The Effects of Extended Unemployment Insurance Over the Business Cycle: Evidence from Regression Discontinuity Estimates over Twenty Years*

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Abstract: A goal of extending the duration of unemployment insurance (UI) in recessions is to reduce the rate of benefit exhaustion and hence increase coverage. However, such extensions potentially come at the cost of increased nonemployment durations. If UI benefit durations vary with the business cycle, it is very difficult to estimate the effects of this policy because of reverse causality. In this paper, we exploit the fact that the duration of UI benefits in Germany is a function of exact age that is invariant over the cycle. Using the universe of unemployment spells and career histories we implement a regression discontinuity design separately for twenty years and across industries and correlate our estimates with measures of the business cycle. The nonemployment effects of UI extensions we find are at best somewhat declining in large recessions. Yet, the UI exhaustion rate, and therefore the additional coverage provided by UI extensions, rises substantially during a downturn. To help interpret these findings, we derive a new welfare formula in a model of job search with liquidity constraints that links the net social benefits from UI extensions to the exhaustion rate and the disincentive effect of UI.

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1 Introduction

Providing unemployment insurance (UI) benefits is one of the most common policy tools to ease the hardship of job losers in recessions. Despite the widespread existence of UI systems, there is remarkable heterogeneity across countries in how these systems react to the business cycle. While in most countries, including Germany, potential UI benefit durations are constant over the business cycle, in others, in particular the United States, potential UI durations are regularly extended during recessions.[1] Whether or not such countercyclical potential UI durations are socially beneficial is highly debated among economists.[2] One justification for increases in UI durations is that absent the extensions a large fraction of recipients would exhaust benefits and experience significant declines in consumption (e.g., Gruber 1997, Browning and Crossley 2001, Congressional Budget Office 2004). However, a long literature suggests that extensions in UI durations entail a cost in terms of a reduction in individuals' labor supply (e.g., Solon 1979, Moffitt 1985, Katz and Meyer 1990, Meyer 1990, Hunt 1995). As of now, there is no clear consensus how this disincentive effect changes during recessions, with some observers arguing that it is larger during a downturn (e.g., Ljungqvist and Sargent 1998, 2008) while others have suggested that it may be smaller (e.g., Krueger and Meyer 2002).[3]

Identifying the effect of UI extensions in recessions relative to booms is extremely difficult in a setting where UI extensions are endogenous to the state of the labor market.[4] To circumvent

[1] In the United States, in each major downturn since 1975 extensions of UI benefit durations occurred at the state and federal level, reaching up to 99 weeks in 2010 (e.g., Lake 2002).

[2] The question of the incidence and effects of benefit exhaustion on workers and the appropriate response in UI duration dates back to the beginnings of the UI system in the United States (e.g., Myers and Maclaurin 1942). The economic effects of UI durations have played an important role in the debate about additional extensions in UI benefits stalled in congress for several months in the spring of 2010. Similar aspects played a role in evaluations of UI extensions in previous recessions (e.g., Needels and Nicholson 2004).

[3] The intuition for the view of stronger disincentive effects is that the incidence and cost of job loss is particularly severe in a recession (von Wachter, Song, and Manchester 2009). In this case the effective replacement rate may rise beyond the typical replacement rate and imply stronger and possibly lasting effects on unemployment as in Ljungqvist and Sargent (1998, 2008). On the other hand, consistent with our model, in recessions higher costs of job search may reduce the effect of UI parameters on labor supply and on the aggregate unemployment rate. In addition to the variation in incentives, aggregate factors determining nonemployment durations, such as the vacancy rate or congestion effects, could vary with the business cycle.

[4] In the United States, the importance of trigger-based state-level extended benefits relative to discretionary federal temporary benefits has declined since the mid-1980s (Congressional Budget Office 2004, Figure 2), making identifi-

this problem, in this paper we provide new estimates of the variation of the effect of UI durations on nonemployment and benefit durations over the business cycle using a regression discontinuity design and exceptional data from Germany. Our strategy exploits the fact that the German UI system implies large differences in the duration of UI benefits by exact age of the UI claimant. This policy is invariant to the business cycle and hence allows us to circumvent the endogeneity problem. Using exceptional day-to-day administrative data on the universe of unemployment spells and ensuing employment outcomes in Germany from the mid-1980s to 2008, we implement the RD approach by year and by industry, and correlate our estimates with indicators of the business cycle.

To help clarify the potential implications of our results for the welfare effect of UI extensions over the business cycle, we use a search model with endogenous search intensity and liquidity constraints (e.g., Card, Chetty, and Weber 2007a, Chetty 2008). From this model we derive a formula that directly relates the welfare gain of UI extensions over the business cycle to increases in the UI exhaustion rate and the welfare costs to the effect of potential UI durations on nonemployment and program duration. As in the related literature (e.g., Kiley 2003, Sanchez 2008), our formula also implies that optimal potential UI durations vary inversely with a summary measure of the disincentive effect of UI extensions: the ratio of the effects of UI extensions on nonemployment duration and UI benefit duration. This measure implicitly controls for changes in the number of individuals 'at risk' of exhausting benefits who are most affected by UI extensions, and is thus ideal for comparing changes in the disincentive effects over the business cycle.

To obtain a benchmark we begin by using our RD strategy to obtain labor supply effects with respect to UI durations in Germany for large differential expansions for mature workers with stable labor force attachment. For this sample, our estimates imply a moderate rise in nonemployment of about 0.1 months for each additional month of potential UI benefit duration that is robust across many alternative specifications. The effects on labor supply we obtain are similar for different

cation based on changes in the effect of benefit duration across states for recent recessions more difficult. Card and Levine (2000) examine the effects of an extension in UI unrelated to local unemployment conditions in New Jersey, and find more moderate effects on employment than previous studies.

increases in UI duration, similar across demographic groups, similar for workers with weaker labor force attachment, and somewhat larger for workers unlikely to take up extended unemployment assistance after exhausting UI benefits.

Our analysis of variation in the effect of UI extensions over the business cycle point to moderate, and for the most part statistically insignificant, declines in the disincentive effects of UI durations in larger recessions. On the other hand, we find that the effect of UI extensions on benefit durations, and thus the additional coverage provided by UI, increases significantly in recessions, mainly due to a rise in the UI exhaustion rate. The ratio of the two estimates, our alternative measure of the disincentive effect of UI benefits that implicitly controls for changes in UI take-up over the business cycle is clearly countercyclical, indicating that for each actual increase in UI durations, the response of labor supply to UI extensions falls in recessions. These results are robust to considering variation by year or year-by-industry, to the use of alternative measures of the business cycle, to reweighting to hold characteristics of UI claimants constant, to including workers with weaker labor-force attachment, and to an extensive robustness analysis.

We contribute to several aspects of the empirical literature on the effect of UI durations on employment of UI beneficiaries. This is the first paper to replicate regression discontinuity estimates in different economic regimes to assess whether the duration of UI has stronger or weaker employment effects in booms and recessions. This complements an earlier literature on cyclical effects of UI durations (e.g., Moffitt 1985, Jurajda and Tannery 2003) and related recent work on UI benefit levels (Kroft and Notowidigdo 2010) using state-level differences in unemployment and UI parameters in the United States.[5] We also are the first paper to explicitly assess changes in potential benefits of UI extensions over the business cycle through our analysis of fluctuations in actual benefit durations and the exhaustion rate. Our estimates also complement existing studies of the labor supply effects of UI durations by combining large increases in UI durations and a large

[5]Using variation in UI rules and unemployment rates across U.S. states, Moffitt (1985) and Kroft and Notowidigdo (2010) find that the effect of UI durations and UI benefit levels, respectively, on nonemployment duration tends to decline in slack labor markets. Jurajda and Tannery (2003) examine the effect of state and federal extensions in UI duration in Pennsylvania during the early 1980s recession, and find no difference in the effect on labor supply between more and less depressed regions of the state.

number of years and observations with a sharp regression discontinuity design (e.g., Meyer 1990, Katz and Meyer 1990, Hunt 1995, Lalive 2008). In contrast to studies using region or time variation in UI durations, our regression-discontinuity design allows us to hold market-level factors constant, such that we identify changes in the actual behavioral response, net of any market level factors that may change over time or across regions.

The paper also contributes to the literature concerned with the welfare implications of parameters of the current unemployment insurance system. By deriving the welfare effects of extensions in the duration of UI benefits, we extend the existing literature focused on UI benefit levels (e.g., Baily 1978, Kiley 2003, Shimer and Werning 2007, Chetty 2008, Sanchez 2008, Kroft and Notowidigdo 2010). Thereby, our formula also clarifies existing 'rules of thumb' regarding the optimal extension of UI benefits. According to our formula, the duration of UI should neither be only extended until the exhaustion rate is constant (Corson and Nicholson 1982), nor only extended to hold the nonemployment effect of UI constant (Moffitt 1985), but take into account both factors. Since the welfare formula depends on the actual nonemployment and benefit response to UI durations influenced by market-level factors our estimates hold constant (e.g., Landais, Michaillat, and Saez 2010), we do not derive direct welfare implications from our results.

The outline of the paper is as follows. In Section 2 we derive the welfare effect of extensions in UI benefits. Section 3 describes the institutional environment in Germany, the administrative data, and our empirical approach. Sections 4 and 5 contain our main findings regarding the effect of extended UI on labor supply and benefit duration over the business cycle. Section 6 concludes, relates our empirical findings to our theoretical welfare formula, and makes suggestions for future research.

2 The Costs and Benefits of UI Extensions in a Search Model

In this section we use a model of job search with endogenous search intensity and liquidity constraints (Card, Chetty, and Weber 2007a, Chetty 2008) to show that the welfare costs of UI benefit extensions rise with the adverse labor supply effect of UI durations, while the welfare benefits rise with the exhaustion rate of UI benefits. We then show that, as in the literature on optimal benefit levels, the welfare effect of UI extensions can be written as a function of a single parameter capturing individuals' labor-supply responses to UI extensions. Here, we only state the main results of the model and their intuition, relegating derivations and further discussions to the Web Appendix.

Worker's Problem. The model describes optimal behavior of a worker living T discrete periods (e.g., months) who is unemployed and receiving UI benefits in period zero.[6] In each period, the worker decides how intensely to search for a job. Let s_t denote search intensity, which is normalized to be equal to the probability of finding a job. Employment is an absorbing state and when employed a worker receives a wage of w_t and pays a tax of τ used exclusively to finance unemployment insurance benefits. Furthermore, in each period the worker owns assets A_t, the level of which is constrained by a lower bound. As in Chetty (2008), in our baseline case we assume that the wage a worker can receive is fixed (such that there is no role for reservation wages), the initial asset level A_0 is fixed, and there is no heterogeneity. Relaxing these assumptions does not affect our main conclusions (see the Web Appendix).

While unemployed, the worker receives a fixed level of UI benefits $b < w_t$ for at most a fixed number of P periods. During this period, the worker's flow utility function of consumption is denoted by $u(c_t^u)$, which can differ from the utility function of consumption during employment $v(c_t^e)$. After exhausting UI benefits, the worker receives a fixed baseline utility and no further transfer payments (though this is easily generalized). The total duration of nonemployment is $D \equiv \sum_{t=0}^{T-1} S_t$, where $S_t \equiv \prod_{j=0}^{t}(1-s_j)$ is the survivor function at time t. Total lifetime of workers at the time of entering unemployment is thus broken up into 3 periods: duration of receiving UI benefits ($B \equiv \sum_{t=0}^{P-1} S_t$), the duration of nonemployment without receiving UI benefits ($D-B$), and the duration of employment ($T-D$).

Welfare Effect of UI Extensions. Assuming the social planner sets taxes to achieve a balanced

[6] In our model UI durations do not affect the probability of jobs ending. An effect on the dismissal rate would probably be most likely if workers are eligible for UI after short employment spells and if UI induces workers to take up seasonal jobs. In our empirical analysis we do not find that longer UI durations affect the inflow rate into UI. Since individuals have to work for at least 12 months in Germany to be eligible for UI this should not create incentives to take on seasonal jobs.

budget of the UI system and that workers respond optimally to incentives, we can derive the effects of changes in the potential duration of UI benefits P on welfare.[7] Social welfare at time $t = 0$ is given as W_0, the expected life-time utility of an unemployed worker.[8] The budget constraint of the social planner requires $\tau = \frac{B}{T-D} b$. After some algebra, we obtain our first main result. The marginal welfare gain of increasing P is

$$\frac{dW_0}{dP} = \left.\frac{dB}{dP}\right|_1 b\left[u'(c_P^u) - E_{0,T-1}v'(c_t^e)\right] - \left[\left.\frac{dB}{dP}\right|_2 b + \frac{dD}{dP}\tau\right] E_{0,T-1}v'(c_t^e) \tag{1}$$

where $\frac{dB}{dP}\big|_1 \equiv S_P$ is the exhaustion rate of UI benefits, and $\frac{dB}{dP}\big|_2 \equiv \sum_{t=0}^{P-1} \frac{dS_t}{dP}$ is the increase in benefit duration due to reduced search intensity among unemployed individuals before the exhaustion point; $\frac{dD}{dP}$ is the increase in the total nonemployment duration in response to a rise in potential UI duration. The total effect of potential on actual benefit duration is $\frac{dB}{dP} \equiv \frac{dB}{dP}\big|_1 + \frac{dB}{dP}\big|_2$.

The first term in this expression states that the marginal welfare benefit (per person) of extending UI benefits is the transfer, financed by taxes, of consumption from the employed to the unemployed at the exhaustion point times the probability of exhaustion $\left(\frac{dB}{dP}\big|_1\right)$. This term is positive as long as the marginal utility of consumption of the unemployed in period P, $u'(c_P^u)$, is higher than the expected marginal utility of consumption of the employed, $E_{0,T-1}v'(c_t^e)$. If the two marginal utilities are equal, then there is no rationale for unemployment insurance in this model. In the model at hand this would be the case if the individual is not liquidity constrained.

The second term captures the costs of extending UI benefits due to the behavioral change induced by the more generous UI system. This cost is the per capita increase in taxes levied upon employed individuals times their marginal utility. Taxes rise because the unemployed lower their search intensity and this will increase their receipt of UI benefits $\left(\frac{dB}{dP}\big|_2 \times b\right)$. They also increase

[7] The solution of the worker's decision problem follows standard principles of dynamic optimization. We follow the existing applied literature on the optimality of the UI system by focusing on a constraint optimization within the class of typical UI systems (e.g., Baily 1978, Chetty 2008). A large theoretical literature has derived the full optimal time-path of UI benefits (e.g., Hopenhayn and Nicolini 1997, Shimer and Werning 2007, Pavoni 2007).

[8] As an alternative interpretation, one can assume that at t=0 only a fraction of individuals are unemployed, or that a fraction immediately finds a job at no effort. In this case social welfare W_0 represents the expected average utility of the employed and the unemployed. To analyze marginal changes in P we need to assume that P can be increased by a fraction of 1 (a month in our case), and that if P is not an integer number, it means a fraction of the period $int(P)$ is covered by the higher benefit level b.

because longer nonemployment durations reduce the number of periods in which individuals are employed and pay taxes $\left(\frac{dD}{dP}\right)$.

Empirical Implementation. Potential UI durations are at an optimum if $\frac{dW_0}{dP} = 0$. While solving the model laid out here for the optimal UI durations would require estimating a full structural model - beyond the scope of this paper - equation (1) provides a framework to analyze under what circumstances the welfare benefits of UI extensions are likely to vary over the business cycle. For any given change in UI duration P, the welfare effect potentially varies over the business cycle with different components of the formula.

