

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

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Abstract: This paper evaluates the impact of large extensions in the duration of unemployment insurance (UI) in booms and recessions on labor supply, job quality, and long-term employment outcomes. Using multiple sharp eligibility thresholds by age in the German UI system we estimate regression discontinuity estimates for each year over twenty years using data on the universe of unemployment spells and career histories. We find large extensions in UI durations have modest effects on non-employment durations that are similar across demographic groups, stable over the business-cycle, and at best declining somewhat in large recessions. We do not find strong effects of increased UI duration on multiple measures of average job quality or on longer-term employment outcomes. Our findings imply that large expansions in UI during recessions are unlikely to lead to sizable increases in unemployment duration or the unemployment rate, to contribute to unemployment persistence, or to lead to worsening job outcomes for the long-term unemployed. We also provide evidence that the benefit of extensions in UI benefits in recessions in terms of a reduced benefit exhaustion rate is likely to outweigh its costs in terms of higher non-employment durations.

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1 Introduction

An often used policy tool to ease the hardship of job losers in recessions is to extend the duration of unemployment insurance (UI) benefits.¹ The primary goal of these increases is to provide income replacement and prevent hardship among unemployment workers. Existing estimates suggest UI benefits largely achieve this goal (e.g., Gruber 1997). The beneficial effect of UI benefits is likely to rise in recessions, when unemployment durations are longer. Yet, there is a longstanding concern that the insurance benefit of UI comes at the cost of distorting labor supply incentives. Over the short term, it has been argued that these distortions can lead to increases in the duration of unemployment and a rise in the aggregate unemployment rate. Over the long term, they may contribute to a detachment of UI recipients from the labor force, lower their reemployability and wages, and raise their dependency on public transfers.²

These potential costs receive particular attention in larger recessions, when extension in UI durations can be substantial. On the one hand, it has been argued that the potential cost of UI may be even greater during recessions when the incidence and cost of job loss are particularly severe (von Wachter, Song, and Manchester 2009). In this case the effective replacement rate may raise beyond the typical replacement rate and imply stronger and possibly lasting effects on unemployment (Ljungqvist and Sargent 1998, 2008).³ On the other hand, many observers suggest that disincentive effects of UI decline in times when it is difficult to find a job (e.g., Krueger and Meyer 2002). In addition, it has been argued that in recessions low vacancy rates reduce

¹Extended UI has been a prominent feature of downturns in the United States (e.g., Lake 2002), with potential duration of UI benefits reaching up to two years at the peak of the 2008 recession. Similarly, in many European countries unemployment insurance benefits were raised in the course of the 1980s in response to increasing unemployment. For example, in Germany unemployment insurance benefits were increased from 12 to 18 and up to 32 months, depending on the demographic group (e.g., Hunt 1995).

²These economic arguments 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 repeatedly 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).

³Ljungqvist 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, Ljungqvist and Sargent (1998) 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.

the impact of individuals' search decisions on their chance of getting a job and on the aggregate unemployment rate.⁴

Existing estimates indeed point to non-negligible effects of increased UI benefits and UI durations on non-employment spells (e.g., Katz and Meyer 1990, Hunt 1995). However, based on the current literature it is difficult to evaluate the effect of extensions in UI durations in recessions on labor supply, long-term labor force attachment, or long-term job quality. Existing estimates are often based on relatively modest expansions of UI insurance. Moreover, most estimates do not distinguish between the effect of UI durations between booms or recessions. In fact, it is typically difficult to test for differences in these effects, since at least in the United States extensions in UI durations usually occur in recessionary environments.⁵

Next to providing income support, a second goal of UI extensions is to aid workers laid off in recessions to find higher quality jobs. Yet, not much is known about the effect of large extensions in the duration of UI benefits on job quality (e.g., Krueger and Meyer 2002). While an older literature reports positive effects of UI durations on wages (e.g., Addison and Blackburn 2000), more recent studies report no effects on wages or job tenure (e.g., Card, Chetty and Weber 2007a). Similarly, despite ongoing concerns about persistent effects of unemployment duration (e.g., Maching and Manning 2009), there is also little direct evidence on the longer term consequences of UI extensions on labor force attachment or UI receipt.

In this paper we evaluate the short- and long-term impact of extended UI duration during different labor market states on both non-employment durations and job outcomes. We then use our estimates to assess how the costs and the benefits of UI durations change over the business cycle. To do so, we exploit differences in the UI duration for different age groups under multiple policy regimes in Germany, leading to sharp and large increases in UI eligibility by age. We show that these differences lead to a valid regression discontinuity design of the effect of UI duration on

⁴There 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.

⁵Card 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. Tannery and Jurajda (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.

non-employment, wages, and other labor market outcomes of workers who had stable labor force attachment before receiving UI. We implement this approach using detailed administrative data on the universe of unemployment spells and ensuing job outcomes in Germany from the mid-1980s to the present.

This research design allows us to estimate labor supply elasticities with respect to UI durations in Germany for large differential expansions for mature workers with stable labor force attachment. The effects on labor supply we find are moderate, similar for different increases in UI duration, similar across demographic groups, and similar for workers with weaker labor force attachment. Among others, this suggests that larger expansions in UI durations such as extended UI do not appear to have very different effects on labor supply than shorter UI durations. Our estimates are at the lower end of estimates from the United States. As we discuss, they appear somewhat smaller than estimates for Germany in Hunt (1995), and somewhat larger than comparable estimates from Austria (Card, Chetty, and Weber 2007a).⁶

We then exploit the fact that our regression discontinuity design implies a situation close to the ideal experiment for comparing differences in unemployment regimes on unemployment durations during difficult economic times. By comparing workers just above and below our age cutoffs in periods with a high and low degree of unemployment, we can assess the effect of changes in generosity of UI during different economic environments. Furthermore exploiting variation in the degree of sector-specific changes vis-a-vis the economy-wide state of labor demand also allows us to control for differences in the overall arrival rate of jobs. The results point to little systematic variation of the effect of UI over the business cycle. At best, some specifications suggest disincentive effects of UI durations decline in large recessions.

Our third main finding concerns the question of the effect of large extensions in UI duration on job quality and long-term employment outcomes. We do not find a beneficial effect of increased UI duration for any of the job outcomes we consider, including wages, wage growth, or the probability of finding a job in the same region, occupation, or industry, confirming findings of recent studies

⁶Our elasticities are smaller than in Lalive (2008) who estimated the effects of extended UI in Austria using a regression discontinuity design in Austria on a sample of older workers.

of based on UI extensions that are smaller (Card, Chetty, and Weber 2007a) or in a different context (e.g., Lalive 2007, Van Ours and Vodopivec 2008). Similarly, we find that extensions in UI duration have at most very small effects on the probability of employment, the probability of receiving UI, or the wage five years after beginning of the initial unemployment spell. Thus, the extensions in UI durations we study neither lead to improvements or declines in job quality, nor do they substantially worsen the long-term employment outlook of affected workers. Again, these findings are robust over the business cycle, suggesting UI extensions do not contribute to hysteresis from prolonged unemployment or a decline in job matches due to long unemployment spells.

The paper discusses the implications of these findings for the effect of extended UI on the aggregate rate of unemployment and on welfare. The moderate labor supply elasticities imply substantial increases in the unemployment rate from large extensions in UI only in the absence of frictions. If one accounts for congestion effects, the effect of lower vacancy rates on matching, and potentially incomplete take up of UI, the implied employment effects of extended UI are substantially smaller, in particular in large recessions. We also discuss under what circumstances our results allow us to make statements about the welfare effect of UI extensions. Using a version of the model used by Chetty (2008) to evaluate the welfare effect of the level of UI benefits, we show that increases in the maximum duration of UI benefits in recessions are optimal if the exhaustion rate increases more than the marginal effect of UI duration on non-employment durations. We show that the exhaustion rate is strongly countercyclical, indicating that the benefits of UI extensions in recessions in terms of reduced exhaustion rates is likely to outweigh the costs in terms of higher non-employment durations.

We contribute to several aspects of the literature on the effect of UI on employment and job outcomes of UI beneficiaries. First, we obtain new estimates of labor supply elasticities for mature workers 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. Second, this is the first paper to replicate our regression discontinuity estimates for different economic

regimes to assess whether the duration of UI has stronger or weaker employment effects in booms and recessions. We also explicitly discuss various factors determining the effect of UI extensions on aggregate unemployment in recessions. Third, our paper is the first study examining the longer term effects of UI duration on job quality and employment outcomes. This allows a more complete assessment of the costs and benefits of UI extensions that includes the effects on job matching and longer-term employment outcomes. Finally, we are the first paper to explicitly consider both the costs and benefits of extensions in UI benefits during recessions.

The outline of the paper is as follows. In section 2 we describe the institutional environment in Germany. Section 3 briefly reviews our administrative data and our empirical approach. Sections 4 to 6 contain our main findings regarding the short- and long-term effect of extended UI on labor supply and job search outcomes over the business cycle. Section 7 discusses the implications of our findings for effects of UI extensions on the unemployment rate and on welfare. Section 8 concludes, summarizes caveats of our approach, and derives suggestions for future research.

2 The Unemployment Insurance System in Germany

From the 1980s until the early 2000s the German unemployment insurance (UI) system consisted of two tiers. The first tier provides income replacement to eligible workers who lose their job without fault at a fixed replacement rate over a fixed period of time.⁷ This principal tier is thus of similar structure as the UI system in the United States. For an individual without children the replacement rate is 63 percent of previous net earnings.⁸ A key difference with respect to the United States UI system is that the maximum duration of benefits is tied to recipients' exact age at the beginning of the UI spell and to their prior labor force history. It is this difference

⁷An unemployed worker is eligible to receive unemployment insurance benefits if he has worked for at least 12 months in the previous 3 years. Workers are barred from receiving unemployment benefits if they quit without good cause or are fired for misconduct. Furthermore after a period of 4 months of UIB reciprocity they can be sanctioned for not accepting job offers. The penalty is loss of benefits of up to 12 months, but the sanctions appear to be rarely enforced (Wilke 2005).

⁸For individuals with children the replacement rate is 68 percent. According to Hunt (1995) a cap on the amount one may receive exists but only affects about 1 percent of the recipients. The absence of a cap makes the average replacement rate more generous than in the United States, where maximum weekly benefits imply that nominal replacement rates of about 50 percent turn to average replacement rates of close to 40 percent. In Germany, UI benefits are not taxed themselves, but can push total income into a higher income tax bracket.

which we exploit to estimate the effect of extensions in duration of UI benefits on employment and wages. A second tier of income support called Unemployment Assistance (UA) provides benefits for individuals who exhaust the maximum UI duration. UA benefits have no maximum duration and are 53 percent of previous net earnings; however, unlike regular UI benefits, other sources of income (such as spouses income or income from financial assets) are subtracted and the receipt of UA is means tested.⁹

The main feature of the German UI system we exploit are differences in maximum potential duration of benefit receipt by exact age at the time a worker claims UI benefits. In principle, the maximum benefit duration depends on both age and work experience. As further explained below, to obtain precise measures of potential maximum UI durations, we restrict ourselves to workers who by their employment history are entitled to the maximum durations in their respective age-group. For these workers, the system implies large increases in potential UI duration by age of claiming benefits. In the first period, between July 1987 and March 1999, the maximum UI duration for workers who were younger than 42 was 12 months. For workers age 42 to 43 the maximum increased to 18 months; for workers age 44 to 48 (49 to 54), the maximum duration further rose to 22 (26) months. The resulting discontinuities in maximum benefit duration by age are shown in the upper line in Figure 1 and are those underlying the main results in the paper.

The discontinuities based on exact age allow us to estimate the effect of extensions in UI durations on employment and wages using a regression discontinuity design. The particular institutional structure is ideal in that it provides large extensions in the duration of UI at multiple age thresholds that are stable over long stretches of time. In addition, a reform occurring at the end of our sample period allows us to validate our main sample design. As shown in Figure 1, starting in April 1999 the maximum potential UI durations were lowered and the age thresholds were shifted 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. The goal of this reform was to reduce potential disincentive effects of unemployment insurance. Correspondingly, the reform also imposed stricter sanctions

⁹For individuals with children the UI (UA) replacement rate was 68 (58) percent. These rates were reduced in 1994 to 67 (57) percent for individuals with children and 60 (50) percent for individuals without children.

for individuals who did not comply with eligibility rules (See Boone et al., 2002, 2004).¹⁰

Clearly, a potential concern for implementing a regression discontinuity (RD) design in this context is the fact that workers have control over when to claim UI and employers have control over when to lay off workers. This could in principle lead to sorting around the age thresholds and potential biases of the RD estimates. We will return to this potential threat below at length and show that in our context it is unlikely to affect our findings. Another potential concern is that the presence of an unlimited second tier of benefits through unemployment assistance (UA) and the relatively high replacement rate affects the external validity of our findings, in particular with respect to the UI system in United States. Higher UI benefits imply that our already modest results would overpredict the effect relative to a system with lower replacement rates. The presence of UA would predict a smaller effect. Below, we will argue that the presence of UA is unlikely to strongly affect our findings. On the one hand, we find important responses to UI extensions before benefits are exhausted. On the other hand, among exhaustees, only about 50 percent of workers in our sample actually receive UA. Furthermore, our large and rich data set allows us to use variation in who is likely to receive UA to address the external validity of our estimates.

3 Data and Empirical Approach

3.1 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

¹⁰The reform was enacted in 1997 but phased in gradually, so that for people in the highest experience group, that constitutes our analysis sample, it only took effect in April 1999 (See Arntz, Simon 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). The last reform (Hartz IV) took effect on January 1st, 2005 and overhauled the UI system. The main changes were the merge of UA with the general social assistance (welfare) system, which made payments unrelated to previous earnings and purely means tested, and a change in potential UI durations. 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. In principle the early stages of the Hartz reform that took place in 2003 and 2004 might have affected the responsiveness of workers to UI. For this reason we estimated our results dropping 2003 and 2004 from our sample, which does not affect our results.

unemployment insurance benefits, as well as corresponding wages and benefit levels.¹¹ Compared to many other social security datasets, this data is very detailed. We observe several demographic characteristics, namely gender, education, birthdate, nationality, place of residence and work, as well as detailed job characteristics, such as average daily wage, occupation, industry, and characteristics of the employer.¹² Overall the data covers a total of about 1 billion employment and unemployment spells and about 24 million workers per year.

