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The effects of unemployment insurance on labour supply and search outcomes

Regression discontinuity estimates from Germany

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Abstract

This paper evaluates the impact of large changes in the duration of unemployment insurance (UI) in different economic environments on labour supply, job matches, and search behaviour. We show that differences in eligibility thresholds by exact age give rise to a valid regression discontinuity design, which we implement using administrative data on the universe of new unemployment spells and career histories over twenty years from Germany. We find that increases in UI have small to modest effects on non-employment rates, a result robust over the business cycle and across demographic groups. Thus, large expansions in UI during recessions do not lead to lasting increases in unemployment duration, nor can they explain differences in unemployment durations across countries. We do not find any effect of increased UI duration on average job quality, but show that the mean potentially confounds differential effects on job search across the distribution of UI duration. However, it appears that for a majority of UI beneficiaries' increases in UI duration may lead to small declines in wages.

Zusammenfassung

Dieses Papier wertet den Einfluss von weitreichenden Veränderungen in der Bezugsdauer der Arbeitslosenversicherung in verschiedenen ökonomischen Umgebungen auf Arbeitsangebot, Qualität der Arbeitsstelle und Suchverhalten aus. Wir zeigen, dass Altersbegrenzungen in der Anspruchsberechtigung ein gültiges Regression-Discontinuity-Design erlauben, welches wir mit administrativen Daten auf Basis der Grundgesamtheit der Arbeitslosengeldbezieher in Deutschland über 20 Jahre umsetzen. Wir beobachten, dass Anstiege in der Arbeitslosenversicherung kleine bis mäßige Effekte auf die Nichtbeschäftigungsrate haben, ein Ergebnis, das robust über den Konjunkturzyklus und verschiedene demografische Gruppen ist. Demzufolge führen große Ausweitungen in der Arbeitslosenversicherung während Rezessionen weder zu andauernden Anstiegen der Arbeitslosigkeitsdauer, noch können sie Unterschiede in der Arbeitslosigkeitsdauer verschiedener Länder erklären. Wir können keinen Effekt von höherer Arbeitslosengeldbezugsdauer auf die durchschnittliche Arbeitsplatzqualität finden, aber zeigen, dass der Durchschnitt möglicherweise unterschiedliche Effekte auf die Jobsuche über die Verteilung der Arbeitslosengeldbezugsdauer miteinander vermengt. Es scheint jedoch, dass für eine Mehrheit der Arbeitslosengeldempfänger eine ansteigende Arbeitslosengeldbezugsdauer zu kleinen Abstrichen bei den Gehältern führt.

JEL classification: J30, J65

Keywords: Duration of unemployment insurance (UI), regression discontinuity design

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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. Extended UI has been a prominent feature of downturns in the U.S., 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 to counter increasing unemployment. For example, in Germany unemployment insurance benefits were increased from 12 to 18-32 months, depending on the demographic group.

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). Yet, there is a longstanding concern that the insurance benefit of UI comes at the cost of distorting labour supply incentives. This potential cost of UI may be even greater when recessions involve structural changes that render part of workers' skills obsolete (Ljungqvist and Sargent 1998, 2008). In this case the effective replacement rate may rise beyond the typical replacement rate and imply stronger effects on labour supply. 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 U.S. in the early 1980s. A similar mechanism, in a muted form, may apply during downturns in the U.S., when both UI durations and structural change may rise considerably.

Existing estimates indeed point to non-negligible effects of increased UI benefits and UI durations on non-employment spells (e.g., Krueger and Meyer 2002), although the interpretation of these estimates as pure moral hazard effects has recently been questioned (Chetty 2008). In either case, the magnitude of existing estimates does not imply substantial increases in unemployment as a result from longer UI durations (e.g., Katz and Meyer 1990), and this has been taken to mean that differences in UI regimes are unlikely to explain unemployment differentials across countries (e.g., Hunt 1995). However, most existing estimates are based on expansions of UI insurance at relatively short durations and job loss during relatively mild recessions. It may be that the effect of extended UI during larger downturns differs. Yet, this hypothesis is difficult to test, since usually extension in UI durations occur in conjunction with important changes in the economic environment.

Next to providing income support, a second key goal of UI is to aid workers to find higher quality jobs. In fact, a core prediction from the standard workhorse model of job search is that extended UI allows workers to find a better job match. However, much less is known about this aspect of UI (e.g., Krueger and Meyer 2002). Existing estimates point to a negligible effect of UI on wages.[1] This is a puzzle with respect to the standard search model, and instead points to search at constant wages. Alternatively, it may suggest offsetting effects of match improvement and human capital depreciation over the unemployment spell. The welfare and policy implications of alternative implications clearly differ. Understanding the effect of UI duration on job search is thus important. However, it requires analysis of the full dynamic response of wages and employment to UI expansions, something difficult to do in conventional data sets.

In this paper we evaluate the full impact of extended UI duration during different labour market states on both non-employment durations and job matching. We exploit differences in the UI duration for different age groups under multiple policy regimes in Germany, leading to sharp 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 non-employment, wages, and other labour market outcomes. 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 labour supply elasticites with respect to UI durations in Germany for large differential expansions, and compare these to findings in the United States.[2] We find that labour supply elasticities are moderate and at the lower end of U.S. estimates. They are somewhat larger than results in Card, Chetty, and Weber (2007), which use a comparable research design based on Austrian data.[3] Our elasticities are similar for different increases in UI duration, and similar to estimates based on much smaller increases. This suggests that long expansions in UI durations such as extended UI do not have a larger effect on labour supply, and cannot explain persistent differences in unemployment in Germany and the United States.

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 during difficult economic times. By comparing workers just above and below our age cut offs in periods with a high and low degree of structural change, we can assess the effect of changes in generosity of UI during different

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[1] This is found in a recent study by Card, Chetty, Weber (2007), and also appears in earlier studies (see, e.g., Meyer 2002)

[2] Hunt (1995) estimated labour supply effects of the German UI system in a difference in difference framework. We show below that our results indicate smaller labour supply effects.

[3] Our 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. This difference may partly stem from interactions with early retirement decisions, and thus may be misleading for understanding the labour supply effects among younger workers. We estimate treatment effects for different age groups, some of whom are very unlikely to be affected by early retirement.

economic environments. Furthermore exploiting variation in the degree of sector-specific changes vis-à-vis the economy wide state of labour demand also allows us to control for differences in the overall arrival rate of jobs. This closely approximates the ideal experiment implicit in Ljungqvist and Sargent (1998). The results point to negligible differences in the effect of UI across more turbulent or tranquil times. Based on our findings, there is no reason to believe that the adverse incentive of UI is stronger in recessions.

Our third main finding concerns the question of the effect of UI duration on job search. On the one hand, we do not find a beneficial effect of increased UI duration for any of the job outcomes we consider on *average*. On the other hand, we do find that the wage of UI beneficiaries steeply declines with duration, even though there seems to be no appreciable difference in any worker characteristic. This suggests that wages do decline with benefit duration above and beyond selection of workers. When we implement our regression discontinuity estimates at each point of potential UI duration, we find that workers at shorter duration may experience a small *negative* effect on the wage. Workers exhausting their benefits on the other hand experience a clear increase in wages. However, this increase is partly offset in the average by a clear drop when the new, higher threshold is reached.

Overall, our results imply that even long durations of extended UI are unlikely to significantly contribute to a rise in unemployment or differences in unemployment across countries. Conversely, reductions in UI duration are unlikely to appreciably reduce unemployment. We also confirm that on average increased UI duration has no beneficial effects on job search outcomes. However, our analysis of the dynamic pattern of unemployment durations and wages draw a more nuanced view of the effect of UI duration on job search. While workers exiting at short durations experience a net decline in wages, perhaps due to depreciation in human capital, those exhausting their benefits appear to raise their reservation wages. However, whether this increase leads to an increase in accepted wages of workers exiting at the threshold is doubtful.

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, precise estimates of the labour supply elasticities based on larger increases in UI durations. This complements existing studies mainly focusing on smaller increases at lower levels of duration. It also revises existing estimates for Germany downwards, bringing them closer in line to U.S. results. Second, we replicate our regression discontinuity estimates for different economic regimes to assess whether extended UI can have counterproductive effects in larger recessions. Contrary to predictions from differences in replacement rates driven by technological restructuring, we do not find that either duration effects or corresponding elasticities vary appreciably with the state of the industry or labour market. Third, our paper is one of the few studies thoroughly examining the effect of UI duration on job search behaviour. Our analysis of both average effects and the dynamic response of duration and wages suggests that match effects are at best quite small and concentrated on the subset of workers exhausting their benefits.

The outline of the paper is as follows. In section 2 we give a brief introduction of the main questions surrounding the evaluation of UI insurance that sets the stage for the further analysis. In section 3 we describe our administrative data and the institutional environment in Germany. Section 4 briefly reviews our empirical approach. Section 5 and 6 contain our main findings regarding the effect of extended UI on labour supply and job search outcomes, respectively. Section 7 offers preliminary conclusions.

2 The effect of unemployment insurance duration on labour supply and wages

The main predictions for the effect of changes in the duration of UI on labour supply are typically derived from a model of job search (e.g., Mortensen 1977). Yet, the main predictions are similar in a static model of labour supply (Moffitt and Nicholson 1982). In the standard search model, a worker receiving UI benefits will decrease the reservation wage and raise search intensity in the course of the UI spell as benefits start to run out. Thus, the escape rate from unemployment increases with the UI spell, whereas accepted wages decline. An increase in the potential duration of UI benefits leads workers to lower their search intensity and raise their reservation wage initially, thereby increasing the duration of unemployment. A similar disincentive effect occurs in the static labour supply model. In that model, workers can find a job anytime at the market wage, but face a trade-off between consumption and leisure. The presence of UI benefits affects the slope of the budget constraint until the point of benefit exhaustion. As in the search model, an extension of UI benefits lowers labour supply of UI beneficiaries.[4]

A long literature has estimated the effect of changes in the duration and generosity of UI benefits on duration of non-employment. The majority of these studies are identified by changes in benefit duration or replacement rates within U.S. states (e.g., Moffitt 1985, Meyer 1990, Katz and Meyer 1990a, b, Card and Levine 2000). Overall, the range of estimates indicate that the effect of benefit duration on labour supply is modest to small (e.g., Krueger and Meyer 2002). Certainly, these findings do not imply strong effects of small to modest increases in UI durations on the duration of non-employment spells. However, most estimates are typically based on mid-

[4] The search model also predicts the so-called 'entitlement effect', according to which increased and longer benefits raise the value of reemployment towards the end of a benefit spell (or for a worker that is not entitled to benefits). This leads workers to intensify their search effort and lower their reservation wage further towards the end of the benefit period. Simulations in Mortensen (1977) suggest that under reasonable parameter values both effects should lead the escape rate to increase towards the end of the UI spell. A similar effect can occur in the static labour supply model since a broader range of people will maximize their benefits at the kink point in the budget constraint (Moffitt and Nichols 1982).

dle sized increases in benefit durations, with the larger changes usually being about 2-3 months. Thus, it is difficult to infer from these studies the effect of more drastic increases in benefit durations, as often occurring in times of high unemployment.

This may be worrisome, since a recent extension of this literature has suggested that disincentive effects may be particularly strong in large recessions involving a high amount of structural change. The argument is cast by Sargent and Ljungqvist (1998) in an elaborate search model, but the point can be understood using the static model of labour supply. Benefits are set based on previous wage levels, but the work decision is based on the wage rate prevailing after job loss. Thus, effective replacement rates can be much higher than the average replacement rate. Thus, if workers experience a drop in demand for their industry or occupation specific skills, non-employment durations are likely to increase more than predicted by the average replacement rate. Sargent and Ljungqvist (1998) show that this can lead to lasting increases in unemployment if skill depreciates during the unemployment spell. If disincentives so increase in 'turbulent' times, the estimates from average time periods will underestimate the potential effect of benefit duration on labour supply.

In another recent extension the literature, Chetty (2008) has instead argued that existing estimates will *overestimate* the disincentive effect of UI durations on labour supply if workers are credit constraint. In this case, the estimated labour supply elasticity will capture both the substitution and the income effect from increased UI durations. Distinguishing between the two effects matters, since it affects optimal determination of benefit parameters. Card, Chetty, and Weber (2007) provide evidence that job losers respond similarly to lump-sum severance pay and increases in UI durations, consistent with the presence of credit constraints. This evidence suggests that already modest existing estimates may imply even lower disincentive effects.

Although the primary goal of UI is income insurance, a secondary goal is the subsidy of job search. The potential benefit of longer UI durations is immediately apparent in the standard search model with variable wages. The longer workers wait before accepting a job, the higher the likelihood of obtaining a good draw from the wage offer distribution. The fact that existing estimates do not point to a positive effect of longer UI durations on wages suggests that search may be modelled as a function of search intensity only (Card, Chetty, and Weber 2007). In this case, non-employment durations are predicted to increase with benefit durations, but wages are assumed to be flat. The observed decline in wages with elapsed benefit duration is then entirely attributed to worker heterogeneity. This model is a natural extension of the static labour supply model to a dynamic setting. Thus, the welfare benefits from increased UI durations primarily derive through increased leisure and reduced costs from job search.

In a more standard search model with variable wages, the welfare benefit from increased UI would also arise from obtaining a better job match. The model has a richer set of predictions for the search process; in particular, now there are multiple reasons why wages can fall over the unemployment spell other than heterogeneity, such as reductions in reservation wages, depreciation in human capital, or stigma effects. There is ample empirical evidence that the quality of jobs differs even for workers with the same characteristics. It is thus a puzzle that changes in search intensity and changes in UI durations do not appear to affect the wage.

Distinguishing between these two cases is important for assessing whether and how UI affects job search. Moreover, the welfare effect of UI depends on the effects of job search on wage outcomes.

3 Institutional background and data

3.1 The unemployment insurance system in Germany

This section describes the features of the German unemployment insurance system from the 1980s until the early 2000s.[5] In our empirical analysis we exploit various changes in the system during this time period. We therefore first describe the system as it existed until 1984 and then explain how the system changed in later years.

A worker is eligible to receive unemployment insurance benefits (UIB – Arbeitslosengeld in German) if he worked for at least 12 months in the previous 3 years (called the base period). The potential duration for receiving UIB depends on the number of months worked in the base period and can go up to 12 months. UI benefits are 68 percent of the last net wage of the worker and not means tested.[6] After UI benefits are exhausted an unemployed worker may receive unemployment assistance benefits (UA - Arbeitslosenhilfe in German), which is 58 percent of previous net income, however, unlike UIB, other sources of income (such as spouses income or income from financial assets) are subtracted. Furthermore the receipt of UA is means tested and a person may have to wait if considered too wealthy. There is no maximum duration for UA receipt and cases are reviewed once a year.

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 recipiency 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 2004).

Germany's unemployment insurance system saw a major reform in each of the last three decades. Between 1984 and 1987 the unemployment insurance system was subjected to several gradual changes. In 1984 UI benefit and UA replacement rates were lowered for individuals without children. The UIB replacement rate for this group decreased from 68 percent to 63 percent, while the UA replacement rate de-

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5 The discussion here draws on Hunt (1995), Arntz et al. (2007), and Fitzenberger and Wilke (2009) as well as our own reading of the law (Sozialgesetzbuch III)

6 According to Hunt (1995) a cap on the amount one may receive exists but only affects about 1 percent of the recipients.

creased from 58 to 53 percent. Over the next three years the potential UI durations for older workers with high experience levels were expanded substantially. For example, for workers age 42 to 43 the maximum rose to 18 months by 1988. For workers age 44 to 49 to 22 months and for 50 to 54 to 26 months. The complete mapping from experience during the base period, the age of the worker and the time of the beginning of the unemployment spell can be found in Table 1. Potential UI duration is determined by the age of the worker on the day she starts receiving UI benefits. Thus there are quite sharp discontinuities in potential durations at several age cut-offs, especially for workers with high labour force attachment who are eligible to the maximum UI durations. For these workers, Figure 1 shows the variation of UI durations with age during different time periods. The discontinuities in this function are the basis for our regression discontinuity estimates.

