benefits leads to little or no improvement in job match characteristics suggests that long-term improvements in job match quality are unlikely to provide a strong rationale for such programs.

In future work, it would be interesting to analyze optimal policy in dynamic models that allow for general equilibrium effects, calibrated to match the evidence here. More generally, the idea of using data on labor supply instead of consumption to distinguish between models can be applied in other settings. For example, examining whether work hours or retirement choices exhibit "excess sensitivity" to cash-on-hand may yield further insights into models of household behavior.

Appendix

A. Sample Definition.

The Austrian Social Security Database contains employment records for private sector employees, public sector workers who are not classified as permanent civil servants, and the unemployed. The groups for whom information is missing are self employed and civil servants. Based on Austrian national statistics, about 10% of the labor force were self employed and 7% were civil servants in 1996. Therefore, we estimate that the Social Security Database covers roughly 85% of the total workforce.

For each covered job, the database reports the starting and ending date of the job, the identity of the employer, certain characteristics of the job (e.g., industry, occupation), and total earnings. No information is available on hours of work. Earnings are censored at the Social Security contribution limit, but this only affects a small fraction (2%) of the observations in our sample. The database also includes starting and ending dates for unemployment insurance (UI) claims, and information on whether an individual is registered with the employment office as looking for work. No information is available on the amount of UI payments actually received. We code an individual as "unemployed" if he or she is receiving UI, or registered as looking for work.

From the database, we extract all terminations between 1981 and 2001 from jobs that (a) had lasted for at least one year, (b) were followed by a UI claim, and (c) did not result in a retirement claim within the same calendar year (total of 1,817,221 terminations). We exclude terminations from jobs in schools, hospitals, and other public sector service industries (4% of the total) because some of these jobs are fixed term. We also exclude jobs in the construction sector (17% of the remaining sample) because of the different severance pay regulations. We then eliminate terminations from jobs that lasted for 5 or more years, and for individuals who worked all weeks in the past 5 years. These two restrictions reduce the remaining sample by a further 33%. We eliminate terminations involving people whose age in years is under 20 or over 49 at the time of the job loss (a further 10% of the remaining sample), and individuals who return to the same employer (a further 19% of the remaining sample). Finally, we drop all terminations with a delay of over 28 days between the job termination date and the start of the UI claim. This restriction eliminates job quitters (who face a 4 week waiting period for UI) and eliminates another 10% of the remaining sample. The final analysis sample contains 650,922 observations. Among individuals in the sample at least once, we observe one job loss for 84%, two job losses for 13%, and 3 or more job losses for the remaining 3%.

For the job losses in our sample, we use all available information on employment, unemployment, and earnings in the Social Security database files for the years 1972 to 2003. We merge in information on completed education and marital status from the Austrian unemployment registers, which are available from 1987 to 1998. Spell-specific demographic information is available in this file for each unemployment spell, and we use the information in the last recorded unemployment spell for each individual to assign education and marital status. For individuals whose only spell of unemployment occurred before 1987 or after 1998, however, these variables are missing. We can assign information for 66% of job losses occurring before 1987, and 75% of job losses after 1998.

B. Cash Value of Severance Pay and Extended Benefits.

Severance pay is equal to 2 months of gross wages, but is taxed at roughly 6%. The average tax rate on earnings in Austria is approximately 30 percent. Letting w represent the net monthly wage, the value of severance pay is therefore $2w(1-.06)/(1-0.3) = 2.69w$.

Extended benefits provide 10 extra weeks (2.5 extra months) of eligibility for UI. In the absence of UI, however, people are eligible for unemployment assistance (UA). Thus the value of extended benefits is approximately $2.5w\rho(1-UA/UI)$, where ρ is the replacement rate of regular UI benefits and UA/UI represents the ratio of UA benefits to UI benefits. The statutory replacement rate for UI benefits is 55%. However, most workers receive supplementary UI benefits for their dependents: on average we estimate that this raises the replacement rate to 64%. Offsetting this is the fact that workers in Austria receive 14 "monthly" salaries per year 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 the formula $UA = 0.92UI - F + C$, where F represents other family member's earnings and C represents dependent allowances. Data from the 2004 Survey of Income and Living Conditions show that the average wage earner in Austria between the ages of 20 and 49 contributed just under one-half of his/her family income. Based on this, we assume that F is approximately equal to w for a typical worker in our sample. Dependent allowances were 423 Euros per month for a partner and 213 Euros per month for each dependent child in 2000. Assuming that a typical job loser has a partner and 2 children and a net wage of 1200 Euros per month, we therefore estimate that $UA/UI = 0.38$. Thus, we estimate that the value of extended benefits is $2.5w(0.55)(1 - 0.38) = 0.85w$.

C. Construction of Weights (Column 4, Table 2)

To generate weights to make the sample of job-losers look like the population of workers in Austria, we use a random sample of all wage earners in 1994 from the social security records (see column 1, Table 1 for summary statistics for this sample). Using 3 age groups (20-29, 30-39, 40-49), 5 wage quintiles, two groups for sex, nationality, and worker type (blue collar/white collar), we generate 120 categories in the 1994 employed workers sample. Let p_e denote the fraction of workers in category e.

