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

One goal of extending the duration of unemployment insurance (UI) in recessions is to increase UI coverage in the face of longer unemployment spells. Although it is a common concern that such extensions may themselves raise nonemployment durations, it is not known how recessions would affect the magnitude of this moral hazard. To obtain causal estimates of the differential effects of UI in booms and recessions, this paper exploits the fact that, in Germany, potential UI benefit duration is a function of exact age which is itself invariant over the business cycle. We implement a regression discontinuity design separately for twenty years and correlate our estimates with measures of the business cycle. We find that the nonemployment effects of a month of additional UI benefits are, at best, somewhat declining in recessions. Yet, the UI exhaustion rate, and therefore the additional coverage provided by UI extensions, rises substantially during a downturn. The ratio of these two effects represents the nonemployment response of workers weighted by the probability of being affected by UI extensions. Hence, our results imply that the effective moral hazard effect of UI extensions is significantly lower in recessions than in booms. Using a model of job search with liquidity constraints, we also find that, in the absence of market-wide effects, the net social benefits from UI extensions can be expressed either directly in terms of the exhaustion rate and the nonemployment effect of UI durations, or as a declining function of our measure of effective moral hazard.

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I 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, particularly the United States, potential UI durations are regularly extended during recessions. Whether or not such countercyclical potential UI durations are socially beneficial is highly debated among economists (e.g. Needels and Nicholson 2004). 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 (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 (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. Some observers have argued that it is larger during a downturn (Ljungqvist and Sargent 1998, 2008) while others have suggested that it may instead be smaller (Krueger and Meyer 2002).[1]

Identifying the changing effect of UI extensions over the business cycle is difficult in a setting where UI extensions are endogenous to the state of the labor market.[2] To circumvent 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 (RD) design

[1]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, higher costs of job search may reduce the effect of UI parameters on labor supply and on the aggregate unemployment rate in recessions.

[2]In the United States, the trigger-based state-level extended benefits (EB) can be used together with the timing of discretionary federal temporary emergency unemployment compensation (EUC) to identify the effect of unemployment insurance duration on nonemployment. Rothstein (2011) exploits this stragegy to evaluate the effect of EB and EUC during the 2008 recession, and finds significantly negative but moderate effects. Card and Levine (2000) examine the effects of an extension in UI unrelated to local unemployment conditions in New Jersey, and also find more moderate effects on employment than previous studies based on EB and EUC in the 1970s and 1980s.

and data from Germany. Our strategy exploits the fact that the German UI system implies large differences in the potential 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 day-to-day administrative data on the universe of unemployment spells and ensuing employment outcomes for mature workers 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 *gains* 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 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.

We begin by using our RD strategy to obtain labor supply effects with respect to potential UI durations for large differential expansions for mature workers with stable labor force attachment. Our estimates imply a moderate rise in nonemployment of about 0.1 months for each additional month of potential UI benefit duration. This is robust across a number of alternative specifications. Furthermore, these effects on labor supply are similar for different 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 over the business cycle points to moderate, and for the most part statistically insignificant declines in the effects of a one-month increase in potential UI durations on nonemployment durations in large 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 – which is our summary measure of the disincentive effect of UI benefits that implicitly controls for changes in the probability of being affected by the extensions over the business cycle – is clearly countercyclical. The ratio indicates that, for each additional month in actual UI durations, the response of labor supply to UI extensions falls significantly in recessions. These results are robust to considering variation by year or year-by-industry, the use of alternative measures of the business cycle, holding characteristics of UI claimants constant, including workers with weaker labor-force attachment, and an extensive robustness analysis.

By using regression discontinuity estimates in different economic regimes to assess whether the duration of UI has stronger or weaker employment effects in booms and recessions and by explicitly analyzing the effects on benefit durations and exhaustion rates, the paper complements an earlier literature on cyclical effects of UI durations (Moffitt 1985, Jurajda and Tannery 2003) and related recent work on UI benefit levels using state-level differences in unemployment and UI parameters in the United States (Kroft and Notowidigdo 2010). Our estimates also add to existing studies of the labor supply effects of UI durations by combining large increases in UI durations and a large number of years and observations with a sharp regression discontinuity design (Meyer 1990, Katz and Meyer 1990, Hunt 1995, Lalive 2008, Rothstein 2011). In contrast to studies using region or time variation in UI durations, our RD design allows us to identify changes in the actual behavioral response, while holding constant any market-level factors that may change over time or across regions.

The paper is also related 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 (Baily 1978, Kiley 2003, Shimer and Werning 2007, Chetty 2008, Sanchez 2008, Kroft and Notowidigdo 2010). Our formula therefore also clarifies existing 'rules of thumb' regarding the optimal extension of UI benefits. According to our formula, the duration of UI should neither only be extended until the exhaustion rate is constant (Corson and Nicholson 1982), nor only extended to hold the nonemployment effect of UI constant (Moffitt 1985). Rather, setting optimal potential UI durations should take into account both factors and set durations according to the effective moral hazard parameter we propose. Since the welfare formula depends on the actual nonemployment and benefit response to UI durations influenced by market-level factors which our estimates hold constant (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 II, we derive the welfare effect of extensions in UI benefits. Section III describes the institutional environment in Germany, the administrative data, and our empirical approach. Sections IV and V contain our main findings regarding the effect of extended UI on labor supply and benefit duration over the business cycle. Section VI concludes, relating our empirical findings to a theoretical welfare formula, and providing suggestions for future research.

II 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 $t = 0$. In each period, the worker decides how intensely to search for a job. Let s_t denote search intensity, which is normalized 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, which is 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 assumption is easily relaxed). 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 three periods: a period of receiving UI benefits ($B \equiv \sum_{t=0}^{P-1} S_t$), a period of nonemployment without receiving UI benefits ($D-B$), and a period of employment ($T-D$).

Welfare Effect of UI Extensions. Assuming the social planner sets taxes to achieve a balanced 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.[3] Social welfare at time $t=0$ is given as W_0, the expected lifetime utility of an unemployed worker.[4] 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

[3]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).

[4]Focusing on the welfare of a worker who just lost his job loses no generality. Alternatively, one can assume that, at t=0, only a fraction of individuals are unemployed. In this case, social welfare, W_0, represents the expected average utility of the employed and the unemployed. While this makes the exposition more notationally heavy, it does not affect the welfare results (see Web Appendix for more details). 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.