The system remained fairly stable from 1988 until March 1997, except for a slight decrease in the replacement rates in 1994 (see Table 1). Then in April 1997 the potential UI durations were lowered for older individuals. However the reform was phased in gradually, so that for most people it only took effect in April 1999 (see Arntz, Simon Lo, and Wilke 2007). The main change in this reform was that the age requirements required to qualify for the higher potential UI durations increased by 3 years. Furthermore stricter sanction rules for individual who did not comply with eligibility rules were introduced (see Boone et al. 2002, 2004).

Starting in 2003 a major round of reforms of the social security system came into effect, the so called Hartz reforms. The first three reforms (Hartz I-III) implemented on January 1st, 2003 (Hartz I and II) and January 1st, 2004 (Hartz III) focused on the organisational structure of the public employment services and on active labour market policies (such as training and wage subsidy programs).[7] The last reform (Hartz IV) took effect on January 1st, 2005 and overhauled the UI insurance system. The main changes were the merge of AU with the general social assistance (welfare) system, which made payments unrelated to previous earnings and purely means tested, and a change in potential ALG durations.

3.2 Data

The data for this paper is the universe of social security records in Germany. Between 1975 and 2008, we observe for the entire population of Germany every employment spell in a job that is covered by social security and every spell of receiving unemployment insurance benefits. Individual workers can be followed using a unique person ID and since about 80 percent of all jobs are within the social security system (the main exceptions are self-employed and government employees) this results in nearly complete work histories for the vast majority of individuals. Compared to many other social security datasets, this data is very detailed and we ob-

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[7] See Jacobi and Kluve (2007) for a detailed description and summary of evaluations of Hartz I-III.

serve several demographic characteristics, namely gender, education, birth date, nationality, place of residence and work, as well as detailed job characteristics, such as average daily wage, occupation, industry, exact start and end date for each job. We also know exactly when a person received unemployment insurance benefits (ALG) or unemployment assistance (ALH) and how much. Each employment record also has a unique establishment identifier and various establishment characteristics can thus be merged to individual spells. On the establishment level we can identify plant closings and mass lay-offs.[8] Overall this dataset covers a total of about 1 billion employment and unemployment spells and about 24 million workers per year.

The focus of this paper is the analysis of unemployment spells. Thus we created our analysis sample by selecting all unemployment spells in this data, about 36 million. For each unemployment spell we created variables about the previous work history (such as tenure, experience, wage, industry and occupation at the previous job), the duration of the receiving UI benefits, the level of unemployment benefits, and information about the next job held after unemployment. We do not directly observe whether individuals are unemployed, but instead know whether they receive UI or unemployment assistance (ALH), whether they are in registered employment, or neither of the two. As a proxy for unemployment durations we use the duration of non-employment, which is measured as the time between the start of receiving UI benefits and the date of the next registered employment spell. This may overstate actual non-employment durations if some individuals become self-employed. Since some people take many years until returning to registered employment while other 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.

The main 'treatment' variable we are interested in is the maximum potential duration of unemployment insurance benefits. This is not directly available in the data so we use information about the law in the relevant time periods and our detailed information on the work history of the individuals to impute the potential duration. This imputation works very well for workers who have been employed for a long continuous time for whom the rules are very clear. However, the imputation is not as clear cut for workers with intermittent unemployment spells. We thus define our core analysis sample to be all unemployment spells of workers who have been working for at least 6 of the last 7 years and never received unemployment insurance during that time. In this sample all workers are eligible to the maximum potential durations for their age groups.

[8] See Schmieder, von Wachter and Bender (2009) for an analysis of earnings losses due to mass lay-offs based on the same data.

4 Methodology

A crucial parameter for policy makers is the labour supply elasticity with respect to features of the unemployment insurance system, such as potential benefit durations and replacement ratios. To obtain an estimate for this elasticity, estimates of the causal effect of changes in these features are required. We obtain such estimates for subgroups of the population using a regression discontinuity design which exploits age discontinuities in potential unemployment benefit durations. For our main results we focus on the relatively long period between July 1987 and March 1999 during which the system remained largely unchanged.[9] As described above, during this period there are particularly sharp discontinuities for workers with the highest experience rating.

We estimate variants of the following regression model:

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

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*. We focus on the three sharp thresholds in our sample at age 42, 44 and 49.[10] 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 is age in our case, around the threshold. If this assumption holds then estimates for $\beta_1$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.

One 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 second possibility that has become standard in the literature is to estimate equation 1 locally around the cut-offs and to specify *f(a)* as a linear function while allowing different slopes on both sides of the cut-off. One can then reduce the bandwidth around the cut-off to assess the validity of the RD design, since smaller bandwidths should reduce the bias of the estimator, however at the cost of efficiency. We present our results using the second approach, which in using observations close to the cut-off is generally considered closer in spirit to the RD identifying assumption that treatment is assigned as good as random close to the cut-off. 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.

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[9] As explained above the 1997 reform did not come into effect for the high experience workers until April 1999.

[10] There 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.

It is possible to include other control variables in the RD regressions, in hope 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 dataset have little explanatory power (partly because we estimate our model on a relatively homogenous sample of workers). The efficiency gain from this is very small, so that we prefer to present the raw estimates without controlling for additional variables.

An important potential threat to the RD identification exists if individuals have direct control over the forcing variable. If this is the case the individuals who choose specific values of the forcing variable may well be different from individuals choosing other values. Since they may take the cut-offs into consideration this may lead to a violation of the continuity assumption crucial for identification in the RD setting. 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. If situations where this is possible differ systematically from other situations, then workers to the left of the cut-off may be systematically different from workers to the right of it. In this case, however, there should be breaks in the density of unemployment spells around the age cut-offs - something that can easily be tested for.[11] Furthermore we test whether other predetermined variables vary smoothly around the cut-offs.

The other reason for systematic differences of workers along the forcing variable is that workers can decide on the day on which they first claim unemployment benefits. A worker who becomes unemployed a few days before her 42nd birthday may have a significant incentive to delay claiming unemployment insurance until her birthday, depending on her discount rate and expected unemployment duration (roughly 25 percent of the unemployed just below age 42 exhaust their 12 months ALG durations).[12]

While this incentive may be sizeable for workers very close to the cut-off, it very quickly declines further away from the cut-off. The reason is that by delaying the unemployment benefit claim the worker gives up benefits she would receive with certainty for an increase in benefits she may only receive in case she is unemployed

[11] The 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 cut-offs - something we test for.

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

for a very large time. Workers (close to the age 42 cut-off) who delay claiming and then find a job within the next 12 months lose all the benefits they could have received during the delay time. Furthermore a month of delay means the loss of the full unemployment benefit amount, while the increase in potential durations means that during that time workers receive ALG payments but may otherwise have received ALH payments. 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. Since in our data we observe the last date of employment as well as the day of claiming UI benefits it is easy to test whether the duration between end of the job and claiming UI benefits increases close to the age cut-offs. Furthermore density tests as well as tests for smoothness of other variables allow to investigate whether this may lead to a potential bias.[13]

5 Elasticity results

5.1 The effect of UI durations on non-employment durations

Figure 2 (a) shows how the duration of receiving UI varies with the age at the beginning of the unemployment spell. Workers younger than 42 at the age of claiming UI, are eligible to 12 months UI of which they use about 6.5 months on average. At the age 42 threshold UI eligibility increases to 18 months and the average duration or UI receipt increases to about 8.2 months. There are also clear and large increases at the age 44 and age 49 cut-offs. The increase in receipt duration are quite large, and range from one fourth (at the age 44 cut-off) to one third (at the age 49 cut-off) of the increase in the maximum UI durations. This indicates that a large number of individuals are quite substantially affected by the increase in UI.

The increases in actual UI durations at the cut-offs are a combination of two effects. On the one hand individuals who otherwise would have exhausted their UI benefits, but would have remained unemployed, continue receiving UI benefits under the more generous system. On the other hand individuals respond to more generous UI benefits by remaining unemployed longer.

To isolate the behavioural effect of extended UI, Figure 2 (b) shows the effect on non-employment durations and thus the main outcome variable of our analysis. There is a clear jump in non-employment durations at the age 42 cut-off from about 14.7 to 15.5 months of non-employment. At age 44, non-employment durations in-

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

crease from 15.5 to 16 months and at age 49 from 18 to 18.4. Thus while some of the increase in durations of UI receipt are due to longer coverage of individuals would have been unemployed anyways, there is also a substantial behavioural response.

Regression results for estimating equation 1 for these outcome variables are shown in Table 3. The results are very consistent with the graphical analysis when we choose a bandwidth of 2 years for the local linear regressions. For smaller bandwidths coefficients are extremely stable for the UI duration regressions, even with bandwidths as small as 0.5 or 0.2 years. For the non-employment durations they are also in the same ballpark across different bandwidths, but somewhat larger for tighter bandwidths. From investigating figures with different bandwidths it is clear that this is due to undersmoothing for the smaller bandwidths. Note that 2 years is already a very narrow bandwidth in comparison to other papers, for example Lemieux and Milligan (2008) use a bandwidth of 6 years. We thus have most confidence in estimates with 2 year bandwidths.[14]

Focusing on the 2 year bandwidth results: at the age 42 cut-off non-employment durations increase by 0.8 months (standard error 0.1 months) which corresponds to an elasticity of about 0.14.[15] At age 44 the increase is 0.45 months and at age 49 the increase 0.4 months. Strikingly despite the fact that the increases in UI occur at very different levels of non-employment and UI durations, the implied elasticities are nearly the same for the different cut-offs (0.14, 0.14, and 0.13). This may indicate that the elasticity (rather than the marginal effect) is the right statistic to extrapolate the effect to other settings.

Our non-employment duration variable includes individuals who never return to employment. Thus the increase in non-employment durations could be partly due to people staying out of employment forever, rather than taking longer until returning to a job. In order to investigate this Table 4 column (4) shows the probability of ever returning to registered employment again. There is a slight drop at the age 42 cut-off: individuals above the cut-off have a 0.6 percent lower probability of ever returning to work again. This therefore accounts for a very small increase in the non-employment durations. For the other cut-offs the effect is of the same magnitude but less precisely estimated and thus statistically insignificant. Furthermore there is no statistically significant effect on being employed five years after the start of UI.[16]

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[14] There 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 two year window to the right of the 49 cut-off.

[15] This is calculated as an increase in non-employment durations of 0.83 months over an average non-employment duration around the cut-off of 15 months relative to an increase of 6 months over average potential UI durations of 15 months.

[16] This refers to the employment status on the day 5 years after the start of UI. This is naturally a noisier measure for future labour market participation than whether someone ever returns to employment, which explains the larger standard error on this coefficient.

There is a slightly positive effect of being on unemployment insurance or unemployment assistance five years after the start of UI for the 42 and 44 age cut-off. These could be either individuals who are still on unemployment assistance (which is unlimited) or indicative of matching effects - a possibility to which we will return later. Again this effect is very small though, with a 0.5 percent higher probability of being on UI or UA and standard errors tight enough to rule out much larger effects. Given the small magnitudes of these effects it certainly does not seem like there are strong permanent employment effects.

It is interesting to compare our results with previous research on the German UI system. In particular Hunt (1995) and Fitzenberger and Wilke (2009) evaluate the Germany UI system in the 1980s and 1990s 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. For this approach these papers pool age groups of workers and assume that changes over time are due to the differential increase in UI durations for these age groups. It is important to note that both papers focus more on the older age groups, which may account for some of the differences.

Compared to Hunt (1995) we find smaller non-employment elasticities. We believe the main reason for this difference is the age gradient in non-employment durations, which was revealed in Figure 2. For example between age 44 and 49 non-employment durations increase from 16 to 18 months. A simple difference estimator comparing the 42-44 age group with the 44 to 49 age group would therefore conclude that the difference in UI eligibility of 4 months increases non-employment durations by 1.5 months (from 15.5 to 17) which is much larger than our RD estimate of 0.45 months at the 44 threshold. Hunt (1995) uses a difference in difference estimator, which attempts to control for the age gradient by taking the baseline age gradient (in the Pre 1984 period) into account, where there was no difference in UI eligibility across the age groups. This approach may of course fail if there are differential time trends for the treatment and control groups. When we replicated our analysis for the Pre 1984 period we found that there was no age gradient in non-employment durations.

Thus the difference in difference estimator picks up the increase in the age gradient and interprets this as an increase due to the change in the UI system. This could be valid if we had only one RD estimate which might pick up a local treatment effect and the treatment effect would vary with age. However, we find nearly the same elasticity across all three age cut-offs, making this highly unlikely. It thus seems that there were other reasons for the increase in the age gradients, such as early retirement plans, which lead to an upward bias in Hunt's (1995) estimates.

Fitzenberger and Wilke's (2009) main findings concern the age groups older than 50, which we excluded from our analysis. As Hunt (1995), Fitzenberger and Wilke use a difference in difference estimator, which may therefore in principle have similar problems. 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 no jump at the UI discontinuities. It appears therefore that a difference in difference estimator would again falsely attribute this to the increase in the gradient to the UI changes. The comparison with these difference in difference studies is certainly interesting and clearly shows the potential threats to identification inherent in the difference in difference approach. (See also Lemieux and Milligan 2007 for a similar comparison).

Lalive (2008) evaluates the effects of UI in Austria in a similar regression discontinuity design. 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 0.14.[17] As a rescaled marginal effect however the effect is smaller than our finding: we find that at the age 42 cut-off one additional month of UI increases durations by 0.14 months, while Lalive's results imply an increase in 0.9 months.

This shows an inherent problem with this kind of reduced form analysis: the estimated parameters are reduced form parameters without a clear connection to a deep structural parameter. To compare such parameters across studies with different treatments requires an extrapolation of what one parameter estimate estimated for one treatment would imply for the treatment in another study. Such an extrapolation necessarily requires functional form assumptions (such as implicit when choosing to compare the marginal effect or the elasticity), which are difficult to justify. This is a clear advantage of structural modelling which provides clear guidelines how to extrapolate results to other settings, a point to which we are planning to come back to in the structural part.[18]

5.2 Identification assumptions

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. This assumption can be tested for observable characteristics, both by plotting the observables vs. the forcing variable and by estimating equation 1 with the observables as outcome variables. Table 5

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[17] The 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: $b = [\log(UnempDur_1) - \log(UnempDur_2)] / \log(PotDur_1) - \log(PotDur_2)$. Calculating elasticities this way does not affect our elasticities very much (about 0.137 rather than 0.139) 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.

[18] Lalive (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.

presents results of these regressions. Of the 24 coefficients in Table 5, there are only two statistically significant on the 5 percent level. There is a slight increase in education at the 42 year threshold. However this increase and the statistically significant jump in the fraction female at the age 49 cut-off are quite small (1.6 percent).

A second standard way of testing the RD assumption is to look at the smoothness of the density around the cut-offs. This can be assessed visually and a formal test has been developed by McCrary (2008). Figure 3 (a) shows the number of spells in 2 week age intervals. On average there are around 3000 spells in each bin up until age 47, after which the number of spells begin to decrease. It appears that at each cut-off there is a slight increase in the density in the bin directly on the right of the cut-off. Implementing the McCrary test, this increase is statistically significant on the 5 percent level for the 42 and 49 cut-off but of very small magnitude.

Such an increase could either occur because firms are more likely to lay off worker with higher potential UI durations, because of a higher probability of claiming UI, or because workers wait until their birthdays before claiming UI benefits (as explained in the previous section). To test for the first possibility we show the density of spells with respect of the dates the last job prior to UI ended in Figure 3 (b). If firms either 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 cut-offs, 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. To test for this we check whether the time between job loss and first take up of UI benefits varies around the threshold. The regressions are reported in Table 3, column (3). These provide 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 cut-off. The duration measure is simply too noisy to pick this up. Given the economics incentives it makes sense that only individuals very close to the age cut-off would decide to wait until their birthday. For example given the estimates in Table 3 an individual at the age 42 cut-off can expect to receive UI for about 1.7 months longer if they are eligible to 18 rather than 12 months. The economic cost of delaying the claim is that the individual does not receive UI until claim and if the individual would not exhaust the 12 month benefits, this would be without benefit in the future. So 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 1.7 months for the higher benefit durations.

