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## The Micro and Macro Effects of Changes in the Potential Benefit Duration

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FEBRUARY 2023

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## ABSTRACT

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# The Micro and Macro Effects of Changes in the Potential Benefit Duration\*

We quantify micro and macro effects of changes in the potential benefit duration (PBD) in unemployment insurance. In Poland, the PBD is 12 months for newly unemployed if the previous year's county unemployment rate is more than 150% of the national average, and 6 months otherwise. We exploit this discontinuity using RD estimates on registry data containing the universe of unemployed from 2004 to 2020. For workers whose PBD is directly affected by the policy rule (benefit recipients younger than 50), a PBD increase from 6 to 12 months leads to 13 percent higher unemployment. The aggregate effect on unemployment is entirely explained by this increase. Thus, the micro effect equals the macro effect. We find no evidence of spill-overs on two distinct groups of unemployed whose PBD is unchanged and no effect on measures of labour market tightness. A decomposition analysis reveals that 12 months after an increase in the PBD, changes in exits from and entries into unemployment each contribute to about one half of the overall increase in unemployment.

**JEL Classification:** H55, J20, J65

**Keywords:** unemployment benefits, extended benefits, spell duration, separation rate, regression discontinuity

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# 1 Introduction

What is the effect of a change in the potential unemployment benefit duration (PBD) on the level of unemployment? Unemployment insurance duration can impact unemployment through three channels. First, it impacts individual job search effort. We refer to this as the micro effect. Second, it impacts labour market tightness (vacancies divided by aggregate search effort) and thus the job finding rate per unit of search. The sign of this effect is theoretically ambiguous. Third, it can affect the separation rate. The effects on these three channels together constitute the macro response. While there is ample evidence that an increase in the PBD leads to lower job search effort by the directly affected unemployed,<sup>1</sup> the evidence on the effect on aggregate labour market outcomes is mixed (e.g., Chodorow-Reich et al., 2019; Hagedorn et al., 2013; Johnston and Mas, 2018; Karahan et al., 2022; Lalive et al., 2015; Marinescu, 2017). A major challenge to identification is that the PBD is often endogenous to macroeconomic conditions, as is the case for the Extended Benefit programme in the US or for benefit extensions in many countries during the COVID-19 pandemic.

In this paper, we assess the importance of all three channels and quantify the effect of a higher PBD on aggregate unemployment. We use rich registry data of the universe of unemployment spells in Poland covering 2004 to 2020 to estimate the impact of changes in the PBD in Polish counties. These data enable us to provide evidence over a long time period for many different labour market outcomes, such as the stock of unemployed, benefit and unemployment duration, inflows into unemployment and measures of labour market tightness. For identification we leverage the unique Polish set-up, where the PBD of newly unemployed benefit recipients in a given year depends on the unemployment rate of the county of residence in the previous year relative to the national average. Specifically, if a county's unemployment rate on June 30 was above 150% of the national average, the PBD of eligible newly unemployed under 50 years of age in the following calendar year is 12 months, and 6 months otherwise.<sup>2</sup> The cut-off creates exogenous variation in the PBD between otherwise similar counties. We use a regression discontinuity (RD) estimation to

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<sup>1</sup> E.g., past studies have estimated effects on newly unemployed for specific US states (Card and Levine, 2000; Katz and Meyer, 1990; Landais, 2015), Germany (Caliendo et al., 2013; Hunt, 1995), France (Baguelin and Remillon, 2014; Le Barbanchon, 2016), Slovenia (van Ours and Vodopivec, 2006), Austria (Lalive, 2007, 2008; Lalive et al., 2015, 2006, 2011), Finland (Kyyrä and Pesola, 2020a,b), the Netherlands (de Groot and van der Klaauw, 2019), Spain (Rebollo-Sanz and Rodriguez-Planas, 2020) and Poland (Galecka-Burdziak et al., 2021).

<sup>2</sup>The threshold has been changed twice in our sample period, which we describe in more detail in section 2. Since 2009 it has remained constant at 150%. As regression discontinuity estimates identify local average treatment effects around the cut-off, our results concern counties in proximity to the thresholds, i.e. those with relatively high unemployment rates. But, as can be seen in Appendix Figure A.1, in our sample period substantial variation exists in the country-wide unemployment rate. E.g., in 2018 the unemployment rate in Poland was 4%, such that a relative unemployment rate of 150% corresponds to an unemployment rate of 6%. Consequently, our findings are not restricted to unfavourable economic conditions.

account for differences between counties linked to their relative unemployment rate with the only detectable discontinuity due to the threshold-induced difference in the PBD. We then construct impulse response functions to gauge the effects for six months prior to the PBD increase up to 24 months afterwards for a wide range of outcomes. This mimics the experiment of comparing a county where the PBD was increased to 12 months with a county with a PBD of 6 months, holding everything else constant.

We calculate aggregate labour market outcomes at the county-month-level from the daily individual unemployment spells. As these aggregate outcomes are constructed from individual spells, we can distinguish between outcomes for the *directly affected unemployed* (newly unemployed younger than 50 and eligible to benefits—one third of all unemployed) and the *indirectly affected* (older workers and those ineligible to receive benefits) to assess market externalities. These are important to consider for at least two reasons. First, it allows to calculate the effect of a hypothetical change in the PBD for all workers. Second, the optimal PBD over the business cycle depends on the effect of the PBD on labour market tightness (Landais et al., 2018b). Finally, we quantify the impact of a longer PBD on inflows into unemployment.

We find that the stock of all unemployed rises by 0.03 log points 12 months after an increase in the PBD from 6 to 12 months. This increase can be fully attributed to the 0.13 increase in the log stock of directly affected unemployed, corresponding roughly to an increase by 13 percent. The stock of benefit recipients, relevant to assess direct fiscal effects, increases by 0.59 log points. The unemployment duration of directly affected unemployed increases by 0.19 log points.

Looking at two distinct groups of indirectly affected unemployed, we identify no effect on their unemployment duration. These findings are in line with an increase in search effort by the directly affected and no change in labour market tightness. A change in tightness would affect the unemployed with no change in the PBD because it would directly affect their job finding rate (holding their individual job search effort constant). The absence of spill-over effects is corroborated by the fact that we see no changes in wages (one channel through which the PBD could affect tightness) and the vacancy-filling rate (a direct measure of tightness).

