CASH-ON-HAND AND COMPETING MODELS OF INTERTEMPORAL BEHAVIOR: NEW EVIDENCE FROM THE LABOR MARKET*

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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] There is still no firm consensus on the extent to which individuals 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.

This paper provides 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. Our analysis is conceptually similar to existing studies of sensitivity to cash-on-hand. We simply use a different measure of "consumption" – search intensity instead of purchased goods. The sensitivity of search intensity to cash-on-hand distinguishes between the permanent income hypothesis (PIH) and other dynamic models in the same way as the sensitivity of consumption. Indeed, in a simple job search model 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 Austrian labor market allow a sharper research design. Eligibility for severance pay is based on a discontinuous rule: people with 3 or more years of job tenure are eligible, whereas those with shorter tenures are not. In addition, administrative wage

[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].

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 estimate the effects of cash grants on subsequent job quality. The size of these effects is an important unresolved issue of independent interest in the job search literature.[4]

We use a regression discontinuity (RD) design to estimate the effects of severance pay, essentially comparing the search behavior of people who were laid off just before and just after the 36 month cutoff for severance pay 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 eligibility threshold. We evaluate this possibility in three ways: by testing whether the frequency of layoffs and the observable characteristics of job losers evolve smoothly through the discontinuity, by focusing on subsamples where selective firing is less plausible (such as group layoffs), and by 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. The absence of selective layoffs is consistent with relatively strict firing regulations in Austria and laws prohibiting 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 unemployment (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 can smooth consumption perfectly. Second, using a parallel analysis of a discontinuity in the unemployment insurance (UI) benefit system, we find that job

[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.

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 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.

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 implied by 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 I). 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, who are typically younger and less skilled than non-job-losers. Reweighting our sample to match the observable characteristics of the overall Austrian population leads to estimates that are very close to our basic estimates. Although this suggests that a more representative sample would exhibit similar intertemporal behavior, our conclusions are necessarily based on the behavior of people 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

[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.

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 alternative 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] and Lalive, van Ours, and Zweimuller [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 liquidity effect. This implies that the efficiency cost of temporary income support programs such as UI is 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 frame our empirical analysis. 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 three key 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. Let w_t denote the wage rate in period t; we take the path of wages $\{w_t\}_{t=1,...T}$ as exogenous.[6]

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 period t.[7] 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

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

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,

[6] In practice, the wage rate in a given period t is likely to depend on the date at which the individual began at that job. Allowing for wages to depend on job tenure complicates the algebra but does not affect our results.

[7] A more conventional timing assumption in search models without savings is that search in period t leads to a job that begins in period $t + 1$. Assuming that search in period t leads to a job in period t itself simplifies the analytic expressions for $\frac{\partial s_t}{\partial A_t}$, as in Lentz and Tranaes (2005).

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 varies with the difference in marginal utilities between employed and unemployed states, which in turn depends on 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 effects search intensity 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 UI, as shown by Chetty [2006b]. To see this, note that

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

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 (or "liquidity") effect and a price (or substitution) effect The liquidity effect reflects a welfare-improving response to the correction of a market failure, whereas the substitution effects represents a "moral hazard" response to the price distortion induced by subsidizing unemployment. By combining estimates of $\partial s_t^*/\partial b_t$ and $\partial s_t^*/\partial A_t$, one can infer the welfare gain from raising the unemployment insurance benefit level [Chetty 2006b].

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 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 expected present 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 I shows the predicted values for m_2 – the moment we calculate in our empirical analysis – for a range 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 the next four sections of the paper, we develop an empirical estimate of m_2 using data for a sample of job losers in Austria. In Section VII, we return to our theoretical framework and compare the estimate with the values of m_2 predicted by the PIH and credit-constraint models.

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. Firms outside the construction industry are required to pay individuals who are laid off after 3 years of service a lump sum severance payment equal to 2 months of their salary.[8] 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").[9] 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 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.

[8] 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.

[9] 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.

III.A. Data and Sample Definition

We use data from the Austrian social security registry, which covers all workers except civil servants and the self-employed. About 85% of the Austrian workforce is included in the dataset. We consider all job separations that resulted in a UI claim between 1981 and 2001. The register 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.

Although these data allow us to measure severance pay eligibility, we do not have information on actual severance payments. 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]). Likewise, although we can accurately measure EB eligibility, we do not see actual UI payments. As with severance pay, however, we believe the EB rules are closely followed. Consequently, the two program rules create essentially "sharp" discontinuities in eligibility from 0% to 100% [Hahn, Todd, and van der Klaauw 2001].[10]

Starting from the universe of UI 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 special programs 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 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.