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

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 the model here, 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 multiplied by their marginal utility. Taxes rise because the unemployed lower their search intensity and this thus increases their receipt of UI benefits $\left(\frac{dB}{dP}\big|_2 \times b\right)$. They also increase 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 smooths 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 ($\frac{B}{T-D}b$ multiplied by 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.[5] 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 IV.B, 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 is changes in individual incentives affecting search intensity (the partial-equilibrium or micro effect of UI extensions). In the working paper version of this paper, we showed 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, possibly 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 due mainly to differences in incentives. In Section IV 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. However, we find that the cyclical variation of the exhaustion rate,

[5] To be more precise, the marginal utility of employed individuals who are affected by a marginal increase in taxes can be considered constant 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 earning losses in recessions for job losers, the fluctuations in earning trajectories, and hence expected marginal utility, of the average employed taxpayer are typically weak (von Wachter, Song, and Manchester 2009).

$\frac{dB}{dP}\big|_1$, is the main contributor to the variation in $\frac{dB}{dP}$ over time. On the other hand, $\frac{dB}{dP}\big|_2$is driven by the shift in the survivor function before the exhaustion point and thus has nearly the same cyclical variation as $\frac{dD}{dP}$, while at the same time having very little impact on the variation in $\frac{dB}{dP}$. Thus, in the discussion of our main results, we will focus on the properties of $\frac{dB}{dP}$ and $\frac{dD}{dP}$. In fact, for the case of a constant hazard of leaving nonemployment (i.e., $s_t = s$), one can show that $\frac{dB}{dP}\big|_2$ is proportional to the nonemployment effect: $\frac{dB}{dP}\big|_2 = \frac{dD}{dP}\xi$, where $\xi \equiv \left(1 - Ps(1-s)^{P-1} - (1-s)^P\right)$ is a constant. Furthermore, in this case, the welfare effect of extensions in potential UI durations indeed depends only on the two parameters $\frac{dB}{dP}$ and $\frac{dD}{dP}$ and one can thus derive 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 $\Omega \equiv \xi u'(c_P^u) + \frac{B}{T-D} E_{0,T-1} v'(c_t^e) > 0$. This formula indexes the welfare gain by the effect of potential benefit duration 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 close to constant (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 multiplied by the benefit level ($\frac{dB}{dP} b$). The term $\frac{dB}{dP} b$ measures how many additional dollars of UI benefits are paid for an additional month of potential UI durations. Dividing $\frac{dW_0}{dP}$ by $\frac{dB}{dP} b$ thus yields the marginal welfare gain (in dollars) from spending one additional dollar on UI expenditures. The approximate welfare formula becomes

$$\frac{dW^*}{dP} \equiv \frac{dW_0}{dP} \Bigg/ \left(\frac{dB}{dP} b\right) = \left[u'(c_P^u) - E_{0,T-1} v'(c_t^e)\right] - \frac{dD}{dB}\Omega, \tag{3}$$

where $\frac{dD}{dB} = \frac{\frac{dD}{dP}}{\frac{dB}{dP}}$. This equation captures the welfare gain in dollars from an extension of UI benefits *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.[6] Second, our welfare formula in equation (3) is similar in spirit to those in Baily (1974) and Chetty (2008), which contain the elasticity of nonemployment with respect to benefit levels as a single measure of the disincentive effect. Our measure of the disincentive effect, $\frac{dD}{dB}$, and the nonemployment duration elasticity with respect to benefit levels or durations, provide alternative normalizations for the number of individuals 'at risk'. From this point of view, our measure is well-suited for comparisons of the effect of UI over time since it more directly normalizes for changes in UI benefit take-up. Hence, in parallel to the prior literature, *ceteris paribus*, a decline in the disincentive 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 III, we cannot obtain the rescaled marginal effects $\frac{dB}{dP}$ and $\frac{dD}{dP}$ separately. Therefore, we

[6] The IV estimator can be interpreted as a local average treatment effect (LATE), 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.

will also report estimates of this ratio in our empirical analysis.

III 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 (RD) 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 RD design over multiple years.

III.A The Unemployment Insurance System in Germany

The German unemployment insurance system provides income replacement to eligible workers who lose their job without fault. The replacement rate is fixed, as is the duration of disbursement. For an individual without children, the replacement rate is 63 percent of previous net earnings.[7] From the 1980s onwards, the maximum duration of benefits was tied to recipients' exact age at the beginning of the UI spell and 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 I shows the discontinuities in potential benefit duration by age when claiming for the group of workers who, given their employment histories, 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 between 42 and 43, potential UI duration

[7] Workers become eligible to receive UI benefits if they have worked for at least 12 months in the previous three years. Workers who lose their jobs because of their own fault or quit are also eligible for UI benefits after a waiting period of 12 weeks which are subtracted from their potential UI durations. 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. 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. We provide more information on the institutional details in the Web Appendix.

increased to 18 months; for workers from 44 to 48 (49 to 53), the maximum duration further rose to 22 (26) months. As further explained below, in our main analysis, we restrict ourselves to workers in this age range who qualify for the maximum potential durations to obtain precise measures of potential UI durations. At the end of the 1990s, a reform occurred which was meant to reduce potential disincentive effects of unemployment insurance. As shown in Figure I, starting in April 1999, the potential UI durations were lowered and the age thresholds were shifted upwards by three 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.[8]

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 percent, 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 percent more than her, the UA benefits are zero. Given that about 80 percent of individuals in our cohort and age range are married, UA benefits are, on average, about 35 percent for men and 10 percent for women. Among all new UI spells in our sample, about 10 to15 percent end up taking UA benefits. We study the potential effect of UA on our findings in our empirical analysis.

III.B 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 period of employment in a job covered by social security and every period of receiving 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

[8] 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 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 last Hartz reform took place.

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 employer characteristics.[9]

To study the effect of extensions in potential UI durations, 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. This yields over 9 million individual periods of unemployment. (Column 2 of Table A-1). For each period of nonemployment , we created variables about the previous work history (such as job tenure, experience, wage, industry and previous occupation), 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 (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 before 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 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 observation in our sample, we use information about the law in the relevant time periods together with information on exact dates of birth and work histories. This yields precise 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

[9] 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 a majority of individuals.

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 seven 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.

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 reemployment 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-14), 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 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 US Displaced Worker Survey (DWS) we analyzed. In the DWS, among 40 to 49 year old displaced workers who have received UI , about 15 percent are still not employed after three years, a figure comparable to Germany, where the fraction of individuals whose spell is censored at 36 months is 23 percent.[10]

III.C 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 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.[11] 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 two 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 outcomes.

[10] For more information regarding data construction and the comparison with the US, see the Web Appendix.

[11] There is a fourth 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.

All results are robust to an extensive sensitivity analysis summarized in Section V.