Finally, we document that inflows into unemployment increase strongly with a longer PBD. Thus, it is crucial to take this channel into account in order to inform policy. In addition, we see intertemporal substitution of inflows between periods with a PBD of 6 and those with a PBD of 12 months. This indicates that some workers respond strategically to changes in the PBD. We use our estimates to decompose the increase in the stock of unemployed into what can be attributed to effects on the inflow into unemployment and the exit rate from unemployment. For the directly affected group, the effect on inflows dominates immediately after the PBD increase. After 12 months both effects are roughly similar in size.

In general, the job finding rate depends both on individuals' search effort and on the success rate per unit of search, which is a function of labour market tightness.<sup>3</sup> The sign of the effect of PBD extensions on labour market tightness is theoretically ambiguous (Landais et al., 2018b). In models with diminishing returns to labour as a production factor and fixed wages, a rat-race effect occurs, where individuals looking for a new job displace other workers. In this case, the total effect of an increase in the PBD is smaller than the direct effect and potentially zero. In contrast, in the canonical model with Nash bargaining as in Pissarides (2000), an improvement of workers' outside options through an increase in the PBD boosts workers' bargaining position resulting in higher wages and thus fewer job openings (wage effect). In that case, the total (macro) effect of an increase in the PBD is larger than the direct (micro) effect as the reduced number of openings negatively affects the job finding rate of those not directly affected by the reform.<sup>4</sup> In contrast, our finding of no spill-over of PBD changes in Poland is in line with a horizontal labour demand curve as in Hall (2005).

The wedge between the micro elasticity of unemployment with respect to UI and the macro elasticity is of interest per se. Landais et al. (2018b) derive theoretically that the optimal replacement rate is a Baily-Chetty (Baily, 1978; Chetty, 2006) replacement rate—which captures the incentive-insurance trade-off, but does not take spill-over effects into account—plus a correction term including the elasticity wedge. When the elasticity wedge is positive, i.e. the micro elasticity exceeds the macro elasticity, an increase in UI raises labour market tightness. As tightness is commonly found to be inefficiently low in slumps and inefficiently high in booms, optimal PBD should then be countercyclical. As we identify no impact of PBD changes on labour market tightness, our findings imply that the optimal PBD in Poland does not vary over the business cycle.

The literature on aggregate effects of PBD changes focuses on benefit extensions in the US and finds mixed results (Boone et al., 2021; Chodorow-Reich et al., 2019; Dieterle et al., 2020; Hagedorn et al., 2013, 2015, 2019a; Johnston and Mas, 2018; Marinescu, 2017; Rothstein, 2011). To overcome the identification issue that the PBD is endogenous to macroeconomic conditions, various sources of variation have been used. Hagedorn et al. (2013) exploit discontinuities at state borders by comparing neighbouring counties with different policies in their states. They find large increases in unemployment in response to benefit extensions, reductions in vacancy creation and employment, and an increase in wages. Dieterle et al. (2020), however, argue that such boundary designs suffer from

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<sup>3</sup>This is illustrated in Hagedorn et al. (2013) who decompose the job finding rate into two elements:

$$\text{job finding rate}_{it} = \underbrace{s_{it}}_{\text{search intensity}} \times \underbrace{f(\theta_t)}_{\text{finding rate per unit of search}}$$

<sup>4</sup>The micro effect is the effect on unemployment durations that we would observe if the PBD was extended for only a small subset of unemployed in a given labour market. The macro effect is the change in unemployment durations if the PBD was changed for all unemployed in some labour markets, while it remained unchanged in others (see Landais et al., 2018a).

two biases and estimate much smaller effects (see also [Boone et al., 2021](#)). In contrast, [Chodorow-Reich \(2019\)](#) use errors in the measurement of the real-time data that determine benefit extensions and find no effect on state-level macroeconomic outcomes. In turn, this approach is criticised by [Hagedorn et al. \(2016\)](#) who argue that this strategy does not resolve the endogeneity problem.

[Johnston and Mas \(2018\)](#) study an unexpected cut of the PBD in Missouri in 2011, leading to a difference in the PBD of 16 weeks of claimants applying just before and after a cut-off date, and find no evidence for market-level externalities as the observed drop in the unemployment rate closely matches the drop predicted by individual-level estimates assuming no spill-overs. [Karahan et al. \(2022\)](#) show that the benefit cut affected equilibrium labour market conditions. In response to the benefit cut, job finding rates increased because both the search effort of unemployed workers and the availability of jobs increased. In contrast, [Marinescu \(2017\)](#) finds no effect of PBD extensions in several US states on vacancies posted.

Evidence from outside the US on aggregate effects of the PBD is scarce. [Lalive et al. \(2015\)](#) calculate the aggregate effect of PBD changes using estimates on the effects on directly and indirectly affected unemployed. They study a programme which extended benefits by three years in Austria, but only affected workers over 50 years old in some regions. Unemployment durations of the directly affected increased strongly, while they decreased by a smaller magnitude for ineligible unemployed, suggesting that equilibrium conditions for the latter group were affected. Consequently, the micro effect is larger than the macro effect. [Fredriksson and Söderström \(2020\)](#) study the macro and micro effects of UI replacement rates in Sweden. They make use of the fact that the national UI benefit formula features a benefit ceiling and thus the average replacement rate varies between regions and individuals. In opposition to the finding in [Lalive et al. \(2015\)](#), the estimated macro elasticity is about twice as large as the micro elasticity.

Our paper contributes to this literature by studying a novel setting using quasi-experimental variation in the PBD and rich administrative data to quantify direct and spill-over effects of PBD changes.

To fully capture the incentive effect of UI, it is important to also account for its impact on job separations. This element has largely been overlooked by the literature on aggregate effects of UI. [Hartung et al. \(2022\)](#) show that a German reform that reduced the maximum duration of unemployment assistance and reduced the benefit level after expiration of unemployment assistance substantially led to a decrease in the separation rate, which explains much of the decrease in German unemployment (see also [Dlugosz et al., 2014](#)). [Winter-Ebmer \(2003\)](#), [Kyyrä and Wilke \(2007\)](#), [Lalive et al. \(2015\)](#), and [Tuit and van Ours \(2010\)](#) all find sizeable effects of longer benefit durations for older workers, de facto early retirement schemes, on separation rates of affected workers. [Albanese et al. \(2020\)](#) show that the separation rate increases as soon as workers reach eligibility for UI.