[10] As noted above, there are a few individuals in the sample who are eligible for 39 weeks of UI benefits. This fraction evolves smoothly around the EB discontinuity we focus on, and accounts for roughly 3% of the sample on either side of the discontinuity. Consequently, average eligibility for UI benefits rises by exactly 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.

Table I 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.[11] 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 call the duration of "nonemployment."[12] 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.[13] 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, there is substantial dispersion in the wage growth distribution (standard deviation = 51%).[14] This

[11] 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.

[12] 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, van Ours, and Zweimuller 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 IIIa, column 4 in working paper).

[13] 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.

[14] 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.

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, it is plausible that people with different values of job tenure on their previous job (JT) have different expected search durations: $E[\varepsilon|JT] \neq 0$. 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:

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

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 II shows the number of job losers entering unemployment, by months of job tenure.[15] 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 IIIa 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 before and after the cutoff. Figure IIIb 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

[15] 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.

size. The jump in the best-fit lines shown in Figure IIIb is approximately 300 Euros/year, or about 1.6% of the mean wage for people with 35 months of tenure.[16] 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 IV 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 IV. The downward trend indicates that people with longer job tenure have observable characteristics that are associated with longer durations, on average. The predicted hazards are smooth through the 36 month threshold, however, implying that any small discontinuities in the observable characteristics 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 IV 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

[16] 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.

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 IV, 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 IIIb, 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. However, there is a subset of individuals – those whose only job in the past 5 years is the job they just lost – for whom job tenure and work experience are perfectly colinear. Because of this subgroup (which comprises roughly 20% of the sample), the fraction of individuals in the full sample who are eligible for extended benefits jumps from 80% at 35 months of job tenure to 100% at 36 months of tenure. Consequently, any discontinuous change in behavior at 36 months of job tenure is mainly due to severance pay, but includes a small (20 percentage point) effect of extended benefits. A similar issue arises at the threshold for extended benefits eligibility, where there is a 20% jump in the fraction eligible for severance pay. This "double discontinuity" complicates the analysis relative to the standard RD design proposed by Thistlewaite and Campbell [1960].

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 way to separate 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 V 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 V to the effects of severance pay because the fraction of individuals receiving EB also jumps at the cutoff. In Figure VI we adjust for the 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 by including cubic polynomials in months worked, a dummy for extended benefit eligibility, and their interaction:

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

The 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 VI plots the estimated θ_{JT}'s from this model.

Consistent with the results in Figure V, 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 in the full sample.

A potential concern with Figure VI is the yearly cyclical pattern in the hazard rates associated with job tenure. In particular, the estimated hazard rates for people who left their job just prior to the anniversary date of their hiring (i.e., in months 21-23, 33-35, etc.) are 2-3% higher than for those who left in nearby months.[17] To gauge the importance of this seasonality pattern, we estimated a parametric RD model with an "end of tenure year" indicator for exits in the three months just before completion of a full year of service (i.e., after 21-23, 33-35, 45-47, and 57-59 months). We then adjusted for tenure seasonality by subtracting the estimated end-of-tenure-year effect from the associated months' hazards. This seasonality adjustment fully eliminates the potentially worrisome patterns in Figure VI, but does not affect the discontinuity at $JT = 36$ significantly (Figure VIc in the working paper).

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. Figure VII explores how the effect of severance pay varies with the duration of search by plotting average weekly job finding hazards for individuals laid off just before the severance pay threshold (with 33-35 months of tenure) or just after the threshold (with 36-38 months of tenure). To eliminate any double discontinuity effects, the figure is based on data for the "restricted" subsample of individuals with at least one month of work at another employer in the past 5 years. The figure indicates 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 V-VII 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 VIIIa plots the relationship between average nonemployment durations and months worked

[17] 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.

(MW) in the past five years. As in Figure V, 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 VIIIb, we examine how the average hazard rates over the first twenty weeks of the spell vary around the EB discontinuity. We estimate a proportional hazards model analogous to the one used for Figure VI, 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). There is a discontinuous drop of approximately 7% in the average hazard rate at the cutoff for EB eligibility.

In Figure IX, 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 VII, 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 VIIIb) 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.[18] 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

[18] 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.

derivative of the control function to change discontinuously at the eligibility cutoffs:

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

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 VIIIb and IX.

Table II 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 IV (see the notes to Table II for the full list). 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.

As we noted in the discussion of Table 2, job losers are different from the overall population. To evaluate whether our estimates would differ in a sample with characteristics closer to those of the average labor market participant, we reweighted the sample by age, gender, wage, nationality, and occupation to reflect the characteristics of all workers (see the appendix for details on the construction of the weights). We then replicated specification (2) with these weights. As shown in column 4, the coefficient estimates do not change significantly. We also estimated separate models for workers in different wage quartiles and age groups, and by gender, blue collar status, education, region, and time period. Consistent with the evidence from the reweighted sample, the estimates do not vary significantly across these subgroups.[19]

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 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 II, which replicates specification (2) for the subsample of people who were laid off from a firm that fired four or more workers within a single month. See Table IIIb 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 II. 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 II. 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.

