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IZA DP No. 14340

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ABSTRACT

The Effects of Shortening Potential Benefit Duration: Evidence from Regional Cut-Offs and a Policy Reform*

We quantify labour market effects of changes in the potential benefit duration (PBD) in Poland. Individual workers' PBD depends on the county unemployment rate relative to the national average—12 months of PBD above a cut-off of 125 per cent and 6 months below. This cut-off shifted from 125 to 150 per cent in a 2009 reform. We utilize i) the natural experiment of the reform, and ii) the sharp discontinuity generated by the cut-offs to estimate effects of shortening the PBD. Our administrative data cover unemployment spells for prime age workers during the years 2006-2018. A one-month shorter PBD decreases average benefit duration by 0.5 months and average unemployment duration by 0.4 months. The PBD reduction by six months increased the job finding rate within the first 9 months by 6 percentage points. Using the stock of unemployed per county, we find evidence for positive aggregate employment effects.

JEL Classification: H55, J20, J65

Keywords: unemployment benefits, extended benefits, spell duration

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1 Introduction

To strike the right balance between providing insurance and generating job-search incentives is key for policy makers when designing unemployment insurance (UI) regulations. On the one hand, a more generous UI guarantees adequate social protection for individuals who are out-of-work; on the other hand, a less generous UI incentivises unemployed workers to search for a job. In general, the generosity of the UI system is a function of (a) the level of unemployment benefits and (b) the potential benefit duration (PBD), i.e. the maximum time period during which an unemployed worker can draw benefits.[1] In this paper, we estimate the impact of changes in the maximum duration of unemployment benefits on the durations of benefit receipt and unemployment, and on the probability of re-entering into employment, holding benefit levels constant. To examine the importance of market externalities, we then compare these direct responses to the macro effect on county-level unemployment.

Our administrative data cover the universe of unemployment spells in Poland from January 2006 to January 2018, where new spells start during the period 2006-2011. We exploit the unique setting of the Polish unemployment benefit system, where the an individual's PBD depends directly on the unemployment rate of the county of residence relative to the country mean in the past year. Specifically, unemployed workers in counties with a relative unemployment rate of more than 125 per cent of the national average could receive up to 12 months of benefits, compared to 6 months for workers in counties below 125 per cent. In 2009, this threshold was increased from 125 to 150 per cent, making UI less generous for workers in affected counties. This set-up generates two sources of identifying variation to assess policy effects: *i)* the 2009 reform as a natural experiment shortening PBD for affected counties; and *ii)* the discontinuity at the threshold that links the county unemployment rate to the PBD.

First, we apply difference-in-differences (DD) to estimate the causal effect of the 2009 reform on the duration of unemployment benefit receipt, the duration of unemployment, and the probability to start a new job. Second, we use a regression discontinuity design

[1] Baily (1978) resolves the insurance-incentive trade-off and shows that the optimal *level* of unemployment insurance is negatively related to the elasticity of unemployment duration with respect to the benefit rate, i.e., the moral hazard cost of unemployment benefits. Chetty (2006) and Landais et al. (2018b) generalise the result and show how the optimal benefit level depends on sufficient statistics. An analogous argument can be made regarding the optimal eligibility length. In the models in Shavell and Weiss (1979) and Hopenhayn and Nicolini (1997), the optimal benefit level declines during a jobless spell, which supports the idea of short PBDs to incentivise job search.

(RD) to estimate the effect of different PBDs for all years 2006-2011 on the same outcomes. The empirical results from both approaches mutually reinforce each other: reducing the PBD from 12 to 6 months reduces the average length of unemployment benefit receipt by 2.9 months. The average duration of unemployment decreases by 1.9-2.5 months. The probability to start a new job in the first 9 months after becoming unemployed increases by 6 percentage points. Conditional on re-entering the labour market in the first 18 months after becoming unemployed, the entry into employment is sped up by 0.9 months.

In the presence of market externalities, the estimated individual-level effects need not directly translate to aggregate effects on unemployment. On the one hand, those starting a new job due to the reduction in the PBD might displace other workers. On the other hand, less generous UI might worsen the bargaining position of workers leading to lower equilibrium wages and more job postings. Few papers provide evidence on market externalities of changes in the PBD (e.g. Levine, 1993; Lalive et al., 2015; Marinescu, 2017; Johnston and Mas, 2018).

Johnston and Mas (2018) directly estimate individual-level and market-level effects of a cut in the benefit entitlement period in Missouri on unemployment durations. In contrast to the findings in Lalive et al. (2015) and Marinescu (2017), the aggregate effect on the level of unemployment based on individual-level effects assuming no spill-over is smaller than the actual aggregate effect from a DD estimate comparing Missouri to other states. Our approach to examining the presence of market externalities builds on that of Johnston and Mas (2018). We calculate the expected impact of the reform on the level of unemployment based on the direct effects and the number of individuals subject to the shorter entitlement period for each month of 2009 following the reform at the start of the year. As expected, the unemployment reducing effect increases with time as more and more individuals are subject to the shorter PBD. We compare these numbers to market-level aggregate effects estimated via DD on the unemployment rates at the county levels and find these to be of a similar magnitude.

The institutional set-up in Poland, where the maximum benefit duration is longer in *counties* where the unemployment rate was high in the *previous* year, is—to our knowledge—unique, but it shares a similarity with the US, where, when unemployment is high and growing, the Extended Benefits program increases the PBD in states with high unemployment rates. States can adopt additional extensions at their discretion. In the 2007-09 recession, this led to state-level variation in the PBD. An important difference to the Polish regulation is that recipients did not know about the extension, when their initial benefit spell started. Recent papers find limited (Rothstein, 2011; Chodorow-Reich et al., 2018; Boone et al., 2021) or large (Hagedorn et al., 2013, 2015) effects of these PBD extensions on unemployment rates.[2] As shown in Landais et al. (2018b), it might indeed be optimal that the transfer system is more generous in economic slumps. The reason is that, in economic downturns, labour market tightness, i.e. the ratio of aggregate vacancies to aggregate job-search effort, tends to be inefficiently low and in booms it tends to be inefficiently high. Most of the literature finds that an increase in the level of unemployment benefits increases tightness (Landais et al., 2018a). Thus, varying generosity of the benefit system can be used to improve labour market efficiency. In contrast, in Poland the generosity depends on the *previous* year's unemployment rate relative to the previous year's country mean.

Our main contributions are the following: First, we are able to exploit a setting with two sources of identifying variation to estimate treatment effects of changes in the PBD: We use RD exploiting differences in local unemployment rates and DD using the natural experiment induced by the 2009 reform. This goes beyond comparing nested specifications or specifications relying on comparable identifying assumptions as a robustness check and allows us—besides also serving as a robustness check—to investigate the effect of PBDs in several years. By using the two identification strategies we can also explore both within- and between-county variation over time. Second, the setting also allows us to identify effects for prime-age workers, instead of older workers who are often the focus of other empirical studies.[3] Third, our sample covers the universe of newly unemployed workers in Poland. The rich administrative data allow to obtain precise estimates and we can explore several dimensions of heterogeneity (gender, age, education). Fourth, we estimate aggregate effects using administrative data on the stock of individuals in unemployment per employment office and compare those to direct aggregate effects using our econometric individual-level estimates under the assumption of no spill-overs. Finally, we are the first to provide quasi-experimental evidence on the effects of PBD in Poland; adding one new case to the few larger countries covered in the existing literature, which has predominantly studied relatively small countries and specific US federal states.[4] These papers

[2] Depken and Gaggl (2016) find sizable effects of the end of long-term insurance in North Carolina on the unemployment rate.

[3] Several studies use age thresholds for the PBD as a source of exogenous variation (e.g. Caliendo et al., 2013; Lalive et al., 2015; Kyyrä and Pesola, 2020b).

[4] Past studies have estimated effects for specific US states (Katz and Meyer, 1990; Card and Levine, 2000; Landais, 2015; Johnston and Mas, 2018), Germany (Hunt, 1995; Caliendo et al., 2013), France (Baguelin and Remillon, 2014; Le Barbanchon, 2016), Slovenia (van Ours and Vodopivec, 2006), Austria (Lalive et al., 2006; Lalive, 2007, 2008; Lalive et al., 2011, 2015), Finland (Kyyrä and Pesola, 2020a,b),

use variations in the PBD due to age-dependent rules or due to policy reforms. They generally find statistically significant incentive effects of the maximum benefit period, which impacts both actual duration of benefit receipt and the probability of re-entry into employment.

Our estimates imply that an increase in potential benefit duration by one month increases *benefit duration* by 0.49 months on average. The size of this effect is in the upper end of previous studies, but is comparable to recent estimates for the US. Johnston and Mas (2018) find a marginal effect of 0.45 months for Missouri. In contrast, older US studies find substantially smaller effects. For instance, Katz and Meyer (1990) for several US states and Card and Levine (2000) for New Jersey report marginal effects of 0.1-0.2 months. At the lower end of the distribution, Lalive et al. (2006) estimate a marginal effect of 0.05 months for Austria. We find that an increase in the PBD by one month increases the average *unemployment duration* by 0.42 months, which is larger than other estimates found in the literature (e.g. 0.25 months for Missouri in Johnston and Mas (2018)).

Another strand of the unemployment benefit literature is concerned with spikes of unemployment exit rates and employment entry rates around the date of benefit expiry. A common finding is that the unemployment exit increases substantially around the date of benefit expiry, but this effect is much smaller for the re-employment rate (e.g. Card et al., 2007, and papers cited therein). In recent work, DellaVigna et al. (2020) aim to better understand the underlying mechanisms behind the spike at exhaustion and for this conduct a large survey of unemployed workers in Germany. They show that search effort increases before exhaustion and decreases thereafter, which is in line with a reference-dependent search model allowing for loss aversion with respect to recent income. Our analysis shows that in the Polish case, unemployment exit and job finding rates increase sharply just before benefit expiry, but remain high and decrease slowly in the subsequent months.

The next section introduces the institutional set-up, and Section 3 describes the data and delineates the empirical strategy. Section 4 presents results from a graphical analysis, followed by econometric results at the aggregate level in Section 5. Section 6 concludes.

and the Netherlands (de Groot and van der Klaauw, 2019).

2 Institutional Set-up

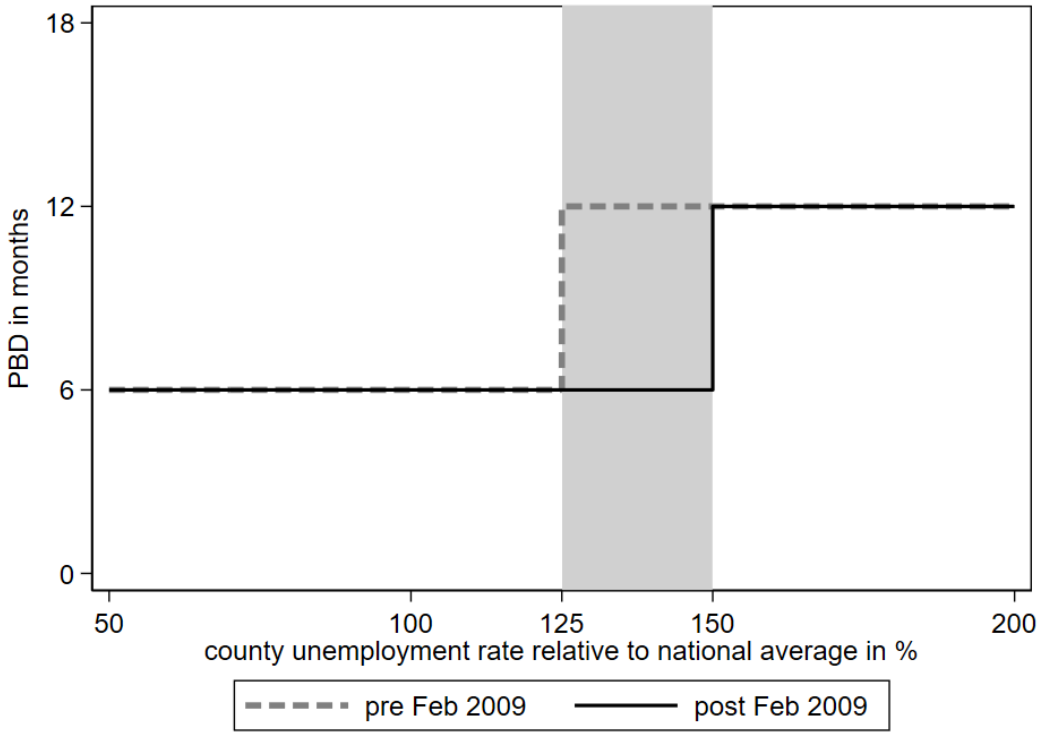
After 1989, the Polish economy went through a fundamental overhaul. Unemployment had barely existed in the previous decades and was a new phenomenon to accommodate economically, socially and politically. There was no institutional infrastructure to manage newly emerging problems and the unemployment benefit system had to be created from scratch with no possibility to refer to previous institutional arrangements (See e.g. Kluve et al. (2008) for an overview). A number of changes were introduced over the years, including the reform of interest in 2009. Over time, the unemployment rate has fallen substantially: after reaching a peak of 19.9 per cent in 2002, it fell to 7.2 per cent in 2008 and 3.5 per cent in 2019 (data from ILOSTAT database). Among OECD countries, unemployment benefits in Poland were relatively ungenerous in our period of interest. In 2007, Poland ranked 21st out of 29 countries in the net replacement rate in the first year of an unemployment spell (OECD, 2009). In 2009, monthly unemployment benefits amounted to 575 Polish zloty (308 US$ PPP).

The PBD of newly unemployed individuals depends on the county-level unemployment rate relative to the Polish national average. Specifically, if an unemployment spell begins at any time in a given year, say 2008, the unemployment benefit category of the county depends on its unemployment rate relative to the country mean as of 30 June of the previous year (2007), implying a longer PBD for individuals in counties above a given threshold. In February 2009, the threshold above which the PBD is extended was increased, thus reducing the PBD for unemployed workers in affected counties.

This is illustrated in Figure 1. Before February 2009 (dashed line), individuals from counties with a relative unemployment rate of up to 125 per cent could receive six months of benefits and those from counties with higher rates could receive 12 months. In February 2009, a reform was implemented—based on an act published on January 17th 2009—that substantially shortened the PBD for many individuals. The eligibility threshold for receiving 12 rather than six months of benefits was shifted from a relative unemployment rate of 125 to 150 per cent. For individuals above 50 years of age, different and more complex rules apply. Therefore, we exclude them from the analysis and focus on prime-age workers. Further details on the institutional setting are provided in Appendix C.

The institutional set-up yields three types of counties defined by their unemployment rate relative to the national average: counties with *i)* "low" relative unemployment rates ($\leq$125), *ii)* "medium" ($>$125 and $\leq$150), and *iii)* "high" ($>$150) relative unemployment

Figure 1: Reform of unemployment benefits



Notes: Figure shows an overview of the 2009 reform of the PBD. The grey shaded area indicates the relative unemployment range of counties treated by the reform, where the PBD was reduced.

rates. The second, "medium", category was directly affected by the 2009 reform and is the treatment group (the grey shaded area in Figure 1), the other two categories serve as control groups. Of the newly unemployed in 2009, 12 per cent were of the "medium" category. This moderate share alleviates concerns about country-wide equilibrium effects in the labour market. Note that as the unemployment category in a given year is defined by the relative unemployment rate of 30 June in the preceding year, the category of a county can be subject to change from year to year.