We then apply same categorization to the sample of job-losers. To control for wage growth over 1980-2002, we inflate the quintile cutoffs from the 1994 nominal wage distribution by the nominal wage growth rate from aggregate statistics. Note that this procedure ignores changes over time in female labor force participation and the share of immigrants. We also disregard differences in wage growth across the distribution. Let p_u denote the fraction of observations in each category in our analysis sample of job losers. Finally, we weight each observation by $w_i = \frac{p_e}{p_u}$ and re-estimate the hazard model in column (2) of Table 2.

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TABLE 1

Summary Statistics for Austrian Workers, Job Losers, and Estimation Sample

	All Workers (1994) (1)	Job Losers (1981-2001) All (2)	Job Losers (1981-2001) Estimation Sample (3)
Worker Characteristics:			
Age in Years	33.4	33.3	31.2
Female (%)	43.0	42.0	51.5
Post-compulsory Schooling (%)	--	59.4	59.7
Married (%)	--	37.8	43.4
Austrian Citizen (%)	90.5	88.7	88.0
Blue Collar Occupation (%)	49.1	64.2	57.5
Previous Job/Employment:			
Months of Tenure	--	44.4	25.6
Months Worked in Past 5 Years	--	47.0	41.1
Eligible for Severance Pay (%)	--	38.5	20.8
Eligible for Extended UI (%)	--	78.4	66.4
Previous Wage (Real Euros/yr)	22,096.0	18,782.0	17,033.7
Wage Top-Coded (%)	5.5	2.6	2.0
Number of Employees at Firm	--	278.7	299.5
Post-Layoff:			
Mean Duration of Nonemployment (months)	--	14.5	16.9
Median Duration of Nonemployment (months	--	3.5	4.3
Nonemployed < 20 Weeks (%)	--	58.4	51.5
Nonemployed < 52 Weeks (%)	--	81.4	76.9
Observed in New Job (%)	--	93.5	92.4
Among those with New Job:			
Mean Duration of Nonemployment	--	7.4	9.0
Change in Log Wage (×100)	--	-5.5	-3.4
Std. Dev. of Change Log Wage (×100)	--	46.0	50.7
Sample Size	37,738	1,379,730	650,922

NOTE--Table entries are means unless otherwise noted. Column 1 is based on random sample of all workers between the ages of 20-50 in 1994. Column 2 includes individuals losing a job in the private sector over the period 1980-2001 who are between age 20-50, worked at their previous firm for more than 1 year, and took up UI benefits within 28 days of job loss (eliminating quitters). Sample in column 3 further eliminates job losers from construction, those who returned to their previous employer, or those who worked for more than 5 years at their previous firm. Wages expressed in real (year 2000) Euros. Nonemployment duration is time from end of lost job to start of next job.

TABLE 2

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

	No controls	Basic controls	Full controls	Full samp. reweighted	≥4 layoffs by firm
	(1)	(2)	(3)	(4)	(5)
Severance pay	-0.125	-0.115	-0.094	-0.119	-0.132
	(0.017)	(0.018)	(0.019)	(0.021)	(0.058)
Extended benefits	-0.093	-0.064	-0.064	-0.064	-0.079
	(0.016)	(0.017)	(0.018)	(0.019)	(0.058)
Sample size	650,922	565,835	509,355	565,835	48,390

NOTE--All specifications report estimates of Cox hazard models specified in equation (14). Nonemployment durations are censored at twenty weeks; hence, coefficient estimates can be interpreted as percent change in average job finding hazard over first twenty weeks of the spell. All specifications include cubic polynomials for both job tenure and months worked interacted with severance pay and EB indicators. Specifications 1-4 are estimated on the full sample, defined in notes to Table 1. Specification 5 is estimated on the subsample of individuals who were laid off from a firm that laid off four or more workers within one month. Specifications 2, 4 and 5 include the following covariates: gender, marital status, Austrian nationality, "blue collar" occupation indicator, age and its square, log previous wage and its square, and dummies for month and year of job termination.

Specification 3 adds the following covariates to those used in specification 2: total number of employees at firm from which the work was laid off, total years of work experience and its square, indicator for having a job before the one just lost, the duration of the job before the one just lost, "blue collar" status at job prior to the one lost, a dummy for being recalled to the job before the one just lost, indicator for having a prior spell of nonemployment, the last nonemployment duration before the current spell, total number of spells of nonemployment in career, and dummies for education, industry, and region of job loss. Standard errors clustered by individual (to correct for correlation in errors across spells within person) shown in parentheses.

TABLE 3

Effects of Severance Pay and Extended Benefits on Match Quality

	Dep. Variable: Change in Log Wage		Dep. Variable: Duration of Next Job	
	No controls	Full controls	No controls	Full controls
	(1)	(2)	(3)	(4)
Severance pay	-0.009	-0.002	-0.017	0.000
	(0.007)	(0.006)	(0.013)	(0.015)
Extended benefits	-0.005	-0.008	-0.005	0.007
	(0.006)	(0.006)	(0.012)	(0.013)
Sample size	553,607	445,926	601,152	476,307

NOTE--All specifications include cubic polynomials for job tenure and months worked interacted with severance pay and EB indicators. All specifications are estimated on the full sample of workers who find a new job before the sample ends. Columns 1 and 2 report coefficients from OLS regressions of change in log wage from last year of lost job to first year of next job. Columns 3 and 4 report coefficient estimates from Cox hazard model for duration of next job, censored at five years. Coefficient estimates in columns 3 and 4 can be interpreted as average change in job leaving hazard over first five years of next job. Specifications 1 and 3 include no additional controls; specifications 2 and 4 include full control set used in specification 3 of Table 2 (see notes to Table 2 for the list) Standard errors clustered by individual shown in parentheses.