We contribute to this literature by showing that changes in inflows into unemployment explain a large part of the total effect of PBD changes on unemployment.

The next section introduces the institutional set-up, and section 3 describes the data used in our analysis. We delineate the empirical strategy and present RD diagnostics in section 4. The main results are contained in section 5, followed by a decomposition of the total effects into effects on the exit rate and on inflows in section 6. We assess the robustness of our results in section 7. Section 8 concludes.

## 2 Institutional Set-up

After 1989, Poland shifted from a centrally planned to a market economy. Unemployment had barely existed in previous decades and was a new phenomenon to accommodate economically, socially and politically. There was no institutional infrastructure to handle this phenomenon and the unemployment benefit system had to be created from scratch. We plot the Polish unemployment rate since the early 1990s in Appendix Figure A.1. Unemployment in Poland peaked at above 20% in the early 2000s which led to several reforms of the unemployment system. The unemployment rate dropped dramatically from the mid-2000s onward and Poland now, as of 2023, has one of the lowest unemployment rates among OECD countries.

In Poland, the PBD is determined at the time an individual registers as unemployed. The PBD depends on the unemployment rate of the county (powiat) of residence<sup>5</sup> in June<sup>6</sup> of the previous year *relative to the country-wide mean*. If the county unemployment rate is above a certain threshold, the PBD for eligible prime-age workers is 12 months, and 6 months otherwise. The system shares some similarities with the Extended Benefits programme in the US where the PBD can be extended in states when unemployment is high and growing (Rothstein, 2011), which led to state-level variation in PBD in the 2007-2009 recession. As shown in Landais et al. (2018b), it might indeed be optimal that the transfer system is more generous in economic slumps. A crucial difference in the Polish system is that the threshold for receiving a higher PBD depends on the *relative* unemployment rate of a county and not on *absolute* macroeconomic circumstances. This set-up leads to heterogeneous PBDs across the country in every year regardless of the overall state of the economy. Another key difference is that in the US changes in the PBD can be applied retrospectively to those already receiving benefits, whereas in Poland the PBD is determined at the start of the benefit spell.

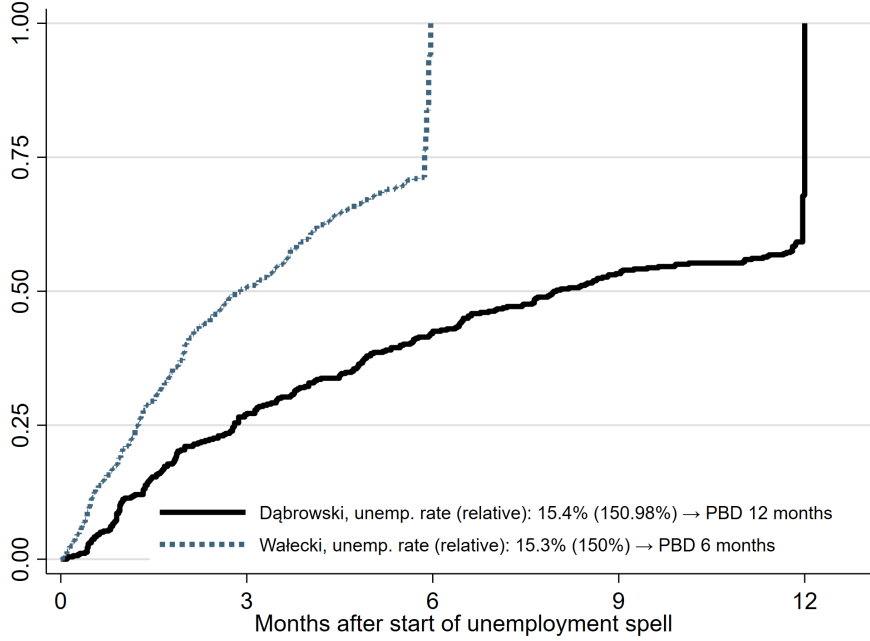
The threshold of the relative unemployment rate at which the PBD in a county is 12

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<sup>5</sup>In principle, workers could move to a county with a higher PBD, but as a proof of residence such as a rental contract is required, the cost of actually moving would typically exceed the gain of a 6 months longer PBD.

<sup>6</sup>The revised and PBD-determining June unemployment rates for Poland and each county are announced in September.

Figure 1: Exit from unemployment benefits in Dąbrowski and Wałęcki



*Notes:* The figure shows Kaplan-Meier failure functions for exiting benefits in two counties in 2016. Unemployment rates refer to June 30, 2015. Benefit recipients in Dąbrowski could receive 12 months of benefits as their county had just exceeded the threshold of 150%. Recipients in Wałęcki could receive only 6 months. Sample consists of benefit recipients under 50 who were laid off (*eligibles*).

months has been raised several times. Up to June 2004, the threshold was 100% and was then increased to 125%. In February 2009, it was further increased to 150%, which is still the relevant cut-off as of 2023. Thus, a PBD of 12 months applied to an increasingly lower share of counties—and unemployed individuals. Appendix Figure A.2 illustrates the set-up over time. For newly unemployed to receive 12 months of benefits, their county of residence must have exceeded the threshold on June 30 of the preceding calendar year. The PBD is then constant for all newly unemployed of a county per year.

To illustrate this set-up, consider the year 2016, where the PBD depends on the relative unemployment rate of June 2015. The average unemployment rate in Poland was 10.2% in that month. In the county Wałęcki the unemployment rate was 15.3% and in the county Dąbrowski it was 15.4%. The macroeconomic conditions in these two counties were thus almost identical, but as the relative unemployment rate in Wałęcki was 150% and the one in Dąbrowski 150.98%, benefit recipients in Wałęcki were eligible for 6 months of benefits compared to 12 months in Dąbrowski. Figure 1 shows Kaplan-Meier failure functions for exiting benefit receipt for these two counties. Despite the very similar unemployment rates in the previous year, benefit recipients in Wałęcki collected benefits for a much shorter time period. We exploit this sharp discontinuity in PBD around the threshold in an RD framework (see section 4).