This data source is ideal for the purposes of studying the short- and long-term effects of UI extensions on search outcomes over the business cycle in several respects. Detailed information and large sample sizes allow us to implement a regression discontinuity (RD) design based on the age thresholds in UI eligibility described in the previous section. Thereby, longitudinal day-to-day information on career histories allows us to precisely date the beginning and end of UI spells, and information on exact dates of birth allows us to impute maximum eligibility for UI benefits. Further, detailed demographic characteristics and career histories enable us to assess the assumptions required for our research design. The large sample sizes and comparability over time also allow us to replicate our RD design over different states of the business cycle, by industries, and by demographic groups. Finally, we exploit the information on wages, job characteristics, and long-term employment and wage outcomes to assess the effect of extended UI on a range of additional outcomes not typically studied.

To study the effect of extensions in duration of UI, we created our analysis sample by selecting all non-employment spells in this data, about 36 million. 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, the level of UI benefits, and information about the next job held after non-employment, as well as longer term employment

¹¹Individual 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).

¹²Each 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).

and wage outcomes. Corresponding to the age thresholds we study, we restrict our samples to ages 40 to 49.

Since we do not directly observe whether individuals are unemployed we follow the previous literature and use length of non-employment spells 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 or the end of the observation period. 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. Our results are very robust to the exact choice of the cap.¹³

The main 'treatment' variable we are interested in is the maximum 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 and our detailed information on work histories. This approach works very well for workers who have been employed for a long continuous time and are eligible for the maximum potential durations for their age groups, since for them the rules are very clear. 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 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 stable employment who absent a layoff or a recession would have been unlikely to become unemployed. Nevertheless, it would be interesting and important to broaden of our sample and include less attached workers. We leave this exercise for future work.

Given changes in the institutional framework discussed in the previous section, we consider

¹³An alternative would be to use time-to-next-job for workers who return to employment. Since we find the incidence of censoring does not vary strongly at the eligibility thresholds, our results are largely unaffected by this choice. This was confirmed when we replicated all of our findings with this alternative measure. See the Web Appendix for a summary table of various steps in the sensitivity analysis. See Card, Chetty, and Weber (2007b) for further discussion of alternative measures of unemployment spells.

unemployment spells starting any time between July 1987 and December 2004. Statistics for various samples over this time period are shown in the Data Appendix to the paper. Three observations are noteworthy. First, 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 reemployment are substantially 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 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.¹⁴

Second, 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 mean duration of UI spells is lower, but because the maximum duration of benefits is considerably smaller; the mean duration of non-employment or time between jobs for those reemployed by three years is similar. The average duration of spells is larger than what is typically found in the United States (e.g., Machin and Manning 1999). However, the comparison is made difficult because of differences in data sources and a lack of data on the duration of non-employment spells, particularly in the United States. Where comparable data is available the differences can be smaller. This is found for the duration of UI spells in Card and Levine (2000), or for non-employment durations in the Displaced Worker Survey. 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.¹⁵ As discussed in detail elsewhere, the differences in the duration of unemployment spells are partly due to differences in institutions, partly due to differences in the economic environment.¹⁶ As a

¹⁴This assessment is not affected if in addition we also restrict the sample to workers between 40 and 49 years of age, our main analysis sample for the first threshold (nor if we restrict it to age-ranges of any of the other thresholds, see the Web Appendix).

¹⁵In Appendix Table A1 the fraction of individuals whose spell is censored at 36 months is 23 percent. Given the time since job displacement in the Displaced Worker Survey is based on calendar years and the survey is either in January or 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).

¹⁶The duration of unemployment is smaller in the survey data used by Katz and Meyer (1990a,b), but they discuss

result, the difference is likely to be smaller in larger recessions, when duration of unemployment insurance and non-employment can both increase substantially in the United States.

Third, other observable characteristics of unemployed and UI recipients such as tenure on the previous job, age, fraction citizen, industry distribution, and the exhaustion rate of UI benefits in Germany are broadly similar to their counterparts in the the United States. This is shown in the Web Appendix, which displays average characteristics of unemployed and UI recipients obtained from the March Current Population Survey and the Displaced Worker Survey from 1986 to 2004.¹⁷ The extent and effect of these differences on the interpretation of our estimates is further taken up below.

3.2 Methodology and Internal Validity

The institutional structure and data allow us to estimate the causal effect of UI benefit durations on nonemployment duration and other outcomes using a regression discontinuity design. In a first step, we exploit the sharp age thresholds in eligibility rules for workers with previously high labor force attachment in Germany to estimate the effect of large extensions in UI durations on labor supply, and establish robustness and validity of our regression discontinuity design along several dimensions. We then replicate this approach for every year in our sample, and correlate it with indicators of the business cycle. Finally, we use it to assess the effect of large UI extensions on wages, other job characteristics, and long-term employment outcomes.

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 2009). To obtain parameter estimates for the main causal effects, we follow the now standard regression discontinuity methodology, by estimating variants of the following regression

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). No comparable data on non-employment spells across countries is currently available.

¹⁷As expected, the fraction of the sample that is female is lower and the fraction employed in manufacturing is higher in Germany. 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.

model:

$$y_{ia} = \beta_0 + \beta_1 D_{a \geq a^*} + f(a) + \varepsilon_{ai}, \quad (1)$$

where y_{ia} is an outcome variable, such as non-employment duration, of an individual i of age a . $D_{a \geq a^*}$ is a dummy variable that indicates that an individual is above the age threshold a^* . For our main estimates we focus on the longest period for which the UI system was stable, July 1987 - March 1999, and we use the three sharp thresholds at age 42, 44 and 49.¹⁸ We then replicate this approach for different years, industries, demographic groups, and different outcomes.

The standard RD assumption is that all factors that influence the outcome variable, other than the treatment variable, vary continuously with the forcing variable (which in our case is age of claiming of UI benefits) around the threshold. If this assumption 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. We estimate equation 1 locally around the three cutoffs and specify $f(a)$ as a linear function while allowing different slopes on both sides of the cutoff. We discuss the robustness of our estimates under different bandwidths around the cutoff to assess the validity of the RD design, and settle for a relatively small bandwidth of two years on each side of cutoff.¹⁹

It is possible to include other control variables in the RD regressions, in order to increase the efficiency of the estimates. It turns out that for most of the outcomes we consider, in particular unemployment and non-employment durations, other variables in our data set have little explanatory power (partly because we estimate our model on a relatively homogeneous sample of workers). The efficiency gain from this is therefore very small, so that we prefer to present the raw estimates without controlling for additional variables.

¹⁸There is a 4th discontinuity during this threshold is 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 2009.

¹⁹Another possibility would be to estimate equation 1 with three indicators, one for each age threshold, and to specify $f(a)$ as a global polynomial. The approach that we are presenting here, in using observations close to the cutoff, is generally considered closer in spirit to the RD identifying assumption that treatment is assigned as good as random close to the cutoff. However in practice this does not matter very much and the main results are all apparent from the graphical evidence that we present as well.

An important potential threat to identification exists if individuals have direct control over the forcing variable and this leads to sorting around the cutoff by their underlying unobserved propensity to be non-employed. 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. There are two reasons why this may lead to a bias in our setting: on the one hand employers may prefer to layoff workers who have longer potential benefit durations, perhaps feeling that it is less costly for them. On the other hand those workers with longer potential non-employment durations could decide to delay claiming unemployment benefits until after their birthday if it falls on a cutoff.²⁰ While this incentive may be sizable for workers very close to the cutoff, it very quickly declines further away from the cutoff. By delaying claiming of unemployment benefits the worker gives up benefits she would receive with certainty for an increase in benefits she may only receive if she is unemployed for a long time. As we show below, a majority of workers exit UI before benefit exhaustion. The precise calculation depends of a number of factors, such as the discount rate, but our sense of this is that delaying claiming should only be a relevant option for workers within a few weeks of their birthdays.

Fortunately, our data allow us to investigate the potential of sorting around the eligibility cutoff in detail. In both cases there should be breaks in the density of unemployment spells around the age cutoffs, which we test for.²¹ Furthermore, we test whether other predetermined variables vary smoothly around the cutoffs. To further test for employer-induced sorting, we analyze the date of layoff as an alternative forcing variable.²² To also investigate whether individuals laid-off close

²⁰This is not as far fetched as it may sound. From conversations with an unemployment agency employee we learned that at least in recent years case workers at the agencies are supposed to make unemployed workers aware of the possibility to delay their claims for this reason.

²¹The density test may fail to detect violations of the RD assumptions if some employers prefer to lay off high potential benefit duration workers while others prefer to do the opposite – perhaps because they dislike generous UI insurance – thus counteracting the change in the density. This seems unlikely to us. Furthermore if this were the case and would lead to systematic differences in worker characteristics, it should also show up as discontinuities in other baseline variables at the cutoffs – something we test for.

²²If 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.

to the age cutoff appear to wait to obtain higher benefits, our day-to-day employment data allows us to test for discontinuities in the duration between the last date of employment and the day of claiming UI benefits. To anticipate results from the next section, we conclude from this analysis that our findings are unlikely to be affected by widespread systematic sorting around eligibility thresholds.

4 The Effect of Large UI Extensions on Labor Supply

4.1 The Effect of UI Durations on Nonemployment Durations

Our first set of results pertain to the effect of large increases in UI duration at the three age thresholds on actual take up of UI and labor supply. Our main finding is that the labor supply elasticities of UI duration implied by our regression discontinuities are modest, similar across age thresholds, and robust across alternative specifications. We also show that these effects arise mainly from an increase in time spent between jobs, less from a rise in the fraction of workers permanently leaving the labor force.

Figure 2 (a) shows how the duration of receiving UI varies with the age at the beginning of the unemployment spell. The figure implies that a large number of individuals are substantially affected by the increase in 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 of UI receipt increases to about 8.5 months. There are also clear and large increases at the age 44 and age 49 cutoffs. The increases in receipt duration are quite large, 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 2 (b). There is a clear jump in nonemployment durations at the age 42 cutoff from about 15.6 to 16.4 months of nonemployment. At age 44, nonemployment durations increase from 16.5 to 16.9 months and at age 49 from 19.9 to 20.3. Thus, visual evidence clearly suggests the UI extensions we study lead to significant increases in both take up and non-employment durations at all thresholds.

The marginal effects from estimating equation (1) corresponding to the figures are shown in Table 1. The regression results are very consistent with the graphical analysis. Consider first the estimates using a bandwidth of 2 years for the local linear regression. At the age 42 cutoff nonemployment durations increase by 0.78 months (standard error 0.1 months), at age 44 the increase is 0.41 months and at age 49 the increase is 0.43 months. 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 in the same ballpark across age-groups (0.13, 0.1, and 0.1 for age 42, 44, and 49, respectively), and suggest that for each month 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 nonemployment and UI durations, the implied elasticities are nearly the same for the different cutoffs and of the order of 0.12 to 0.14.²³

These findings have two implications. On the one hand, extensions in UI durations lead to a significant rise in the duration of non-employment. On the other hand, the fact that actual UI durations respond more strongly than non-employment durations imply that a substantial fraction of exhaustees would have exited the labor force in the absence of the extension. Under the more generous system they continue receiving UI benefits instead.

We obtained two additional findings. First, the regression results are robust to the choice of bandwidth. The point estimates are very similar to what is implied by the graphical analysis when we choose a bandwidth of 2 years for the local linear regressions.²⁴ For smaller bandwidths coefficients are very stable for the UI duration regressions, even with bandwidths as small as 0.5 or 0.2 years. For the nonemployment durations they are also in the same ballpark across different bandwidths, but somewhat larger for tighter bandwidths. We have investigated figures with different bandwidths and found that this is due to undersmoothing for the smaller bandwidths. We thus have

²³This is calculated as an increase in nonemployment durations of 0.78 months over an average nonemployment duration around the cutoff of 15 months relative to an increase of 6 months over average potential UI durations of 15 months.

²⁴There is another age discontinuity at age 50 in the eligibility for early retirement. We therefore only use observations between 49 and 50 for estimates of the effect of the age 49 discontinuity, while still using a 2 year window to the right of the 49 cutoff.

most confidence in estimates with 2 year bandwidths. Note that 2 years is already a very narrow bandwidth in comparison to other papers with a similar RD design,²⁵

Second, we 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 2 column (4) shows the probability of ever returning to registered employment again. As shown in Table 2, there is a slight drop at the age 42 cutoff: individuals above the cutoff have a one percent lower probability of ever returning to work again. The effect is even smaller for the other two age thresholds. These effects represent a decline of less than one percent relative to the mean.²⁶ Thus, even though it is statistically significant, the slight decline in the fraction of workers ever returning to work therefore accounts for a very small increase in overall nonemployment durations. As further discussed below, consistent with this finding we also find small effects on other long-term employment outcomes.

4.2 Identification Assumptions

Before further interpreting our findings in the context of the literature, we address potential threats to internal validity discussed at the outset. The overall conclusion from this sensitivity analysis is that our labor supply elasticities represent valid regression discontinuity estimates. The identification assumption of the regression discontinuity design requires that, except for the treatment variable, all factors influencing the outcome variable vary continuously at the points of discontinuity. One approach to assess this assumption is to test for discontinuities in observable characteristics at the threshold by estimating equation (1) with observable characteristics as outcome variables. Table 3 presents results of these regressions. Of the 24 coefficients in Table 3, there are only two statistically significant on the 5 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 RD plots (shown in the Web Appendix) confirm the conclusion that pre-determined characteristics change very little at the thresholds.

²⁵For example Lemieux and Milligan (2008) use a bandwidth of 6 years.

²⁶The fraction of 40 to 49 year old UI recipients ever employed again is 0.84, see Appendix Table 1.

A second standard way of testing the RD assumption is to look at the smoothness of the density around the cutoffs. Figure 3 (a) shows the number of spells in 2 week age intervals. On average there are around 4300 spells in each bin 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 on the 5 percent level for the 42 and 49 cutoff but of very small magnitude.

As explained in the previous section, 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 birthdays 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 permanently shift up.

To see whether workers wait before claiming UI until they are eligible for extended UI durations column (1) of Table 2 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. From the density plots this result is probably not surprising, since if anything the average increase in the duration until claiming would be very small, as we only found a change in the density right around the cutoff. 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 in Table 1 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.

