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

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Abstract: A common policy in the United States is to increase the duration of unemployment insurance (UI) benefits in recessions. Theory suggests that the optimal duration of the extension should depend on the exhaustion rate of benefits and on the size of the effect of UI benefits on non-employment. Since in the United States benefit duration varies with the business cycle, it is difficult to estimate how the exhaustion rate or non-employment effects of UI vary with the business cycle. In this paper, we exploit the fact that the duration of UI benefits in Germany is a function of exact age that is invariant over the cycle. Using the universe of unemployment spells and career histories we implement a regression discontinuity strategy separately for twenty years and across industries and correlate our estimates with measures of the business cycle. The non-employment effects of UI extensions we find are at best somewhat declining in large recessions. Yet, the UI exhaustion rate, and therefore the additional coverage provided by UI extensions, rises substantially during a downturn. We derive a new welfare formula in a model of job search with liquidity constraints that links the net social benefits from UI extensions to the exhaustion rate and the disincentive effect of UI. Together with this result our empirical findings imply that the optimal UI benefit duration rises with the exhaustion rate.

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1 Introduction

An often used policy tool in the United States to ease the hardship of job losers in recessions is to increase the duration of unemployment insurance (UI) benefits. In each major downturn since 1975, extensions of UI benefit durations occurred at the state and federal level, reaching up to 99 weeks in 2010.[1] The effects of these UI extensions and hence their social benefit are typically debated among economists.[2] One justification for increases in UI durations is that absent the extensions a large fraction of recipients would exhaust benefits and experience significant declines in consumption (e.g., Gruber 1997, Browning and Crossley 2001, Congressional Budget Office 2004). However, a long literature suggests that extensions in UI durations entail a cost in terms of a rise in the length of non-employment (e.g., Solon 1979, Moffitt 1985, Katz and Meyer 1990, Meyer 1990, Hunt 1995). As of now, there is no clear consensus how this disincentive effect changes during recessions, with some observers arguing that it is larger during a downturn (e.g., Ljungqvist and Sargent 1998, 2008) while others have suggested that it may be smaller (e.g., Krueger and Meyer 2002).[3]

Whether the effects of UI benefit extensions vary with the business cycle has potentially important policy implications. One rule of thumb suggests that the duration of UI should be extended until the exhaustion rate is constant (Corson and Nicholson 1982). Another rule of thumb proposed is to vary the duration of UI benefits to hold the non-employment effect of UI constant (Moffitt 1985). An increasing literature assessing the optimality of the *level* of UI benefits also stresses the role of the non-employment effect of UI benefits (e.g., Baily 1978, Chetty 2008) and its variation

[1] See, e.g., Lake (2002) for an account of increases in UI durations at the state and federal level in downturns in the United States.

[2] The economic effects of UI durations have played an important role in the debate about additional extensions in UI benefits stalled in congress for several months in the spring of 2010. Opponents of further extensions referred to the negative effect of UI extensions on labor supply, its potential role in explaining in exceptionally high average unemployment durations, and the risk of creating long-term dependency on UI benefits. Similar criticisms arose in evaluations of UI extensions in previous recessions (e.g., Needels and Nicholson 2004).

[3] The intuition for the view of stronger disincentive effects is that the incidence and cost of job loss is particularly severe in a recession (von Wachter, Song, and Manchester 2009). In this case the effective replacement rate may rise beyond the typical replacement rate and imply stronger and possibly lasting effects on unemployment as in Ljungqvist and Sargent (1998, 2008). On the other hand, as further discussed in Section 2, in recessions higher costs of job search may reduce the effect of UI parameters on labor supply and on the aggregate unemployment rate

over the business cycle (e.g., Kiley 2003, Sanchez 2008). Yet, in the United States it is difficult to estimate the cyclicality of the exhaustion rate or of the disincentive effect because benefit duration in recessions is endogenous to the state of the labor market.[4] Even if such estimates were available, the current literature offers little guidance as to the welfare effects of extensions in UI durations nor a theoretical justification for either 'rule of thumb.'[5]

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

To interpret the implications of our results for the welfare effect of UI extensions over the business cycle, we use a search model with endogenous search intensity and liquidity constraints (e.g., Card, Chetty, and Weber 2007a, Chetty 2008). From this model we derive a formula that directly relates the welfare gain of UI extensions over the business cycle to increases in the UI exhaustion rate and the welfare costs to the effect of UI durations on non-employment and program duration. The model can imply opposite conclusions about the welfare effects of UI extensions in recessions, but this depends crucially on whether the effect of UI durations on non-employment rises or falls in recessions as well as the behavior of the UI exhaustion rate.

[4]The importance of trigger-based state-level extended benefits relative to discretionary federal temporary benefits has declined since the mid-1980s (Congressional Budget Office 2004, Figure 2), making identification based on changes in the effect of benefit duration across states for recent recessions more difficult. Card and Levine (2000) examine the effects of an extension in UI unrelated to local unemployment conditions in New Jersey, and find more moderate effects on employment than previous studies. Jurajda and Tannery (2003) examine the effect of state and federal extensions in UI duration in Pennsylvania during the early 1980s recession, and find no difference in the effect on labor supply between more and less depressed regions of the state.

[5]The question of the incidence and effects of benefit exhaustion on workers and the appropriate response in UI duration dates back to the beginnings of the UI system in the United States (e.g., Myers and Maclaurin 1942).

To obtain a benchmark we begin by using our RD strategy to obtain labor supply elasticities with respect to UI durations in Germany for large differential expansions for mature workers with stable labor force attachment. For this sample, our estimates imply a moderate rise in non-employment of about 0.1 months for each additional month of UI benefits that is robust across many alternative specifications we tried. The effects on labor supply, we find, are similar for different increases in UI duration, across demographic groups, for workers with weaker labor force attachment, and somewhat larger for workers unlikely to take up extended unemployment assistance after exhausting UI benefits.

Our analysis of variation in the effect of UI extensions over the business cycle point to small, and for the most part statistically insignificant, declines in the disincentive effects of UI durations in larger recessions. On the other hand, we find that the effect of UI extensions on benefit durations, and thus the additional coverage provided by UI, increases significantly in recessions, mainly due to a rise in the UI exhaustion rate. These results are robust to considering variation by year or year-by-industry, to the use of alternative measures of the business cycle, to reweighting to hold characteristics of UI claimants constant, and to an extensive robustness analysis.

These findings have implications for the debate about the effect of extensions in UI durations in recessions. Based on the welfare formula we derive, substantial increases in exhaustion rates and stagnant or declining non-employment effects imply that extensions of UI durations in recessions are likely welfare enhancing. Since our estimation strategy holds macroeconomic conditions in the labor market constant between the treatment and control group, the estimated labor supply response can be interpreted as a partial-equilibrium or micro effect. In the paper, we show that under plausible assumptions, this effect can be interpreted as an upper bound for a general-equilibrium or macro effect that incorporates the effects of search externalities or effects from cyclical labor market conditions. Once we account for congestion effects and potentially incomplete take up of UI using standard specifications of the matching function (e.g., Mortensen and Pissarides 1999), the implied employment effects of a general extension in UI durations are likely smaller than what our main estimates imply, in particular in larger recessions.[6]

We contribute to several aspects of the empirical literature on the effect of UI durations on employment of UI beneficiaries. This is the first paper to replicate regression discontinuity estimates in different economic regimes to assess whether the duration of UI has stronger or weaker employment effects in booms and recessions. This complements an earlier literature on cyclical effects of UI durations (e.g., Moffitt 1985) and related recent work on UI benefit levels (Kroft and Notowidigdo 2010) using state-level differences in unemployment and UI parameters in the United States. We also are the first paper to explicitly assess changes in potential benefits of UI extensions over the business cycle through our analysis of fluctuations in actual benefit durations. We also obtain new estimates of labor supply effects based on large increases in UI durations, large samples, and a regression discontinuity design. This complements existing studies based on broader samples but mainly focusing on smaller variations in UI duration, based on less precise sources of variation, or using fewer years. Our estimates are in a similar range as estimates from Germany (e.g., Hunt 1995) and from Austria (Card, Chetty, and Weber 2007a, Lalive 2008), and, once we consider a comparable group of workers, also as estimates from the United States (e.g., Meyer 1990, Katz and Meyer 1990).

The paper also contributes to the literature concerned with the welfare implications of parameters of the current unemployment insurance system. By deriving the welfare effects of extensions in the duration of UI benefits in a search model with liquidity constraints, we extend the existing literature focused on UI benefit levels (e.g., Bewly 1978, Shimer and Werning 2007, Chetty 2008). This leads to an alternative welfare formula that allows us to assess fluctuations in both costs *and* benefits of UI extensions over the business cycle. In addition, our formula can be expressed as a function of observable 'sufficient statistics' that allows researchers to calculate the size of the welfare gain without resorting to estimates of deep structural parameters which are not directly observable from data (Chetty 2008). This is complementary to Kiley (2003) and Sanchez (2008), who show that the path of optimal UI benefits moves inversely with the efficiency costs of UI over

[6]There are other potential effects of UI on the labor market, such as through an increase in the rate of layoffs or an effect on aggregate demand. These effects are not the focus of this paper, but are briefly discussed in our implications.

the business cycle, and Kroft and Notowidigdo (2010) who derive how the efficiency cost varies with the aggregate unemployment rate. We further add to the literature on optimal UI over the business cycle by showing that in recessions several market-wide effects may substantially reduce the efficiency costs of general UI extensions implied by our partial equilibrium RD estimates. This extends similar calibrations in the existing literature not taking into account such general equilibrium effects (e.g., Katz and Meyer 1990). It is also related to Landais, Michaillat, and Saez (2010), who show theoretically in the context of a general equilibrium model with job rationing that the general equilibrium effect of UI benefit levels determines optimal benefit levels, and that this effect will diverge in recessions from partial equilibrium estimates.

The outline of the paper is as follows. In section 2 we derive the welfare effect of extensions in UI benefits and discuss contrasting hypothesis about the effect of UI in booms and recessions in the context of a search model with liquidity constraints. Section 3 describes the institutional environment in Germany, the administrative data, and empirical approach. Sections 4 and 5 contain our main findings regarding the effect of extended UI on labor supply and benefit duration over the business cycle. Section 6 discusses the implications of our findings for effects of UI extensions on the unemployment rate and on welfare. Section 7 concludes, summarizes caveats of our approach, and makes suggestions for future research.

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

In this section we use a model of job search with endogenous search intensity and liquidity constraints (Card, Chetty, and Weber 2007a, Chetty 2008) to show that the welfare costs of extensions in the duration of UI benefits rise with the adverse labor supply effect of UI durations, while the welfare benefits rise with the exhaustion rate of UI benefits. We then use the model to nest two contrasting hypotheses about changes in the effect of UI extensions in recessions in the existing literature. Finally, we solve the model to derive a version of our formula based on sufficient statistics (Chetty 2008). The exposition of the model is kept purposefully brief, with further discussion and derivations relegated to the Web Appendix.

Worker's Problem. The model describes optimal behavior of a worker living T discrete periods (e.g., months) who is unemployed and receiving UI benefits in period zero.[7] Without loss of generality, we set the worker's discount rate equal to zero. In each period, the worker decides how intensely to search for a job. Let s_t denote search intensity, which is normalized to be equal to the probability of finding a job. It is assumed that a worker who finds a job during a period t starts it immediately at the beginning of period t. Employment is an absorbing state and when employed a worker receives a wage of w_t and pays a tax of τ used exclusively to finance unemployment insurance benefits. Furthermore, in each period the worker owns assets A_t, the level of which is constrained by a lower bound L. As in Chetty (2008), in our baseline case we make the following simplifying assumptions. The wage a worker can receive is fixed in advance (though perhaps varies from period to period), thus reservation wages play no role and any job offer which a worker receives is accepted. The worker's initial asset level A_0 is fixed. There is no heterogeneity in the model. Relaxing these assumptions does not affect our main conclusions (see the Web Appendix).

Using these specifications, the life-time value of utility if a person finds a job at the beginning of period t can be written as

$$V_t(A_t) = \max_{A_{t+1} \geqq L} \left(v\left(A_t - A_{t+1} + w_t - \tau\right) + V_{t+1}\left(A_{t+1}\right)\right),$$

where $v(c_t^e)$ is the flow utility while employed. While unemployed, the worker receives a fixed level of UI benefits $b < w_t$ for at most a fixed number of P periods. After exhausting UI benefits, the worker receives a fixed baseline utility and no further transfer payments (though this is easily generalized). The duration of non-employment is $D \equiv \sum_{t=0}^{T-1} S_t$, where $S_t \equiv \prod_{j=0}^{t}(1-s_j)$ is the survivor function at time t. Total lifetime of workers at the time of entering unemployment is thus broken up into 3 periods: duration of receiving UI benefits ($B \equiv \sum_{t=0}^{P-1} S_t$), the duration of

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

non-employment without receiving UI benefits ($D-B$), and the duration of employment ($T-D$).

The value for a person who does not find a job at the beginning of a period is

$$U_t(A_t) = \max_{A_{t+1} \geqq L} \left(u\left(A_t - A_{t+1} + b_t\right) + J_{t+1}\left(A_{t+1}\right)\right),$$

where $u(c_t^u)$ is the flow utility while unemployed. The value of job search in each period can be expressed as

$$J_t(A_t) = \max_{s_t} \left(s_t V_t(A_t) + (1-s_t)U_t(A_t) - \psi(s_t)\right),$$

where $\psi(s_t)$ is the differentiable, increasing, and convex cost of job search (below, we allow search costs to vary over time). If we assume that $U(.)$ is concave,[8] optimal search intensity in each period is implicitly defined by

$$V(A_t) - U(A_t) = \psi'(s_t).$$

This formula will be used below to assess the effect of changes in search costs and reemployment wages on the path of search intensity.