The benefit levels b are, apart from changes in the sample composition, which we control for, unchanged over the business cycle. As long as the government smoothes taxes over the business cycle, which is approximately the case in most countries, the welfare cost of decreasing the tax base by one worker month ($\frac{B}{T-D}b$ times the average marginal utility of the employed individual who is taxed $E_{0,T-1}v'(c_t^e)$), can be considered fixed from a welfare perspective.[9] The components that potentially vary over the business cycle are the increase in nonemployment durations ($\frac{dD}{dP}$), the increase in benefit durations ($\frac{dB}{dP}\big|_1$ and $\frac{dB}{dP}\big|_2$) and the marginal utility of an unemployed individual at the exhaustion point ($u'(c_P^u)$). As further discussed in Section 4.2, changes in the latter can arise if liquidity – and hence the ability to self-insure – varies over the cycle.

The model remains agnostic as to the potential sources of cyclical variation in $\frac{dD}{dP}$, $\frac{dB}{dP}\big|_1$, and $\frac{dB}{dP}\big|_2$. One source of variation are changes in individual incentives affecting the choice in search intensity (the partial-equilibrium or micro effect of UI extensions). In the working paper version of this paper, we show that an increase in search costs in recessions, due for example to a decline in job-offer arrival rates, reduces $\frac{dD}{dP}$. On the other hand, a decline in reemployment wages, pos-

[9]To be more precise, the marginal utility of employed individuals who are affected by a marginal increase in taxes can be considered constant from the perspective of this analysis as long as the government chooses an optimal tax policy that levies additional taxes in periods when the costs of taxation are lowest, rather than balancing the budget every period (e.g., Andersen and Svarer 2010). In practice there appears to be considerable smoothing of UI taxes over the business cycle. For example, in Germany payroll taxes used to finance UI benefits do not vary with the business cycle. Similarly, in the United States, the states' UI trust funds run deficits in recessions. Such smoothing, rather than levying high taxes in recessions when UI expenditures are high, would be optimal as long as the marginal utility of the employed is approximately constant over the cycle. Compared to large earnings losses in recessions for job losers, the fluctuations in earnings trajectories, and hence expected marginal utility, of the average employed worker who pays the tax are typically weak (e.g., von Wachter, Song, and Manchester 2009).

sibly due to reallocation in recessions, would tend to increase $\frac{dD}{dP}$. The effective exit rate from nonemployment, and hence nonemployment duration, will also be affected by cyclical variation in market-wide factors, such as congestion effects, vacancy rates, or the take-up of UI benefits (the general-equilibrium or macro effect). In our empirical application, we will control for market-level variation, and hence will identify variation in nonemployment duration mainly due to differences in incentives. In Section 4 and the Conclusion we discuss under what circumstances this is sufficient to sign the change in the welfare effect of UI extensions over the business cycle.

Approximate Formula. Since our data allows us to obtain estimates of the effect of UI extensions on the full survivor function, in our empirical analysis we will measure the three relevant marginal effects separately. Yet, we find that most of the cyclical variation in $\frac{dB}{dP}$ is driven by variation in the exhaustion rate $\left(\frac{dB}{dP}\big|_1\right)$, whereas $\frac{dB}{dP}\big|_2$changes little. Thus, in the discussion of our main results we will focus on the properties of $\frac{dB}{dP}$ and $\frac{dD}{dP}$. For the case of a constant hazard of leaving nonemployment (i.e., $s_t = s$), one can show that the welfare effect of extensions in potential UI durations indeed depends only on these two parameters. In this case, the welfare effect of a change in P is given by the alternative formula

$$\frac{dW_0}{dP} = \frac{dB}{dP} b \left[u'(c_P^u) - E_{0,T-1} v'(c_t^e)\right] - \frac{dD}{dP} b\Omega \tag{2}$$

where $\Omega \equiv \xi u'(c_P^u) + \frac{B}{T-D} E_{0,T-1} v'(c_t^e) > 0$, $\xi \equiv \left(1 - Ps(1-s)^{P-1} - (1-s)^P\right)$, and $\frac{dB}{dP}\big|_2 = \frac{dD}{dP}\xi$. This formula indexes the welfare gain by the effect of potential on actual benefit duration $\left(\frac{dB}{dP}\right)$, and the welfare cost by the disincentive effect of UI extensions on labor supply $\left(\frac{dD}{dP}\right)$. Again, the main source of variation over the business cycle in this formula should be the employment and benefit effects of UI extensions. Even though the hazard in our sample is declining somewhat over the nonemployment spell, in simulations we found that the alternative welfare formula in equation (2) approximates the exact welfare formula in equation (1) quite well. The approximation is likely to work even better in settings such as the United States, where the hazard has been shown to be approximately constant (e.g., Katz and Meyer 1990).

Rescaled Formula. To determine whether optimal UI duration should change in a recession the approach so far requires separately measuring the effect of UI extensions on nonemployment and benefit duration. To express the formula in terms of a single statistic capturing the role of disincentive effects, we can rescale the formula by the effect of UI extensions on actual benefit duration times the benefit level $(\frac{dB}{dP}b)$. The approximate welfare formula becomes

$$\frac{dW^*}{dP} = \left[u'(c_P^u) - E_{0,T-1}v'(c_t^e)\right] - \frac{dD}{dB}\Omega, \tag{3}$$

where $\frac{dD}{dB} = \frac{\frac{dD}{dP}}{\frac{dB}{dP}}$. This equation captures the welfare gain from an extension in UI benefits expressed in dollars *relative* to the additional dollar amount of expenditures on UI. The key new parameter in this equation is the ratio of the effect of UI extensions on nonemployment and actual benefit durations, which is the instrumental variable (IV) estimator of the effect of actual UI duration (B) on nonemployment duration (D), using the maximum potential benefit duration (P) as instrument. This interpretation is appealing for several reasons. First, this estimator effectively rescales the marginal effect of the UI extension on nonemployment duration by the effective take-up of UI benefits due to UI extensions and hence by the population 'at risk' of being affected by the benefit extension. This implies that if $\frac{dD}{dP}$ is constant over the business cycle, but $\frac{dB}{dP}$ is increasing in recessions, the effective behavioral response to UI durations is declining.[10] Second, by introducing a single measure of the disincentive effect, our welfare formula in equation (3) is similar in spirit to those in Baily (1974) and Chetty (2008), where the elasticity of nonemployment with respect to benefit levels is replaced by the IV estimate of actual benefit durations on nonemployment durations.[11] Hence, in parallel to the prior literature, ceteris paribus a decline in the disincentive

[10]The IV estimator can be interpreted as a local average treatment effect, which is the weighted sum of the effects of an increase in actual UI duration on nonemployment duration at each duration up to the maximum duration P, weighted by the fraction of people whose benefit take up is affected by the UI extension. The weighting function is the difference in the survivor functions in the case with and without benefit extension up to the new maximum benefit duration (Angrist and Imbens 1995). If individuals were myopic and only responded by altering search intensity at benefit exhaustion, we would have $\frac{dB}{dP} = S_P$. In this case, the IV estimator measures the effect of the benefit extensions on nonemployment durations for those exhausting benefits and we would rescale the welfare formula by the exhaustion rate times the benefit level.

[11]By dividing by the average duration of nonemployment, the nonemployment elasticity $\eta \equiv \frac{\partial D}{\partial P}\frac{P}{D}$ effectively provides an alternative normalization for the number of individuals 'at risk'. Since nonemployment durations are not exclusively determined by UI benefit durations, this yields a more indirect normalization than that implicit in the formula (3).

effect as measured by the IV estimator implies a rise in optimal benefit duration. Finally, a key advantage for our purposes is that we can obtain an estimate of $\frac{dD}{dB}$ for broader samples of workers, for whom for reasons discussed in Section 3 we cannot obtain the rescaled marginal effects $\frac{dB}{dP}$ and $\frac{dD}{dP}$ separately. Therefore, we will also report estimates of this ratio in our empirical analysis.

3 Institutions, Data and Methodology

Several aspects of the German UI system make it ideal for studying the costs and benefits of UI extensions over the business cycle. Discontinuities in eligibility based on exact age allow us to estimate the effect of extensions in UI durations using a regression discontinuity design. A particular advantage is that the discontinuities lead to large extensions in the duration of UI at multiple age thresholds that are stable over long stretches of time, and thus do not depend on the business cycle. The system also provides the necessary detailed longitudinal data on UI and employment spells for large samples needed to credibly implement the regression discontinuity design for multiple years.

3.1 The Unemployment Insurance System in Germany

The German unemployment insurance system provides income replacement to eligible workers who lose their job without fault at a fixed replacement rate over a fixed period of time. For an individual without children the replacement rate is 63 percent of previous net earnings.[12] From the 1980s onwards the maximum duration of benefits was tied to recipients' exact age at the beginning of the UI spell and to their prior labor force history. It is this difference which we exploit to estimate the effect of extensions in duration of UI benefits on nonemployment durations. Figure 1 shows the discontinuities in potential benefit duration by age at claiming for the group of workers who

[12] Workers become eligible to receive UI benefits if they have worked for at least 12 months in the previous 3 years. Workers who quit their jobs are eligible for UI benefits after a waiting period of 12 weeks. While exact data on the number of quits among UI recipients are not available for our time period, our own calculations show a small spike of 3.5% of UI take-up 85 days after the end of a job. While receiving UI sanctions for not taking suitable jobs exist but appear to be rarely enforced (Wilke 2005). For individuals with children the replacement rate is 68 percent. There is a cap on earnings insured, but according to Hunt (1995) it affects a small number of recipients. Since they are derived based on net earnings, in Germany UI benefits are not taxed themselves, but can push total income into a higher income tax bracket.

by their employment history are entitled to the maximum durations in their respective age-group. Between July 1987 and March 1999, the potential UI duration for workers who were younger than 42 was 12 months. For workers age 42 to 43 potential UI duration increased to 18 months; for workers age 44 to 48 (49 to 53), the maximum duration further rose to 22 (26) months. As further explained below, to obtain precise measures of potential UI durations, we restrict ourselves to this sample of workers in our main analysis. At the end of the 1990s a reform occurred which was meant to reduce potential disincentive effects of unemployment insurance. As shown in Figure 1, starting in April 1999 the potential UI durations were lowered and the age thresholds were shifted upwards by 3 years. Thus in order to be eligible for 18 months or 22 months of benefits a worker had to be at least 45 or 47 on the claiming date. We will use these alternative thresholds to validate our main research design.[13]

Individuals who exhaust regular UI benefits and whose net liquid wealth falls below a threshold are eligible for unemployment assistance (UA), which does not have a limited duration. The nominal replacement rate is 53%, but UA payments are reduced substantially by spousal earnings and other sources of income. For example, for a woman whose husband earns as much as 10% more than her the UA benefits are zero. Given that about 80% of individuals in our cohort and age range are married, based on average earnings levels UA benefits are on average about 35% for men and 10% for women.[14] Among all new UI spells in our sample, about 10-15% end up taking UA benefits. We study the potential effect of UA on our findings in our empirical analysis.

[13]The reform was enacted in 1997 but phased in gradually, so that for people in the highest experience group, which constitutes our analysis sample, it only took effect in April 1999 (See Arntz, Lo, and Wilke 2007). To avoid confusion we refer to this as the 1999-regime in the text. In 2003 and 2004, the entire German social security system underwent a comprehensive series of reforms (the so-called Hartz reforms). We use the period between April 1999 and December 2004 as a second sample period, thus excluding workers who became unemployed after the Hartz IV reform took place. The implementation of the post-1987 regime occurred stepwise between 1983-1987 and is analyzed by Hunt (1995). We do not analyze these changes here, since the sample size in each of the short periods in which the UI system is stable is relatively small. Besides being potentially imprecisely estimated, they would not be easily compared to labor supply effects in other years, since both the economic environment and the magnitude of the cutoffs is different.

[14]UI benefits are paid for by worker and employer contributions, whereas UA benefits are funded by general revenues. The wealth threshold is not very stringent, but given the wealth distribution in Germany it is likely to be binding for part of our sample. UI and UA replacement rates were reduced by one (three) percentage points in 1994 for individuals with (without) children. Yet, controlling for a post-1994 dummy in the cyclical analysis below does neither show a significant decline in labor-supply effects nor affect the main results.

3.2 Social Security Data

The data for this paper is the universe of social security records in Germany. For each individual working in Germany between 1975 and 2008, the data contains day-to-day longitudinal information on every employment spell in a job covered by social security and every spell of receipt of unemployment insurance benefits, as well as corresponding wages and benefit levels. Compared to many other social security data sets, this data is very detailed. We observe several demographic characteristics, namely gender, education, birth date, nationality, place of residence and work, as well as detailed job characteristics, such as average daily wage, occupation, industry, and characteristics of the employer.[15]

To study the effect of extensions in duration of UI, we created our analysis sample by selecting all nonemployment spells in this data in the age range of 40 to 49. Given changes in the institutional framework discussed in the previous section, we consider unemployment spells starting any time between July 1987 and December 2004, yielding over 9 million spells (Column 2 of Table A-1). For each nonemployment spell we created variables about the previous work history (such as job tenure, experience, wage, industry and occupation at the previous job), the duration of receipt of UI benefits in days, the level of UI benefits, and information about the next job held after nonemployment.

Since we do not directly observe whether individuals are unemployed we follow the previous literature and use length of nonemployment as a measure for unemployment durations (e.g., Card, Chetty, and Weber 2007b). The duration of nonemployment is measured as the time between the start of receiving UI benefits and the date of the next registered employment spell. Since some people take many years until returning to registered employment while others never do so, we cap nonemployment durations at 36 months and set the duration of all longer spells at this cap. This

[15] Individual workers can be followed using a unique person identifier. Since about 80 percent of all jobs are within the social security system (the main exceptions are self-employed, students, and government employees) this results in nearly complete work histories for the vast majority of individuals. For additional description of the data see Bender, Haas, and Klose (2000). Each employment record also has a unique establishment identifier that can be used to merge establishment characteristics to individual spells. Below, we will use information on occurrences of establishment-level mass-layoffs constructed, described, and analyzed further by Schmieder, von Wachter and Bender (2009).

has the advantage of reducing the influence of outliers and avoiding censoring due to the end of the observation period in 2008. Our results are robust to the exact choice of the cap.

The main 'treatment' variable we are interested in is the potential duration of unemployment insurance benefits for any given nonemployment spell. To calculate potential UI duration for each spell in our sample, we use information about the law in the relevant time periods together with information on exact dates of birth and on work histories. This yields exact measures for workers who have been employed for a long continuous time and are eligible for the maximum potential durations for their age groups. However, the calculation is not as clear cut for workers with intermittent unemployment spells because of complex carry-forward provisions in the law. We thus define our core analysis sample to be all unemployment spells of workers who have been working for at least 52 months of the last 7 years and did not receive unemployment insurance benefits during that time period. This reduces our sample to about two million new UI spells (Column 4 of Table A-1). Below, we show that our results are robust to broadening our sample to include workers with weaker labor force attachment.[16]

Statistics for various samples are shown in the Data Appendix to the paper. As expected, relative to a general sample of nonemployment spells in Germany in the same age-range, the sample resulting from our restrictions on employment histories is more likely to be male, has higher job tenure, and has higher earnings prior to nonemployment. As a result, wage losses upon re-employment are larger and elapsed nonemployment spells are somewhat longer. Yet, there is little difference in educational attainment, nor are there strong differences in other post-UI career outcomes. We conclude that while our main sample is not representative for the full sample of nonemployment spells in Germany over this time period, it is likely to be typical of mature unemployed workers who lost a job during a recession. In our Web Appendix (Table W-13), we also show that the degree of job stability and other characteristics of our sample before unemployment is comparable to UI recipients in the same age-range in the United States. Hence, our main sample also bears similar features of UI populations of the same age-range studied in part

[16]For workers age 42, the maximum benefit can be obtained after 36 months. We were able to replicate our main results for this threshold using this broader sample as well.

of the prior literature.