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 4 year intervals that we use for our RD estimation.²⁷ Since the magnitude of this effect is very small (in particular relative to our nonemployment results) and there are essentially no discontinuities in other variables we do not think this is a big threat to the validity of our main estimates. As a robustness check we estimated all our main results excluding observations within one month of the cutoffs. 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.²⁸

4.3 Dynamic Effects, Reverse Experiment, Differences by Sub-Groups

To better understand the mechanisms behind our main labor supply effects, we have performed several additional exercises only possible due to our detailed and large data source. Due to space constraints, we limit ourselves to briefly summarizing our findings here. Our first additional finding confirms that our main effects are truly driven by age-related discontinuities in UI eligibility. After the reform of the UI system in the late 1990s intended to reduce UI duration, the eligibility thresholds for extended UI were shifted to ages 45 and 47 starting in 1999. This led to substantial reductions in potential benefit durations (Figure 1). Figure 4 shows the regression discontinuity effect on non-employment durations comparable to Figure 2, but for the new, post-1999 regime. The figure shows that the discontinuities in non-employment durations move to the new age thresholds, confirming the assumptions implicit in our main analysis. Estimates of labor supply elasticities of UI duration (shown in the Web Appendix) are now somewhat smaller than our main findings,

²⁷In smaller data sets this effect would almost certainly not be detectable.

²⁸It is interesting to note that the density discontinuities are somewhat larger for the 1999 to 2004 sample (see the Web Appendix). This is consistent with the fact that unemployment agency caseworkers were advised in recent years to make UI claimants aware of the possibility to delay UI claiming to be eligible for longer potential durations. However the discontinuity is still very small relative to the overall number of spells and does not seem large enough to be a threat to RD estimation for this sample.

though they are of the same order of magnitude and still similar across age-groups. This reduction may be partly due to stricter monitoring of job search behavior and penalties for not accepting suitable jobs in the new regime.²⁹

Our second additional finding is that the effect of large UI extensions at the age thresholds has an effect throughout the duration distribution; i.e., not only recipients who exhaust their UI benefits are affected, but also those recipients whose UI spell would have ended well beforehand in absence of the extension stay non-employed longer. This is shown in Figure 5, which displays non-parametric estimates of the hazard of exiting non-employment by duration based on regression discontinuity estimates.³⁰ The hazard function for non-employment duration of individuals eligible to 18 months of UI relative to individuals eligible to 12 months is already clearly shifted upwards around 3-4 months after the beginning of UI. Thus unemployed individuals adjust their search behavior a long time before running out of UI when they are eligible for longer durations (e.g., Card, Chetty, and Weber 2007a). In addition, 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. About 28 percent exhaust their UI benefits in the 12 month eligibility group, while only about 20 percent in the 18 month eligibility group. However, consistent with findings discussed in Card, Chetty, and Weber (2007b), the spikes are substantially smaller for the hazard of exiting non-employment than for the hazard of exiting UI, confirming that a sizable fraction of UI recipients transit to non-employment after benefit exhaustion.³¹

Finally, to further understand the mechanisms operating behind our main results, Table 4 shows our regression discontinuity estimates for several relevant sub-groups. While the table displays

²⁹From Figure 7 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.

³⁰A more detailed discussion can be found in the Web Appendix.

³¹These findings suggest our main effects reported in Table 1 are averages of behavioral responses along the entire duration distribution. The corresponding regression discontinuity estimates along different points of the duration distribution are shown in the Web Appendix. 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.

some expected differences in the labor supply response to UI extensions, the overall picture that emerges from this analysis is that the elasticities we find are remarkably robust throughout the population we study. Certainly, it does not appear that our findings are driven by any particular sub-group in our sample. The labor supply elasticity is slightly larger for high educated and high tenured workers, and larger for women. Together with the similarities across age-groups, the point estimates in Table 4 are supportive of a modest common labor supply elasticity in the range from 0.12 to 0.16.

Of particular importance for interpreting our estimates is the role of extended unemployment assistance (UA) in explaining our findings. We have thus 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 large disparities here. The last row of Table 4 shows this is clearly not the case. For each UI recipient in our sample we predicted the propensity to receive UA based on education, demographic characteristics, and their earnings histories.³² The elasticity for individuals whose propensity is above and below 0.5 is 0.13 and 0.16, respectively. If we include an interaction with the individual propensity and extrapolate linearly, for individuals with propensity of receiving UA close to 1 the elasticity is 0.08. Yet, even for those whose propensity is zero it is 0.21, well within the overall magnitude of our main findings. Thus, we conclude that while possibly an important factor, the presence of UA is unlikely to be the main source behind our modest labor supply elasticities. This finding is consistent with the fact that an important part of our main results is driven by individuals that do not exhaust benefits (and are thus not eligible for UA).

Finally, to examine whether our main findings are affected by our focus on stable workers, in the Web Appendix we also replicated our main RD estimates without any restriction on labor force attachment before the UI receipt. While as explained above we cannot calculate an elasticity for

³²The corresponding linear probability model is shown in the Web Appendix, and suggests our specification has a good fit. 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.

this group, the RD estimates are smaller for the duration of both UI receipt and non-employment duration. Since the underlying averages changes in potential UI durations at the thresholds are also smaller, this is consistent with the underlying true marginal effects being similar. This is confirmed when we normalize 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 are likely to be robust to a weakening of our restriction on labor force participation.

4.4 Interpretation in Context of Literature

It is helpful to compare our labor supply effects with previous literature either using different research designs or smaller extensions in UI duration. Our findings imply similar labor supply effects than previous research on the German UI system. In particular, Hunt (1995) evaluates the Germany UI system over the period 1983 to 1988 using a difference-in-difference approach, comparing the change in non-employment durations for different age groups before and after the reforms in the 1980s. Hunt (1995) finds that the 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. Since the marginal effects in Table 1 imply an increase of non-employment durations of about 0.1 to 0.13 months per additional month of UI, this implies the estimates are quite comparable despite differences in underlying samples. However, while we look at exits from non-employment to employment, Hunt analyzes exits from unemployment to employment and leaving the labor force. Furthermore, since this approach averages over different potential UI durations, a direct comparison with our estimates via marginal effects or elasticities is difficult.³³

³³ Another paper analyzing the age-thresholds of the German UI system, Fitzenberger and Wilke's (2009), also focuses on age groups older than 50, which we excluded from our analysis. Fitzenberger and Wilke use a difference-in-difference estimator. Their main finding is a strong increase in spells that never return to employment. In Figure 6 we show that there is a strong age gradient in the probability of ever returning to work but only very small jumps at the UI discontinuities. As further described below, 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.

Lalive (2008) evaluates the effects of UI in Austria in a regression discontinuity 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 corresponds to an elasticity of 0.48 and is thus substantially larger than our elasticity of 0.13.³⁴ As a rescaled marginal effect however the effect is smaller than our finding: we find that at the age 42 cutoff one additional month of UI increases durations by 0.13 months, while Lalive's results imply an increase in 0.9 months.³⁵ In a different context, Card, Chetty, and Weber (2007a) also analyze age-related increases in benefit durations in the Austrian UI system using a similar design as ours but with smaller increases in potential UI durations. Their estimates point to similarly modest labor supply elasticities of UI durations as our findings do.

This discussion implies that our findings confirm previous assessments that that effects of parameters of the UI system appear to be too small to imply that differences and changes in institutional environments are a major determinant of the large observed differences in the evolution of unemployment durations and unemployment within and between countries (Hunt 1995 and Katz and Meyer 1990). Clearly, this comparison will depend on a multitude of other factors whose effect is difficult to quantify (e.g., Burtless 1987). We return to this question when we discuss implications of our findings in more detail below.

It is also instructive but more difficult to compare our estimates to related studies based on data from the United States. Care should be taken in any comparison due to differences in data set, research design, and size of the extensions we study. Our main estimates 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.,

³⁴The formula we use (see notes of Table 2) may be considered inappropriate for such very large changes in UI durations. Instead one could assume that the relationship between unemployment durations and UI durations is given by a constant elasticity function. Such a function has the form $y = ax^b$, where b is the elasticity. For one treatment effect estimate the implied elasticity is then: $b = \frac{\log(UnempDur_1) - \log(UnempDur_2)}{\log(PotDur_1) - \log(PotDur_2)}$. Calculating elasticities this way does not affect our elasticities very much (about 0.125 rather than 0.127) and reduces Lalive's elasticity from 0.48 to 0.39. Thus while this does matter for large changes in potential durations this elasticity is still much larger than ours.

³⁵Lalive (2008) shows that the extended UI Program in Austria had important interactions with early retirement decisions, which may explain part of the differences in the effects which we find.

Meyer 1990, Katz and Meyer 1990). There are several potential reasons for this difference. It could be that the effect is larger for extensions occurring at shorter baseline durations, since more individuals are likely to be constrained by the limit in UI durations. Since we find that the effect of the extensions we study is substantial throughout the duration distribution, while likely to be part of an explanation, the differences are unlikely to be purely due to such a “mechanical” effect.

Another source of difference could be that United States estimates are typically based on more representative populations, while we focus on mature workers with high labor force attachment. This is an important question that we cannot resolve in the current paper. The small variation in treatment effects among sub-groups such as age, education, or gender and the similarity in reduced form effects once we include workers with weaker labor force attachment suggests that difference in characteristics of UI recipients may be one among many rather than a decisive factor. To directly assess the role of differences in observable characteristics, we re-estimated our main RD specifications after re-weighting our sample.³⁶ We find similar marginal effects and elasticities even when the German sample has the same distribution of observable characteristics as a comparable sample of UI recipients from the United States. Conversely, it is possible that some of the large findings of studies based on data from United States in the 1980s are driven by the presence of workers on temporary layoffs, who have been shown to be particularly responsive to incentives inherent in the UI system (e.g., Katz and Meyer 1990). We have examined the role of temporary layoffs in our sample, and find it does not affect our results. The decline of temporary layoffs in the United States in the 1990s should have decreased a potential source of discrepancy between our study and previous studies from the United States.

It could also be that as suggested by Card and Levine (2000) the state-specific triggers in extended UI in the United States sometimes used in empirical analyses are endogenous to local economic circumstances, potentially leading to larger elasticities. The regression discontinuity

³⁶As described in the Web Appendix, using information from the Current Population Survey we first estimated 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).

estimates used here are immune to such influences.³⁷ More generally, the regression discontinuity approach requires fewer assumptions for identification than approaches using more standard hazard models, parametric controls for heterogeneity, and region- or group-differences in UI duration to obtain counterfactuals.

Finally, labor supply effects of UI durations may be smaller in Germany due to the presence of unlimited unemployment assistance (UA) after UI benefits are exhausted. Although intuitive, this hypothesis is difficult to assess because of lack of information on effective income replacement rates post-UI in both the United States and Germany. While presence of UA is likely to play a role in explaining our findings, as argued above it certainly cannot explain all. On the one hand, even workers very unlikely to access UA after running out of UI exhibit modest labor supply responses. On the other hand, our findings are also based on substantial responses of individuals further from benefit exhaustion for whom UA should be less of a concern. Yet, absent more comparable United States estimates it will be difficult to make more than approximate comparisons.

5 Variation of Labor Supply Effects with the Business Cycle

Often, extensions in the duration of UI benefits occur among high or rising unemployment rates. If the labor supply effect of UI duration varies with economic circumstances, the mean estimates presented so far may not be sufficient to assess the effect of extended UI as a policy to help the unemployed in recessions. The existing literature has suggested several reasons why the effect of UI should vary with the business cycle. On the one hand, many observers believe that the effect of UI on labor supply falls in recessions (e.g., Krueger and Meyer 2002), possibly because low job offer arrival rates weaken the incentive effects of parameters of the UI system.³⁸ On the other hand, recessions can lead to large earnings losses among job losers (e.g., von Wachter, Song and Manchester 2009), partly due to a loss in specific skills as labor is reallocated between sectors.

³⁷Tannery and Jurajda (2003) try to circumvent this problem by analyzing the effect of extended and emergency UI in the early 1980s recession in different regions within the same state, Pennsylvania. While they find the same large spike at exhaustion of UI benefits in more or less depressed parts of the state, the exact magnitude of the spike appears unknown because of a coding error (see the working paper version of Card, Chetty, and Weber 2007b).

³⁸Although seldom formalized, in a standard model of job search the disincentive effect of UI benefits can decline in recessions if search costs increase at a faster rate.

Since UI benefits are typically based on past earnings, this can lead to increases in the effective replacement rate, and may imply larger effects of UI on the duration of unemployment in recessions (e.g., Sargent and Ljungqvist 1998).

It is generally difficult to test for such differences, since the duration of UI often varies with the state of the labor market. A particular advantage of our setting is that it provides quasi-experimental increases in UI duration for each year in our sample, allowing us to study variation in treatment effects 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 examined whether the resulting labor supply effects varied systematically with the business cycle. Thereby, it is particularly helpful that 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.

The findings from this exercises suggest the labor supply elasticities of UI durations we estimate are quite robust over the business cycle. This is also true when we consider the entire hazard across boom and bust periods, which is essentially unchanged. At best, some of our models suggest a weak decline in the effect of extended UI on non-employment in recessions. Since average UI durations are weakly counter-cyclical, this is also what follows when we consider the cyclicity of marginal effects instead of elasticities. All of these findings are very robust to how we measure business cycles and how we correlate cycles with our regression discontinuity estimates.

The first panel of Figure 6 plots elasticities 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 elasticity. While there appears to be some variation over time, from the figure there appears to be no clear systematic variation with the business cycle of the elasticities for the age 42 threshold.³⁹ In the second panel of Figure 6 we investigate this further by plotting the elasticities 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

³⁹The range of elasticities is between 0.03 and 0.22 over the business cycle. The range is similar for the other age thresholds; elasticities are substantially more variable, somewhat smaller, and declining over time for 49 year olds.

elasticity appears quite stable over the business cycle.

The findings from Figure 6 (b) are summarized and extended in Table 5. 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.⁴⁰ The findings are also similar if we use marginal effects instead of elasticities. Second, in the last row 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). 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 elasticity with the predicted wage change.