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

[19] Chetty [2006b] shows using data from the U.S. that the effect of UI benefits on durations is largest among liquidity constrained groups – e.g. households with low assets prior to job loss. Unfortunately, the Austrian Social Security database does not contain proxies for liquidity such as assets or family circumstances of job losers.

our sample that 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. In practice, we find that current nonemployment durations evolve smoothly through the 36 month cutoff for lagged job tenure (see Figure VII and Table IIIb in the working paper), providing further support for the causal interpretation of our results.

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 Xa 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 Xb. 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 XI 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 III 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 II 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 III 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 feature of the estimates in Table III is their precision. The standard errors in specifications 1 and 2, for example, are small enough that even a 1% improvement in wages caused by either severance pay or EB would be detectable. 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 IVb 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.

There are several possible explanations for the absence of significant match quality effects from severance pay or extended benefits. 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 II, 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 II, 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 IV.

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 $r/(1+r)A$.[20] 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

[20] 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).

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.[21] 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:

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

where s_1^* is the optimal level of search intensity in period 1.[22] 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^*.[23] 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

[21] 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 $A_t \geqslant A_0 - (1+r)\sum_{k=0}^{t-1}(w - b_k)$.

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

[23] 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.

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 IV. 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 IV 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 lower bound to the predictions in Panel B of Table IV, 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 I 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.[24]

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 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

This paper uses methods and data from the labor economics literature to address 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

[24] 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.

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.

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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 II)

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 I 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. We denote the fraction of workers in category e by p_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 II.

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Table I

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 II

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

	No controls (1)	Basic controls (2)	Full controls (3)	Full samp. reweighted (4)	≥4 layoffs by firm (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 I. 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 III

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 II (see notes to Table II for the list) Standard errors clustered by individual shown in parentheses.

Table IV

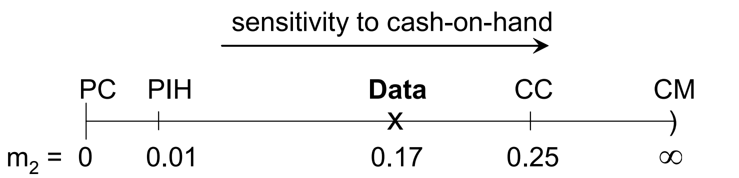
Calibration Results vs. Empirical Estimates of Sample Moment m_2

	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 II. Standard errors are calculated using delta method. Values in bold correspond to those shown in Figure I.

Figure I

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

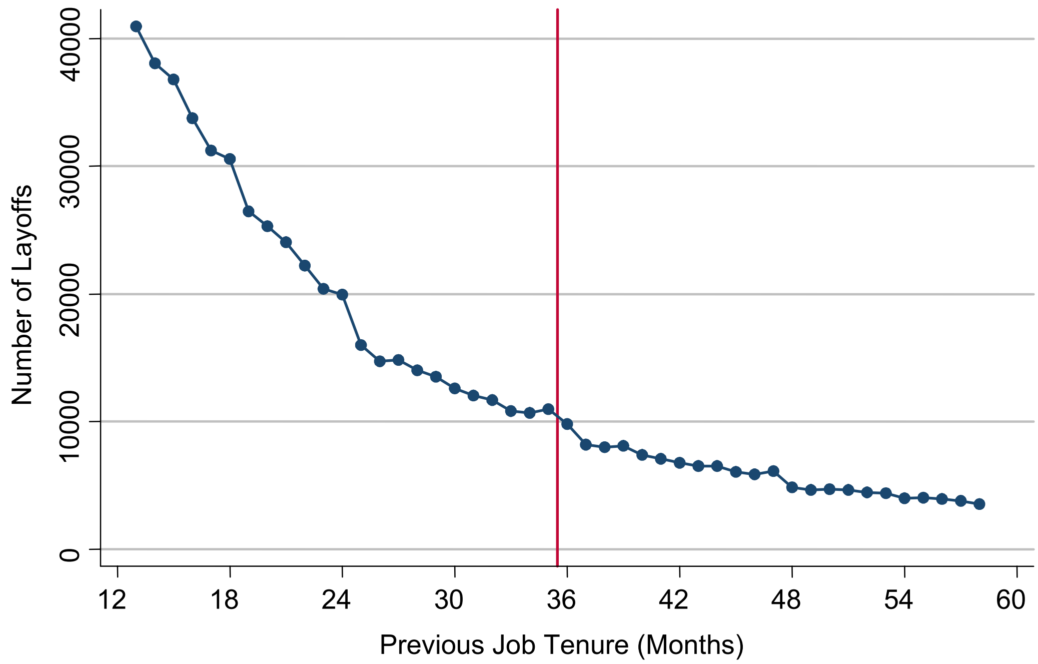
CC. Credit constrained: binding asset limit but forward looking

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 IV 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 II; see section VII for details.