III.D Identification Assumptions

The identification assumption of the RD design requires that all factors other than the treatment variable 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 for assessing the identification assumption is to test for discontinuities in observable characteristics at the threshold by estimating equation (4) with observable characteristics as outcome variables. Table I presents results of these regressions using two-year bandwidths around the cutoffs. Of the 21 coefficients in Table I, 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 thresholds, however the magnitudes are still 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 RD assumption is to look at the smoothness of the density of unemployment spells around the cutoffs. Figure II (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-year and 49-year 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 III 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 after the threshold have waited longer to claim than the people who had claimed before 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. We show in the Web Appendix (Section 3.5) that the presence of UA benefits approximately reduces the months worth waiting by half. In combination with behavioral explanations, this may be the reason for the small amount of waiting we observe. To directly assess the potential impact of waiting, we also re-estimated our models after dropping UI spells with longer gaps between job ending and claiming (Web Appendix Table W-4 and Table V). This had no effect on the results.

To assess whether firms selectively lay off workers eligible for higher benefit durations, in Figure II (b) we show the density of spells with respect to the dates the previous job 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 immediately to the right of the 42-year and 49-year cutoffs, but less clearly as in Figure II (a). If anything, this could indicate that firms are waiting for a short time to lay off workers until they are eligible to higher UI benefit levels. However, it does not appear that firms are systematically more likely to lay off workers with higher levels of UI benefits, since, in such a 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 replicate our findings with new UI spells that originate from employers experiencing multiple layoffs who hence have less scope for selectively laying off workers (Web Appendix Tables W-16 and W-17). 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. As an additional, conservative approach to assessing the impact of this change in density, we calculated bounds on our coefficients. For this purpose, we picked a number of workers equal to the excess mass in the density above the threshold and reassigned them to the left hand side of the threshold. To simulate the extreme case that workers with long potential nonemployment spells selectively deferred filing claims in order to receive higher UI durations, we purposefully reallocated those workers with the highest nonemployment durations in our sample. This provided very conservative lower bounds of the treatment effect. We used a symmetrical approach to obtain upper bounds of the effect (assuming the excess mass is selected 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.

IV THE EFFECT OF UI EXTENSIONS ON LABOR SUPPLY OVER THE BUSINESS CYCLE

IV.A 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 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 II.

Figure III (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 when claiming UI are eligible to 12 months of UI benefits, of which they use an average of 6.7 months. At the 42-year 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. They range from one fourth (at the 44-year cutoff) to one third (at the 49-year 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 III (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 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 II. Our main regression results in column (1) are 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), at age 44, the increase is 0.41 months, and at age 49, the increase is

0.43 months.[12] 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 ages 42, 44, and 49, respectively). This suggests 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. They range between 0.12 and 0.13 (see Appendix Table W-5).

After the reform of the UI system, the eligibility thresholds for extended UI were shifted to ages 45 and 47 starting in 1999. Figure IV 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 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.[13]

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 additional month of potential UI benefit duration, Table II implies that, on average, individuals remain on UI benefits for 9 to 12 additional days, but in

[12] 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.

[13] From Figure IV 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 IV.B 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 III.D apply.

nonemployment for only an additional three days . This means that a significant fraction of workers remain in nonemployment at benefit exhaustion and are thus directly benefiting from an extension of potential UI durations. For example, about 28 percent of unemployed individuals in our analysis sample to the left of the age 42 cutoff, i.e. 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 eight percent return to employment, while the majority enter 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 V displays non-parametric regression discontinuity estimates of the hazard of exiting nonemployment by duration for individuals with 12 and 18 months of potential UI benefits. Consistent with previous studies (Meyer 2002), the Figure shows 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 substantially in advance of exhausting their UI benefits (Card, Chetty, and Weber 2007a). These findings imply that our main effects reported in Table II are averages of behavioral responses along the entire duration distribution.

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 III.D. The bounds are relatively narrow, with both lower and upper bounds 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 II. Given that the economic incentives to wait are 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 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 increase between two and six months at the age thresholds, depending on the employment history. The unrestricted sample's RD estimates for duration of UI receipt and nonemployment , shown in columns (2) and (3) of Table III, are smaller than the main sample's . Since the 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 III, 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 II and III 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 II, this ratio is effectively an IV estimator of the effect of UI duration on nonemployment. This ratio is quite similar for both our main sample and the unrestricted sample.

Discussion. Our results are consistent with the theoretical model we discussed in Section II. The model predicts that a rise in the potential duration of UI benefits encourages individuals to lower their search effort (s_t). Consistent with our finding in Table II, 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 explained by a behavioral response. An important part of increased coverage is due to individuals who would otherwise have exhausted benefits while remaining in nonemployment continuing to receive UI benefits. 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 IV.B and the Conclusion.

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

Germany, Austria, and, with some caveats, 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.[14] 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, as was found in studies examining the United States. Our main estimates of a marginal effect 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 (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 V.B.

[14]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, difference-in-difference estimates would be equal to RD estimates in our case, 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.

IV.B 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 labor market conditions. Using the large samples in our data, we replicated the regression discontinuity estimates for the 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 chose their search intensity, including at the moment of benefit exhaustion. Due to high inter-temporal correlations of unemployment rates this is, however, not feasible. 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 first year of a worker's UI spell.[15]

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 indicate a weak decline in the effect of extended UI on nonemployment in recessions. In contrast, 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, the nonemployment response to actual UI durations is clearly countercyclical.

The first panel of Figure VI plots the rescaled marginal effects of a one-month increase in potential UI on nonemployment duration over time. The estimates are obtained by replicating the RD estimates separately for each calendar year for the threshold at age 42 (and age 45, after the 1999 reform). This yields the most precise estimates for the effects. The unemployment rate shows

[15] 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. 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. These results are quite similar and available in the Web Appendix.

how the German economy exhibited large economic swings during the 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, there appears to be no clear systematic variation with the business cycle. In the first panel of Figure VII 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 VII (a) are extended and confirmed in column (2) of Table IV. 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 statistically insignificant correlation between the labor-supply effect of UI extensions and the business cycle, indicating that the disincentive effect tends to fall when the economy contracts. The second row uses 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, thus its representation of labor market conditions for newly unemployed workers would be imperfect. 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 the main independent variable, whereas row 4 shows the annual mass-layoff rate at the establishment level,(Schmieder, von Wachter and Bender 2009). 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 these estimates might be that they are purely based on the time-series variation in economic conditions shown in Figure VI (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 inflow. 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 VI (b), Figure VII (b) and Table IV replicate the same analysis for the effect of potential duration of UI benefits on the actual duration of UI benefits. Contrary to our previous findings, it now appears that the effect on actual UI durations is significantly countercyclical. The lower panel of Figure VI 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 VII shows that there is a positive correlation between the change in the unemployment rate (which captures 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 likewise strong correlation between current inflows and the future level unemployment.