Essentially it appears that the discontinuity in the density is driven by maximally a few hundred spells shifted to the right just around the cut-offs. It should be noted that this is relative to around 300,000 spells in each of the 4 year intervals that we use for our RD estimation.[19] Since the magnitude of this effect is very small (in particular relative to our non-employment results) and there are essentially no discontinuities in other variables we do not think this is a big threat to the validity of our main estimates. As a robustness check we estimated all of our main results excluding observations within one month of the cut-offs in Panel B of Table 4. 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.[20]

5.3 Interpretation and reverse experiment

The overall magnitude of our estimates of the labour supply elasticities with respect of UI duration are quite comparable to similar elasticities found in the United States (e.g., Meyer 1990, Katz and Meyer 1990, Card and Levine 2000). They are lower than those reported by Hunt (1995) for Germany, and in the ballpark of results in Card, Chetty, and Weber (2007). This is an important finding since our estimates are based on much larger increases in unemployment durations than in these previous papers. As noted by Moffitt (1985), it is in difficult economic times when large extensions in UI take place and are likely to matter most.

These relatively low elasticities imply that changes in the duration of UI benefits do not have a large effect on unemployment durations and hence on the unemployment rate. As a result, all else equal, periods of extended UI would not be predicted to substantially raise the duration of non-employment. This calculation misses an effect on the inflow rate into unemployment, but our discussion above suggests the impact of extended UI duration on entries into unemployment in Germany is small at best.[21] The conclusion might be different for the United States, where UI has been found to increase layoff rates through incomplete experience rating. However, by the time extended UI is typically enacted, most of layoffs are likely to have already occurred.

By the same token, reforms of the UI system that reduce benefit durations are not predicted to have a substantial effect on non-employment duration or on unemployment rates. In 1997 Germany reformed its UI system, substantially reducing the

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[19] In smaller datasets this effect would almost certainly not be detectable.

[20] It is interesting to note that the density discontinuities are larger for the 1999 to 2004 sample (see 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.

[21] Most of the positive effect of UI on layoff rates in the United States has been attributed to the presence of experience rating (e.g., see Table 2.3 in Krueger and Meyer 2002). Since there is no experience rating in the German unemployment system, so the result of no effect on layoffs of increased UI durations is quite consistent with U.S. studies.

length of benefit durations for some groups of workers. Part of the intended effect was to reduce unemployment durations. Figure 4 shows the regression discontinuity effect on non-employment durations comparable to Figure 2, but for the new, post-1997 regime.[22] The figure shows that the discontinuities in non-employment durations move to the new age thresholds. This confirms that the assumption implicit in our analysis of the discontinuities at the pre-1997 age thresholds is valid. Table 6 shows that the elasticities are now smaller than what was shown in Table 3, although they are still similar across age-groups. From Figure 4 it is also apparent that the duration of the average unemployment spell decreased for each age. On the one hand, this is due to stricter monitoring of job search behaviour and penalties for not accepting suitable jobs. On the other hand, it may be partly due to an increased in incidence of short-term low wage jobs. As we will see next, the differences are unlikely to be due to changes in the economic environment in the labour market.

5.4 Variation of labour supply effects with the business cycle

Katz and Meyer (1990) and Hunt (1995) concluded that variation in the generosity of UI is unlikely to explain the substantial differences in elapsed unemployment duration in the US and Germany. Since the elasticities we report are lower than theirs, our findings confirm this conclusion. The average duration of uncompleted non-employment spells in Table 2 is 19 months for workers with high labour force attachment. The duration of non-employment spells in the U.S. is substantially lower - more on the order of magnitude of 4-5 months.[23] However, according to Table 3 (column 3) an increase in UI duration of six months leads to an increase in non-employment of only about one month for all age groups.

This comparison makes important simplifications among several dimensions. Perhaps most importantly, it makes no statement about the effect of continued income assistance through unemployment assistance (ALH), which may well account for part of the remaining difference (however, Hunt (1995) reports that few workers switch from unemployment insurance to ALH).[24] Yet, the main criticism has been that large increases in non-employment durations due to differences in generosity of UI should arise mainly in periods of increased structural change (Sargent and Ljungqvist 1998).

To try and assess this prediction, we can use the fact that Germany has gone through a dramatic boom-bust period after unification, plus an ensuing protracted

[22] For the high experience workers that we focus on the reform did not come into effect until April 1999. Therefore when we speak of our post-1997 reform results we refer to a sample of individuals starting UI between April 1999 and December 2004.

[23] Finding an exact counterpart to the numbers in Table 2 is difficult, since a comparable sample would include only unemployed individuals with high attachment to the labour force.

[24] On the other hand, the elasticity at the worker level might overstate the effect at the macroeconomic level, since increased duration for workers covered by UI may imply lower durations for uncovered workers.

slump. The first panel of Figure 5 plots elasticities obtained by replicating our regression discontinuity estimates separately for each calendar year and age group. The figure has two messages. First, the range of elasticities for 42 year olds is between 0.1 and 0.4. The range is the same for 44 year olds; elasticities are substantially more variable, somewhat smaller, and declining over time for 49 year olds (not shown). Second, it is apparent that there is no systematic variation with any aspect of the business cycle of the elasticities. The latter pattern can also be seen clearly from the second panel of Figure 5, where we plot the elasticities for all ages against the unemployment rates. In fact, if we were to exclude age 49, the relationship would be *negative*.

These findings are summarized and extended in Table 8, where we extend the results from Figure 5 (b). First, we show that the main finding holds when we use different indicators of the change in labour market conditions, including the annual mass-layoff and plant closing rate, which we calculated from our data.[25,26]

Second, in the bottom panel we show changes in labour supply elasticities for workers losing their jobs in industries with high or low average wage losses (as measured by quintiles). The average wage loss may be a better measure of the amount of specific skill a worker is likely to lose. Moreover, we can then control for a potential confounding effect from changes in overall labour demand by either adding the rate of unemployment or year effects. The results are pretty unequivocal - there is no difference in elasticity with the predicted wage change.

Table 9 shows estimates of the discontinuities for the three age-thresholds for periods of increasing and declining unemployment. The table confirms the results from the figure. While at 42 and 44 the elasticities decline with increases in unemployment, they appear to rise for 49 year olds. In separate results, we also found that these pattern hold for the level and the change in unemployment rates, as well as for the annual incidence of mass-layoffs and plant closings.

These findings are similar to results reported in Moffitt (1985). 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. A negative relationship can arise since low arrival rates imply that parameters of the UI benefits system have smaller effects on the probability of finding a job. Thus, a potential criticism of our measure of 'turbulence' is that it

[25] In fact, if we regress the elasticities weighted by the inverse of their standard error on a constant, 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.

[26] See Schmieder, von Wachter and Bender (2009) for information on how these rates are calculated in the German data.

does not capture incidence when workers are likely to lose a higher amount of, say, industry specific skills, but when the arrival rate is low. This effect will work counter a potential increase in moral hazard.

Part of this criticism is taken care of by the fact that the elasticities implicitly account for a worsening state of the labour market since they normalize by the average duration. In fact, this implies that the coefficient estimates (normalized by the increase in potential benefit duration) should be positively correlated with our indicators of the state of the labour market. This is true, yet the positive correlation is still weak. This is because the average unemployment duration is only weakly procyclical.

5.5 Differences by gender

We explored differences in our findings among several subgroups. The differences we found were typically relatively small. The most telling and pervasive difference was with respect to gender. This is shown in Table 7. First, perhaps not surprisingly, the effect of increases in potential benefit duration leads to a bigger effect on actual benefit receipt for women. This effect does translate to only slightly higher elasticities of non-employment duration, implying a stronger effect on labour force participation. This is born out by the fact that women experience an increase in the probability of being on UI and a decline in the probability of working 5 years after the beginning of the initial spell. For men, on the other hand, the effect on employment or UI receipt five years later is a pretty clean zero. Second, women also experience a larger decline in wages at reemployment significant at the 10 % level, at least for the 42 and 49 year olds. For men, the point estimates are less than half and very imprecisely estimated, even though the sample size is double that of women.

We also tried various other sample splits, including differences by education, job tenure, and tenure in a given industry or occupation, most of which yielded inconclusive results. The only difference worth noting is for workers with high job tenure, shown in Table 8. As expected, these workers have higher elasticities of actual UI benefit receipt and of non-employment durations at all ages. They also are less likely to be ever employed again. There is no difference in wage losses, though this may be partly driven by selection. Overall, considering the fact that estimated elasticities and match effects do not vary substantially across age groups, across the size of the expansion in potential UI benefits, or the state of the business cycle, we interpret our findings to imply very stable small to moderate effects on labour supply and no effects on average match quality.

6 UI expansions and job matching: Average and dynamic effects

6.1 Average effects

If there is no skill depreciation during an unemployment spell (or stigmatization of the long term unemployed), then the standard search model with stochastic wages predicts that the wage at the accepted job improves with the generosity of the unemployment insurance system and in particular with an increase potential UI durations. Only if wages are not stochastic, so individuals' only choice variable is search intensity, would the standard search model predict a zero wage effect.

On the other hand if there is skill depreciation, or stigmatization, the job quality after unemployment may decrease with longer potential benefit durations. Table 10 tests whether longer potential benefit durations increase post unemployment wages. There appears to be no effect of potential durations either on the post unemployment wage or on the wage change between relative to the previous wage. The graphical (Figure 6) analysis supports this conclusion. If anything there is a slightly negative effect of longer durations on post wages.

We have also analyzed the effect of increases in potential UI durations on a range of other outcomes, shown Table 10 and Figure 6. At most age-thresholds and for most variables we consider, the effect is small and insignificant. In particular, we do not find an effect on wages five years after the start of the UI spell, suggesting workers do not take initially lower paying jobs because of high growth potential. Similarly, it does not appear that workers experience wage increases over time. We also do not find any robust effects on the incidence of industry or occupation changes. More time to search does not lead workers to be more likely to find a job in their occupation or industry.

6.2 Changes in the dynamics of exits

The analysis so far focused on effects on the mean. However, the effect may be quite different for different parts of the distribution. For example it may be that a large fraction of unemployed individuals search very intensely for jobs and find jobs in a short period of time, like a month or two. The search behaviour of this group may not be affected by whether or not they can continue to receive unemployment benefits after the 12 month period. Similarly workers who have extremely high expected unemployment durations may not be affected very much.

Figure 7 provides a way of looking at how the distribution of unemployment durations changes at the age cut-off by providing nonparametric estimates of the survival functions just before and just after the cut-off using regression discontinuity estimates. RD estimates for the survival functions are created by estimating the following equation:

$$P(Dur \geq x)_{ia} = \beta_{0,x} + \beta_{1,x} D_{a \geq a^*} + f(a) + \varepsilon_{ai}$$

This equation is the same as the main RD estimation equation, except for the difference that in the regression the left hand side variable is a dummy for the duration being longer than *x* months, where we estimate this for $x=1,...,25$. Since $F(x) = P(Dur \geq x)$ is the survival function, the estimates for $\beta_{1,x}$ are estimates for the shift of the survival function at the discontinuity, while $\beta_{0,x}$ are estimates for the survival function just to the left of the cut-off (with the right normalization of the age variable).

Figure 7 (a) shows the results for duration of UI benefit receipt. The survival function for individuals eligible to 18 months of UI relative to individuals eligible to 12 months is already clearly shifted outward around 3-4 months after the beginning of UI. Thus unemployed individuals adjust their search behaviour a long time before running out of UI depending on whether they are eligible to longer durations. This clearly rejects the hypothesis that individuals are myopic and only affected at the point where they actually run out of UI benefits. Mechanically the survival functions drop almost to zero at the end of UI eligibility.[27]

The figure also reveals that about 28 percent exhaust their UI benefits in the 12 month eligibility group, while only about 20 percent in the 18 month eligibility group. The limits in UI are thus clearly binding for a large fraction of individuals in both groups.

Similarly, Figure 7 (b) shows that the probability of remaining in non-employment also increases along the entire duration distribution. It is therefore not the case that the majority of workers exit non-employment right when UI runs out and that this point is shifted outwards. Rather, when UI durations are increased workers' search behaviour is affected at all points in time, thus shifting out the distribution.

Figure 8 shows the corresponding hazard functions, also based on regression discontinuity estimates, separately for men and women. For men the hazard rate of leaving non-employment declines from about 0.12 in the first 4 months to less than 0.04. In addition, consistent with previous studies (e.g. Meyer 1990 and Card, Chetty, and Weber 2007) there are clear spikes in the hazard rate at the benefit exhaustion points for the two respective groups. The figure also confirms our finding that job finding rates for individuals with 18 months of UI eligibility are lower as early as 3 months after the beginning of the UI spell. This is reversed around month 15, when individuals under the more general UI regime have higher hazard rates. For women the shape of the hazard function is much flatter initially while of a similar level after about 6 months. The spikes in the hazard rate at the end of UI eligibility are nearly twice as big as for men, which is consistent with the larger mean duration effect that we found for women.

In Table 11 Panel A provides regression discontinuity estimates of the change in the escape hazard rates along the duration distribution for all age discontinuities. For all discontinuities, there is a clear decrease in the escape hazard for the higher UI eligibility group in the last month of UI for the lower UI eligibility group. This drop in the hazard is mirrored by an increase in the last UI month of the higher UI eligibility group. The table also shows that the hazard rate is lower for the higher UI eligibility group before UI runs out for the lower group at each cut-off. This clearly confirms the forward looking behaviour of the unemployed. Furthermore column (1) shows

[27] The reason that a few individuals have UI durations longer than the maximum may be due to interruptions (but not due to employment) of receiving UI benefits.

that the escape hazard over the first 12 months decreases at each of the discontinuities, even when potential UI durations increase from 22 to 26 months, the hazard rate over the first 12 months falls by 1.4 percent, suggesting that individuals are forward looking over quite a long time horizon.

6.3 Changes in the dynamics of wages

Figure 9 shows how wage losses vary with non-employment durations for individuals under different UI regimes. The figure shows the average wage loss in Euro of individuals accepting jobs conditional on the month (since the start of UI) in which they exit. The figure shows separate line for different age groups from our main RD estimations by UI eligibility. The timing is such that month 0 refers to individuals exiting within the first month of receiving UI. The vertical lines are in month 12, 18, 22, and 26, the maximum UI durations for the 4 groups. Individuals exiting in month 12 in the 40-41 age group are thus exiting within the 4 week period after they run out of UI benefits.

Two results are apparent from the figure. First, wages decline on average during a non-employment spell. The decline is similar across age-groups until month 20, after which older workers begin experiencing stronger reductions in wages. This decline in wages after a job loss is not matched by a corresponding decline of 'predicted' wages, i.e., wages predicted based on workers' gender, education, job, industry, and occupation tenure, and the prior wage. In other words, there is literally no corresponding change in observable worker heterogeneity with non-employment duration. Thus, the decline in average wages is unlikely to be driven by selection; instead, it is consistent with a decline in reservation wages as the non-employment spell progresses. Note that for reasonable parameter values the search model predicts reservation wages should start to decline towards the end of the UI spell, whereas here they are falling throughout.

The second noteworthy feature of the figure is that wages show a clear drop at the exhaustion point relevant for the respective age group. This pattern is predicted by a standard search model, as workers are eager to return to employment as UI runs out to regain eligibility for future UI use. A spike in job finding at benefit exhaustion is often found, and is sometimes called the 'entitlement' effect. The longer the potential duration of UI, the more attractive is a return to employment, the larger should be the spike - something clearly borne out in the figure. We also see a slight flattening of the wage gradient after the exhaustion point, as predicted by a search model. However, the wage decline continues, suggesting that something else is going on, such as human capital depreciation. Alternatively, the same effect through reservation wages is continuing since some workers might be on ALH.