Figure II

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 IIIa

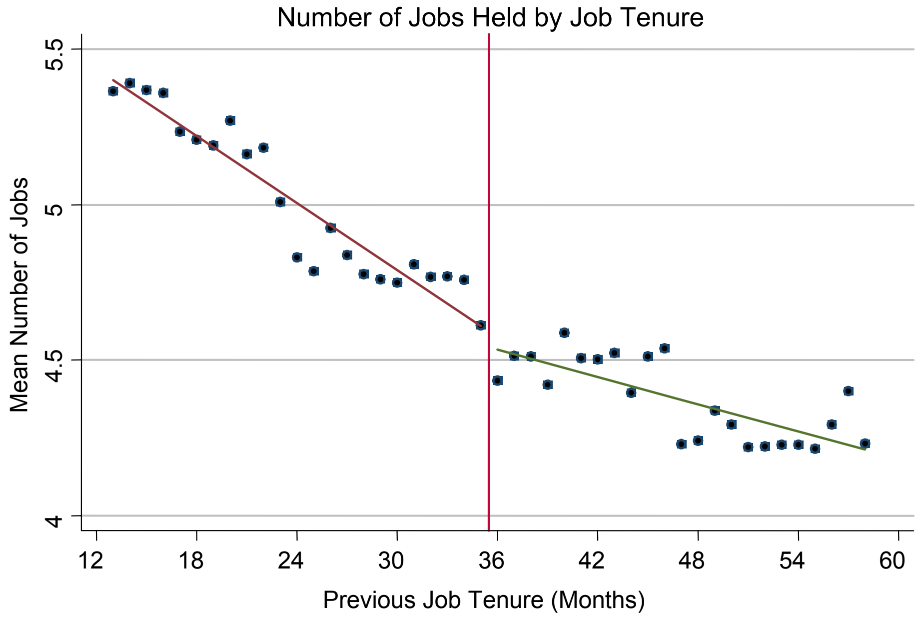
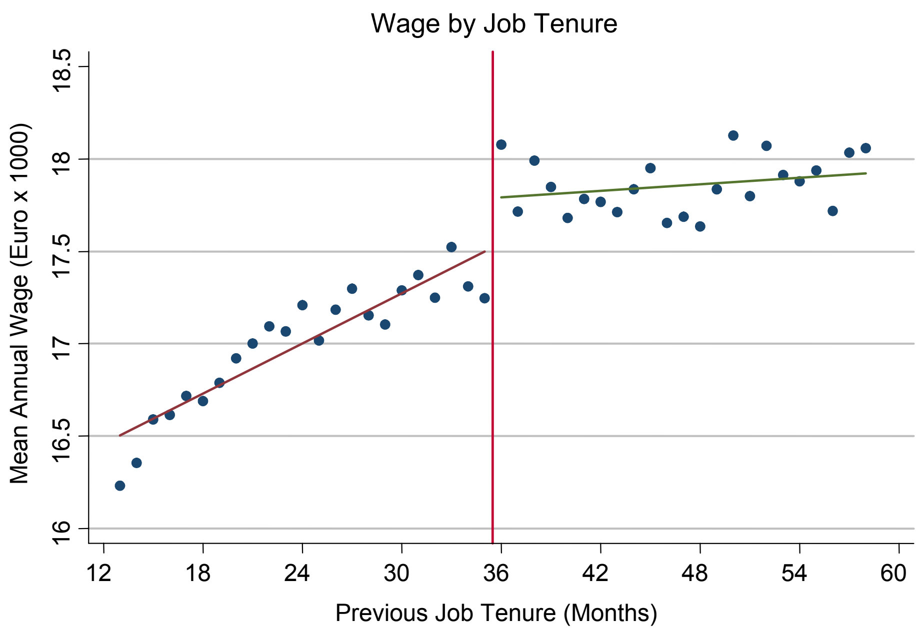
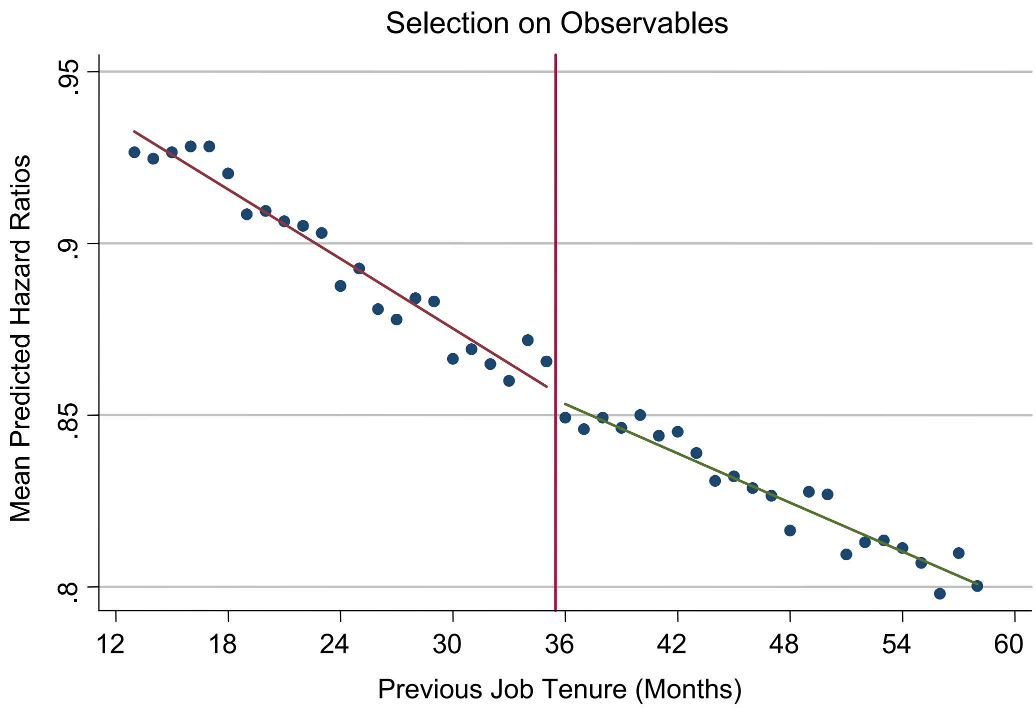