Elapsed duration in UI and nonemployment spells is large, but similar to what is found in studies using comparable data. For example, in the Austrian case the mean duration of nonemployment or time between jobs for those reemployed by three years is similar (Card, Chetty, and Weber 2007b). The average duration of spells is larger than what is typically found in the United States. Yet, the differences are smaller where comparable data is available. This is found for the duration of UI spells in Card and Levine (2000), or for nonemployment durations in the Displaced Worker Survey (DWS) we analyzed. In the DWS, among 40 to 49 year old displaced workers who have received UI after displacement, after three years about 15 percent are still not employed, a figure comparable to Germany, where the fraction of individuals whose spell is censored at 36 months is 23 percent.[17]

3.3 Methodology

The institutional structure and data allow us to estimate the causal effect of UI benefit durations on nonemployment duration and other outcomes using a regression discontinuity design. In a first step, we exploit the sharp age thresholds in eligibility rules for workers with previously high labor force attachment in Germany to estimate the effect of large extensions in UI durations on labor supply. We then replicate this approach for every year or year-by-industry in our sample, and correlate it with indicators of the business cycle.

Throughout the paper, the analysis proceeds in two steps. We follow common practice and show smoothed figures to visually examine discontinuities at the eligibility thresholds (Lee and Lemieux 2010). To obtain estimates for the main causal effects, we follow standard regression

[17] See Appendix Table A-1 and Web Appendix Table W-14. Given the time since job displacement in the Displaced Worker Survey is based on calendar years and the survey is either in January of February, at 36 months after displacement the actual number is likely to be higher (for two years after displacement, the fraction not employed is about 21 percent in the DWS). The duration of unemployment is smaller in the survey data used by Katz and Meyer (1990a,b), but they discuss potential sources of measurement error due to recall problems. The average duration of spells in unemployment as defined by statistical authorities is also smaller, yet this ignores duration of time spent out of the labor force and is affected by institutional features of the labor market (e.g., Machin and Manning 1999).

discontinuity methodology and estimate variants of the following regression model

$$y_{ia} = \beta_0 + \beta_1 D_{a \geq a^*} + f(a) + \varepsilon_{ia}, \tag{4}$$

where y_{ia} is an outcome variable, such as nonemployment duration, of an individual i of age a. $D_{a \geq a^*}$ is a dummy variable that indicates that an individual is above the age threshold a^*. For our pooled estimates we focus on the longest period for which the UI system was stable, July 1987 - March 1999, and we use the three sharp thresholds at age 42, 44 and 49.[18] We estimate equation (4) locally around the three cutoffs and specify $f(a)$ as a linear function while allowing different slopes on both sides of the cutoff. In our main results we use a bandwidth of 2 years on each side of the cutoff, but confirm the robustness of our finding to using even smaller bandwidths. We then replicate this approach for different years, industries, demographic groups, and different outcomes. All results are robust to an extensive sensitivity analysis summarized in Section 5.

3.4 Identification Assumptions

The identification assumption of the regression discontinuity design requires that all factors other than the treatment variable that influence the outcome vary continuously at the age threshold. If this holds then estimates for β_1 can be interpreted as the causal effect of an increase in potential UI durations on the outcome variable, since the flexible continuous function $f(a)$ captures the influence of all other factors. In our setting both the employer who lays off workers as well as the individual have some influence on the timing of job loss and the claiming of unemployment benefits. Our data allow us to investigate in detail whether this leads to sorting around the eligibility cutoffs. The overall conclusion from this analysis is that our labor supply effects represent valid regression discontinuity estimates.

One approach to assess the identification assumption is to test for discontinuities in observ-

[18] There is a 4th discontinuity during this period at age 54. Since at this age early retirement becomes common and various policies to facilitate early retirement interact with the UI system we focus on younger workers in this paper. Early retirement in the context of the German UI system has been analyzed for example in Fitzenberger and Wilke (2010).

able characteristics at the threshold by estimating equation (4) with observable characteristics as outcome variables. Table 1 presents results of these regressions using 2 year bandwidths around the cutoffs. Of the 21 coefficients in Table 1, there are only two statistically significant at the five percent level. There is a statistically significant increase in the fraction female at the 42 year and 49 year threshold, however the magnitude of this is quite small. Examination of corresponding regression discontinuity plots (shown in Web Appendix Figure W-1) confirms the conclusion that pre-determined characteristics change very little at the thresholds.

A second standard way of testing the regression discontinuity (RD) assumption is to look at the smoothness of the density of unemployment spells around the cutoffs. Figure 2 (a) shows the number of spells in two-week age intervals. On average there are around 4300 spells in each interval up until age 47, after which the number of spells begins to decrease. It appears that at each cutoff there is a slight increase in the density in the bin directly on the right of the cutoff. Implementing the test proposed by McCrary (2008), this increase is statistically significant at the five percent level for the 42 and 49 cutoff but of very small magnitude.

Such an increase could either occur because workers wait until their birthday before claiming UI benefits, because the probability of claiming UI rises with potential durations, or because firms are more likely to lay off worker with higher potential UI durations. To see whether workers wait before claiming UI until they are eligible for extended UI durations column (1) of Table 3 below shows how the time between job loss and first take up of UI benefits varies around the threshold. This provides no indication that people who claim UI to the right of the threshold have waited longer before claiming than the people to the left of it. This is consistent with the quite small change in the density right around the cutoff we found. Only individuals relatively close to the age cutoff have economic incentives to wait until after their birthday to claim benefits. Taking into account that an individual does not receive UI until claiming, if one ignores the possibility of receiving UA after the end of UI and assumes zero discounting, one can show that the average individual (i.e., for whom the survivor function of remaining in nonemployment is the same as the empirical survivor function in the sample) could have an incentive to wait for up to 3.5 months.

Taking UA benefits and discounting into account reduces these incentives, and in combination with behavioral explanations may be the reason for the small amount of waiting we observe.[19]

To assess whether firms selectively lay off workers eligible for higher benefit durations, in Figure 2 (b) we show the density of spells with respect of the dates the last job prior to UI ended. If firms are more likely to lay off workers with higher UI benefits, the discontinuity should appear in this figure as well. Again there appear to be slight outliers right to the right of the 42 and 49 cutoffs, but less clearly as in Figure 2 (a). If anything this would indicate that firms may wait for a short time to lay off workers until they are eligible to higher UI benefit levels. It does not appear that firms are systematically more likely to lay off workers with higher levels of UI benefits, since in this case the density would shift up permanently. To ascertain that the small shift in the density of the age of layoff does not affect our results, we follow Card, Chetty, and Weber (2007a) and replicated our findings with new UI spells that originate from employers experiencing multiple layoffs and who hence have less scope for selectively laying off workers (Web Appendix Tables W-17 and W-18) . All our results are robust to this extension.

Overall, it appears that the discontinuity in the density is driven by maximally a few hundred spells shifted to the right just around the cutoffs. This is relative to around 450,000 spells in each of the four-year intervals that we use for our RD estimation.[20] As an additional conservative approach to assessing the impact of this change in density, we calculated bounds on our coefficients. For this purpose we pick a number of workers equal to the excess mass in the density above the threshold and reassign them to the left hand side of the threshold. To simulate the extreme case that workers with long potential nonemployment spells selectively deferred claiming to receive higher UI durations, we purposefully reallocate those workers with the highest nonemployment durations in our sample. This provides very conservative lower bounds of the treatment effect. We use a symmetrical approach to obtain upper bounds of the effect (assuming the excess mass is selected

[19]Taking into account presence of UA benefits approximately reduces the months it is worth waiting by one half. See Web Appendix Section 3.5 for how these numbers can be derived. To directly assess the potential impact of waiting, we also reestimated our models dropping UI spells with longer gaps between job ending and claiming (Web Appendix Table W-4 and Table 5), without an effect on the results.

[20]In smaller data sets this effect would almost certainly not be detectable.

based on shorter nonemployment durations). As further discussed below, this exercise yielded relatively tight bounds and hence confirmed that even extreme effects of selection would not overturn our results.

Overall, since the magnitude of the change in the density is very small (in particular relative to the size of our nonemployment results) and there are essentially no discontinuities in other variables we do not think this is a threat to the validity of our main estimates. As a robustness check we estimated all our main results below excluding observations within one month of the cutoffs (Web Appendix Tables W-2 and W-3). This has virtually no effect on the magnitude of the coefficient at age 42 and a very small effect on the other two coefficients. Furthermore we estimated our main specifications controlling for observables, and again obtained virtually the same coefficients.

4 The Effect of Large UI Extensions on Labor Supply Over the Business Cycle

4.1 The Average Effect of Large Extensions of UI Durations

Our first set of results pertain to the effect of large increases in potential UI duration at the three age thresholds on actual take up of UI and labor supply pooled over all years. Of interest in their own right, these findings provide a benchmark for our main analysis and interpretation of differences in the effect of UI extensions over the business cycle. Our main finding is that the labor supply effects of potential UI duration implied by our regression discontinuity estimates are modest, similar across age thresholds, smaller than the response of actual durations of UI benefits, and consistent with the theoretical model outlined in Section 2.

Figure 3 (a) shows how the duration of UI receipt varies with age at the beginning of the unemployment spell. The figure implies that a large number of individuals are substantially affected by the increase in potential UI durations. Workers younger than 42 at the age of claiming UI, are eligible to 12 months of UI benefits, of which they use about 6.7 months on average. At the age 42 threshold UI eligibility increases to 18 months and the average duration or UI receipt increases to about 8.5 months. The increases in benefit receipt duration at the other cutoffs are also quite substantial, and range from one fourth (at the age 44 cutoff) to one third (at the age 49 cutoff) of the increase in the maximum UI durations. The effects of the large UI extensions at the age thresholds on nonemployment durations are shown in Figure 3 (b). There is a clear jump in nonemployment durations at the age 42 cutoff from about 15.6 to 16.4 months of nonemployment. At age 44, nonemployment durations increase from 16.5 to 16.9 months and at age 49 from 19.9 to 20.3. Thus, visual evidence clearly suggests that the UI extensions lead to significant increases in both coverage and nonemployment durations at all thresholds.

The marginal effects obtained by estimating equation (4) separately for each age cutoff are shown in Table 2. Our main regression results in column (1) are very consistent with the graphical analysis. As shown in Panel (b), at the age 42 cutoff nonemployment durations increase by 0.78 months (standard error 0.1 months), at age 44 the increase is 0.41 months, and at age 49 the increase is 0.43 months.[21] To account for the fact that increases in UI durations differ across thresholds, one can consider the marginal effects of an increase of a single month of UI. These effects are shown in bold in the table and are in the same ballpark across age-groups (0.13, 0.10, and 0.11 for age 42, 44, and 49, respectively), and suggest that for each month of additional UI, affected workers spend three more days in nonemployment. An alternative approach to make the estimates comparable is to follow Meyer (2002) and calculate corresponding labor-supply elasticities. Despite the fact that the increases in UI occur at different levels of nonemployment and UI durations, the implied elasticities are nearly the same for the different cutoffs and range between 0.12 and 0.13 (see Appendix Table W-5).

After the reform of the UI system in the late 1990s, the eligibility thresholds for extended UI were shifted to ages 45 and 47 starting in 1999. Figure 4 shows that the basic results still hold in the post-1999 regime. The discontinuities in nonemployment durations move to the new age thresholds, confirming the assumptions implicit in our main analysis. Estimates of labor supply effects of potential UI duration (shown in Web Appendix Table W-7) are now somewhat smaller

[21] Since our main source of variation is not at the individual level but effectively at the time relative to the age threshold, we cluster our standard errors by days relative to the threshold to correct our degrees of freedom. This also allows for random specification errors due to the introduction of discrete bins (Lee and Card 2008). Choosing alternative dimensions of clustering does not affect the precision of our results.

than our main findings, but of the same order of magnitude and still similar across age-groups. This reduction may partly be due to stricter monitoring of job search behavior and penalties for not accepting suitable jobs in the new regime. When we investigate the cyclical variation of the effects of UI in the next section we will control for this slight shift in the level of the effects.[22]

Our findings imply that extensions in potential UI durations lead to a significant rise in the duration of nonemployment. They also suggest that actual UI durations respond more strongly than nonemployment durations. For each month of potential additional UI benefits, Table 2 implies on average individuals stay 9 to 12 days longer in UI, but only 3 days longer in nonemployment. This means a significant fraction of workers stay in nonemployment at benefit exhaustion and are thus directly benefiting from an extension of UI durations. For example, about 28 percent of unemployed individuals with 12 months of potential durations exhaust their UI benefits (Web Appendix Figures W-3). An analysis of the hazard function reveals that among exhaustees only 8 percent return to employment, while the majority enters nonemployment (Card, Chetty, and Weber 2007b).

An analysis of the hazard function also shows that the nonemployment effect we find is not purely due to an outward shift of the spike at the benefit exhaustion point. Figure 5 displays non-parametric regression discontinuity estimates of the survivor function (Panel a) and the hazard of exiting nonemployment (Panel b) by duration for individuals with 12 and 18 months of UI benefits. Panel (a) of Figure 5 illustrates how the labor supply effects are related to behavior throughout the nonemployment spell. Integrating over the area between the two survivor functions yields the total increase in nonemployment durations associated with the increase in the potential UI durations. The area between the two survivor functions from month 0 to month 18 represents the increase in actual UI durations that is due to the shift in the survivor functions, which - rescaled to marginal changes - corresponds to $\frac{dB}{dP}\big|_2$ in the theory section. The area between months 12 and 18 under the

[22] From Figure 4 it is also apparent that the duration of the average unemployment spell decreased for each age. Besides being a result from stricter monitoring, this might also be driven by an increasing incidence of temporary low-wage jobs over this time period. Yet, the coefficient on the dummy for the post-1999 period in our regression model of the annual effects of UI extensions in Section 4.2 is not statistically significant. Appendix Figure W-2 also shows a more visible increase in the density in the two age weeks at the two age cutoff points. Yet, the same arguments as in Section 3.4 apply.

lower survivor function, represents the additional UI coverage provided when individuals do not adjust their search behavior, which - again rescaled to marginal changes - corresponds to $\frac{dB}{dP}\big|_1$ in the theory section.[23]

To better display changes in the survivor functions, Panel (b) of Figure 5. Consistent with previous studies (e.g., Meyer 2002), in Panel (b) there are clear spikes in the hazard rate at the benefit exhaustion points for the two respective groups. However, there are also clear and statistically significant differences well before the exhaustion point, indicating that when eligible for longer durations, unemployed individuals adjust their search behavior a long time before running out of UI (e.g., Card, Chetty, and Weber 2007a). These findings imply that our main effects reported in Table 2 are averages of behavioral responses along the entire duration distribution.[24]

To benchmark the potential bias from sorting of individuals at the age thresholds we computed upper and lower bounds for the employment effects using the method described in Section 3.4. The bounds are relatively narrow, with both the lower and upper bounds being clearly statistically significantly different from zero. For the rescaled marginal effect of actual UI durations at the age 42 threshold, the lower bound is estimated as 0.29 while the upper bound is 0.32. For the nonemployment effects the bounds are 0.095 and 0.17 respectively - well within the overall range of our estimates in Table 2. Given that the economic incentives to wait are clearly stronger for individuals with longer nonemployment duration, the true effect most likely lies between the main estimate and the lower bound. The bounds for the other age thresholds are similar and are reported in the Web Appendix (Table W-4).