One potential concern with the estimates in Table 5 is that they mask differential effects over the cycle in different parts of the hazard distribution. We compared shifts in the entire hazard function across boom and bust periods in the Web Appendix, 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 decomposition could offset potential cyclical variation in labor supply effects of UI. We examined this possibility, and found it not to affect our result. Table 5 shows cyclical variation in two summary indices of observable characteristics in our data, the predicted propensity to receive UA and the predicted post-UI wage. Overall, relative to the mean we see 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 (4) and (5) of Table 5 and confirms that our findings are very robust to changes in characteristics of UI recipients over time or the business

⁴⁰Since the elasticities or marginal effects are weighted by the inverse of their standard error in the regression, the resulting mean-squared error is the test-statistic for a standard Chi-Squared goodness of fit test. If we assume estimates for the elasticities are asymptotically normally distributed, the mean-squared error is asymptotically Chi-Squared distributed with degrees of freedom equal to the number of elasticities in the regression minus the number of regressors. For all but one specification in the table we cannot reject the null-hypothesis that the elasticity (or marginal effect) is constant over time.

cycle. As for our main estimates, we also replicated the estimates in Table 5 re-weighting our sample to reflect the distribution of observable characteristics in the United States for a given year. We do not find any differences in cyclicalities for this re-weighted sample either (see the Web Appendix).

These findings are robust to many alternative specifications we tried. The results of our extensive sensitivity analysis are contained in a Web Appendix and only briefly summarized here. For example, when we split our sample by worsening and improving labor market conditions, the elasticity seems to be somewhat lower in worsening times. We also tried several ways to further raise precision of our estimates by imposing increasingly strict functional form assumptions. For example, when we estimated a cox-proportional hazard model in the spirit of Meyer (1990), we find a slight decline in the predicted labor supply elasticities when unemployment is increasing. We also estimated a linear and log-linear model that pools the effect of UI extensions across our different age-thresholds while flexibly controlling for age. Again, the changes over time we find are relatively small, with at best weakly negative coefficients on the interaction between UI duration and business cycle indicators.

Overall, we feel comfortable to conclude that our main estimates for the effect of UI durations on labor supply do not vary strongly with the business cycle. At best, in some specifications we find a small decrease in the labor supply effect of UI durations in recessions, albeit it is not estimated very precisely. 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. Few other studies have analyzed this relationship in depth. Our findings are similar to results reported in Moffitt (1985) and Tannery and Jurajda (2003). Using administrative data from 13 states covering information on unemployment spells begun between 1978-1980, Moffitt (1985) finds that the disincentive effect of UI declines with the level of unemployment rate. Tannery and Jurajda (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.⁴¹

6 UI Extensions, Job Matching, and Long-Term Employment Outcomes

Extensions in UI may affect other outcomes than employment, but often additional information on accepted jobs or longer term outcomes is not available for large samples in conventional data sources. For example, intuition from models of job search is frequently invoked to argue that longer UI durations may allow workers to improve the quality of their new job. Similarly, UI may allow workers to avoid switching industries or occupations or moving to take a job.⁴² At the same time, it is a common concern that extension of UI may weaken the attachment of unemployed workers to the labor market and create a dependency on UI benefits. Such patterns could arise in the presence of stigmatization or human capital depreciation.

To obtain a more complete picture of the effect of extended UI, our data allows us to study a range of short-term and long-term career outcomes. We thereby add to a small but growing literature using administrative data to study the effects of UI extensions on job outcomes in different contexts.⁴³ Table 6 tests whether longer potential benefit durations increase post-unemployment wages. Despite our finding of significant effects on short-term employment, we do not find an effect of potential UI durations on the post-unemployment wage or on the wage change relative to the previous wage. The graphical analysis (Figure 7) supports this conclusion. If anything there is a slightly negative effect of longer durations on post wages for 49 year olds, but the magnitude is quite small. We also do not find an effect on the other measures of quality job matches we have tried, including the probability of moving to another region to take up a new job. While there is a small significant increase in the probability of switching occupation, relative to the mean the effect is less than one percent. This evidence confirms findings from recent studies of UI durations in dif-

⁴¹The unemployment rate in Philadelphia was about 10% throughout most of the period studied, whereas the unemployment rate in Pittsburgh was on the order of 16% during the same time period.

⁴²In the canonical search model with stochastic wages, these predictions hold if job search off-the-job is more effective than search on-the-job.

⁴³Card, Chetty and Weber (2007a) use smaller UI extensions on a range of job outcomes, and is the most comparable study to ours. Lalive (2007) finds no effect on wages of UI extensions for older workers. Van Ours and Vodopivec (2008) find no adverse effect of reductions in UI durations on wages and job durations. Caliendo, Tatsiramos and Uhlenndorf (2009) find positive effects on wages of UI extensions for workers about to exhaust their benefits.

ferent contexts and suggests that even large extensions in UI benefits neither improve nor worsen the quality of jobs that recipients find after unemployment.⁴⁴

Finally, we have also assessed whether extended UI leads to longer-term adverse consequences on employment and wages. Table 2 and Figure 7 show the effect of potential UI durations on employment and receipt of UI or UA five years after the onset of the non-employment spell. While workers respond by reducing their labor supply in the short run, we do not find strong evidence of a long-term reduction in employment. The effects are numerically quite small, and only significant for one out of three age thresholds. Even for that threshold, the size of the effect is less than two percent relative to the mean (shown in the Data Appendix). Similarly, while we find increases in the propensity to receive UI or UA benefits, at about half a percentage point these are again numerically small. Thus, while longer non-employment duration correlates strongly with the probability of exiting the labor force, increases in non-employment induced by extended UI lead to small reductions in workers' long-run attachment to the labor force.

We also analyzed the effect of UI extensions on long-term wage outcomes, without finding any appreciable effect. We do not find an effect on wages five years after the start of the UI spell or on wage growth over that period. This suggests workers do not take initially lower paying jobs because of high growth potential. It also confirms that there does not appear to be a significant adverse effect of UI extensions on workers' longer run earnings potential.⁴⁵ The absence of appreciable wage effects or long-term employment outcomes will facilitate the assessment of the consequences of UI for the labor market and its potential effect on welfare.

⁴⁴The effects of UI on wages and on long-term outcomes is robust over the business-cycle. We replicated models corresponding to Table 6 using these other outcomes as dependent variables, finding at best minor changes around very small average effects.

⁴⁵We have also estimated wage effects for other fixed time intervals starting from the beginning of unemployment, confirming the absence of an appreciable wage effect. These findings confirm that our main wage effects based on the wage at the first job after unemployment are unlikely to be affected by sample selection.

7 Implications for Aggregate Unemployment Rate and Welfare

7.1 Implications for Aggregate Unemployment Rate

Our results show that substantial increases in benefit durations lead to moderate increases in non-employment durations in the short run, but there are little long-term effects on labor force attachment or observable job quality. The point estimates indicate that the effects on employment are approximately constant over the business cycle or slightly smaller during economic downturns. In this section we develop further implications of our results by approximating the effect of extending UI benefits on the aggregate unemployment rate. We show that when ignoring general equilibrium effects, only fairly large increases in potential benefit durations are predicted to have sizable effects on average non-employment durations. When we also account for congestion effects in the labor market, as implied by standard search models, the effects turn out to be significantly smaller. The decline of vacancies relative to job seekers during recessions or incomplete take-up of UI benefits can further reduce the effect of UI extensions on the aggregate unemployment rate.

In order to use our empirical estimates to make out-of-sample predictions for the effect of UI duration on actual duration of unemployment and the unemployment rate, some functional form regarding the relationship between actual durations and potential durations has to be assumed. Since we find that the elasticity is very similar across different changes in UI, for different subgroups, and for different time periods, it is natural to assume that the relationship is given by a constant-elasticity function: $ActDur = a * PotDur^\eta$, where η is the elasticity and a is a constant. Given an estimate for the elasticity η , such as our main estimate of 0.13, one can approximate a using current actual and potential durations.⁴⁶

For illustrative purposes, here we consider the effect of recent extensions in UI duration from 26 to 104 weeks in the United States implied by our estimates. Using data from the Bureau of Labor Statistics we calibrate a for May 2008, before the recession started, to be about 22.3.⁴⁷ We

⁴⁶Note that a could vary with the business cycle to capture changes in the economic environment.

⁴⁷In May 2008 unemployment was 5.2 percent and the average duration unemployed people had been in unemployment was 17 weeks. These number were in fact very stable for the entire first half of 2008 and then started to increase slightly over the summer and then rapidly in the post-Lehmann crisis. Unfortunately the Bureau of Labor Statistics

can then use the formula $\hat{ActDur} = 22.3 * PotDur^{0.13}$ to predict what actual durations would have been for various assumed potential durations. For an increase in potential benefit durations from 26 to 104 weeks, actual durations are expected to increase from 34 to 40.8 weeks. Since the actual increase in unemployment duration was to about 60 weeks, this would imply that ignoring issues related to matching discussed below, according to our estimates extended UI is predicted to be responsible for 26% of the increase in average duration of unemployment.

To say something about the unemployment rate consider a simple steady state framework (see the Web Appendix for details). In steady state, the effect of potential UI durations on the aggregate unemployment rate is given by

$$\hat{UR} = \frac{\delta}{\frac{1}{a * PotDur^\eta} + \delta}$$

where δ is the job destruction rate.⁴⁸ As further discussed below, using a calibrated value for the job destruction rate and our constant-elasticity formula and ignoring any issue related to matching, an extension of potential benefit duration from 26 to 104 weeks would imply a rise in unemployment rates from 5.2% to 6.1%.

This approach does not take into account several channels that may lead to general equilibrium effects that modify the implications from the partial-equilibrium elasticities estimated in a regression discontinuity framework. A major reason why the general-equilibrium effect may be different is the presence of search externalities. Standard search models assume that the speed at which jobs form is a function, the “matching function”, of the number of workers searching for jobs weighted by their search intensity and the number of vacancies in the economy. The more vacancies and the more job searchers, the more matches are formed per time unit.

Consider the standard case of a Cobb Douglas matching function: $m(v, u) = m_0 v^{1-\alpha} u^\alpha$. If a single worker increases his search intensity by, for example, 10 percent (one can think of this of

does not report the mean duration of completed spells (which is really what corresponds to the actual duration in the equation above). 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. Given this the unemployment duration before the crisis was approximately 34 weeks. Potential duration of UI benefits was 26 weeks.

⁴⁸The 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.

sampling vacancies 10 percent faster), then this should decrease his expected non-employment duration by about 10 percent. If everyone does this however, the number of matches does not go up by 10 percent but by α times 10 percent (see the Web Appendix). Since a typical estimate of α is 0.5 (see Mortensen and Pissarides 1999), this implies the relevant general equilibrium elasticity is only about 50 percent of the estimated partial equilibrium elasticity.

Table 7 shows the simulated effect of increases in potential UI durations under different assumptions. The top panel shows how unemployment rates are predicted to increase if in the economic environment in May 2008 benefit durations had been permanently increased to 104 weeks. The bottom panel instead shows the effect on the unemployment rate of a decrease in benefit durations during the peak of the recession in February 2010.

If we do not account for search externalities, our main estimate of the non-employment elasticity from Table 1, $\eta = 0.13$, implies fairly substantial effects between 0.9 to 1.6 percent on the aggregate steady state unemployment rate. For example, during the economic environment of February 2010 lowering benefit durations from 104 back to 26 weeks would have lowered the unemployment rate from 10.4 percent to 8.8 percent. Yet, taking search externalities into account reduces the effect by one half, as shown in the second row in the two panels. 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), it reduces the effect further to 0.2 percent (in the good economic environment) or to 0.4 percent (in the bad economic environment).⁴⁹

The second column shows simulated unemployment rates if we take into account that our point estimates in Table 5 imply that in large recessions the non-employment elasticity can decline. Real GDP dropped by about 4 percent during the 2008 recession, so the model in Column 3 of Table 5, implies an elasticity of $\eta = 0.11 - 4 * 0.014 = 0.054$. As shown in Table 7, the effects on the predicted unemployment rates are now significantly lower even for the base case. Taking search externalities and the fraction of unemployed on UI into account, the benefit extension increases the unemployment rate by about 0.1 to 0.2 percentage points depending on the economic environment.

⁴⁹These are effectively still partial equilibrium approximations, since we ignore feedback effects of changes in search intensity on unemployment and vacancies.

Even if we took the upper bound of the 95 percent confidence interval of our main estimate at a 4 percent plunge in GDP this implies an elasticity of 0.113 and thus limited effects of UI extensions on the unemployment rate. Finally, the last column shows the implied effects for our largest elasticity, estimated for workers who are predicted to not take up unemployment assistance after exhausting UI benefits, and who thus should be most responsive to UI extensions. Even for this larger elasticity we only get sizable predicted effects on the unemployment rate if we ignore search externalities or imperfect coverage of UI.

These simulations show that straightforward extensions of the basic approximation lead to substantially smaller predicted effects of extensions in the duration of UI benefits on the unemployment rate. Similar mechanisms operate when we consider the effect of extensions in UI durations on the average duration of non-employment spells. For example, in the presence of search externalities, one can show that the effect of UI extensions is also reduced by α , the coefficient determining the slope of the aggregate matching function. This can lead to a significant reduction in the predicted efficiency costs of extended UI in terms of longer average non-employment durations.

The simulations in Table 7 do not take several channels into account that are likely to further reduce the aggregate impact of UI extensions. As mentioned at the outset, if the number of vacancies per unemployed job seekers (the vacancy ratio) is small individual search effort – and with it UI extensions – has a smaller effect on the job finding rate. This channel is again made explicit by the matching function since the marginal effect of search effort on the job finding rate declines with the vacancy ratio. For low levels of the vacancy ratio, the reduction in the marginal effect of individuals' search effort predicted by the matching function is substantial.⁵⁰ Another aspect is that the basic steady state approximation does not account for is the timing of UI extensions and labor market developments. When UI extensions are enacted step by step, different workers are eligible for different durations, and workers may have difficulties predicting how long they will actually

⁵⁰In 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 an additional decline in the effect of search effort on the job finding rate of about two thirds.

be eligible to receive UI benefits.⁵¹ Both of these factors are likely to reduce the effect of UI extensions on labor supply relative to what is implied by our steady state simulation. For example in the United States, at the peak of the 2008 recession, only a subgroup of workers could reasonably expect to be able to receive UI benefits for full 2 years, suggesting that even modified simulations such as in Table 7 would over-predict the effect of UI extensions on aggregate unemployment or non-employment durations.

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. Some estimates imply that this effect could be fairly substantial (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 spend on something with an equal GDP multiplier.