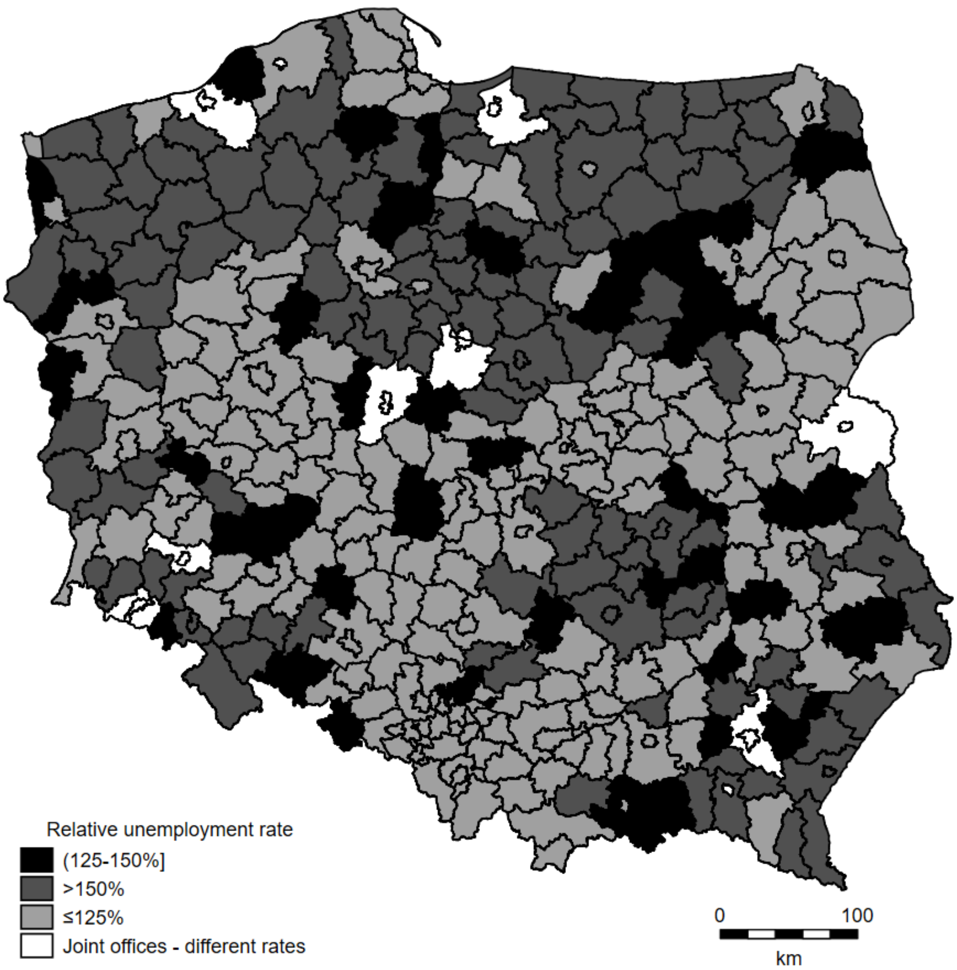
Poland had a total of 379 counties at the time of the reform, and 340 employment offices, as some counties shared an employment office. This is usually the case when a city is a county on its own and shares the employment office with the surrounding county. Two counties sharing an employment office typically belong to the same unemployment category. However, it also occurs that the counties have sufficiently different unemployment rates, so that the counties belong in different categories.[5] As we cannot assign individuals unambiguously to the correct treatment status, we omit those individuals

[5]10 out of 40 joint employment offices in 2009 and 2011, 11 in 2008, and 13 in 2010. Before 2008, an average of the two counties sharing an employment office was used and the counties thus belonged to the same category.

from the analysis (4.32 per cent of the sample).[6]

Figure 2 shows the county distribution of unemployment categories for 2008, which—as described above—determines the PBD for 2009. Counties with lower or equal to 125 per cent of the national mean are coloured light grey and the countries with rates above 150 per cent of the mean in dark grey. The potential durations of benefit payments (six and 12 months respectively) in these counties did not change due to the reform. The counties coloured black are the treated counties and saw a reduction of the PBD from 12 to six months in February 2009.

Figure 2: County distribution of relative unemployment rates in 2008



Note: Figure shows the unemployment rates of Polish counties for 30 June 2008 relative to the country mean. These determine the PBDs in 2009. Counties with a relative unemployment rate between 125 per cent and 150 per cent form the treatment group.

The map shows that the counties experiencing a shortening in benefits are distributed

[6]Two counties, Kielecki and Świdnicki, appear to have been wrongly assigned based on the unemployment rates provided by Statistics Poland. In 2008 (the former) and 2009 (the latter), many individuals received benefits for more than six months although according to their unemployment category they should not have been eligible for that. We have manually re-assigned these counties. As they jointly account for only 0.18 per cent of observations, this does not affect the results.

relatively evenly throughout the country. This means that our effects are not driven by certain regional developments or local economic shocks. Counties with high unemployment rates are somewhat clustered in the north and in the southeast of Poland. One driver of the regional diversity in unemployment rates is the distance to key economic centres of the country (Warsaw, Wrocław, Kraków, Poznan and Łódz). Figure A.1 in the appendix shows the respective county distributions for the years 2006 to 2011, corroborating that the relative magnitude of unemployment rates is quite persistent within counties over time, but there is also a non-negligible share of counties changing their status.

3 Data and Empirical Strategy

3.1 The data

We use administrative data of the universe of unemployed in Poland who started receiving unemployment benefits during the years 2006-2011. We observe the full length of these spells up to an upper censoring date at 31 January 2018, i.e. we do not observe the exit date of individuals who exited unemployment thereafter or not at all (this affects only 0.41 per cent of our sample). In total, our analysis sample contains 2,141,804 unemployment spells from 1,938,433 individuals. We observe the precise start and end date of each unemployment spell, and the number of days for which the benefit was received within these spells. The spells also indicate the pre-unemployment status of individuals (for example employment, imprisonment, welfare) as well as the exit state that individuals move into after unemployment, e.g., regular employment, retirement, active labour market policy (ALMP), which allows us to construct a variable of the date of return into employment—one of our main outcome variables.

We only use unemployment spells from fired individuals (85 per cent), as individuals who quit (15 per cent) received benefits for a shorter time period (see Appendix C). Among the fired individuals, we exclude those who were unemployed previously with a brief spell of employment in between the previous and current unemployment spell, since this reduced their potential benefit duration. Our analysis sample consists of 72 per cent of all individuals over 50 who entered unemployment between 2006-2011. Results including voluntarily unemployed individuals, whose choice of having become unemployed might be endogenous to the generosity of the benefit system, are qualitatively the same and available upon request.

Table 1: Summary statistics

Unemployment category Relative rate	Low ≤ 125% (1)	Medium (treated) (125%, 150%] (2)	High > 150% (3)	All (4)
Female	0.52	0.48	0.48	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Age at start of spell	32.49	32.28	32.87	32.57
	(8.34)	(8.54)	(8.59)	(8.44)
At most primary education	0.14	0.17	0.19	0.16
	(0.35)	(0.37)	(0.39)	(0.37)
Some secondary education	0.63	0.67	0.66	0.65
	(0.48)	(0.47)	(0.47)	(0.48)
Post-secondary/tertiary education	0.22	0.16	0.15	0.19
	(0.41)	(0.37)	(0.35)	(0.39)
Unemployment duration in months	9.22	11.21	12.73	10.51
	(12.09)	(13.50)	(14.42)	(13.09)
Benefit duration in months	4.48	6.35	7.88	5.72
	(2.04)	(3.94)	(4.50)	(3.56)
Observations	1,218,393	317,178	606,233	2,141,804

Notes: Table entries are means with standard deviations in parentheses at the individual spell level grouped by the relative unemployment level of counties. Sample consists of all spells of individuals who involuntarily became unemployed between 2006 and 2011. Individuals in counties sharing an employment office are excluded when the counties belong to different unemployment categories (see text).

The data further include a limited set of individual background characteristics; date of birth, sex, and highest education obtained. Importantly, we observe which of the 340 employment offices individuals are assigned to, corresponding to their residence. This determines for each year the respective county and associated PBD that individual workers face.

Table 1 presents summary statistics for the sample pooled over 2006-2011 by unemployment category of the counties and for all counties. Half of all newly unemployed individuals are female and individuals are just below 33 years old on average (individuals above 50 are excluded in the analysis sample). The indicators for education show that counties with lower unemployment rates have a higher share of unemployed individuals with a post-secondary or tertiary education and a lower share of unemployed with at most primary education. The respective durations of unemployment and unemployment benefit receipt, both of which are directly influenced by the unemployment category that counties belong to, are positively correlated with the relative unemployment level. The average and median length of unemployment spells—10.5 and 6.8 months, respectively—are somewhat longer than the typical lengths observed in other European countries that have been subject to previous studies.[7]

[7] For instance, de Groot and van der Klaauw (2019) report a median length of benefit receipt of 166

3.2 Econometric specification

To obtain estimates of the effect of reducing the PBD, we implement a difference-in-differences (DD) strategy. In its basic form, DD contains two groups and two periods. One group (the treatment group of counties with "medium" level of unemployment) is exposed to a *treatment* in the second time period (the reduction of the PBD in 2009). We first focus on the years 2008 and 2009 and compare the treatment group to each of the two control groups separately (counties with "low" unemployment and counties with "high" unemployment, respectively) in a textbook DD setting. We estimate the following equation:

$$y_{it} = \beta_0 + \beta_1 \text{treat}_{it} + \beta_2 \text{post}_t + \beta_3 \text{treat}_{it} \times \text{post}_t + \epsilon_{it}, \tag{1}$$

where y_{it} is an outcome of individual i at time t. *treat* indicates that the individual resides in a county affected by the reform and *post* is a dummy variable that equals one for 2009 (we exclude spells starting in January of either year in this specification, because the reform becomes effective in February). β_3, the average treatment effect on the treated (ATT), is the coefficient of interest. Standard errors are clustered at the employment office level. To obtain consistent estimates, we need to assume that assignment into treatment is random conditional on covariates and in particular that individuals do not choose their entry date into unemployment because of the reform. For instance, if despite being fired individuals can to some degree influence the start of their unemployment spell, one would expect that a disproportionately high number of individuals start taking up benefits at the end of January 2009 instead of February 2009 in order to enjoy a longer maximum entitlement period. If, in addition, unemployment spell durations of these individuals were to differ systematically from others, this would lead to inconsistent estimates. Figure A.2 in the appendix plots the frequency of entry into benefit receipt for each week in January and February 2008 and 2009 in treated counties, indicating no such pattern: entry frequencies are higher in January than in February in both years, and the frequency is slightly larger in 2009 in all weeks, in line with higher unemployment rates. No shift from early February to the last weeks of January can be observed in 2009.

In a next step, we use both control groups simultaneously, and the full data spanning three years pre- and post-reform, i.e. all unemployment spells starting in 2006-2011. This

days for the Netherlands in the period 2004-2006. van Ours and Vodopivec (2006) report a median unemployment duration of 5.3 months in Slovenia before 1998. The average length of unemployment spells in Finland between 2000 and 2004 in Kyyrä and Pesola (2020a) is 23 weeks.

allows for a more flexible estimation, increases the sample size and precision and allows us to control for more potential confounding factors. We estimate the following equation:

$$y_{it} = \gamma_0 + \gamma_1 \text{cat}_{it} + \gamma_2 \text{treat}_{it} \times \text{post}_t + \text{FE}_{it} + \gamma_3 \text{cat}_{it} \times \text{startmonth}_{it} + u_{it} \tag{2}$$

The vector cat_{it} denotes binary variables for the three unemployment categories as described in Section 2. It controls for baseline differences between the unemployment categories. The high dimensional FE vector controls further for month $\times$ year and employment office fixed effects (the latter fixed effect does not make the unemployment category fixed effect redundant as employment offices can be assigned to different unemployment categories in different years). The interaction $cat_{it} \times \text{startmonth}_{it}$ is a vector of interactions between the county categories and a linear time trend depending on the starting month of unemployment spells, thus allowing for differential time trends by unemployment category.[8] γ_2 is the coefficient of interest.

An attractive feature of the Polish institutional setting is that it incorporates multiple sources of variation that can be exploited for identification, as highlighted in the previous sections. The assignment to different county unemployment categories that determine different PBDs is a natural starting point for a DD analysis, as it affected a subset of counties at a specific time (the 2009 reform). However, the assignment into county unemployment categories itself relies on sharp cut-off rules of the relative unemployment rate of a county. For instance, counties with a relative unemployment rate of 124.9 per cent and 125.1 per cent in 2009 are assigned to different benefit categories, but plausibly face comparable economic conditions influencing the job finding rate. Pronounced differences in outcomes are then likely to be driven by the different incentive structure induced by the differing potential benefit duration. Note that to exploit this variation we do not rely on the reform introduced in 2009, but can simply use the different cut-off rules by year. We estimate the following regression discontinuity (RD) equation:

$$y_{it} = \delta_0 + \text{f}(\text{ur}_{it}) + \delta_1 \times \text{belowcutoff}_{it} + \theta \text{year}_t + \phi_{it} \tag{3}$$

$f(ur_{it})$ controls for the forcing variable (the relative unemployment rate of a county in year t). In the main specification we control for a quadratic polynomial. *belowcutoff*

[8] In an alternative specification, we additionally allowed for a change in this trend at the time of the reform. In that specification, the estimated treatment effect is a combination of the average effect of this change in the time trend and $\gamma 2$. Results are very similar to the main results in the paper and are available upon request.

is a dummy that equals one if an unemployment office is below the year-specific cut-off rule (changed in 2009) in year t.[9] The advantage of having two very distinct identification strategies is that it serves as a robustness check going beyond changing minor aspects of the same strategy. In addition, using the RD we can identify the effect of a shorter benefit receipt period in every year and not just in the time period surrounding the reform introduction.

3.3 Outcomes

The three main outcomes are exit from benefit receipt, exit from unemployment, and exit into employment (which includes work as an employee or self-employment/firm founding). Exit into employment is defined by the direction of the outflow, i.e. the status following the unemployment spell. In the main tables, the outcome variables are binary variables for cumulative exit at different points in time (3, 6, 9, 12, 15, and 18 months (see e.g. van Ours and Vodopivec, 2006). As all outcomes are available at the daily level, we also use a more fine-grained level when presenting survival graphs and hazard rates. Analogously to the binary variables indicating entry into employment in given time spans, we construct binary variables for entry into an ALMP or founding a firm. Additionally we use the duration in months until individuals exit from benefits or unemployment or until they enter into employment as dependent variables.

Censoring plays no role for the binary outcome variables. In principle, censoring could play a role for the outcome variable indicating the unemployment spell *duration* (in months). We observe spells for a long time horizon, until 2018, while the latest start date of spells in the analysis is 2011. Therefore, censoring of unemployment and benefit receipt spells are no relevant issues and the duration in months of these spells can be used as dependent variables in a straight-forward way (only 0.41 per cent of spells are censored in this way, see Section 3).

We observe the first exit state from unemployment and cannot follow individuals thereafter. Therefore, to construct the binary variables indicating entry into employment in a certain time span, we need to assume that individuals who exit to another non-employment state temporarily, such as ALMP, do not return to employment in the first 18 months after becoming unemployed. This is a plausible assumption, as the majority of exits from unemployment are directly into employment (66.2 per cent).

[9] In this specification we exclude all counties sharing an employment office, as we cannot identify the county-specific unemployment rates in this context.

We investigate the duration until entry into employment for the subsample of individuals who have exited into employment within 18 months after the start of the unemployment spell. We use this subsample for two reasons. First, we do not observe if individuals who exit from unemployment into a non-employment state, return to a job afterwards. Second, non-employment spells are truncated for those who never return to employment. Note that this estimate is a close approximation of the *total* effect of PBD on time until re-employment, since it is likely that the full effect of the reform is realised in the first 18 months after becoming unemployed. To obtain consistent estimates for the subsample, we need to assume that selection into the sample is independent of the reform. Given the above assumption that individuals who exit into a non-employment state such as ALMP do not return to employment in the first 18 months, we can directly test the assumption of no selection by testing if the reform has had an effect on the cumulative probability to become re-employed in the first 18 months after becoming unemployed (reported in Table 3). We cannot reject the null hypothesis of no effect at conventional significance levels, i.e. there is no indication of sample selection.

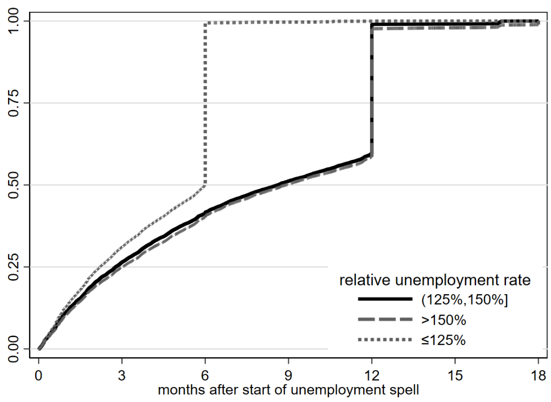
4 Graphical Analysis

Figure 3 shows Kaplan-Meier failure functions estimates for our three main outcomes separately for 2008 and 2009.[10] Panels (a) and (b) show cumulative exit rates from benefit receipt by months after start of unemployment benefits. We differentiate counties with low, medium, and high relative unemployment rates. In 2008 (Panel a), the curves of the medium and high relative unemployment categories are barely distinguishable with a continuous increase up to 12 months after the start of unemployment when slightly fewer than half of individuals still receive benefits. After 12 months, eligibility ends and the cumulative exit rate jumps to one. In the low unemployment counties the sudden jump occurs at six months, when about 50 per cent still received benefits. In 2009 (Panel b) the medium unemployment counties have their eligibility reduced to six months and the cumulative exit rate shifts closely to that of the low unemployment ones, but remains slightly lower. Note that the effect of the change in PBD seems to go beyond the mechanical effect at the benefit exhaustion date, as even in the first six months of

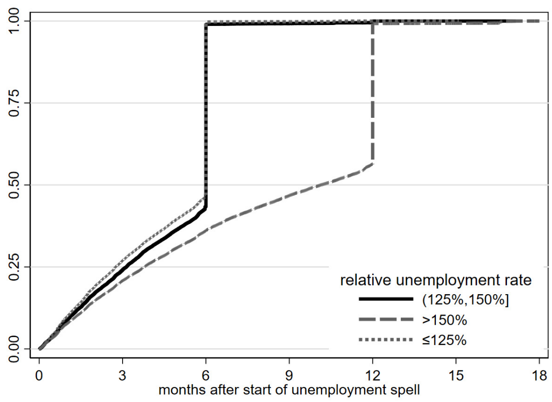
[10]Corresponding to this figure, Appendix Figures A.3 and A.4 show the same failure functions for the years 2006/2007 and 2010/2011, respectively. In addition, Appendix Table B.1 shows the share of individuals who exited at discrete points in time (3, 6, 9, 12, 15, 18 months) and the average duration in months.

unemployment the failure functions are notably higher in the counties with a PBD of six months than in those with a PBD of 12 months.