Figure 2 shows for all 380 counties whether their relative unemployment rates were

above or below the cut-off in June 2015, which determines the PBD for 2016. Counties with lower or equal to 150% of the national mean are coloured light grey and the counties with rates above 150% of the mean in dark blue. Counties with a PBD of 12 months are more frequently located in the north of Poland. Importantly, regional spill-overs of PBD increases are unlikely to play a relevant role in our context. According to the Polish Labour Force Survey, around 80% of workers work in their municipality of residence. The average county consists of seven municipalities; commuting to neighbouring counties or beyond is thus relatively uncommon.

As the PBD is determined every year anew depending on the national and county unemployment rate, the PBD for new benefit recipients can change in every year, leading to benefit recipients registering in the same county in December or January potentially having different PBDs. While counties far away from the cut-off are unlikely to cross the threshold regularly, leading to a change in the PBD, for counties in proximity to the threshold this could happen regularly. Over the time period we consider, we find that less than half of all counties have the same PBD in every year.

The *level* of unemployment benefits is unaffected by the regional relative unemployment rate. Benefits in Poland are less generous than in most OECD countries. After 6 months of unemployment, average unemployment benefits relative to the previous income are 37%, ranking 33rd out of 40 countries, as of 2021.<sup>7</sup> In the first three months of unemployment, minimum benefits in 2023 amounted to 1043.3 Złoty (211.65 euro) and can rise up to 1565 Złoty for unemployed with longer work experience. For the remaining months, benefits are reduced by around 21.5%.<sup>8</sup> The unemployed in Poland have additional incentives to be and remain registered, as it guarantees free health insurance (if ineligible otherwise), access to job search assistance, and to receive social assistance benefits a proof of unemployment is required. Unlike in other institutional settings, it is accordingly unlikely that deregistering from the unemployment register usually is a transition into non-employment or inactivity. Of the unemployed eligible to receive benefits, around two-thirds directly enter into employment when leaving the unemployment register.

There are some notable exceptions to the institutional rules which are relevant for our analysis.<sup>9</sup> First of all, not all newly unemployed are eligible to receive benefits. Of the 34 million unemployment spells we observe from 2004, 28.2% are eligible to receive benefits.

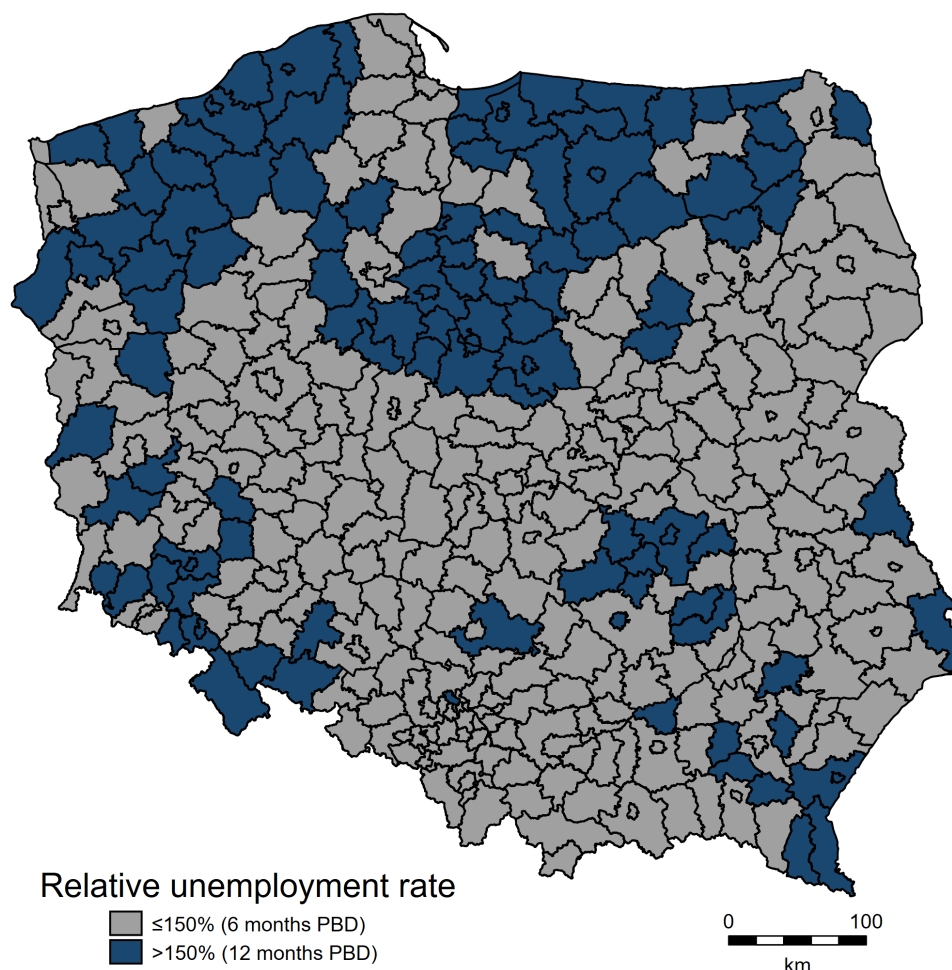
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<sup>7</sup><https://data.oecd.org/benwage/benefits-in-unemployment-share-of-previous-income.htm>

<sup>8</sup>After benefit expiration, one can receive social assistance benefits which are subjected to means-testing with regard to the family income. Maximum social assistance benefits are always lower than minimum unemployment benefits.

<sup>9</sup>A special, but rare, case occurs when parents' unemployment spells overlap. If a parent of at least one child under 15 years becomes unemployed, the same PBD as for other unemployed in the county is 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 February 2009, and 12 months thereafter, irrespective of the relative unemployment rate of the county of residence.

Figure 2: County distribution of relative unemployment rates in June 2015



*Notes:* The figure shows the unemployment rates of Polish counties for 30 June 2015 relative to the country mean. These determine the PBDs in 2016. Counties with a relative unemployment rate of  $\leq 150\%$  have a PBD of 6 months in 2016, county with a relative unemployment rate of  $>150\%$  have 12 months.

To be eligible for benefits, unemployed workers must have been in employment for at least 12 of the past 18 months and must have earned at least the minimum wage with social security contributions being paid. Additionally, different rules apply for workers above 50 years of age if they had at least 20 contributory years; since February 2009 these older workers have a PBD of 12 months irrespective of the relative unemployment rate of their county.<sup>10</sup> We use these two groups of unemployed for whom their counties' relative unemployment rates does not affect PBD, ineligible for benefits and older workers, to examine market externalities of a longer PBD.