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 on welfare of changes in the potential duration of UI benefits P.[9] Social welfare at time $t=0$ can be written as $W_0 = s_0 V_0(P,\tau) + (1-s_0)U_0(P,\tau) - \psi(s_0)$.

The budget constraint of the social planner requires $\tau = \frac{Bb}{T-D}$. After some algebra, we obtain our first main result.[10] The marginal welfare gain of increasing P is

[8]See Lentz and Tranaes (2005) and Chetty (2008) for a discussion of this point.

[9]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 2006, Pavoni 2007).

[10]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.

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

where $\left.\frac{\partial B}{\partial P}\right|_1 \equiv S(P)$ is the exhaustion rate of UI benefits, and $\left.\frac{\partial B}{\partial P}\right|_2 \equiv \sum_{t=0}^{P-1}\frac{\partial S_t}{\partial P}$ is the increase in benefit duration due to reduced search intensity among unemployed before the exhaustion point; $\frac{\partial D}{\partial P}$ is the increase in the total non-employment duration in response to a rise in potential UI duration. The total effect of potential on actual benefit duration is $\frac{\partial B}{\partial P} \equiv \left.\frac{\partial B}{\partial P}\right|_1 + \left.\frac{\partial B}{\partial P}\right|_2$.

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 (which is positive as long as the marginal utility of the unemployed in period P is higher than the average marginal utility of the employed) times the probability of exhaustion $\left(\left.\frac{\partial B}{\partial P}\right|_1\right)$. The second term captures the costs of extending UI benefits due to the behavioral change induced by the more generous UI system. This cost is the per capita increase in taxes levied upon employed individuals times their marginal utility. Taxes rise because the unemployed lower their search intensity and this will increase their receipt of UI benefits $\left(b \times \left.\frac{\partial B}{\partial P}\right|_2\right)$. They also increase because longer non-employment durations reduce the number of employed individuals $\left(\frac{\partial D}{\partial P}\right)$ who pay taxes, where the tax rate is the rate of UI beneficiaries to the employed $\frac{B}{T-D}$ times the unemployment benefit b.

We are particularly interested in changes of the welfare effect of benefit extensions over the business cycle. The main components in the welfare formula that are likely to exhibit fluctuations over the business cycle are the exhaustion rate $\left(\left.\frac{\partial B}{\partial P}\right|_1\right)$, the effect of potential UI duration on benefit duration before exhaustion $\left(\left.\frac{\partial B}{\partial P}\right|_2\right)$, and the effect of potential UI duration on total non-employment duration $\left(\frac{\partial D}{\partial P}\right)$. The benefit levels are, apart from changes in the sample composition which we control for, unchanged over the business cycle. In Section 6 we discuss possible changes of the marginal utility of the unemployed over the business cycle and how that may affect our conclusions. The remaining components in the formula, $\frac{B}{T-D}$ and the average marginal utility of the employed, can be considered fixed from a welfare perspective, as long as the government smoothes taxes over the business cycle, which is approximately the case in most countries.[11]

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

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

where $\Omega \equiv \xi u'(c_P^u) + \frac{B}{T-D} E_{0,T-1} v'(c_t^e) > 0$, $\xi \equiv \left(1 - Ps(1-s)^{P-1} - (1-s)^P\right)$, and $\left.\frac{\partial B}{\partial P}\right|_2 = \frac{\partial D}{\partial P}\xi$. This formula indexes the welfare gain by the effect of potential on actual benefit duration $\left(\frac{\partial B}{\partial P}\right)$, and the welfare cost by the disincentive effect of UI extensions on labor supply $\left(\frac{\partial D}{\partial P}\right)$. 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 non-employment spell, we found that the alternative welfare formula in equation (2) approximates the exact welfare formula in equation (1) quite well. The approximation is likely to work even better in settings such as the United States, where the hazard has been shown to be approximately constant (e.g., Katz and Meyer 1990).

The formula for the welfare effect of extensions in UI durations at constant benefit levels con-

[11] To be more precise, the marginal utility of employed can be considered constant from the perspective of this analysis as long as the government chooses an optimal tax policy that levies taxes in periods when the costs of taxation are low, 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. While earnings losses in recessions are large for job losers, the fluctuations in earnings trajectories, and hence expected marginal utility, of the average employed worker who pays the tax are typically weak (e.g., von Wachter, Song, and Manchester 2009).

stitutes a new and important finding. From a theoretical point of view, the formula shows that the approximate welfare effect of UI extensions trades off the benefits of UI extensions indexed by the exhaustion rate against the costs indexed by the disincentive effect of UI benefits. It thereby clarifies existing 'rules of thumb' about the optimal response of UI durations to the business cycle. On the one hand, Corson and Nicholson (1982) suggested that the duration of UI benefits in recessions should be extended so that the exhaustion rate remains constant. Our welfare formula shows that this is only true if there is no change in the disincentive effect of UI. On the other hand, Moffitt (1985) suggested that extensions should be chosen to keep the disincentive effect constant. This, in turn, is only true if the exhaustion rate does not change. The formula also complements a growing literature examining the optimality of the *level* of UI benefits (e.g., Baily 1978, Gruber 1997, Shimer and Werning 2007, Chetty 2008) and the variation of optimal UI benefits in recessions without explicit focus on benefit duration or a role for liquidity (e.g., Kiley 2003, Sanchez 2008, Kroft and Notowidigdo 2010).

From an empirical point of view, it implies that for realistic scenarios estimates of the effect of extensions in UI benefits on non-employment duration and benefit duration (as implied by changes in the survivor function) can be used to assess the changes in the welfare effects of UI extensions over the business cycle. An additional advantage of the approximate formula in equation (2) is that it depends only on $\frac{\partial B}{\partial P}$ and $\frac{\partial D}{\partial P}$, two parameters commonly estimated by empirical studies of UI duration. Thus, our formula also applies when the effect of potential UI durations on the entire survivor function is difficult to analyze.

Variation with the Business Cycle. The welfare formula implies that absent changes in taxes the welfare effect of UI durations unambiguously increases in recessions only if the exhaustion rate rises (or remains constant) and the non-employment effect falls (or remains constant). As we discuss below, the exhaustion rate typically rises in recessions. Thus, if the disincentive effect declines, as suggested by Krueger and Meyer (2002), the welfare effect of UI durations rises in recessions, justifying common practice. If, as suggested by Ljungqvist and Sargent (1998), the disincentive effect of UI rises in recessions, the welfare effect of UI durations becomes ambiguous.

For substantial increases in the disincentive effect, a rise in UI durations in recessions may thus reduce welfare.

To learn more about the potential welfare effects of UI extensions, our search model allows us to explicitly compare the alternative hypotheses of the effect of UI on labor supply over the business cycle. To do so, we contrast the effect of an increase in search cost and a decline in reemployment wages on the effect of UI duration on search intensity. These can be interpreted as two common aspects of labor markets in recessions - a decline in the job offer rate due to slack labor demand, and a decline in reemployment wages due to losses in occupation or industry specific skills in times of reallocation.[12]

Under reasonable parameterizations of the cost function, we find the model predicts an unambiguous rise in the exhaustion rate, but predicts opposite effects for the disincentive effect of UI durations. Let θ denote a proportional rise in search costs, and $\overline{w}$ the mean post-employment wage. Then one can show that

$$\frac{\partial^2 B}{\partial P \partial \theta} > 0,\ \frac{\partial^2 B}{\partial P \partial \overline{w}} < 0, \text{ while } \frac{\partial^2 D}{\partial P \partial \theta} < 0,\ \frac{\partial^2 D}{\partial P \partial \overline{w}} < 0,$$

i.e., the effect of potential UI duration on benefit take up (approximately the exhaustion rate) rises when either search costs rise or reemployment wages fall; yet, the effect of potential UI duration on non-employment durations differs in sign - when search costs rise, increases in potential UI duration have smaller effects on non-employment durations, as suggested by Krueger and Meyer (2002); when reemployment wages fall, potential UI duration lead to larger non-employment effects.[13]

[12] Ljungqvist and Sargent (1998) argue that larger recessions can involve structural changes that render part of workers' skills obsolete and thereby raise replacement rates. If skills further depreciate during unemployment, they show that longer UI benefits can lead to lasting increases in unemployment. They argue that such a pattern could explain the divergence in unemployment rates in Germany and the United States in the early 1980s.

[13] These findings do not hold for general specifications for the cost function. To see this, consider the marginal effect of UI durations on search intensity $\frac{\partial s_t}{\partial P} = -\frac{b}{\psi''(s_t)} \frac{\partial U_t}{\partial b}$. The direct effect of raising the search cost is negative through the denominator; similarly, the direct effect of lowering reemployment wages works by raising non-employment durations and thus the marginal effect of benefits on the value of unemployment. Yet, since in both cases the entire path of search intensities is affected, higher derivatives and cross-derivatives of the cost functions matter, neither of which is known directly from the data. The results in this paragraph hold if the third derivative of the cost function is positive and higher cross-derivatives with respect to search costs are negligible.

To illustrate explicitly the differing implications of the two channels for the welfare effects of UI extensions, we calibrated our model for a set of realistic parameter values. We set benefit levels and durations, and reemployment wages according to the values in our sample. Following Chetty (2008) we specified the cost function to be exponential ($\psi(s) = \theta s^{1+\kappa}/(1+\kappa)$) and the flow utilities in employment and unemployment to be constant relative risk aversion (CRRA) ($u(c) = v(c) = c^{1-\gamma}/(1-\gamma)$). We then calibrated the parameter vector (θ, κ, γ) to match the actual mean non-employment duration $\overline{D}$ and the estimated effect of potential UI durations on D ($\frac{\partial D}{\partial P}$) according to a quadratic loss function.[14] Using the resulting parameter values, we then simulated the effect of changes in θ and in w on $\frac{\partial D}{\partial P}$, $\frac{\partial B}{\partial P}$, and $\frac{dW_0}{dP}$, while imposing the government budget constraint that expected taxes have to match benefit payments. The results are shown in the two panels of Figure 1. Starting from a search cost parameter of one (the calibrated value), the simulations in the upper panel show how $\frac{\partial D}{\partial P}$ falls and $\frac{\partial B}{\partial P}$ rises with a rise in search cost, leading to a *rise* in $\frac{dW_0}{dP}$. Starting from a monthly reemployment wage of about 1900 Euros (the average in our data), the lower panel shows that a reduction in reemployment wages raise both $\frac{\partial D}{\partial P}$ and $\frac{\partial B}{\partial P}$. For our parameterization this leads to an unambiguous *decline* in $\frac{dW_0}{dP}$.[15]

These simulations underscore that recessions can have opposite impacts on the non-employment effect of UI durations, leading potentially to contrasting conclusions with respect to the welfare benefit of UI extensions. Yet, in most applications neither the exact source of business cycle fluctuations in terms of search costs or wage changes, nor the appropriate specifications of the model parameters are known. A particular advantage of our welfare formula is that empirical estimates of changes in the effect of potential UI durations on non-employment and benefit durations are sufficient to assess the changes in the welfare effects of extensions in UI durations over the business cycle.

In the empirical part of this paper we provide estimates of $\frac{\partial D}{\partial P}$ and $\frac{\partial B}{\partial P}$. Since by construction

[14] The remaining parameter values were $T = 120$ (corresponding to ten years), $\tau = 0.06$ (corresponding to the German tax rate, which is endogenized below), $P = 12$, and $\beta = 0.995$. The resulting parameter values are $\hat{\kappa} = 1.67$, $\hat{\theta} = 1.03$, $\widehat{\gamma} = 2.17$.

[15] The marginal welfare effect in these simulations is calculated numerically. The same findings hold if we instead used $\left.\frac{\partial B}{\partial P}\right|_1$ and $\left.\frac{\partial B}{\partial P}\right|_2 + \frac{\partial D}{\partial P}\frac{B}{T-D}$ as our indices of benefits and costs, respectively, to calculate W_0.

the RD design is holding the macroeconomic environment between treatment and control group constant this estimate can be viewed as the partial equilibrium marginal effect or the micro marginal effect. In section 6 we show that under plausible assumptions in a recessionary environment this micro marginal effect represents an upper bound for the marco marginal effect that takes feedback channels through overall changes in labor market tightness, search externalities, imperfect take up of UI benefits, and vacancy creation into account.

3 Institutions, Data and Methodology

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

3.1 The Unemployment Insurance System in Germany

The German unemployment insurance system provides income replacement to eligible workers who lose their job without fault at a fixed replacement rate over a fixed period of time. For an individual without children the replacement rate is 63 percent of previous net earnings.[16] From the 1980s until the early 2000s the maximum duration of benefits was tied to recipients' exact age at the beginning of the UI spell and to their prior labor force history. It is this difference which we exploit to estimate the effect of extensions in duration of UI benefits on non-employment durations. Figure 2 shows the discontinuities in potential benefit duration by age at claiming for the group of

[16] Workers losing a job through no fault of their own are eligible to receive unemployment insurance benefits if they have worked for at least 12 months in the previous 3 years. Sanctions for not taking suitable jobs exist but appear to be rarely enforced (Wilke 2005). For individuals with children the replacement rate is 68 percent. There is a cap on earnings insured, but according to Hunt (1995) it affects a small number of recipients. Since they are derived based on net earnings, in Germany UI benefits are not taxed themselves, but can push total income into a higher income tax bracket.

workers who by their employment history are entitled to the maximum durations in their respective age-group. Between July 1987 and March 1999, the potential UI duration for workers who were younger than 42 was 12 months. For workers age 42 to 43 potential UI duration increased to 18 months; for workers age 44 to 48 (49 to 54), the maximum duration further rose to 22 (26) months. As further explained below, to obtain precise measures of potential UI durations, we restrict ourselves to this sample of workers in our main analysis. At the end of the 1990s a reform occurred which was meant to reduce potential disincentive effects of unemployment insurance. As shown in Figure 2, starting in April 1999 the potential UI durations were lowered and the age thresholds were shifted upwards by 3 years. Thus in order to be eligible for 18 months or 22 months of benefits a worker had to be at least 45 or 47 on the claiming date. We will use these alternative thresholds to validate our main research design.[17]

Individuals who exhaust regular UI benefits and whose net liquid wealth falls below a threshold are eligible for unemployment assistance (UA), which does not have a limited duration. The nominal replacement rate is 53%, but UA payments are reduced substantially by spousal earnings and other sources of income. For example, for a woman whose husband earns as much as 10% more than her the UA benefits are zero. Given that about 80% of individuals in our cohort and age range are married, based on average earnings levels UA benefits are on average about 35% for men and 10% for women.[18] Among all new UI spells in our sample, about 10-15% end up taking UA benefits. We study the potential effect of UA on our findings in our empirical analysis.