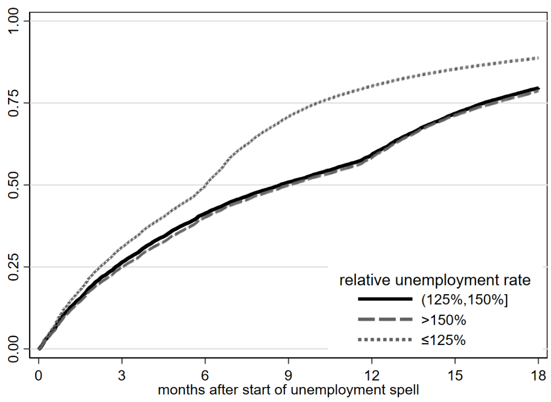
Figure 3: Kaplan-Meier failure functions by county unemployment type



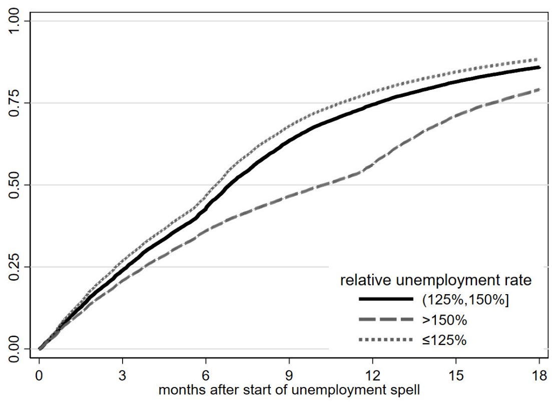
(a) Exit from benefits 2008



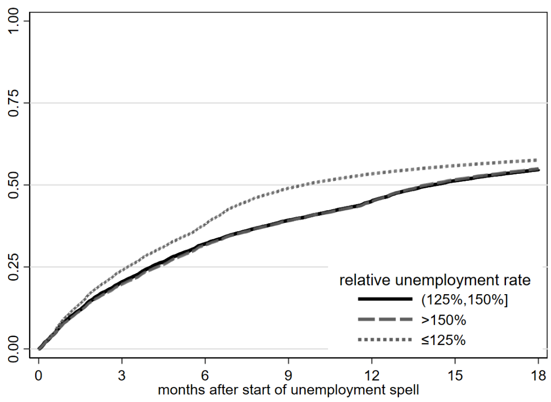
(b) Exit from benefits 2009



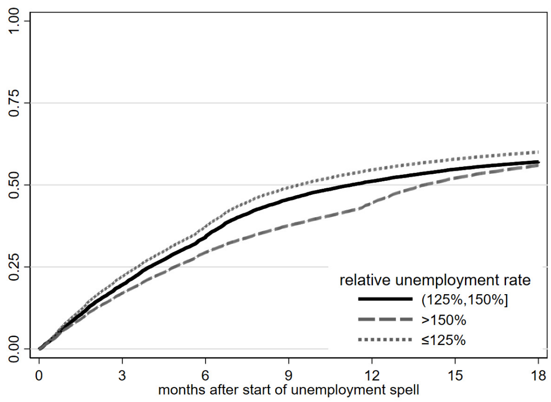
(c) Exit from unemployment 2008



(d) Exit from unemployment 2009



(e) Entry into employment 2008



(f) Entry into employment 2009

Note: Figure shows Kaplan Meier failure functions by the relative unemployment rate of counties. Panels (a) and (b) show cumulative exit rates from benefit receipt. Panels (c) and (d) show cumulative exit rates from unemployment. Panels (e) and (f) show cumulative entry rates into employment. In both years January is excluded.

While the impact of the PBD on the probability of exiting from benefit receipt is partly deterministic, this not the case for the probability of exiting from *unemployment*. Panels (c) and (d) show cumulative exit rates from unemployment for the two years. A similar pattern emerges that the counties affected by the reform (the solid line) closely resemble the high unemployment counties in 2008 (Panel (c)) and the low unemployment counties in 2009 (Panel (d)). However, exit rates in 2009 are somewhat lower for the medium unemployment counties compared to those with low unemployment. Comparing pre- and post-reform, the probability to exit unemployment within 12 months increased by more than 12 percentage points in the counties affected by the reform. Moreover, relative to high unemployment counties, the probability to exit from unemployment increased in treated counties already during the first six months, during which individuals are not mechanically affected by the change in duration. Finally, the two bottom panels (e) and (f) show entry into *employment*. The pronounced behavioural pattern of the other outcomes is retained (at a lower level, as not all individuals who leave unemployment enter directly into employment): entry rates into employment in the treated countries increase after the start of unemployment spells in 2009.

For low and high unemployment counties (not affected by the reform) the probabilities of exiting unemployment and into employment are reduced slightly in 2009, potentially due to the increasing overall unemployment rate in the financial crisis. For instance, the probability of having left unemployment in the first six months after becoming unemployed is reduced from 41 to 36.7 per cent in high unemployment countries, see Table B.1.

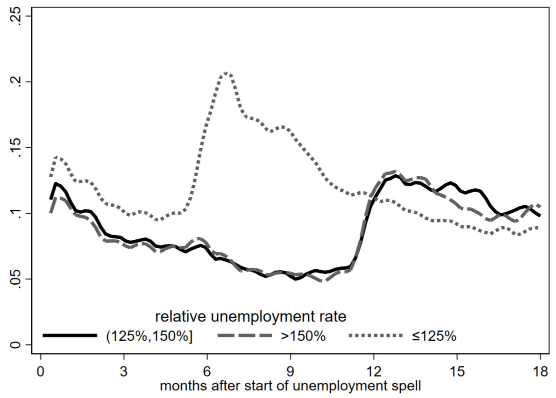
Next we examine hazard rates, i.e. exit probabilities conditional on survival at points in time, in Figure 4. A common finding in the literature on unemployment benefits are the pronounced spikes in exit from unemployment around the time of benefit exhaustion (e.g. Card and Levine, 2000; Card et al., 2007; DellaVigna et al., 2020; Katz and Meyer, 1990). For exit from unemployment (Panels (a) and (b)) we find that the hazard rate increases strongly after benefit exhaustion (six and 12 months for the different groups respectively), followed by a slow decline. This is in contrast to parts of the literature on other countries, which finds a spike in exits from unemployment around exhaustion, directly followed by a strong decrease in hazard rates to a low level (e.g. Card et al., 2007; DellaVigna et al., 2020; van Ours and Vodopivec, 2006).[11]

For entry into employment the bottom panels (c) and (d) show a continuous decline in the hazard rate since the beginning of the unemployment spell for both years. This

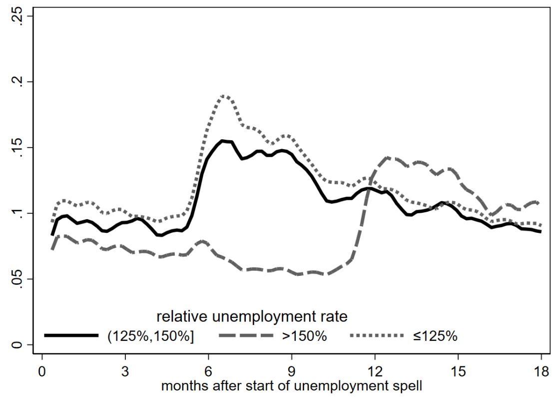
[11] We refrain from showing hazard rates for benefit exit due to its deterministic relationship around the expiration date.

suggests that individuals who do not find a job within the first few months after becoming unemployed, have a harder time re-entering employment. Selection (more able or motivated individuals finding work faster) or signalling (longer unemployment spells are interpreted as a negative signal by employers) may drive this. As is the case for exit from unemployment, the uptick in job finding is stronger after six than after 12 months.[12]

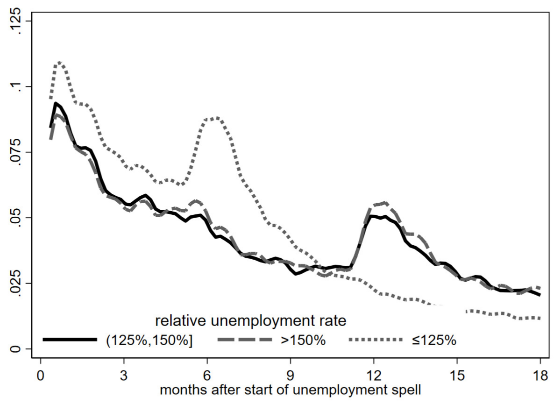
Figure 4: Hazard rates by county unemployment type



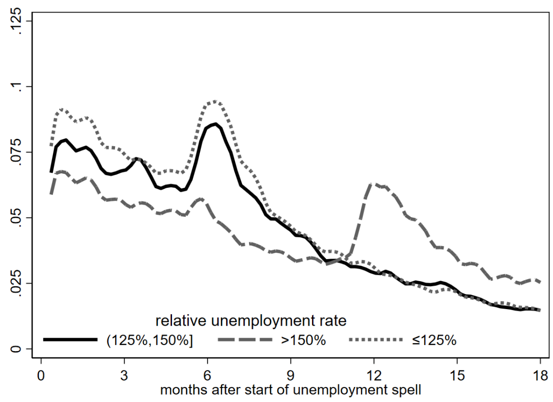
(a) Exit from unemployment 2008



(b) Exit from unemployment 2009



(c) Entry into employment 2008



(d) Entry into employment 2009

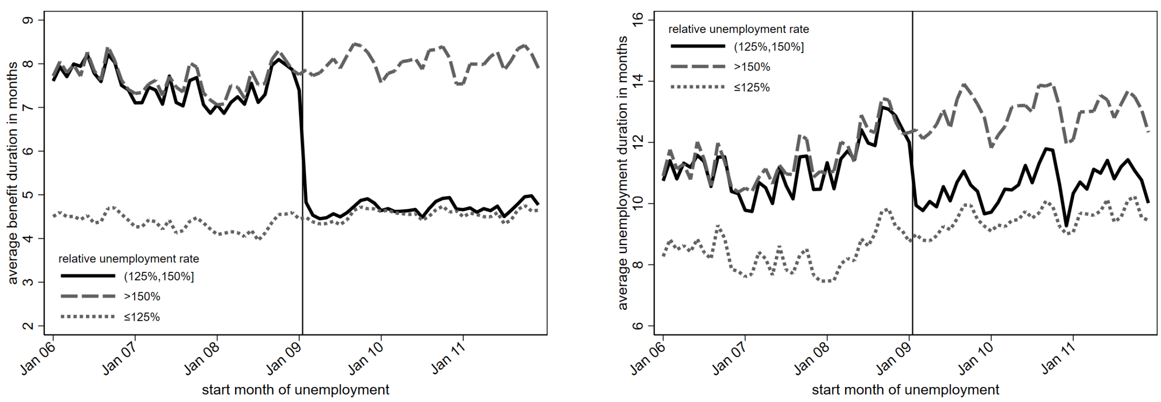
Note: Figure shows hazard rates of exiting unemployment and entry into employment by the relative unemployment rate of counties. Excluding January, bandwidth is set to one week. Hazard rates correspond to survival estimates from Figure 3. Hazard rates for the years 2006, 2007, 2010 and 2011 and hazard rates by year for low unemployment counties (to show time effects) are shown in Appendix Figures A.5 and A.6.

Before turning to the econometric results, we examine the validity of the identifying assumptions of the DD estimator. Figure 5 plots the average benefit length and unemployment length by start month of the unemployment spell from January 2006 to December 2011. Preceding the February 2009 reform, not only the trend, but also the level of the

[12]Again we also show the hazard rates for 2006, 2007, 2010 and 2011 in Appendix Figure A.5 and hazard rates by year for low unemployment counties in Appendix Figure A.6. The purpose of the latter figure is to inspect whether the 2010 reduction in the benefit amount after three months increased hazard rates around that time. We don't find convincing evidence for this.

unemployment categories with the same benefit structure are almost indistinguishable. With equal pre-treatment *levels*, the post-period common trend assumption is less reliant on the functional form (see Kahn-Lang and Lang, 2019). After the reform comes into effect in February 2009 the average benefit duration in affected counties is abruptly reduced by about 3 months. After the reform, the average benefit duration is then very similar to that of low unemployment counties. For the average unemployment length economic conditions appear to matter more as the average duration remains higher for the treatment group after the reform. Regardless, the lower average unemployment duration following the reduction of benefits is also apparent for this outcome as is the common pre-trend.

Figure 5: Benefit and unemployment duration by start date of unemployment spell



(a) Benefit duration

(b) Unemployment duration

Note: Figure shows the average benefit and unemployment duration in months by the relative unemployment rate of counties.

5 Econometric Results

5.1 Difference-in-differences

Basic DD — Table 2 shows results for the interaction term of equation (1), i.e. the ATT of the 2009 reform based on the basic DD specification using only the years 2008 and 2009.[13] The first two columns show the effects of the reform on benefit duration. Using either control group, the probability to end the benefit receipt in the first three months increases by about two percentage points (although estimates are not statistically

[13] As we report results for the basic specification without monthly dummies and the reform came into effect in February 2009, we exclude the month January from both years. Table B.1 in the appendix shows the cumulative probability of exits from benefits and unemployment as well as into employment for the three unemployment category counties before and after the reform. This allows to calculate the point estimates of the treatment effects presented in Table 2.

significant at the five per cent level), suggesting that the reform induces individuals to start looking for alternatives to benefits before the exemption. The reform effect is strongest with a point estimate of about 50 percentage points and statistically highly significant for exiting in the first 6 months as it includes the mechanical effect of benefit exhaustion. The point estimates for 12 and 15 months are close to zero and are driven entirely by groups with extended benefit duration, which cannot be identified in the data. While the first six rows in Table 2 display coefficients of linear probability models, the outcome variable of the regression coefficients reported in the first two columns of the last row denotes months until individuals stop collecting benefits. The reform effect on the duration of unemployment benefit receipt is a reduction of 2.7-2.8 months.

Columns 3 and 4 report the effects on exiting unemployment. First, we observe that the effect on exits from benefits by no means translates into an exit from unemployment of the same size. While the reform leads to a decrease in benefit receipt in the first 6 months by about 50 percentage points, the exit rate from unemployment increases by about 5 percentage points. The effect on the probability to exit unemployment remains positive and economically substantial even for the first 18 months after the start of benefits. It is above 5 percentage points at a point when individuals in both treatment and control groups are not eligible for benefits. The estimated effect on the duration of unemployment is a reduction of 2.1-2.3 months.

Finally, columns 5 and 6 show the effects on entry into employment. The effect is largest 9 and 12 months after benefit start for both control groups and more pronounced when high unemployment counties are used as a control group. The probability to start a job in this period increases by 4-7 percentage points, depending on the control group. The reform reduced the average number of months until individuals enter into employment by 0.8-0.9 months, conditional on starting a new job in the first 18 months (last row).[14]

Flexible DD — Table 3 displays results for the flexible DD specification based on equation (2) using all unemployment spells starting between 2006 and 2011. In addition to the dependent variables displayed in the previous table, it also shows effects on the probabilities to participate in an ALMP or to found a firm. The empirical results in the first three columns refer to the same outcome variables as those reported in Table 2. The

[14] The conditioning on individuals who return to employment in the first 18 months after becoming unemployed could lead to selection bias if the reform had an impact on whether individuals start a job in this time span (see Section 3.3). As we cannot reject the null hypothesis of no effect for entry into employment in the first 18 months, there is no evidence that the reform drives selection into the sample of individuals who return to employment in the first 18 months after losing their job.