In some cases, the PBD can also be cut below the regular duration. For instance, the eligibility period is cut by three months if a person quit a job instead of being laid off by their employer. In most analyses we restrict our sample to unemployed who were laid off, around 73% of all benefit recipients. The motivation for this is that for laid off workers

<sup>10</sup>Before February 2009, older unemployed could receive 18 months of benefits if their county's relative unemployment rate exceeded 200% and they had at least 20 years of contributory spells.

the increase in the PBD is always from 6 to 12 months. With this uniform increase we can directly calculate the elasticities of unemployment and benefit duration to the PBD.<sup>11</sup> We also include quitters in some analyses, as depending on the relative unemployment rate they have a PBD of either 3 or 9 months, i.e. the same difference of 6 months exist depending on whether the unemployed live in counties below or above the threshold.

### 3 The Data

**Individual Data.** We use administrative data of the universe of unemployment spells in Poland from January 2004 to July 2021 as our main data source. Unemployment spells are registered at the daily level, i.e. the precise start and end date of the unemployment spell is observed and, for those who are eligible for benefits, also the start and end date of benefit receipt. The data contain a total of more than 34 million unemployment spells, of which 9.6 million include a period of benefit receipt. The spells indicate the pre-unemployment status of individuals (e.g., employment, parental leave, imprisonment) as well as the exit state that individuals move into after unemployment (e.g., regular employment, active labour market policies or retirement). We also observe the date of birth and years of contributory spells, both which are relevant to determine the PBD of individuals. Through the county of residence of the unemployed at the time of registration we can calculate the PBD for all newly unemployed. As is common in administrative data, other individual background characteristics are limited and we only have information on the sex and highest schooling degree obtained.

We use the individual unemployment spells to calculate aggregate outcomes at the county-month-level. We define the stock of unemployed in each county as the total number of unemployed who are registered at public employment offices at the beginning of each month. We also calculate total inflows into and outflows from unemployment by summing all individuals who either register or de-register in a given month. The change in stock between two months in a county can then be decomposed into inflows and outflows, allowing us to directly speak to the channels through which a higher PBD may affect the stock of unemployed: the number of inflows into unemployment and the exit rate. This would not be possible with aggregate data of the unemployment rate or the stock of unemployed. We also obtain the average unemployment duration per month for all individuals who have started an unemployment spell.

We additionally calculate some aggregate outcomes for benefit recipients only. As for all unemployed, we derive the stock, inflows and outflows, but also the duration of the benefit spell in addition to the duration of the entire unemployment spell. This outcome is of importance from a policy perspective as it is informative of the effect on public

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<sup>11</sup>In later analyses we will provide evidence that in at least some cases the timing of dismissals also appears to be responsive to the PBD.

finances.

As not all individuals are impacted similarly by changes in the PBD (see section 2), we construct the aggregate outcomes for five groups based on individual characteristics of the unemployed—another advantage of being able to derive aggregate outcomes from individual spells. In particular, we consider (i) all unemployed, (ii) unemployed ineligible to receive benefits and under 50 years of age, (iii) benefit recipients under 50 who quit or were laid off, (iv) benefit recipients under 50 who were laid off, (v) benefit recipients aged 50 or above with at least 20 contributory years.<sup>12</sup> We are then able to consider total effects, (i), assess market externalities by looking at two distinct groups unaffected by PBD regulations, (ii) and (v), and to look at individuals directly affected, (iii) and (iv). In large parts of the paper, we concentrate our analysis on groups (i) and (iv). For ease of notation, we refer to group (iv) simply as “eligibles”, although groups (iii) and (v) are strictly speaking also eligible for benefits.

Panels A and B of Table 1 show summary statistics for the county-level outcomes that we have constructed from the individual unemployment spells. Column (1) pools all counties, columns (2) and (3) distinguish by the PBD of a county in the year the outcomes are measured. In Panel A we consider all unemployed. The average unemployment duration is 9 months, and the duration is more than a month longer in counties with 12 months PBD compared to counties with a PBD of 6 months. The stock of unemployed per county is 4782 on average. Smaller, idiosyncratic fluctuations in the stock of unemployed, say due to one smaller plant opening or closing down, can quickly change the unemployment rate and then determine whether a county will have a different PBD.<sup>13</sup> Inflows into and outflows from unemployment are 450 and 479 individuals per month, respectively. The exit rate, calculated as the monthly share exiting unemployment, is 11% on average and 2 percentage points lower in counties with 12 months PBD.

Panel B focuses on eligibles. These are individuals whose PBD is directly affected by whether a county is above or below the annual cut-off. For this subgroup the unemployment duration varies much more depending on the PBD than is the case for all unemployed. The second row of Panel B shows the benefit duration, which on average is about half of the total unemployment duration. Consequently, the exit rate from benefit receipt is much higher than the exit rate from unemployment. The exit rate from benefit receipt is almost twice as large in counties with a 6 months PBD compared to those with 12 months. While this is highly suggestive of a strong influence of a county’s PBD on the unemployment spells of individuals, we do not make a causal claim based on these

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<sup>12</sup>The shares of groups (ii) to (v) relative to all unemployed are 56.9%, 33.4%, 29% and 1.3%, respectively. The remaining share of all unemployed are benefit recipients, who had received benefits just before a brief employment spell and therefore are not eligible for full benefits, as well as older unemployed ineligible for benefits or with fewer than 20 years of contributory spells.

<sup>13</sup>With the average unemployment rate of 14.32% and given the average workforce, an increase in the stock of unemployed by 27 workers is sufficient to raise the unemployment rate by 0.1 percentage points.