We also abstract from channels that may raise the aggregate effects of UI, but that may be less important in a large downturns when UI extensions often occur. For example, in a typical search model, lowering search intensity of the workforce lowers the expected profits from creating vacancies (since it takes longer to fill them). In principle this would lead to a larger effect of UI extensions on unemployment rates, but since it should in general be pretty easy to fill vacancies during times when the labor market is slack, we think at the margin this effect is very small. Finally there might be general equilibrium effects of a more generous UI system on job destruction, but again during large recessions this effect may be small.

Overall, our discussion and the findings in Table 7 suggest that in large downturns the effect of extensions in the duration of UI on the aggregate unemployment rate or mean non-employment duration is fairly small. However, the discussion also shows that this effect is likely influenced by several difficult-to-measure determinants. Thus, given our findings care has to be taken before attributing, say, differences in average non-employment duration between recessions, or differences

⁵¹For a detailed history of the gradual and complex nature of UI extensions in recent recessions in the United States see Lake (2002) and Needels and Nicholson (2004).

in aggregate unemployment rates between countries to differences in the potential duration of UI benefits.

7.2 Potential Implications for Welfare

The preceding section argued that the adverse effect of UI extensions on the duration and rate of unemployment in recessions is likely to be small to moderate. It is a natural question whether our findings can be used to assess whether UI extensions in recessions and the associated costs in terms of reduced employment are welfare improving. A growing literature has evaluated the welfare effects of UI benefits. Yet, the existing applied literature is mostly concerned with the level of benefits, not their duration (e.g., Gruber 1997, Chetty 2008). A growing theoretical literature calculates the entire optimal path of benefits (e.g., Shimer and Werning 2006, Pavoni 2007), but is less concerned with evaluating existing UI systems based on constant benefits over a fixed horizon, such as in Germany or in the United States. Neither literature considers changes in optimal UI benefits over the business cycle. Here, we use a version of the model introduced by Chetty (2008) to relate the effect of benefit duration on welfare to the effect of benefit durations on non-employment and the exhaustion rate. Since the non-employment effect tends to fall in recessions, but, as we will show below, the effect of UI durations on the exhaustion rate rises significantly, the model implies that UI extensions in recessions are likely to be welfare improving.

To succinctly summarize the factors that determine the welfare effect of the UI extensions we study here, we use a standard partial equilibrium model of job search based on Chetty (2008). Given that we find no effect of UI extensions on wages we abstract from considerations based on reservation wages, and follow Chetty (2008) in using search intensity as the main determinant of non-employment durations. At the beginning of the model individuals are unemployed, and decide how intensely to search for a job at a fixed wage.⁵² Search entails a cost that can vary with the business cycle. While unemployed individuals receive benefits b for a maximum duration of P . After

⁵²Chetty (2008) shows how the main findings are unaffected by allowing for stochastic wages. A wage distribution can arise if we allow for, say, continuous differences in individuals' human capital. For further details of the role of assets in a related search model see Lentz and Tranaes (2007).

exhausting regular benefits, we assume individuals receive a lower bound of utility. Given these parameters, the individual chooses a path of search intensity s_t to maximize the present discounted value of utility over T periods under unemployment, non-employment, and employment. In contrast to Chetty (2008) and Shimer and Werning (2006), we follow the majority of the theoretical literature on the path of UI benefits and abstract from savings in this model, such that individuals are perfectly liquidity constrained. We discuss the effect of relaxing this assumption below.

The planner's problem is to set the length of benefits P to maximize welfare, while at the same time balancing the budget by setting a tax on wages. Since we consider the cases of extensions in benefit durations in business cycles, we assume that the benefit level is fixed. When choosing the optimal duration of benefits, the social planner takes the individual's optimal responses into account. Let B and D be the number of periods the individual spends receiving UI benefits and in non-employment, respectively. We follow Chetty (2008) in normalizing search intensity to be the exit rate from non-employment, such that $B = \sum_{t=0}^P \prod_{j=0}^t (1 - s_t)$ and $D = \sum_{t=0}^T \prod_{j=0}^t (1 - s_t)$.

Using this notation, one can show that the effect of extending the maximum duration of UI benefits P on welfare can be approximately written as

$$\frac{\partial W}{\partial P} = \frac{\partial B}{\partial P} b(u'(0) - v'(\tilde{w})) - \frac{\partial D}{\partial P} (v(\tilde{w}) - u(0) + \tau w v'(\tilde{w})) - \frac{\partial C}{\partial P}$$

where $u(\cdot)$ and $v(\cdot)$ are the utility while unemployed and employed, respectively, \tilde{w} is the wage, τ is the tax rate, and b is the utility during non-employment after UI benefits have run out. The effect of maximum UI duration on welfare depends on three components. The first component is the benefit of extending UI benefits, and is proportional to the effect of potential benefit duration on the actual duration of benefits ($\frac{\partial B}{\partial P}$). The second component represents the cost of UI extensions due to a reduction in labor supply, and depends on the marginal effect of UI duration on non-employment duration ($\frac{\partial D}{\partial P}$). The third term represents the increase in life-time search costs related to a rise in benefit durations.⁵³

⁵³I.e., $C \equiv \sum_{t=0}^T S_t \psi(s_t)$, where $S_t \equiv \prod_{j=0}^t (1 - s_j)$ is the survivor function and $\psi(\cdot)$ is the function representing search costs.

The social planner chooses the optimal maximum UI duration by setting the marginal derivative to zero. Without assumptions on the utility functions, we cannot solve for the optimum benefit level. However, we can use this formula to assess the direction of changes in the optimal level of benefits over the business cycle. From our data we can measure the change in $\frac{\partial B}{\partial P}$ and $\frac{\partial D}{\partial P}$, the two main variable components of this formula. Using simulations, we then assess the direction of change of the third component representing search costs.

We have established above that the positive marginal effect of P on non-employment $\frac{\partial D}{\partial P}$ tends to decline in recessions. Figure 8 and Table 8 replicate the same analysis for the marginal effect of P on the actual duration of UI benefits $\frac{\partial B}{\partial P}$. The figure clearly shows how there is a substantial positive relationship between $\frac{\partial B}{\partial P}$ and the *lead* in unemployment rates. This is confirmed in Table 8, where we assess the correlation of $\frac{\partial B}{\partial P}$ with a range of alternative measures of the business cycle. The table shows that $\frac{\partial B}{\partial P}$ correlates more strongly with the change in unemployment rates or the unemployment rates in $t + 1$. The reason is that an important component of $\frac{\partial B}{\partial P}$ is the exhaustion rate. Since benefits last at least 12 months and up to 26 months in our sample, the unemployment rate at exhaustion matters.⁵⁴

These findings strongly suggest that from an optimal level $\frac{\partial W}{\partial P}$ would rise in a recession. To assess the direction of change of the third term related to search costs, we simulated our search model.⁵⁵ To select the parameters for the simulation, we first calibrated the model to match the marginal effects in our model. The simulation suggests that for the calibrated parameter values, as well as for a wide range of reasonable values, the cost term $\frac{\partial C}{\partial P}$ declines in a recession.

These findings suggests that it is optimal to extend UI benefits during recessions. The size of the extension depends on the parameters of the model and the change in the marginal effect of maximum UI durations P on non-employment duration and on, effectively, the exhaustion rate. This result formalizes two rules of thumb on the degree of optimal UI durations proposed in the

⁵⁴One can write $B = \sum_{t=0}^P S_t$ where $S_t \equiv \prod_{j=0}^t (1 - s_j)$ is the survivor function. Then $\frac{\partial B}{\partial P} = S_P + \sum_{t=0}^P \frac{\partial S_t}{\partial P}$, where S_P is the exhaustion rate. The second term represents the shift in the survivor function due to a change in P , and is small relative to the exhaustion rate.

⁵⁵The term depends on the unknown function of search costs, among others, and the derivative of $\partial C/\partial P$ with respect to time has two counteracting components.

literature. On the one hand, Corson and Nicholson (1982) suggested that UI should be extended in recessions to keep the exhaustion rate constant. The discussion suggests that this is true only when the cost in terms of higher non-employment durations is unchanged. Given our empirical section suggests the non-employment effect tends to decline in recessions, keeping the exhaustion rate constant would not be a bad approximation to the optimal extensions in UI durations, as long as the level before the recession was optimal. On the other hand, Moffitt (1985) suggested that the optimal extension in UI durations should hold the disincentive effect constant. Our discussion confirms that changes in the disincentive effect are a key component in evaluating the welfare effect of UI durations. Yet, they have to be considered in conjunction with fluctuations in the exhaustion rate. Our empirical results suggests that for the case we study, fluctuations in the effect of UI durations on the exhaustion rate dominate changes in the non-employment effect of UI durations.

Chetty (2008) has shown that the income effect of UI benefits in presence of liquidity constrained households is an important component of the overall welfare effect. Extending our model to include a role for assets is beyond the scope of this paper. Yet, we suspect that allowing for asset accumulation while imposing that individuals can face a liquidity constraint would strengthen our conclusions. This is because existing evidence suggests that the fraction of UI recipients that is credit constrained in recessions tends to increase. This would further raise the relative benefit of extensions of UI durations in recessions relative to more tranquil times. The ability to smooth consumption is likely to fall for many unemployed workers in particular in large recessions because spouses are likely to be unemployed as well;⁵⁶ because existing wealth stocks are spread over longer expected unemployment spells (e.g., Gruber 2001); because banks are more hesitant to lend to workers without a job or collateral (e.g., Keys 2010); and because asset prices are likely to decline, reducing the value of individual wealth.

This discussion and findings from the existing literature suggest that the welfare effect of UI extensions may increase during recessions despite the (possibly limited) associated costs in terms

⁵⁶Using tabulations from the Survey of Income and Program Participation, the Congressional Budget Office (2004) has shown that spousal earnings is a key source of income for unemployed individuals and that its presence is correlated with significantly lower poverty rates, in particular once UI benefits run out.

of rising unemployment durations and unemployment rates, at least locally around the pre-existing level. Of course, it is important to bear in mind that this statement is only possible because of our findings that $\frac{\partial D}{\partial P}$ changes little over the business cycle, that there is no effect on wages, and that there is no long-term effect on employment outcomes. Any of these effects would complicate the assessment of the welfare implications of extensions in the duration of UI benefits during recessions.⁵⁷

8 Conclusion

In this paper, we have evaluated the impact of large changes in the duration of unemployment insurance (UI) on labor supply, the quality of job matches, and long-term employment outcomes. We show that differences in eligibility for UI by exact age in the German UI system give rise to a valid regression discontinuity design with several age-thresholds that have changed over time. We exploit these multiple quasi-experiments using administrative data on the universe of new unemployment spells and career histories over twenty years from Germany. We use our estimates to assess three open questions about the economic effects of extensions in UI durations. First, the negative consequences on employment of large extensions in UI may fluctuate over the business cycle.. Second, it is still an open question whether in addition to subsidizing income UI allows workers to obtain better job matches. Third, not much is known about the longer term effects of UI extensions on employment and wages.

The elasticities of labor supply with respect to the duration of UI benefits we find are modest. They are in the lower range of estimates from the United States (Krueger and Meyer 2002), slightly lower than some previous estimates for Germany (Hunt 1995), but similar to recent results for Austria from a similar research design (Card, Chetty, and Weber 2007a). This suggests that even large increases in UI durations – such as typically occurring in the United States in larger recessions – do not have a different impact than the smaller increases typically analyzed in the

⁵⁷Although business cycles have not been analyzed explicitly, comparative static results from intertemporal principal-agent models without hidden savings suggest a decline in the hazard rate shifts the declining benefits path outward (e.g., Pavoni 2007). In models with hidden savings, a decline in the hazard rate within an unemployment spell can shift the profile of UI benefits out- or even upward (Shimer and Werning 2006).

existing literature.

We also find that labor supply elasticities are very robust over time and across groups of workers. They do not vary strongly with the state of the business cycle or the average industry-specific wage loss holding the business cycle constant. At best, some specifications suggest that elasticities appear to decline somewhat in large recessions. Overall, our results indicate that extensions in UI during large recessions are unlikely to lead to a sizable or lasting increase in unemployment durations, contrary to what would have been predicted by increasing earnings losses and rising effective replacement rates. Similarly, these findings confirm that differences in the generosity of UI across countries are unlikely to explain a majority of observed differences in the duration of unemployment spells.

While we find adverse effects on labor supply in the short run, we do not find effects on average outcomes of job search or longer-term outcomes. Our regression discontinuity estimates of the effect of UI extensions on wages, wage growth, long-term employment outcomes, or the probability of switching industry, occupation, or region are all close to zero or small. These results are consistent with an earlier literature and recent findings by Card, Chetty, and Weber (2007a) that wage effects of UI insurance appear to be small. They also imply that UI extensions are unlikely to lead to appreciable hysteresis effects from increased unemployment duration.

These findings have several important implications. In our discussion, 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 or a role of vacancy rates. 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. We also show that in a simple model of job search, the exhaustion rate and the disincentive effect of benefit durations are two key indicators of the cost and benefits of the UI system, respectively. Given our findings of weakly declining disincentive effects and strongly countercyclical exhaustion rates, the welfare effect of UI extensions in economic downturns is likely to be positive. Finally, our results on the effect of extended UI on job quality and long-term employment effects suggest

that these aspects do not play a fundamental role when evaluating the cost and benefits of UI extensions.

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, our research design does not allow us to assess differences in the effect of UI for younger workers or workers with weaker labor force attachment. Our research design does not allow us to directly assess the potential effect on our findings of indefinite unemployment assistance available in Germany after UI is exhausted. Again our results indicate a robustness to variation in the likelihood unemployment assistance, but assessing this effect directly is an important avenue for future research. Similarly, given our findings, 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 ALG duration on Months of ALG Receipt and Non-employment

	(1) Age bandwidth around age discontinuity 2 years	(2) 1 year	(3) 0.5 years	(4) 0.2 years
Panel A: Dependent Variable: Duration of UI Benefit receipt				
D(age>=42)	1.78 [0.036]**	1.82 [0.052]**	1.73 [0.072]**	1.65 [0.11]**
Elasticity	0.60	0.62	0.59	0.57
Observations	452749	225774	112436	45301
D(age>=44)	1.04 [0.047]**	1.16 [0.065]**	1.13 [0.092]**	1.24 [0.15]**
Elasticity	0.55	0.62	0.60	0.66
Observations	450280	225134	112597	45258
D(age>=49)	1.40 [0.074]**	1.44 [0.084]**	1.44 [0.12]**	1.72 [0.18]**
Elasticity	0.70	0.72	0.72	0.87
Observations	329680	217942	109238	43812
Panel B: Dependent Variable: Nonemployment Duration				
D(age>=42)	0.78 [0.086]**	0.92 [0.12]**	1.04 [0.17]**	0.79 [0.27]**
Elasticity	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]**
Elasticity	0.12	0.19	0.19	0.24
Observations	450280	225134	112597	45258
D(age>=49)	0.43 [0.11]**	0.52 [0.13]**	0.56 [0.19]**	0.79 [0.29]**
Elasticity	0.13	0.16	0.17	0.25
Observations	329680	217942	109238	43812

Notes: The coefficients estimate the magnitude of the jump in non-employment 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 on day level (* P<.05, ** P<.01).