Table 2: Effects of reducing the PBD by six months—basic DD

	Exiting from				Entry into	
	benefits		unemployment		employment	
Control counties:	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)
Interaction term: Treat × post						
$\leq$ 3 months	0.0173	0.0181*	0.0160	0.0168	0.0178*	0.0097
	(0.0119)	(0.0109)	(0.0118)	(0.0108)	(0.0095)	(0.0086)
$\leq$ 6 months	0.5421***	0.4925***	0.0531***	0.0464***	0.0420***	0.0287***
	(0.0155)	(0.0113)	(0.0148)	(0.0133)	(0.0117)	(0.0107)
$\leq$ 9 months	0.4511***	0.4136***	0.1418***	0.1394***	0.0709***	0.0560***
	(0.0163)	(0.0123)	(0.0169)	(0.0150)	(0.0129)	(0.0114)
$\leq$ 12 months	-0.0041	0.0092***	0.1522***	0.1505***	0.0586***	0.0419***
	(0.0032)	(0.0007)	(0.0165)	(0.0149)	(0.0131)	(0.0117)
$\leq$ 15 months	-0.0033	0.0080***	0.0872***	0.0933***	0.0288**	0.0130
	(0.0028)	(0.0006)	(0.0142)	(0.0134)	(0.0126)	(0.0112)
$\leq$ 18 months	0.0000	0.0000	0.0541***	0.0610***	0.0151	0.0007
	(.)	(.)	(0.0123)	(0.0117)	(0.0123)	(0.0107)
length in months	-2.8203***	-2.6586***	-2.2526***	-2.1307***	-0.9108***	-0.8127***
	(0.1519)	(0.1175)	(0.5547)	(0.5102)	(0.1384)	(0.1152)
Clusters	169	240	169	240	169	240
N	328,556	486,083	328,556	486,083	328,556	486,083
N (months)					184,794	286,390

Notes: Estimation of equation (1). Table entries are interaction terms between counties with medium employment levels (125-150 per cent relative to the country average) and a binary indicator that equals one for 2009. The first six rows show results of a linear probability model. Sample is restricted to years 2008 and 2009 and to individuals below 50 years of age. Months until entry into employment are conditional on doing so in the first 18 months of the unemployment spell. Standard errors, clustered at the employment office level, in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

estimated treatment effects are slightly larger and more precisely estimated. Column 4 shows that some of those who exit from unemployment participate in an active labour market policy, but the probability to do so in the first 12 months after the start of benefit receipt increases by only 1 percentage point. Column 5 shows that the reform did not have an economically meaningful effect on the probability to found a firm, which implies that the positive effect on entry into employment is driven by regular employment.[15]

Weekly effects and discussion of effect sizes — In Figure 6 we plot weekly estimates of equation 2 to inspect the evolution of treatment effects in more detail. Coefficients after 3, 6, ..., 18 months correspond to those displayed in the first three columns of Table 3. The figure illustrates that the effect of the benefit reduction is most distinct after six months,

[15] 4.3 per cent of the unemployment spells in our sample are followed by an ALMP and 5.1 per cent are followed by a self-employment spell.

Table 3: Effects of reducing the PBD by six months—flexible DD

	Exit from		Entry into		
	benefits (1)	unemployment (2)	employment (3)	ALMP (4)	self-employment (5)
Interaction term: Treat × post					
≤ 3 months	0.0282***	0.0270***	0.0218***	0.0019	0.0013
	(0.0075)	(0.0075)	(0.0052)	(0.0013)	(0.0011)
≤ 6 months	0.5566***	0.0559***	0.0361***	0.0056***	0.0014
	(0.0130)	(0.0092)	(0.0070)	(0.0017)	(0.0015)
≤ 9 months	0.4555***	0.1515***	0.0593***	0.0116***	0.0022
	(0.0127)	(0.0099)	(0.0073)	(0.0021)	(0.0017)
≤ 12 months	0.0034**	0.1623***	0.0464***	0.0125***	0.0016
	(0.0017)	(0.0097)	(0.0079)	(0.0023)	(0.0018)
≤ 15 months	0.0028*	0.1063***	0.0209***	0.0073***	-0.0003
	(0.0015)	(0.0075)	(0.0077)	(0.0023)	(0.0018)
≤ 18 months	0.0000	0.0690***	0.0042	0.0035	-0.0022
	(.)	(0.0064)	(0.0076)	(0.0023)	(0.0020)
duration in months	-2.9248***	-2.5488***	-0.9462***		
	(0.1034)	(0.2299)	(0.0892)		
Year × month FEs	Y	Y	Y	Y	Y
Unemployment category FEs	Y	Y	Y	Y	Y
Employment office FEs	Y	Y	Y	Y	Y
Clusters	340	340	340	340	340
N	2,141,804	2,141,804	2,141,804	2,141,804	2,141,804
N (months)			1,270,131		

Notes: Estimation of equation (2). Table entries are interaction terms between counties with medium employment levels (125-150 per cent relative to the country average) and a binary indicator that equals 1 from February 2009. The remaining categories i) <125 per cent and ii) ≥ 150 per cent. The first six rows show results of a linear probability model. Full population of unemployment spells for prime age workers started between 2006 to 2011 after being laid off. Months until entry into employment are conditional on doing so in the first 18 months of the unemployment spell. Standard errors, clustered at the employment office level, in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

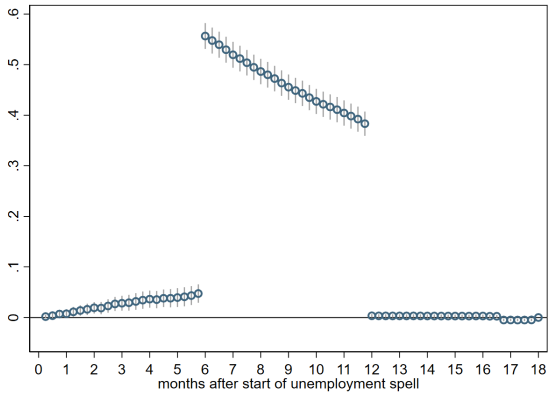
but a behavioural adjustment beforehand is also clearly visible. The strongest cumulative effect on exiting unemployment occurs at eleven months, and treatment effects for entry into employment peak after ten months. For the latter outcome, effects remain statistically significant until month 16 of the unemployment spell, and they are statistically significant and economically sizeable for exit from unemployment throughout the full 18 months.

As benefit spells do not exceed 18 months, the monthly estimates can be used to approximate the effect of the reform on the average duration of benefit spells. Making the simplifying assumption that the benefit spell of a person exiting between months k and $k-1$ ends precisely k months after becoming unemployed, the effect on average benefit duration is given by

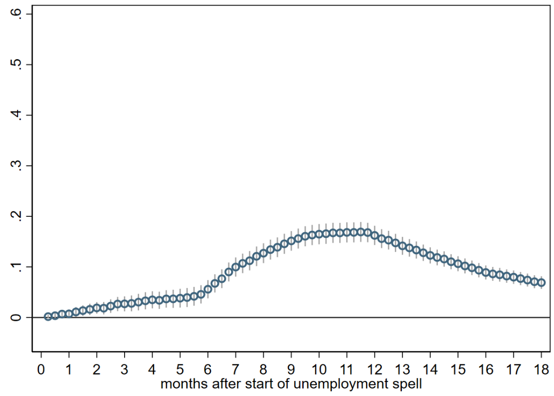
$$duration = \gamma_{2,1} + \sum_{k=2}^{18} (\gamma_{2,k} - \gamma_{2,k-1})k, \tag{4}$$

where $\gamma_{2,k}$ is the estimated reform effect on the probability to exit benefits in the first k

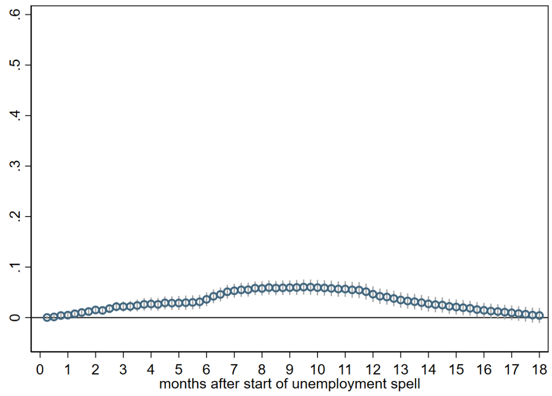
Figure 6: Weekly effects of reducing the PBD by six months



(a) Exit from unemployment benefits



(b) Exit from unemployment



(c) Entry into employment

Notes: Figure plots weekly treatment effects based on equation (2) as shown in 3-month windows in Table 3. See table notes for further details. Whiskers indicate 95 per cent confidence intervals.

months of unemployment. Using this formula, the reform effect on the duration of benefit receipt is -2.99 months, which is very close to the directly estimated effect of -2.92 months reported in Table 3.

Equation (4) can be used to decompose the reform effect on benefit duration into a behavioural part, which occurs before benefit exhaustion, and a mechanical part, which results directly from benefit exhaustion. The average mechanical effect of the reform is obtained by setting the coefficients $\gamma_{2,1}$ to $\gamma_{2,5}$ to zero. This mechanical effect amounts to -2.86 months. Thus, about four per cent of the average reduction in benefit duration is due to behavioural adjustments occurring before benefit expiry and 96 per cent is due to the direct effect of earlier benefit expiry.

To get a sense of the overall magnitude of the reduction in benefit duration due to the reform, consider an individual who becomes unemployed in February 2009 in a county with medium unemployment (treatment group). On average these individuals received benefits for 4.3 months. Based on the estimate for the ATT in Table 3, the expected duration of benefit receipt without the reform would have been $4.83 + 2.92 = 7.75$ months. Thus, in total, the reform reduced direct expenditures for these individuals by more than one third. The corresponding reduction in the average unemployment duration is by 20 per cent $(2.58/(10.3+2.58))$ and reduction in time until entry into employment is reduced by about 15 per cent $(0.95/(5.46+0.95))$, conditional on entering employment in the first 18 months after becoming unemployed.

Heterogeneous effects — Table 4 reports heterogeneous treatment effects for exits from unemployment and entry into job. They are obtained using a fully interacted version of the flexible DD specification (equation (2)). Thus, for instance, when comparing the effects on male and female workers, the coefficients are the same as those that would be obtained from two separate estimations.[16] The advantage of the joint estimation is that the results directly show whether differences in effect sizes are statistically significant.

Panel A displays effects on exits from unemployment. The first line shows the effects for male workers as a baseline and the second line shows how the effect for female workers differs. For exiting unemployment the difference is statistically significant and economically large after six months and remains so throughout. For instance, the reform effect on the probability to exit from unemployment in the first 12 months is almost 6 percentage points higher for females than for males, a relative effect of 43 per cent. Thus, women react more strongly to this change in incentives, which is in line with higher female labour supply elasticities commonly reported in the literature (see, e.g. Keane, 2011). The treatment effect on the unemployment duration is 1.1 months larger for women than for men.

Heterogeneity regarding education—comparing individuals with at least some secondary education ("high education") to those with lower attainment—is less pronounced with no statistically significant differences at the 5-per cent level. Finally, individuals above 32 years of age react more strongly to the reform: the increase in the probability of exiting unemployment within 12 months is 22 per cent larger than for younger unemployed. Again, the finding that older individuals react more strongly to short-term labour supply incentives is in line with the labour supply literature. For the young, the opportunity cost of time is larger because the returns to labour market experience are higher. Therefore,

[16] Specifically, the first coefficient equals the coefficient if the estimation were restricted to males, and the effect in a separate regression for females would be that coefficient plus the female interaction. E.g. after six months the effect on males is 0.0478 and the effect on females is 0.0633.

Table 4: Heterogeneous effects of a reduction of unemployment benefit duration

	$\leq$ 3m (1)	$\leq$ 6m (2)	$\leq$ 9m (3)	$\leq$ 12m (4)	$\leq$ 15m (5)	$\leq$ 18m (6)	duration in months (7)
Panel A: exit from unemployment							
Interaction	0.0260***	0.0478***	0.1237***	0.1321***	0.0825***	0.0511***	-1.9307***
	(0.0098)	(0.0118)	(0.0113)	(0.0106)	(0.0079)	(0.0063)	(0.2402)
Interaction × Female	0.0020	0.0155	0.0527***	0.0568***	0.0438***	0.0322***	-1.1439***
	(0.0097)	(0.0125)	(0.0108)	(0.0106)	(0.0088)	(0.0069)	(0.2700)
Interaction	0.0262***	0.0555***	0.1571***	0.1699***	0.1073***	0.0667***	-2.6559***
	(0.0081)	(0.0109)	(0.0118)	(0.0115)	(0.0091)	(0.0075)	(0.2873)
Interaction × High education	0.0007	0.0016	-0.0100	-0.0142*	-0.0020	0.0047	0.1390
	(0.0067)	(0.0089)	(0.0092)	(0.0082)	(0.0074)	(0.0061)	(0.2529)
Interaction	0.0201**	0.0467***	0.1323***	0.1471***	0.0995***	0.0663***	-2.3302***
	(0.0088)	(0.0101)	(0.0106)	(0.0099)	(0.0081)	(0.0069)	(0.2516)
Interaction × Above 32	0.0156**	0.0199**	0.0415***	0.0333***	0.0149**	0.0061	-0.5276*
	(0.0076)	(0.0084)	(0.0083)	(0.0077)	(0.0069)	(0.0066)	(0.2705)
Panel B: entry into employment							
Interaction	0.0200***	0.0261**	0.0420***	0.0309***	0.0070	-0.0078	-0.8243***
	(0.0069)	(0.0102)	(0.0097)	(0.0099)	(0.0092)	(0.0089)	(0.1155)
Interaction × Female	0.0014	0.0166	0.0305***	0.0265**	0.0237**	0.0200**	-0.2459*
	(0.0084)	(0.0115)	(0.0110)	(0.0108)	(0.0106)	(0.0100)	(0.1317)
Interaction	0.0185***	0.0294***	0.0526***	0.0398***	0.0121	-0.0032	-1.0062***
	(0.0064)	(0.0097)	(0.0099)	(0.0100)	(0.0093)	(0.0091)	(0.1138)
Interaction × High education	0.0056	0.0136	0.0141	0.0138	0.0179**	0.0148*	0.1052
	(0.0064)	(0.0087)	(0.0094)	(0.0089)	(0.0087)	(0.0083)	(0.1021)
Interaction	0.0160***	0.0318***	0.0439***	0.0323***	0.0124	-0.0024	-0.8320***
	(0.0058)	(0.0072)	(0.0079)	(0.0083)	(0.0085)	(0.0079)	(0.0969)
Interaction × Above 32	0.0125*	0.0093	0.0331***	0.0301***	0.0179**	0.0136*	-0.2583***
	(0.0069)	(0.0081)	(0.0086)	(0.0089)	(0.0082)	(0.0080)	(0.0961)
Year × month FEs	Y	Y	Y	Y	Y	Y	Y
Unemployment category FEs	Y	Y	Y	Y	Y	Y	Y
Employment office FEs	Y	Y	Y	Y	Y	Y	Y
Clusters	340	340	340	340	340	340	340
N	2,141,804	2,141,804	2,141,804	2,141,804	2,141,804	2,141,804	2,141,804
N (months)							1,270,131

Notes: Estimation based on a fully interacted version of equation (2): table entries are interaction terms between counties with medium employment levels (125-150 per cent relative to the country average) and a binary indicator that equals 1 after February 2009. The simple interaction shows the effect for the omitted group (e.g. males in the first row). Interactions with binary indicators for being female, having high education, and being older than 32 years, respectively, are included as well as an interaction of all fixed effects with the respective group. This is equivalent to estimating separate regressions for the two groups being compared. The remaining unemployment categories i) <125 per cent and iii) $\geq$ 150 per cent. Full population of unemployment spells for prime age workers started between 2006 to 2011 after being laid off. Standard errors, clustered at the employment office level, in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

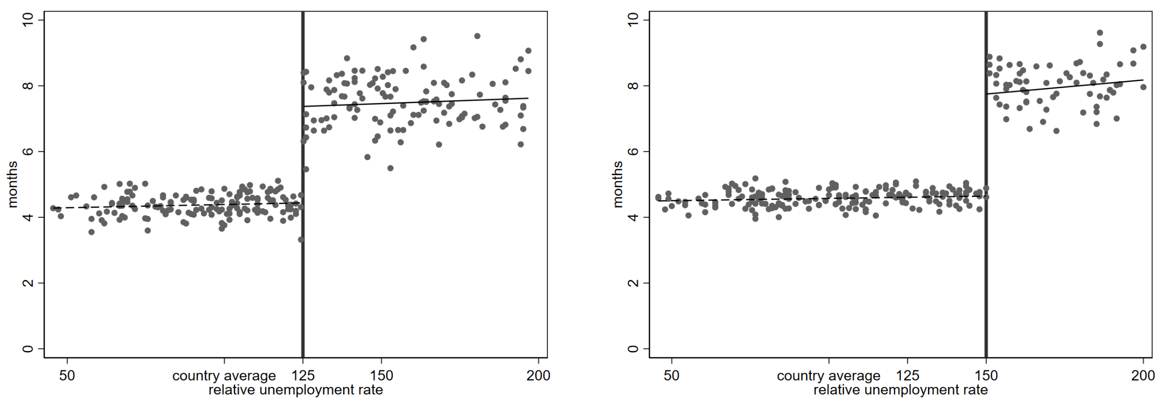
they react less strongly to short-term incentives such as temporary changes in net wage rates or—in our case—changes in the PBD (see, e.g., Keane and Wasi, 2016; Shaw, 1989).