Figure 9 may provide a biased estimate of the effect of potential UI durations on the accepted wage duration profile if there is an age gradient in the wage profile. For this reason Figure 10 (a) provides RD estimates of the change in the wage loss profile. This is created using the same method as for estimating the change in the survival functions, except that the left hand side variable in equation 2 is the log wage loss of workers exiting in a given month. By lining up the RD estimates for each exit month one receives an estimate for how this function changes at the discontinuity (and thus an estimate of the causal effect of UI on the shape of this function). The figure shows the variation for the age 42 cut-off, the cleanest of the discontinuities. The vertical bars between the lines indicate that the difference is statistically significant (5 percent level).

Figure 10 (a) suggests several key findings. First, wages for the treatment and control groups are very similar for workers exiting in the first 11 months of the non-employment spell. Below, we will see that on average wages tend to decline for these workers. Second, there is a clear drop in earnings for the control group at the exhaustion of benefits at 12 months. Third, there is a similarly sized drop in earnings for the control at the new exhaustion point. These patterns are robust to different ways of implementing the RD, measuring wages, or controlling for observables.

Table 11 Panel B and C provide regression discontinuity estimates of the effect of increased UI eligibility on wages during different months of exiting unemployment. The regressions confirm the result from the figure, that despite the clear decrease in the exit hazards over the first 12 months of non-employment at each increase in UI eligibility, there is no clear corresponding change in mean accepted wages: in Panel A Column (1) the age 49 coefficient is negative, but this is not significant in Panel B where the log wage change is the outcome variable. Similarly in the log wage change specification there is a positive effect at the age 44 cut-off, which is however not significant for the mean post wages in Panel B. Given this discrepancy across the two specifications, we interpret this as no evidence for systematic changes in the wage-non-employment duration relationship at those discontinuities. The wage dips at the benefit exhaustion point are still clearly visible for the age 42 cut-off for both specifications. There are no corresponding dips at the 44 and 49 cut-off, but this may just be due to statistical power, since the respective confidence intervals cover dips of the 42 cut-off magnitude.

As argued above, the standard search model with reservation wages can explain the dip and the shift in the dip as the exhaustion point changes. This dip is the counterpart to commonly observed increases in the job finding rate at that point. However, the model cannot explain why wages of UI recipients with longer durations are similar or even smaller than that of workers with shorter duration. In the basic model, the reservation wage path should increase with potential UI duration at all points of the unemployment spell until benefits run out. The fact that the reservation wage path does not rise, and perhaps even declines, with increased potential duration may indicate human capital depreciation or stigma effects are present. These effects should increase with spell duration, yet this is not borne out with the figure.

This confounds two factors: workers may change their reservation wage in each month since the start of UI, thus affecting the accepted wage path and the composition of workers may change due to the policy change. Thus a movement of the wage loss conditional on month of exit does not necessarily mean that for a specific worker the wage loss changed but instead could reflect that they are exiting in a different month. If the policy change increases durations by shifting individuals to longer durations and this shift is monotonically increasing with duration, then the rank ordering of individuals does not change. For example the individuals in the 5th percentile under both policy regimes would be comparable groups.

Under this strong assumption, one can estimate how wage losses affect subgroups of individuals at different intervals of non-employment duration. For Figure 10 (b) we break up all individuals below the age 42 cut-off into 20 quantile intervals (i.e. individuals in the 0th to 4th percentile, 5th to 9th, ...) of the non-employment duration distribution of these individuals. The individuals above the threshold are divided into the quantiles of their respective employment distribution. Under the assumption that the policy does not affect this rank ordering, the individuals in the same quantiles across age groups are comparable and we can investigate wage losses within each of these quantile groups. In Figure 10 (b) we show how log wage losses vary with duration quantiles using the same RD methodology as for Figure 10 (a).

Figure 10 (b) clarifies that there may be a decline in wages at lower non-employment durations. It also shows that if constrained workers retain their same rank, they are predicted to experience an increase in accepted wages. However, given that we observe a spike of similar magnitude at the new exhaustion point, it is likely that the rank assumption is violated.

Overall, we conclude that workers are clearly forward looking in the job search decisions, and that both reservation wages and search intensity are likely to play a role in the job search process. Our findings also suggest that the average wage effect hides complex dynamics occurring over the job spell. It appears that both search intensity and reservation wages are differentially affected across the non-employment spell. However, the patterns can only partly be explained by a basic model of on the job search.

7 Conclusion

In this paper, we have evaluated the impact of large changes in the duration of unemployment insurance (UI) on labour supply, the quality of job matches, and job search behaviour. We show that differences in eligibility thresholds by exact age give rise to a valid regression discontinuity design, which we implement using administrative data on the universe of new unemployment spells and career histories over twenty years from Germany. The German UI system gives rise to multiple age-thresholds that have changed over time, leading to multiple quasi-experiments. We use these to assess two unresolved hypotheses about UI. First, disincentive effects may be worse in recessions when the effective replacement rate rises for many workers due to a loss in skills. Second, it is still an open question whether in addition to subsidizing income UI allows workers to obtain better job matches.

The elasticities of labour supply with respect to the duration of UI benefits we find are modest to low. They are in the lower range of estimates from the U.S. (Krueger and Meyer 2002), 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 2007). This suggests that even large increases in UI durations - such as typically occurring in the U.S. as UI is extended for up to two years in larger recessions - do not have a different impact than the smaller increases typically analyzed in the existing literature.

We also find that labour supply elasticities are very robust over time and across groups of workers. In particular, 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, for workers in middle-age elasticities appear to decline somewhat with increases in the unemployment rate. Overall, our results indicate that extensions in UI during large recessions are unlikely to lead to a sizable or lasting increase in unemployment durations. Similarly, differences in the generosity of UI across countries are unlikely to explain a majority of large observed differences in the duration of unemployment spells.

While we find adverse effects on labour supply, we do not find effects on average outcomes of job search. Our regression discontinuity estimates of the effect of UI extensions on wages, wage growth, long-term employment outcomes, or the probability of switching industry or occupation are all zero. These results are consistent with an earlier literature and recent findings by Card, Chetty, and Weber (2007) that match effects of UI insurance appear to be small. This points to a world were search occurs through variation in search intensity and at constant wages. However, these findings represent a puzzle for the standard search model and a vast literature demonstrating that similar workers get paid different wages.

Our findings from the effect of UI expansions on the dynamics of employment and wages over the UI spell draw a more nuanced view of job search, and confirm some predictions of the search model with variable wages. The hazard of job finding begins to decline prior to benefit exhaustion, consistent with a decline in search intensity. Similarly, we find a weak decline in wages for this group. We also find an increase in job finding and a steep drop in wages close to the exhaustion point.

As duration is expanded, the spike in job finding rates and wages shifts to the new threshold. Whether the accepted wage actually increases for the workers constrained by the benefit exhaustion is doubtful, however, since we observe a similar spike again at the new exhaustion point. On average, for the majority of workers induced to stay on UI longer the wage effects are at best negative.

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Attachment

Table 1
Potential unemployment insurance benefit (ALG) durations as a function of age and months worked in previous 7 years

Months worked in previous 7 years	January 1983 - December 1984	January 1985 - December 1985	January 1986 - June 1987	July 1987 - March 1997	April 1997* - December 2004	January 2005 - December 2007
12	4	4	4	6	6	6
16	4	4	4	8	8	8
18	6	6	6	8	8	8
20	6	6	6	10	10	10
24	8	8	8	12	12	12
28	8	8	8	14 (≥42)	14 (≥45)	12
30	10	10	10	14 (≥42)	14 (≥45)	15 (≥55)
32	10	10	10	16 (≥42)	16 (≥45)	15 (≥55)
36	12	12	12	18 (≥42)	18 (≥45)	18 (≥55)
40	12	12	12	20 (≥44)	20 (≥47)	18 (≥55)
42	12	14 (≥49)	14 (≥44)	20 (≥44)	20 (≥47)	18 (≥55)
44	12	14 (≥49)	14 (≥44)	22 (≥44)	22 (≥47)	18 (≥55)
48	12	16 (≥49)	16 (≥44)	24 (≥49)	24 (≥52)	18 (≥55)
52	12	16 (≥49)	16 (≥44)	26 (≥49)	26 (≥52)	18 (≥55)
54	12	18 (≥49)	18 (≥49)	26 (≥49)	26 (≥52)	18 (≥55)
56	12	18 (≥49)	18 (≥49)	28 (≥54)	28 (≥57)	18 (≥55)
60	12	18 (≥49)	20 (≥49)	30 (≥54)	30 (≥57)	18 (≥55)
64	12	18 (≥49)	20 (≥49)	32 (≥54)	32 (≥57)	18 (≥55)
66	12	18 (≥49)	22 (≥54)	32 (≥54)	32 (≥57)	18 (≥55)
72	12	18 (≥49)	24 (≥54)	32 (≥54)	32 (≥57)	18 (≥55)
Replacement rates on gross wages in percent:						
ALG (children)	68	68	68	67‡	67	67
ALG (no children)	63†	63	63	60‡	60	60
ALH (children)	58	58	58	57‡	57	57
ALH (no children)	53†	53	53	50‡	50	50

* The reform in 1997 was phased in gradually: For workers who had worked for more than one year during the three years before April 1997, the old rules applied until March 1999 (see Arntz, Simon Lo, and Wilke 2007).

† ALG and ALH replacement rates were lowered starting in January 1984. Until December 1983, ALG was 68 percent and ALH 58 percent of the previous gross wage, irrespective of whether the recipient had children.

‡ ALG and ALH were lowered starting in January of 1994.

Source: Hunt (1995) and Bundesgesetzblatt (1983-2008).

Table 2
Means and standard deviations of main variables

	(1) All UI spells 1975 - 2004	(2) All UI spells July 1987 - March 1999	(3) As column (2) but only spells with max UI dur*	(4) As column (3) but only age 40 to 44
Panel A: UI Variables				
Maximum ALG Duration (imputed)			17.1 [5.8]	15.0 [3.0]
Duration of ALG receipt in months	5.9 [5.6]	7.2 [6.7]	9.1 [8.1]	8.0 [6.3]
Non-employment duration in months	13.3 [24.9]	17.2 [26.4]	19.1 [29.6]	19.0 [28.8]
Time from end of job until start of ALG	1.5 [7.1]	1.51 [7.0]	1.8 [5.6]	1.6 [5.4]
Daily Wage Post Unemployment Wage in Euro	52.4 [28.0]	52.9 [26.8]	66.3 [31.5]	67.4 [32.2]
Post Wage - Pre Wage in Euro	-4.7 [26.3]	-4.2 [26.8]	-13.5 [28.8]	-13.9 [28.6]
Log(Post Wage) - Log(Pre Wage)	-0.15 [0.74]	-0.11 [0.58]	-0.27 [0.63]	-0.26 [0.60]
Switch Industry after Unemployment	0.61 [0.49]	0.65 [0.48]	0.75 [0.44]	0.76 [0.43]
Switch Occupation after Unemployment	0.55 [0.50]	0.59 [0.49]	0.65 [0.48]	0.66 [0.47]
Post Unemp. Spell: Is in fulltime Employment	0.68 [0.47]	0.73 [0.45]	0.71 [0.45]	0.75 [0.44]
Post Unemp. Spell: Has any Employment	0.88 [0.33]	0.90 [0.30]	0.83 [0.38]	0.86 [0.35]
Panel B: Demographic Variables				
Last Wage prior to UE	58.2 [29.2]	59.7 [29.6]	80.8 [29.7]	82.4 [30.6]
Education years	10.8 [2.2]	10.9 [2.3]	11.0 [2.3]	11.1 [2.4]
Fraction female	0.40 [0.49]	0.45 [0.50]	0.35 [0.48]	0.35 [0.48]
Fraction non-german	0.19 [0.39]	0.078 [0.27]	0.12 [0.32]	0.13 [0.34]
Pre Unemployment experience in years	7.1 [5.8]	6.3 [5.6]	13.3 [4.1]	14.1 [4.0]
Pre Unemp occupation tenure in years	3.7 [4.7]	3.4 [4.5]	9.2 [5.3]	9.6 [5.5]
Pre Unemp industry tenure in years	2.7 [3.87]	3.1 [4.4]	9.0 [5.5]	9.3 [5.7]
Employment size of previous establishment	678 [1950]	821 [2070]	1049 [3078]	897 [2686]
Number of Spells	36784166	11544815	974360	156565

Notes: Mean of main variables, standard deviations in brackets. Wages are in prices of 2000.

* Individuals who worked for 52 months in the previous 7 years without intermittent spell of receiving UI benefits.

Source: Authors' own calculations based on BLH.

Table 3
Regression discontinuity estimates of ALG duration on months of ALG receipt and non-employment

	(1)	(2)	(3)	(4)
	Age bandwidth around age discontinuity			
	2 years	**1 year**	**0.5 years**	**0.2 years**
Panel A: Dependent variable: Duration of ALG receipt				
D(age>=42)	1.73 [0.043]**	1.79 [0.061]**	1.68 [0.081]**	1.64 [0.12]**
Elasticity	0.58	0.60	0.57	0.55
Observations	311252	155306	77333	31215
D(age>=44)	1.02 [0.053]**	1.11 [0.074]**	1.04 [0.10]**	1.29 [0.17]**
Elasticity	0.55	0.60	0.56	0.70
Observations	310140	155253	77626	31106
D(age>=49)	1.37 [0.087]**	1.40 [0.099]**	1.45 [0.14]**	1.74 [0.20]**
Elasticity	0.69	0.71	0.74	0.89
Observations	220837	144977	72665	29121
Panel B: Dependent variable: Non-employment duration				
D(age>=42)	0.83 [0.10]**	0.98 [0.15]**	1.08 [0.20]**	0.86 [0.32]**
Elasticity	0.14	0.16	0.18	0.15
Observations	311252	155306	77333	31215
D(age>=44)	0.45 [0.10]**	0.63 [0.14]**	0.62 [0.21]**	0.95 [0.35]**
Elasticity	0.14	0.20	0.20	0.30
Observations	310140	155253	77626	31106
D(age>=49)	0.40 [0.14]**	0.53 [0.16]**	0.62 [0.22]**	0.95 [0.35]**
Elasticity	0.13	0.18	0.21	0.32
Observations	220837	144977	72665	29121

Notes: Each coefficient from separate RD regressions. Local linear regressions with different slopes on each side of cut-off. Standard errors clustered (in parentheses) 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: (RD Coefficient / Change Potential Durations) × (Average Pot. Dur. Around cut-off / Average Aa Dur around Cut-off).

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 cut-off 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.

Source: Authors’ own calculations based on BLH.

Table 4
Regression discontinuity estimates of effect of potential ALG duration on employment outcomes

	(1) ALG duration	(2) Non-Emp duration	(3) Time until claim	(4) Ever emp. again	(5) Emp. 5 years later	(6) UI 5 years later
Panel A: All observations						
D(age>=42)	1.73 [0.046]**	0.83 [0.10]**	-0.031 [0.044]	-0.0058 [0.0023]*	-0.0017 [0.0035]	0.0056 [0.0025]*
Observations	311252	311252	311252	311252	311252	311252
D(age>=44)	1.02 [0.053]**	0.45 [0.10]**	0.021 [0.040]	-0.0039 [0.0024]	-0.0062 [0.0035]	0.0059 [0.0027]*
Observations	310140	310140	310140	310140	310140	310140
D(age>=49)	1.37 [0.087]**	0.40 [0.14]**	0.030 [0.046]	-0.0049 [0.0039]	-0.0018 [0.0047]	0.0047 [0.0036]
Observations	220837	220837	220837	220837	220837	220837
Panel B: Excluding observations within 1 month of discontinuity						
D(age>=42)	1.75 [0.047]**	0.83 [0.11]**	-0.036 [0.047]	-0.0063 [0.0025]*	-0.0032 [0.0038]	0.0055 [0.0027]*
Observations	298317	298317	298317	298317	298317	298317
D(age>=44)	0.99 [0.058]**	0.38 [0.11]**	0.024 [0.044]	-0.0036 [0.0027]	-0.0030 [0.0037]	0.0040 [0.0029]
Observations	297196	297196	297196	297196	297196	297196
D(age>=49)	1.30 [0.11]**	0.33 [0.16]*	0.033 [0.055]	-0.0062 [0.0047]	-0.0032 [0.0055]	0.0072 [0.0042]
Observations	208594	208594	208594	208594	208594	208594

Notes: Coefficients from RD regressions. Local linear regressions (different slopes) on each side of cut-off. Standard errors 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.