[17]The reform was enacted in 1997 but phased in gradually, so that for people in the highest experience group, which constitutes our analysis sample, it only took effect in April 1999 (See Arntz, Lo, and Wilke 2007). To avoid confusion we refer to this as the 1999-regime in the text. In 2003 and 2004, the entire German social security system underwent a comprehensive series of reforms (the so-called Hartz reforms). We use the period between April 1999 and December 2004 as a second sample period, thus excluding workers who became unemployed after the Hartz IV reform took place. The implementation of the post-1987 regime occured stepwise between 1983-1987 and is analyzed by Hunt (1995). We do not analyze these changes here, since the sample size in each of the short periods in which the UI system is stable is too small to analyze these regimes separately with sufficient precision.

[18]UI benefits are paid for by worker and employer contributions, whereas UA benefits are funded by general revenues. The wealth threshold is not very stringent, but given the wealth distribution in Germany it is likely to be binding for part of our sample.

3.2 Social Security Data

The data for this paper is the universe of social security records in Germany. For each individual working in Germany between 1975 and 2008, the data contains day-to-day longitudinal information on every employment spell in a job covered by social security and every spell of receipt of unemployment insurance benefits, as well as corresponding wages and benefit levels. Compared to many other social security data sets, this data is very detailed. We observe several demographic characteristics, namely gender, education, birth date, nationality, place of residence and work, as well as detailed job characteristics, such as average daily wage, occupation, industry, and characteristics of the employer.[19]

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

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

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

the observation period in 2008. Our results are very 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 non-employment spell. To calculate potential UI duration for each spell in our sample, we use information about the law in the relevant time periods together with information on exact dates of birth and on work histories. This yields exact measures for workers who have been employed for a long continuous time and are eligible for the maximum potential durations for their age groups. However, the calculation is not as clear cut for workers with intermittent unemployment spells because of complex carry-forward provisions in the law. We thus define our core analysis sample to be all unemployment spells of workers who have been working for at least 52 months of the last 7 years and did not receive unemployment insurance benefits during that time period. The resulting sample is of intrinsic interest, since it corresponds to workers often the focus of discussion of extensions in UI benefits in difficult economic times – mature workers in with high labor force attachment who absent a layoff or a recession would have been unlikely to become unemployed. Below, we show that our results are robust to broadening our sample to include workers with weaker labor force attachment. We also show that the characteristics of our sample is comparable to similar UI recipients in the United States.

Statistics for various samples are shown in the Data Appendix to the paper. As expected, relative to a general sample of non-employment 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 non-employment. As a result, wage losses upon re-employment are larger and elapsed non-employment 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 non-employment spells in Germany over this time period, it is likely to be typical of mature unemployed workers who lost a job during a recession.

Elapsed duration in UI and non-employment spells is large, but similar to what is found in studies using comparable data. For example, in the Austrian case the mean duration of non-employment 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.[20] 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 non-employment durations in the Displaced Worker Survey (DWS) we analyzed. In the DWS, among 40 to 49 year old displaced workers who have received UI after displacement, after three years about 15 percent is still not employed, a figure comparable to the Germany, where the fraction of individuals whose spell is censored at 36 months is 23 percent.[21]

3.3 Methodology

The institutional structure and data allow us to estimate the causal effect of UI benefit durations on non-employment 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

$$y_{ia} = \beta_0 + \beta_1 D_{a \geq a^*} + f(a) + \varepsilon_{ai}, \quad (3)$$

where y_{ia} is an outcome variable, such as non-employment duration, of an individual i of age a.

[20]The duration of unemployment is smaller in the survey data used by Katz and Meyer (1990a,b), but they discuss potential sources of measurement error due to recall problems. The average duration of spells in unemployment as defined by statistical authorities is also smaller, yet this ignores duration of time spent out of the labor force and is affected by institutional features of the labor market (e.g., Machin and Manning 1999).

[21]See Appendix Table A-1. Given the time since job displacement in the Displaced Worker Survey is based on calendar years and the survey is either in January of February, at 36 months after displacement the actual number is likely to be higher (for two years after displacement, the fraction not employed is about 21 percent in the DWS).

$D_{a \geq a^*}$ 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.[22] We estimate equation (3) locally around the three cutoffs and specify $f(a)$ as a linear function while allowing different slopes on both sides of the cutoff. We use a relatively small bandwidth of 2 years on each side of the cutoff. We then replicate this approach for different years, industries, demographic groups, and different outcomes. All results are also robust to an extensive sensitivity analysis summarized in Section 5.

3.4 Identification Assumptions

The identification assumption of the regression discontinuity design requires that all factors other than the treatment variable that influence the outcome 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 durations on the outcome variable, since the flexible continuous function $f(a)$ captures the influence of all other variables. 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 elasticities represent valid regression discontinuity estimates.

One approach to assess the identification assumption is to test for discontinuities in observable characteristics at the threshold by estimating equation (3) with observable characteristics as outcome variables. Table 1 presents results of these regressions using 2 year bandwidths around the cutoffs. Of the 21 coefficients in Table 1, there are only two statistically significant at the five percent level. There is a statistically significant increase in the fraction female at the 42 year and 49 year threshold, however the magnitude of this is quite small. Examination of corresponding

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

regression discontinuity plots (shown in Web Appendix Figure W1) confirm the conclusion that pre-determined characteristics change very little at the thresholds.

A second standard way of testing the regression discontinuity (RD) assumption is to look at the smoothness of the density of unemployment spells around the cutoffs. Figure 3 (a) shows the number of spells in two-week age intervals. On average there are around 4300 spells in each interval up until age 47, after which the number of spells begins to decrease. It appears that at each cutoff there is a slight increase in the density in the bin directly on the right of the cutoff. Implementing the test proposed by McCrary (2008), this increase is statistically significant at the five percent level for the 42 and 49 cutoff but of very small magnitude.

Such an increase could either occur because firms are more likely to lay off worker with higher potential UI durations, because of a higher probability of claiming UI, or because workers wait until their birthday before claiming UI benefits. To test for the first possibility, in Figure 3 (b) we show the density of spells with respect of the dates the last job prior to UI ended. If firms are more likely to lay off workers with higher UI benefits, the discontinuity should appear in this figure as well. Again there appear to be slight outliers right to the right of the 42 and 49 cutoffs, but less clearly as in Figure 3 (a). If anything this would indicate that firms may wait for a short time to lay off workers until they are eligible to higher UI benefit levels. It does not appear that firms are systematically more likely to lay off workers with higher levels of UI benefits, since in this case the density would shift up permanently.

To see whether workers wait before claiming UI until they are eligible for extended UI durations column (1) of Table 3 below shows how the time between job loss and first take up of UI benefits varies around the threshold. This provides no indication that people who claim UI to the right of the threshold have waited longer before claiming than the people to the left of it. This is consistent with the quite small change in the density right around the cutoff we found. Given the economic incentives it makes sense that only individuals very close to the age cutoff would decide to wait until after their birthday. For example given the estimates below an individual at the age 42 cutoff can expect to receive UI for about 1.8 months longer if they are eligible to 18 rather than 12 months of benefits. Given that the individual does not receive UI until claiming, even ignoring the possibility of receiving UA after the end of UI and assuming zero discounting, there seems to be no incentive to wait longer than 1.8 months for the higher benefit durations.[23]

Overall, it appears that the discontinuity in the density is driven by maximally a few hundred spells shifted to the right just around the cutoffs. This is relative to around 450,000 spells in each of the four-year intervals that we use for our RD estimation.[24] Since the magnitude of this effect is very small (in particular relative to our non-employment 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 Table W3). This has virtually no effect on the magnitude of the coefficient at age 42 and a very small effect on the other two coefficients. Furthermore we estimated our main specifications controlling for observables, and again obtained virtually the same coefficients.

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

4.1 The Average Effect of Large Extensions of UI Durations

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

[23] Note that if this kind of delaying were prevalent one could still get valid RD estimates using a 'fuzzy' RD design, where the age at layoff is used as the forcing variable rather than the age of claiming UI (assuming that the age at layoff is not manipulated by workers or employers). The age of layoff can then be used to instrument for the treatment variable. Since the duration between end of job and claiming UI is non-negligible, the relationship between potential UI durations and age at job loss is somewhat noisy, which is why we prefer the regular RD design over the fuzzy one without evidence that the type of sorting is actually problematic.

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

Figure 4 (a) shows how the duration of UI receipt varies with age at the beginning of the unemployment spell. The figure implies that a large number of individuals are substantially affected by the increase in potential UI durations. Workers younger than 42 at the age of claiming UI, are eligible to 12 months of UI benefits, of which they use about 6.7 months on average. At the age 42 threshold UI eligibility increases to 18 months and the average duration or UI receipt increases to about 8.5 months. The increases in benefit receipt duration at the other cutoffs are also quite substantial, and range from one fourth (at the age 44 cutoff) to one third (at the age 49 cutoff) of the increase in the maximum UI durations. The effects of the large UI extensions at the age thresholds on non-employment durations are shown in Figure 4 (b). There is a clear jump in non-employment durations at the age 42 cutoff from about 15.6 to 16.4 months of non-employment. At age 44, non-employment durations increase from 16.5 to 16.9 months and at age 49 from 19.9 to 20.3. Thus, visual evidence clearly suggests that the UI extensions lead to significant increases in both take up and non-employment durations at all thresholds.

The marginal effects obtained by estimating equation (3) separately for each age cutoff are shown in Table 2. Our main regression results in column (1) are very consistent with the graphical analysis. As shown in Panel (b), at the age 42 cutoff non-employment durations increase by 0.78 months (standard error 0.1 months), at age 44 the increase is 0.41 months, and at age 49 the increase is 0.43 months. To account for the fact that increases in UI durations differ across thresholds, one can consider the marginal effects of an increase of a single month of UI. These effects are shown in bold in the table and are in the same ballpark across age-groups (0.13, 0.10, and 0.11 for age 42, 44, and 49, respectively), and suggest that for each month of additional UI, affected workers spend three more days in non-employment. 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 non-employment and UI durations, the implied elasticities are nearly the same for the different cutoffs and range between 0.12 and 0.13.[25]

[25] This is calculated as an increase in non-employment durations of 0.78 months over an average non-employment duration around the cutoff of 15 months relative to an increase of 6 months over average potential UI durations of 15 months. The elasticities are shown in Appendix Table W4.

After the reform of the UI system in the late 1990s, the eligibility thresholds for extended UI were shifted to ages 45 and 47 starting in 1999. Figure 5 shows that the basic results still hold in the post-1999 regime. The discontinuities in non-employment 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 W6) 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.[26] 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.

Our findings imply that extensions in potential UI durations lead to a significant rise in the duration of non-employment. They also suggest that actual UI durations respond more strongly than non-employment durations. This means a significant fraction of workers stay in non-employment at benefit exhaustion and are thus directly benefiting from an extension of UI durations. For example, about 28 percent of unemployed individuals with 12 months of potential durations exhaust their UI benefits (Web Appendix Figures W3). But an analysis of the hazard function reveals among exhaustees only 8 percent return to employment, while the rest enter non-employment (Card, Chetty, and Weber 2007b).

An analysis of the hazard function also shows that the non-employment effect we find is not purely due to an outward shift of the spike at the benefit exhaustion point. Figure 6 displays non-parametric regression discontinuity estimates of the hazard of exiting non-employment by duration for individuals with 12 and 18 months of UI benefits.[27] Consistent with previous studies (e.g., Meyer 2002) there are clear spikes in the hazard rate at the benefit exhaustion points for the two respective groups. However, there are also clear and statistically significant differences well before the exhaustion point, indicating that when eligible for longer durations, unemployed individuals

[26]From Figure 5 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. Appendix Figure W2 also shows a more visible increase in the density in the two age weeks at the two age cutoff points. Yet, the same arguments as in Section 3.4 apply.

[27]A more detailed discussion of the methodology can be found in the Web Appendix (Section 2.1); the figures of the survivor functions are shown in Web Appendix Figure W3.

adjust their search behavior a long time before running out of UI (e.g., Card, Chetty, and Weber 2007a). These findings suggest that our main effects reported in Table 2 are averages of behavioral responses along the entire duration distribution.[28]

Our results are consistent with the theoretical model we discussed in Section 2. The model predicts that a rise in the potential duration of UI benefits leads individuals to lower their search effort (s_t). Consistent with our finding in Table 2, this implies a rise in the average duration of non-employment. Also consistent with our results, the reduction in search intensity predicts a lower hazard of leaving unemployment for all non-employment durations before benefit exhaustion. Our finding that the mean duration of UI receipt increases more strongly than the non-employment duration implies that the increase in UI coverage is only partly due to a behavioral response. An important part of increased coverage is due to individuals continuing to receive UI benefits, who would otherwise have exhausted benefits while remaining in non-employment. Our model provides a framework for the interpretation of the effects of UI extensions on non-employment and benefit duration, and its implications are taken up again in Section 6. Finally, consistent with our model, in separate work we do not find an effect of UI extensions on wages, other measures of job quality, or long-term employment.