Panel B reports treatment effects on entry into employment. Again, female workers react more strongly than male workers. The effect on the probability to enter employment in the first 12 months is almost twice as large for women as for men. The effect of the reform on those with high educational attainment to start a new job is more pronounced than for those with low education. In contrast to the lower educated, the effect on the probability to enter into employment in the first 18 months is significant at the ten per cent level for the higher educated. The reform also had a stronger effect on the duration until re-entering the labour market for the over-32-years-old with a difference of about 0.3 months.

5.2 Regression discontinuity

In the next step we estimate treatment effects by using the cross-sectional variation induced by the sharp cut-off rules of the relative unemployment rate of counties. In all years in our sample, the PBD is set at 12 months in counties above the respective cut-off (125 or 150 per cent relative unemployment) and is set at six months in counties below the respective cut-off. Note that this is an entirely different source of identifying variation, separate from and adding to the natural experiment generated by the 2009 reform. Figure 7 underpins this identification strategy and plots the average county benefit duration for 2008 and 2009 by the relative unemployment rate of counties. In both 2008 and 2009, the sharp cut-off rules at 125 and 150 per cent of the relative unemployment rate induce a pronounced discontinuity in the average benefit duration. The positive relationship between the relative unemployment rate and benefit length is relatively weak away from the cut-off. This corroborates that the notable increase in average benefit duration around the cut-off is due to the change in the PBD and not due to a general relationship between the relative unemployment rate and the average duration.

Figure 7: Benefit duration and the relative unemployment rate



(a) Unemployment benefit duration, 2008 (b) Unemployment benefit duration, 2009

Notes: Units of observation are counties. Fitted line is weighted by number of observations per counties. January is excluded in both graphs.

Table 5 presents the estimates obtained from equation (3) using the sample of unemployment spells starting in the years 2006-2011. The effect of a shorter benefit receipt in the RD is estimated based on the cut-off in *every year* rather than on the reform of benefits introduced in *one year* (2009) in the DD estimates.[17] If the effect of shorter PBD is stronger in, say, 2011, the identified effects are larger in the RD setting. The coefficients in Table 5 are overall quite similar to the once identified in Table 3. Thus, the estimated effects of the PBD are not specific to this particular reform in 2009.

Table 5: Effects of a six-month shorter PBD—sharp RD design

	Exit from		Entry into
	benefits (1)	unemployment (2)	employment (3)
Below cut-off coefficient			
$\leq$ 3 months	0.0089	0.0083	0.0043
	(0.0062)	(0.0062)	(0.0052)
$\leq$ 6 months	0.5744***	0.0398***	0.0224***
	(0.0066)	(0.0081)	(0.0066)
$\leq$ 9 months	0.4712***	0.1470***	0.0653***
	(0.0076)	(0.0092)	(0.0070)
$\leq$ 12 months	0.0028***	0.1567***	0.0494***
	(0.0008)	(0.0089)	(0.0071)
$\leq$ 15 months	0.0026***	0.0784***	0.0047
	(0.0008)	(0.0074)	(0.0066)
$\leq$ 18 months	-0.0000	0.0467***	-0.0092
	(0.0000)	(0.0063)	(0.0065)
duration in months	-2.9233***	-1.9223***	-0.9020***
	(0.0686)	(0.2791)	(0.0793)
Clusters	298	298	298
N	1,708,224	1,708,224	1,708,224
N (months)			1,011,340

Notes: Table entries are the RD coefficient (δ_1) of equation 3, which is an indicator variable for the county to have an unemployment rate below the cut-off (125 until January 2009, 150 thereafter). Estimates control for a linear and quadratic term of the forcing variable (relative unemployment rate) interacted with the year. Sample excludes all counties that share an unemployment office, as the unemployment rate of the county cannot be identified. Months until entry into employment are conditional on returning to employment in first 18 months. Full population of unemployment spells for prime age workers started between 2006 to 2011 after being laid off. Standard errors, clustered at the employment office level, in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In Table 6 annual RD estimates are presented. This is an additional perspective on the effects of variations in PBD, since effects by year cannot be obtained using the DD specification which relies on the introduction of the reform in February 2009. Overall, similar effect sizes are estimated under different regulatory regimes, i.e. before and after the 2009 reform, and under different macroeconomic conditions. Over the years the

[17]The RD sample is slightly smaller than the DD sample. In the DD estimates we only exclude the few cases where counties with joint offices belong to different unemployment categories. Here we have to exclude all counties with joint offices as we are unable to identify their precise unemployment rate. To investigate if this necessary sample restriction affects the result, we ran the DD regressions using this somewhat smaller RD sample: with only slight differences in the magnitudes of the point estimates (and maintained levels of statistical significance) this is not the case. Corresponding results are presented in Table B.2.

Table 6: Effects of a six-month shorter PBD by year—sharp RD design

Year	2006	2007	2008	2009	2010	2011
	(1)	(2)	(3)	(4)	(5)	(6)
Benefit duration (months)	-3.0707***	-2.5974***	-2.7659***	-2.8835***	-3.0116***	-3.1547***
	(0.1053)	(0.1462)	(0.1257)	(0.1021)	(0.0801)	(0.1035)
Unemployment duration (months)	-1.8000***	-1.1595***	-2.1295***	-2.3045***	-2.0771***	-1.9772***
	(0.3316)	(0.4190)	(0.4787)	(0.4797)	(0.4080)	(0.4307)
Clusters	295	296	296	298	298	298
N	305,155	234,356	231,064	352,799	301,228	283,622
Entry into employment (months)	-1.3345***	-0.9819***	-0.7086***	-0.8489***	-0.6337***	-0.7526***
	(0.1388)	(0.1605)	(0.1289)	(0.1308)	(0.0933)	(0.1126)
Clusters	295	296	296	298	298	298
N	187,230	140,930	131,166	206,344	177,517	168,153

Notes: Table entries the RD coefficient (δ_1) of equation 3, which is an indicator variable for the county to have an unemployment rate below the cut-off (150 until January 2009, 125 thereafter). Estimates control for a linear and quadratic term of the forcing variable (relative unemployment rate). Coefficients are identified from separate regressions. Sample excludes all counties that share an unemployment office, as the unemployment rate of the county cannot be cleanly identified. Months until entry into employment are conditional on returning to employment in first 18 months. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

country level unemployment rate first decreased from 13.97 per cent in 2006 to 7.04 in 2008 and then started to increase again, reaching 9.68 per cent in 2010). In the appendix we further show RD graphs for all years for the outcomes duration of benefit receipt (Figure A.7), duration of unemployment (Figure A.8), and months before entry into employment (Figure A.9).

5.3 Aggregate effects: direct versus total effects

The empirical analysis so far has focused on micro effects, i.e. direct effects on those that are immediately affected by the shortening of the PBD. The aggregate direct effects can be obtained from our micro estimates and the number of individuals who become unemployed after the reform. These direct effects translate one-to-one into total effects on the level of unemployment only if there are no market externalities. In the following we use aggregate unemployment numbers on the county level to quantify the total reform effects and find some evidence for them being statistically significant. Then we construct the sum of direct effects obtained from micro data assuming no spill-overs. We find that these are of a similar magnitude as the total effects. Our approach is similar to that in Johnston and Mas (2018), who obtain total effects by estimating a DD models on Missouri (the intervention unit) and a comparison group of US states. The advantage in our set-up is that there are multiple counties distributed over the entire country in the treatment group.

In principle, total effects might be smaller or larger than the sum of direct effects (Pissarides, 2000; Lalive et al., 2015; Landais et al., 2018b). On the one hand, in models with diminishing returns to labour as a production factor and where labour demand is unaffected by workers' outside options, individuals induced to look for a new job displace other workers (rat race effect). In this case, the total effect of a decrease in the PBD is smaller than the direct effect. On the other hand, in models with Nash bargaining, a worsening of workers' outside options through the decrease in the PBD leads to lower wages and thus more job openings (wage effect). In that case, the total effect of a decrease in the PBD is larger than the direct effect as these additional openings are partly filled by the unemployed who are not directly affected by the reform. In practice, both wage effects and rat race effects might play some role.

In this analysis, we assign the group status of the counties based on the relative unemployment rate in 2008 to avoid discontinuous jumps when the status of some counties changes at the start of a year. We run regressions separately using the two control groups of low and high unemployment counties. To have more comparable counties at the macro-level, we restrict the counties in this analysis to have a relative unemployment rate in 2008 between 100 and 175% relative to the country average.

For our analysis, ideally, changes in the PBD would occur only due to the 2009 reform. In that case, applying DD on unemployment numbers on the county level would yield "clean" treatment effects of the reform. The estimated total effect would then be comparable to the direct effect simply given by the number of individuals who become unemployed in treated counties after the reform multiplied by the individual-level estimates for the reform effects. In the case of no spill-over, the two would be the same. However, in practice, PBDs also change between non-reform years because counties switch, e.g., from the low unemployment to the medium unemployment category as their unemployment rate increases. For instance, only 44 per cent of counties that are in the medium unemployment category in 2008 remain in this category in 2009.[18] Thus, even if there had been no reform and PBD changes were the only source of changes in unemployment in the treatment relative to the control groups, DD on aggregate data would yield a non-zero effect. Consequently, total reform effects obtained from DD estimates at the county level are contaminated. As a consequence, we estimate DD using aggregate county-level data, but account for PBD changes that are unrelated to the 2009 reform when constructing

[18] This does not pose a problem for the previously analysed individual-level effects, as the regulation when entering unemployment determines individual PBD.

aggregate direct effects of PBD changes from micro estimates. Thus, the two estimates are directly comparable. They do not represent clean reform effects, but rather the effects of all PBD changes, regardless if caused by the reform or otherwise, in treatment and control groups.

We obtain treatment effects for 35 post-treatment months from the following DD equation, where observations are employment office by month units:

$$\begin{aligned} \ln(\text{stock unemployed})_{jt} = \psi_0 + \psi_1 treat_j \\ + \sum_{s=1}^{35} \psi_{2,s} \times \text{treat}_j \times \text{post}_t \times \text{month indicator}_{s,t} + \text{FE}_{jt} + \nu_{jt} \end{aligned} \tag{5}$$

The dependent variable is the log of the aggregate number of registered unemployed under 50. We use this as a dependent variable rather than the unemployment rate, as the reform impacted individuals over 50 differently. The unemployment rate for under 50s only is not available. The subscript j denotes employment offices, t denotes months. $\text{FE}_{j,t}$ contains employment-office, and year and month fixed effects. month indicator$_{s,t}$ is one for observations in post-reform month s, zero otherwise. The coefficients $\psi_{2,s}$ approximate the employment office level ATT in per cent of the number of unemployed for post-reform month s. We estimate equation (5) using observations from the years 2008 to 2011. Thus we need the common trend assumption on the aggregate level to hold for this period.

The effect on the stock of unemployed is expected to increase gradually after the reform as an increasing number of unemployed have a shorter PBD. To check if the change in the trend of the number of unemployed is statistically significant, we estimate

$$\begin{aligned} \ln(\text{stock unemployed})_{jt} = \eta_0 + \eta_1 treat_j + \eta_2 \text{treat}_j \times \text{month}_t \\ + \eta_3 \text{treat}_j \times \text{post}_t \times \text{month}_t + \text{FE}_{jt} + \nu_{jt}. \end{aligned} \tag{6}$$

η_2 denotes a trend in the difference between treatment and control group and η_3 denotes the change in this trend after the reform. Essentially, η_3 gives the average of the $\psi_{2,s}$.

The total change in the treatment relative to a control group is compared to aggregate direct effect that we base on the micro estimates. This direct effect quantifies the changes in the stock of unemployed due to PBD changes in the treatment group relative to a control group assuming no spill-over or displacement effects. In principle, the direct reform effect is given by individuals who become unemployed in treated counties after the reform and the micro estimates for the reform effect. However, as mentioned above, to make the direct effect comparable to the estimated total effect, we need to account for the fact that even in the absence of the reform, one can expect small changes in the stock of unemployed in treatment and control groups because the PBD in counties can change when the relative unemployment rate passes a threshold determining the PBD from one year to another. Therefore, we need to calculate the predicted change in unemployment due to PBD changes not only for counties in the treatment group, but also for counties in the two control groups. We calculate the change in the stock of unemployed for the months starting January 2008 relative to the counterfactual that the PBD in this county is always as in 2008. For month t this number, $\Delta y^-_{j,t}$, for counties j with a PBD of 12 months in 2008 (relative unemployment rate over 125 per cent), is given by

$$\Delta^- y_{j,t} = -\sum_{k=1}^{36} \gamma_{2,k} n_{j,t-k} / \left(y_{j,t} - \sum_{k=1}^{36} \gamma_{2,k} n_{j,t-k} \right). \tag{7}$$

$y_{j,t} - \sum_{k=1}^{36} \gamma_{2,k} n_{j,t-k}$ is the number of people who would be unemployed in the counterfactual scenario without the reform and category changes of counties, $-\sum_{k=1}^{36} \gamma_{2,k} n_{j,t-k}$ is the absolute deviation from this counterfactual. $y_{j,t}$ denotes the actual total number of unemployed in employment offices in county j in month t. $\gamma_{2,k}$ is the individual-level treatment effect of the shortening of the PBD on the probability to have left unemployment k months after becoming unemployed obtained from the estimation of equation (2).[19] The coefficients $\gamma_{2,k}$ are plotted in Figure A.10. $n_{j,t-k}$ is the number of people who have entered unemployment k months before month t in county j and whose PBD is 6 months (rather than 12 months as for individuals who became unemployed in 2008). In counties in the treatment group, this is the case for most individuals who became unemployed after the 2009 reform. Only if the county changed to the high unemployment category, the PBD is still 12 months post-reform. For counties with a PBD of 6 months in 2008 (relative unemployment rate up to 125 per cent), the relative deviation from the constant-PBD counterfactual, $\Delta^+ y_{j,t}$, is given by

$$\Delta^+ y_{j,t} = \sum_{k=1}^{36} \gamma_{2,k} n_{j,t-k} / \left(y_{j,t} + \sum_{k=1}^{36} \gamma_{2,k} n_{j,t-k} \right). \tag{8}$$

The difference to equation (7) is that the deviation from the counterfactual is positive

[19]We assume that differences in the PBD have a negligible effect more than 36 months after becoming unemployed.

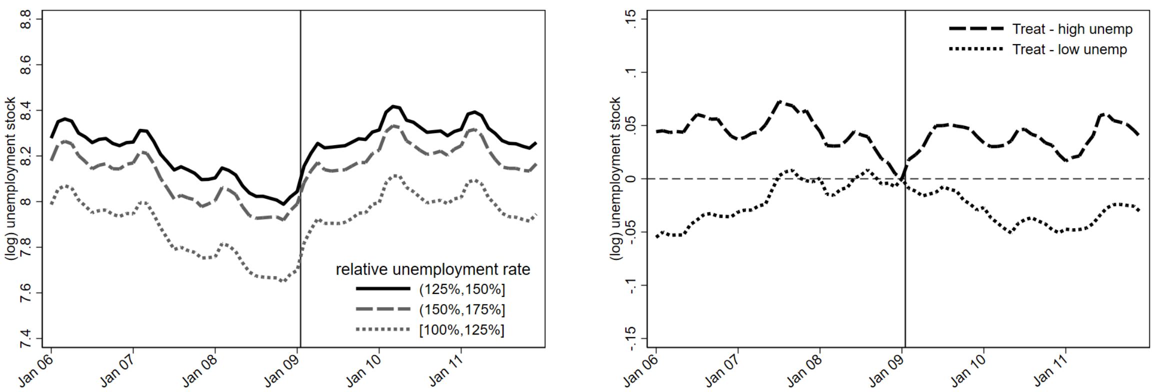
because a switch would increase the PBD and thus increase the unemployment stock. We calculate this number for the months January 2008 to December 2011.

To quantify the direct reform effects comparable to the total effects, we calculate the DD of the direct-effect deviations from the constant-PBD counterfactual for every month t from February 2009. Let $\Delta y_{treat,t}$ and $\Delta y_{control,t}$ denote the average relative deviations from the constant-PBD counterfactual (equations (7) and (8)) over all counties in the treatment and control group. Note that if PBD changes occurred only due to the reform, $\Delta y_{treat,t}$ would yield the direct aggregate effect. However, to account for contamination in treatment and control group due to group switching of counties, we obtain the aggregate direct effect that is comparable to the total effect from

$$\text{Direct effect} = \Delta y_{treat,t} - \Delta y_{control,t} - \frac{1}{12}\sum_{m=1,2008}^{12,2008}\left(\Delta y_{treat,m} - \Delta y_{control,m}\right). \tag{9}$$

The last term is the average difference between treatment and control group over the 12 months in 2008.