Restriction on Labor Force Participation. To examine whether our main findings are affected

[23] In practice the nonemployment and UI survivor functions diverge, since individuals leave the UI system without taking up employment even before their benefits expire. In the Web Appendix we show the survivor functions separately (Figure W-5) and explain how we can compute the components of the welfare formula $\frac{dB}{dP}\big|_1$ and $\frac{dB}{dP}\big|_2$ in this case (Figure W-6). When we analyze the cyclical variation of the two components in Section 4.2, we compute them using the actual UI survivor functions.

[24] A more detailed discussion of the methodology can be found in the Web Appendix (Section 2.1). Regression discontinuity estimates along different points of the duration distribution and for the other age cutoffs are shown in the Web Appendix Table W-9. The table shows significantly negative effects on the hazard prior to the exhaustion point of the control group. These effects are present in the first twelve months even when potential durations increase from 22 to 26 months, suggesting that individuals are forward looking over a long horizon. After the exhaustion point of the control group, the difference reverses, with the hazard of the higher eligibility group exhibiting a significant increase at the new point of exhaustion.

by our focus on stable workers we replicated our main RD estimates without any restriction on labor force attachment before UI receipt. According to the law, for the full sample potential UI durations vary between 2 and 6 months at the age thresholds, depending on the employment history. The RD estimates for the unrestricted sample, shown in the last columns of Table 3, are smaller than for the main sample for the durations of both UI receipt and nonemployment. Since the underlying average changes in potential UI durations at the thresholds are also smaller, this is consistent with the underlying true marginal effects being similar. As explained in Section 3 we cannot calculate a rescaled marginal effect for a single month of UI extension for this group. In order to obtain a measure comparable across samples, using the estimates in Table 2 and 3 we can normalize our estimates of the effect of UI extensions on nonemployment duration by dividing by the effect on actual UI duration. As discussed in Section 2, this ratio is effectively an instrumental variables estimator of the effect of UI duration on nonemployment. This ratio is quite similar for our main sample and the fully unrestricted sample.

Discussion. Our results are consistent with the theoretical model we discussed in Section 2. The model predicts that a rise in the potential duration of UI benefits leads individuals to lower their search effort (s_t). Consistent with our finding in Table 2, this implies a rise in the average duration of nonemployment. Also consistent with our results, the reduction in search intensity predicts a lower hazard of leaving unemployment for all nonemployment durations before benefit exhaustion. Our finding that the mean duration of UI receipt increases more strongly than the nonemployment duration implies that the increase in UI coverage is only partly due to a behavioral response. An important part of increased coverage is due to individuals continuing to receive UI benefits, who would otherwise have exhausted benefits while remaining in nonemployment. Our model provides a framework for the interpretation of the effects of UI extensions on nonemployment and benefit duration, and its implications are taken up again in Section 4.2 and the Conclusion. Finally, consistent with our model, in separate work we do not find an effect of UI extensions on wages, other measures of job quality, or long-term employment.

The labor supply responses we find are consistent with the results from previous studies in

Germany, Austria, and, with some qualification, the United States. Hunt (1995) evaluates the reforms in the German UI system over the period 1983 to 1988 using a difference-in-difference approach between age-groups over time. Hunt (1995) finds that her estimated effect on the hazard rate is slightly smaller than the effect in Moffitt (1985), who reports a marginal effect of 0.16 weeks per additional week of potential UI benefits. This implies the estimates are quite comparable despite differences in underlying samples, methodology, and measures of nonemployment duration.[25] Lalive (2008) evaluates the effects of UI in Austria in a RD design that is similar to ours. He finds that an increase of benefit durations from 30 to 209 weeks for workers age 50 increases unemployment durations for men from 13 to 28 weeks. This implies an increase in 0.09 months of nonemployment for each additional month of UI duration. In a different context, Card, Chetty, and Weber (2007a) analyze increases in benefit durations in the Austrian UI system using a similar RD design as ours but with smaller increases in potential UI durations. Their estimates point to similarly modest labor supply effects of potential UI durations.

The marginal effects of an additional month of potential UI benefits implied by our estimates are also in a similar range of related studies based on data from the United States. Our main estimates of a marginal effects of around 0.1 - 0.13 are at the lower bound of United States estimates of the effect of UI durations on labor supply surveyed in Meyer (2002). The most comparable study to ours (Card and Levine 2000) finds similarly modest effects of exogenous extensions in UI benefits. Other studies tend to find somewhat larger estimates (e.g., Meyer 1990, Katz and Meyer 1990). To what extent these differences could be explained by the German institutional environment will be discussed further in Section 5.2.

[25] Since Hunt's approach averages over different potential UI durations, a direct comparison with our estimates via marginal effects or elasticities is difficult. Another paper analyzing the age-thresholds of the German UI system using difference-in-differences, Fitzenberger and Wilke (2010), focuses on age groups older than 50, which we excluded from our analysis. Note that with flexible controls for age, in our case difference-in-difference estimates would be equal to RD estimates, hence we do not pursue a direct comparison between these approaches. Caliendo, Tatsiramos, and Uhlendorf (2009) use similar data as we do from 2001-2007 to study the effect of UI extensions on job quality, but focus on individuals close to benefit exhaustion at one age threshold.

4.2 Variation of Labor Supply Effects with the Business Cycle

A key advantage of the institutional setting in Germany is that it provides quasi-experimental increases in potential UI duration that do not vary with the business cycle. This allows us to study variation in the effects of UI over the business cycle while holding constant potentially confounding conditions in the labor market. Using the large samples in our data we replicated our regression discontinuity estimates for our multiple age thresholds for each year and major industry, and examined whether the resulting labor supply effects and benefit durations varied systematically with the business cycle. To do so, we could have in principle regressed the marginal effects on business cycle indicators for the entire period during which individuals choose their search intensity, including at the moment of benefit exhaustion. Due to high inter-temporal correlations of unemployment rates this is unfeasible. Instead, we include a single indicator of the state of the business cycle in our regressions. After some experimentation, as further discussed below, we have settled on a common subset of measures capturing the current state of the labor market and the rate of new inflows in the year of the start of a worker's UI spell.[26]

Overall, the findings from this exercise suggest that the nonemployment effects of potential UI durations are quite stable over the business cycle. At best, some of our results suggest a weak decline in the effect of extended UI on nonemployment in recessions. In contrast, we find that the effect of UI extensions on benefit duration, and thus the additional coverage provided, increases significantly in recessions, mainly driven by a rise in the exhaustion rate. As a result, we show that the nonemployment response to actual UI durations is clearly countercyclical.

The first panel of Figure 6 plots the rescaled marginal effects of a one-month increase in potential UI on nonemployment duration over time. The estimates are obtained by replicating our regression discontinuity estimates separately for each calendar year for the threshold at age 42

[26]As a helpful referee has pointed out, another justification for this 'reduced-form' approach is that from the policy maker's point of view, what matters is to optimally predict the exhaustion rate and nonemployment effect based on the current state (and predicted evolution) of the economy, not to estimate the full underlying behavioral relationship. Note that given the timing and duration of UI spells in our sample, measures of future levels and changes in unemployment rates would also capture well the economic environment during which workers make choices. However, at the behest of the referee, we moved these results to the Web Appendix, and only include variables that are available to the policy maker in the current year.

(and age 45, after the 1999 reform), which yields the most precise estimates for the effects. The unemployment rate shows how the German economy has gone through large economic swings during our sample period, such as the dramatic boom-bust period after unification, plus an ensuing protracted slump. Yet, while there is some variation of the estimated marginal effect over time, from the figure there appears to be no clear systematic variation with the business cycle. In the first panel of Figure 7 we investigate this further by plotting the marginal effect for all ages against the *change* in the unemployment rate from t-1 to t, where t is the year in which the UI spell starts. As discussed below, the change captures the flow of newly unemployed, and is an alternative measure of the state of the labor market. There is a slight negative correlation, but overall the marginal effect appears to be quite stable over the business cycle.

The findings from Figure 7 (a) are extended and confirmed in column (2) of Table 4. The first four rows of the table show results from regressing the rescaled marginal effects obtained from separate RD estimates for each year and age group on different indicators of the change in economic conditions. The first row shows that when we use a standard measure of the state of the business cycle – GDP growth – there is a positive albeit not statistically significant correlation between the labor-suppy effect of UI extensions and the business cycle, indicating that the disincentive effect tends to fall when the economy contracts. The second row uses the unemployment as a more direct measure of the state of the labor market, and finds a negative effect, also implying that the labor supply-effect declines in recessions. Although relative to the mean labor supply effect of 0.1, a rise in the unemployment rate of two standard-deviations (column 1) would lead to a non-negligible decline in the effect, this is not quite statistically significant at the 10% level. One concern with using the level of the unemployment rate is that its variation may be partly driven by the stock of long-term unemployed and thus represent labor market conditions facing newly unemployed workers only imperfectly. Hence, the next two rows shows two measures of the inflow rate into unemployment. Row 3 shows the findings when we use the change in the unemployment rate as main independent variable, whereas row 4 shows findings from using the annual mass-layoff rate at the establishment level as calculated from our data. Again the results indicate a negative relationship between the state of the labor market and the effect of UI extensions on nonemployment duration of somewhat smaller size than the unemployment rate, but the coefficients are imprecisely estimated.

One concern with this estimates might be that they are purely based on the time-series variation in economic conditions shown in Figure 6 (a). Hence, in the final two rows we show changes in labor supply elasticities for workers losing their jobs in broad industries with high or low rates of job destruction or with high or low average wage losses in their two-digit industry (as indicated by quintiles of average industry wage loss). The job destruction rate at the industry level provides a measure of the inflow rate. The average wage loss can be used as a proxy for the amount of specific skill a laid-off worker is likely to lose. This is interesting because the theory predicts unemployed workers facing higher wage losses should respond more strongly to benefit extensions. When working at the industry level we control changes in overall labor demand by introducing year fixed effects. The results suggest there is little significant difference in the effect of UI on nonemployment with our measures of industry-specific economic conditions.

Figure 6 (b), Figure 7 (b) and Table 4 replicate the same analysis for the effect of potential duration of UI benefits on the actual duration of UI benefits. Contrary to our finding for the effect of UI extensions on nonemployment durations, it now appears that the effect on actual UI durations is significantly countercyclical. The lower panel of Figure 6 clearly shows how there is a substantial positive relationship between the effect of UI extensions and benefit duration and the *lead* in unemployment rates. As we will see below, this correlation is driven by the cyclicality in the UI exhaustion rate. Given potential benefit durations in our sample, it is the state of the labor market in years t+1 or t+2 that matters. The lower panel of Figure 7 shows that there is a positive correlation between the change in the unemployment rate, capturing a rise in the inflow to unemployment, and actual UI benefit duration. This is not surprising, since given a high auto-correlation of unemployment rates and relatively long unemployment durations in Germany there is a strong correlation between current inflows and the future level unemployment.

These graphical findings are confirmed in Table 4, where we assess the correlation of the response in UI duration to benefit extensions with the same range of alternative measures of the business cycle as we did for the nonemployment effect. The table shows that the effect of UI extensions on benefit duration tends to increase in recessions. The effect correlates strongly with the change in unemployment rates, the mass-layoff rate, or our industry-specific measures of labor market conditions. For these measures, a rise of two-standard deviations lead to increases in the marginal effect of about 25-30 percent relative to the mean. While GDP growth has a smaller, insignificant effect, the effect of the level of the unemployment rate is of the wrong sign. Given the evidence in Figure 6 (b), it is likely that the reason is that the unemployment rate when entering unemployment correlates only weakly with the labor market conditions at UI exhaustion. That the cyclicality of the UI exhaustion rate is a key driver of the cyclicality in the response of actual benefit durations to UI extensions is shown explicitly in two remaining columns of Table 4. Column 5 shows how the exhaustion rate is strongly procyclical for the measures in rows 3 to 6, while column 6 shows that the increase in benefit duration prior to the exhaustion point, which is driven by a shift in the survivor function, varies little with the cycle. As further discussed below, this has potentially important implications for the welfare impact of UI extensions.

In the theory section we showed that the ratio of the nonemployment effect and the effect on effect on actual benefit durations, $dD/dB = \frac{dD}{dP} \Big/ \frac{dB}{dP}$ represents a summary measure of the disincentive effect that is relevant from a welfare perspective. The ratio, which captures the effect of an actual increase in UI benefit duration on nonemployment duration, effectively normalizes for changes in the intensity of treatment over the business cycle, since $\frac{dB}{dP}$ rises with the exhaustion rate and hence with the number of people potentially affected by UI benefit extensions. The cyclical variation of the so 'normalized' disincentive effect is shown in Column (4) of Table 4. Unlike the marginal effect $\frac{dD}{dP}$ this normalized disincentive effect is strongly countercyclical. Out of the 6 regressions, it is statistically significantly correlated with 5 measures of the business cycle. The effect of a worsening of our measures by two-standard deviations is substantial and in the same range for all measures but for the level of the unemployment rate, for which it is a bit smaller. However, the effect of the level of the unemployment rate is now of the right sign.

Our main findings in Table 4 are very robust and in some cases stronger when we consider important extensions in Table 5. To save space, Table 5 only shows the results for using the level and change of the unemployment rate as our measures of cyclical conditions, and relegate the estimates for additional measures to the Web Appendix (Table W-16). The first column of the table replicates our main findings in Table 4 for a bandwidth of one year. In several instances, the negative correlation of the marginal nonemployment effect and the ratio $\frac{dD}{dP} \big/ \frac{dB}{dP}$ is now stronger and statistically significant. It is worth noting that the nonemployment elasticity (shown for our main estimates in column (7) of Table 4), is now also significantly negatively correlated with the business cycle.[27]

The second column shows the lower bound discussed in Section 3.4, where we reallocate the excess mass of spells from the right to the left of the cutoff, and assign them the highest nonemployment durations in the sample. This would be the right estimator if all these workers had purposefully waited to gain access to longer benefits because they have the longest spells. Similar to the findings in column (1), these results tend to show somewhat stronger counter-cyclicality of the nonemployment effect and hence the ratio. To further account for potential selection due to voluntary quitters, who are required by law to wait 85 days before becoming eligible for UI, and also address the issue discussed in Section 3 that voluntary quitters may have a stronger incentive to wait to gain access to benefits above the threshold, column (3) drops anyone waiting more than two weeks before taking up UI benefits. The estimates are very similar to the main results in Table 4.

If characteristics of UI recipients change over time or vary with the business cycle and treatment effects vary across groups, another concern could be that such changes in composition could offset potential cyclical variation in labor supply effects of UI. We examined this possibility, and found it not to affect our result. We analyzed cyclical variation in two summary indices of observable characteristics in our data, the predicted propensity to receive unemployment assistance (UA) and

[27]This suggests that the elasticity is another possible way to rescale the marginal effect by the intensity of treatment as suggested in Section 2. However, the counter cyclicality is considerably weaker than that of the ratio dD/dB, because the average duration of nonemployment is also determined by other factors than treatment intensity.

the predicted post-UI wage. Overall, relative to the mean we found at best very small variations in observable characteristics with the business cycle. To nevertheless make sure these changes do not affect our findings, we used the standard re-weighting procedure to hold distribution of characteristics constant across years. This is shown in column (4) of Table 5 and confirms that our findings are very robust to changes in characteristics of UI recipients over time or the business cycle.

Finally, to address the concern that our findings are limited to our main sample of workers with high employment attachment, columns (5) and (6) of Table 5 show results when we include all workers irrespective of labor market experience. As discussed in Section 3.2, for this sample we cannot calculate rescaled marginal effects. However, the ratio of marginal effects has the same interpretation as for the restricted sample. The third panel of the table shows the results when using two and one year bandwidths to generate the estimates. The point estimates for the measures in the table are somewhat smaller compared to our more restricted sample when we use a bandwidth of two years (though identical and precisely estimated when we use the mass-layoff rate, as shown in Web Appendix Table W-16) and quite similar and more precisely estimated when we use a bandwidth of two years. Hence, our main results holds for a very broad sample of unemployed individuals and are not restricted to high-attachment workers.