The labor supply responses we find are consistent with the results from previous studies in Germany, Austria, and, with some qualification, the United States. Hunt (1995) evaluates the reforms in the German UI system over the period 1983 to 1988 using a difference-in-difference approach between age-groups over time. Hunt (1995) finds that her estimated effect on the hazard rate is slightly smaller than the effect in Moffitt (1985), who reports a marginal effect of 0.16 weeks per additional week of potential UI benefits. This implies the estimates are quite comparable despite differences in underlying samples, methodology, and measures of non-employment duration.[29]

[28] Regression discontinuity estimates along different points of the duration distribution and for the other age cutoffs are shown in the Web Appendix Table W7. The table shows significantly negative effects on the hazard prior to the exhaustion point of the control group. These effects are present in the first twelve months even when potential durations increase from 22 to 26 months, suggesting that individuals are forward looking over a long horizon. After the exhaustion point of the control group, the difference reverses, with the hazard of the higher eligibility group exhibiting a significant increase at the new point of exhaustion.

[29] 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,

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 non-employment for each additional month of UI duration. In a different context, Card, Chetty, and Weber (2007a) analyze increases in benefit durations in the Austrian UI system using a similar RD design as ours but with smaller increases in potential UI durations. Their estimates point to similarly modest labor supply effects of potential UI durations.

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

4.2 Variation of Labor Supply Effects with the Business Cycle

A key advantage of the institutional setting in Germany is that it provides stable quasi-experimental increases in potential UI duration for each year in our sample. This allows us to study variation in the effects of UI over the business cycle while holding constant potentially confounding conditions in the labor market. Using the large samples in our data we replicated our regression discontinuity estimates for our multiple age thresholds for each year and major industry, and examined whether the resulting labor supply effects and benefit durations varied systematically with the business cycle.

The findings from this exercise suggest the non-employment effects of potential UI durations

Fitzenberger and Wilke (2010), focuses on age groups older than 50, which we excluded from our analysis. Fitzenberger and Wilke also use a difference-in-difference estimator. Their main finding is a strong increase in spells that never return to employment. While we found a strong age gradient in the probability of ever returning to work there are only very small jumps at the UI discontinuities (Table 3). 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.

are quite stable over the business cycle. At best, some of our results suggest a weak decline in the effect of extended UI on non-employment in recessions. In contrast, we find that the effect of UI extensions on benefit duration, and thus the additional coverage provided, increases significantly in recessions, mainly driven by a rise in the exhaustion rate.

The first panel of Figure 7 plots the rescaled marginal effects of one month increase in potential UI obtained by replicating our regression discontinuity estimates separately for each calendar year for the threshold at age 42 (and age 45, after the 1999 reform), which yields the most precise estimates for the effects. The unemployment rate shows how the German economy has gone through large economic swings during our sample period, such as the dramatic boom-bust period after unification, plus an ensuing protracted slump. Yet, while there is some variation of the estimated marginal effect over time, from the figure there appears to be no clear systematic variation with the business cycle. In the second panel of Figure 7 we investigate this further by plotting the marginal effect for all ages against the unemployment rate at the time of the start of the unemployment spell. There is a slight negative correlation, but overall the marginal effect appears to be quite stable over the business cycle.

The findings from Figure 7 (b) are summarized and extended in Table 4. First, we show that the main finding holds when we use different indicators of the change in labor market conditions, such as unemployment rates, GDP growth, or annual mass-layoff rates calculated from our data. The findings are also similar if we use elasticities instead of marginal effects. Second, in the last two rows we show changes in labor supply elasticities for workers losing their jobs in industries with high or low average wage losses (as measured by quintiles of average wage loss) and with high or low rates of job destruction. The average wage loss can be used as a proxy for the amount of specific skill a laid-off worker is likely to lose. Moreover, we can then control for a potential confounding effect from changes in overall labor demand by either adding the rate of unemployment or year effects. The results again suggest there is little difference in the effect of UI on non-employment by our measures of industry-specific economic conditions.

Figure 8 and Table 4 replicate the same analysis for the effect of potential duration of UI benefits on the actual duration of UI benefits. Contrary to our finding for the effect on non-employment durations, it now appears that the effect of potential UI duration is significantly countercyclical. The figure clearly shows how there is a substantial positive relationship between the effect of UI extensions and benefit duration and the *lead* in unemployment rates. This is confirmed in Table 4, where we assess the correlation with a range of alternative measures of the business cycle. The table shows that the effect of UI extensions on benefit duration correlates strongly with the change in unemployment rates or any other measure of contemporaneous changes in labor market conditions. The reason is that, as discussed in Section 2, an important component of the benefit effect of UI extensions is the UI exhaustion rate. Since benefits last at least 12 months and up to 26 months in our sample, the unemployment rate at exhaustion matters. This is shown explicitly in the last two columns of the table. Column 7 shows how the exhaustion rate is strongly procyclical, while column 8 shows that the increase in benefit duration prior to the exhaustion point varies little with the cycle. This has important implication for the welfare impact of UI extensions discussed in Section 6.

One potential concern with the estimates in Table 4 is that they mask differential effects over the cycle in different parts of the duration distribution. We compared shifts in the entire hazard function across boom and bust periods in the Web Appendix (Figure W4), and did not find this to be the case. If characteristics of UI recipients change over time or vary with the business cycle and treatment effects vary across groups, another concern could be that such changes in composition could offset potential cyclical variation in labor supply effects of UI. We examined this possibility, and found it not to affect our result. We analyzed cyclical variation in two summary indices of observable characteristics in our data, the predicted propensity to receive unemployment assistance (UA) and the predicted post-UI wage. Overall, relative to the mean we found at best very small variations in observable characteristics with the business cycle. To nevertheless make sure these changes do not affect our findings, we used the standard re-weighting procedure to hold distribution of characteristics constant across years. This is shown in columns (5) and (6) of Table 4 and confirms that our findings are very robust to changes in characteristics of UI recipients over time or the business cycle.

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 some specifications we find a small decrease in the labor supply effect of UI durations in recessions, but the point estimate is sufficiently small that despite fairly precise standard errors in most specifications this is not statistically significant. Only in large recessions do our point estimates imply more substantial declines in the effect of extensions in the duration of UI benefits on labor supply. The previous literature has provided very little information regarding this relationship. Our findings are consistent with results reported in Moffitt (1985) and Jurajda and Tannery (2003). Using both differences and changes in UI benefit parameters between 13 states as source of identification, based on administrative information on UI spells begun between 1978-1980 Moffitt (1985) finds that the labor supply effect of UI duration declines with the level of the unemployment rate. Jurajda and Tannery (2003) compare the effect of the same extended UI regime in more and less depressed parts in Pennsylvania during the recessions of the early 1980s. They conclude that the effect of the extensions is similar in Philadelphia and Pittsburgh, despite distinctly different local levels of unemployment rates (about 10% and 16% respectively).[30]

5 Robustness and External Validity

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

5.1 Robustness Analysis

Choice of Bandwidth. We investigated whether the choice of the bandwidth of the RD estimator affects our conclusions (Table 2). Using a bandwidth of 2 years, the point estimates from the RD

[30] While Jurajda and Tannery (2003) find the same large spike in the hazard rate at the exhaustion point of UI benefits in more or less depressed parts of Pennsylvania, the exact magnitude of the spike appears unknown because of a coding error (see the working paper version of Card, Chetty, and Weber 2007b).

regressions are very similar to what is implied by the graphical analysis. For smaller bandwidths coefficients are very stable for the effects on UI durations, even with bandwidths as small as 0.5 or 0.2 years. For the non-employment durations the estimates are in the same ballpark across different bandwidths, but somewhat larger for tighter bandwidths. Investigating figures with different bandwidths revealed that this is due to under-smoothing for very small bandwidths, so that we have most confidence in estimates with 2 year bandwidths.[31]

Measure of Non-Employment. We also find that the increase in non-employment durations is mainly due to workers taking longer until returning to a job, not due to individuals staying out of employment forever. In order to investigate this Table 3 column (2) shows the probability of ever returning to registered employment. 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 non-employment durations.[32] Similarly we investigated whether our estimates are affected by the choice of our non-employment 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.[33]

Differences by Subgroups. To further examine the robustness of our main estimates, Table 5 shows our regression discontinuity estimates for several subgroups. While the table displays some expected differences in the labor supply response to UI extensions, overall the labor supply effects are remarkably robust throughout the population. In particular, it does not appear that our findings are driven by any particular sub-group in our sample. The labor supply effects are slightly larger for highly educated and high tenure workers, and larger for women. Together with the similarities across age-groups, the point estimates in Table 5 imply a common labor supply effect of an

[31] It should be noted that 2 years is a very narrow bandwidth in comparison to other papers with a similar RD design. For example Lemieux and Milligan (2008) use a bandwidth of 6 years.

[32] There is a slight drop of one percent relative to the mean at the age 42 cutoff, and the effect is even smaller for the other two age thresholds. The fraction of 40 to 49 year old UI recipients with high labor force attachment ever employed is 0.77, see Appendix Table A-1.

[33] Web Appendix Table W4 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.

additional month of UI benefits in the range from 0.1 to 0.18.

Restriction on Labor Force Participation. To also examine whether our main findings are affected by our focus on stable workers, in the Web Appendix (Table W5) we replicated our main RD estimates without any restriction on labor force attachment before UI receipt.[34] The RD estimates for the unrestricted sample are smaller for the duration of both UI receipt and non-employment duration. Since the underlying average changes in potential UI durations at the thresholds are also smaller, this is consistent with the underlying true marginal effects being similar. As explained in Section 3 we cannot calculate a rescaled marginal effect for this group. In order to get a comparable measure for this group, we normalized our estimates on non-employment duration by dividing by the effect on UI duration. This ratio is effectively an instrumental variables estimator of the effect of UI duration on non-employment, and is very similar for our main sample and the fully unrestricted sample. Thus, our results appear robust to a weakening of our restriction on labor force participation.

Robustness of Differences over Cycle. We estimated many alternative specifications to further investigate the robustness of the findings regarding the cyclical variation of the effect of UI extensions. For example dropping UI spells from East Germany from our sample, or excluding temporary lay-offs (workers who return to their old employer), did not affect the results reported in Table 4. We also tried several ways to further raise precision of our estimates. For example, when we split our sample by worsening and improving labor market conditions, the labor supply effect seems to be somewhat lower in worsening times (Web Appendix Table W8). Alternatively we estimated a cox-proportional hazard model in the spirit of Meyer (1990) and find a slight decline in the predicted labor supply elasticities when unemployment is increasing (Web Appendix Table 10). We also estimated a linear and log-linear models that pool the effect of UI extensions across our different age-thresholds while flexibly controlling for age (Web Appendix Table W11). Again, the changes over time we find are relatively small, with at best weakly negative coefficients on the

[34]Potential UI durations vary with age for all individuals with at least 14 months of employment prior to the unemployment insurance claim. The extensions at the age thresholds vary between 2 and 6 months depending on the employment history.

interaction of potential UI duration and business cycle indicators.

5.2 External Validity

In this section we discuss to what extent our results may be affected by our sample and institutional environment. This is helpful when assessing the broader relevance of our findings, especially vis-a-vis the United States where the endogenous nature of UI extensions makes it difficult to obtain comparable results. The evidence we provide gives no reason to expect that the estimated variation of the effect of UI extensions over the cycle are substantially driven by particular aspects of our sample or our institutional environment.

Unemployment Assistance. A potential concern is the difference in UI replacement rates and the existence of unemployment assistance (UA). Higher UI replacement rates in Germany should in principle lead to a larger disincentive effect than in the United States.[35] This implies 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, being on average more generous than most welfare programs in the United States, should lead to smaller effects. In this context, the fact that women have only somewhat higher responses than men is interesting, since for the typical married woman with a working husband (which is a majority in our age-range) the benefit provided by UA after exhaustion of UI benefits is close to zero. This suggests that presence of UA per se may not strongly affect our estimates. To learn more about the potential role of extended UA in explaining our findings, we replicated our main regression discontinuity estimates for individuals with high and low propensities to receive UA. If our main estimates were mainly driven by individuals entering UA after exhausting benefits, we should see significant disparities here. The last rows of Table 5 shows that this is not the case. About 10-15% of UI beneficiaries and 50% of exhaustees receive UA. For each UI recipient in our sample we predicted the propensity to receive UA based

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

on education, demographic characteristics, and their earnings histories.[36] 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%). If we include an interaction with the individual propensity and extrapolate linearly, for individuals with propensity of receiving UA close to 80% the rescaled marginal effect is below 0.05. Yet, even for those whose propensity is 20% it is 0.25, well within the overall magnitude of our main findings and very close to results reported in Katz and Meyer (1990). Thus, we conclude that while the presence of UA may lead to somewhat smaller overall estimates, it is unlikely to be the main source behind the labor supply effects we find.