At the age 42 discontinuity UI benefit durations (ALG) 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 elasticity is calculated as:

$$\frac{RD \text{ Coefficient}}{\text{Change Pot. Durations}} \times \frac{\text{Average Pot. Dur around cutoff}}{\text{Average Act. Dur around cutoff}}$$

The sample consists of individuals starting unemployment spells between July 1987 and March 1999, who had worked for 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.

Table 2: Regression Discontinuity Estimates of Effect Of Potential ALG Duration on Employment Outcomes

	(1) Time until Claim	(2) Ever emp. again	(3) Emp. 5 years later	(4) UI 5 years later
D(age \geq 42)	-0.00089 [0.020]	-0.010 [0.0022]**	-0.0041 [0.0029]	0.0049 [0.0021]*
Observations	452749	452749	452749	452749
D(age \geq 44)	0.016 [0.021]	-0.0056 [0.0024]*	-0.0076 [0.0030]*	0.0051 [0.0023]*
Observations	450280	450280	450280	450280
D(age \geq 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 jump 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 on day level (* P<.05, ** P<.01).

The sample consists of individuals starting unemployment spells between July 1987 and March 1999, who had worked for 52 months in the last 7 years without intermittent UI spell.

Table 3: Regression Discontinuity Estimates of Smoothness of Predetermined Variables around Age Discontinuities

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

Notes: The coefficients estimate the magnitude of the jump 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 on day level (* P<.05, ** P<.01)). The sample consists of individuals starting unemployment spells between July 1987 and March 1999, who had worked for 52 months in the last 7 years without intermittent UI spell.

Table 4: Regression Discontinuity Estimates of Effect of Potential UI durations on Non-emp. Durations by Subgroups

	Coefficient	Standard Error	Elasticity
Education - with or without Abitur (College Entrance Exam)			
Less than Abitur	0.75	(0.10)	0.12
Abitur or more	0.79	(0.22)	0.13
Tenure			
≤ 5 years	0.73	(0.11)	0.12
> 5 years	0.88	(0.22)	0.15
Gender			
Men	0.64	(0.11)	0.11
Women	0.94	(0.16)	0.14
Probability of receiving Unemployment Assistance after UI Benefits			
<i>Prob</i> > 0.5	0.62	(0.11)	0.11
<i>Prob</i> ≤ 0.5	1.07	(0.16)	0.18

Notes: The coefficients estimate the magnitude of the jump 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 on day level (* $P < .05$, ** $P < .01$).

The sample consists of individuals starting unemployment spells between July 1987 and March 1999, who had worked for 52 months in the last 7 years without intermittent UI spell.

Table 5: The Correlation of Labor Supply Elasticities from Regression Discontinuity Estimates with the Economic Environment

	(1) Mean & SE	(2) Non-Emp Duration Elasticity	(3) Non-Emp Duration Marg. Effect	(4) Non-Emp Duration Elasticity Reweighted to Sample Charact. in Year 2002	(5) Non-Emp Duration Marg. Effect Reweighted to Sample Charact. in Year 2002	(6) Predicted Pre-UI Log Wage	(7) Predicted Unemp. Assist. (ALH) Eligibility
Unemployment Rate in Percent	9.06 [1.64]	-0.013 [0.0071]	-0.066 [0.038]	-0.017 [0.010]	-0.069 [0.039]	-0.018 [0.0027]**	0.010 [0.0015]**
Change in Unemployment Rate	0.14 [0.78]	-0.018 [0.014]	-0.0033 [0.079]	-0.0057 [0.021]	-0.017 [0.080]	0.013 [0.0075]	0.000048 [0.0044]
Real GDP Growth	3.03 [1.66]	0.014 [0.0075]	0.033 [0.043]	0.010 [0.011]	0.035 [0.043]	-0.0028 [0.0036]	-0.00084 [0.0020]
Mass Layoff Rate	1.30 [0.52]	-0.039 [0.022]	-0.13 [0.13]	-0.043 [0.033]	-0.13 [0.13]	-0.000022 [0.012]	0.010 [0.0069]
Average Log Wage Loss in Year-Quintile Cell	-0.14 [0.14]	0.090 [0.17]	-0.44 [0.66]	-0.082 [0.20]	-0.36 [0.67]		
Mean of Dep Var		0.12	0.49	0.11	0.47	3.98	0.56
Observations in Row 1-4		51	51	51	51	51	51
Observations in Row 5		238	240	238	240		

Notes: Columns (2)-(5) report coefficients from a 2 step regression. In the first step the effect of Extended UI durations on non-employment durations are estimated separately for all years and age thresholds using the regression discontinuity estimator. In the second step the resulting elasticities/marginal effects are regressed on measures of the economic environment. Each reported coefficient represents the coefficient on those measures, given in the row names. The second step regressions are weighted by the inverse of the standard errors of the estimates and include a dummy the Post-1999 reform period.

Local linear regressions (different slopes) on each side of cutoff.

Stars indicate confidence levels (* P<.05, ** P<.01).

Table 6: Regression Discontinuity Estimates of Effect of Potential UI durations on Post Unemployment Match Quality

	(1) Log Post Wage	(2) Log Wage Loss	(3) Log Wage 5 y. later	(4) Log Wage Growth 5 y.	(5) Switch Ind.	(6) Switch Occ.	(7) Switch County
D(age>=42)	-0.0022 [0.0032]	-0.0036 [0.0032]	-0.0014 [0.0040]	-0.0026 [0.0028]	0.0026 [0.0032]	0.0067 [0.0034]*	-0.00028 [0.0032]
Observations	373613	354986	231261	231026	363254	374487	374305
D(age>=44)	0.00060 [0.0033]	-0.0015 [0.0032]	0.0033 [0.0040]	-0.0057 [0.0031]	0.0034 [0.0030]	0.0056 [0.0033]	0.0014 [0.0034]
Observations	358823	341290	218158	217955	348958	359778	359595
D(age>=49)	-0.0083 [0.0045]	-0.0070 [0.0045]	-0.011 [0.0057]*	-0.0000021 [0.0041]	0.010 [0.0042]*	0.011 [0.0044]*	0.0078 [0.0046]
Observations	229886	217822	133179	133063	224683	230838	230722

Notes: The coefficients estimate the magnitude of the jump 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 on day level (* P<.05, ** P<.01)). The sample consists of individuals starting unemployment spells between July 1987 and March 1999, who had worked for 52 months in the last 7 years without intermittent UI spell.

Table 7: Simulating the Effect of Unemployment Insurance Extensions on the Unemployment Rate in the United States

	Baseline Estimate	Interacted Model Point Estimate Change GDP -4%	Interacted Model Point Estimate Change GDP -4% Upper Bound of CI	Extrapolated Elasticity for Probability of Unemp Ass. = 0
Elasticity of Non-emp Duration	0.13	0.054	0.113	0.21
Simulation 1: Extending UI Durations from 26 to 104 weeks in March 2008 Baseline UR = 5.2%				
Partial Equilibrium Extrapolation	6.1%	5.5%	6.0%	6.8%
Assuming Matching Function $\alpha = 0.5$	5.7%	5.4%	5.6%	6.0%
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 UR = 10.4%				
Partial Equilibrium Extrapolation	8.8%	9.7%	9.0%	8.0%
Assuming Matching Function $\alpha = 0.5$	9.6%	10.1%	9.7%	9.2%
Assuming Matching Function and 50% of Unemployed receive UI	10.0%	10.2%	10.1%	9.8%

Notes: For details on the simulation assumptions see text.

Table 8: The Correlation of the Marginal Effect of Potential UI Duration on Actual UI Duration with the Business Cycle

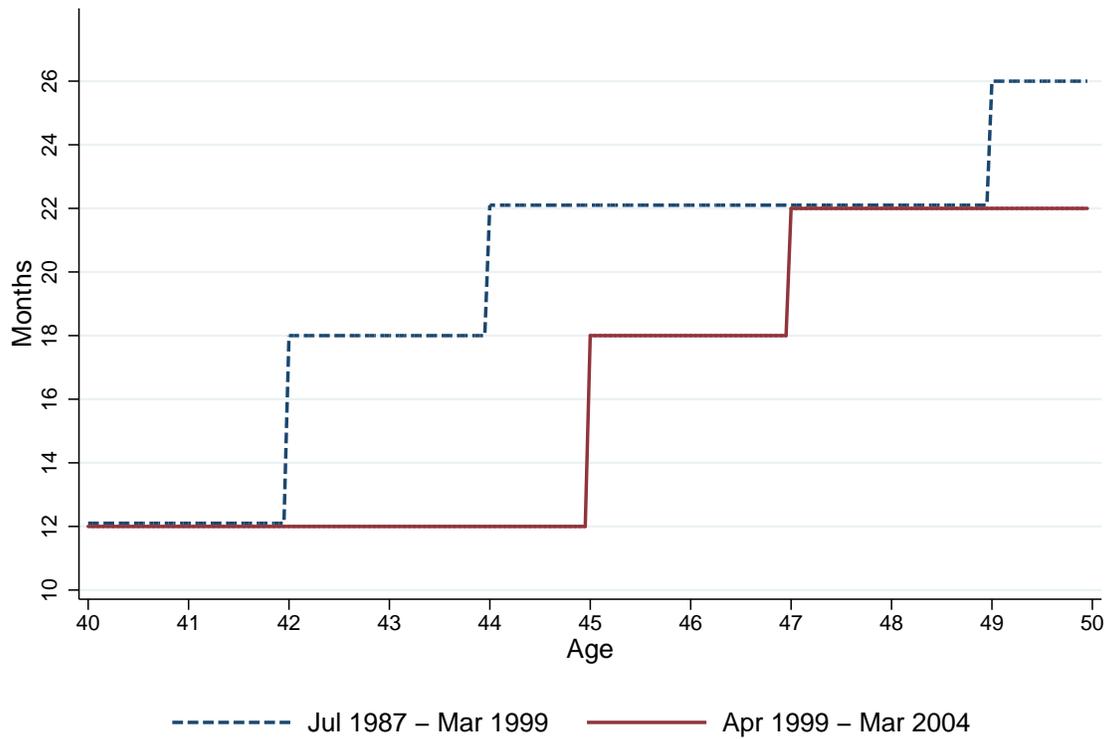
	(1) Mean & SE	(2) UI-Benefits Duration Marg. Effect $\frac{dB}{dP}$	(3) Marg. Effect Non-emp Dur with resp. to UI-Benefit Dur $\frac{dD}{dB}$
Real GDP Growth	3.07 [1.67]	-0.015 [0.0070]*	0.045 [0.028]
Real GDP Growth Year t+1	3.25 [1.71]	-0.026 [0.0061]**	0.037 [0.029]
Change in Unemployment Rate	0.13 [0.78]	0.042 [0.012]**	-0.065 [0.053]
Change in Unemployment Rate Year t+1	0.15 [0.83]	0.046 [0.011]**	-0.048 [0.051]
Unemployment Rate in Percent	9.09 [1.64]	-0.0095 [0.0066]	-0.036 [0.028]
Unemployment Rate Year t+1	9.23 [1.67]	0.0038 [0.0066]	-0.046 [0.026] [†]
Unemployment Rate Year t+2	9.37 [1.66]	0.014 [0.0067]*	-0.039 [0.027]
Mass Layoff Rate	1.31 [0.53]	0.058 [0.020]**	-0.17 [0.081]*
Average Log Wage Loss in Year-Quintile Cell	-0.14 [0.14]	-0.47 [0.078]**	0.81 [0.37]*
Mean of Dep Var		0.27	0.33
Observations in Row 1-4		51	51
Observations in Row 5		240	240

Notes: Columns (1) and (2) report coefficients from a 2 step regression. In the first step the effect of Extended UI durations on non-employment durations and actual UI durations are estimated separately for all years and age thresholds using the regression discontinuity estimator. In the second step the resulting elasticities/marginal effects are regressed on measures of the economic environment. Each reported coefficient represents the coefficient on those measures, given in the row names. The second step regressions are weighted by the inverse of the standard errors of the estimates and include a dummy the Post-1999 reform period.

Stars indicate confidence levels: [†]P<.1, * P<.05, ** P<.01.