Source: Authors' own calculations based on BLH.

Table 5
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) Experience Last Job	(6) Occ Tenure Last Job	(7) Ind Tenure Last Job	(8) Pre Wage
Panel A: All observations								
D(age>=42)	0.037	0.0061	0.0040	0.014	0.0062	0.035	0.059	0.37
	[0.018]*	[0.0034]	[0.0023]	[0.039]	[0.037]	[0.038]	[0.040]	[0.23]
Observations	311252	311252	311252	311252	311252	311252	311252	303446
D(age>=44)	-0.012	0.0028	0.0014	0.053	0.011	0.025	0.061	-0.024
	[0.017]	[0.0034]	[0.0025]	[0.041]	[0.041]	[0.041]	[0.044]	[0.23]
Observations	310140	310140	310140	310140	310140	310140	310140	302272
D(age>=49)	0.021	0.016	0.0013	0.013	-0.034	-0.029	0.032	0.051
	[0.019]	[0.0046]*	[0.0035]	[0.058]	[0.051]	[0.056]	[0.054]	[0.29]
Observations	220837	220837	220837	220837	220837	220837	220837	214311
Panel B: Excluding observations within 1 month of discontinuity								
D(age>=42)	0.024	0.0055	0.0037	0.020	0.036	0.052	0.061	0.20
	[0.020]	[0.0037]	[0.0026]	[0.042]	[0.041]	[0.042]	[0.043]	[0.24]
Observations	298317	298317	298317	298317	298317	298317	298317	290832
D(age>=44)	-0.020	0.0021	0.0014	0.053	0.0030	0.014	0.065	-0.21
	[0.018]	[0.0037]	[0.0027]	[0.045]	[0.044]	[0.045]	[0.047]	[0.24]
Observations	297196	297196	297196	297196	297196	297196	297196	289629
D(age>=49)	0.010	0.012	0.0012	-0.018	-0.056	-0.066	-0.0013	-0.28
	[0.021]	[0.0050]*	[0.0040]	[0.067]	[0.060]	[0.063]	[0.062]	[0.35]
Observations	208594	208594	208594	208594	208594	208594	208594	202443

Notes: Coefficients from RD regressions. Local linear regressions (different slopes) on each side of cut-off. Standard errors 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.

Source: Authors' own calculations based on BLH.

Table 6
Regression discontinuity estimates of effect of potential ALG – post 1997 reform

	(1) ALG Duration	(2) Non-Emp Duration	(3) Time until Claim	(4) Ever emp. again	(5) Wage Loss	(6) Post Wage	(7) Pre Wage	(8) Emp. 5 years later	(9) UI 5 years later
Panel A: All observations									
D(age>=45)	1.66	0.53	-0.16	-0.0030	-0.93	-0.61	0.32	-0.0024	-0.0016
	[0.047]**	[0.11]**	[0.064]*	[0.0029]	[0.29]**	[0.35]	[0.26]	[0.0038]	[0.0017]
Elasticity	0.58	0.095	-0.25	-0.0088	0.14	-0.028	0.011	-0.019	-0.094
Observations	239394	239394	239394	239394	163322	166115	234218	239394	239394
D(age>=47)	0.94	0.26	-0.017	0.0018	-0.40	-0.65	0.028	0.0032	-0.00035
	[0.061]**	[0.11]*	[0.068]	[0.0031]	[0.31]	[0.35]	[0.28]	[0.0038]	[0.0018]
Elasticity	0.53	0.086	-0.053	0.011	0.11	-0.061	0.0019	0.050	-0.037
Observations	231477	231477	231477	231477	152752	155338	226314	231477	231477
Panel B: Excluding observations within 1 month of discontinuity									
D(age>=45)	1.66	0.46	-0.18	-0.00097	-0.93	-0.70	0.15	-0.00019	-0.0015
	[0.052]**	[0.12]**	[0.067]**	[0.0032]	[0.32]**	[0.38]	[0.29]	[0.0041]	[0.0018]
Elasticity	0.58	0.083	-0.27	-0.0029	0.14	-0.032	0.0050	-0.0015	-0.089
Observations	229371	229371	229371	229371	156450	159112	224419	229371	229371
D(age>=47)	0.88	0.21	-0.047	0.0020	-0.35	-0.69	-0.10	0.0041	0.00014
	[0.067]**	[0.13]	[0.073]	[0.0035]	[0.34]	[0.39]	[0.30]	[0.0041]	[0.0019]
Elasticity	0.50	0.068	-0.15	0.012	0.097	-0.065	-0.0070	0.066	0.016
Observations	221798	221798	221798	221798	146379	148860	216839	221798	221798

Notes: Coefficients from RD regressions. Local linear regressions (different slopes) on each side of cut-off. Standard errors clustered on day level (* P<.05, ** P<.01).

†The 1997 reform only came in to full effect for the workers in the high experience sample in April of 1999. The sample for this table consists therefore of individuals starting unemployment spells between April 1999 and December 2004, who had worked for 52 months in the last 7 years without intermittent UI spell.

Source: Authors' own calculations based on BLH.

Table 7
Regression discontinuity estimates of effect of potential UI durations by gender

	(1) ALG Duration	(2) Non-Emp Duration	(3) Time until Claim	(4) Ever emp. again	(5) Wage Loss	(6) Post Wage	(7) Pre Wage	(8) Emp. 5 years later	(9) UI 5 years later
Panel A: Men Only									
D(age>=42)	1.53	0.72	0.052	-0.0070	-0.31	-0.13	0.34	0.00018	0.0050
	[0.056]**	[0.13]**	[0.059]	[0.0031]*	[0.29]	[0.33]	[0.28]	[0.0046]	[0.0035]
Elasticity	0.55	0.13	0.084	-0.020	0.062	-0.0045	0.010	0.000830	0.079
Observations	194804	194804	194804	194804	164805	168350	190071	194804	194804
D(age>=44)	0.84	0.36	0.074	-0.0042	-0.032	-0.44	-0.44	0.00050	0.0019
	[0.0707]**	[0.14]**	[0.049]	[0.0033]	[0.29]	[0.33]	[0.29]	[0.00480]	[0.0038]
Elasticity	0.45	0.12	0.26	-0.024	0.013	-0.031	-0.026	0.00470	0.056
Observations	190696	190696	190696	190696	158606	161993	185900	190696	190696
D(age>=49)	1.05	0.19	0.0058	-0.0051	-0.21	-0.34	-0.0065	-0.0054	0.011
	[0.13]**	[0.19]	[0.073]	[0.0051]	[0.43]	[0.47]	[0.39]	[0.0063]	[0.0052]*
Elasticity	0.59	0.065	0.022	-0.038	0.082	-0.030	-0.00045	-0.070	0.32
Observations	131852	131852	131852	131852	102437	104815	127882	131852	131852
Panel B: Women Only									
D(age>=42)	2.13	0.98	-0.20	-0.0048	-0.58	-0.37	0.32	-0.010	0.0071
	[0.080]**	[0.18]**	[0.096]*	[0.0042]	[0.38]	[0.40]	[0.38]	[0.0065]	[0.0044]
Elasticity	0.62	0.15	-0.31	-0.014	0.11	-0.019	0.013	-0.043	0.16
Observations	103513	103513	103513	103513	88024	90044	100761	103513	103513
D(age>=44)	1.26	0.39	-0.065	-0.0024	0.12	0.54	0.38	-0.0094	0.0080
	[0.099]**	[0.17]*	[0.090]	[0.0046]	[0.37]	[0.36]	[0.37]	[0.0064]	[0.0044]
Elasticity	0.58	0.11	-0.22	-0.014	-0.042	0.058	0.030	-0.083	0.32
Observations	106500	106500	106500	106500	88565	90520	103729	106500	106500
D(age>=49)	1.62	0.46	0.087	-0.0077	-0.96	-0.84	0.11	0.00062	0.0026
	[0.18]**	[0.26]	[0.091]	[0.0079]	[0.52]	[0.50]	[0.46]	[0.0093]	[0.0070]
Elasticity	0.70	0.13	0.39	-0.059	0.33	-0.12	0.011	0.0080	0.103
Observations	76742	76742	76742	76742	58844	60142	74561	76742	76742

Notes: Coefficients from RD regressions. Local linear regressions (different slopes) on each side of cut-off. Standard errors 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. Observations within 1 month of discontinuity are excluded.

Source: Authors' own calculations based on BLH.

Table 8
The correlation of labour supply elasticities from regression discontinuity estimates with the economic environment

	(1) Duration Elasticity	(2) Duration Elasticity	(3) Duration Elasticity	(4) Duration Elasticity
The effect of labour market conditions on duration elasticity				
Unemployment Rate†	-0.00096 [0.013]			
Change in Unemployment Rate†		0.061 [1.96]		
Plant Closing Rate†			-0.12 [0.097]	
Mass Layoff Rate†				-0.050 [0.031]
Constant	0.12 [0.016]**	0.12 [0.016]**	0.12 [0.016]**	0.12 [0.015]**
Root MSE	0.11	0.11	0.11	0.10
Observations	47	47	47	47
The effect of turbulence on duration elasticity (within years)				
1st Quintile of Log Wage Losses (Ind-Year cell) - highest losses	-0.013 [0.052]		-0.014 [0.052]	
2nd Quintile of Log Wage Losses (Ind-Year cell)	0.0049 [0.053]		0.0040 [0.052]	
3rd Quintile of Log Wage Losses (Ind-Year cell)	0.034 [0.053]		0.034 [0.052]	
4th Quintile of Log Wage Losses (Ind-Year cell)	0.013 [0.052]		0.012 [0.052]	
5th Quintile of Log Wage Losses (Ind-Year cell) - lowest losses	Omitted Category		Omitted Category	
Average Log Wage Loss in Year-Quantile Cell†		0.20 [0.15]		0.090 [0.17]
Change in Unemployment Rate†	0.11 [2.16]	-0.24 [2.16]		
Unemployment Rate†	-0.0062 [0.014]	-0.0010 [0.014]		
Controlling for Year Fixed Effects	No	No	Yes	Yes
Constant	0.11 [0.037]**	0.12 [0.017]**	0.12 [0.095]	0.12 [0.089]
Root MSE	0.25	0.25	0.25	0.25
Observations	235	235	235	235

Notes: * P<.05, ** P<.01, Standard errors in parentheses. Regressions of estimated labour supply elasticities (one elasticity for each year and age discontinuity - 42, 44, and 49 for the earlier period and 45 and 47 for the later period). Regressions weighted by Precision of Elasticity Estimate (Inverse of Standard error of elasticity).

† Variable is demeaned, so that the constant in the regression is an estimate of the mean elasticity (top panel) or of the elasticity in the omitted group (bottom panel, columns 1 and 3).

Source: Authors' own calculations based on BLH.

Table 9
Regression discontinuity estimates of effect of potential UI durations on non-employment durations by subgroups

	(1) D(age>=42)	(2) D(age>=44)	(3) D(age>=49)
Declining unemployment (1987-1991)			
Marginal Effect in Months	1.29 [0.26]**	0.50 [0.26]	0.23 [0.33]
Elasticity	0.22	0.17	0.081
Observations	57893	55610	49096
Rising unemployment (1992-1997)			
Marginal Effect in Months	0.69 [0.14]**	0.28 [0.14]*	0.48 [0.20]*
Elasticity	0.11	0.084	0.15
Observations	185491	187186	121482
Tenure <= 5 years			
Marginal Effect in Months	0.65 [0.15]**	0.25 [0.16]	0.085 [0.23]
Elasticity	0.12	0.085	0.031
Observations	153173	148791	93289
Tenure > 5 years			
Marginal Effect in Months	1.01 [0.17]**	0.50 [0.16]**	0.56 [0.21]**
Elasticity	0.16	0.15	0.17
Observations	145144	148405	115305

Notes: Coefficients from RD regressions. Local linear regressions (different slopes) on each side of cut-off. Standard errors 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. Observations within 1 month of discontinuity are excluded.

Source: Authors' own calculations based on BLH.

Table 10
Regression discontinuity estimates of effect of potential UI durations on post unemployment match quality

	(1) Pre Wage	(2) Post Wage	(3) Log Post Wage	(4) Wage Loss	(5) Log Wage Loss	(6) Log Wage 5 years later	(7) Log Wage Growth 5 years	(8) Switch Ind.	(9) Switch Occ.
Panel A: All observations									
D(age>=42)	0.37	-0.22	-0.0074	-0.54	-0.013	-0.0059	-0.0057	0.0061	0.011
	[0.23]	[0.24]	[0.0055]	[0.21]**	[0.0051]**	[0.0063]	[0.0053]	[0.0037]	[0.0040]**
Observations	303446	269650	269650	263824	263824	175263	175093	264438	273476
D(age>=44)	-0.024	0.0053	-0.00031	-0.042	-0.0011	-0.0040	-0.012	0.0038	0.0051
	[0.22]	[0.24]	[0.0057]	[0.21]	[0.0053]	[0.0066]	[0.0052]*	[0.0036]	[0.0038]
Observations	302272	263538	263538	257982	257982	168274	168114	258186	267392
D(age>=49)	0.051	-0.96	-0.020	-0.83	-0.019	-0.015	0.013	0.0090	0.0058
	[0.29]	[0.32]**	[0.0083]*	[0.30]**	[0.0078]*	[0.010]	[0.0076]	[0.0048]	[0.0050]
Observations	214311	174522	174522	170611	170611	105651	105544	170743	177174
Panel B: Excluding observations within 1 month of discontinuity									
D(age>=42)	0.20	-0.35	-0.0093	-0.41	-0.010	-0.0052	-0.0051	0.0048	0.0079
	[0.24]	[0.27]	[0.0061]	[0.23]	[0.0055]	[0.0069]	[0.0057]	[0.0039]	[0.0042]
Observations	290832	258394	258394	252829	252829	167936	167775	253429	262067
D(age>=44)	-0.21	-0.12	-0.0019	0.019	-0.00096	-0.011	-0.013	0.0042	0.0042
	[0.24]	[0.27]	[0.0063]	[0.23]	[0.0057]	[0.0071]	[0.0056]*	[0.0040]	[0.0042]
Observations	289629	252513	252513	247171	247171	161243	161094	247392	256196
D(age>=49)	-0.28	-0.83	-0.017	-0.50	-0.012	-0.014	0.014	0.0043	0.0037
	[0.35]	[0.39]*	[0.0099]	[0.35]	[0.0091]	[0.012]	[0.0092]	[0.0057]	[0.0058]
Observations	202443	164957	164957	161281	161281	99878	99773	161378	167458

Notes: Coefficients from RD regressions. Local linear regressions (different slopes) on each side of cut-off. Standard errors 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. Observations within 1 month of discontinuity are excluded.

Source: Authors' own calculations based on BLH.