Figure 8: Unemployed stock by unemployment category

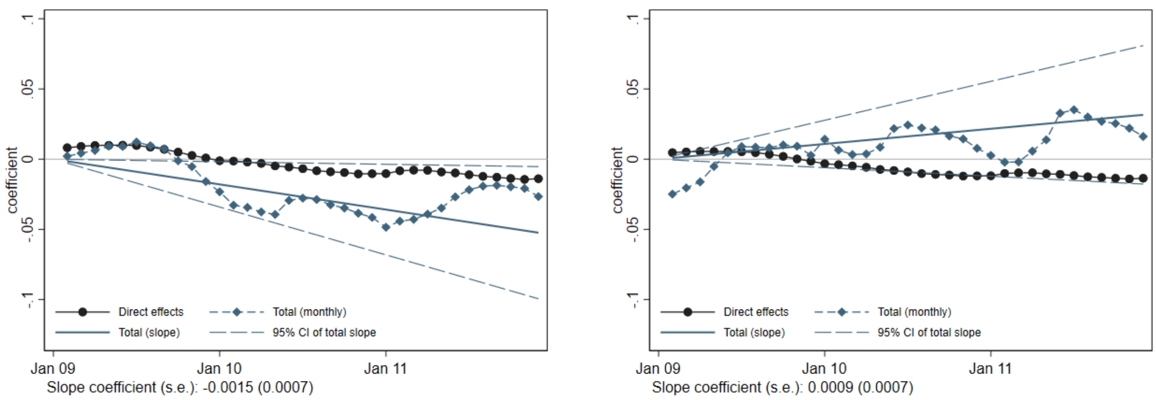


(a) Number of unemployed (b) Difference to control groups

Notes: Panel (a) displays the average log number of unemployed under 50 years of age per employment office by unemployment category over calendar time. Panel (b) shows the difference between employment offices in the treatment group and the other unemployment categories normalised to zero in January 2009.

Figure 8, panel (a), shows the average log stock of unemployed under 50 for counties in the treatment group and the two control groups over time. The stocks of unemployed follow a similar path. Before 2009 the unemployment rate decreased in all groups and following the financial crisis it increased in 2009. Panel (b) shows the difference in the average log of unemployed between the treatment group and the two control groups, normalised to zero in January 2009. Comparing the treatment group to the control group with low unemployment rates, there is a common trend in the period directly before the reform. Following the reform, unemployment in the treatment group decreases relative to the control group. The decrease is gradual. This is expected because the number of individuals with a shorter PBD increases over time. Comparing the treatment group to the control group with high unemployment rates, there are changes in the difference in the stock of unemployed already before the reform and no clear reform effect is visible.

Figure 9: Macro and micro reform effects on unemployment



(a) Control: Low unemployment

(b) Control: High unemployment

Notes: Figure plots the effect of the 2009 PBD reform on the stock of unemployed under 50 based on equation (9) using monthly estimates at the individual level under the assumption of no market externalities (circles), aggregate effects (diamonds) based on equation (5) using registry data on all unemployed under 50 at the employment office level, and the change in the trend based on equation (6). Standard errors for the confidence band are clustered at the county level.

Panel (a) of Figure 9 shows aggregate total and direct effects of the 2009 reform using the control group of low unemployment counties. The circles display predicted changes in unemployment in the treatment group relative to the control group based on estimates from individual-level data, calculated using equation (9). The horizontal line at zero represents the counterfactual of no PBD change in treatment and control groups. In the first few months, there is a predicted increase in unemployment in the treatment group. This is due to pre-reform changes in the PBD because some counties changed from the low unemployment group to the treatment group in January 2008. This increase in the PBD in 2008 still affects unemployment exit probabilities more than a year later (see Figure 6). Then, gradually, the level of unemployment in the treatment group decreases relative to the control group as more and more unemployed are affected by the 2009 reform. In some months, the level of unemployment increases again. This happens in months with a large inflow into unemployment. Individual-level estimated effects are small in the first few months after becoming unemployed. Thus, large inflows diminish the importance of the reform.

The diamonds show the aggregate changes of the treatment group relative to the control group estimated from the number of unemployed registered at the treated employment offices, i.e. the macro perspective. These are obtained from the DD at the county level based on equation (5) using the log of the stock of unemployed as the dependent variable. Directly after the reform, we see an increase followed by a decrease, close to the individual-level effects. After that, the level of unemployment remains lower than in the counterfactual and than predicted from individual-level estimates. The solid line shows the change in slope estimated from (6). The decrease in the level of unemployed is statistically significant. Panel (b) shows the corresponding graph using the control group of counties with high unemployment rates. Individual-level effects are similar to those shown in panel (a), but market-level effects do not differ statistically significantly from zero.

In sum, we find some evidence for market-level effects of similar magnitude as expected based on the individual-level estimates.

6 Conclusion

This paper analyses the effect of a reduction in the PBD in Poland on the benefit and unemployment durations of newly unemployed workers, as well as on their probability to re-enter employment. It makes use of the unique set-up of the Polish unemployment insurance system, where the PBD depends on the previous year's unemployment rate in the county of residence. In counties with relatively high unemployment rates, the potential duration is 12 months, in those with relatively low rates it is six months. In 2009, the cut off was reduced such that the eligibility period was reduced from 12 to six months in many counties. The setting allows for two distinct identification strategies to quantify the effect of shorter PBD on labour market outcomes. We use DD to analyse reform effects and apply RD to estimate the effects of different potential benefit durations in each year of the period 2006-2011.

These different set-ups yield similar results, implying that a one-month shorter PBD decreases average benefit duration by 0.5 months and average unemployment duration by 0.4 months. Moreover, we find that the reduction in PBD by six months increased the probability to find a new job in the first 9 months after starting benefit receipt by about

6 percentage points. A heterogeneity analysis reveals that women and older workers react considerably stronger to changes in PBD. This is in line with the common finding of higher labour supply elasticities for these groups. These individual-level effects translate one-to-one into aggregate effects only in the absence of market externalities. Estimating the market-level reform effects estimated via DD of aggregate unemployment numbers on the county level, we find some evidence for significant market-level effects of similar magnitude as the individual-level effects.

Our results provide insights for the design of optimal unemployment insurance schemes, which trade off insurance against job seeking incentives. In Poland, the PBD has a substantial impact on the unemployment duration. This possible benefit of the 2009 reform, which reduced the PBD and sped up return into employment, must be weighed against the decrease in disposable income for the unemployed who did not succeed in finding a job in six months.

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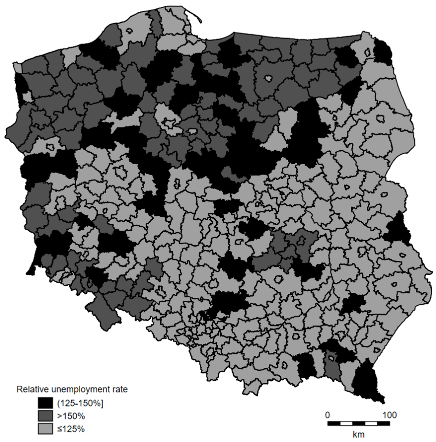
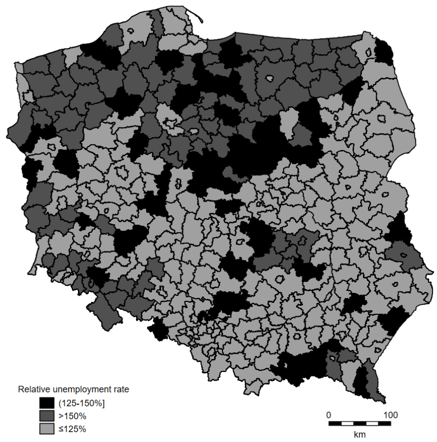
Appendix (for online publication)

A Figures

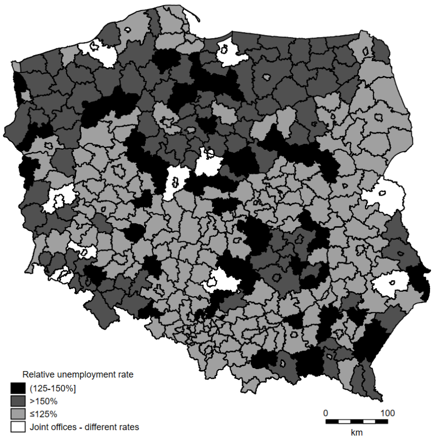
Figure A.1: County distribution of relative unemployment rates by year



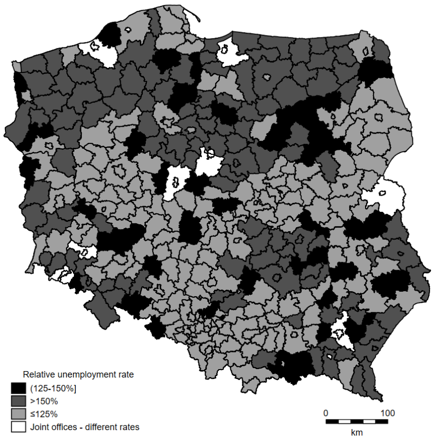
(a) 2006



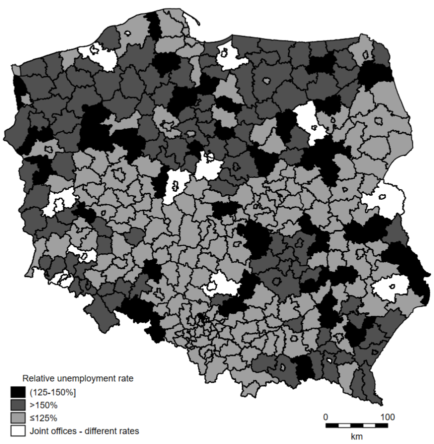
(b) 2007



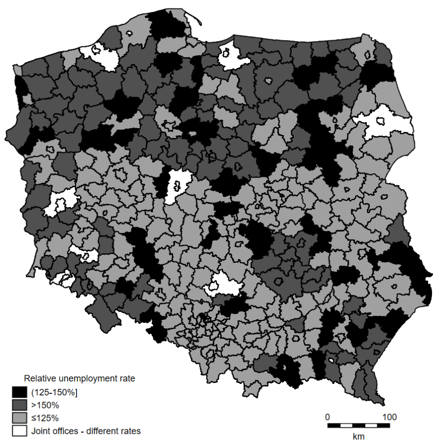
(c) 2007



(d) 2008



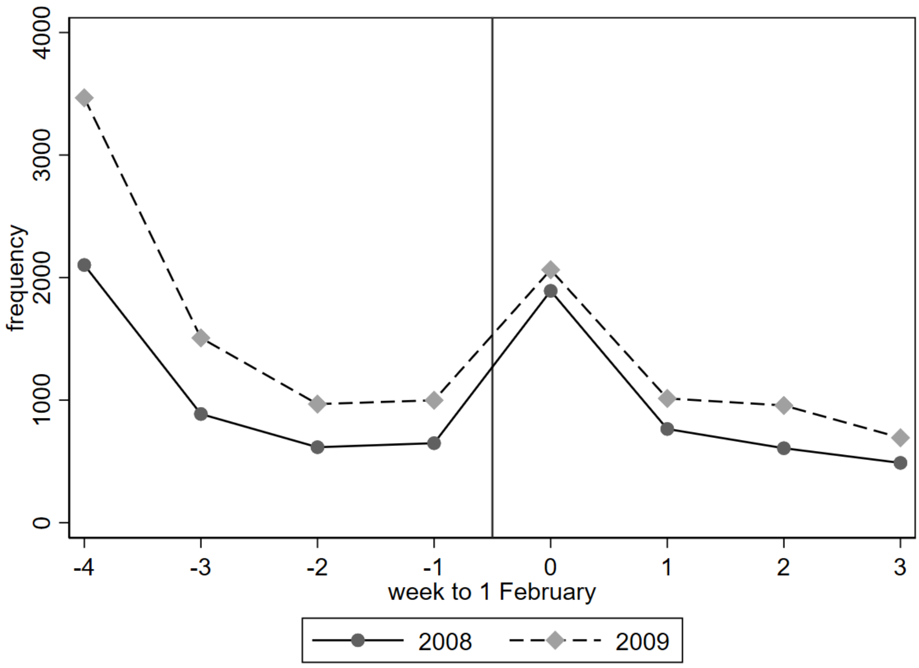
(e) 2009



(f) 2010

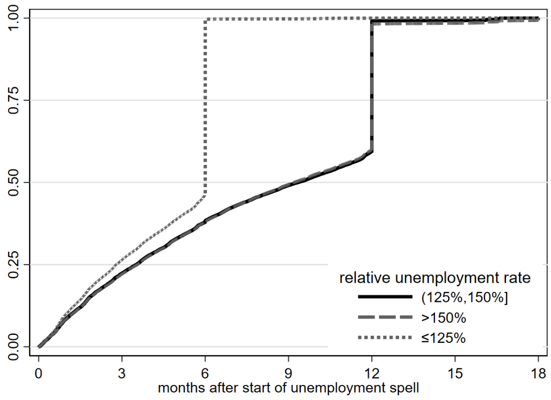
Note: Figures show the relative unemployment rates of Polish counties for 2005-2010 as of June 30, which determines PBD for the following year.

Figure A.2: Weekly start dates of unemployment spells

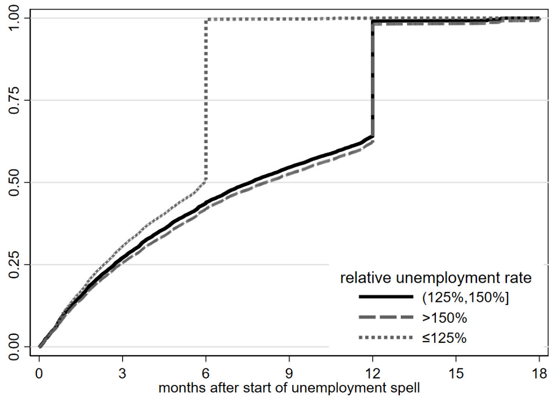


Notes: Figure shows weekly start dates of unemployment spells in 2008 and 2009 for counties with a medium relative unemployment rate, i.e. counties that were affected by the reform on 1 February, 2009.

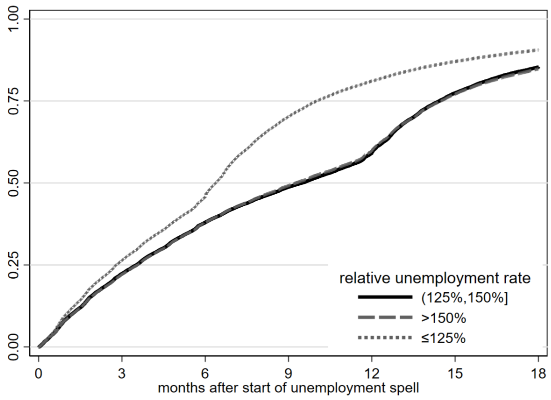
Figure A.3: Kaplan-Meier failure functions: 2006-2007



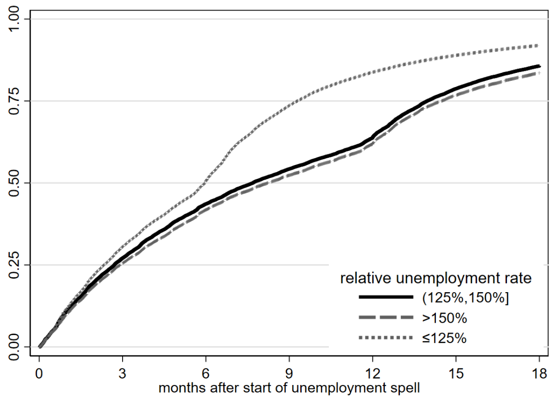
(a) Survival in benefits 2006



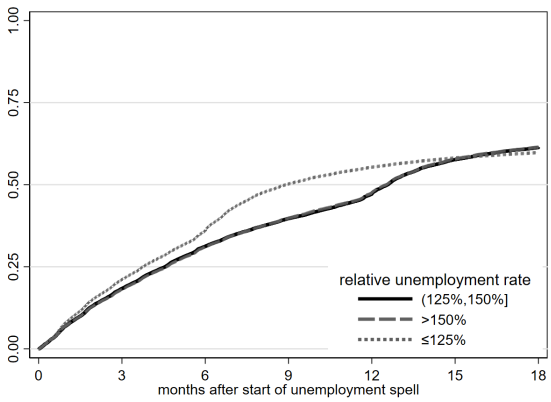
(b) Survival in benefits 2007



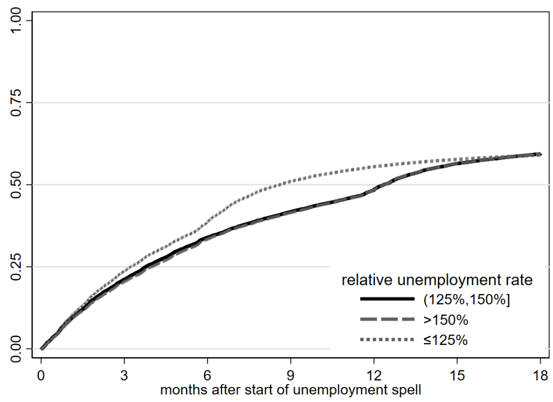
(c) Exit from unemployment 2006



(d) Exit from unemployment 2007



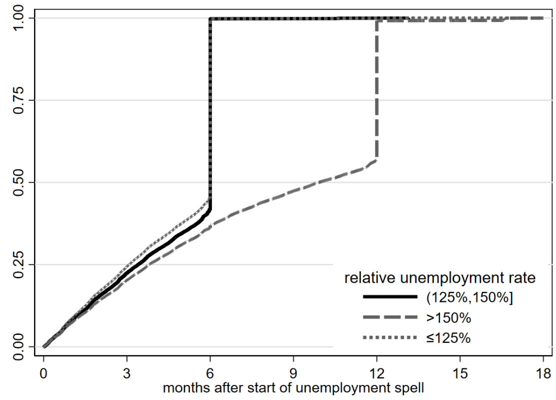
(e) Entry into employment 2006



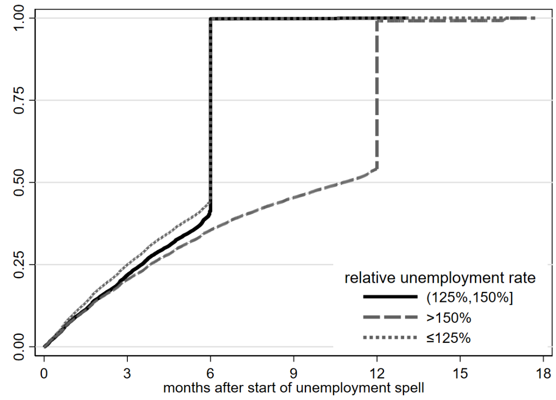
(f) Entry into employment 2007

Note: In both years January is excluded. Hazard rates for the years 2006, 2007, 2010 and 2011 are shown in Appendix Figure A.5.