Figure IIIb



NOTE–These figures show how observable characteristics evolve around the severance pay eligibility threshold. Figure IIIa 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 IIIb plots the average annual wage in the final year of the job from which the individual was laid off.

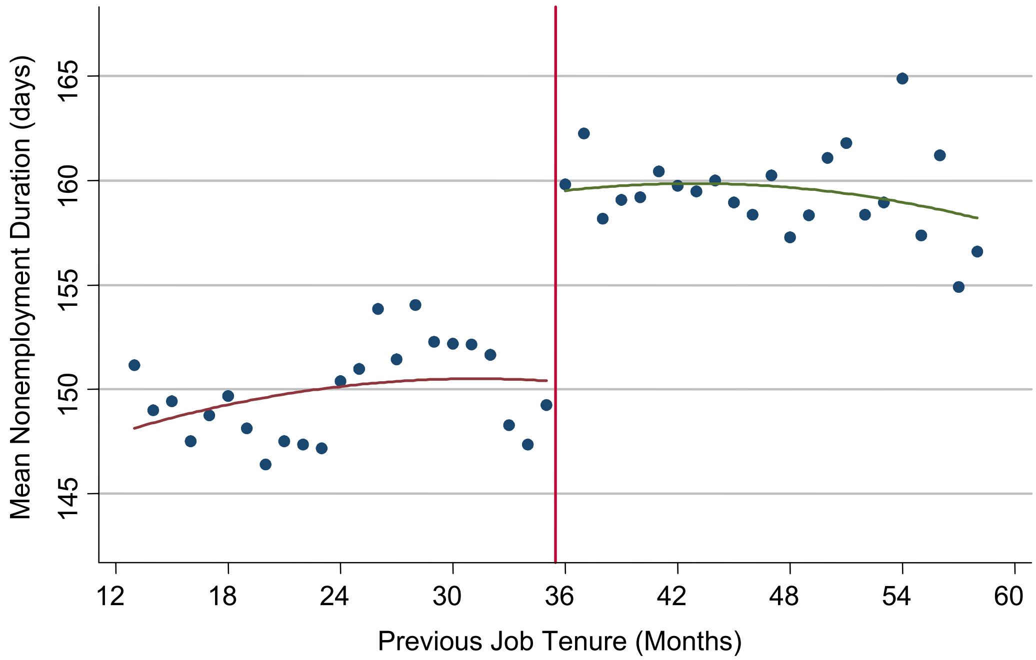
Figure IV



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 V

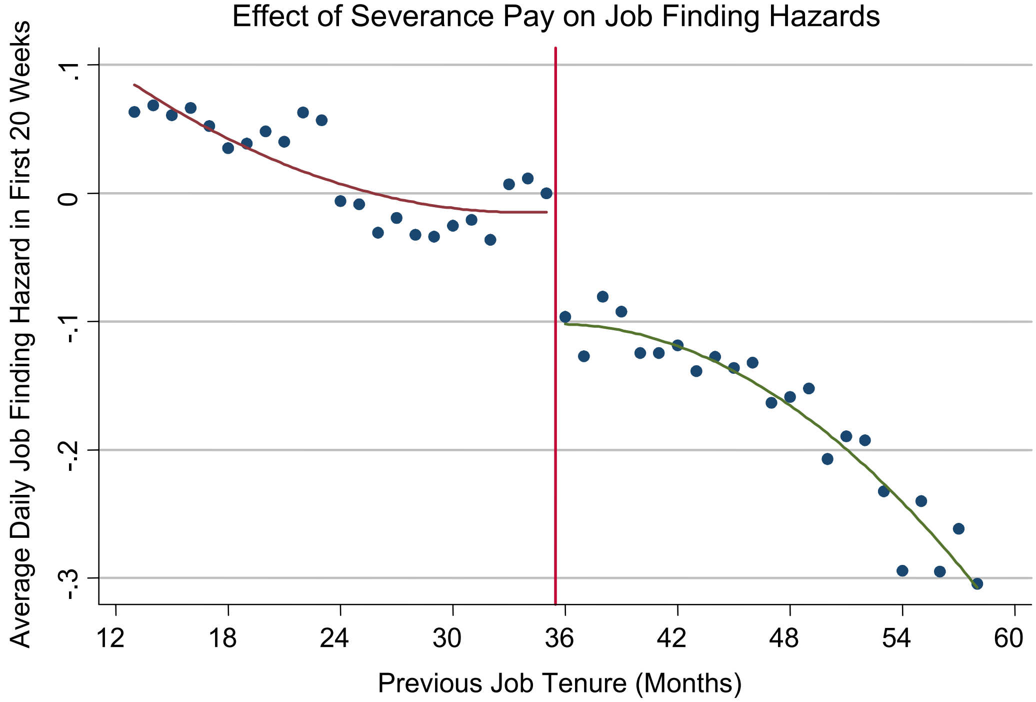
Effect of Severance Pay on Nonemployment Durations



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

Figure VI