TABLE 4

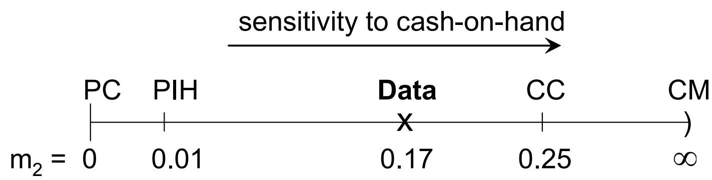
Calibration Results vs. Empirical Estimates of Sample Moment m_2

		Coefficient of Relative Risk Aversion:		
	1.0	2.0	3.0	4.0
A. Credit-Constraint Model				
Discount Rate:				
5%	0.176	**0.248**	0.259	0.242
10%	0.182	0.253	0.264	0.247
15%	0.186	0.258	0.269	0.251
30%	0.197	0.272	0.284	0.266
B. PIH Model with Unrestricted Borrowing				
Discount Rate (=Interest Rate):				
5%	0.007	**0.013**	0.020	0.027
10%	0.014	0.027	0.041	0.054
15%	0.021	0.042	0.062	0.082
30%	0.044	0.088	0.131	0.173
C. Empirical Estimates				
	Point Estimate	Std. Error		
No Controls:	**0.174**	0.041		
Full Controls:	0.192	0.071		

NOTE--Entries in Panel A are implied values of the moment m_2 from a model with consumption equal to current income, with values for the annual discount rate and coefficient of relative risk aversion as shown. Entries in Panel B are upper bounds on m_2 from a simple PIH model with rate of time discount set equal to the interest rate. See text for formulas and additional assumptions used to calculate these numbers. Panel C shows empirical estimates of m_2 using hazard model estimates from Column 1 (no controls) and Column 3 (full controls) of Table 2. Standard errors are calculated using delta method. Values in bold correspond to those shown in Figure 1.

Figure 1

Dynamic Models Ordered by Sensitivity to Cash-on-Hand



PC. Perfect consumption smoothing

PIH. Simple PIH with unrestricted borrowing and lending

Data. Empirical estimate of m_2 using Austrian data

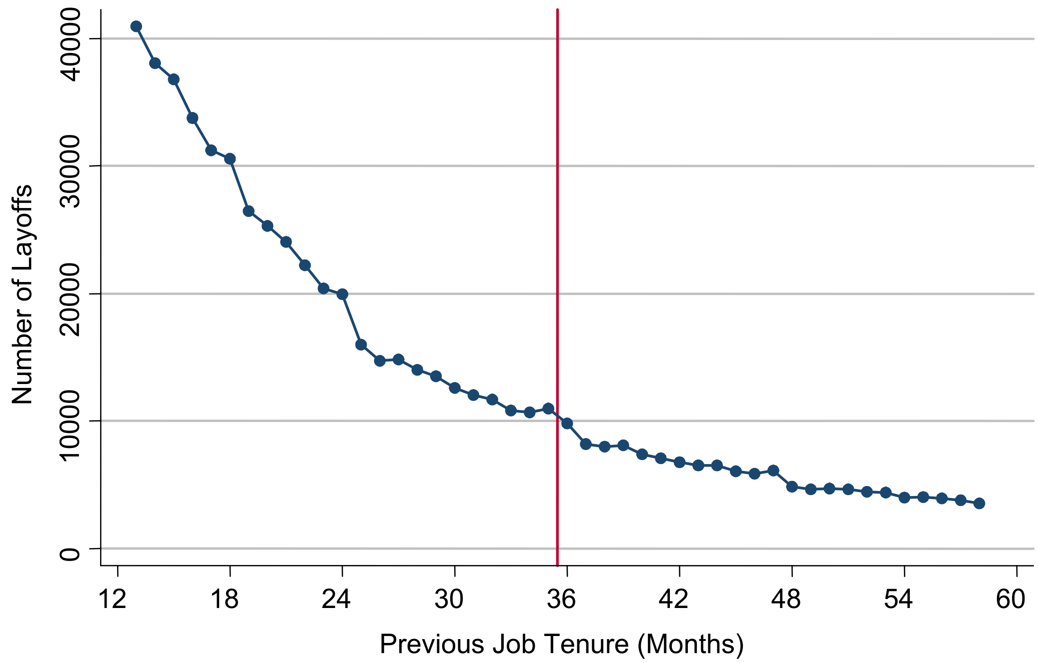
CC. Credit constrained: binding asset limit but forward looking

CM. Complete myopia "rule of thumb" with consumption = income

NOTE–This figure orders a set of intertemporal models by their predicted values of the moment $m_2 \equiv \frac{\partial s_0^*/\partial A_0}{\frac{1}{p_2^*}\partial s_0^*/\partial b_2}$, a normalized measure of sensitivity to cash-on-hand (see section II for details). The values of m_2 shown for the PIH and CC models are calculated in section VII, and assume a coefficient of relative risk aversion of 2. See Table 4 for calibrated values of m_2 for the PIH and CC models under alternative assumptions. The empirical value of m_2 from the data is based on the hazard model estimates in column 1 of Table 2; see section VII for details.