Table 1: Summary statistics—county-level data

	All	6 months PBD	12 months PBD	N
		Means		
	(1)	(2)	(3)	(4)
<hr/> Panel A: All unemployed <hr/>				
Unemployment duration (months)	9.05	8.64	9.87	74646
Stock of unemployed	4781.93	4413.59	5513.46	74375
Inflows into unemployment	450.09	449.52	451.25	74646
Outflows from unemployment	478.86	475.50	485.58	74646
Exit rate from unemployment	0.11	0.11	0.09	74375
<hr/> Panel B: Eligibles <hr/>				
Unemployment duration (months)	10.06	9.15	11.86	74646
Benefit duration (months)	5.47	4.44	7.53	74646
Stock of unemployed	991.06	876.56	1219.62	74646
Stock of benefit recipients	440.85	360.05	602.15	74646
Inflows into unemployment	72.43	71.60	74.06	74646
Outflows from benefit receipt	75.08	74.58	76.08	74646
Outflows from unemployment	81.82	79.99	85.49	74646
Exit rate from unemployment	0.09	0.10	0.08	74646
Exit rate from benefit receipt	0.18	0.21	0.13	74646
<hr/> Panel C: Other outcomes <hr/>				
Unemployment rate	0.14	0.11	0.21	74646
New vacancies posted	262.38	296.93	180.36	42978
Stock of vacancies	185.78	217.87	109.57	42978
Wage of job offers in Złoty	2195.29	2220.86	2134.65	27141
<hr/> Panel D: Other variables <hr/>				
Population	100721.91	115150.84	71917.93	74646

*Notes:* The table shows summary statistics for county-level characteristics at the monthly level from January 2004 to June 2020. Monthly data on vacancies available for 2011-2020, and wages of job offers for 2014-2020. Eligibles refer to unemployed eligible for benefit receipt who are under 50 years and were laid off.

summary statistics.

We construct additional individual-level outcomes such as the durations of unemployment and benefit spells. From those, we define binary indicators for having exited unemployment, benefit receipt or entered into employment at different points in time after the start of an unemployment spell (see [van Ours and Vodopivec, 2006](#)). Appendix Table [B.1](#) displays summary statistics of individual spells.<sup>14</sup> Exit into employment is defined by the direction of the outflow, i.e. the status following the unemployment spell. We only observe the first exit state from unemployment and cannot follow individuals thereafter.

<sup>14</sup>Unfortunately, data on employment and labour force participation at the county-month level is not available.

**Aggregate Data.** Besides the county-level outcomes generated from individual spells, we also use a range of aggregated outcomes that are provided by public employment offices and statistical agencies. Note that we cannot distinguish between directly and indirectly affected groups for these outcomes. We consider the monthly unemployment rate as an outcome in our empirical analysis. The result is in line with that for the stock of all unemployed, but the advantage is that the estimate can be replicated without access to the individual-level registry data. Almost by design, the monthly unemployment rate is substantially higher in counties with a longer PBD (Panel C of Table 1).

Public employment offices post job vacancies, which job seekers have access to and through which the public employment offices can propose job matches. Relative to the monthly inflows of unemployed, a little less than one vacancy is posted for every two unemployed. The end of month stock of vacancies is lower than new postings, indicating that many new postings are quickly removed again from the register when they are filled. For job offers starting in 2014, we also have access to a lower bound of the wage offered by firms. With this outcome we can directly test for the wage effect of a higher PBD; a higher PBD improves the outside option of unemployed workers and increases their bargaining power and thus potentially wages. This mechanism would lead to a drop in labour demand (Hagedorn et al., 2019b; Lalive et al., 2015; Landais et al., 2018b; Pissarides, 2000). Additionally, we consider *annual* average wages on the county level provided by Statistics Poland.

We use additional county-level statistics provided by Statistics Poland in subsection 4.2 to show that county characteristics are balanced smoothly around the cut-off.<sup>15</sup>

## 4 Empirical Strategy

To motivate our RD estimation, Figure 3 plots the distribution of the average benefit and unemployment duration as well as the share with unemployment durations up to 12 months for newly unemployed individuals eligible to receive benefits. Each circle represents a one-percent bin of month-county observations of the running variable centred around the cut-off pooled over the entire sample period. At the cut-off, the average benefit duration jumps by 2.9 months from a baseline of around 4.5 months (Panel (a)). The average benefit duration in counties above the cut-off, where individuals can receive benefits for up to 12 months, exceeds the maximum benefit duration of 6 months in the counties below the cut-off. Average unemployment duration, shown in Panel (b), is substantially higher and, as expected due to worse labour market conditions, increases with the unemployment rate (below the cut-off). We again observe a large jump in the average unemployment duration of 1.9 months at the cut-off. In Panel (c) we also report

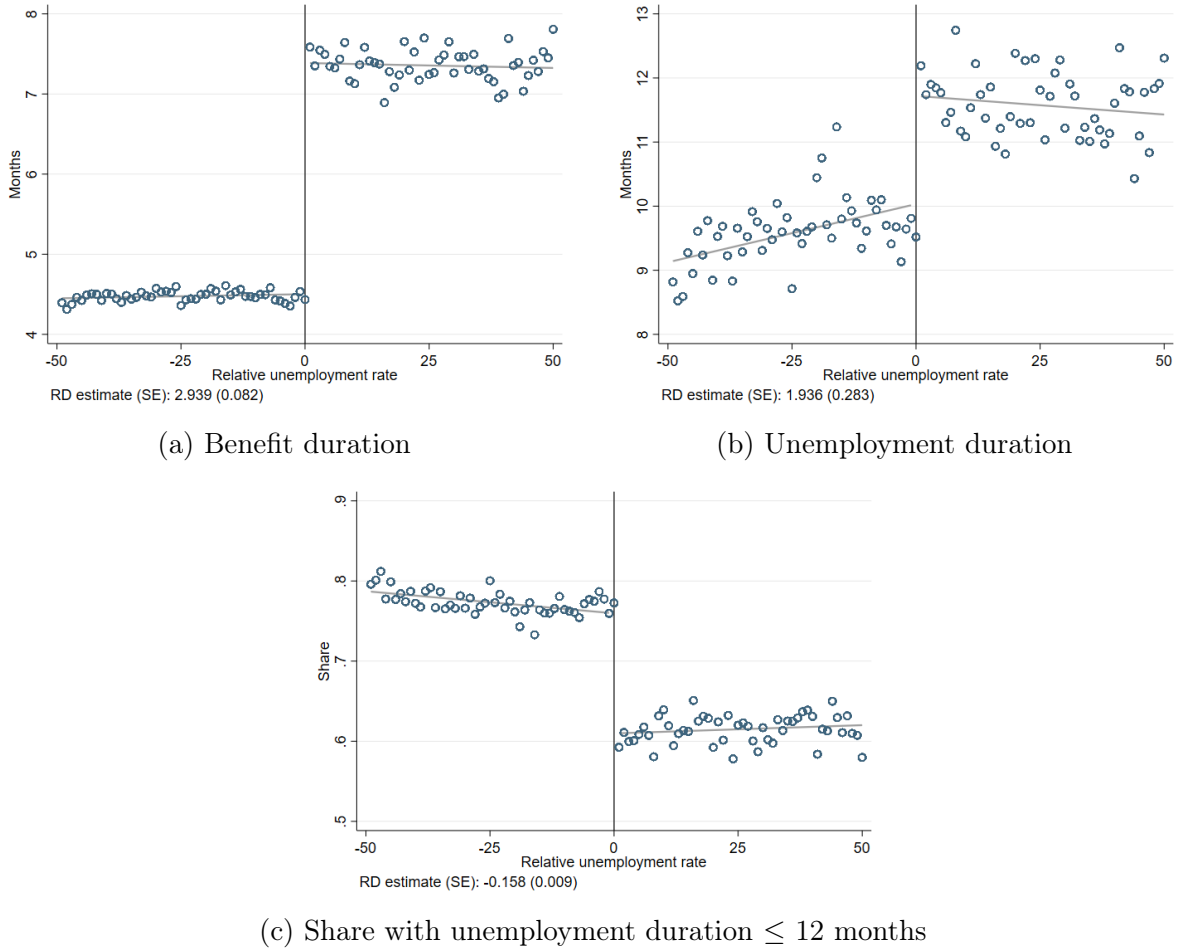
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<sup>15</sup>County-level data from Statistics Poland can be downloaded without registration and free of charge at <https://bdl.stat.gov.pl/bdl/start> (last accessed February 28, 2023).