Worker Characteristics. Another potential concern with our sample is that the characteristics of German mature high attachment workers may be different than that of UI recipients in the United States. When we compared our sample characteristics in Section 3.2 to middle-aged UI beneficiaries from the March CPS and the Displaced Worker Survey over the same time period, we found that along observable characteristics, such as tenure on the previous job, age, fraction citizen, industry distribution, and the exhaustion rate these workers look in fact quite similar.[37] In particular, tenure at the last job was nearly the same for 40-49 year old UI recipients in the United States, suggesting that the German sample may not be composed of much more stable workers for the same age range. To directly assess whether differences in observable characteristics vis-a-vis the United States might affect the results regarding the cyclicality of the labor supply effects, we re-estimated our main RD specifications in Table 4 after re-weighting our sample to reflect the distribution of observable characteristics in the United States for a given year.[38] We find

[36]The corresponding linear probability model is shown in the Web Appendix Table W8, and suggests our specification has a good fit. The average predicted value for the probability of take up of UA at exhaustion for the full sample is 0.54, which can be thought of as an estimate of the fraction of UI recipients who are potentially eligible for UA. Note that given the determination of UA benefits, ideally we would have had also access to wealth, marital status, and spousal earnings to make this prediction. Wealth closely correlates with education and earnings histories. Unfortunately, we currently do not have access to marital status, and neither wealth or spousal earnings are in our data. We are not aware of quasi-experimental variation in the propensity to receive UA.

[37]As expected, the fraction of the sample that is female is lower and the fraction employed in manufacturing is higher in Germany (see Web Appendix Table W12). Average years of schooling are also higher in the United States, which is known and partly arises from a difficulty in counting education within the German apprenticeship system. Since information on race or ethnicity is not available in the German data, we included a dummy for citizenship.

[38]As described in the Web Appendix A2.3, using information from the Current Population Survey we first estimated

similar marginal effects and changes over the cycle even when the German sample has the same distribution of observable characteristics as a comparable sample of UI recipients from the United States. These results together with the small variation in treatment effects among sub-groups such as age, education, or gender, we find, suggests that differences in characteristics of UI recipients are unlikely to be a major threat to external validity.

These findings suggest that for mature UI recipients our results do not appear to be driven by particuliarities of the German sample or institutional environment. Of course, we cannot assess to what extent younger wokers would respond differently to the UI extensions we study. The fact that there is no age-gradient between age 42 and 49 in our findings, and that based on an RD design Card, Chetty, and Weber (2007a) report similar labor supply effects for broader age groups for Austria is reassuring.[39] We return to a comparison of our findings and their implications with related findings for the U.S. in the next section, and further discuss limitations of our study in the conclusion.

6 Implications for Welfare and the Aggregate Unemployment Rate

Our main results show that large increases in potential durations of UI benefits lead to moderate increases in non-employment durations that are weakly procylical, and substantial increases in benefit durations that are strongly countercyclical. In this section we discuss the implications of these findings for welfare and the aggregate unemployment rate.

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a probit-model of presence in the United States in a pooled sample of data from Germany and the United States based on age, education, gender, industry, and nationality. Then we used the ratio of the fitted probabilities as weights when re-estimating the RD (e.g., DiNardo, Fortin, and Lemieux 1997). The result is shown in Web Appendix Table W13.

[39]Another potential concern is that the cyclicality may be different for extensions occurring at shorter baseline UI durations, since more individuals are likely to be constrained by the limit in UI durations. This cannot be directly assessed with our data. However, since we find that the effect of the extensions is substantial throughout the duration distribution, this does not seem like a major concern. Moreover, the differences are likely to be smaller in recessions, which are the focus in this paper, when duration of unemployment insurance benefits and non-employment can both increase substantially in the United States.

6.1 Welfare Effects of UI Extensions

We have argued in Section 4 that the main results of our empirical analysis support the use of a forward looking model based on search intensity to interpret our findings. Thus, our main welfare formula applies, in which the benefits and costs of extensions in UI durations depend on the UI exhaustion rate and the effect of UI on non-employment and program durations, respectively. In Section 2 we had further argued that for realistic scenarios *changes* in the marginal benefit of UI extensions dW_0/dP over the business cycle are determined by *changes* in the effect of potential UI durations on actual benefit and non-employment durations. Since we find the effect of UI on the exhaustion rate rises in recessions, whereas the effect on benefit duration prior to exhaustion and non-employment duration is either unchanged or declining, our results imply that increases in potential UI durations likely lead to larger welfare gains in recessions than in economic expansions. Conversely, the current policy in Germany which keeps UI durations stable over the business cycle may be suboptimal.

A natural question is whether a similar prediction would hold for the recurring extensions in UI durations in recessions in the United States. While difficult to measure, existing figures for the exhaustion rate of regular UI benefits suggest that the rate is strongly countercyclical (Congressional Budget Office 2004). We do not have estimates of fluctuations of the non-employment effect with the business cycle comparable to our RD estimates for the US. However, if we use estimates in Moffitt (1985) based on the comparisons of the effect of UI durations across states, the non-employment effect of UI duration appears decline in recession. Using a similar difference-in-difference strategy, Kroft and Notowidigdo (2010) show that the same holds for the non-employment effect of UI benefit levels. Thus, using our welfare formula, it is likely that extensions in UI durations in recessions are welfare improving in the United States as well.

While we argued that the welfare formulas in Section 2 allows us to assess how the marginal benefit of an extension varies over the business cycle, they do not allow a direct assessment of the potential *magnitude* of the welfare effects of UI extensions. An additional advantage of our welfare formula is that it can be fully expressed in terms of statistics that can potentially be measured empirically. If we follow Chetty (2008) and normalize equation (1) by the expected marginal utility of employed workers, we can show that the rescaled welfare gain is approximately equal to

$$\frac{d\tilde{W}_0}{dP} = \left.\frac{\partial B}{\partial P}\right|_1 b \left[\frac{-\partial s_P/\partial a_P}{\partial s_P/\partial w_P}\right] - b\left[\left.\frac{\partial B}{\partial P}\right|_2 + \frac{\partial D}{\partial P}\frac{B}{T-D}\right] \tag{4}$$

The new term in the first bracket is the ratio of the 'liquidity' effect ($\partial s_P/\partial a_P$) and the 'substitution' effect ($-\partial s_P/\partial w_P$) of a UI extension for workers exhausting UI benefits.[40] This formula is useful for at least three reasons. First, the formula makes it clear that the welfare effect of extensions in UI benefit durations increases with the relative strength of the liquidity vs. substitution effect of UI benefits. In the limit when liquidity effects are absent, the welfare effects of UI benefits is zero. This implication of the model is analogous to Chetty's (2008) finding for the level of UI benefits. Second, if available, using appropriate empirical measures of the income and substitution effects one can assess whether at a given duration of UI benefits, an extension would be welfare improving. Finally, in principle one can use this version of the benefit formula to account for the effect of potential changes in the liquidity effect over the business cycle. Note that this channel was absent in our main welfare formula, since $u'(c_P^u)$ was held constant over the business cycle. Yet, a decline in liquidity in recessions may imply higher marginal utility at the exhaustion point, thereby reinforcing the welfare improving effect of rising exhaustion rates.

In our context, we cannot obtain separate estimates of the income and substitution effect of UI benefits since our data neither offers information on income, wealth, or consumption nor an appropriate research design. However, reliable estimates from the previous literature suggests that values for the ratio are about 1.5 for the United States (Chetty 2008) and 1.4 for Austria (Card, Chetty, and Weber 2007a). Findings in Kroft and Notowidigdo (2010) further suggest that the fraction of liquidity constrained workers at the start of unemployment spells does not vary over the business cycle. Using the figure for Austria and an average unemployment rate (approximately $B/(T-D)$) of nine percentage points appropriate for Germany, we find that the

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

welfare effect of one additional month of UI benefits, dW_0/dP, going from a boom (GDP growth + 4%) to a recession (GDP growth - 4%) rises from about 195 Euro to 311 Euro. Thus, given our normalization, this implies that the net life-time welfare gain from a one-month extension in benefit duration rises by 116 Euros per unemployed person.

In the United States, using a marginal effect of UI durations on non-employment of 0.2 (e.g., Katz and Meyer 1990), an average exhaustion rate of 0.35 (Congressional Budget Office 2004), and an unemployment rate of five percentage points, using our formula one obtains that $dW_0/dP \cong 0.44b > 0$, where b is the monthly benefit level.[41] This suggests that at six months of typical UI duration, extensions in the duration would be welfare improving and substantial. As discussed, this effect is likely to be larger in recessions. Since exhaustion rates typically rose from 0.3 to 0.4 from peak to through in past recessions, all else equal, the rise in the welfare effect of a UI extension of one month would be $0.16b$. This suggests that the added welfare gain from expanding UI durations in recessions can be substantial. With falling non-employment effects and possibly rising liquidity constraints, this may still be an underestimate of the welfare gain.[42]

Finally, the actual efficiency cost of UI extensions in recessions may be smaller than indicated by our RD estimates of extensions of UI durations on non-employment and program durations due to the presence of market-wide effects. We show in the next section that with a matching parameter of $\alpha = 0.5$, search externalities alone would reduce the appropriate marginal effect of a UI extension for all unemployed workers by approximately one half. This point reinforces our finding that the welfare benefit of UI extensions in recessions is likely to rise relative to the welfare effect in better economic times.

[41] Average monthly UI benefits in the U.S. reported in Chetty (2008) are about $700. To implement the formula, we need to obtain an estimate of $(\partial B/\partial P)|_2$, the effect of UI extension on benefit duration prior to the exhaustion point resulting from an increase in non-employment durations. Since this is approximately true for the United States (e.g., Katz and Meyer 1990), we proceed by assuming the hazard rate is constant across the spell, such that $\partial s/\partial P$ and thus $(\partial B/\partial P)|_2$, can be recovered from estimates of $\partial D/\partial P$.

[42] These numbers cannot be directly compared to the welfare gain in Chetty (2008) since his welfare formula is normalized to yield weekly instead of life-time welfare gains. As expected, if one scales the resulting effects to represent a similar time frame and a comparable increase in total benefits, the overall magnitude is comparable.

6.2 Implications for Aggregate Non-Employment Duration and Unemployment Rate

The labor supply effects estimated in our regression discontinuity design keep the macroeconomic environment constant between treatment and control group. In this section we argue that under plausible assumptions these partial-equilibrium or micro effects likely yield an upper bound of the effect of an increase in potential duration in the entire population on the average non-employment duration. We also show that as a result, our estimates are likely to overstate the effect of UI extensions on aggregate unemployment rates and thus to understate the potential effect on welfare.

Three reasons why the general-equilibrium effect may differ from our estimates are search externalities, imperfect UI take up, and a response in vacancy creation. For example, a decline in search effort due to increases in P tends to lower congestion in the labor market. As a result, at each level of effort search will be more effective, which will in turn reduce the negative effect of UI extensions on search intensity. To capture market-wide effects we relate the speed at which jobs are filled to the number of workers searching for jobs (u) weighted by their search intensity (e) and the number of vacancies (v) in the economy via a "matching function" . Using the standard case of a Cobb Douglas matching function the effective exit hazard becomes $s \equiv [v^{1-\alpha}(eu)^{\alpha}]/u$, i.e., the number of total matches divided by the number of unemployed (instead of $s \equiv e$ in our basic model without search externalities). Based on this matching function, we derive the following relationships in the Web Appendix:

$$\left.\frac{\partial s}{\partial P}\right|^{GE} = \left.\frac{\partial s}{\partial P}\right|^{RD} - (1-\alpha)\left.\frac{\partial s}{\partial P}\right|^{RD} + \eta \quad \text{and} \quad \left.\frac{\partial D}{\partial P}\right|^{GE} = \left.\frac{\partial D}{\partial P}\right|^{RD} - (1-\alpha)\left.\frac{\partial D}{\partial P}\right|^{RD} + \eta'.$$

The first term on the left hand side of these two expressions is the direct micro marginal effect that comes from the decrease in search effort on the job finding probability, which is estimated by the RD design. The second term is the indirect effect of an increase in P due to lower overall search efforts leading to less congestion in the labor market. This term is proportional to the micro marginal effect with the factor of proportionality being $1-\alpha$. The third term η is the effect a decrease in search effort has on vacancy creation by firms. It seems likely that in deep recessions when unemployment is high, and many applicants are lining up for each job opening, the effect of reduced search effort on the speed of filling vacancies and thus on the decision by firms whether to create vacancies is much lower than during a boom.

Suppose η is indeed close to 0. Then the general equilibrium effect of potential UI duration on exit hazard (s) and non-employment duration (D) is α times the partial equilibrium effect estimated by our RD strategy. Since a typical estimate of α is 0.5 (see Mortensen and Pissarides 1999), this implies that with search externalities the relevant marginal effects $\partial s/\partial P$, $\partial D/\partial P$ and the relevant elasticities are only about 50 percent of the estimated partial equilibrium effect.

This wedge between the partial and the general equlibrium effect is likely smaller in economic upswings, when vacancy creation is more likely influenced by the availability of suitable workers. Landais, Michaillat and Saez (2010) develop a general equilibrium model of the labor market to analyze the wedge between the partial and general equilibrium effect for the case of benefit levels. In their framework they show that the partial equilibrium effect is in fact an upper bound for the general equilibrium effect in an expansion, but that in a recession the general equilibrium effect can be substantially below the partial equilibrium effect.