Overall, we conclude that our main estimates for the effect of UI durations on labor supply do not vary strongly with the business cycle, but that the exhaustion rate, and with it the effect on benefit duration, is countercyclical. In several specifications we find a small decrease in the labor supply effect of UI durations in recessions, but in most instances the correlation is not statistically significant from zero. However, these findings show that the effect of a rise of actual benefit duration on nonemployment duration, which effectively normalizes for the increase in the number of workers 'at risk' of being affected by extended UI durations in recessions, has a clear and precisely estimated countercyclical pattern.

Discussion. To see how our empirical results relate to the theory we outlined in Section 2, it is useful to consider a slightly modified version of our main welfare formula. To express our welfare formula fully in terms of statistics that can potentially be measured empirically we follow Chetty (2008) and normalize equation (2) by the expected marginal utility of employed workers. The resulting rescaled welfare gain is approximately equal to

$$\frac{d\tilde{W}_0}{dP} = \frac{dB}{dP} b \left[\frac{-\partial s_P / \partial a_P}{\partial s_P / \partial w_P} \right] - \frac{dD}{dP} b\Omega \tag{5}$$

where the new term in the first bracket is the ratio of the 'liquidity' effect ($\partial s_P / \partial a_P$) and the 'substitution' effect ($-\partial s_P / \partial w_P$) of a UI extension for workers exhausting UI benefits.[28] If available, using appropriate empirical measures of the income and substitution effects one can assess whether at a given duration of UI benefits, an extension would be welfare improving. Changes in the welfare effect of extensions in UI benefit durations over the business cycle arise from variation in three main components, $\frac{dB}{dP}$ (or effectively the exhaustion rate), the nonemployment effect ($\frac{dD}{dP}$), and the relative strength of the liquidity vs. substitution effect of UI benefits.

As mentioned in Section 2, to relate our empirical findings to the theoretical welfare formula it is important to realize that what is relevant for the formula is the effective exit rate from nonemployment, and hence what we labeled a general-equilibrium or macro effect in Section 2. Our empirical estimates hold variation in the macroeconomic environment constant and thus obtain the partial-equilibrium or micro effect. Changes arising from the exhaustion rate (which is a mechanical effect) suggest that the number of beneficiaries from UI extensions are rising in recessions. Yet, as further discussed in our conclusion in the Conclusion, whether our empirical estimates imply that the effective nonemployment effect of UI durations, and hence the efficiency cost from UI extensions, also declines in recessions requires further information.

[28] We have that $d\tilde{W}_0/dP \equiv (dW_0/dP)/E_{0,T-1}v'(c_t^e)$. The result in our second formula holds as long as on average unemployment durations are short relative to life-time employment, such that the marginal utility after unemployment is similar to the expected marginal utility at employment in $t = 0$. For details see the Web Appendix Section 3.

5 Robustness

Our main results are very robust to many alternative specifications, which are briefly summarized in this section. Additional details are relegated to our Web Appendix.

5.1 Robustness Analysis

Choice of Bandwidth. We investigated whether the choice of the bandwidth of the RD estimator affects our conclusions (Table 2). Using a bandwidth of 2 years, the point estimates from the RD regressions are very similar to what is implied by the graphical analysis. For smaller bandwidths coefficients are very stable for the effects on UI durations, even with bandwidths as small as 0.5 or 0.2 years. For the nonemployment durations the estimates are in the same ballpark across different bandwidths, but somewhat larger for tighter bandwidths. Investigating figures with different bandwidths revealed that this is due to under-smoothing for very small bandwidths, so that we have most confidence in estimates with 2 year bandwidths. As discussed in Section 4.2, our estimates in column (1) of Table 5 also imply that our results regarding cyclicality are robust to choosing a narrower bandwidth.[29]

Measure of Nonemployment. We also find that the increase in nonemployment durations is mainly due to workers taking longer until returning to a job, not due to individuals staying out of employment forever. In order to investigate this Table 3 column (2) shows the probability of ever returning to registered employment. There is a slight drop of one percent relative to the mean of 0.77 (Appendix Table A-1) at the age 42 cutoff, and the effect is even smaller for the other two age thresholds. Even though it is statistically significant, the slight decline in the fraction of workers ever returning to work accounts for a very small increase in overall nonemployment durations. Similarly we investigated whether our estimates are affected by the choice of our nonemployment duration measure. For example, as an alternative we replicated all of our findings with time-to-

[29] It should be noted that 2 years is a very narrow bandwidth in comparison to other papers with a similar RD design. For example, Card, Chetty, and Weber (2007) use a polynomial in age without a bandwidth; Lalive (2008) shows a specification with a two-year bandwidth, but his preferred specifications use polynomials in age without bandwidth; Lemieux and Milligan (2008) use bandwidths of six years

next-job for workers who return to employment. Consistent with the result that the incidence of censoring does not vary strongly at the eligibility thresholds, our results are largely unaffected by this choice.[30]

Differences by Subgroups. To further examine the robustness of our main estimates, Table 6 shows our regression discontinuity estimates for several subgroups. While the table displays some expected differences in the labor supply response to UI extensions, overall the labor supply effects are remarkably robust throughout the population. In particular, it does not appear that our findings are driven by any particular sub-group in our sample. The labor supply effects are slightly larger but not significantly different for highly educated and high tenure workers, and larger and significantly different for women. Together with the similarities across age-groups, the point estimates in Table 6 imply a common labor supply effect of an additional month of UI benefits in the range from 0.1 to 0.18.

Robustness of Differences over Cycle. We estimated many additional specifications to those reported in Section 4.2 to further investigate the robustness of the findings regarding the cyclical variation of the effect of UI extensions. For example, dropping UI spells from East Germany from our sample, or excluding temporary lay-offs (workers who return to their old employer), did not affect the results reported in Table 4. One potential concern with our main estimates in Table 4 is that they mask differential effects over the cycle in different parts of the duration distribution. We compared shifts in the entire hazard function across boom and bust periods in the Web Appendix (Figure W-4), and did not find this to be the case. We also tried several ways to further raise precision of our estimates. For example, when we split our sample by worsening and improving labor market conditions, the labor supply effect seems to be somewhat lower in worsening times (Web Appendix Table W-11). Alternatively we estimated a cox-proportional hazard model in the spirit of Meyer (1990) and find a slight decline in the predicted labor supply elasticities when unemployment is increasing (Web Appendix Table W-12). We also estimated a

[30]Web Appendix Table W-5 provides a summary of the various steps in the sensitivity analysis, such as using different censoring rules. See Card, Chetty, and Weber (2007b) for further discussion of alternative measures of unemployment spells.

linear and log-linear models that pool the effect of UI extensions across our different age-thresholds while flexibly controlling for age (Web Appendix Table W-13). Again, the changes over time we find are relatively small, with at best weakly negative coefficients on the interaction of potential UI duration and business cycle indicators.

5.2 The Role of Unemployment Assistance

In this section we discuss to what extent our results may be affected by relatively generous UI replacement rates and by the presence of unemployment assistance (UA). On the one hand, high replacement rates implies that our relatively modest effects would over-predict the effect relative to a system with lower replacement rates.[31] On the other hand, the presence of UA after exhaustion of UI benefits should lead to smaller effects, since the strength of the disincentive effect of UI depends on the net change in replacement rates at exhaustion. In this context, the fact that women have only somewhat higher responses than men is interesting, since as discussed in Section 3 for the typical married woman with a working husband (which is a majority in our age-range) the benefit provided by UA after exhaustion of UI benefits is close to zero. This suggests that presence of UA per se may not strongly affect our estimates. To learn more about the potential role of extended UA in explaining our findings, we replicated our main regression discontinuity estimates for individuals with high and low propensities to receive UA. If our main estimates were mainly driven by individuals entering UA after exhausting benefits, we should see significant disparities here. The last rows of Table 6 shows that this is not the case. About 10-15% of UI beneficiaries and 50% of exhaustees receive UA. For each UI recipient in our sample we predicted the propensity to receive UA based on education, demographic characteristics, and their earnings histories.[32] The

[31] The presence of a cap on earnings implies the average replacement rate is likely to be somewhat lower than the nominal rate, but still more generous than, say, in the United States, where more stringent maximum weekly benefits imply that nominal replacement rates of about 50 percent turn to average replacement rates of close to 40 percent. Since UI benefits in the United States are subject to income taxes, but usually taxed at a lower rate than earnings, this is likely to somewhat understate the effective replacement rate.

[32] The corresponding linear probability model is shown in the Web Appendix Table W-10, and suggests our specification has a good fit. The average predicted value for the probability of take up of UA at exhaustion for the full sample is 0.54, which can be thought of as an estimate of the fraction of UI recipients who are potentially eligible for UA. Note that given the determination of UA benefits, ideally we would have had also access to wealth, marital status, and spousal earnings to make this prediction. Wealth closely correlates with education and earnings histories.

rescaled marginal effect for individuals whose propensity is above and below 0.5 is 0.1 and 0.18, respectively (the proportion of workers with with low propensity is about 30%). If we include an interaction with the individual propensity and extrapolate linearly, for individuals with propensity of receiving UA close to 80% the rescaled marginal effect is below 0.05. Yet, even for those whose propensity is 20% it is 0.25, well within the overall magnitude of our main findings. Thus, we conclude that while individuals seem to respond to the incentives inherent in UA, and hence the presence of UA may lead to somewhat smaller overall estimates, it is unlikely to be the main source behind the labor supply effects we find.

6 Conclusion

In this paper, we estimate how the effect of extensions in the duration of unemployment insurance benefits (UI) on nonemployment and benefit duration varies over the business cycle. To do so, we use the universe of unemployment spells in Germany over 20 years, where differences in potential UI durations by age allow for the implementation of a regression discontinuity design. Since the age discontinuities do not vary with economic conditions, they provide multiple valid quasi-experiments throughout the business cycle, allowing us to avoid endogeneity problems arising when parameters of UI respond to the business cycle. Our findings indicate a modest effect of extensions in UI durations on nonemployment durations of comparable magnitude to what has been found before. This effect is quite stable in different economic environments. At best some specifications point to slightly smaller nonemployment effects during recessions. On the other hand we find that the additional coverage provided by UI extensions is strongly increasing in recessions, mainly due to a sharp increase of the fraction unemployed who otherwise would have exhausted their UI benefits. As a result, the ratio of the effect of UI extensions on nonemployment and benefit durations – which captures the reduction in nonemployment duration for a given rise in UI durations and hence is a measure of the disincentive effect that controls for changes in the take-up of UI benefits over the business cycle – is significantly counter-cyclical.

Unfortunately, we currently do not have access to marital status, and neither wealth or spousal earnings are in our data.

To help interpret these findings, we show in a search model with liquidity constraints that the welfare effect of UI extensions is the sum of two components: the benefit provided by the additional coverage for individuals who otherwise would have exhausted UI benefits, and the cost due to the disincentive effect of UI, which leads to an increased tax burden for the employed. This result clarifies that the optimal duration of UI benefits does not only depend on the the exhaustion rate (Corson and Nicholson 1982) or on the nonemployment effect (Moffitt 1985) alone, but on both. We also show that the welfare gain from UI extensions can be expressed as a function of the ratio of the effect of UI extensions on nonemployment and benefit durations. As in prior related literature on UI benefit levels, a decline in this ratio and hence a reduction in the effective moral hazard of UI extensions ceteris paribus raises the optimal duration of UI benefits.

The welfare formula that we derive from our model, together with our findings of weakly declining nonemployment effects and strongly countercyclical exhaustion rates implies that the optimal duration of UI benefits should rise in recessions only under two conditions. First, if the marginal utility of the unemployed is constant or increasing during recessions. Second, if the cyclical movement of the partial-equilibrium effects identified in this paper provide a good approximation of the cyclicality of general equilibrium effects. In this case our results also suggest that countries with constant UI durations over the cycle, such as Germany and most other European countries, may raise welfare by moving to a system with counter-cyclical potential UI durations.

Whether these two important conditions hold are important empirical questions beyond the scope of this paper. For example, a reduction in the ability to self-insure through a decline in wealth in recessions would lead to a rise in the marginal utility of consumption of the unemployed and thus in the value of insurance. Existing evidence from the United States suggests that this "liquidity" effect does not appear to vary significantly with the business cycle. A bigger challenge, both theoretically as well as empirically, is that UI extensions may affect nonemployment durations by affecting the overall state of the labor market. The importance of such general equilibrium channels can not be estimated in our regression discontinuity design, which by construction compares individuals with different potential UI durations in the same labor market. To what extent the general equilibrium or macro effect of UI extensions is smaller or greater than the partial equilibrium or micro effect we measure depends on a range of factors lying outside the scope of our model and our empirical analysis. In the presence of search externalities; if there is incomplete take up of UI benefits; if UI extensions raise aggregate demand; or if recessions involve job rationing (as in the model of Landais, Michaillat, and Saez 2010), then our partial equilibrium effects represent a lower bound of the efficiency cost of UI. If on the other hand recessions involve the need for reallocation (e.g., as in Sargent and Ljungvist 1998) or if UI extensions reduce the incentives to create vacancies, then our estimates may represent upper bounds.

An advantage of the welfare formula we derive is that it can be fully expressed in terms of 'sufficient statistics' that can be estimated in the data (Chetty 2008). Thus, to implement the welfare formula and calculate the welfare gain from an extension in UI durations in recessions, future research should obtain estimates of variation in both the liquidity effect of UI extensions and the general equilibrium effect of UI extensions on nonemployment durations. Although the fact that Card, Chetty, and Weber (2007a) report similar labor supply effects for broader age groups for Austria with a similar RD design as ours is reassuring, a further useful extension of our work would be to obtain similar estimates of the effect of UI extensions on nonemployment and benefit durations over the business cycle for younger workers. Our research design also does not allow us to directly assess the labor supply effect of indefinite unemployment assistance (UA) available in Germany after UI is exhausted. Our results indicate that variation in the likelihood unemployment assistance has predictable effects, but assessing the labor supply effect of UA directly is an important avenue for future research.

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Tables

Table 1: Regression Discontinuity Estimates of Smoothness of Predetermined Variables around Age Discontinuities in Potential Duration of Unemployment Insurance (UI) Benefits

	(1) Years of Education	(2) Female	(3) Foreign Citizen	(4) Tenure Last Job	(5) Occupation Tenure Last Job	(6) Industry Tenure Last Job	(7) Wage Last Job
D(age>=42)	0.027	0.0056	0.0023	-0.010	-0.038	-0.017	0.28
	[0.014]	[0.0028]*	[0.0021]	[0.028]	[0.036]	[0.016]	[0.21]
Observations	452749	452749	452749	452749	452749	452749	418667
D(age>=44)	-0.0092	0.00016	-0.00088	-0.045	-0.052	-0.023	0.078
	[0.013]	[0.0028]	[0.0024]	[0.029]	[0.037]	[0.017]	[0.20]
Observations	450280	450280	450280	450280	450280	450280	413874
D(age>=49)	0.026	0.010	-0.000038	-0.0072	-0.070	-0.011	-0.12
	[0.014]	[0.0036]**	[0.0034]	[0.034]	[0.045]	[0.021]	[0.26]
Observations	329680	329680	329680	329680	329680	329680	292706

Notes: The coefficients estimate the magnitude of the change in the dependent variable at the age threshold. Each coefficient is estimated in a separate RD regression that controls linearly for age with different slopes and bandwidth of two age years on each side of cutoff. Standard errors (in parentheses) are clustered at the day level (* P<.05, ** P<.01).
The sample consists of individuals starting unemployment spells between July 1987 and March 1999, who had worked for at least 52 months in the last 7 years without intermittent UI spell. Last job refers to the last job prior to starting the unemployment insurance spell. Means are shown in Appendix Table A-1.