Figure 2

Frequency of Layoffs by Job Tenure



NOTE–In this figure, individuals in the analysis sample are grouped into “tenure-month” categories based on the number of whole months they worked at the firm from which they were laid off. The figure plots the frequency of layoffs by tenure-month category, i.e. the total number of individuals in the sample within each tenure-month category. The vertical line denotes the cutoff for severance pay eligibility.

Figure 3a

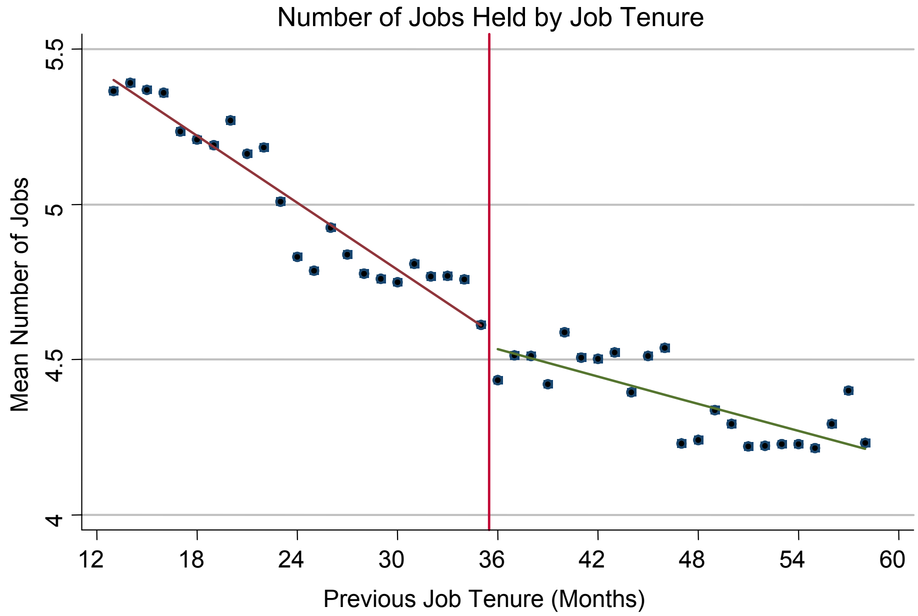
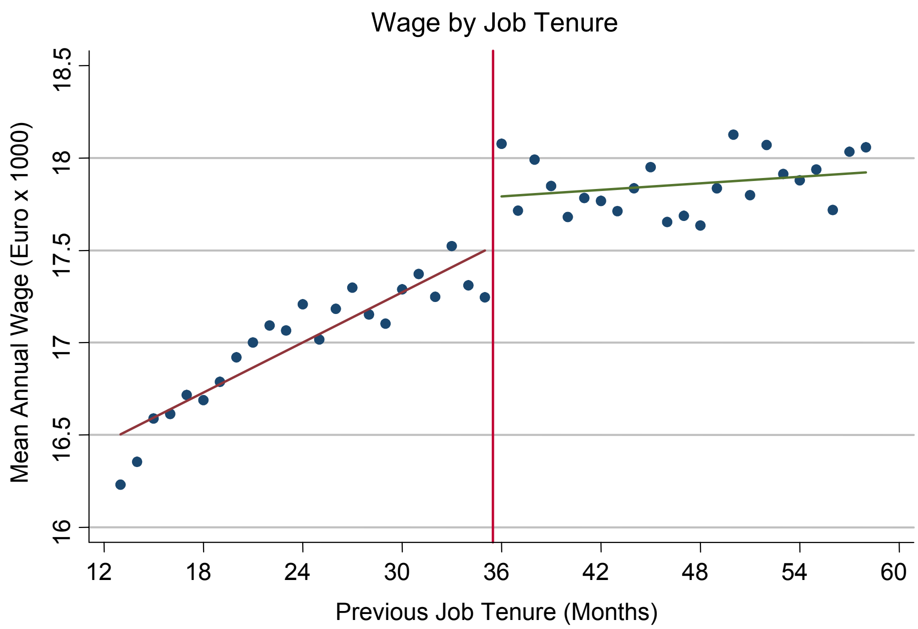