This discussion has implications for the predicted effect of UI extensions on aggregate employment outcomes. For illustrative purposes, we consider the effect of recent extensions in UI duration from 26 to 99 weeks in the United States on the average non-employment duration and the unemployment rate implied by our estimates. Using a constant elasticity formula, in the absence of macroeconomic factors our regression discontinuity estimates imply that this increase in potential benefit durations would lead to a rise in actual durations from 34 to 40.8 weeks.[43] Since the actual increase in unemployment duration was to about 60 weeks, this would imply that extended UI was responsible for 26% of the increase in average duration of unemployment. To predict the effect

[43] We set $ActDur = a * P^{\eta^{RD}_{D,P}}$, where $\eta^{RD}_{D,P}$ is the elasticity of non-employment duration (D) with respect to potential UI durations (P) implied by the RD estimates in Table 2 and a is a constant. This is a natural choice since our results imply that the elasticity is very similar across different changes in UI, for different subgroups, and for different time periods. Using data from the Bureau of Labor Statistics we calibrate a for May 2008, before the recession started, to be about 22.3. (In May 2008 the average duration unemployed people had been in unemployment was 17 weeks. In a steady state, the actual durations of completed spells should just be twice the length of current durations of a random sample of currently unemployed, leading to the calibrated value).

of an increase in potential UI durations on the unemployment rate we use a steady state framework.[44] Again ignoring any issue related to matching, based on our main estimates an extension of potential benefit duration from 26 to 99 weeks would be predicted to lead to a substantial rise in unemployment rates from 5.2 to 6.1 percentage points.

These predictions change considerably when we extend our calibrations to take into account various macroeconomic factors. Table 6 shows the simulated effect of increases in potential UI durations on the unemployment rate for different baseline values of unemployment, under different assumptions on search externalities, for increases and decreases in UI durations, and for different values of the RD (partial equilibrium) elasticity. The main point of the table is that allowing for search externalities can substantially reduce the implied aggregate effects of estimated responses to UI extensions at the individual-level. For example, if we account for search externalities, our main estimate of the non-employment elasticity from Table 2 now implies an increase in the unemployment rate of 0.5 instead of 0.9 percentage points, a reduction of the relative increase from 20 to 10 percent.

These effects are further reduced if we also account for the fact that in the United States only about 50 percent of unemployed workers actually receive UI (Congressional Budget Office 2004). As shown in the Web Appendix, the general equilibrium effect, which takes into account that only the insured fraction of the population is directly affected by the extension and that there may be spillover effects on the unemployed, is given as: $\frac{dD}{dP}\Big|^{GE} = \alpha\rho \frac{dD}{dP}\Big|^{RD}$, where ρ denotes the fraction of unemployed receiving UI benefits. In our calculations shown in Table 6, this reduces the effect further to an increase in unemployment rates of 0.2 percentage points.[45] If we were to further

[44]I.e., we use the formula for the unemployment rate $\hat{UR} = \delta / \left(\delta + 1/(a * P^{\eta^{RD}_{D,P}})\right)$, where δ is the job destruction rate. The job destruction rate can be calibrated from the current unemployment rate, actual unemployment durations and a: $\delta = \frac{UR * \frac{1}{ActDur}}{1-UR}$. The resulting number is similar to estimates for the weekly job destruction rate obtained from other sources.

[45]Our institutional setting is ideal for estimating the micro effect, but because the treatment and control groups are also almost perfect substitutes in the labor markets, our RD design does not yield variation at the market wide level, which would be required to estimate a macro effect. Identification of the macro-effect require sharp, exogenous market-wide extensions in UI durations in one market, with a similar, unaffected market as control variable. In most settings, time trends and market differences in unemployment rates make this approach difficult to implement. To our knowledge the only paper that provides some evidence regarding search externalities is Levine (1993). Levine (1993) estimates the spillover effects of higher benefit levels from the insured to the uninsured. He finds substantial

assume that the vacancy ratio was low for reasons other than UI durations, the reduction in the marginal effect of individuals' search effort predicted by the matching function is substantial.[46]

The simulations in Table 6 do not take several channels into account that are likely to further reduce the aggregate impact of UI extensions. For example, when UI extensions are enacted step by step, as typically done in the United States, different workers are eligible for different durations, and workers may have difficulties predicting how long they will actually be eligible to receive UI benefits (e.g., Needels and Nicholson 2004), likely reducing the effect of UI extensions on labor supply relative to our steady state simulation. Another channel that our approach ignores is the potential effect of UI on aggregate demand. UI has long been viewed as an automatic business cycle stabilizer by increasing government spending during recessions, thus dampening the economic downturn (e.g., Congressional Budget Office 2010). Since we do not have any direct estimates for this effect in our sample, it is best to view our counterfactual simulation as holding government spending constant, i.e. the money that is not spent on UI would have been spent on something with an equal GDP multiplier.

We also abstract from channels that may raise the aggregate effects of UI, such as responses in vacancy rates and job destruction to the increased generosity of UI benefits. As argued above, such a response may be weaker in large downturns when UI extensions typically occur. Even if such responses are present, our approximations can be thought of as holding in the short run.

Overall, our discussion and the findings in Table 6 suggest that in large downturns the effect of extensions in the duration of UI on the aggregate unemployment rate or mean non-employment duration are likely smaller than what is implied by our main RD estimates. This reinforces our main conclusion regarding the welfare effects of extensions in UI durations in recessions.

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spillover effects, that indicate that most of the disincentive effect for the insured may be cancelled out by higher job finding probabilities of the uninsured. As a result, he finds no effect of a rise in the generosity of UI in on state-level unemployment rates.

[46] For example, in the United States, the number of unemployed workers per vacancy was estimated by the Bureau of Labor Statistics to be five in the peak of the 2008 recession, compared to two in the recovery after the 2002 recession. At $\alpha = 0.5$, this leads to a decline in the effect of search effort on the job finding rate of about two thirds.

7 Conclusion

In this paper, we evaluate the cost and benefits of large extensions in the duration of unemployment insurance benefits (UI) in recessions. We show in a search model with liquidity constraints that the welfare effect of UI extensions is the sum of two components: the benefit provided by the additional coverage for individuals who otherwise would have exhausted UI benefits, and the cost due to the disincentive effect of UI, which leads to an increased tax burden for the employed. We estimate these two effects using the universe of unemployment spells in Germany, 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 quasi experiments throughout the business cycle. This allows us to estimate the labor supply and extended coverage effect of UI in very different states of the economy. We find a modest effect of extended UI on non-employment durations of comparable magnitude to what has been found before. This effect is quite stable in different economic environments. At best some specifications point to slightly smaller non-employment effects during deep 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.

These findings have several important implications. First the welfare formula that we derive from our model, together with our findings of weakly declining disincentive effects and strongly countercyclical exhaustion rates implies that it is socially optimal for UI benefits durations to be counter-cyclical. This provides a justification for the current system in the United States, since it indicates that the welfare effect of UI extensions in economic downturns is likely positive. Furthermore it suggests that countries with constant UI durations over the cycle, such as Germany and most other European countries, may raise welfare by moving to a system with counter-cyclical durations. Second, we show that the modest labor supply elasticities we obtain imply non-negligible effects on aggregate unemployment rates only in the absence of search externalities, imperfect take up of UI benefits, or a role for slack labor markets. In the presence of congestion effects, an effect of vacancy rates on matching, and potentially incomplete take-up of UI benefits, the effect of UI extensions on unemployment rates are reduced, especially in larger recessions. Third, our findings confirm previous assessments that effects of parameters of the UI system appear too small to explain the large observed differences in the evolution of unemployment durations and unemployment rates within and between countries.

Finally, we should point out some limitations of our sample and extensions for future work. Our results are based on middle-aged workers who become unemployed after a prolonged spell of continuous employment. While our findings are very robust across all of the sub-groups we consider, including workers with weaker labor force attachment, our research design does not allow us to assess differences in the effect of UI for younger workers. Our research design also does not allow us to directly assess the labor supply effect of indefinite unemployment assistance (UA) available in Germany after UI is exhausted. Again our results indicate a robustness to variation in the likelihood unemployment assistance, but assessing the labor supply effect of UA directly is an important avenue for future research. Similarly, given we show that as in Chetty (2008) the welfare effect of UI extensions can be expressed in terms of sufficient statistics depending on income and substitution effects of UI extensions, a promising avenue for further research is the potentially changing role of assets in job search decisions over the business cycle.

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Tables

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

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

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

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

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

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

Table 3: Regression Discontinuity Estimates of Effect Of Potential Unemployment Insurance (UI) Benefit Duration on Additional Employment Outcomes

	(1) Time until Claim	(2) Ever emp. again	(3) Emp. 5 years later	(4) UI or UA 5 years later
D(age>=42)	-0.00089 [0.020]	-0.01 [0.0022]**	-0.0041 [0.0029]	0.0049 [0.0021]*
Observations	452749	452749	452749	452749
D(age>=44)	0.016 [0.021]	-0.0056 [0.0024]*	-0.0076 [0.0030]*	0.0051 [0.0023]*
Observations	450280	450280	450280	450280
D(age>=49)	-0.0027 [0.025]	-0.0076 [0.0036]*	-0.0012 [0.0038]	0.0047 [0.0032]
Observations	329680	329680	329680	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 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. UA refers to means tested unlimited unemployment assistance available at exhaustion of UI benefits (see text).

Table 4: The Correlation of Regression Discontinuity Estimates of the Effect of Potential Unemployment Insurance (UI) Duration (P) on Months of Acutal Benefit Receipt and Months of Nonemployment with the Economic Environment over Time and Across Industries

	(1) Mean & Standard Deviation	(2) Nonemp. Duration Elasticity $\eta_{D,P}$	(3) Nonemp. Duration Marg. Effect $\frac{dD}{dP}$	(4) UI-Benefits Duration Marg. Effect $\frac{dB}{dP}$	(5) Nonemp. Duration Marg. Effect Reweighted to Characteristics of Year 2000	(6) UI-Benefits Duration Marg. Effect Reweighted to Characteristics of Year 2000	(7) Positive Welfare Effect of UI Ext.: Additional UI-Ben. holding Survivor Function Constant $\left.\frac{dB}{dP}\right\|_1$	(8) Negative Welfare Effect of UI Ext.: Additional UI-Ben. due to Shift of Survivor Function $\left.\frac{dB}{dP}\right\|_2$
Unemployment Rate	9.09	-0.014	-0.013	-0.0096	-0.015	-0.011	-.00009	-0.003
	[1.64]	[0.0067]*	[0.0061]*	[0.0059]	[0.0075]†	[0.0066]	[.0052]	[0.0064]
Real GDP Growth Year t to Year t+1	2.20	0.0068	0.0038	-0.017	0.0059	-0.018	-0.019	-0.0036
	[1.61]	[0.0075]	[0.0070]	[0.0062]**	[0.0086]	[0.0071]*	[0.0042]**	[0.0061]
Change in Unemployment Rate Year t to Year t+1	0.15	0.0087	0.013	0.045	0.0072	0.048	0.034	0.0089
	[0.83]	[0.013]	[0.012]	[0.0090]**	[0.015]	[0.011]**	[0.0075]**	[0.013]
Mass Layoff Rate	1.31	-0.033	-0.021	0.055	-0.029	0.058	0.069	0.015
	[0.53]	[0.021]	[0.020]	[0.017]**	[0.024]	[0.020]**	[0.013]**	[0.021]
Average Log Wage Change in Year-Quintile Cell	-0.047	0.10	-0.016	-0.56	-0.021	-.61	-0.72	0.011
	[0.079]	[0.16]	[0.15]	[0.087]**	[0.21]	[0.10]**	[0.11]**	[0.13]
Job Destruction Rate in Industry-Year Cell	0.090	0.28	0.54	1.46	0.51	1.53	1.61	0.42
	[0.032]	[0.48]	[0.47]	[0.27]**	[0.52]	[0.31]**	[0.28]**	[0.61]
Mean of Dependent Variable		0.10	0.095	0.31	0.088	0.27	0.22	0.069
Observations in Rows 1-3		51	51	51	51	51	51	51
Observations in Row 4		240	240	240	240	240	240	240
Observations in Row 5		501	501	501	501	501	501	501

Notes: Stars indicate confidence levels: †P<.1, * P<.05, ** P<.01.
Columns (2)-(5) report coeffients from a two-step regression. In the first step the effect of extended UI durations on actual benefit durations (B) and nonemployment durations (D) are estimated separately for all years (or industry-years) and age thresholds using the regression discontinuity estimator used in the main analysis. In the second step the resulting elasticities/marginal effects are regressed on different measures of the economic environment. Each reported coefficient represents the coefficient on those measures, given in the row names. The second step regressions also include a dummy for elasticities measured after the 1999 reform.

Table 5: Regression Discontinuity Estimates of Effect of Potential Unemployment Insurance (UI) Durations (P) on Months of Actual UI Benefit Receipt and Months of Nonemployment by Subgroups

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

Notes: The coefficients estimate the magnitude of the change in the dependent variable at the age threshold. Each coefficient is estimated in a separate RD regression that controls linearly for age with different slopes on each side of cutoff. Standard errors (in parentheses) are clustered at the day level (* P<.05, ** P<.01).
The sample consists of individuals starting unemployment spells between July 1987 and March 1999, who had worked for at least 52 months in the last 7 years without intermittent UI spell. The probability of receiving unemployment assitance is estimated by probit using predetermined demographic characteristics and career outcomes (see the text and Web Appendix).