These graphical findings are confirmed in column (3) of Table IV, where we assess the correlation of the response in UI duration to benefit extensions using the same range of alternative business cycle measures 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 leads to increases in the marginal effect of about 25 to 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 VI (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 columns (5) and (6) of Table IV. 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.[16] 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 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, 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 thus 'normalized' disincentive effect is shown in column (4) of Table IV. Unlike the marginal effect $\frac{dD}{dP}$, this normalized disincentive effect is strongly countercyclical. Out of the six regressions, it is statistically significantly correlated with five 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

[16]The nonemployment and UI survivor functions diverge somewhat, since individuals leave the UI system without taking up employment even before their benefits expire. For the regressions in Table IV, we compute the UI exhaustion rate $\frac{dB}{dP}\big|_1$ and increase in benefit duration prior to the exhaustion point $\frac{dB}{dP}\big|_2$ using the actual UI survivor functions. 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).

measures, except for the level of the unemployment rate. However, the effect of the level of the unemployment rate is now of the right sign.

Our main findings in Table IV are very robust and in some cases stronger when we consider important extensions in Table V. To save space, Table V only shows the results when using the level and change in unemployment rates as our measures of cyclical conditions, and relegate the estimates for additional measures to the Web Appendix (Table W-15). The first column of the table replicates our main findings in Table IV 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 IV), is now also significantly, negatively correlated with the business cycle.[17]

The second column shows the lower bound discussed in Section III.D, 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 had 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 III 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 IV.

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

[17]This suggests that the elasticity is another possible way to rescale the marginal effect by the intensity of treatment as suggested in Section II. 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.

it not to affect our results. We analyzed cyclical variation in two summary indices of observable characteristics in our data, the predicted propensity to receive unemployment assistance (UA) and 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 the distribution of characteristics constant across years. This is shown in column (4) of Table V 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 V show results when we include all workers, irrespective of labor market experience. As discussed in Section III.B, we cannot calculate rescaled marginal effects for this sample. Nevertheless, the ratio of marginal effects has the same interpretation as it did 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-15) and quite similar and more precisely estimated when we use a bandwidth of two years. Hence, our main results hold 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 significantly different from zero. However, these findings show that the effect of a rise of actual benefit duration on nonemployment duration, which effectively controls for any increases in workers 'at risk' of being affected by extended UI durations during recessions, has a clear and precisely-estimated countercyclical pattern.

Discussion. To see how our empirical results relate to the theory we outlined in Section II, 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 (the partial derivative of search intensity with respect to an increase in assets in the period of UI exhaustion P) and the 'substitution' effect ($-\partial s_P / \partial w_P$, the partial derivative of search intensity with respect to the wage in period P) of a UI extension for workers exhausting UI benefits.[18] 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: the effective exhaustion rate ($\frac{dB}{dP}$), the nonemployment effect ($\frac{dD}{dP}$), and the relative strength of the liquidity vs. substitution effect of UI benefits.

As mentioned in Section II, 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 II. 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 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 during recessions requires further information.

[18] 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 lifetime 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.

V ROBUSTNESS

Our main results are robust to a number of alternative specifications, which are briefly summarized in this section. Additional details are relegated to the Web Appendix.

V.A Robustness Analysis

Choice of Bandwidth. We investigated whether the choice of bandwidth for the RD estimator affects our conclusions (Table II). Using a bandwidth of two 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. This is true 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 two-year bandwidths. As discussed in Section IV.B, our estimates in column (1) of Table V also imply that our results regarding cyclicality are robust to choosing a narrower bandwidth.

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 III column (4) 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 two other 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-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.[19]

Differences by Subgroups. To further examine the robustness of our main estimates, we estimated the labor supply effects for several subgroups. 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 (See Web Appendix). While we found some small differences in the labor supply response to UI extensions, overall the labor supply effects are remarkably robust throughout the population.

Robustness of Differences over Cycle. We estimated many additional specifications to those reported in Section IV.B 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 IV. One potential concern with our main estimates in Table IV 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 seemed to decrease in worsening times (Web Appendix Table W-10). Alternatively, we estimated a cox-proportional hazard model in the spirit of Meyer (1990) and found a slight decline in the predicted labor supply elasticities when unemployment was increasing (Web Appendix Table W-11). We also estimated linear and log-linear models that pooled the effect of UI extensions across our different age-thresholds while flexibly controlling for age (Web Appendix Table W-12). Again, the changes over time were relatively small, with, at best, weakly negative coefficients on the interaction of potential UI duration and business cycle indicators.

[19]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.

V.B 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, basic search theory predicts that that our relatively modest effects would over-predict the effect relative to a system with lower replacement rates. 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 III, 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 the 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 primarily driven by individuals entering UA after exhausting benefits, we should see significant disparities here. About 10 to 15 percent of UI beneficiaries and 50 percent 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.[20] As reported in Web Appendix Table W-18, the 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 percent). If we include an interaction with the individual propensity and extrapolate linearly, for individuals with propensity of receiving UA close to 80 percent, the rescaled marginal effect is below 0.05. Yet, even for those whose propensity is 20 percent, 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

[20]The corresponding linear probability model is shown in the Web Appendix Table W-9, 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..

labor supply effects we find.

VI 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 economic conditions. Our findings indicate a modest effect of a one-month extension in UI durations on nonemployment durations of comparable magnitude to what has been found before. This effect is quite stable over 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 – is significantly counter-cyclical.

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 nonemployment effect of UI, which leads to an increased tax burden for the employed. This result clarifies the notion 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. 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 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 business cycle, such as Germany and most other European countries, may raise welfare by moving to a system with countercyclical potential UI durations.

Whether these conditions hold are 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. 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 (as in Ljungqvist and Sargent 1998) or if UI extensions reduce the incentives to create vacancies, then our estimates may represent upper bounds.

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Tables

Table I: 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 each age threshold. Each coefficient is estimated in a separate RD regression that controls linearly for age using different slopes on each side of the cutoff and bandwidth of two age years. 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 II: 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 42-year discontinuity, potential UI benefit durations (P) increase from 12 to 18 months, at the 44-year discontinuity from 18 to 22 months and at the 49-year 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 spells. For the age 49 cutoff and two-year bandwidth column, the regression only includes individuals between 47 and 50 years of age, due to an early retirement discontinuity at age 50.