Table 2: Regression Discontinuity Estimates of Potential Unemployment Insurance (UI) Benefit Duration (P) on Months of Actual UI Benefit Receipt and Months of Nonemployment

	(1)	(2)	(3)	(4)
	Age bandwidth around age discontinuity			
	2 years	1 year	0.5 years	0.2 years
Panel A: Dependent Variable: Duration of UI Benefit receipt (B)				
D(age>=42)	1.78	1.82	1.73	1.65
	[0.036]**	[0.052]**	[0.072]**	[0.11]**
Effect of 1 add. Month of Benefits $\frac{dB}{dP}$	**0.30**	**0.30**	**0.29**	**0.28**
Observations	452749	225774	112436	45301
D(age>=44)	1.04	1.16	1.13	1.24
	[0.047]**	[0.065]**	[0.092]**	[0.15]**
Effect of 1 add. Month of Benefits $\frac{dB}{dP}$	**0.26**	**0.29**	**0.28**	**0.31**
Observations	450280	225134	112597	45258
D(age>=49)	1.40	1.44	1.44	1.72
	[0.074]**	[0.084]**	[0.12]**	[0.18]**
Effect of 1 add. Month of Benefits $\frac{dB}{dP}$	**0.35**	**0.36**	**0.36**	**0.43**
Observations	329680	217942	109238	43812
Panel B: Dependent Variable: Nonemployment Duration (D)				
D(age>=42)	0.78	0.92	1.04	0.79
	[0.086]**	[0.12]**	[0.17]**	[0.27]**
Effect of 1 add. Month of Benefits $\frac{dD}{dP}$	**0.13**	**0.15**	**0.17**	**0.13**
Observations	452749	225774	112436	45301
D(age>=44)	0.41	0.63	0.62	0.78
	[0.089]**	[0.13]**	[0.18]**	[0.30]*
Effect of 1 add. Month of Benefits $\frac{dD}{dP}$	**0.10**	**0.16**	**0.15**	**0.20**
Observations	450280	225134	112597	45258
D(age>=49)	0.43	0.52	0.56	0.79
	[0.11]**	[0.13]**	[0.19]**	[0.29]**
Effect of 1 add. Month of Benefits $\frac{dD}{dP}$	**0.11**	**0.13**	**0.14**	**0.20**
Observations	329680	217942	109238	43812

Notes: The coefficients estimate the magnitude of the change in benefit or Nonemployment duration at the age threshold. Each coefficient is estimated in a separate RD regression that controls linearly for age with different slopes on each side of cutoff. Standard errors (in parentheses) are clustered at the day level (* P<.05, ** P<.01).
At the age 42 discontinuity potential UI benefit durations (P) increase from 12 to 18 months, at the age 44 discontinuity from 18 to 22 months and at the age 49 discontinuity from 22 to 26 months. The sample consists of individuals starting unemployment insurance spells between July 1987 and March 1999, who had worked for at least 52 months in the last 7 years without intermittent UI spell. For the age 49 cutoff and bandwidth 2 years column, the regression only includes individuals 47 and older and younger than 50, due to the early retirement discontinuity at age 50 (see text).

Table 3: Regression Discontinuity Estimates of Effect Of Potential Unemployment Insurance (UI) Benefit Duration on Additional Employment Outcomes and Estimates Based on Sample without Labor-Force Restrictions

	(1) Time until UI Claim	(2) Ever emp. again	(3) Emp. 5 years later	(4) UI or UA 5 years later	(5) UI Duration No Exp. restrictions	(6) Nonemp Duration No Exp. restrictions
D(age>=42)	-0.00089	-0.01	-0.0041	0.0049	0.98	0.45
	[0.020]	[0.0022]**	[0.0029]	[0.0021]*	[0.016]**	[0.036]**
Observations	452749	452749	452749	452749	2467954	2467954
D(age>=44)	0.016	-0.0056	-0.0076	0.0051	0.46	0.21
	[0.021]	[0.0024]*	[0.0030]*	[0.0023]*	[0.019]**	[0.036]**
Observations	450280	450280	450280	450280	2293865	2293865
D(age>=49)	-0.0027	-0.0076	-0.0012	0.0047	0.76	0.40
	[0.025]	[0.0036]*	[0.0038]	[0.0032]	[0.032]**	[0.050]**
Observations	329680	329680	329680	329680	1550099	1550099

Notes: The coefficients estimate the magnitude of the change in the dependent variable at the age threshold. Each coefficient is estimated in a separate regression discontinuity model that controls linearly for age with different slopes and bandwidth of two age years on each side of cutoff. Standard errors (in parentheses) are clustered at the day level (* P<.05, ** P<.01).
The sample for columns (1) to (4) consists of individuals starting unemployment spells between July 1987 and March 1999, who had worked for at least 52 months in the last 7 years without intermittent UI spell. The sample for columns (5) and (6) consists of all individuals starting unemployment spells between July 1987 and March 1999 without restriction on employment history or UI receipt prior to the current UI spell. UA refers to means tested unlimited unemployment assistance available at exhaustion of UI benefits (see text).

Table 4: The Correlation of Annual Regression Discontinuity Estimates of Extensions in UI Benefit Durations on Nonemployment and Actual Benefit Duration with Alternative Measures of the Economic Environment

Dependent Variable	(1) Mean & Standard Deviation	(2) Nonemployment Duration: Rescaled Marginal Effect	(3) UI-Benefits Duration: Rescaled Marginal Effect	(4) Nonemp. Duration Marg. Effect scaled by UI-Benefits Duration Marg. Effect	(5) UI Exhaustion Rate (Additional UI Beneficiaries holding Survivor Function constant)	(6) Additional UI Beneficaries due only to Shift of Survivor Function	(7) Nonemployment Duration Elasticity
Independent Variable		$\frac{dD}{dP}$	$\frac{dB}{dP}$	$\frac{dD}{dP} / \frac{dB}{dP}$	$\left.\frac{dB}{dP}\right\|_1$	$\left.\frac{dB}{dP}\right\|_2$	$\eta_{D,P}$
Dependent Variable Varies by Year and Age-Threshold							
Real GDP Growth from	2.15	0.0078	-0.012	0.045	-0.011	-0.00086	0.011
Year t-1 to t	[1.58]	[0.0065]	[0.0081]	[0.023]†	[0.0055]†	[0.0048]	[0.0071]
National Unemployment Rate	9.09	-0.0099	-0.0053	-0.032	-0.00059	-0.0047	-0.011
in Year t	[1.64]	[0.0063]	[0.0084]	[0.024]	[0.0061]	[0.0046]	[0.0070]
Change in Unemployment Rate	0.13	-0.012	0.038	-0.10	0.029	0.0086	-0.019
from Year t-1 to t	[0.78]	[0.013]	[0.014]*	[0.042]*	[0.0093]**	[0.0091]	[0.014]
Fraction of Establishments with	1.31	-0.022	0.059	-0.17	0.048	0.010	-0.035
Mass Layoffs in Year t	[0.53]	[0.020]	[0.022]*	[0.065]*	[0.014]**	[0.015]	[0.022]
Dependent Variable Varies by Year and 1-digit Industry and Age-Threshold							
Job Destruction Rate in 1-digit	0.090	0.52	1.39	-2.32	1.25	0.14	0.26
Industry from Period t-1 to t	[0.032]	[0.49]	[0.28]**	[1.20]†	[0.17]**	[0.27]	[0.51]
Dependent Variable Varies by Year and Quintile of Average 2-digit Industry Wage Change and Age-Threshold							
Mean Change in Log Wages within	-0.047	-0.033	-0.56	0.98	-0.62	0.063	0.089
Quintile of 2-dig. Ind. Wage Change	[0.079]	[0.15]	[0.093]**	[0.47]*	[0.047]**	[0.089]	[0.17]
Mean of Dep Var		0.10	0.28	0.36	0.44	-0.15	0.11

Notes: Stars indicate confidence levels: †P<.1, * P<.05, ** P<.01.
Columns (2)-(7) report coeffients from a 2 step regression. In the first step the effect of Extended UI durations on nonemployment durations are estimated separately for all years and age thresholds using the regression discontinuity estimator. All RD marginal effects are computed using a 2 year bandwidth and control for linear age splines with different slopes on each side of the cutoff. In the second step the resulting marginal effects (columns) are regressed on measures of the economic environment (rows). Each reported coefficient represents the coefficient on those measures, given in the row names. The second step regressions also include a dummy for marginal effects measured after the 1999 reform. Standard errors are computed to allow for a common error-component at the year level. A mass-layoff is defined as a 30A mass-layoff is defined as a 30% drop in employment over a year. The rate is calculated among all establishments with at least 50 employees in the baseline year.

Table 5: Alternative Specifications for the Correlation of Annual Regression Discontinuity Estimates of Extensions in UI Benefit Durations on Nonemployment and Actual Benefit Duration with the Economic Environment - Smaller Bandwidth, Reweighted, Unrestricted Sample

	(1) Bandwidth for RD Est. 1 Year	(2) Lower Bound for Estimates in RD Est.	(3) Sample Restr. to UI take up within 15 Days of Job Ending	(4) Sample Reweighted to Characteristics of Year 2000	(5) Full Sample: No Experience Restrictions Bandwidth 2 Years	(6) Full Sample: No Experience Restrictions Bandwidth 1 Year
Nonemp. Duration Marginal Effect: $\frac{dD}{dP}$						
National Unemployment Rate in Year t	-0.019 [0.0065]*	-0.011 [0.0057]†	-0.012 [0.0096]	-0.014 [0.011]	–	–
Change in Unemployment Rate from Year t-1 to t	-0.019 [0.015]	-0.022 [0.011]†	-0.0100 [0.020]	-0.020 [0.022]	–	–
UI-Benefit Duration Marginal Effect: $\frac{dB}{dP}$						
National Unemployment Rate in Year t	-0.0059 [0.0093]	-0.0044 [0.0087]	-0.0060 [0.0092]	-0.0049 [0.0090]	–	–
Change in Unemployment Rate from Year t-1 to t	0.042 [0.015]*	0.040 [0.014]*	0.040 [0.015]*	0.036 [0.015]*	–	–
Nonemp. Duration scaled by UI-Benefit Duration Marginal Effect: $\frac{dD}{dB} = \frac{dD}{dP} / \frac{dB}{dP}$						
National Unemployment Rate in Year t	-0.059 [0.024]*	-0.039 [0.026]	-0.026 [0.029]	-0.037 [0.026]	-0.00095 [0.027]	-0.039 [0.027]
Change in Unemployment Rate from Year t-1 to t	-0.13 [0.045]*	-0.12 [0.045]*	-0.099 [0.053]†	-0.12 [0.045]*	-0.059 [0.051]	-0.12 [0.047]*
Nonemp. Duration Elasticity : $\eta_{D,P}$						
National Unemployment Rate in Year t	-0.022 [0.0077]*	-0.012 [0.0067]†	-0.015 [0.012]	-0.014 [0.0098]	–	–
Change in Unemployment Rate from Year t-1 to t	-0.029 [0.017]	-0.028 [0.013]*	-0.021 [0.025]	-0.026 [0.019]	–	–

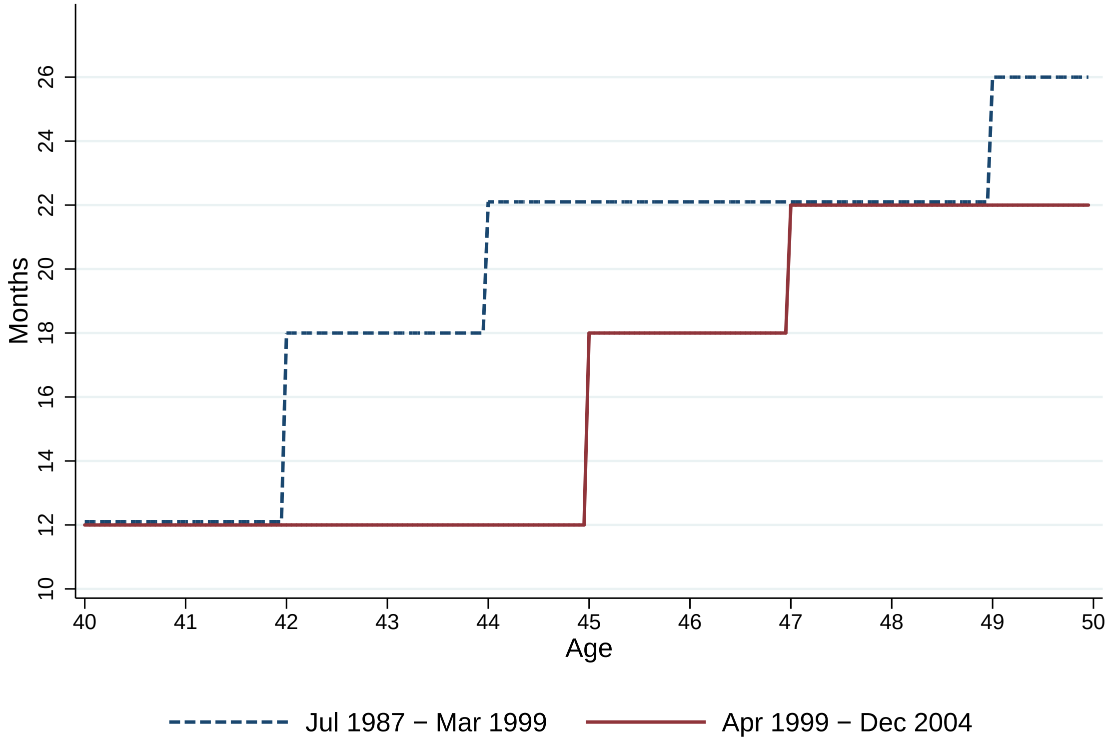
Notes: Stars indicate confidence levels: †P<.1, * P<.05, ** P<.01.
The specifications correspond to the first 4 rows in Table 4. Each coefficient is from a separate regression and based on the same 2 step method as before. The unit of observation in the second stage is the RD coefficient in 51 age-threshold X year cells. Column (1) is identical to the specifications in Table 4, but uses a 1 year bandwidth for obtaining the RD estimates. Column (2) obtains the RD estimates using the lower bound analysis described in the text. Column (3) restricts the sample to individuals who take up UI benefits within 15 days of the end of their last job. Column (4) uses a reweighting method to keep the observable characteristics constant across all years. Columns (5) and (6) show estimates for the full sample without any experience restrictions for one and two year bandwidths. The mean of the dependent variable in Columns (5) and (6) is 0.46 and 0.62, respectively. Since for the unrestricted sample the actual potential benefit duration is not known (but changes in the fraction of workers with high labor-force attachment over the business cycle can lead to changes in treatment intensity and hence to 'spurious' variation in the regression discontinuity effects), the rows referring to rescaled marginal effects are left empty.