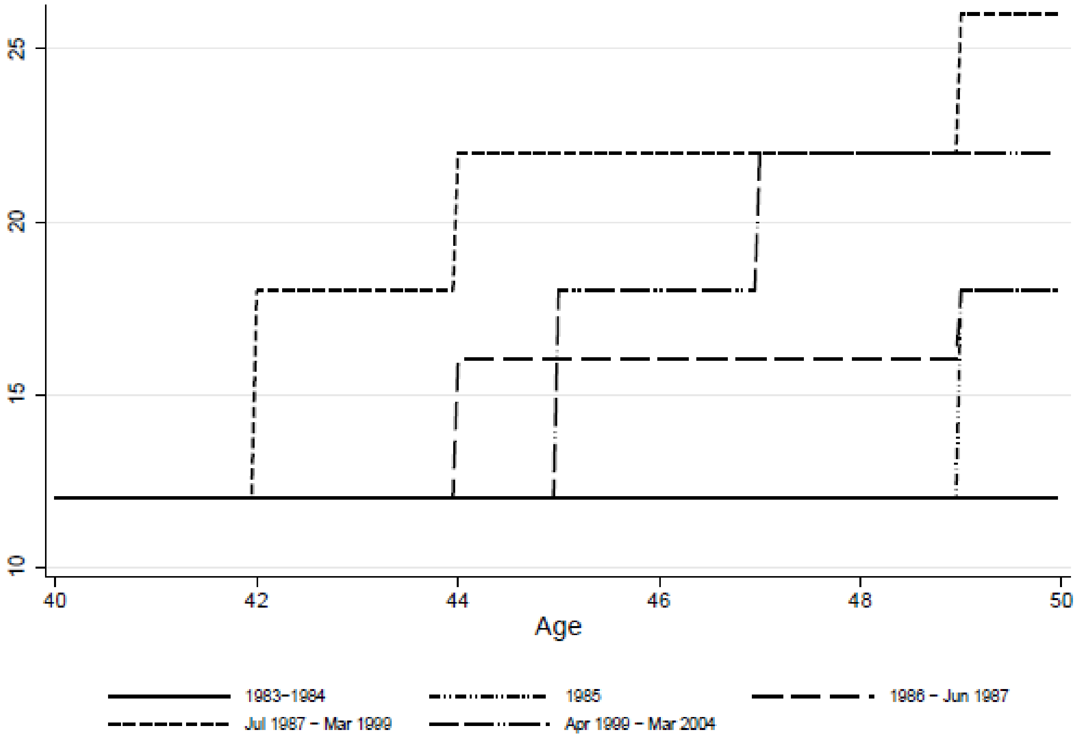
Table 11
Regression discontinuity estimates of potential UI durations on escape hazards and accepted wages during different periods of the unemployment spell

	(1) Month 0-11	(2) Month 12	(3) Month 13-17	(4) Month 18	(5) Month 19-21	(6) Month 22	(7) Month 23-36
Panel A: Exit Hazards							
D(age>=42)	-0.031	-0.024	-0.0054	0.013	0.0035	0.0041	0.0050
	[0.0037]**	[0.0025]**	[0.0041]	[0.0023]**	[0.0034]	[0.0021]	[0.0052]
Observations	311252	140609	132866	110560	106235	97280	94763
D(age>=44)	-0.012	-0.0020	-0.014	-0.013	-0.0060	0.013	0.0054
	[0.0036]**	[0.0020]	[0.0040]**	[0.0022]**	[0.0032]	[0.0022]**	[0.0054]
Observations	310140	148755	142709	120474	115964	106077	102679
D(age>=49)	-0.014	0.0037	0.0048	-0.0043	-0.0100	-0.015	0.015
	[0.0046]**	[0.0022]	[0.0042]	[0.0020]*	[0.0029]**	[0.0023]**	[0.0061]*
Observations	220837	118696	114844	100241	97611	90648	87853
Panel B: Post Unemployment Wage in Euro							
D(age>=42)	0.24	4.68	1.82	-4.41	0.17	-2.46	0.021
	[0.28]	[1.47]**	[0.81]*	[2.03]*	[1.34]	[2.37]	[0.86]
Observations	170567	7872	22321	4412	8994	2556	22111
D(age>=44)	0.42	-0.65	0.50	1.92	-1.29	-2.22	1.05
	[0.27]	[1.58]	[0.82]	[1.78]	[1.19]	[1.96]	[0.76]
Observations	161261	6144	22252	4588	9939	3438	24127
D(age>=49)	-0.80	0.75	-1.54	3.15	-2.31	-0.084	-0.91
	[0.36]*	[2.13]	[1.08]	[2.65]	[1.52]	[2.60]	[0.97]
Observations	101997	3908	14598	2667	7008	2831	18896
Panel C: Log Wage Loss							
D(age>=42)	-0.00053	0.061	0.024	-0.094	0.0096	0.027	0.032
	[0.0044]	[0.028]*	[0.017]	[0.040]*	[0.029]	[0.057]	[0.022]
Observations	167870	7721	21788	4295	8758	2452	21388
D(age>=44)	0.0091	0.046	0.018	0.045	-0.037	-0.021	0.033
	[0.0046]*	[0.033]	[0.017]	[0.042]	[0.027]	[0.049]	[0.021]
Observations	158784	6031	21740	4463	9687	3339	23378
D(age>=49)	-0.0042	0.0026	-0.028	0.036	-0.065	0.036	-0.041
	[0.0064]	[0.045]	[0.025]	[0.062]	[0.038]	[0.070]	[0.028]
Observations	100322	3833	14287	2599	6825	2752	18318

Notes: Coefficients from RD regressions. Local linear regressions (different slopes) on each side of cut-off. Standard errors clustered on day level (* P<.05, ** P<.01). For sample definition see Table 2.

Source: Authors' own calculations based on BLH.

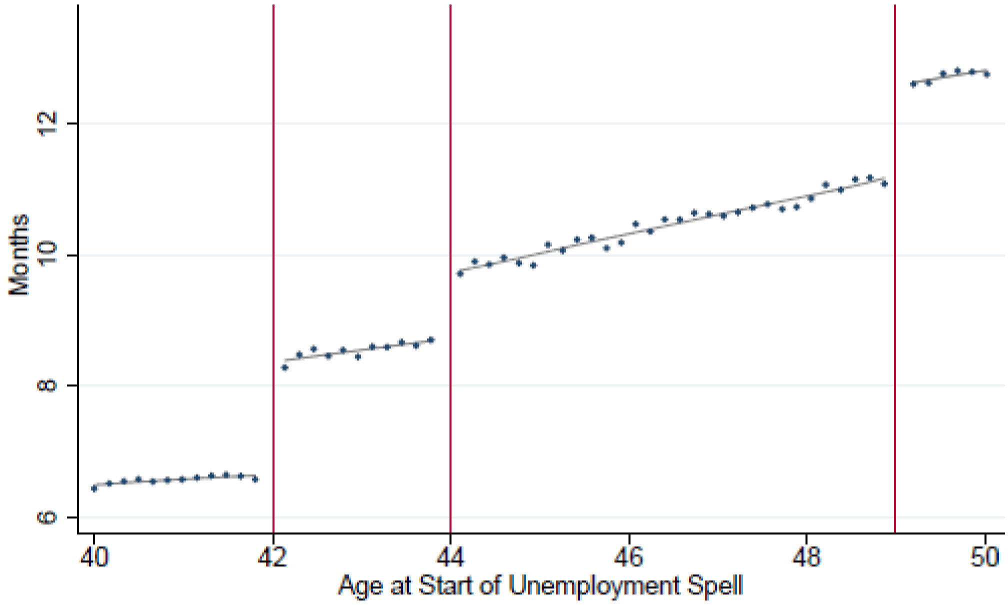
Figure 1
Potential UI durations by period (Workers in highest experience category)



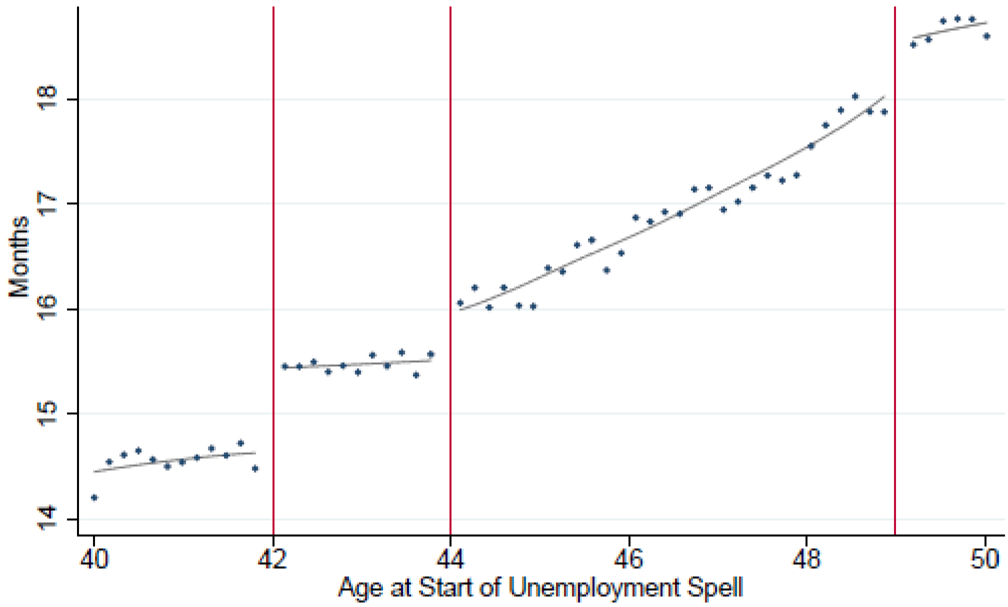
Note: The figure shows how potential unemployment insurance (UI) durations for workers in the highest experience group vary with age and over time. For details on the required experience to be eligible for the maximum durations see Table 1.

Source: Authors' own calculations based on BLH.

Figure 2
Actual unemployment insurance benefit (ALG) durations and non-employment durations by age – period 1987 to 1999



(a) Actual UI Duration

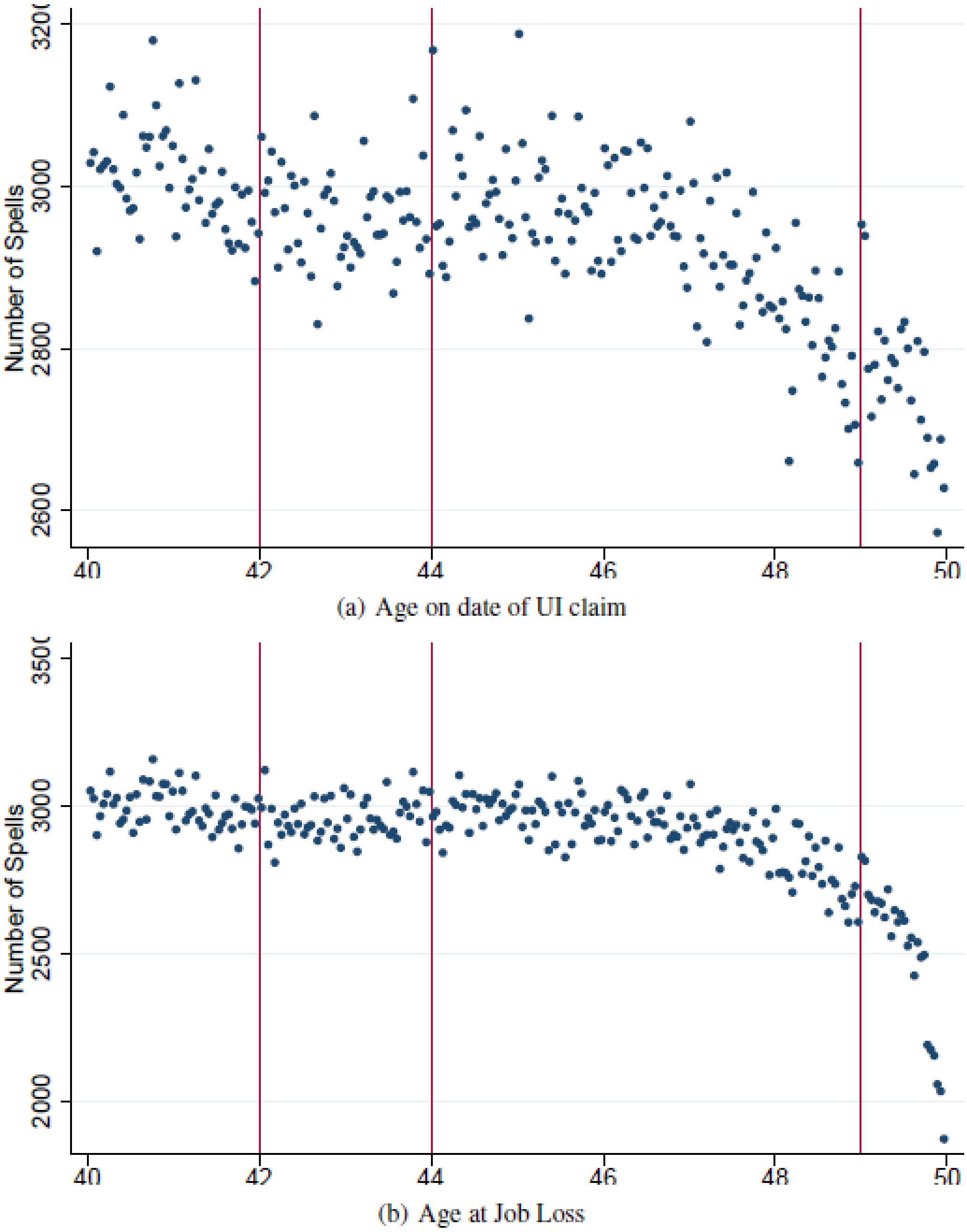


(b) Non-employment Duration

Note: 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 cut-offs 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 consists of 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).

Source: Authors' own calculations based on BLH.

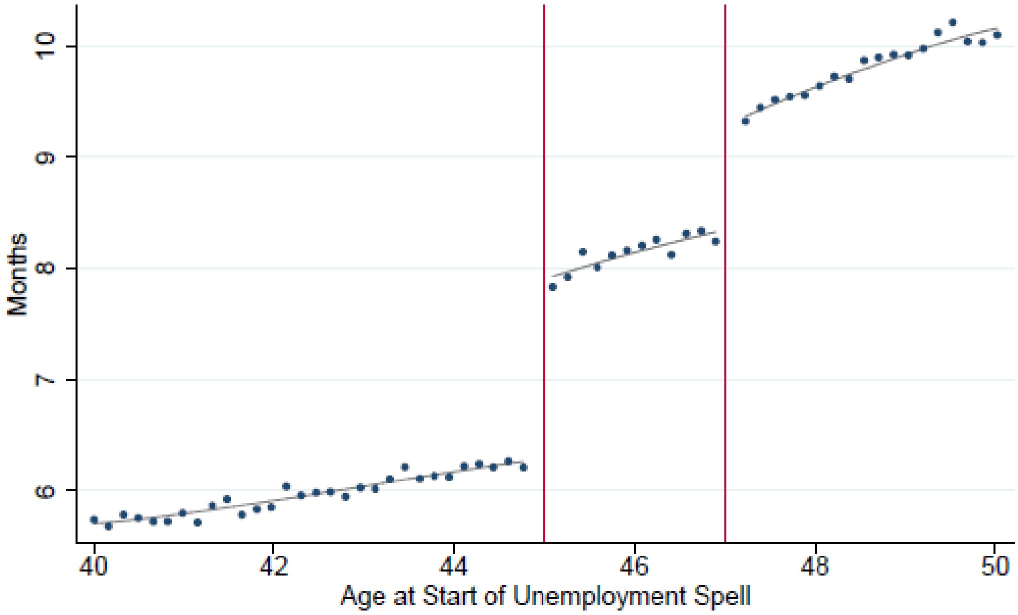
Figure 3
Density around age cut-offs for potential UI durations – period 1987 to 1999



Note: 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 figures shows the density by age at the end of the last job before the UI spell. The vertical lines mark age cut-offs 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 consist of 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 – April 1999.

Source: Authors' own calculations based on BLH.