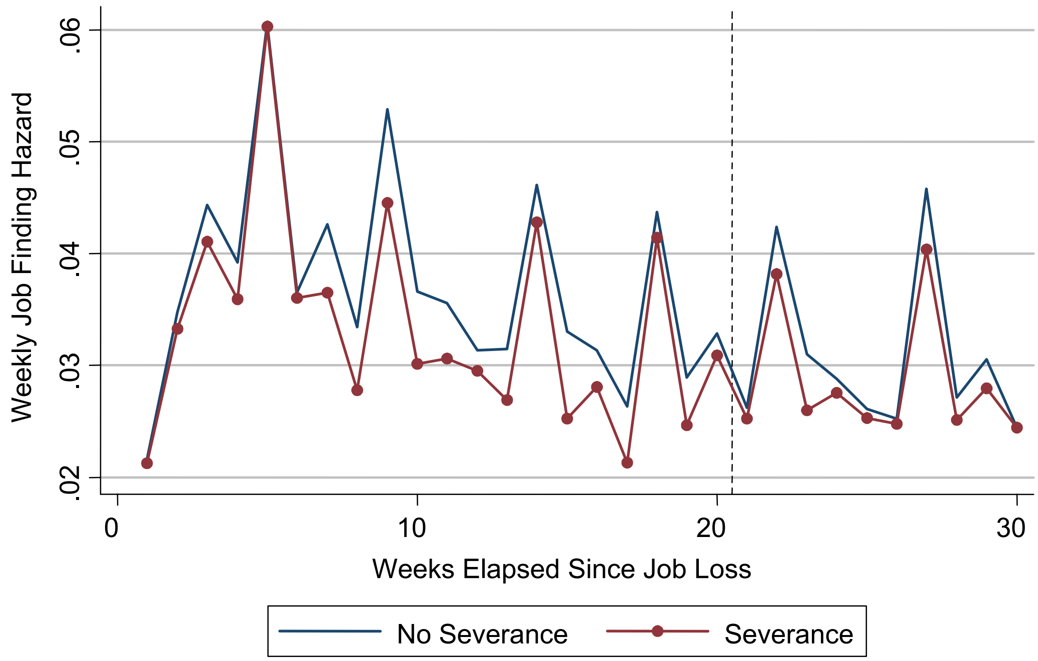
Effect of Severance Pay on Job Finding Hazards



NOTE–This figure plots 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 the group with 35 months of job tenure. For example, 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 VII

Effect of Severance Pay on Job Finding Hazards by Week



NOTE–This figure plots 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. 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. The dashed vertical line denotes the point at which the UI benefit extension applies (20 weeks).