Table 6: Regression Discontinuity Estimates of Effect of Potential Unemployment Insurance (UI) Durations (P) on Months of Actual UI Benefit Receipt and Months of Nonemployment by Subgroups

		UI Benefit Duration	Nonemployment Duration
Education - with or without Abitur (College Entrance Exam)			
Less than Abitur	D(age>=42)	1.83	0.75
		[0.04]	[0.10]
	Effect of 1 add. Month of Benefits $\frac{dy}{dP}$	0.31	0.12
Abitur or more	D(age>=42)	1.64	0.79
		[0.09]	[0.22]
	Effect of 1 add. Month of Benefits $\frac{dy}{dP}$	0.27	0.13
	P-Value for Equality of Effects	0.05	0.87
Job Tenure			
$\leq$ 5 years	D(age>=42)	1.81	0.73
		[0.04]	[0.11]
	Effect of 1 add. Month of Benefits $\frac{dy}{dP}$	0.30	0.12
$>$ 5 years	D(age>=42)	1.75	0.88
		[0.09]	[0.22]
	Effect of 1 add. Month of Benefits $\frac{dy}{dP}$	0.29	0.15
	P-Value for Equality of Effects	0.54	0.54
Gender			
Men	D(age>=42)	1.54	0.64
		[0.05]	[0.11]
	Effect of 1 add. Month of Benefits $\frac{dy}{dP}$	0.26	0.11
Women	D(age>=42)	2.27	0.94
		[0.07]	[0.16]
	Effect of 1 add. Month of Benefits $\frac{dy}{dP}$	0.38	0.16
	P-Value for Equality of Effects	0.00	0.12
Probability of receiving Unemployment Assistance after UI Benefits			
$Prob > 0.5$	D(age>=42)	1.58	0.62
		[0.05]	[0.11]
	Effect of 1 add. Month of Benefits $\frac{dy}{dP}$	0.26	0.10
$Prob \leq 0.5$	D(age>=42)	1.95	1.07
		[0.07]	[0.16]
	Effect of 1 add. Month of Benefits $\frac{dy}{dP}$	0.32	0.18
	P-Value for Equality of Effects	0.00	0.02

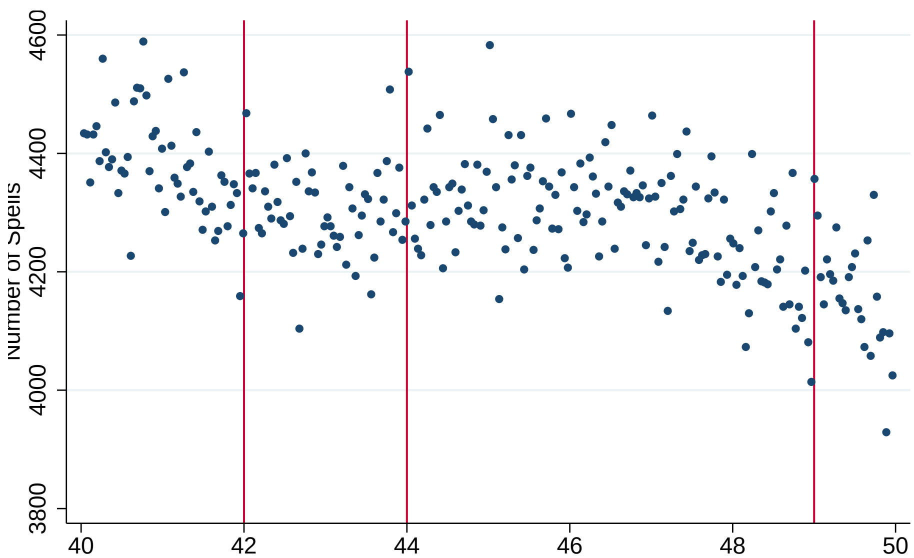
Notes: The coefficients estimate the magnitude of the change in the dependent variable at the age threshold. The two coefficients within each column of each panel are from a fully interacted RD regression, where the age-splines, constant and RD dummy are interacted with a dummy for the subgroup. The reported P-value corresponds to a test of equality between the two coefficients based on this interacted method. Standard errors (in parentheses) are clustered at the day relative to the threshold level (* P<.05, ** P<.01).
The sample consists of individuals starting unemployment spells between July 1987 and March 1999, who had worked for at least 52 months in the last 7 years without intermittent UI spell. The probability of receiving unemployment assitance is estimated by probit using pre-determined characteristics (see the text and Web Appendix).

Figure 1: Potential Unemployment Insurance Durations by Period for Workers with High Prior Labor Force Attachment

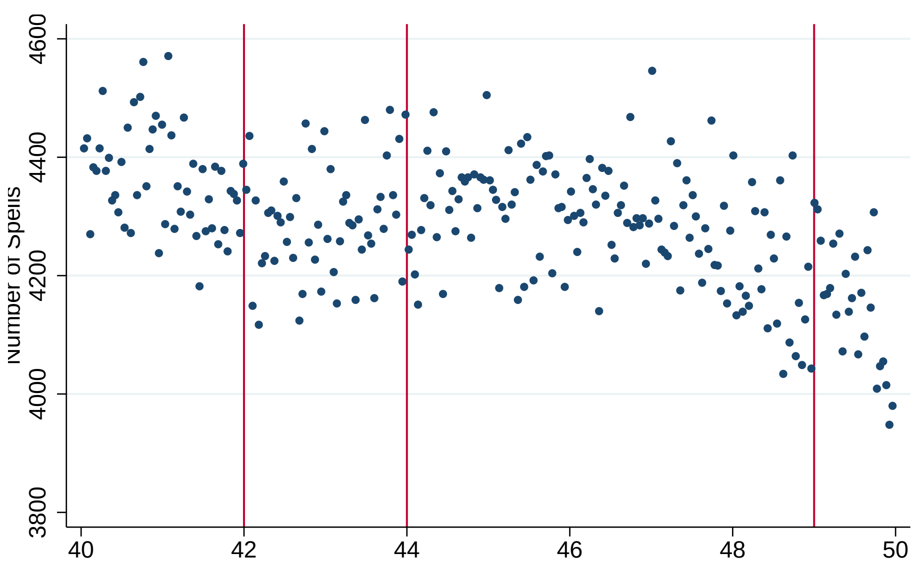


Notes: The figure shows how potential unemployment insurance (UI) durations vary with age and over time for unemployed individuals workers who had worked for at least 52 months in the last 7 years without intermittent UI spell.

Figure 2: Frequency of Spells Around Age Cutoffs for Potential Unemployment Insurance (UI) Durations - Period July 1987 to March 1999



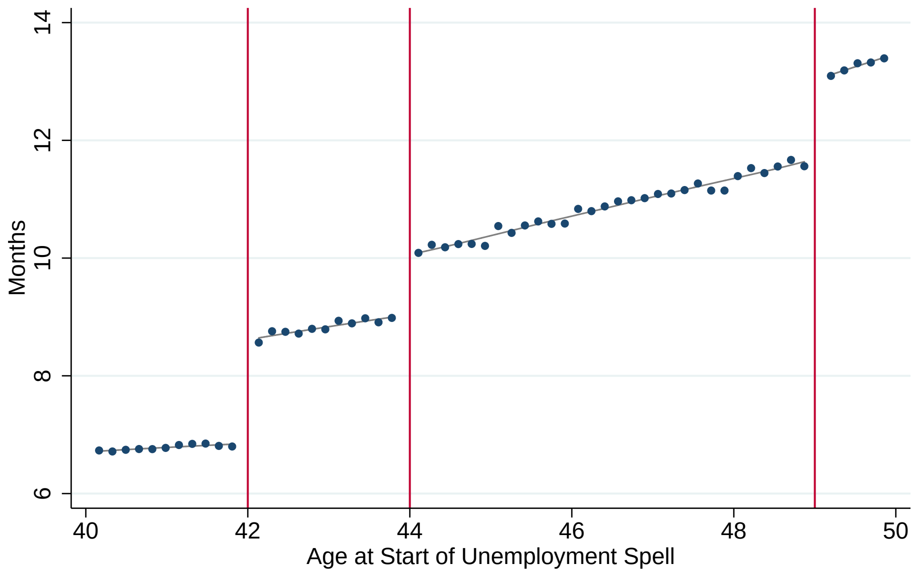
(a) Age on date of UI claim



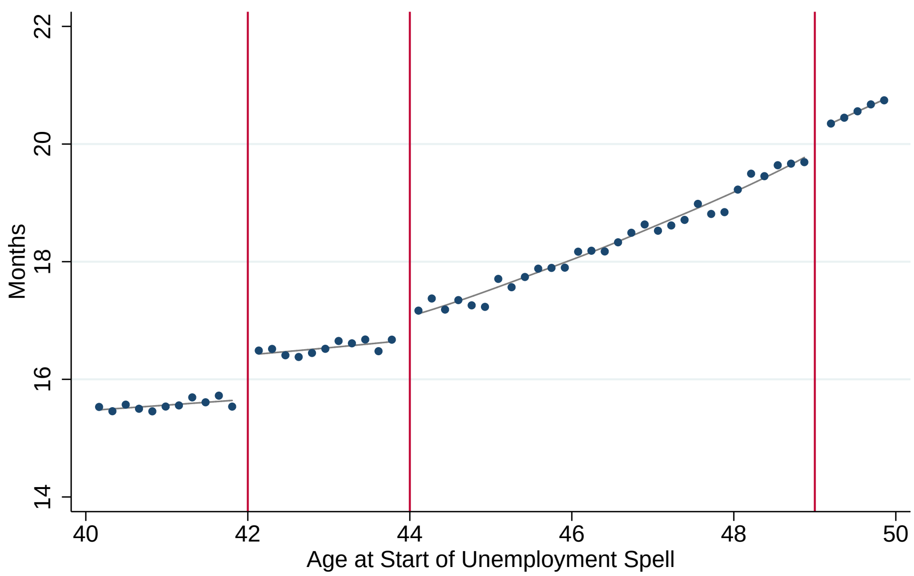
(b) Age at Job Loss

Notes: The top figure shows density of spells by age at the start of receiving unemployment insurance (i.e. the number of spells in 2 week interval age bins). The bottom figure shows the density by age at the end of the last job before the UI spell. The vertical lines mark age cutoffs for increases in potential UI durations at age 42 (12 to 18 months), 44 (18 to 22 months) and 49 (22 to 26 months). The sample are unemployed worker claiming UI between July 1987 and March 1999 who had worked for at least 52 months in the last 7 years without intermittent UI spell.

Figure 3: The Effect of Potential Duration in Unemployment Insurance (UI) Benefits on Months of Actual UI Benefit and Months of Nonemployment by Age - Period 1987 to 1999



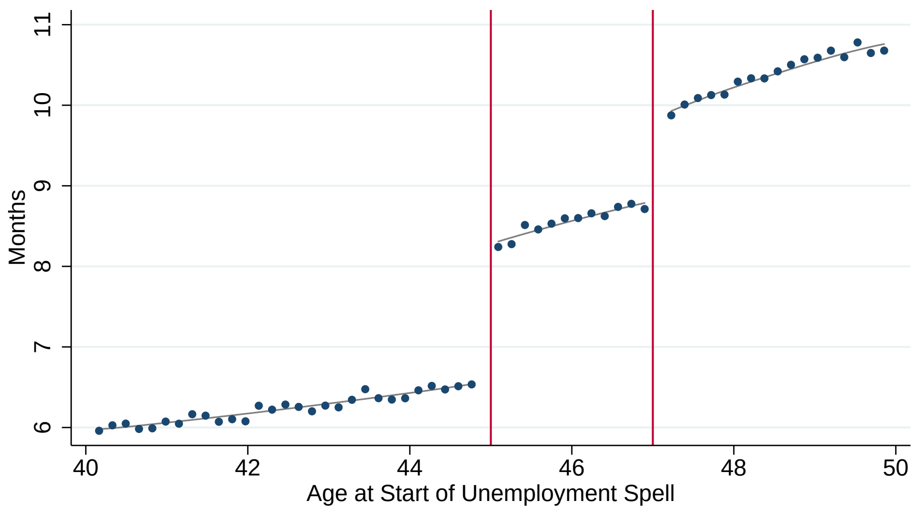
(a) Actual UI Duration



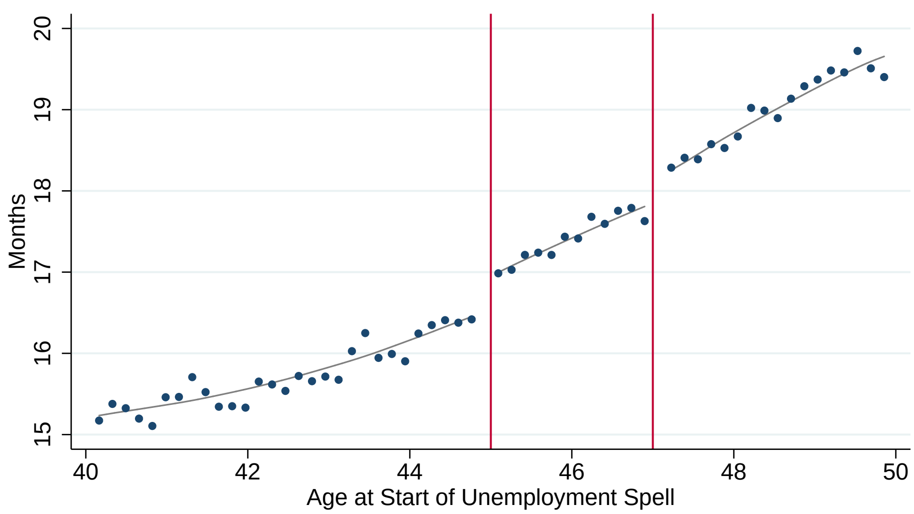
(b) Nonemployment Duration

Notes: The top figure shows average durations of receiving UI benefits by age at the start of unemployment insurance receipt. The bottom figure shows average non-employment durations for these workers, where non-employment duration is measured as the time until return to a job and is capped at 36 months. Each dot corresponds to an average over 120 days. The continuous lines represent polynomials fitted separately within the respective age range. The vertical lines mark age cutoffs for increases in potential UI durations at age 42 (12 to 18 months), 44 (18 to 22 months) and 49 (22 to 26 months). The sample are unemployed worker claiming UI between July 1987 and March 1999 who had worked for at least 52 months in the last 7 years without intermittent UI spell.