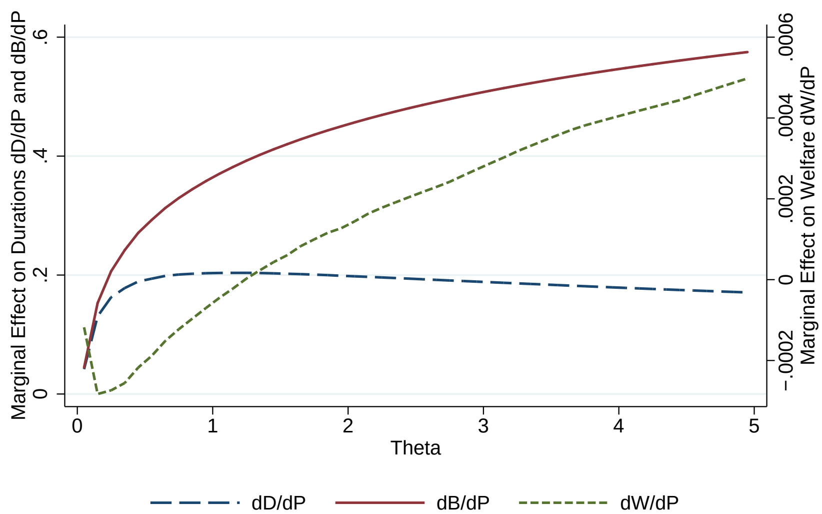
Table 6: Simulating the Effect of Unemployment Insurance (UI) Extensions on the Unemployment Rate in the United States in the Presence of Search Externalities and Imperfect UI Take-Up

	Baseline Estimate (Table 1)	Interacted Model Point Estimate Change GDP -4% (Table 6)	Interacted Model Point Estimate Change GDP -4% Upper Bound of C.I.	Extrapolated Elasticity for Probability of Unemp Ass. = 0
Implied Elasticity of Nonemp. Duration	**0.13**	**0.054**	**0.113**	**0.24**
Simulation 1: Extending UI Durations from 26 to 104 weeks in March 2008 Baseline **Unemployment Rate = 5.2%**				
Partial Equilibrium Extrapolation	6.1%	5.5%	5.9%	7.0%
Assuming Matching Function $\alpha = 0.5$	5.7%	5.4%	5.6%	6.1%
Assuming Matching Function and 50% of Unemployed receive UI	5.4%	5.3%	5.4%	5.6%
Simulation 2: Decreasing Potential UI Durations from 104 to 26 weeks in February 2010 Baseline **Unemployment Rate = 10.4%**				
Partial Equilibrium Extrapolation	8.9%	9.7%	9.1%	7.8%
Assuming Matching Function $\alpha = 0.5$	9.6%	10.1%	9.7%	9.1%
Assuming Matching Function and 50% of Unemployed receive UI	10.0%	10.2%	10.1%	9.7%

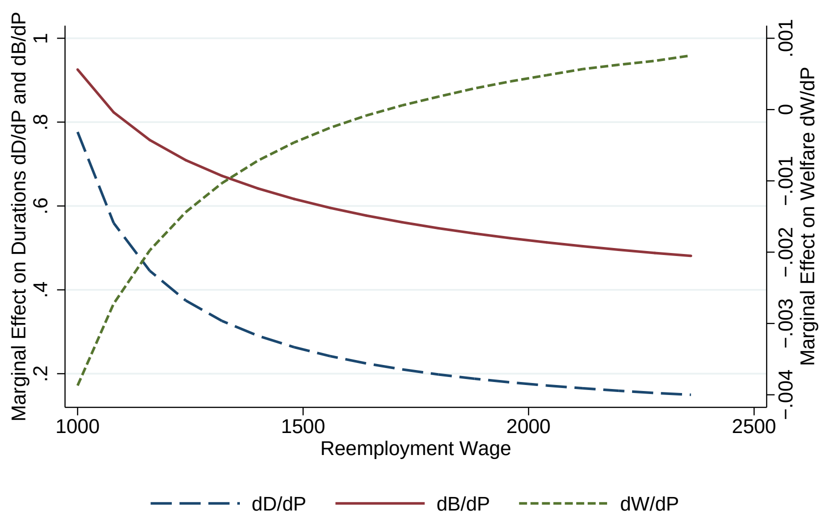
Notes: The partial equilibrium extrapolation uses $UR = \delta/(\lambda+\delta)$, where $\lambda = aP^{\eta}$ is a constant elasticity function relating our estimates to the outflow rate, and δ, the job destruction rate, is calibrated based on 2008 data. The remaining extrapolations modify the elasticity η imposing a matching function or imperfect take-up, respectively. For further details on the simulation see text in Section 6.1.

Figures

Figure 1: The Variation of the Effect of Extensions in Potential Unemployment Insurance (UI) Duration (P) on Months of Actual UI Benefits (B), Months of Nonemployment (D) and Welfare (W) - Simulation of Differences in Search Costs and Reemployment Wages in Calibrated Job Search Model



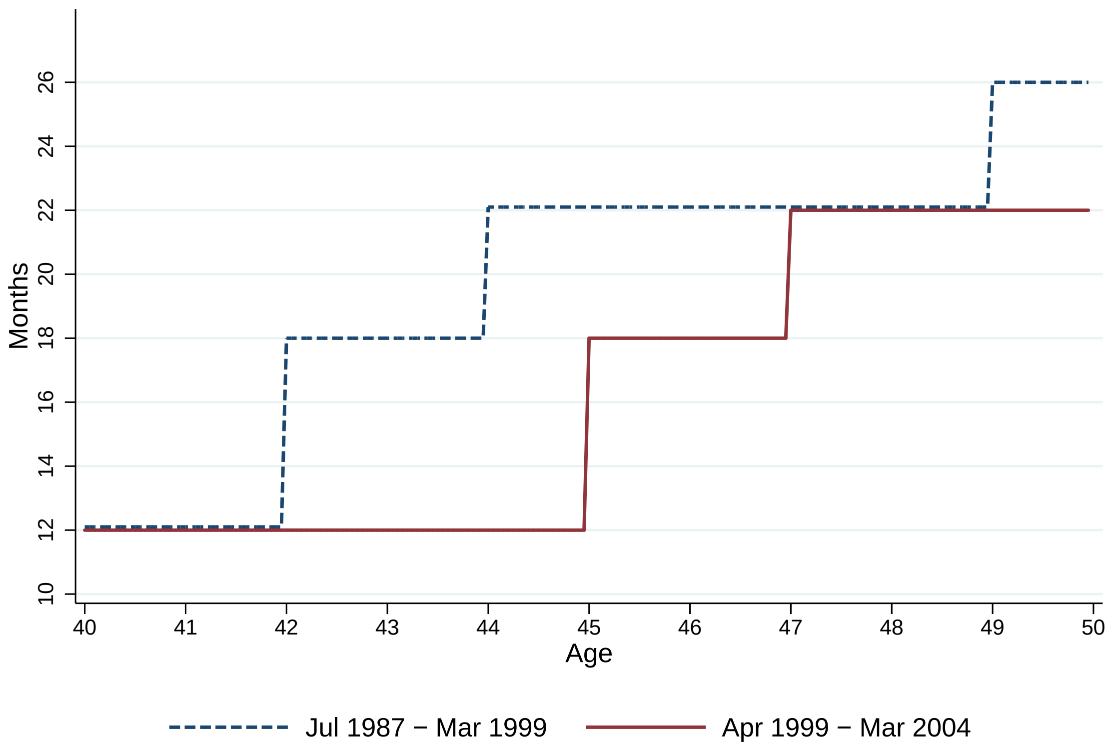
(a) Variation in Marginal Search Cost (θ)



(b) Variation in Reemployment Wage

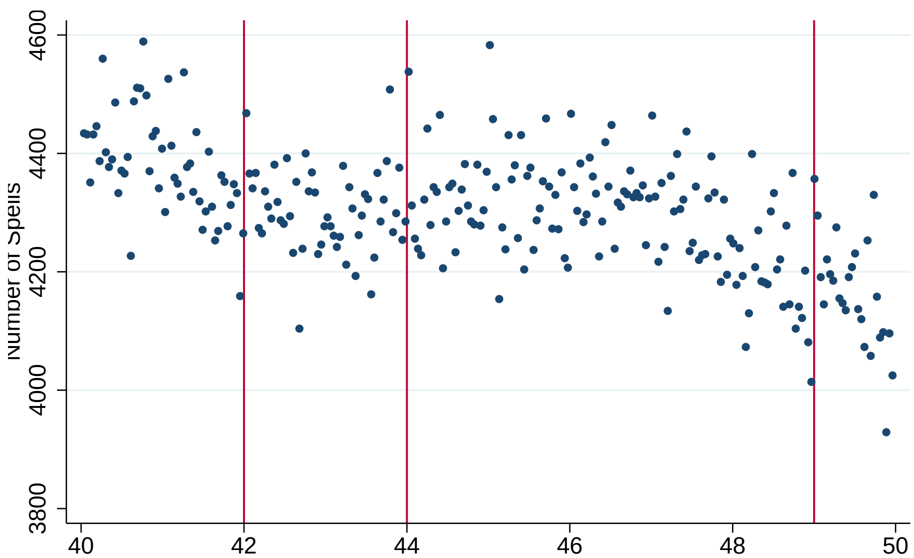
Notes: Simulation of search model calibrated to match characteristics of German UI spells during the sample period (see text in Section 2 for details).

Figure 2: Potential Unemployment Insurance Durations by Period for Workers with High Prior Labor Force Attachment

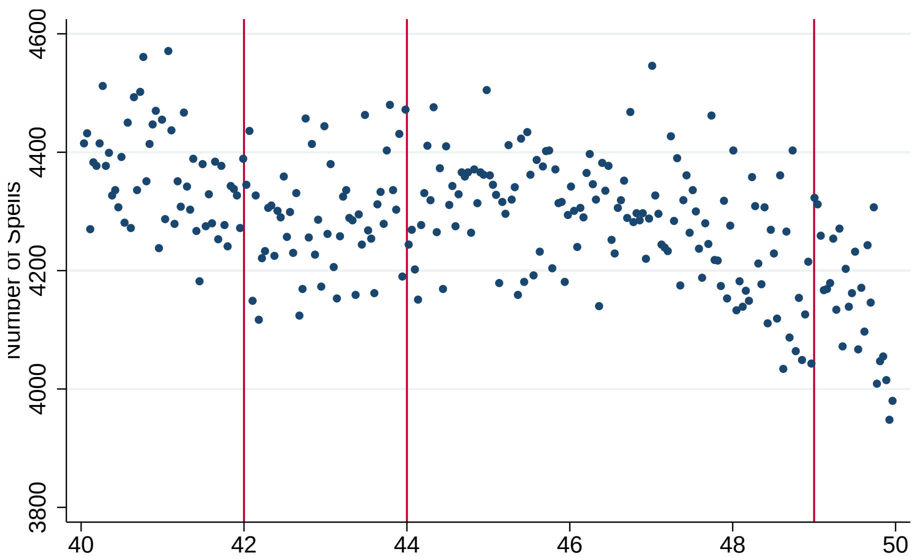


Notes: The figure shows how potential unemployment insurance (UI) durations vary with age and over time for unemployed individuals workers who had worked for at least 52 months in the last 7 years without intermittent UI spell.

Figure 3: Frequency of Spells Around Age Cutoffs for Potential Unemployment Insurance (UI) Durations - Period July 1987 to March 1999



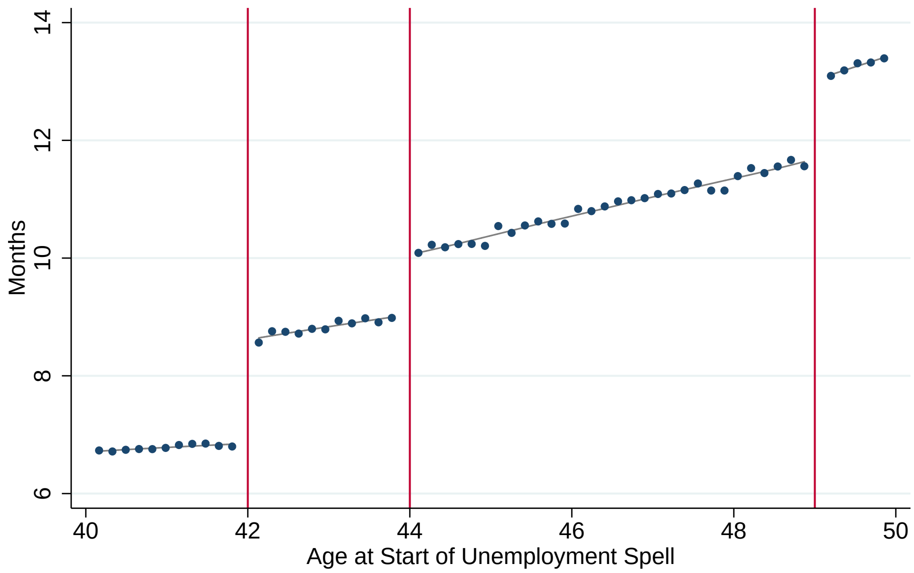
(a) Age on date of UI claim



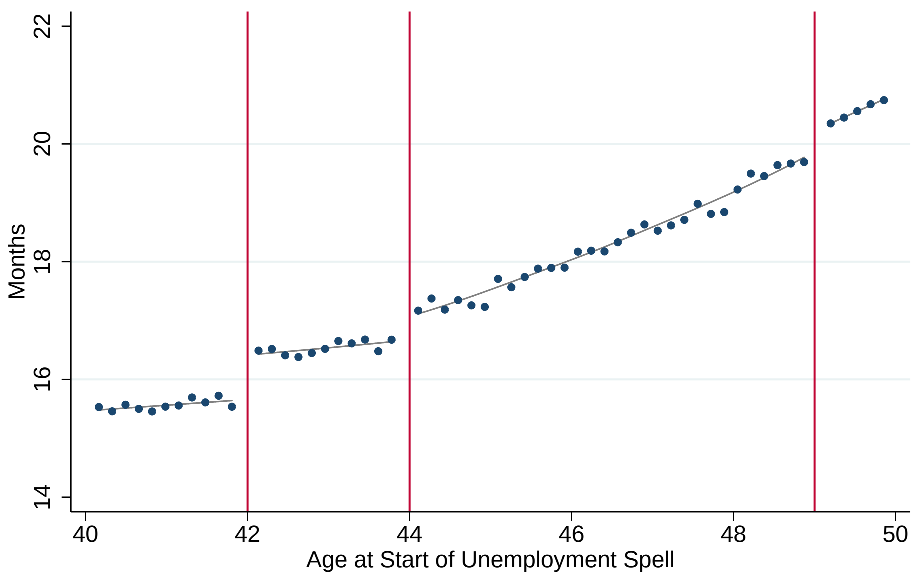
(b) Age at Job Loss

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

Figure 4: The Effect of Potential Duration in Unemployment Insurance (UI) Benefits on Months of Actual UI Benefit and Months of Nonemployment by Age - Period 1987 to 1999