Figure 4
Actual unemployment insurance benefit (ALG) durations and non-employment durations by age – post 1997 reform



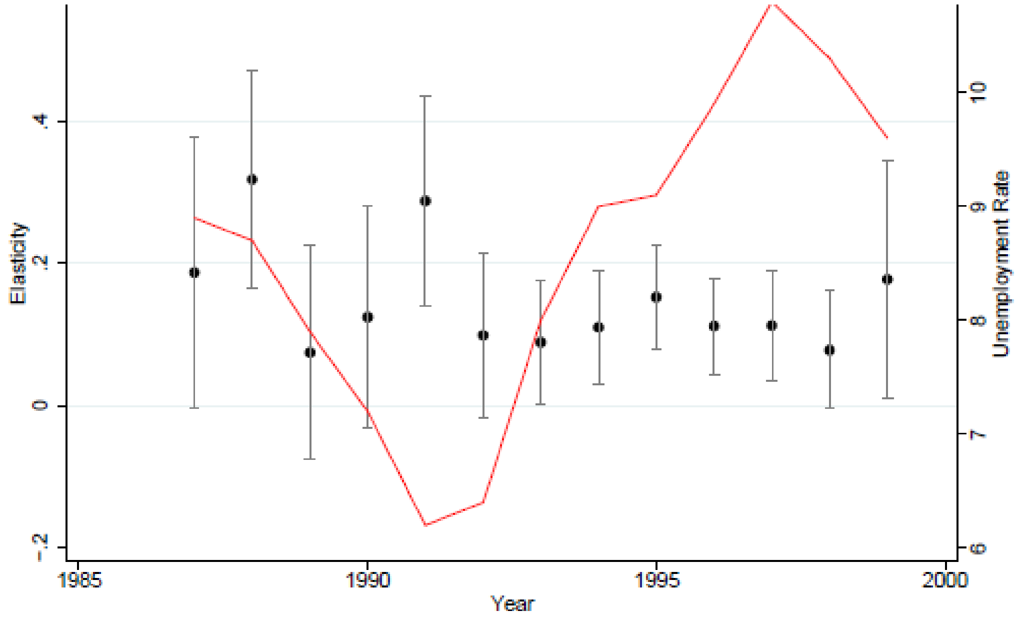
(a) Actual UI Durations

(b) Non-employment Durations

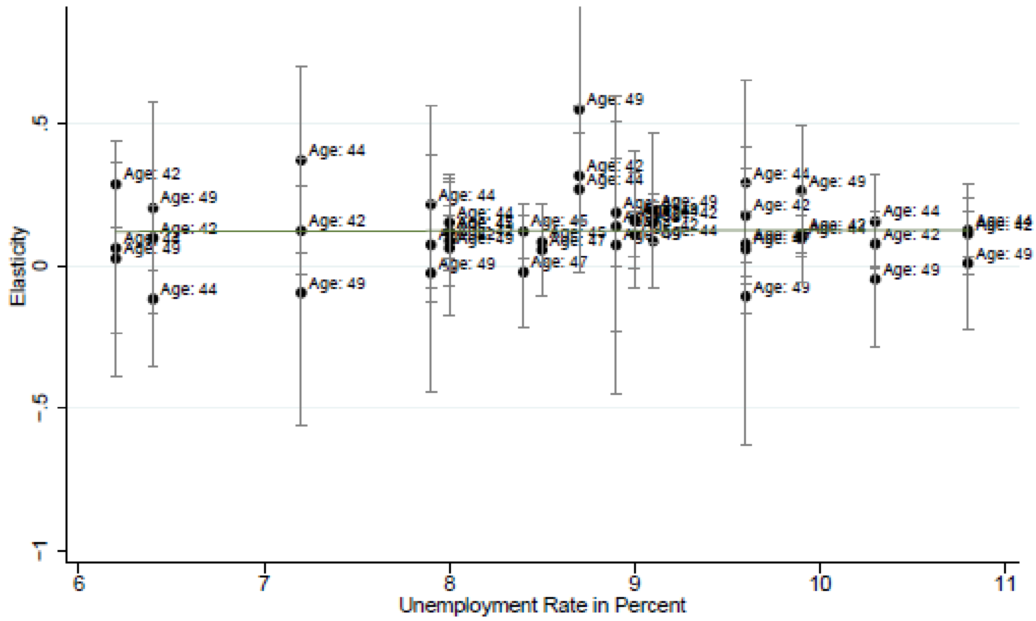
Note: The top figure shows average durations of receiving UI benefits by age at the start of receiving unemployment insurance. The bottom figures shows average non-employment durations for these workers, where non-employment duration is measured as the time until return to a job and is capped at 36 months. Each dot corresponds to an average over 120 days. The vertical lines mark age cut-offs for increases in potential UI durations at age 45 (12 to 18 months) and 47 (18 to 22 months). The sample consist of 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).

Source: Authors' own calculations based on BLH.

Figure 5
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 Discontinuity by Year and the Unemployment Rate

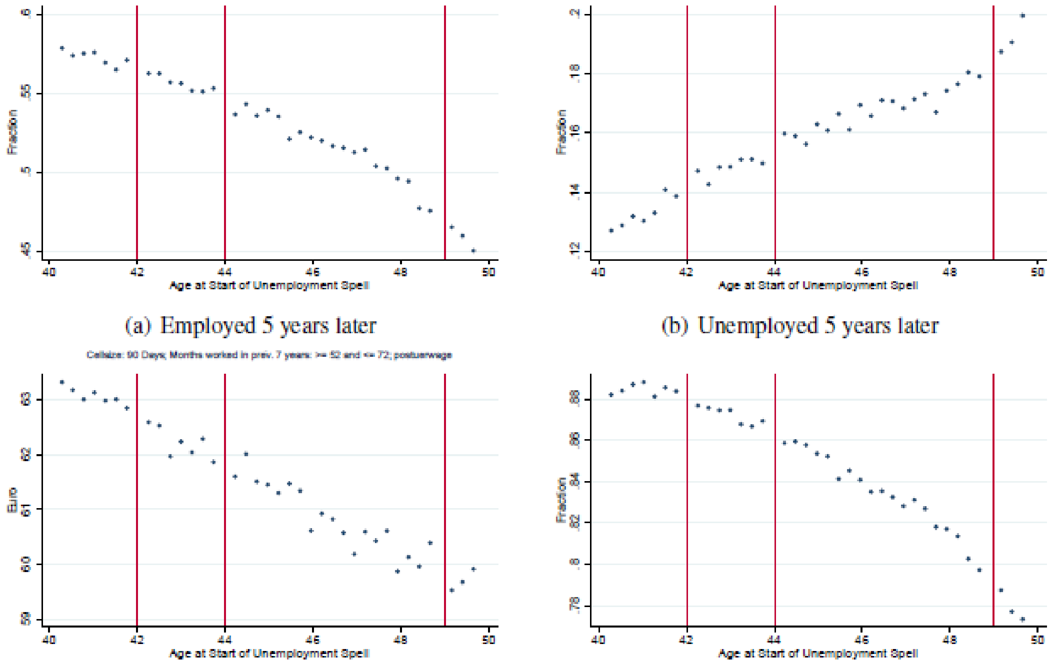


(b) Scatter Plot all estimated Elasticities vs. Unemployment Rate

Note: Each dot in the bottom figure corresponds to a non-unemployment duration elasticity estimated at an age cut-off in one year between 1987 and 2004 at any of the available cut-offs (42, 44, 45, 47, and 49). The horizontal line in the bottom figure is the regression line from the regression of elasticity's on the employment rate.

Source: Authors' own calculations based on BLH.

Figure 6
Future employment status and post unemployment wages by age



(a) Employed 5 years later

(b) Unemployed 5 years later

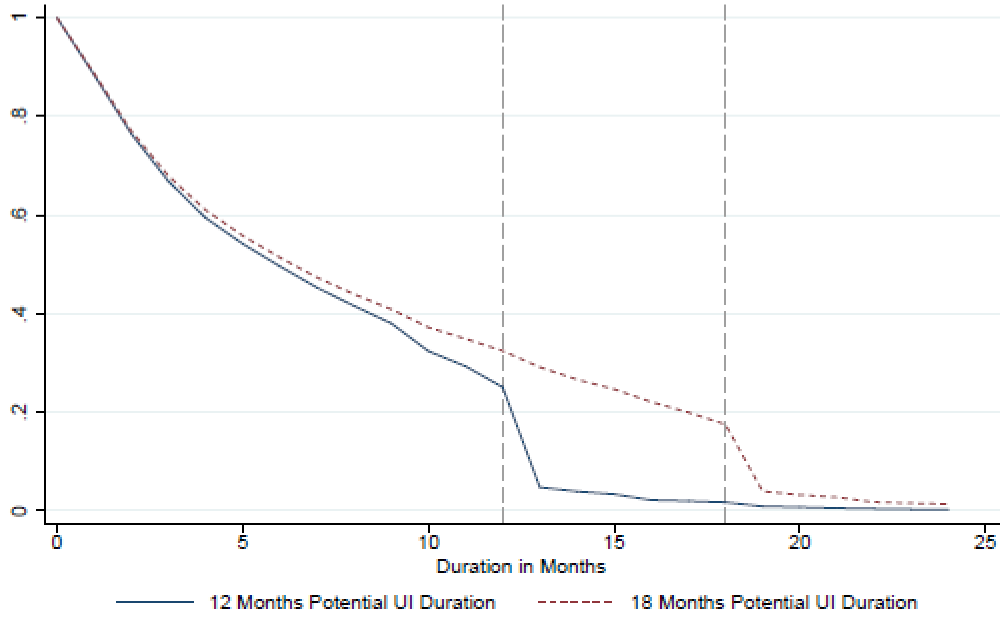
(c) Wage (per day) at next Job

(d) Ever employed again

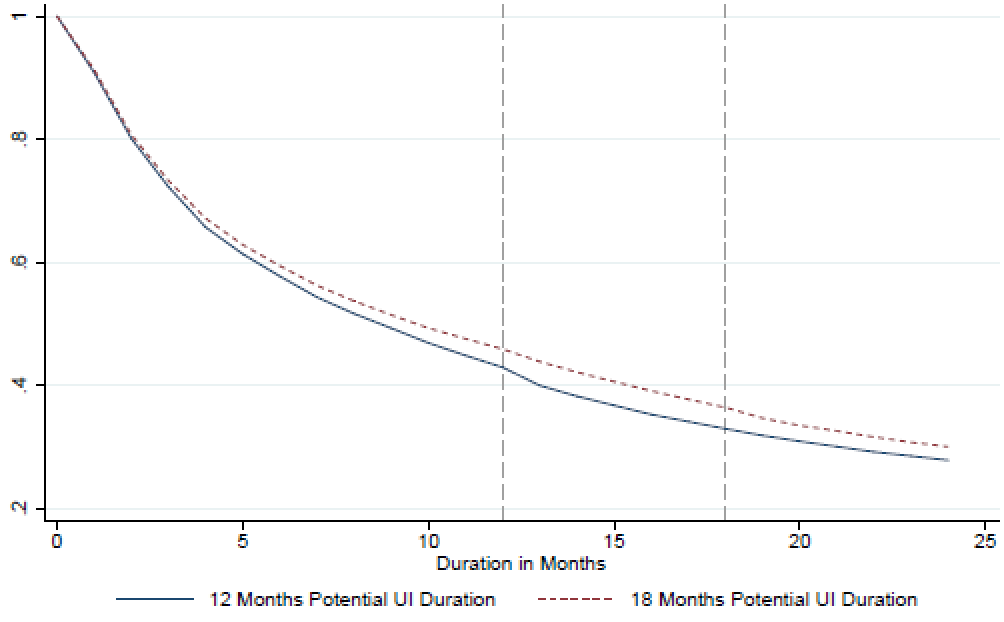
Note: For sample description see Figure 2.

Source: Authors' own calculations based on BLH.

Figure 7
Effect of increasing potential UI durations from 12 to 18 months on the survival functions – Regression discontinuity estimate at age 42 discontinuity



(a) Survival functions for staying in UI (ALG) built up from RD estimates

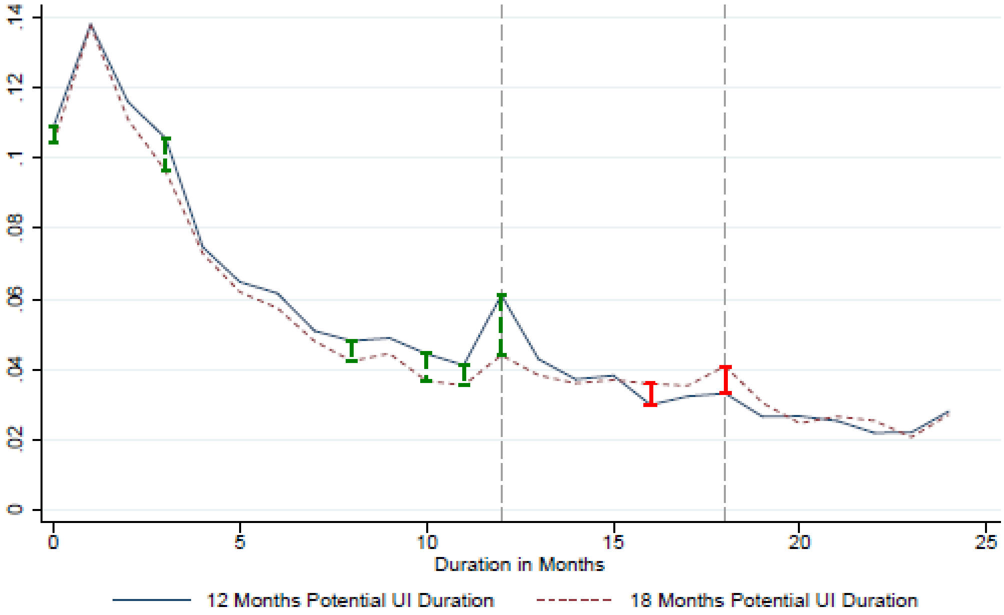


(b) Survival functions for staying in non-employment built up from RD estimates

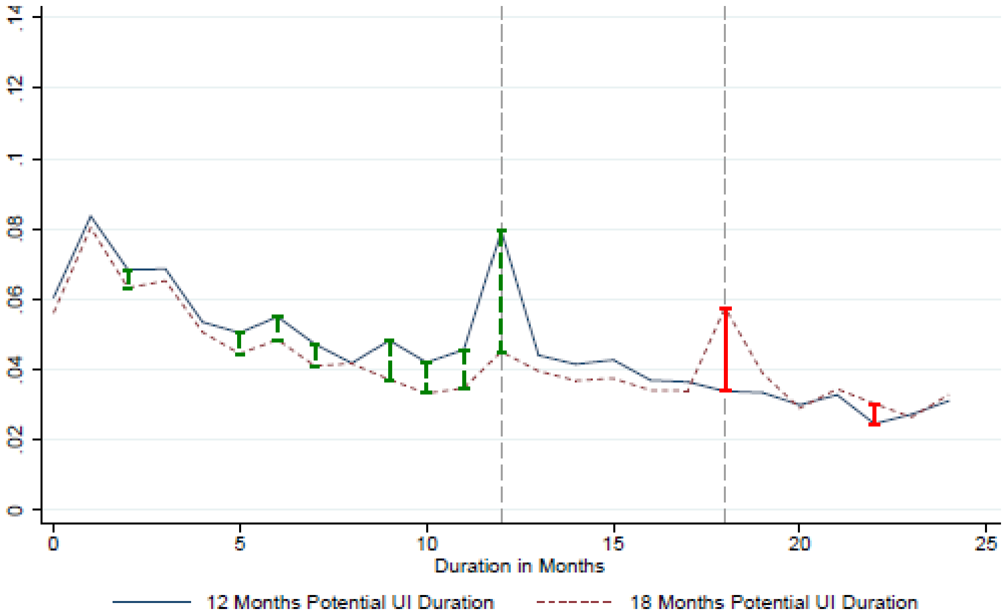
Note: The survival functions in both figures are estimated pointwise at each point of support using regression discontinuity estimation. For details see text.

Source: Authors' own calculations based on BLH.