results for a binary outcome that unemployment duration is 12 months or lower and find a drop in the share of 16 percentage points. In Appendix Figures A.3 and A.4 we show that this pronounced relationship between benefit or unemployment duration and the relative unemployment rate of a county holds in every year covered in our analysis.

Figure 3: Benefit and unemployment duration of eligibles



*Notes:* The figure shows how benefit duration and unemployment duration relate to the relative unemployment rate of the previous year and the cut-off for benefit recipients. The relative unemployment rate is centred around the threshold for a higher PBD (150% from February 2009 onward, 125% before that). Each circle represents a one-percent bin of month-county observations of the running variable. RD estimate obtained following [Calonico et al. \(2014\)](#) with a symmetric bandwidth of 50 and a linear polynomial. Sample period is 2005 to 2019. Eligibles refer to unemployed eligible for benefit receipt who are under 50 years and were laid off.

The basic intuition of RD estimates is that observations directly below and directly above the cut-off are similar and systematically only differ in the PBD. Our empirical strategy is to use this exogenous variation in the PBD around the cut-off in order to construct impulse responses to an increase in the PBD from 6 to 12 months. The effects estimated via RD can be interpreted as local average treatment effects around the cut-off ([Lee and Lemieux, 2010](#)).

## 4.1 Empirical Method

**Estimation Equation.** We now describe our estimation equation for aggregate effects<sup>16</sup> and go on to show RD diagnostics to validate the main identifying assumption in subsection 4.2. For outcome  $Y$  of county  $i$  in month  $t$ , we estimate the following equation:

$$\begin{aligned}
 Y_{i,t} = & \delta_0 + \sum_{j=0}^{12} f_{1,j}(r_{i,t-j}, PBD12_{i,t-j}) + \sum_{j=0}^{12} \delta_{1,j} PBD12_{i,t-j} \\
 & + \sum_{j=1}^6 f_{2,j}(r_{i,t+j}, PBD12_{i,t+j}) + \sum_{j=1}^6 \delta_{2,j} PBD12_{i,t+j} \\
 & + \sum_{j=1}^{12} \delta_{3,j} Y_{i,t-j} + time_t + county_i + \epsilon_{i,t}.
 \end{aligned} \tag{1}$$

$PBD12$  is a binary variable indicating whether a county’s PBD is 12 months (rather than 6 months). The first sum includes polynomials of the contemporaneous running variable  $r_{i,t}$ —the relative unemployment rate normalised to zero at the PBD threshold—and 12 *lags* of the running variable, which we allow to differ on either side of the cut-off. In our main estimation we use a linear specification, so  $f_{1,j}$  is a linear function of the running variable interacted with the PBD. The PBD can only change in January of each year, so by including 12 lags we ensure that estimates for each month include a lag of the previous calendar year. The second sum contains the contemporaneous PBD dummy and 12 lags. The third and fourth sums contain six *leads*. Recall that the PBD in a calendar year depends on the relative unemployment rate on June 30 in the previous calendar year. By including only six leads, we ensure that the leads of the running variable are already determined at  $t$ . This holds because the earliest lead observation from the following year enters the estimation equation for July, when the PBD of the following year is already determined. Through the leads we are able to identify anticipation effects in the months before a PBD change, which might occur as workers’ update their beliefs about future outside options as soon as the PBD in the following calendar year is known.

We additionally include 12 lags of the outcome variables (fifth sum), and month-of-the-year and county fixed effects. Through the latter two we account for general time trends and business cycle effects, as well as time-constant heterogeneity between counties. This improves precision by absorbing variation in the outcome variables.

It is important to understand that the included PBD indicators are conditionally exogenous, given that we control for the running variable. As a consequence, a PBD change is very similar to an exogenous anticipated policy shock as, e.g., in [Mertens and](#)

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<sup>16</sup>In Appendix Table B.2 we show effects for estimates on the benefit duration, unemployment duration and duration until re-employment at the individual level (see also the large literature from other countries referred to in footnote 1). In [Galecka-Burdziak et al. \(2021\)](#), individual-level effects for Poland are analysed in more detail.

Ravn (2012). Consider two counties, where in one county the relative unemployment rate was slightly above 150% and in the other county it was slightly below this threshold (as in Figure 1). There is no reason to think that other factors that may have an impact on labour market outcomes differed notably between these counties. This view is supported by the fact, which we report in subsection 4.2, that county characteristics are smoothly distributed around the threshold. Moreover, it is not possible for counties to strategically over- or underreport unemployment rates around the cut-off as the denominator of the unemployment rate relative to the country average is not known at the time of reporting.

Now consider two counties that are somewhat further away on each side of the threshold. One might worry that macroeconomic conditions between a county with a relatively high unemployment rate differ from those in a county with a comparatively low one. These factors would be correlated with the PBD and potentially the outcome variable of interest. However, these factors are controlled for by including the running variable in equation (1). Consequently, they are not contained in the error term. The exogeneity assumption also holds for the six leads of the PBD as the running variable at time  $t$ , which determines the PBD, is determined by the relative unemployment rate in June of the *previous* year. As a consequence, the PBD of the following year is determined from July onwards.