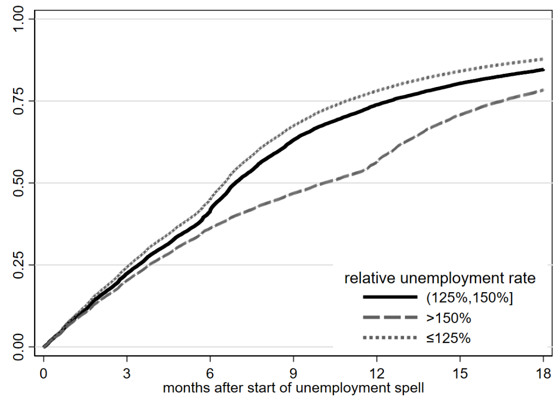
Figure A.4: Kaplan-Meier failure functions: 2010-2011



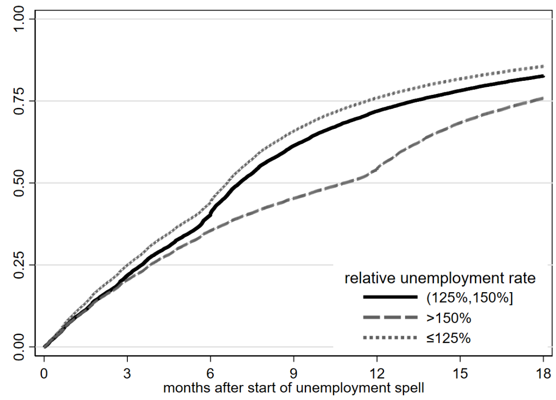
(a) Survival in benefits 2010



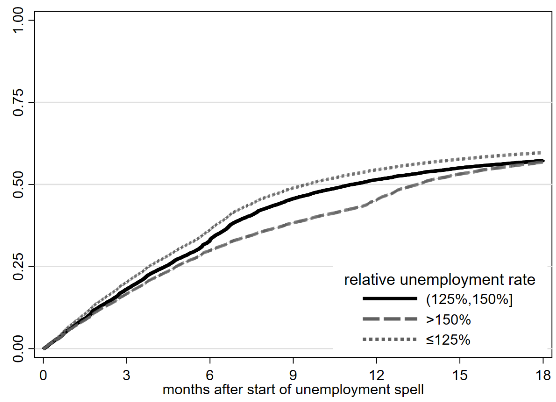
(b) Survival in benefits 2011



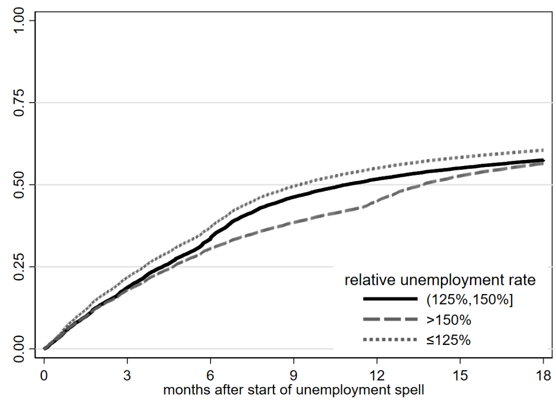
(c) Exit from unemployment 2010



(d) Exit from unemployment 2011



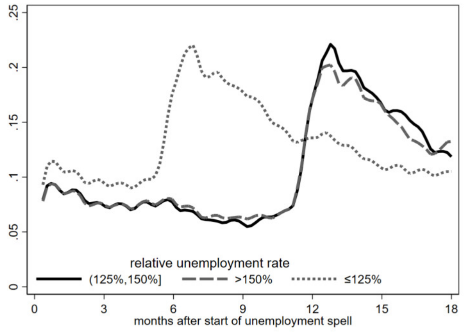
(e) Entry into employment 2010



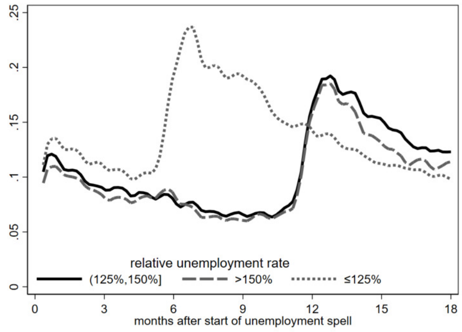
(f) Entry into employment 2011

Note: In both years January is excluded. Hazard rates for the years 2006, 2007, 2010 and 2011 are shown in Appendix Figure A.5.

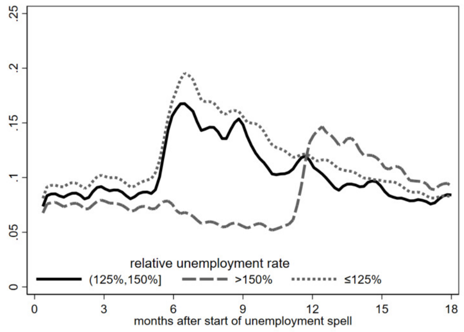
Figure A.5: Hazard rates - 2006, 2007, 2010 and 2011



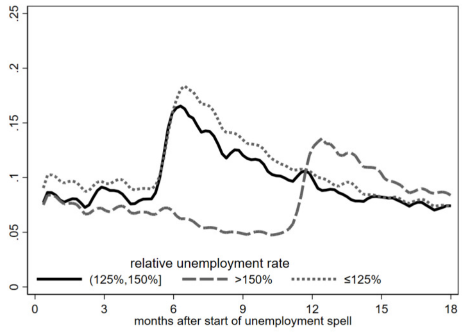
(a) Exit from unemployment 2006



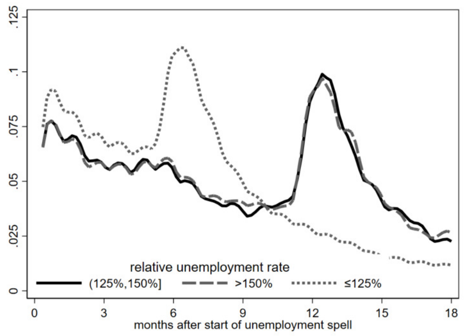
(b) Exit from unemployment 2007



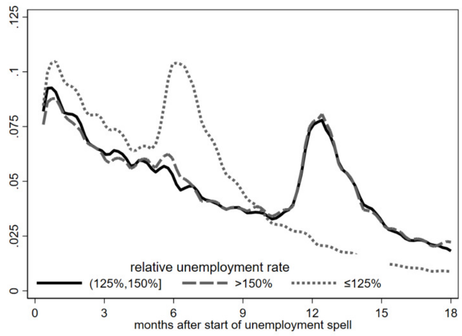
(c) Entry from unemployment 2010



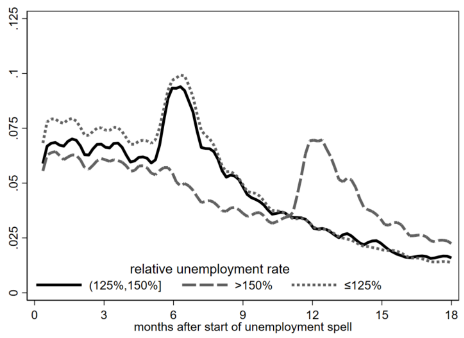
(d) Entry from unemployment 2011



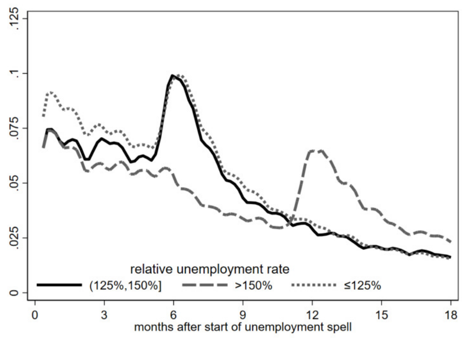
(e) Entry into employment 2006



(f) Entry into employment 2007



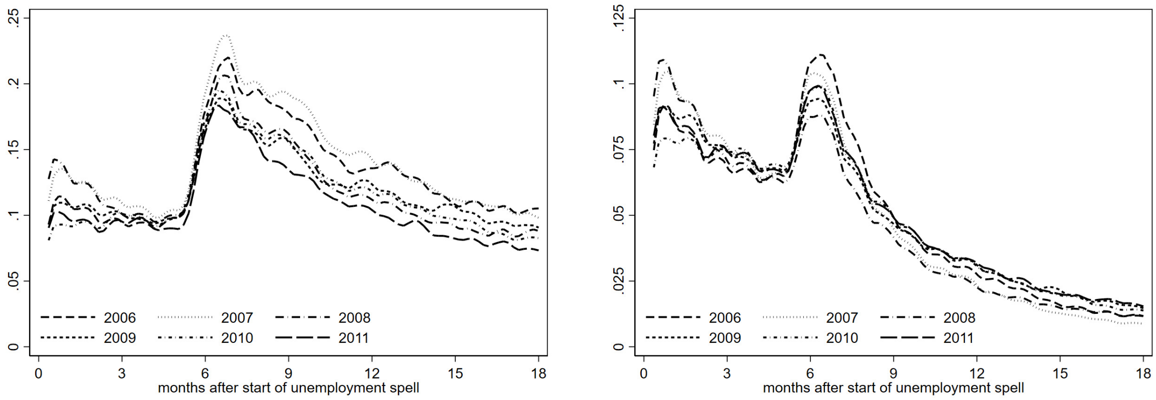
(g) Entry into employment 2010



(h) Entry into employment 2011

Note: Figure shows hazard rates of exiting unemployment and entry into employment. Excluding January. Hazard rates correspond to survival estimates from Figures A.3 and A.4.

Figure A.6: Hazard rates - by years for low unemployment counties

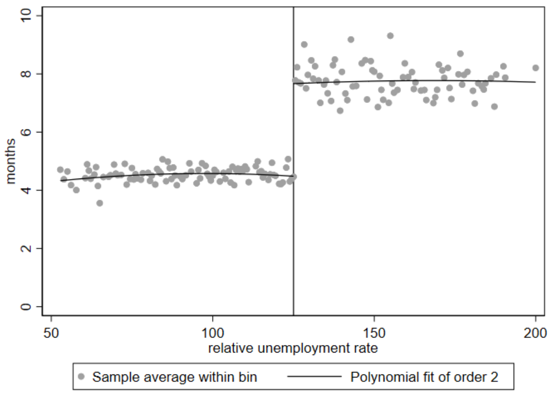


(a) Exit from unemployment

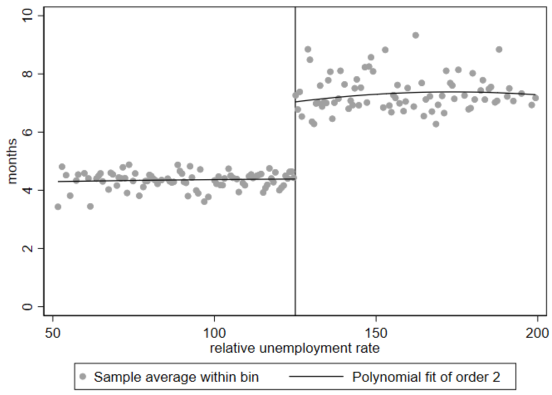
(b) Entry into employment

Note: Figure shows hazard rates of exiting unemployment and entry into employment by year excluding January. The sample is restricted to low-unemployment counties (≤125 per cent) which did not experience a decrease in PBD.

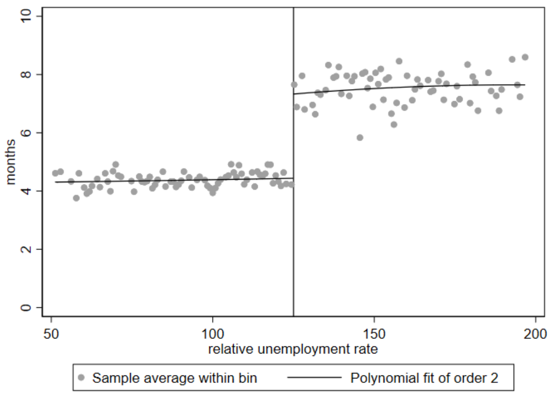
Figure A.7: Data-driven regression discontinuity plots - unemployment benefit duration



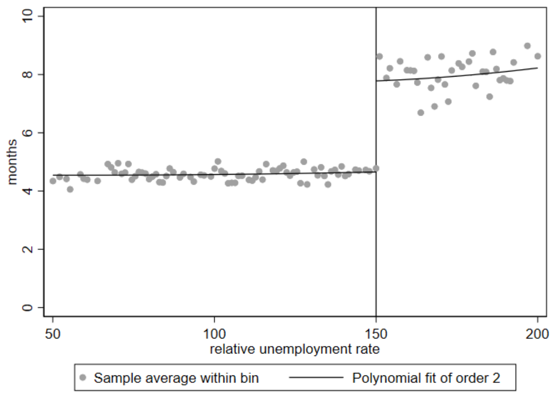
(a) 2006



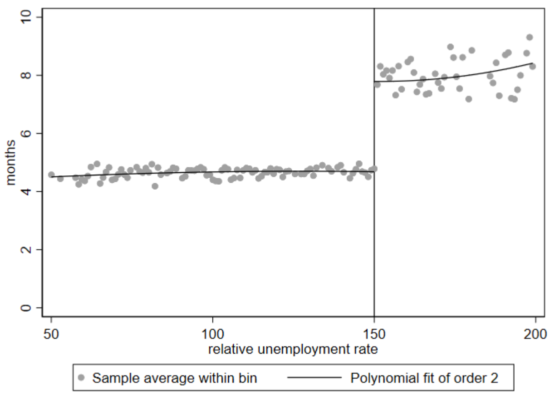
(b) 2007



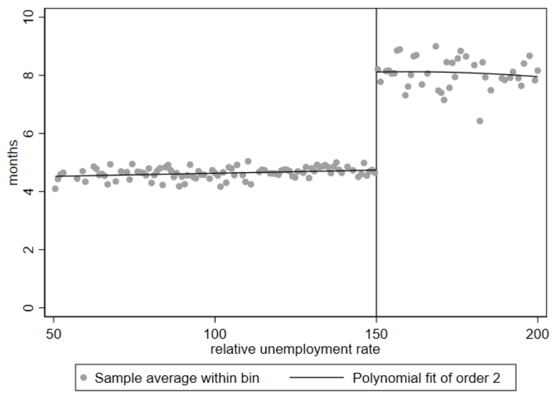
(c) 2008



(d) 2009



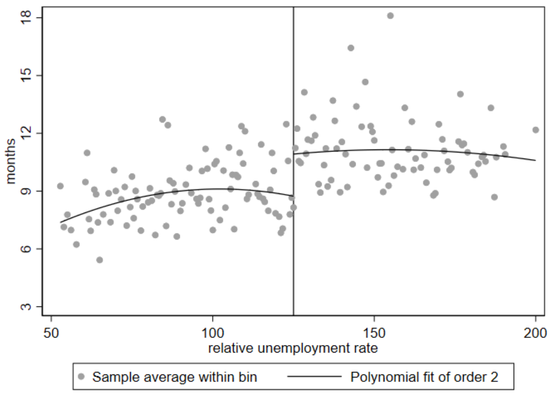
(e) 2010



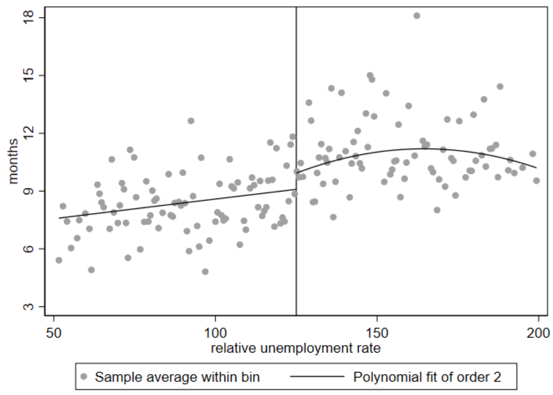
(f) 2011

Note: Figures show discontinuity plots following Calonico et al. (2015). For illustration purposes only counties within the relative unemployment range of 50 to 200 are shown.