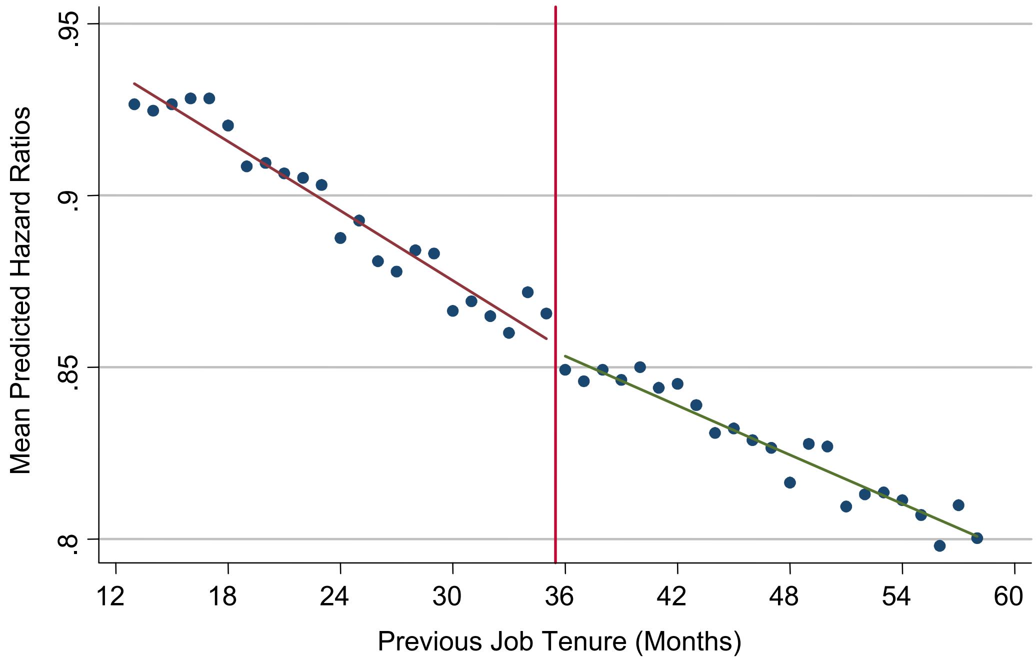
Figure 3b



NOTE–Figures 3a-b show how observable characteristics evolve around the severance pay eligibility threshold. Figure 3a plots the average number of previous jobs (number of continuous employment spells since the start of the data) held by job losers in each tenure-month category. Figure 3b plots the average annual wage in the final year of the job from which the individual was laid off.

Figure 4

Selection on Observables



NOTE–This figure plots average predicted hazard ratios by tenure-month category. The hazards are predicted using a Cox model with the following set of covariates: gender, marital status, Austrian nationality, "blue collar" occupation indicator, age and its square, log previous wage and its square, dummies for month and year of job termination, total number of employees at firm from which the work was laid off, total years of work experience and its square, indicator for having a job before the one just lost, the duration of the job before the one just lost, "blue collar" status at job prior to the one lost, a dummy for being recalled to the job before the one just lost, indicator for having a prior spell of nonemployment, the last nonemployment duration before the current spell, total number of spells of nonemployment in career, and dummies for education, industry, and region of job loss.

Figure 5

Effect of Severance Pay on Nonemployment Durations

NOTE–This figure plots average nonemployment durations (time to next job) in each tenure-month category using the full sample. Observations with nonemployment durations of more than two years are excluded. The vertical line denotes the cutoff for severance pay eligibility.

Figure 6a

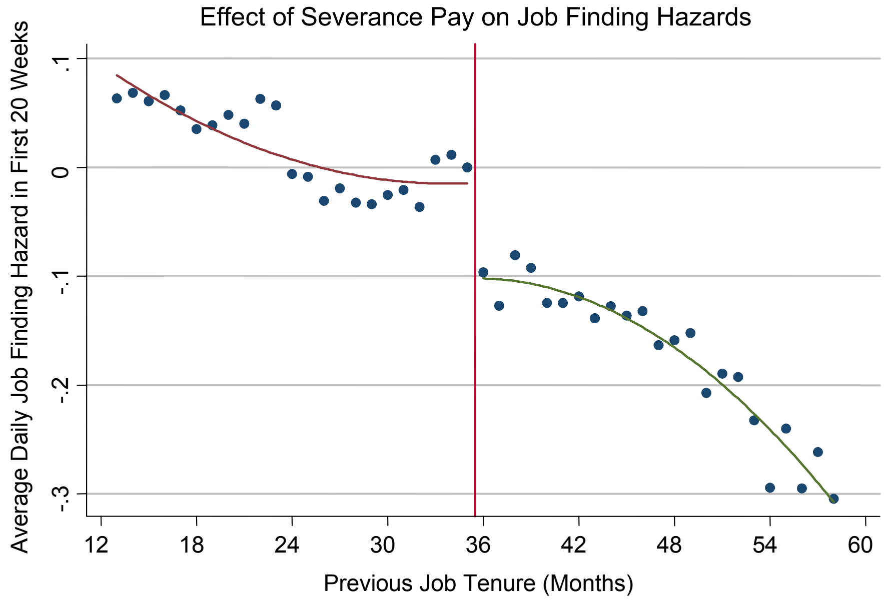
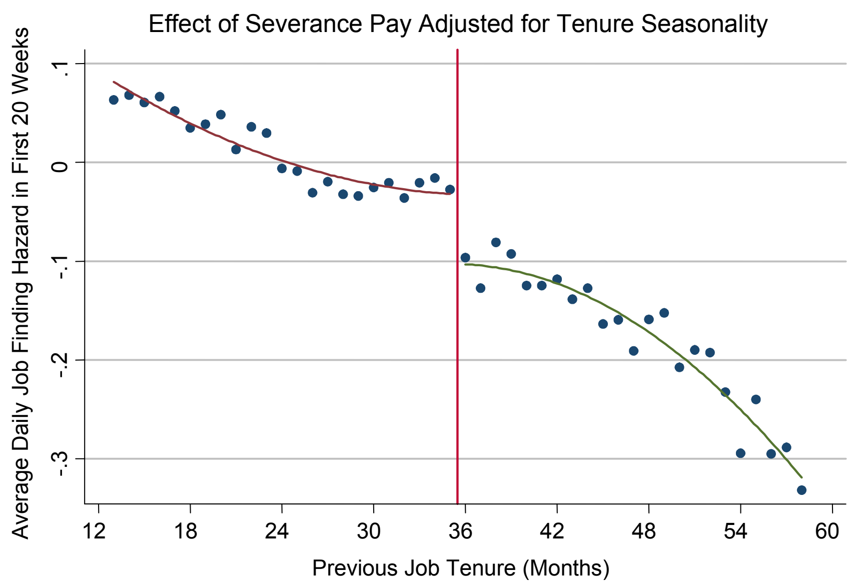