Table III: 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) UI Duration No Exp. restrictions	(3) Nonemp Duration No Exp. restrictions	(4) Ever emp. again
D(age>=42)	-0.00089 [0.020]	0.98 [0.016]**	0.45 [0.036]**	-0.01 [0.0022]**
Observations	452749	2467954	2467954	452749
D(age>=44)	0.016 [0.021]	0.46 [0.019]**	0.21 [0.036]**	-0.0056 [0.0024]*
Observations	450280	2293865	2293865	450280
D(age>=49)	-0.0027 [0.025]	0.76 [0.032]**	0.40 [0.050]**	-0.0076 [0.0036]*
Observations	329680	1550099	1550099	329680

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) and (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 (2) and (3) 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.

Table IV: 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		$\frac{dD}{dP}$	$\frac{dB}{dP}$	$\frac{dD}{dP} / \frac{dB}{dP}$	$\left.\frac{dB}{dP}\right\vert_1$	$\left.\frac{dB}{dP}\right\vert_2$	$\eta_{D,P}$
Dependent Variable Varies by Year and Age-Threshold							
Real GDP Growth from Year t-1 to t	2.15 [1.58]	0.0078 [0.0065]	-0.012 [0.0081]	0.045 [0.023]†	-0.011 [0.0055]†	-0.00086 [0.0048]	0.011 [0.0071]
National Unemployment Rate in Year t	9.09 [1.64]	-0.0099 [0.0063]	-0.0053 [0.0084]	-0.032 [0.024]	-0.00059 [0.0061]	-0.0047 [0.0046]	-0.011 [0.0070]
Change in Unemployment Rate from Year t-1 to t	0.13 [0.78]	-0.012 [0.013]	0.038 [0.014]*	-0.10 [0.042]*	0.029 [0.0093]**	0.0086 [0.0091]	-0.019 [0.014]
Fraction of Establishments with Mass Layoffs in Year t	1.31 [0.53]	-0.022 [0.020]	0.059 [0.022]*	-0.17 [0.065]*	0.048 [0.014]**	0.010 [0.015]	-0.035 [0.022]
Dependent Variable Varies by Year and 1-digit Industry and Age-Threshold							
Job Destruction Rate in 1-digit Industry from Period t-1 to t	0.090 [0.032]	0.52 [0.49]	1.39 [0.28]**	-2.32 [1.20]†	1.25 [0.17]**	0.14 [0.27]	0.26 [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 Quintile of 2-dig. Ind. Wage Change	-0.047 [0.079]	-0.033 [0.15]	-0.56 [0.093]**	0.98 [0.47]*	-0.62 [0.047]**	0.063 [0.089]	0.089 [0.17]
Mean of Dep Var		0.10	0.28	0.36	0.44	-0.15	0.11

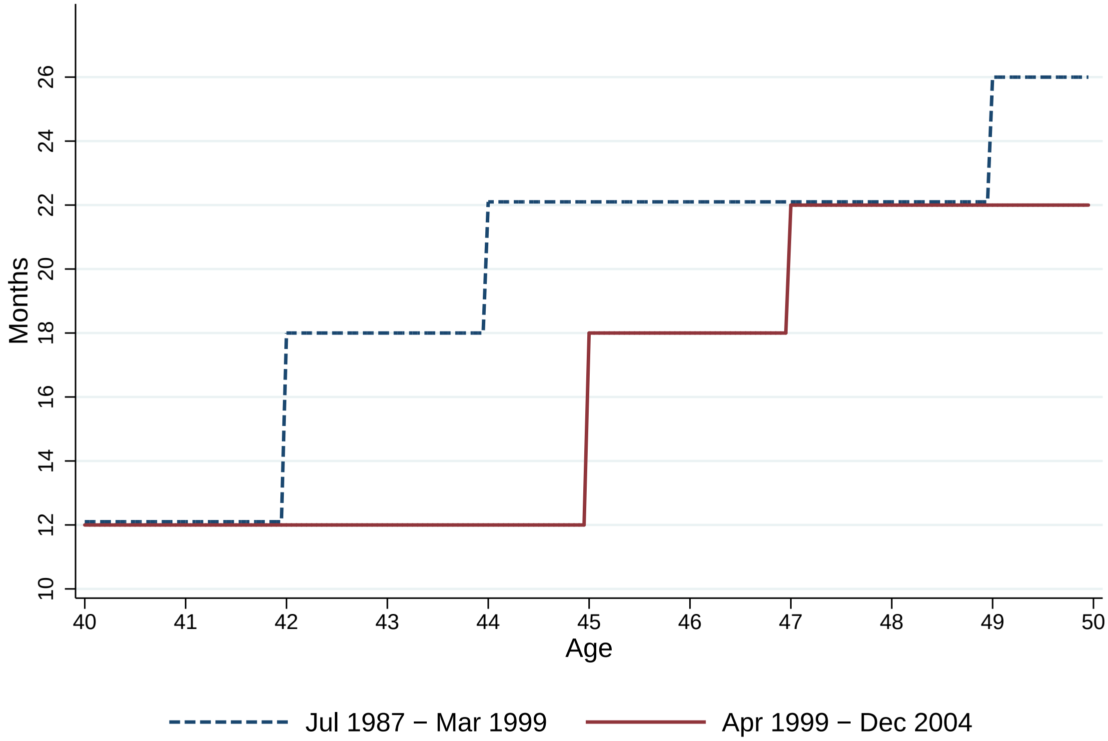
Notes: Stars indicate confidence levels: †P<.1, * P<.05, ** P<.01.
Columns (2)-(7) report coeffients from a 2 step regression. In the first step the effect of Extended UI durations on nonemployment durations are estimated separately for all years and age thresholds using the regression discontinuity 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 30% drop in employment over a year. The rate is calculated among all establishments with at least 50 employees in the baseline year. The regressions for each coefficient in rows 1-4 are at the year times age threshold level and have 51 observations in the second stage. The regressions in row 5 are at the year times age times 1-digit industry level and have 450 observations in the second stage. For the regressions in row 6 we first compute average wage losses of people who become unemployed at the 2 digit industry times year level. We then assign industries to quintiles of the average wage change within each year and estimate the marginal effects at the year times age times quintile level. The second stage regresses these estimates against the average log-wage change in the quintiles and is based on 240 observations.