Figure 8
Effect of increasing potential UI durations from 12 to 18 months on the hazard functions – Regression discontinuity estimate at age 42 discontinuity



(a) Empirical hazard of leaving non-emloyment for men

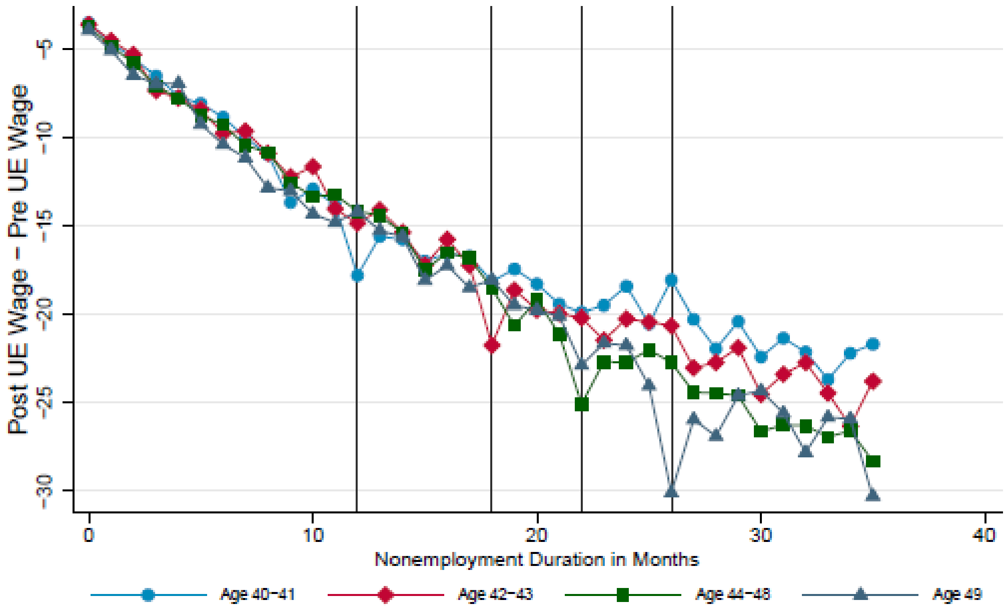


(b) Empirical hazard of leaving non-emloyment for women

Note: The hazard functions in both figures are 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. For details see text.

Source: Authors' own calculations based on BLH.

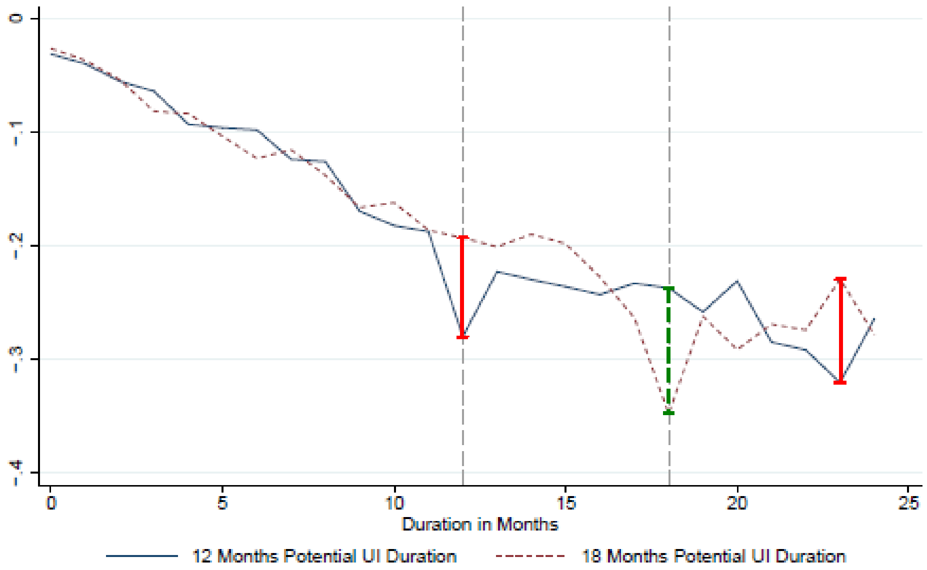
Figure 9
The decline of wages with the duration of non-employment



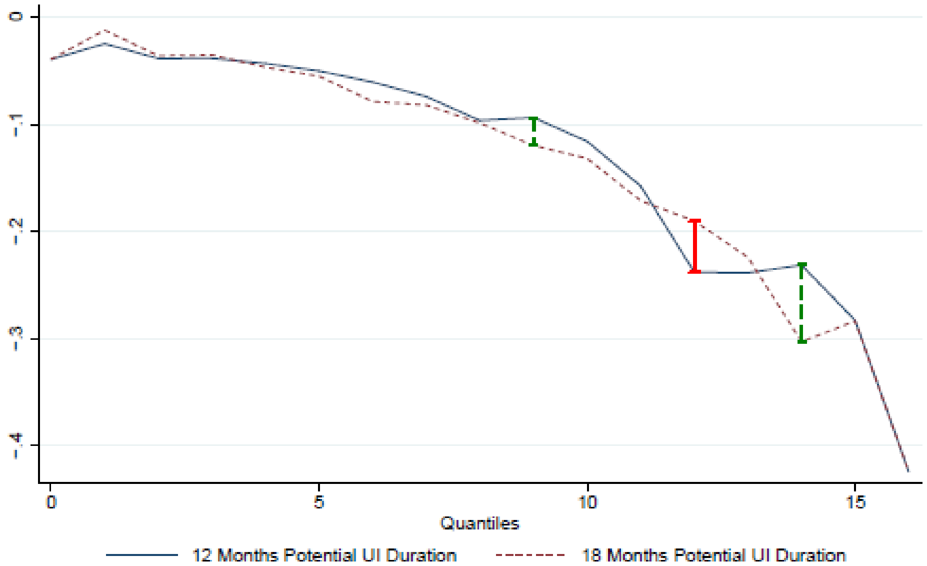
Note: The figure shows the average wage loss of individuals exiting UI conditional on the month of exit for different age groups (that were eligible to different UI durations).

Source: Authors' own calculations based on BLH.

Figure 10
Mean accepted wage by unemployment duration for workers with 12 and 18 month of UI eligibility



(a) Wage Loss by Non-employment Duration



(b) Log Wage Loss by Non-employment Duration Quantile

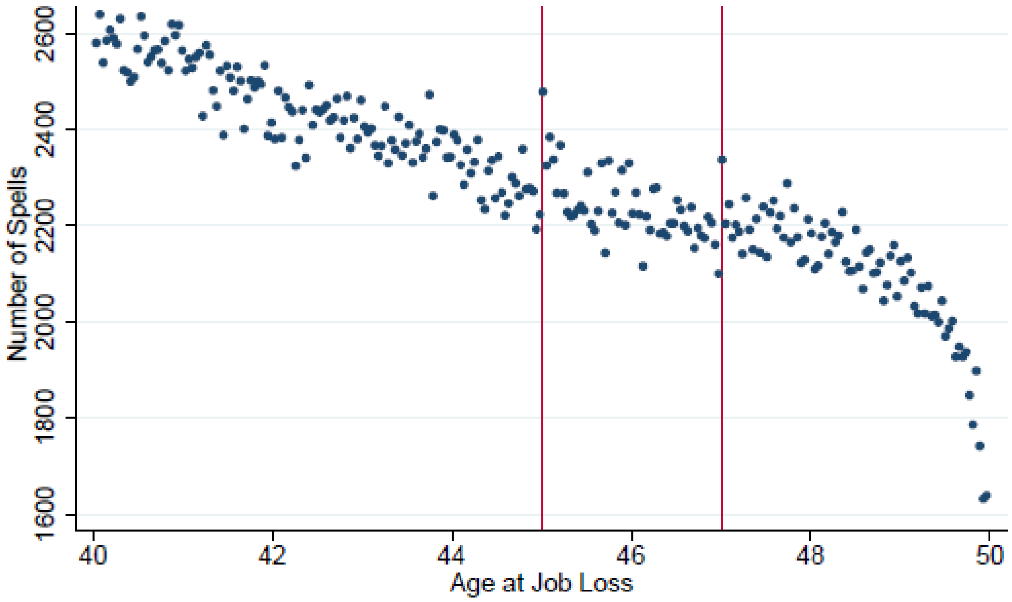
Note: The top figures shows the average log wage loss in Euro of individuals accepting jobs conditional on the month of exit (since the start of UI) for workers eligible for 12 months of UI benefits (the control group) and 18 months of UI benefits (the treatment group). The difference between the two functions is estimated using regression discontinuity at each point of support around the age 42 cut-offs. The bottom figure uses the same principle but shows wage losses by quantiles (in 5 percent intervals) of the respective duration distribution below and above the eligibility cut-off. Vertical bars between the lines indicate statistical significance on the 5 percent level.

Source: Authors' own calculations based on BLH.

Appendix

Figure A-1
Density around cut-offs Period: 1999 – 2004

(a) Age on date of UI claim

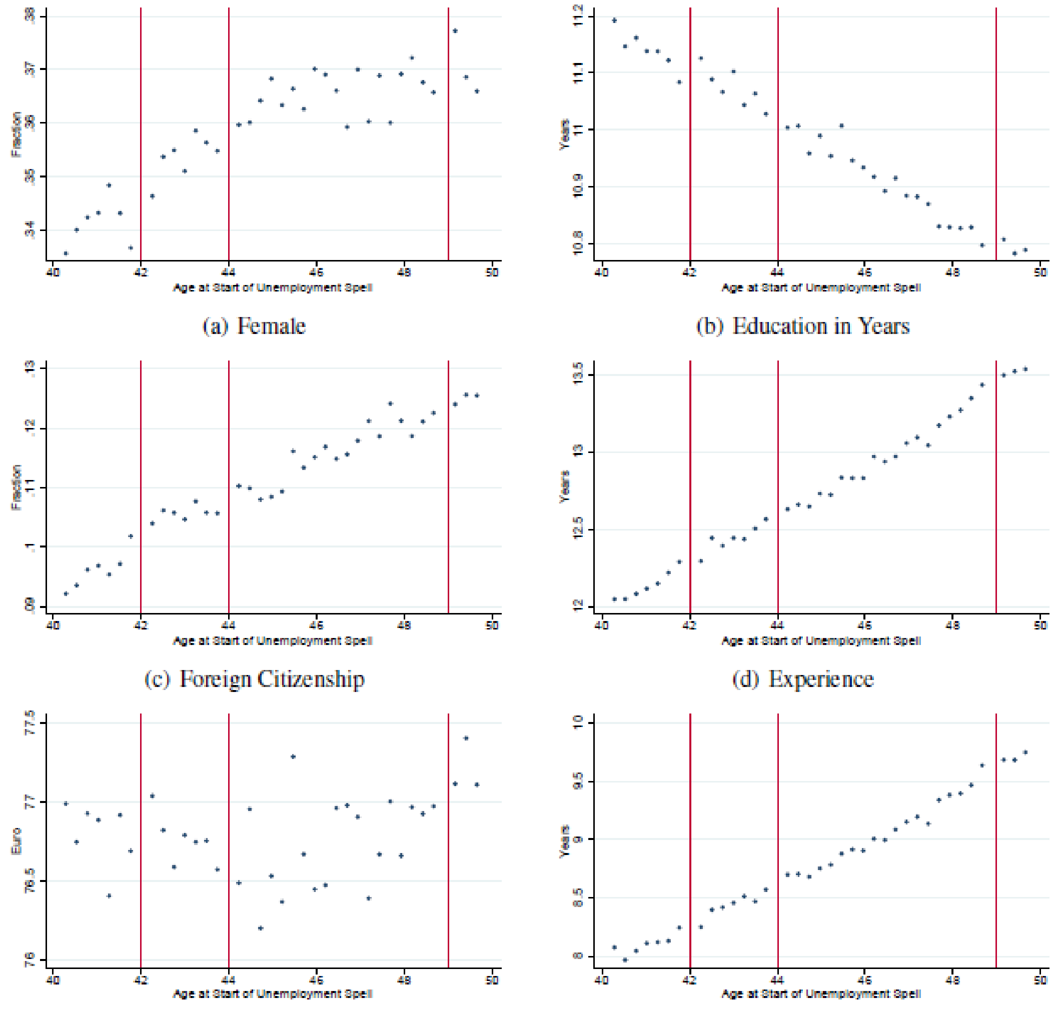


(b) Age at Job Loss

Note: 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 cut-offs for increases in potential UI durations at age 45 (12 to 18 months) and 47 (18 to 22 months). The sample consists of unemployed worker who had worked for at least 6 out of the last 7 years (and did not receive UI benefits in that time).

Source: Authors' own calculations based on BLH.

Figure A-2
Baseline characteristics around age discontinuities



(a) Female (b) Education in Years

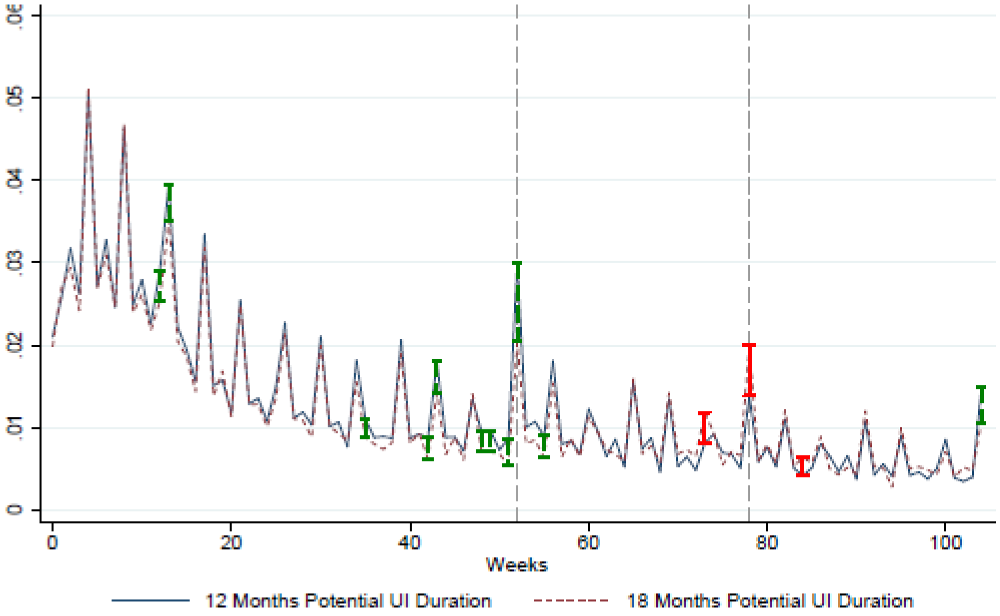
(c) Foreign Citizenship (d) Experience

(e) Previous Wage (f) Occupation Tenure

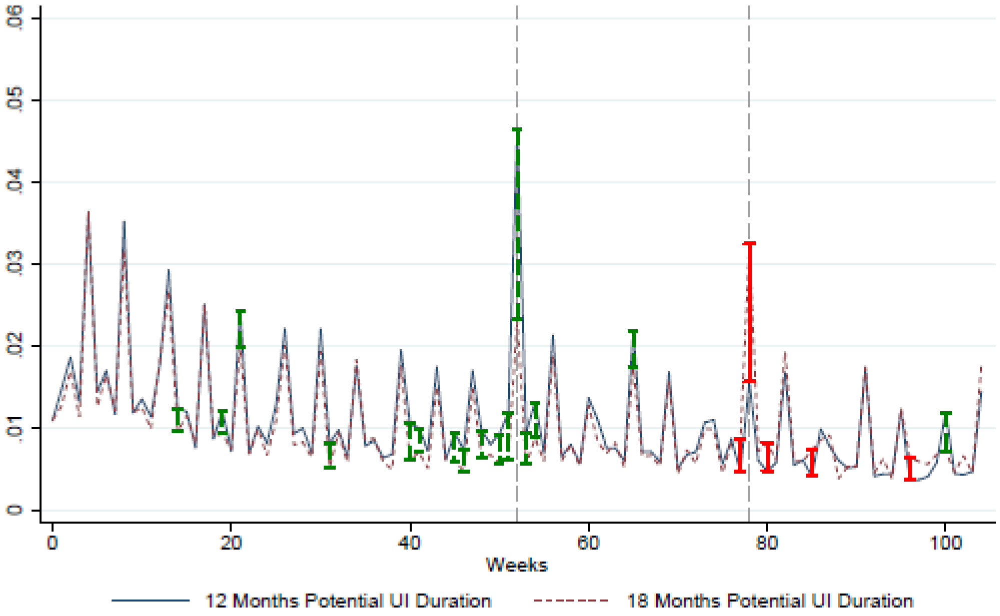
Notes: For sample description see Figure 1.

Note: For sample description see Figure 1.
Source: Authors' own calculations based on BLH.

Figure A-3
Effect of increasing potential UI durations from 12 to 18 month on the weekly hazard functions – Regression discontinuity estimate at age 42 discontinuity



(a) Empirical hazard of leaving non-emloyment for men



(b) Empirical hazard of leaving non-emloyment for women

Note: The hazard functions in both figures are 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. For details see text.

Source: Authors' own calculations based on BLH.

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