Figure VIIIa

Effect of Benefit Extension on Nonemployment Durations

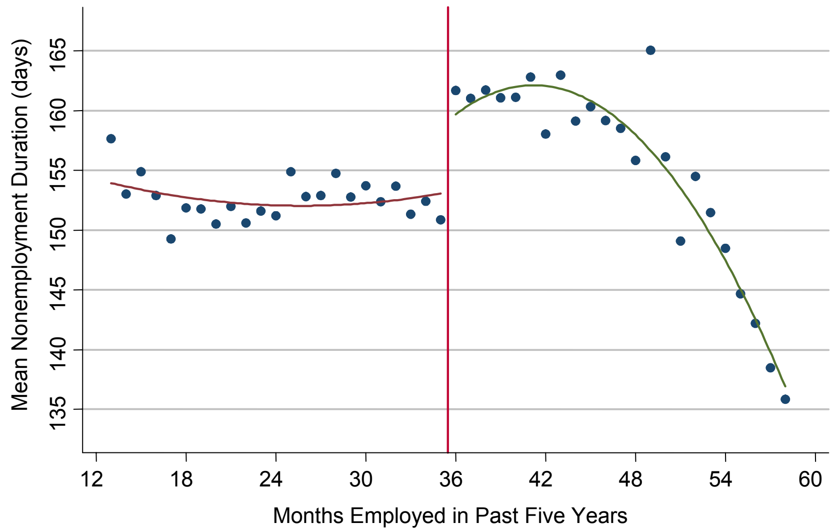
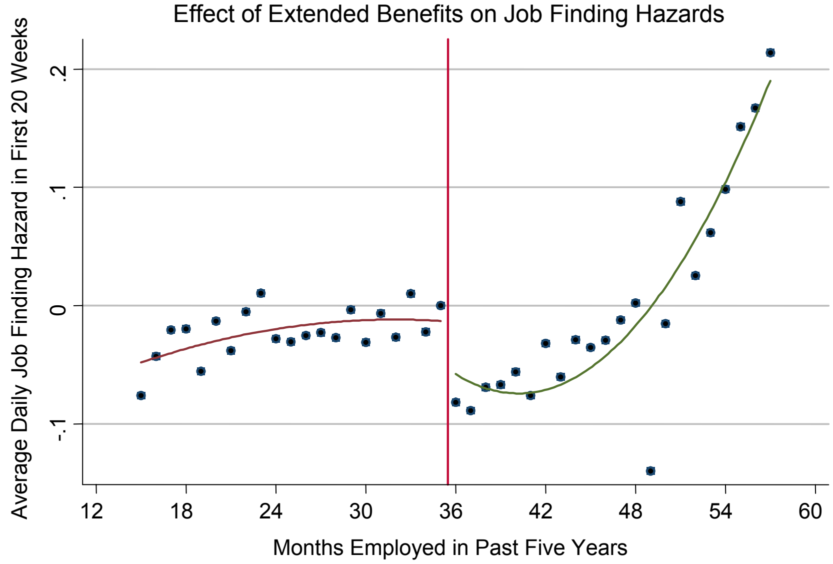


Figure VIIIb

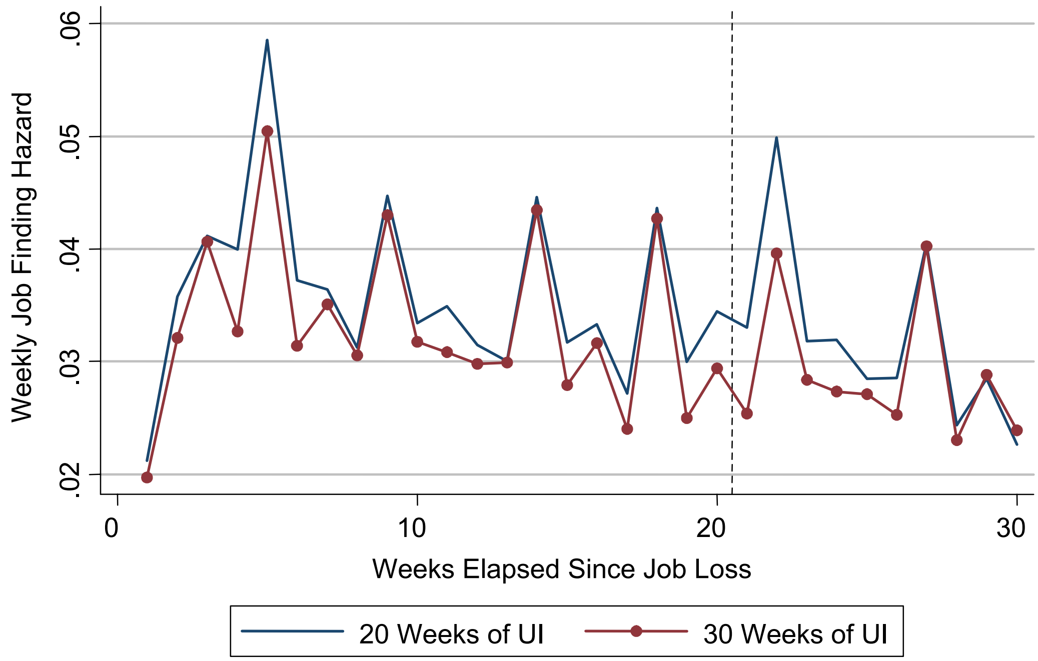
Effect of Extended Benefits on Job Finding Hazards