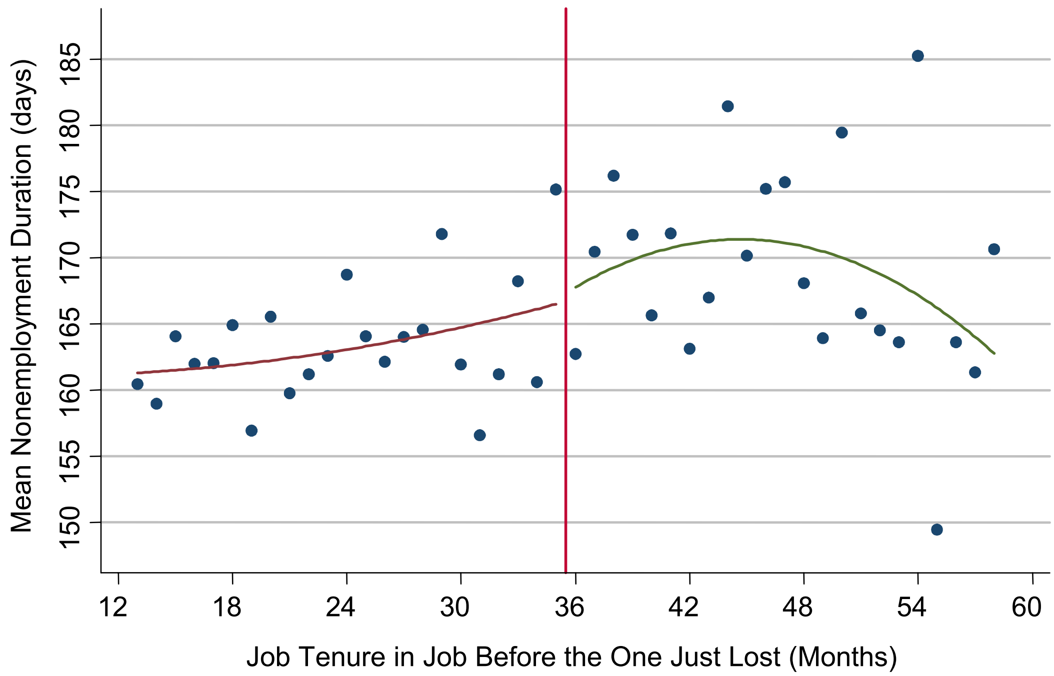
Figure 6b



NOTE–Figures 6a-b plot the θ_{JT} coefficients from the Cox proportional hazards regression specified in equation (13). The values can be interpreted as the percentage difference in the average job finding hazard during the first twenty weeks after job loss between each tenure-month group and tenure-month group 35. For example, Figure 6a shows that the average hazard among individuals laid off with 36 months of job tenure is 10% below that of individuals laid off with 35 months of job tenure. Figure 6a plots the coefficients from a regression that controls only for the EB effect. Figure 6b corrects for the “tenure seasonality” pattern visible in Figure 6a as described in the text.

Figure 7

Placebo Test: Lagged Job Tenure and Nonemployment Durations



NOTE–The sample for this figure includes all individuals who have two or more unemployment spells in the data. Individuals are grouped into categories based on the number of whole months they worked at the firm *prior* to the one from which they were most recently laid off. The figures plots mean nonemployment durations (time to next job) in the current spell by tenure at the prior firm. The figure excludes observations with nonemployment durations of more than two years and ignores censoring. The vertical line denotes completion of three years of service at the prior firm. Note that the fraction receiving severance pay in the current spell evolves smoothly through this cutoff.