Table V: 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) No Experience Restrictions Bandwidth 2 Years	(6) No Experience Restrictions Bandwidth 1 Year
Nonemp. Duration Marginal Effect: $\frac{dD}{dP}$						
National Unemployment Rate	-0.019	-0.011	-0.012	-0.014	–	–
in Year t	[0.0065]*	[0.0057]†	[0.0096]	[0.011]		
Change in Unemployment Rate	-0.019	-0.022	-0.0100	-0.020	–	–
from Year t-1 to t	[0.015]	[0.011]†	[0.020]	[0.022]		
UI-Benefit Duration Marginal Effect: $\frac{dB}{dP}$						
National Unemployment Rate	-0.0059	-0.0044	-0.0060	-0.0049	–	–
in Year t	[0.0093]	[0.0087]	[0.0092]	[0.0090]		
Change in Unemployment Rate	0.042	0.040	0.040	0.036	–	–
from Year t-1 to t	[0.015]*	[0.014]*	[0.015]*	[0.015]*		
Nonemp. Duration scaled by UI-Benefit Duration Marginal Effect: $\frac{dD}{dB} = \frac{dD}{dP} / \frac{dB}{dP}$						
National Unemployment Rate	-0.059	-0.039	-0.026	-0.037	-0.00095	-0.039
in Year t	[0.024]*	[0.026]	[0.029]	[0.026]	[0.027]	[0.027]
Change in Unemployment Rate	-0.13	-0.12	-0.099	-0.12	-0.059	-0.12
from Year t-1 to t	[0.045]*	[0.045]*	[0.053]†	[0.045]*	[0.051]	[0.047]*
Nonemp. Duration Elasticity : $\eta_{D,P}$						
National Unemployment Rate	-0.022	-0.012	-0.015	-0.014	–	–
in Year t	[0.0077]*	[0.0067]†	[0.012]	[0.0098]		
Change in Unemployment Rate	-0.029	-0.028	-0.021	-0.026	–	–
from Year t-1 to t	[0.017]	[0.013]*	[0.025]	[0.019]		

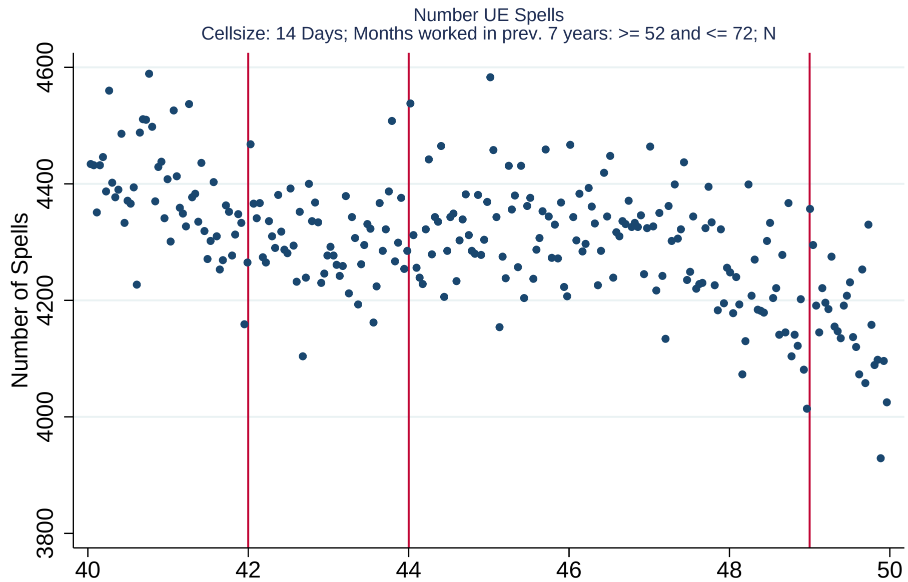
Notes: Stars indicate confidence levels: †P<.1, * P<.05, ** P<.01.
The specifications correspond to the first 4 rows in Table 4. Each coefficient is from a separate regression and based on the same 2 step method as before. The unit of observation in the second stage is the RD coefficient in 51 age-threshold 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.

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

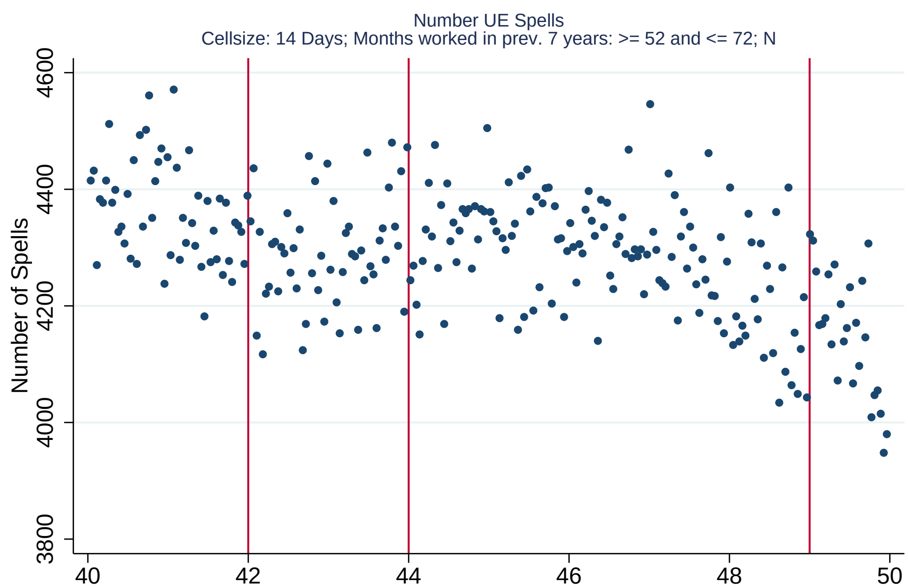


Notes: The figure shows how potential unemployment insurance (UI) durations vary with age and over time for unemployed individuals workers who had worked for at least 52 months in the last 7 years without intermittent UI spell.

Figure II: 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

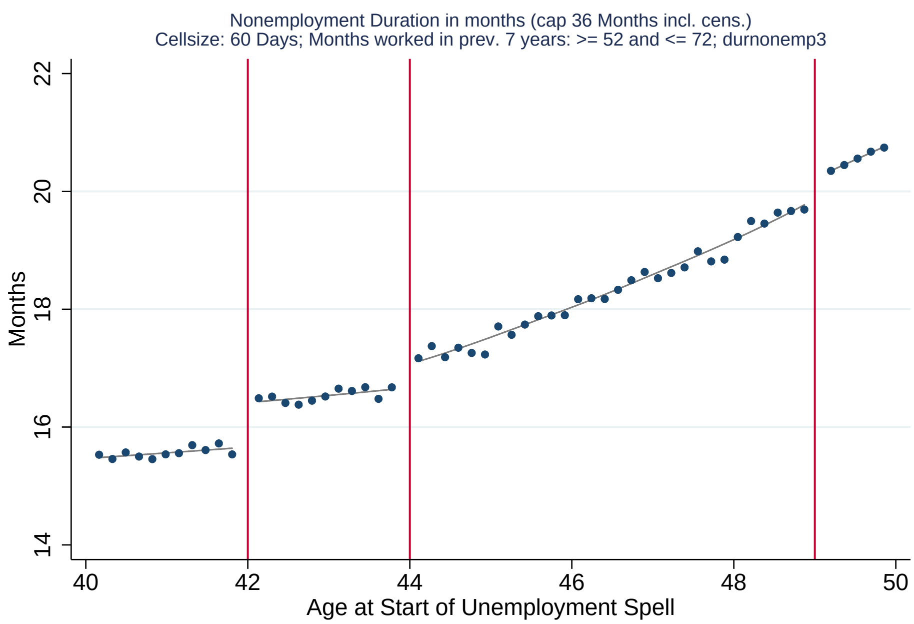


(b) Age at Job Loss

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 is composed of unemployed workers 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 III: 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