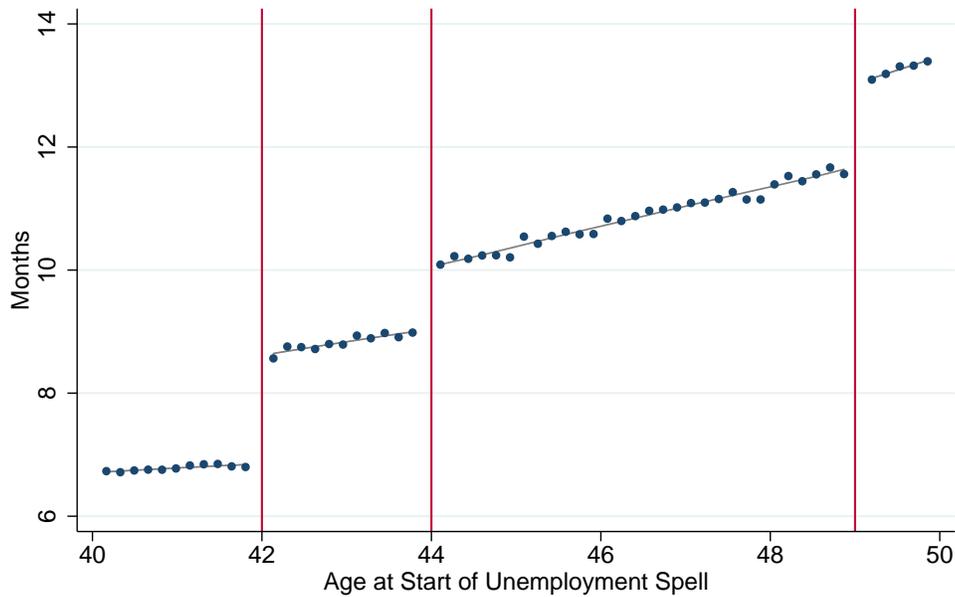
Figures

Figure 1: Potential UI Durations by Period (Workers in highest experience category)

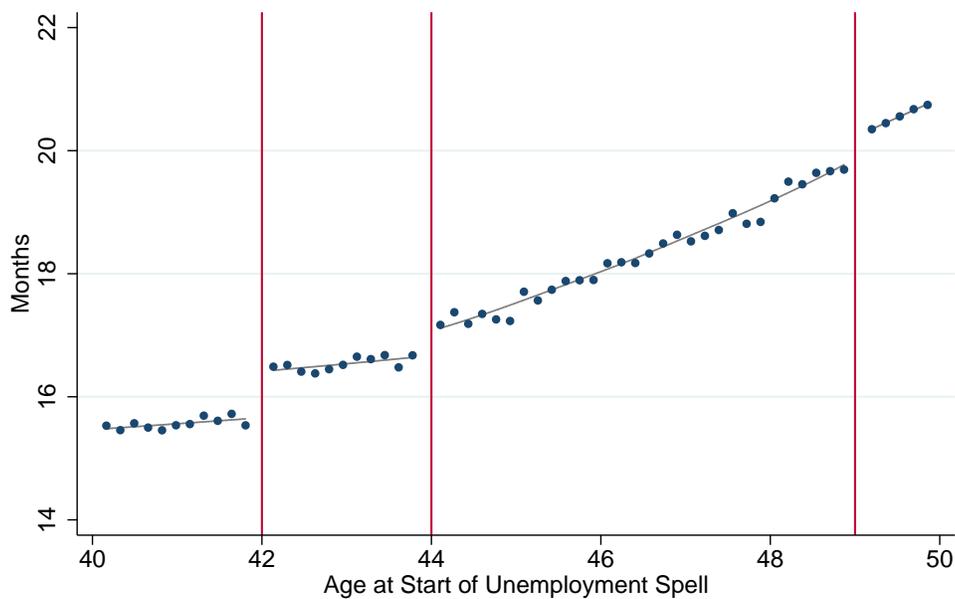


Notes: The figure shows how potential unemployment insurance (UI) durations for workers in the highest experience group vary with age and over time. For details on the required experience to be eligible for the maximum durations see Table 1.

Figure 2: Actual Unemployment Insurance Benefit (ALG) Durations and Non-employment Durations by Age - Period 1987 to 1999



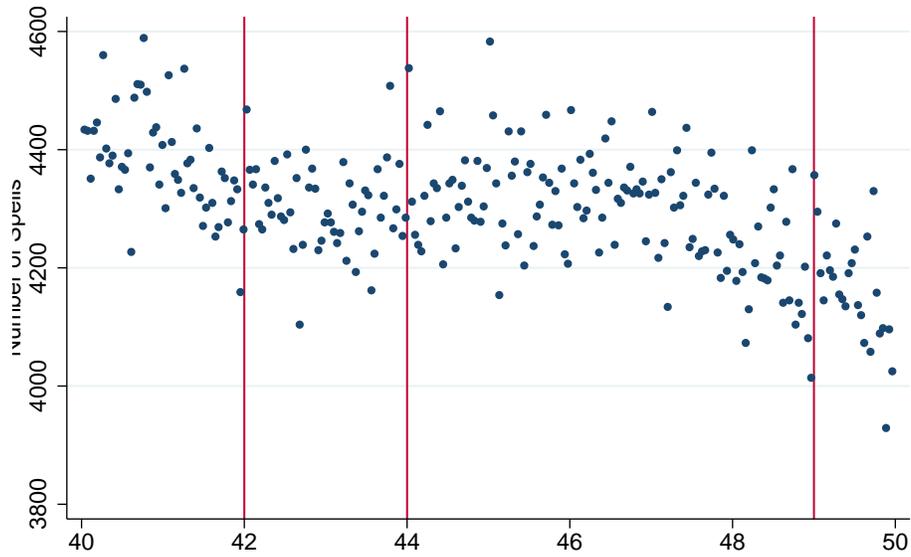
(a) Actual UI Duration



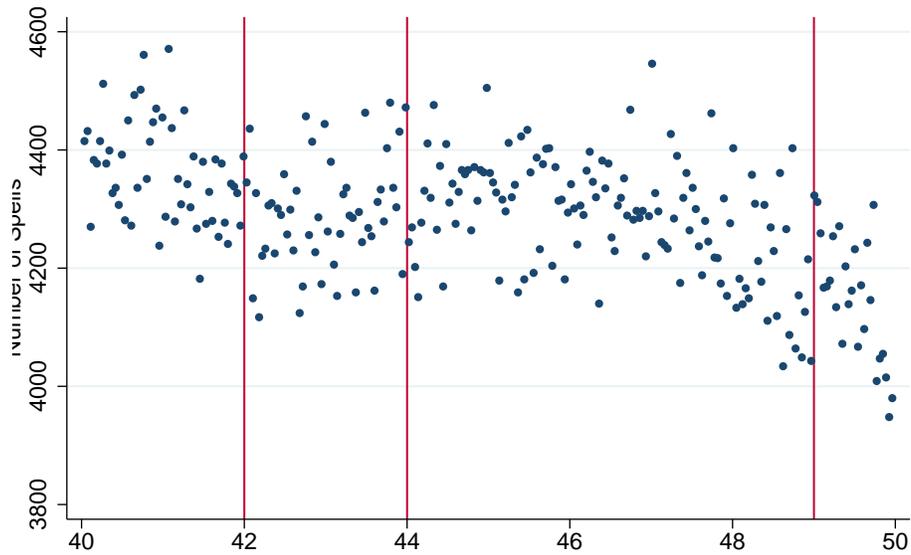
(b) Non-employment Duration

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 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 5 out of the last 7 years (and did not receive UI benefits in that time).

Figure 3: Density around Age Cutoffs for Potential UI Durations - Period July 1987 to March 1999



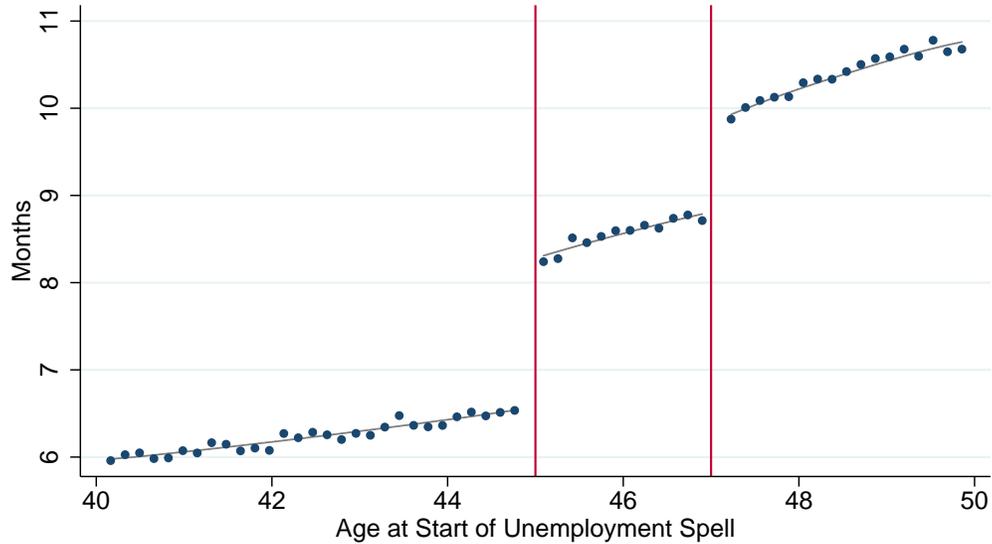
(a) Age on date of UI claim



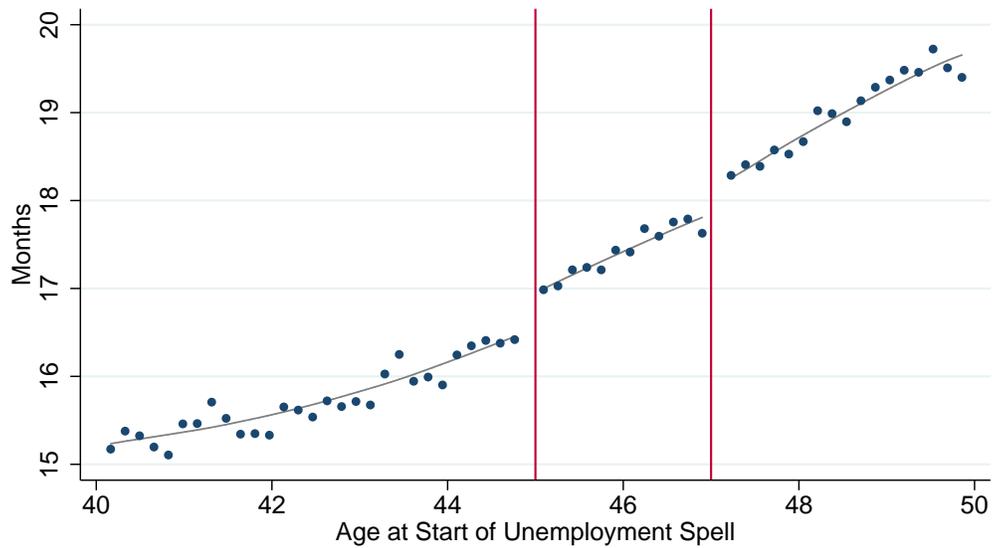
(b) Age at Job Loss

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 who had worked for at least 5 out of the last 7 years (and did not receive UI benefits in that time). Sample period: July 1987 - March 1999

Figure 4: Actual Unemployment Insurance Benefit (ALG) Durations and Non-employment Durations by Age - Period March 1999 to December 2004



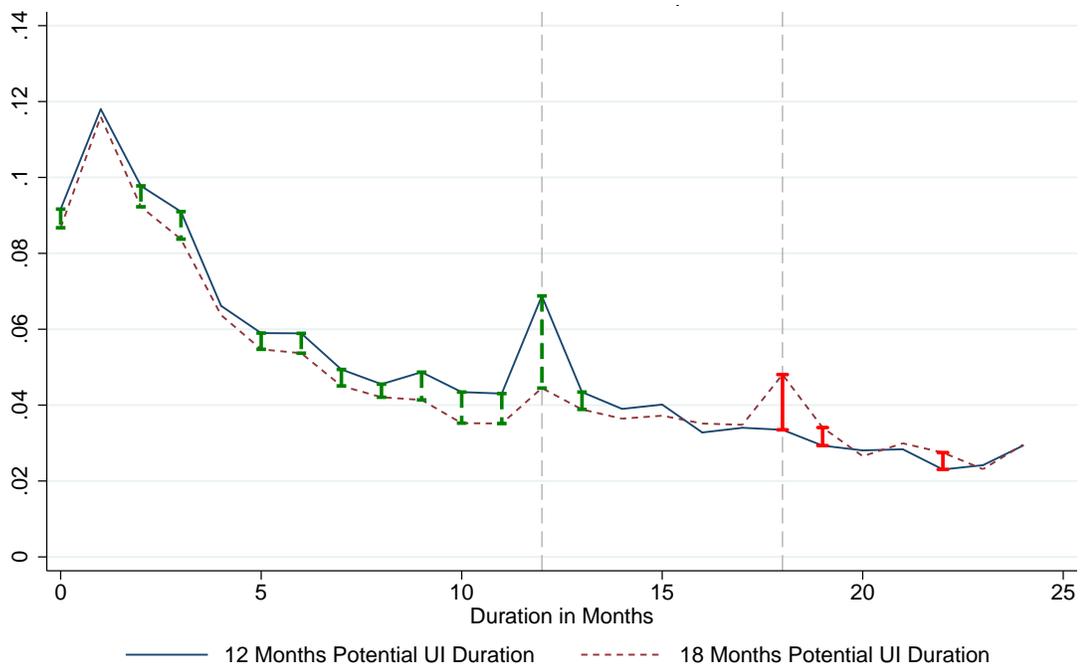
(a) Actual UI Durations



(b) Non-employment Durations

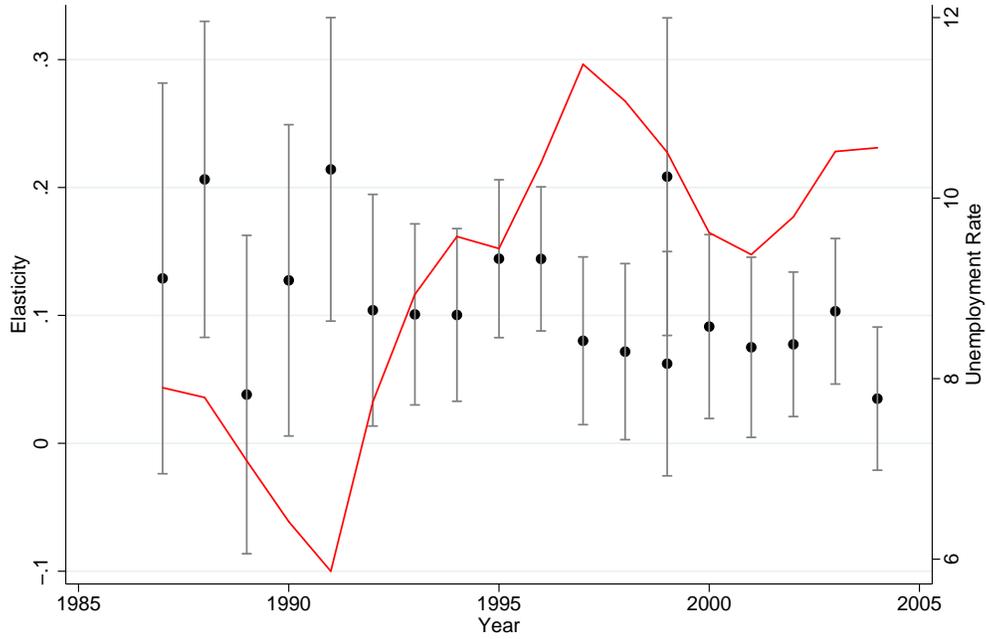
Notes: The top figure shows average durations of receiving UI benefits by age at the start of receiving unemployment insurance. The bottom figures shows average non-employment durations for these workers, where non-employment duration is measured as the time until return to a job and is capped at 36 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 5 out of the last 7 years (and did not receive UI benefits in that time).