Figure 4: The Effect of Potential Duration in Unemployment Insurance (UI) Benefits on Months of Actual UI Benefit and Months of Nonemployment by Age - Period 1999 to 2004



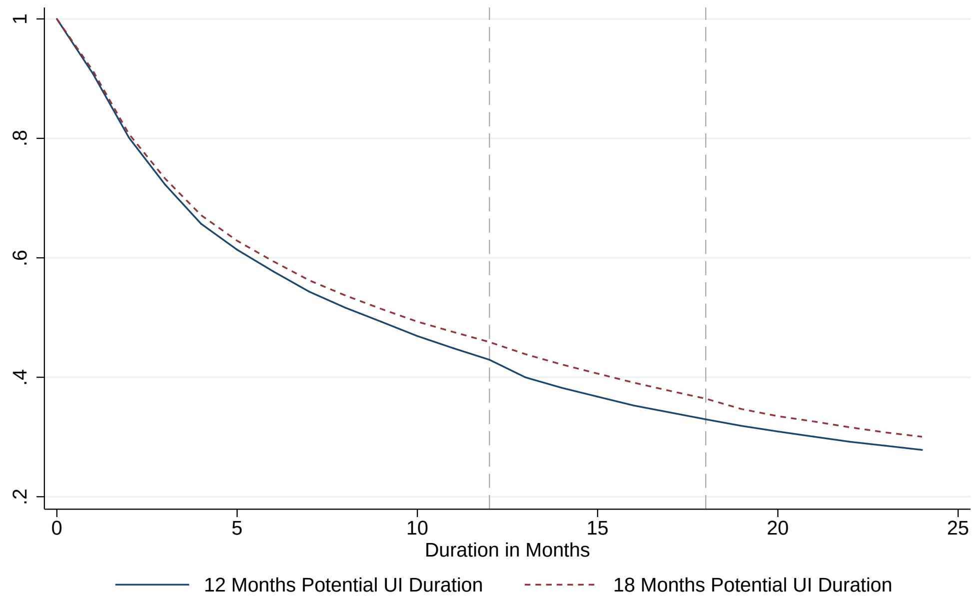
(a) Actual UI Durations



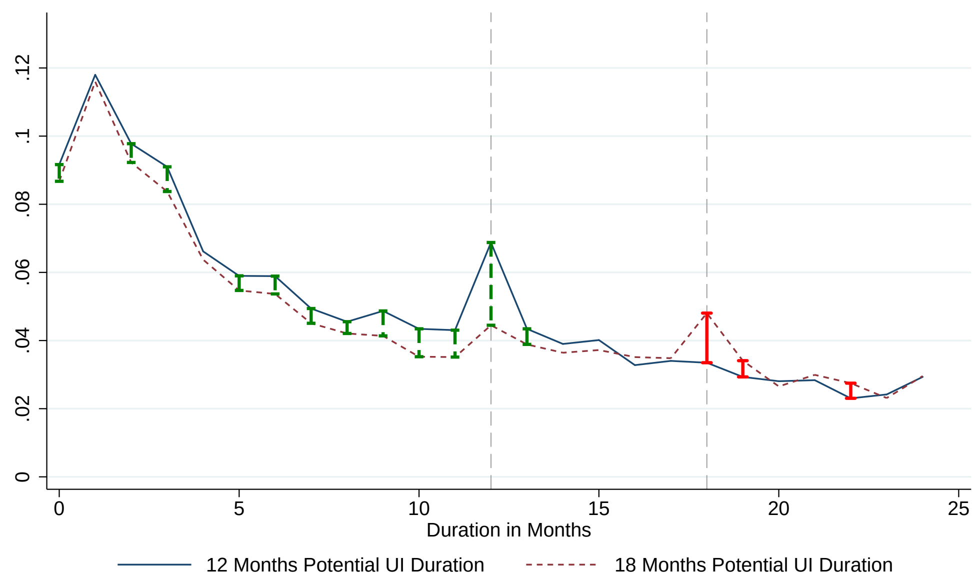
(b) Non-employment Durations

Notes: The top figure shows average durations of receiving UI benefits by age at the start of receiving unemployment insurance. The bottom figures shows average non-employment durations for these workers, where non-employment duration is measured as the time until return to a job and is capped at 36 months. Each dot corresponds to an average over 120 days. The vertical lines mark age cutoffs for increases in potential UI durations at age 45 (12 to 18 months) and 47 (18 to 22 months). The sample are unemployed worker claiming UI between April 1999 and December 2004 who had worked for at least 52 months in the last 7 years without intermittent UI spell.

Figure 5: Effect of Increasing Potential Unemployment Insurance (UI) Durations from 12 to 18 Months on the Survivor and Hazard Functions - Regression Discontinuity Estimate at Age 42 Discontinuity



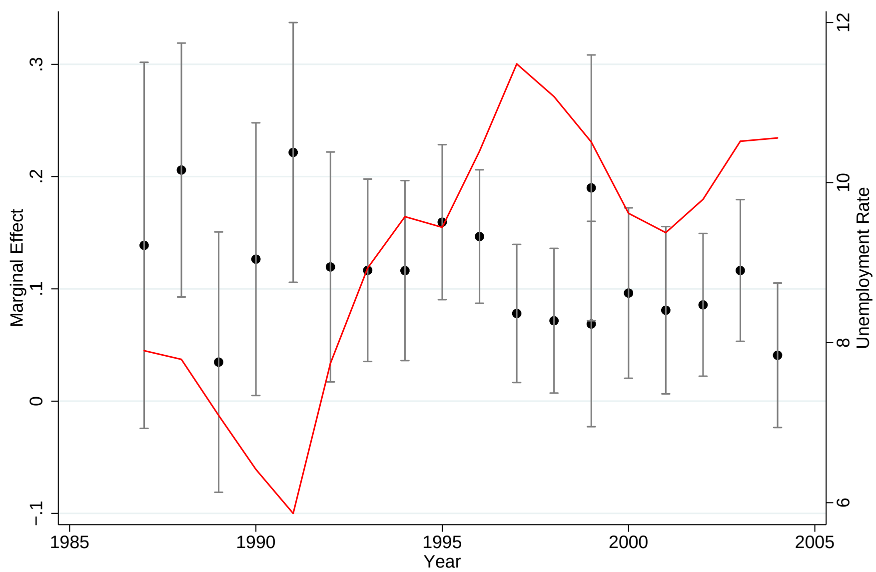
(a) Survivor functions for staying in UI at the age threshold



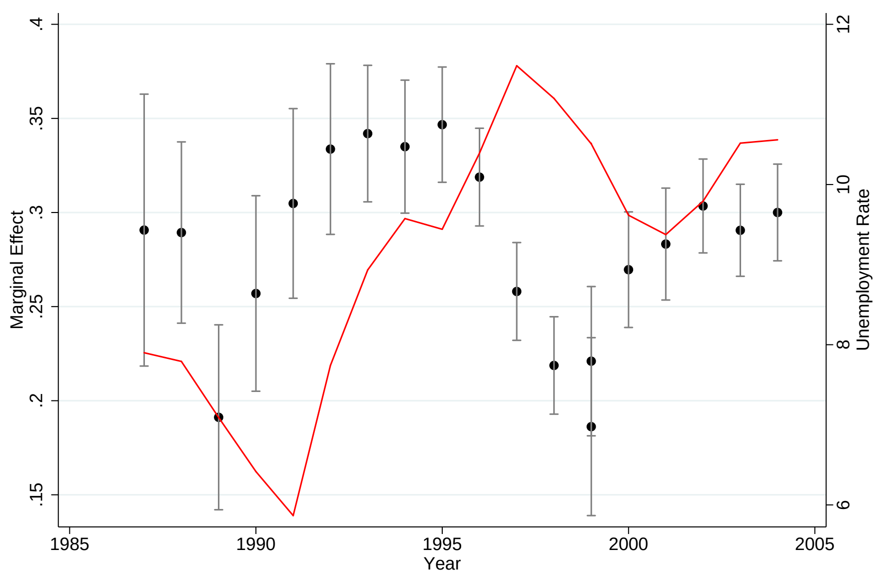
(b) Hazard functions of leaving nonemployment at the age threshold

Notes: The difference between the hazard and survivor functions is estimated pointwise at each point of support using regression discontinuity estimation. In the bottom figure, vertical bars indicate that the hazard rates are statistically significant from each other at the five percent level. The sample are unemployed worker claiming UI between July 1987 and March 1999 who had worked for at least 52 months in the last 7 years without intermittent UI spell. For details see text and Web Appendix.

Figure 6: Variation in Regression Discontinuity Estimates of Marginal Effects of Potential Unemployment Insurance Duration at the Age 42 and Age 45 Discontinuities over Time



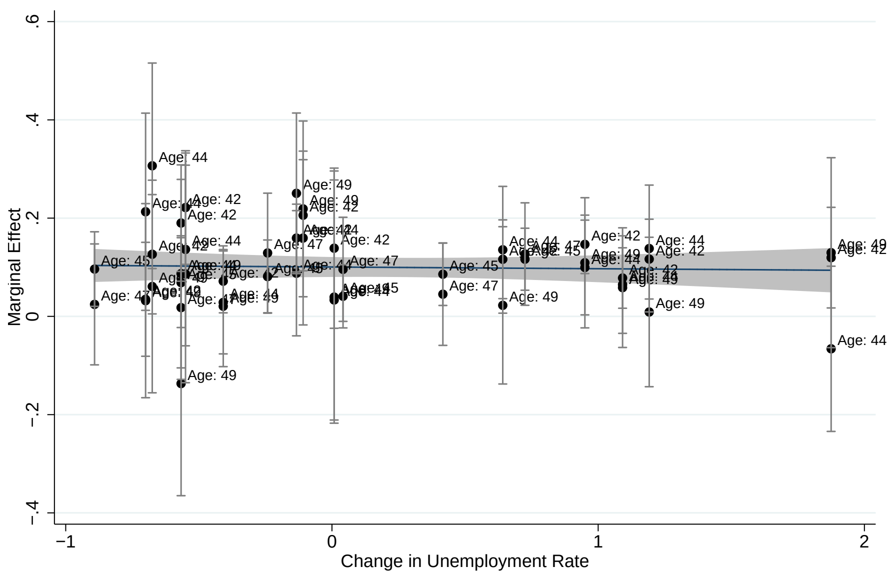
(a) Effect of Pot. UI Durations on Nonemployment Durations $\frac{dD}{dP}$ and the Unemployment Rate



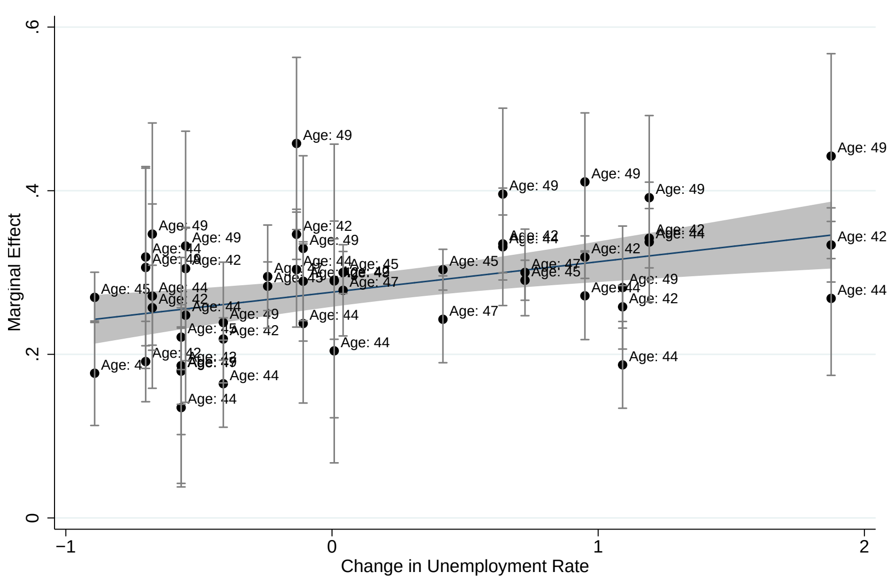
(b) Effect of Pot. UI Durations on Actual UI Durations $\frac{dB}{dP}$ and the Unemployment Rate

Notes: Each dot in the bottom figure corresponds to a rescaled marginal effect of one month additional potential UI duration estimated at an age cutoff in one year between 1987 and 2004 at the age 42 (before the 1999 reform) and the age 45 (after the 1999 reform) cutoff, where pot. UI durations increased from 12 to 18 months. Since the 1999 reform occurred during the year there are 2 estimates for 1999. The samples are described in Figures 2 and 4. The line shows the German unemployment rate in each year.

Figure 7: Variation in Regression Discontinuity Estimates of Marginal Effects of Potential Unemployment Insurance Duration with the Economic Environment



(a) Effect of Pot. UI Durations on Nonemployment Durations $\frac{dD}{dP}$ vs. Change in Unemployment Rate



(b) Effect of Pot. UI Durations on Actual UI Durations $\frac{dB}{dP}$ vs. Change in Unemployment Rate

Notes: Each dot in the bottom figure corresponds to a rescaled marginal effect of one month additional potential UI duration estimated at an age cutoff in one year between 1987 and 2004 at any of the available cutoffs (42, 44, 45, 47, and 49). The horizontal lines are the regression lines from a regression of the estimated marginal effects on the change in the unemployment rate from year t-1 to t. The samples are described in Figures 2 and 4.

Appendix

Table A-1: Means and Standard Deviations of Main Variables from German Social Security Data on Unemployment Insurance (UI) Spells from 1987 to 2004

	(1) Unemp. Insurance Spells 1987 to 2004	(2) As Column (1) but only Age 40 to 49	(3) As Column (1) but only max pot UI duration	(4) As Column (2) but only max pot UI duration
Panel A: Unemployment Variables				
Maximum UI benefit duration (imputed)	.	.	16.0	18.0
			[5.3]	[4.7]
Duration of UI benefit receipt in months	6.5	6.9	8.1	9.0
	[6.0]	[6.4]	[7.2]	[7.6]
Non-employment duration in months	14.5	14.7	16.7	17.3
	[13.9]	[13.9]	[14.6]	[14.5]
Duration until next job (censored 2008)	13.3	12.7	14.6	14.2
	[20.1]	[18.3]	[22.2]	[19.9]
Duration until next job if job within 36 months	8.0	8.1	8.4	8.9
	[8.4]	[8.4]	[8.6]	[8.8]
Time between end of job and UI claim	1.6	1.4	1.5	1.4
	[8.1]	[8.3]	[3.8]	[3.5]
Daily Post Unemployment Wage in Euro	54.5	53.9	62.5	62.2
	[26.4]	[26.2]	[29.0]	[29.5]
Post Wage - Pre Wage in Euro	-3.7	-4.4	-10.1	-11.4
	[24.8]	[24.3]	[27.7]	[27.9]
Log(Post Wage) - Log(Pre Wage)	-0.067	-0.079	-0.17	-0.19
	[0.48]	[0.47]	[0.50]	[0.50]
Switch industry after unemployment	0.62	0.60	0.70	0.70
	[0.49]	[0.49]	[0.46]	[0.46]
Switch occupation after unemployment	0.56	0.55	0.62	0.62
	[0.50]	[0.50]	[0.49]	[0.49]
Ever employed again	0.85	0.84	0.78	0.77
	[0.36]	[0.37]	[0.41]	[0.42]
Non-employment spell censored at 36 months	0.23	0.23	0.30	0.31
	[0.42]	[0.42]	[0.46]	[0.46]
Next job is fulltime employment	0.84	0.83	0.89	0.89
	[0.37]	[0.37]	[0.31]	[0.31]
Log(Wage) 5 years after start of UI	4.01	3.97	4.15	4.12
	[0.49]	[0.48]	[0.49]	[0.49]
Employed 5 years after start of UI	0.38	0.36	0.41	0.38
	[0.49]	[0.48]	[0.49]	[0.49]
Unemployed 5 years after start of UI	0.14	0.15	0.10	0.11
	[0.34]	[0.35]	[0.30]	[0.32]
Panel B: Pre-Determined Variables				
Daily Wage in Euro (Pre-UI for Col 2-4)	59.2	58.9	74.1	74.5
	[29.4]	[29.8]	[32.4]	[33.5]
Education years	10.9	10.8	11.0	10.9
	[2.30]	[2.20]	[2.31]	[2.32]
Female	0.42	0.43	0.35	0.34
	[0.49]	[0.49]	[0.48]	[0.47]
Non-German	0.082	0.078	0.089	0.096
	[0.27]	[0.27]	[0.28]	[0.29]
Actual experience (censored 1975)	10.7	10.6	12.2	13.5
	[8.49]	[8.49]	[5.64]	[6.15]
Years of firm tenure	2.58	2.58	6.14	6.65
	[4.60]	[4.60]	[5.29]	[5.72]
Years of occupation tenure (1-digit)	5.44	8.27	9.07	5.56
	[6.28]	[6.28]	[5.64]	[6.12]
Years of industry tenure (1-digit)	2.17	6.65	7.16	2.28
	[2.71]	[2.71]	[5.76]	[6.29]
Number of Spells	24593548	9315548	4983468	1990812

Notes: The table shows means and standard deviations (in brackets) for the main variables used in the analysis. The first column shows characteristics of all UI spells age 30 to 52 that started between July 1987 and December 2004 (witht the observation window running until December 2008). The second column restricts the sample to individuals age 40 to 49. Column (3) restricts the UI sample to individuals who have worked for at least 52 months since their last UI spell within the last 7 years without intermittent UI spell and thus are, with certainty, eligible for the maximum potential benefit durations. Column (4) restricts this sample further to Age 40 to 49, which is the sample used in the regression analysis. Wages are in prices of 2000.