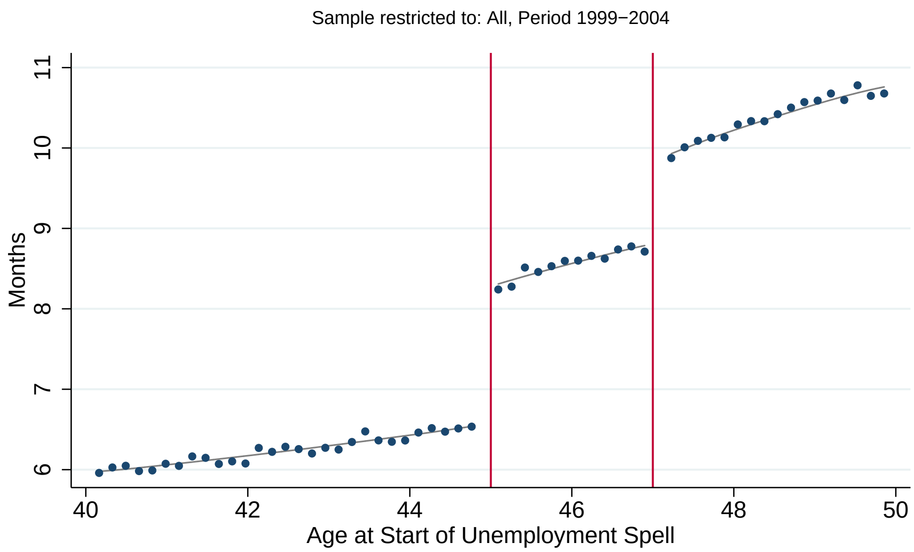
(a) Actual UI Duration



(b) Nonemployment Duration

Notes: The top figure shows average durations of receiving UI benefits by age at the start of receiving unemployment insurance. The bottom figure shows average non-employment durations for these workers, where nonemployment 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 is composed of unemployed workers 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 IV: 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

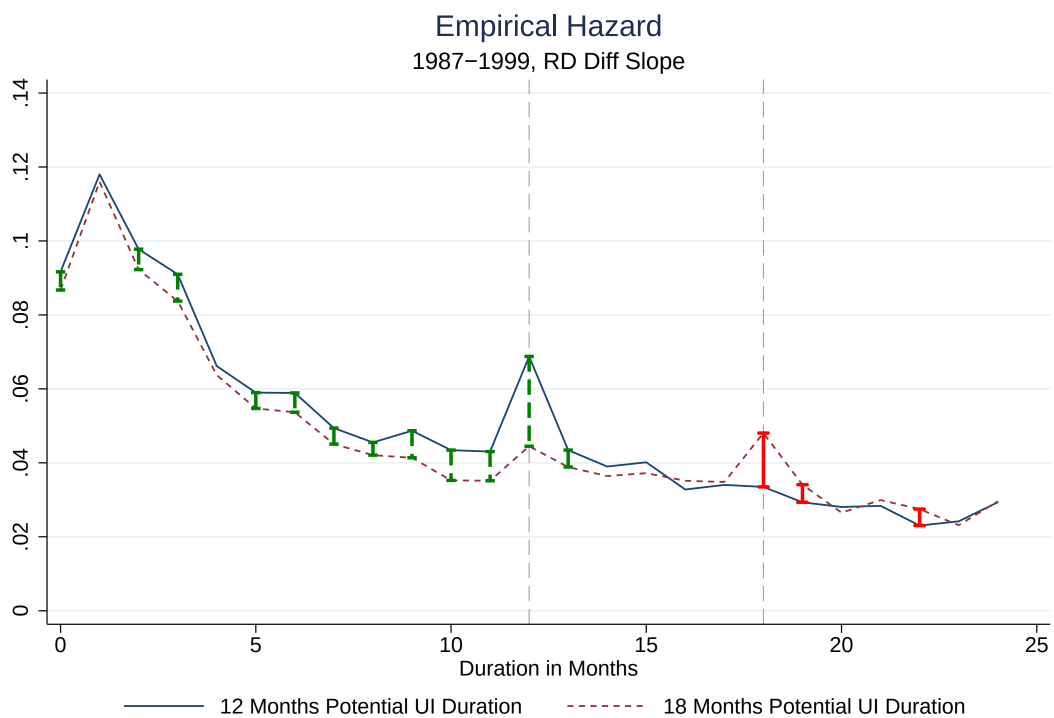


(a) Actual UI Durations

(b) Non-employment Durations

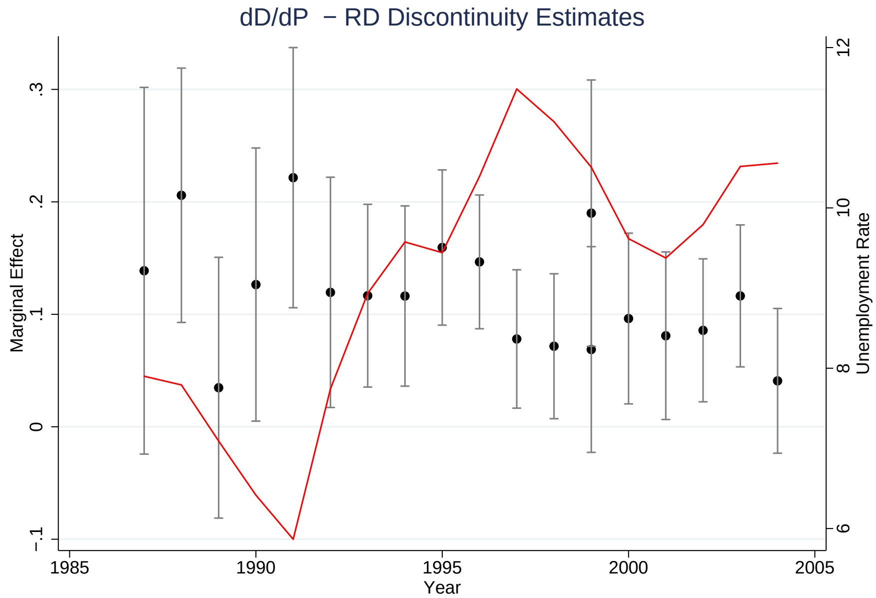
Notes: The top figure shows average durations of receiving UI benefits by age at the start of receiving unemployment insurance. The bottom figure shows average nonemployment durations for these workers, where nonemployment duration is measured as the time until return to a job and is capped at 36 months. 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 is composed of unemployed workers 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 V: The Effect of Increasing Potential Unemployment Insurance (UI) Durations from 12 to 18 Months on Hazard Function - Regression Discontinuity Estimate at Age 42 Discontinuity