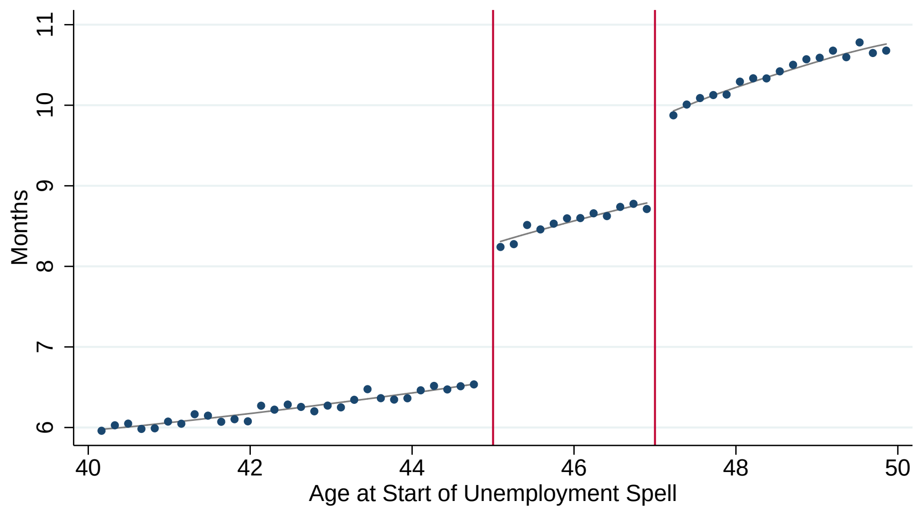
(a) Actual UI Duration



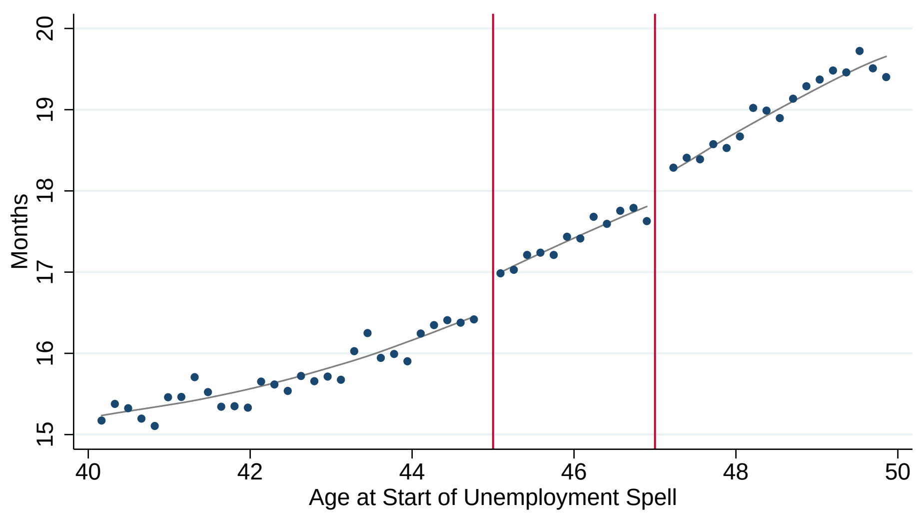
(b) Nonemployment Duration

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

Figure 5: The Effect of Potential Duration in Unemployment Insurance (UI) Benefits on Months of Actual UI Benefit and Months of Nonemployment by Age - Period 1999 to 2004



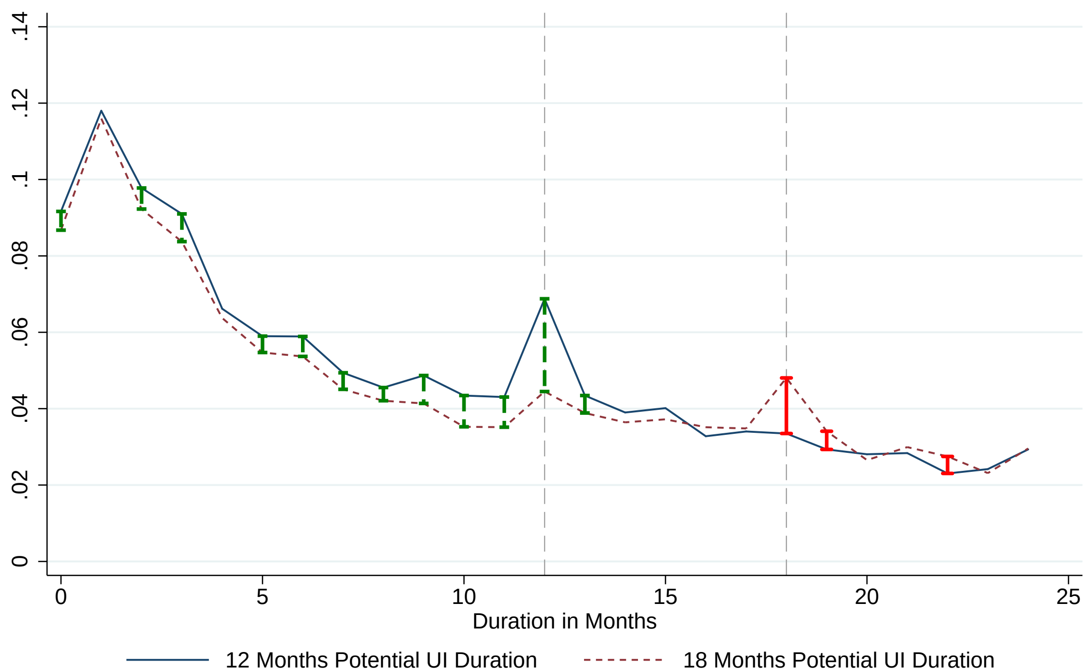
(a) Actual UI Durations



(b) Non-employment Durations

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

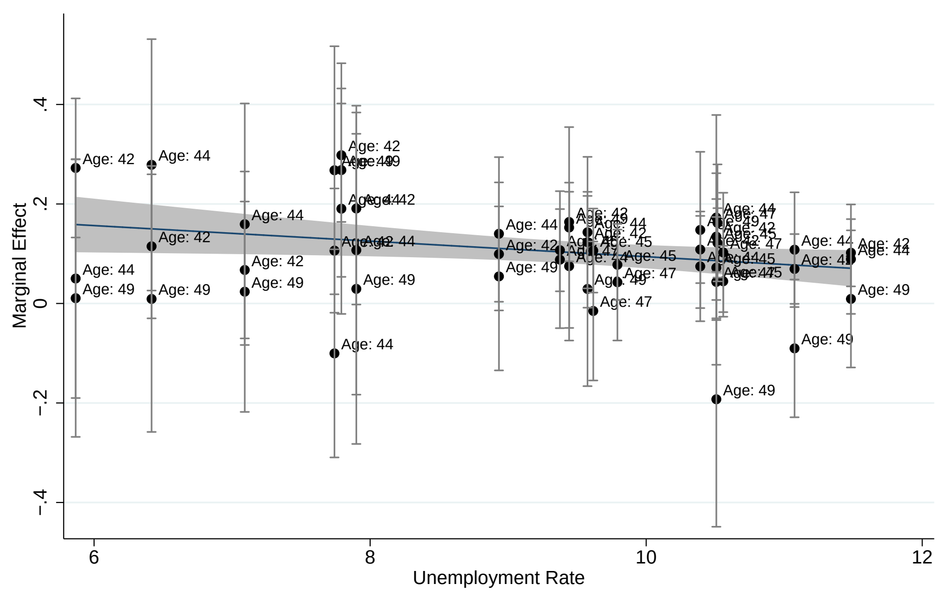
Figure 6: Effect of Increasing Potential Unemployment Insurance (UI) Durations from 12 to 18 Months on the Hazard Functions - Regression Discontinuity Estimate at Age 42 Discontinuity



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 are unemployed worker claiming UI between July 1987 and March 1999 who had worked for at least 52 months in the last 7 years without intermittent UI spell. For details see text and Web Appendix.

Figure 7: Variation in Regression Discontinuity Estimates of Marginal Effect of Potential Unemployment Insurance (UI) Duration on Months of Nonemployment over Time and with Economic Environment

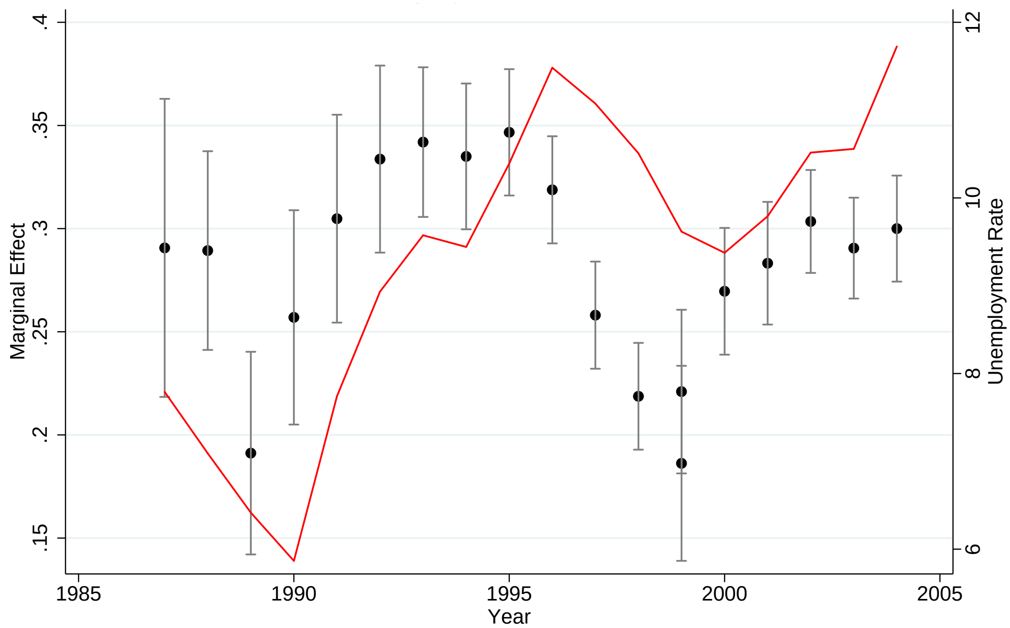
(a) Elasticities at the Age 42 and Age 45 Discontinuities by Year and the Unemployment Rate



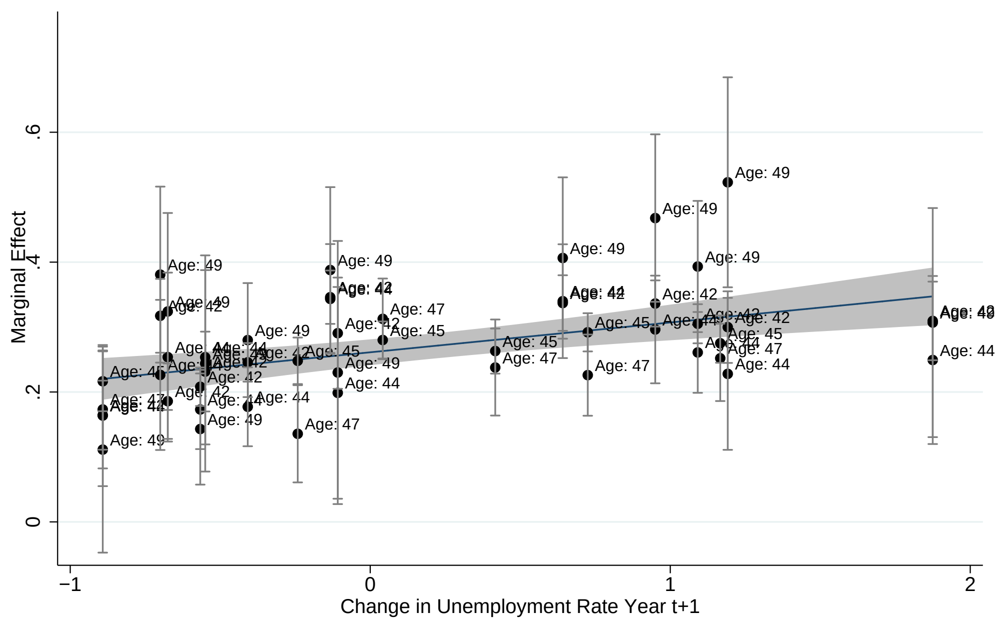
(b) Scatter Plot all estimated Elasticities vs. Unemployment Rate

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 samples are described in Figures 3 and 5. The horizontal line in the bottom figure is the regression line from the regression of elasticities on the unemployment rate.

Figure 8: Variation in Regression Discontinuity Estimates of Marginal Effect of Potential Unemployment Insurance (UI) Duration on Months of Actual UI Duration over Time and with Economic Environment



(a) Marginal Effect at the Age 42 and Age 45 Discontinuities by Year and the Unemployment Rate in Year t+1



(b) Scatter Plot $\frac{dB}{dP}$ vs. Change in Unemployment Rate between t and t+1

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 line in the bottom figure is the regression line from the regression of elasticities on the unemployment rate. The samples are described in Figures 3 and 5.

Appendix

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

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

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