Figure 8a

Effect of Severance Pay on Job Finding Hazards by Week

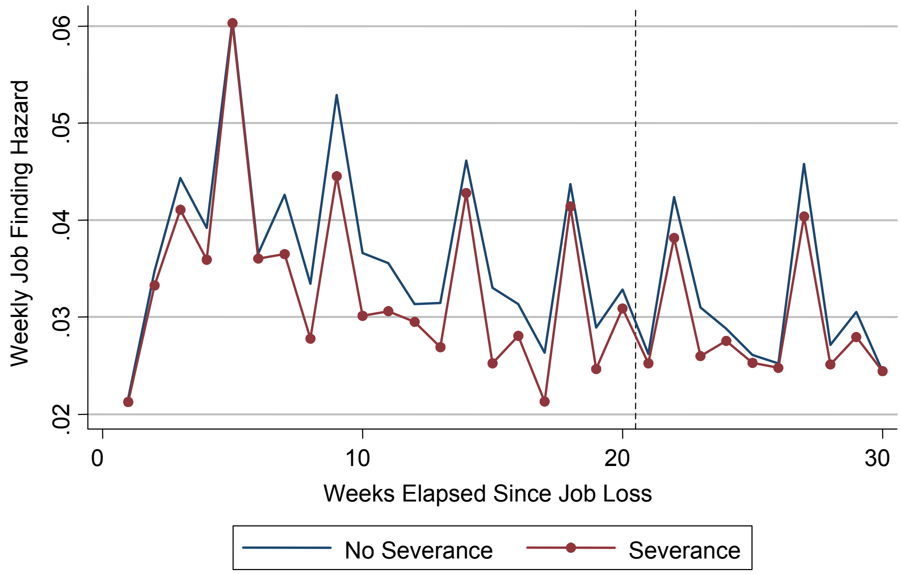
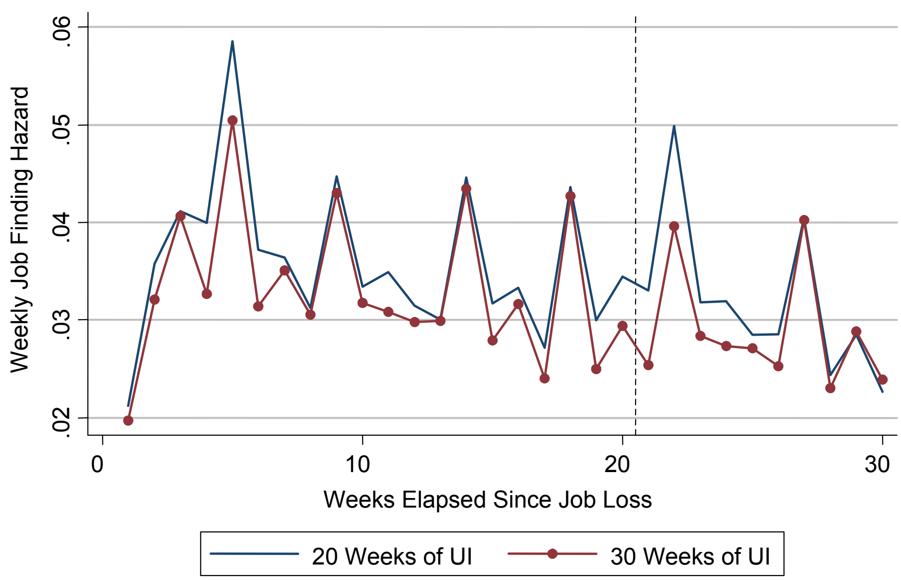


Figure 8b

Effect of Extended Benefits on Job Finding Hazards by Week



NOTE–These figures plot average weekly job finding hazards in the “restricted” subsample of individuals with at least one month of work at another employer in the past 5 years. In Figure 8a, individuals in the “no severance” group are those laid off with between 33 and 35 whole months of job tenure; individuals in the “severance” group have between 36 and 38 whole months of job tenure . In Figure 8b, individuals in the “20 weeks of UI” group have worked for between 33 and 35 whole months in the past five years; individuals in the “30 weeks of UI” group have between 36 and 38 months worked. The dashed vertical line denotes the point at which the UI benefit extension applies (20 weeks).

Figure 9a

Effect of Benefit Extension on Nonemployment Durations

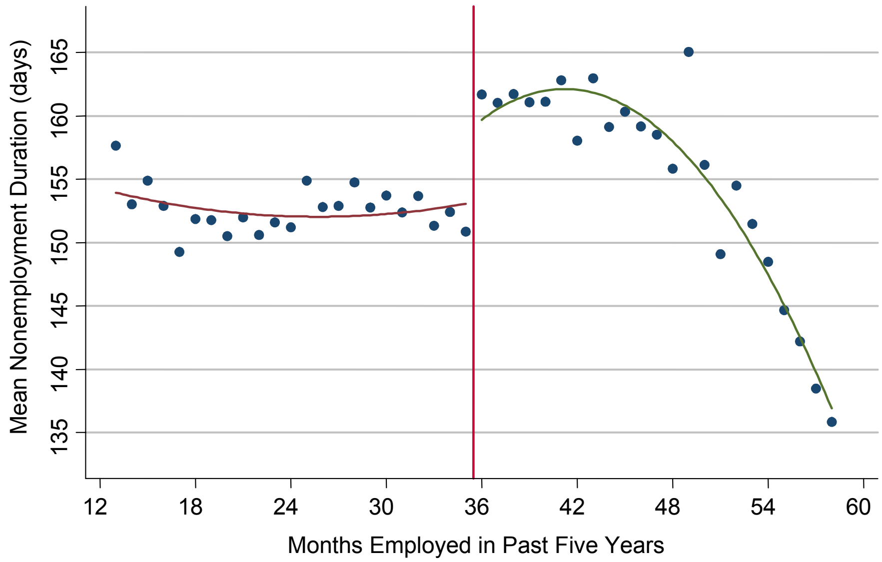
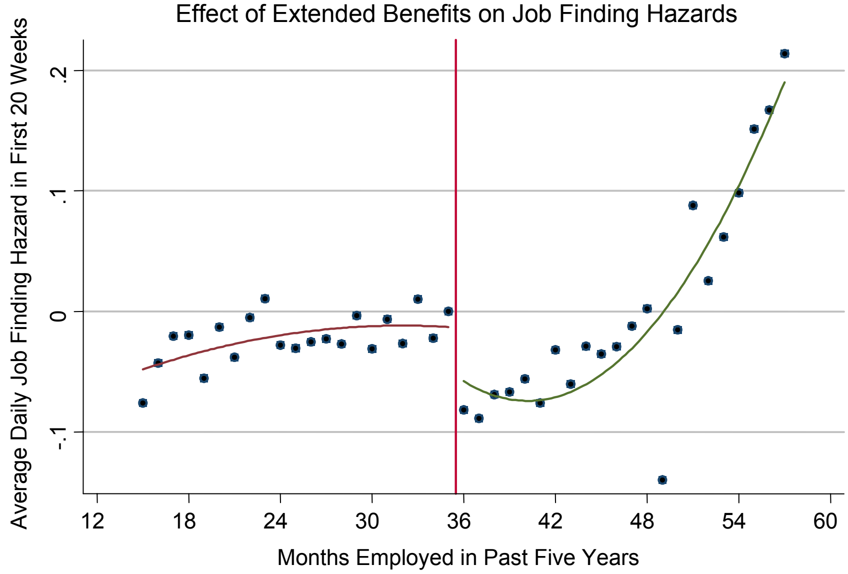