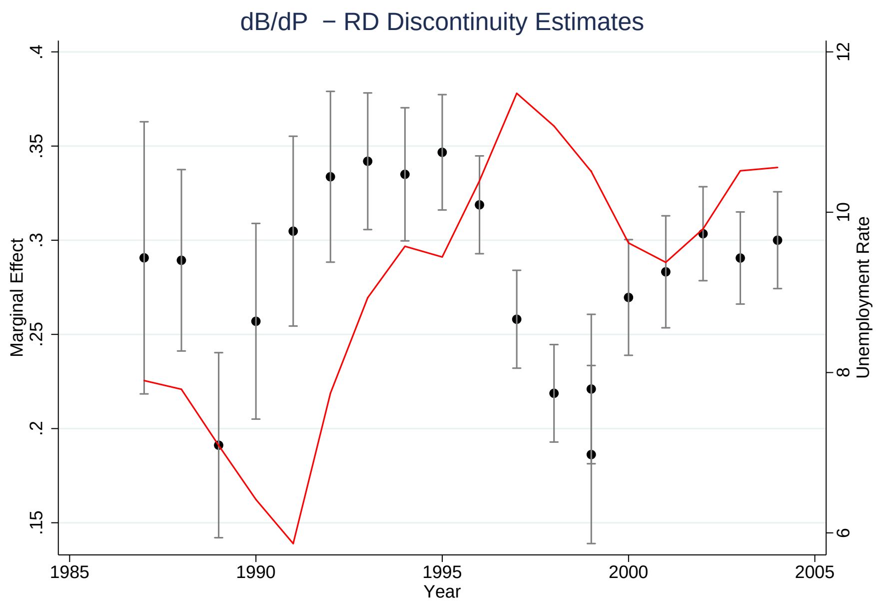


Notes: The difference between the hazard functions is estimated pointwise at each point of support using regression discontinuity estimation. Vertical bars indicate that the hazard rates are statistically significant from each other at the five percent level. The sample is composed of unemployed workers 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 VI: Variation in Regression Discontinuity Estimates of Marginal Effects of Potential Unemployment Insurance Duration at the Age 42 and Age 45 Discontinuities over Time



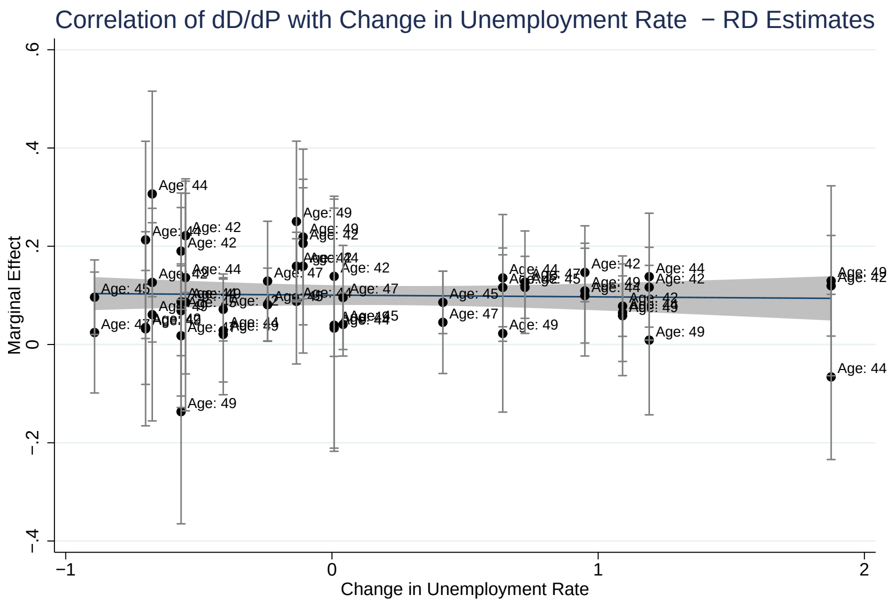
(a) Effect of Pot. UI Durations on Nonemployment Durations $\frac{dD}{dP}$ and the Unemployment Rate



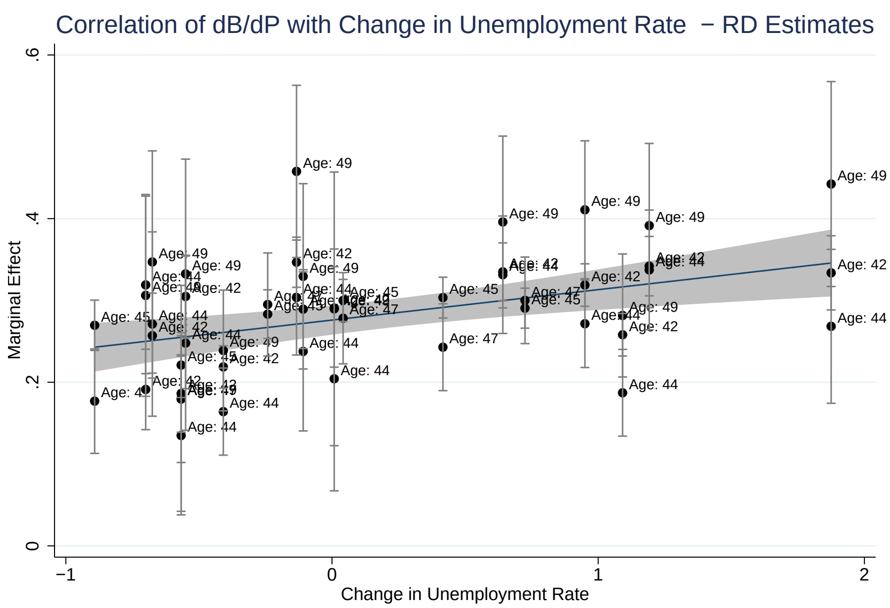
(b) Effect of Pot. UI Durations on Actual UI Durations $\frac{dB}{dP}$ and the Unemployment Rate

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 II and IV. The line shows the German unemployment rate in each year.

Figure VII: 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 II and IV.

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 year 2000 prices.