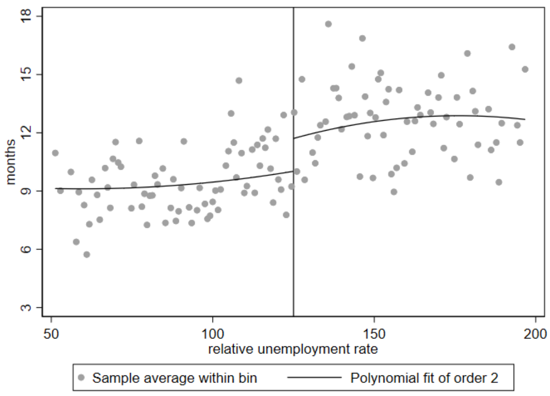
Figure A.8: Data-driven regression discontinuity plots - unemployment duration



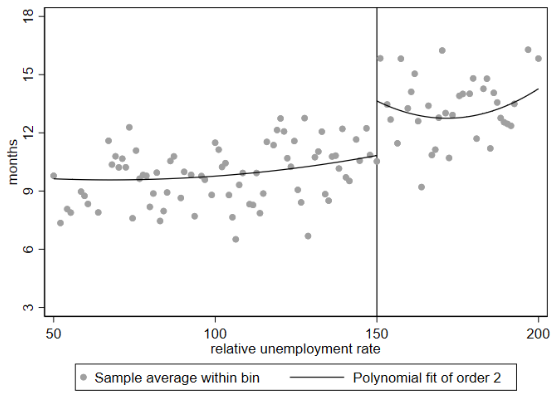
(a) 2006



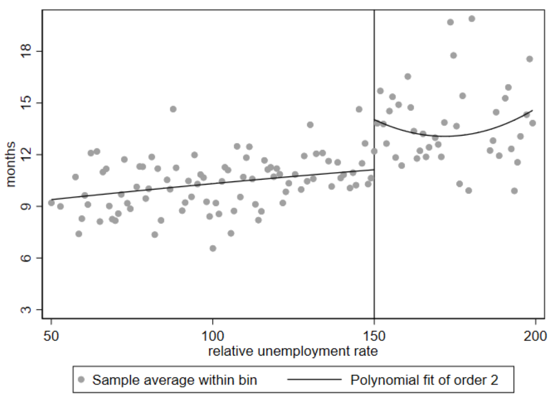
(b) 2007



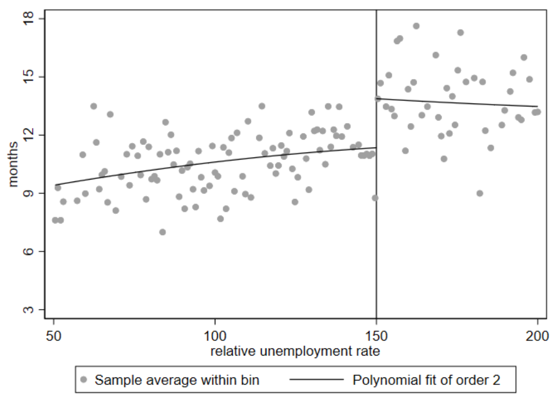
(c) 2008



(d) 2009



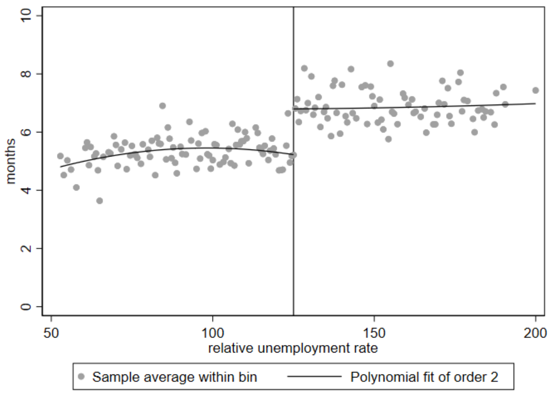
(e) 2010



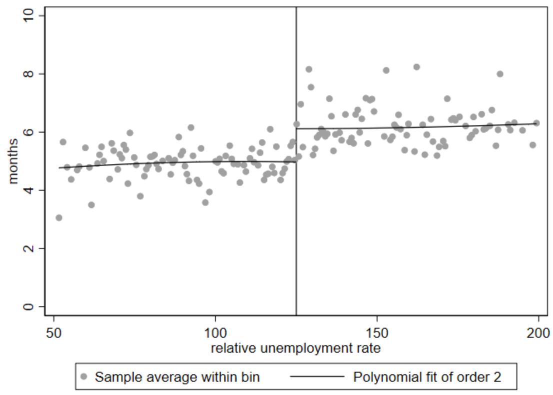
(f) 2011

Note: Figures show discontinuity plots following Calonico et al. (2015). For illustration purposes only counties within the relative unemployment range of 50 to 200 are shown.

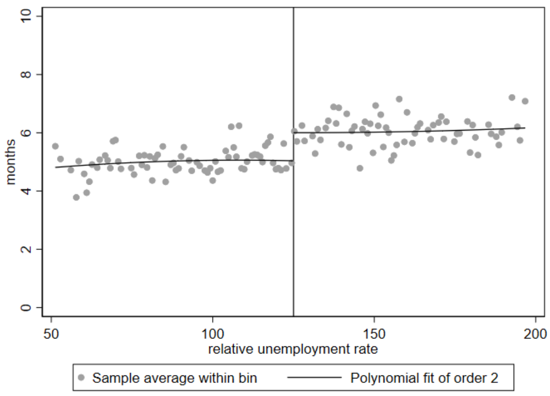
Figure A.9: Data-driven regression discontinuity plots -
entry into employment



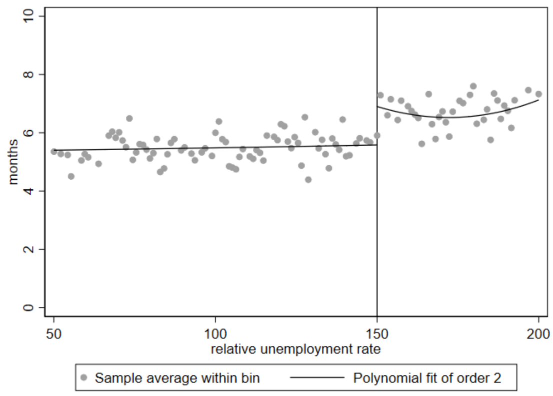
(a) 2006



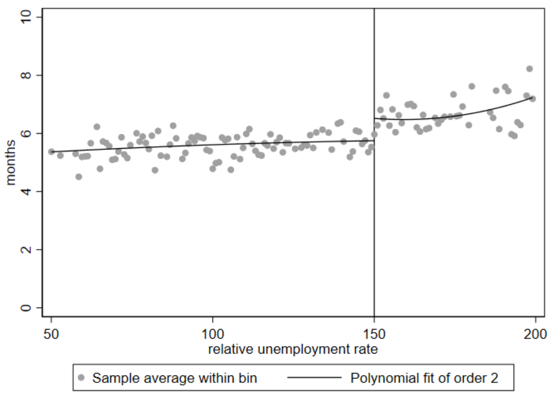
(b) 2007



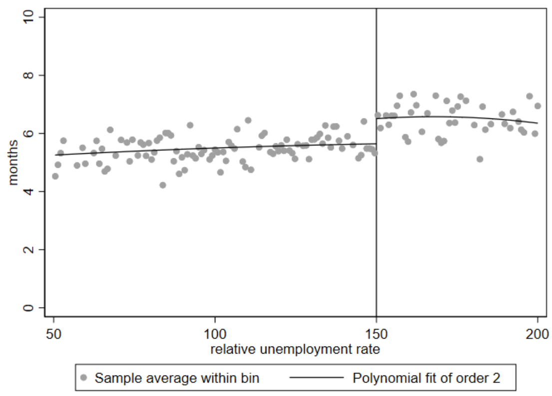
(c) 2008



(d) 2009



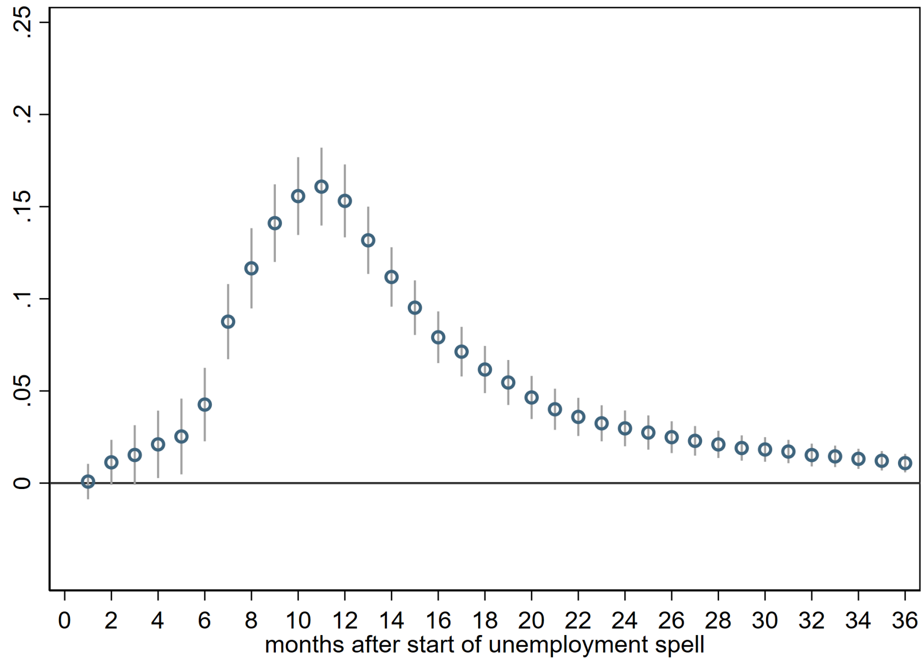
(e) 2010



(f) 2011

Note: Figures show discontinuity plots following Calonico et al. (2015). For illustration purposes only counties within the relative unemployment range of 50 to 200 are shown.

Figure A.10: Monthly effects on exiting unemployment



Notes: Figure plots monthly treatment effects based on equation (2) as shown in 3-month windows in Table 3. See table notes for further details. The sample is restricted to counties with a relative unemployment between 100 and 175% in 2008. Whiskers indicate 95 per cent confidence intervals.

B Tables

Table B.1: Cumulative probability of exiting unemployment benefits and unemployment, and of exiting into employment 2008 and 2009

	Exit from benefits			Exit from unemployment			Entry into employment		
	2008	2009	Diff.	2008	2009	Diff.	2008	2009	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Medium unemployment ((125%, 150%], *treated counties*)									
$\leq$ 3 months	26.8	24.0	-2.8	26.8	23.9	-3.0	21.0	19.6	-1.4
$\leq$ 6 months	42.2	91.7	49.5	41.9	42.9	1.0	32.9	34.6	1.7
$\leq$ 9 months	52.0	93.4	41.5	51.6	62.3	10.7	40.2	45.7	5.4
$\leq$ 12 months	99.0	99.9	0.9	59.9	72.7	12.8	46.0	51.0	5.0
$\leq$ 15 months	99.1	99.9	0.8	72.1	80.2	8.1	52.1	55.0	2.9
$\leq$ 18 months	100.0	100.0	0.0	79.8	85.2	5.4	55.4	57.5	2.1
in months	7.5	5.0	-2.4	12.4	10.9	-1.6	6.0	5.7	-0.3
High unemployment (> 150%)									
$\leq$ 3 months	25.5	20.9	-4.6	25.5	20.9	-4.6	20.4	17.2	-3.2
$\leq$ 6 months	41.5	36.8	-4.7	41.0	36.7	-4.3	32.7	30.2	-2.5
$\leq$ 9 months	51.2	47.6	-3.6	50.8	47.4	-3.5	40.1	38.5	-1.7
$\leq$ 12 months	97.6	99.0	1.3	59.1	56.7	-2.4	45.9	45.0	-0.8
$\leq$ 15 months	98.0	99.1	1.1	71.7	71.1	-0.6	52.4	52.4	0.0
$\leq$ 18 months	100.0	100.0	0.0	79.2	79.2	0.0	55.7	56.4	0.6
in months	7.6	8.0	0.4	12.6	13.3	0.7	6.1	6.7	0.6
Low unemployment ($\leq$ 125%)									
$\leq$ 3 months	31.7	27.0	-4.6	31.5	26.9	-4.6	24.6	22.2	-2.4
$\leq$ 6 months	99.4	99.7	0.3	50.8	47.2	-3.6	39.0	37.8	-1.2
$\leq$ 9 months	99.7	99.8	0.1	71.8	68.6	-3.2	49.9	49.7	-0.2
$\leq$ 12 months	100.0	100.0	0.0	80.8	78.6	-2.2	54.1	55.0	0.8
$\leq$ 15 months	100.0	100.0	0.0	85.8	84.6	-1.2	56.6	58.2	1.6
$\leq$ 18 months	100.0	100.0	0.0	89.1	88.4	-0.7	58.2	60.3	2.1
in months	4.3	4.5	0.2	8.9	9.5	0.6	4.8	5.3	0.5

Notes: Table shows average cumulative probabilities to have exited benefits and unemployment, and entered into employment. Respective last rows show average duration in months, for entry into employment this is conditional on return within 18 months. January is excluded in both years.

Table B.2: Effects of reducing the PBD by six months (RD sample) —flexible DD

	Exit from		Entry into
	benefits	unemployment	employment
	(1)	(2)	(3)
$\leq$ 3 months	0.0231***	0.0220***	0.0186***
	(0.0080)	(0.0080)	(0.0055)
$\leq$ 6 months	0.5576***	0.0487***	0.0309***
	(0.0138)	(0.0092)	(0.0068)
$\leq$ 9 months	0.4574***	0.1460***	0.0570***
	(0.0132)	(0.0098)	(0.0072)
$\leq$ 12 months	0.0027	0.1570***	0.0439***
	(0.0017)	(0.0097)	(0.0078)
$\leq$ 15 months	0.0022	0.1003***	0.0182**
	(0.0015)	(0.0078)	(0.0079)
$\leq$ 18 months	-0.0000	0.0638***	0.0015
	(0.0000)	(0.0067)	(0.0078)
duration in months	-2.9009***	-2.3408***	-0.9032***
	(0.1077)	(0.2391)	(0.0897)
Clusters	298	298	298
N	1708224	1708224	1011340

Notes: Table corresponds to Table 3 but with the same sample restrictions imposed as in the RD estimation in Table 5. See Table 3 for other notes. Standard errors, clustered at the employment office level, in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C Institutional details

There are some specific cases in which there are exceptions to the main regulations for unemployment benefits described in Section 2. For this reason we exclude individuals over the age of 50: people in this age group were eligible for 12 months of unemployment benefit receipt if they had worked for at least 20 years, irrespective of the relative unemployment rate of the county of residence. In counties with a relative unemployment rate of more than 200 per cent, the PBD for older workers who had worked at least 20 years was even longer, 18 months before February 2009, and was reduced to 12 months in the course of the reform.

Another special case are parents with overlapping unemployment spells. If a parent of at least one child below 15 years becomes unemployed, the same PBD as for other individuals applied. If the second parent became unemployed during the spell of the first parent, the second parent was eligible for benefits for 18 months (prior to the reform), or 12 months after the 2009 reform, irrespective of the relative unemployment rate of the county of residence.

In some cases, the PBD can be cut below the regular duration. For instance, the eligibility period could be cut by three months if a person quit a job instead of being fired. Due to this, we focus our analysis on individuals who were fired. Further, in the case of disciplinary dismissal or refusal to participate in an ALMP, the PBD could be cut by up to six months.

In addition to the reduction of the benefit *duration* in February 2009, benefit *levels* were adjusted in January 2010. Whereas the former part of the reform was only implemented in some counties, the latter part was implemented in the whole of Poland. While the benefit level was 575 Polish Zloty for every month of benefit receipt in 2009, it was raised to 717 for the first three months of benefit receipt and reduced to 563 Zloty for the remaining months in 2010.[20]

[20] Note that decreasing benefit levels during the jobless spell are in line with the prescriptions by Shavell and Weiss (1979) and Hopenhayn and Nicolini (1997).