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

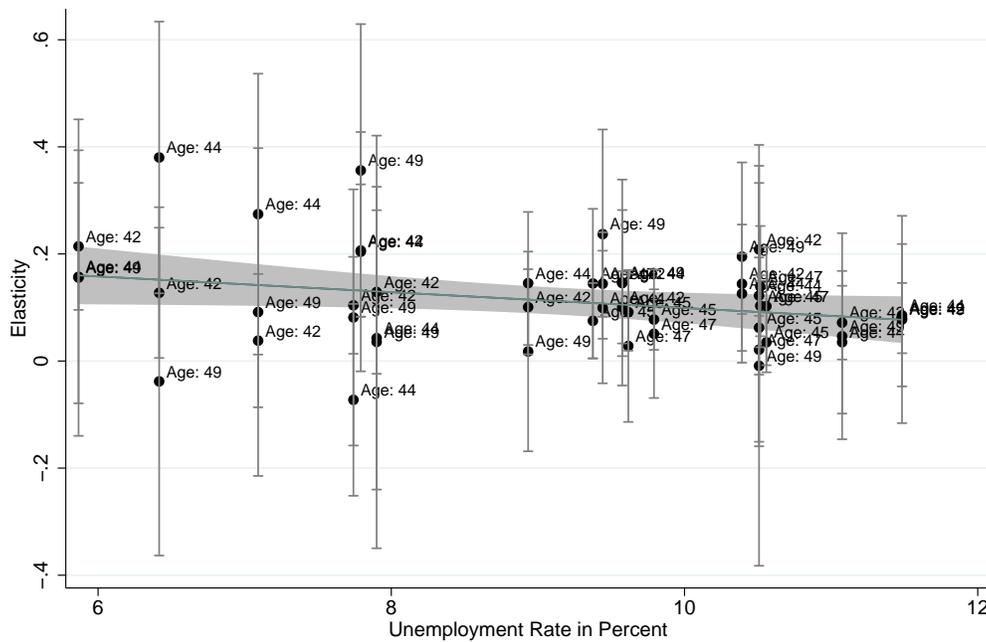


Notes: The difference between the hazard functions is estimated pointwise at each point of support using regression discontinuity estimation. Vertical bars indicate that the hazard rates are statistically significant from each other on the 5 percent level. Period July 1987 to March 1999. For details see text.

Figure 6: Variation in Regression Discontinuity Estimates of Non-employment Duration Elasticities with Respect to Potential UI Duration over Time and with Economic Environment



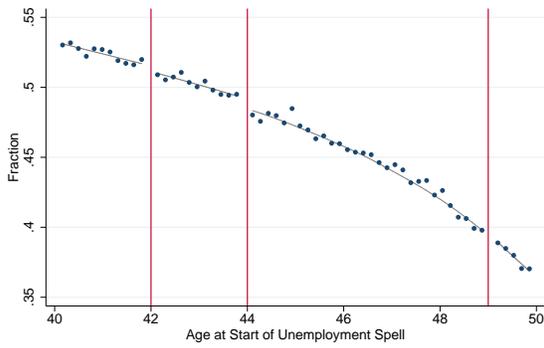
(a) Elasticities at the Age 42 and Age 45 Discontinuities by Year and the Unemployment Rate



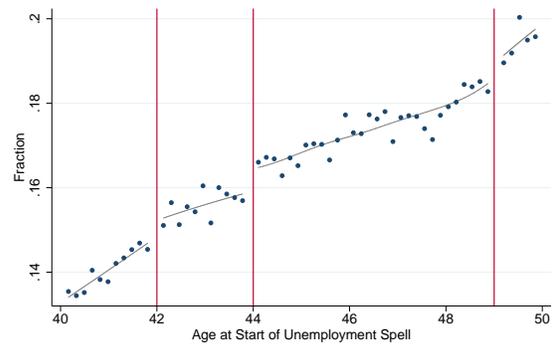
(b) Scatter Plot all estimated Elasticities vs. Unemployment Rate

Notes: Each dot in the bottom figure corresponds to a non-employment duration elasticity 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.

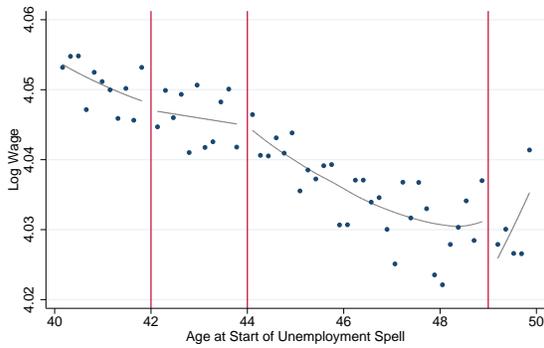
Figure 7: Future Employment Status and Post Unemployment Wages by Age



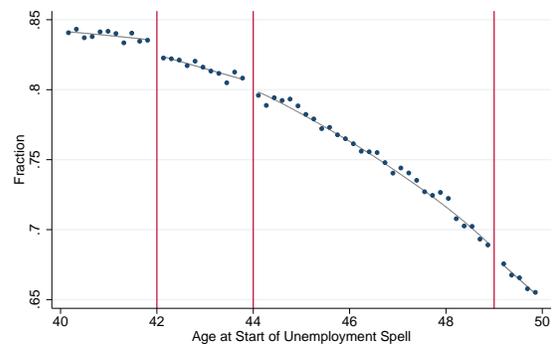
(a) Employed 5 years later



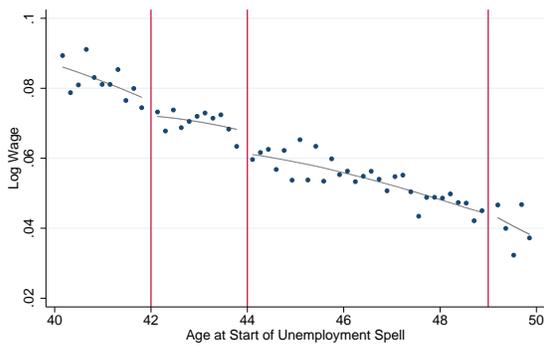
(b) Unemployed 5 years later



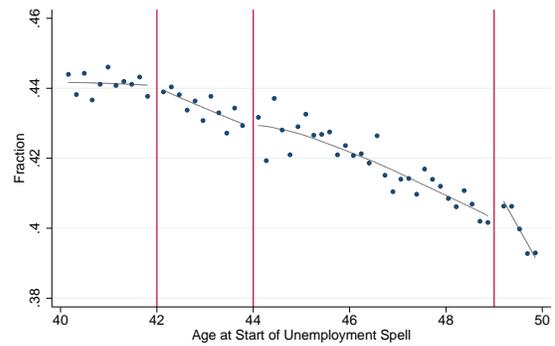
(c) Log wage at next Job



(d) Ever employed again



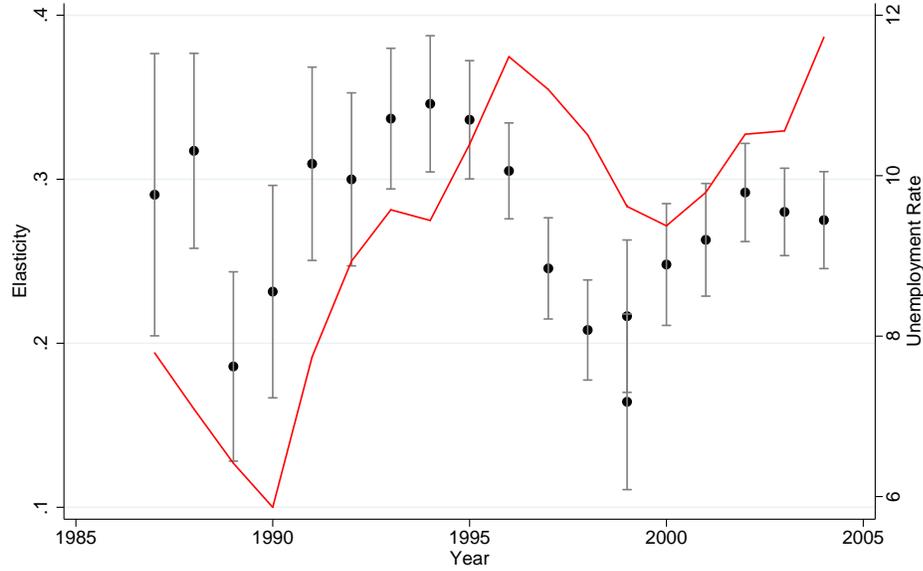
(e) Log Wage Growth over next 5 years



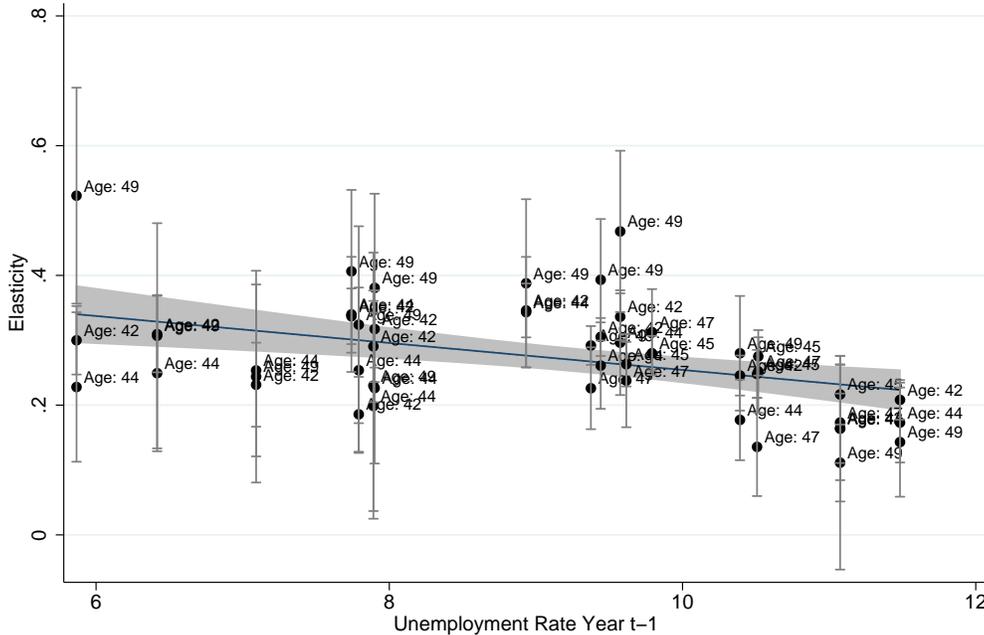
(f) Moved to a new region for next job

Notes: For sample description see Figure 2.

Figure 8: Variation in Regression Discontinuity Estimates of Marginal Effect of Potential UI Duration on 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. Unemployment Rate in Year t+1

Notes: Each dot in the bottom figure corresponds to a non-employment duration elasticity 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.

Appendix

Table A-1: Means and Standard Deviations of Main Variables

	(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 [5.3]	18.0 [4.7]
Duration of UI benefit receipt in months	6.5 [6.0]	6.9 [6.4]	8.1 [7.2]	9.0 [7.6]
Non-employment duration in months	14.5 [13.9]	14.7 [13.9]	16.7 [14.6]	17.3 [14.5]
Duration until next job (censored 2008)	13.3 [20.1]	12.7 [18.3]	14.6 [22.2]	14.2 [19.9]
Duration until next job if job within 36 months	8.0 [8.4]	8.1 [8.4]	8.4 [8.6]	8.9 [8.8]
Time between end of job and UI claim	1.6 [8.1]	1.4 [8.3]	1.5 [3.8]	1.4 [3.5]
Daily Post Unemployment Wage in Euro	54.5 [26.4]	53.9 [26.2]	62.5 [29.0]	62.2 [29.5]
Post Wage - Pre Wage in Euro	-3.7 [24.8]	-4.4 [24.3]	-10.1 [27.7]	-11.4 [27.9]
Log(Post Wage) - Log(Pre Wage)	-0.067 [0.48]	-0.079 [0.47]	-0.17 [0.50]	-0.19 [0.50]
Switch industry after unemployment	0.62 [0.49]	0.60 [0.49]	0.70 [0.46]	0.70 [0.46]
Switch occupation after unemployment	0.56 [0.50]	0.55 [0.50]	0.62 [0.49]	0.62 [0.49]
Ever employed again	0.85 [0.36]	0.84 [0.37]	0.78 [0.41]	0.77 [0.42]
Non-employment spell censored	0.23 [0.42]	0.23 [0.42]	0.30 [0.46]	0.31 [0.46]
Next job is fulltime employment	0.84 [0.37]	0.83 [0.37]	0.89 [0.31]	0.89 [0.31]
Log(Wage) 5 years after start of UI	4.01 [0.49]	3.97 [0.48]	4.15 [0.49]	4.12 [0.49]
Employed 5 years after start of UI	0.38 [0.49]	0.36 [0.48]	0.41 [0.49]	0.38 [0.49]
Unemployed 5 years after start of UI	0.14 [0.34]	0.15 [0.35]	0.10 [0.30]	0.11 [0.32]
Panel B: Demographic Variables				
Daily Wage in Euro (Pre-UI for Col 2-4)	59.2 [29.4]	58.9 [29.8]	74.1 [32.4]	74.5 [33.5]
Education years	10.9 [2.30]	10.8 [2.20]	11.0 [2.31]	10.9 [2.32]
Female	0.42 [0.49]	0.43 [0.49]	0.35 [0.48]	0.34 [0.47]
Non-German	0.082 [0.27]	0.078 [0.27]	0.089 [0.28]	0.096 [0.29]
Actual experience (censored 1975)	10.7 [8.49]	10.6 [8.49]	10.8 [8.49]	10.8 [8.49]
Firm tenure	2.58 [4.60]	2.58 [4.60]	2.69 [4.60]	2.69 [4.60]
Occupation tenure	5.44 [6.28]	5.44 [6.28]	5.56 [6.28]	5.56 [6.29]
Industry tenure	2.17 [2.71]	2.17 [2.71]	2.29 [2.71]	2.28 [2.70]
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. 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 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.