NOTE–In these figures, 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 VIIIa plots mean nonemployment durations, excluding observations with nonemployment durations of more than two years. Figure VIIIb plots coefficients from a Cox model analogous to that used in Figure VI, 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 IX

Effect of Extended Benefits on Job Finding Hazards by Week



NOTE–This figure plots 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. 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 Xa

Effect of Severance Pay on Subsequent Wages

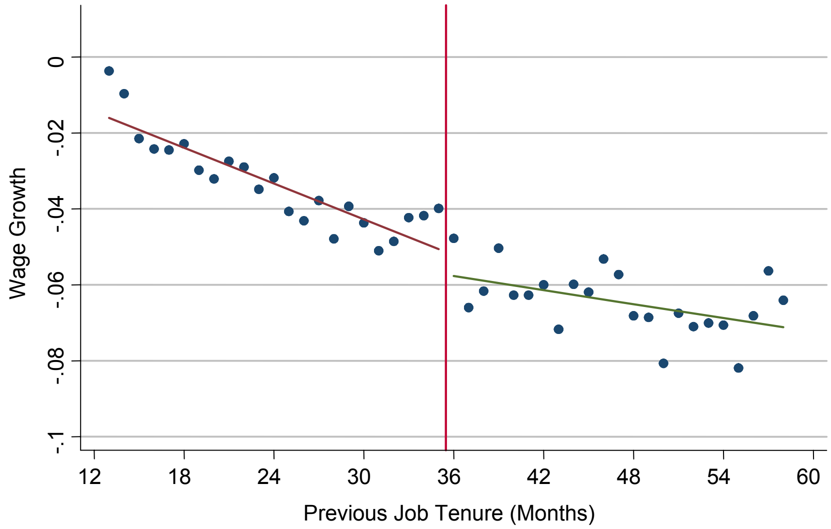
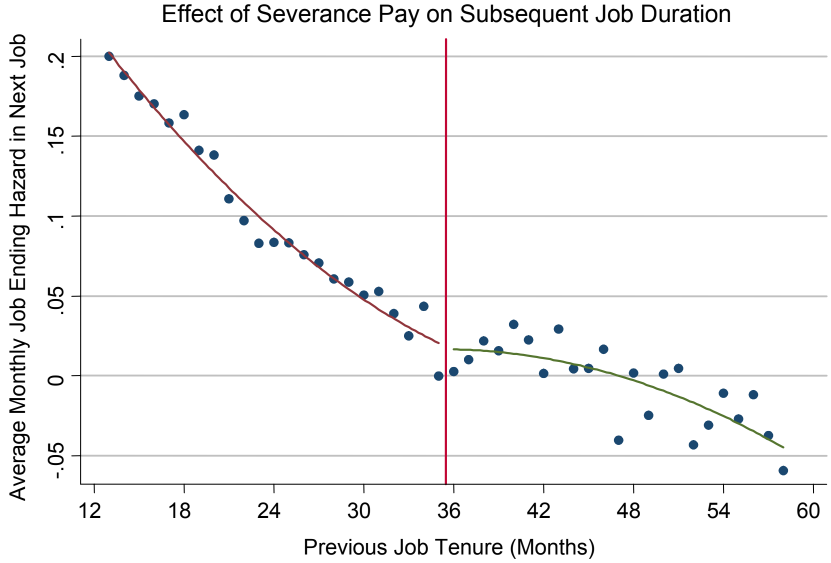


Figure Xb

Effect of Severance Pay on Subsequent Job Duration



NOTE–Figure Xa 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 Xb 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 observed in a new job.