Figure 9b

Effect of Extended Benefits on Job Finding Hazards



NOTE–In Figures 9a-b, individuals are grouped into "months-employed" categories based on the number of whole months they worked at any firm within the past five years. Figure 9a plots mean nonemployment durations, excluding observations with nonemployment durations of more than two years. Figure 9b plots coefficients from a Cox model analogous to that used in Figure 6a, controlling for the severance pay effect using a cubic polynomial. The values plotted can be interpreted as the percentage difference in the average job finding hazard during the first twenty weeks of the spell between each months-worked group and the group with months-worked equal to 35.

Figure 10a

Effect of Severance Pay on Subsequent Wages

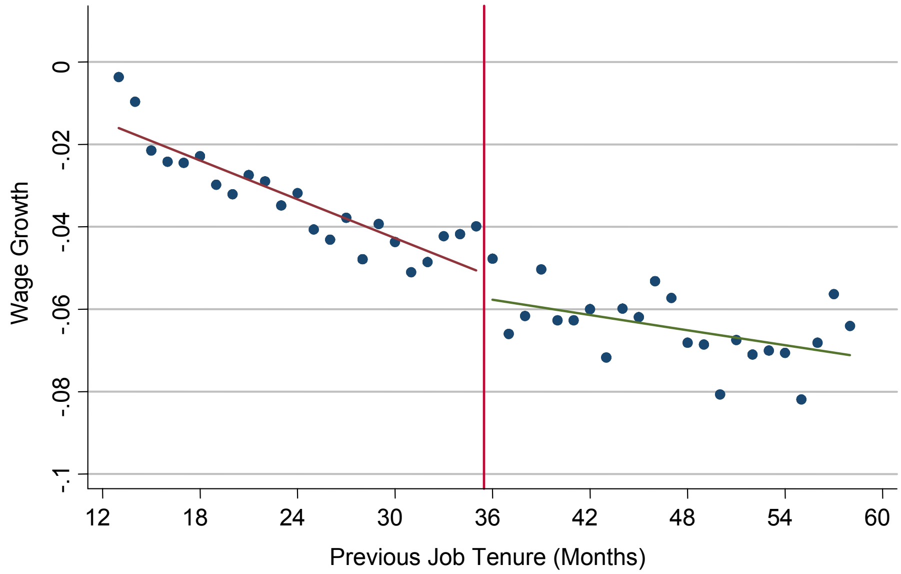
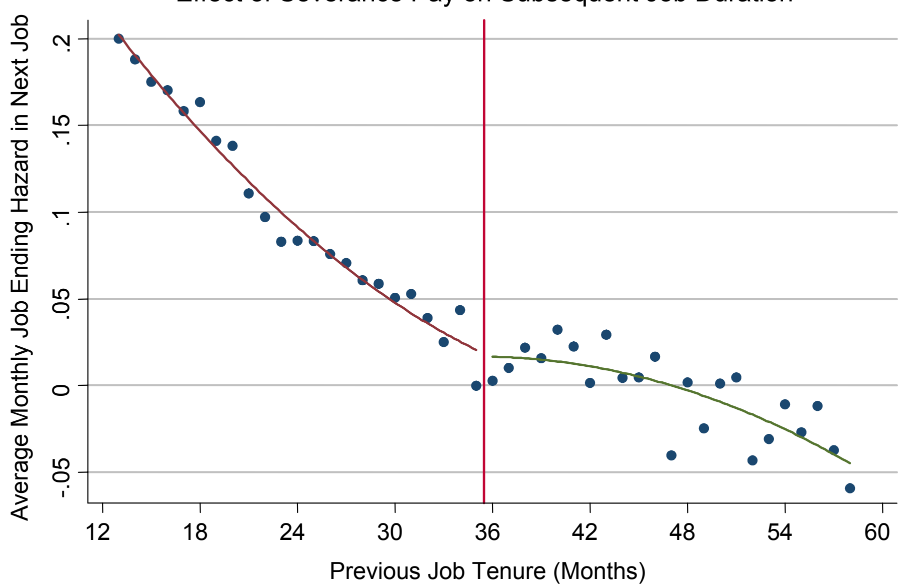


Figure 10b

Effect of Severance Pay on Subsequent Job Duration



NOTE–Figure 10a plots average wage growth (difference in log annual wage between next job and the job from which the individual was laid off) in each tenure-month group. Figure 10b plots coefficients from a Cox proportional hazards model for the duration of the next job with dummies for each job tenure category. The values can be interpreted as the percentage difference in the average job leaving hazard during the first five years of the next job between each job tenure group and the group with job tenure equal to 35. The sample for both figures includes all individuals in the full sample observed in a new job.