**Impulse Responses.** Similarly to Hagedorn et al. (2019a), we calculate the expected cumulative effect of a change in the PBD. We quantify the effect of an increase in the PBD from 6 to 12 months on different labour market outcomes for every month in the half year leading up to the increase and the two years afterwards, i.e., for the months  $m \in [-6, 23]$ , where the PBD raise occurs in  $m = 0$ .

The impulse response shows the expected time path for a particular outcome in a county where previously the PBD was 6 months and where the running variable, which determines the PBD in  $m = 0$ , has just passed the threshold, compared to the baseline time path where the running variable is slightly below the threshold in  $m = 0$ . In other words, it describes the effect of an exogenous PBD increase holding everything else constant. Generally, the PBD might depend on past values of the PBD. In the time span relevant for the construction of the impulse response, the PBD will take the values reported in Table 2 in the two simulated time paths. We will refer to the period where  $m \in [-6, -1]$  as year  $-1$ ,  $m \in [0, 11]$  as year 0, the period where  $m \in [12, 23]$  as year 1 and the period where  $m \in [24, 35]$  as year 2. The PBD in year 2 is only needed for the leads for the construction of the impulse response in year 1.

As the PBD is set each January depending on the relative unemployment rate of June in the previous year, the PBD in the first path remains 12 for the rest of the year while  $m \in [0, 11]$ . The PBD in the following year,  $m \in [12, 23]$ , depends non-linearly on the relative unemployment rate (the running variable) and its response to the PBD increase in  $m = 0$ . Therefore, the impulse response is a *conditional* impulse response as it depends

Table 2: Values of the potential benefit duration in simulated paths

Month	Year	PBD increase	Baseline
$[-6, -1]$	-1	6	6
$[0, 11]$	0	12	6
$[12, 23]$	1	determined through simulation	
$[24, 35]$	2	determined through simulation	

on the previous value of relative unemployment. We apply a simulation method, which we describe below, to obtain the values of the PBD for  $m \geq 12$  for the two simulated paths. It is similar to the Monte Carlo integration procedures described in [Koop et al. \(1996\)](#) and [Kilian and Lütkepohl \(2017\)](#).<sup>17</sup> Given the evolution of the PBD, the response of relevant labour market outcomes is linear and can thus be derived directly from the estimation of equation (1).

The effect of an exogenous increase in the PBD in January on the outcome six months prior (July) is simply the immediate impact of the lead of the PBD dummy:

$$\tilde{\delta}_{-6} = \delta_{2,6} \quad (2)$$

The effect five months prior to the PBD increase is given by the immediate impacts of the fifth and sixth lead of the PBD dummy plus the dynamic effect via the lag of the previous month's effect:

$$\tilde{\delta}_{-5} = \delta_{2,5} + \delta_{2,6} + \delta_{3,1}\tilde{\delta}_{-6} \quad (3)$$

In general, the effect for the months leading up to the PBD increase is given by the following equation for  $m \in [-6, -1]$ , namely immediate effects of the included leads of the PBD dummy plus the dynamic effects via the lags of the relevant outcome:

$$\tilde{\delta}_m = \sum_{j=-m}^6 \delta_{2,j} + \sum_{j=1}^{m+6} \delta_{3,j}\tilde{\delta}_{m-j} \quad (4)$$

The cumulative effect in the first month *after* the PBD increase additionally contains the contemporaneous effect of the PBD increase,  $\delta_{1,0}$ ,

$$\tilde{\delta}_0 = \delta_{1,0} + \sum_{j=1}^6 \delta_{2,j} + \sum_{j=1}^6 \delta_{3,j}\tilde{\delta}_{-j}, \quad (5)$$

and generally, the cumulative effect in the months  $m$  following the PBD increase is given by the effects of  $\Delta PBD12_{i,t}$ , that is the expected difference between the two simulated paths in the values of an indicator that equals one if the PBD is 12 months and zero

<sup>17</sup>A somewhat similar problem is that of obtaining impulse responses, e.g., to monetary policy shocks, that depend on the state of the economy, which, in turn, is endogenous (see [Gonçalves et al., 2022](#)).

otherwise, and the lagged effects on the outcome of interest:

$$\tilde{\delta}_m = \sum_{j=0}^{12} \delta_{1,j} \Delta PBD12_{i,m-j} + \sum_{j=1}^6 \delta_{2,j} \Delta PBD12_{i,m+j} + \sum_{j=1}^{\min\{m+6,12\}} \delta_{3,j} \tilde{\delta}_{m-j} \quad (6)$$

As described above,  $\Delta PBD12_{i,t}$  is zero in the six months before the PBD increases in the time path with a PBD increase, one in the twelve months after the raise, and the value afterwards depends on the response of the (relative) unemployment rate to the PBD raise. The simulated values of the PBD dummy in the two simulated paths impact the impulse responses in the months  $m \in [6, 11]$  through leads and in the months  $m \in [12, 23]$  additionally through the contemporaneous effect and lags.

**Simulation of the PBD Response.** To calculate impulse responses from equation (6), we need to obtain the path of the expected difference in the PBD between the scenario of a PBD increase and a baseline scenario, where the PBD equals 6 months in  $m \in [0, 11]$ . As RD identifies a local average treatment effect around the threshold, we simulate these expected differences for counties in our sample that are close to the cut-off, where the running variable  $r \in [-10, 10]$ . The PBD depends non-linearly on the running variable. If an increase in the PBD increases the running variable by a small amount, the impact on the probability that the running variable is above zero in the following year is negligible for counties far from the threshold, while it might be substantial for counties close to it. We predict the running variable through a regression and by adding draws of residuals. For the two time paths, the share of counties with a simulated PBD of 12 equals the share of counties where the simulated running variable is larger than zero. We apply the following procedure:

- 1) Estimate a linear model of the running variable to predict its evolution. Recall that PBD and running variable change only once a year, so that the frequency of the data is annual. Hence, the regression is based on annual data. Regress the running variable of county  $i$  in year  $y$  on a function of the previous year's running variable and PBD as well as time dummies and calculate residuals from the regression:

$$r_{i,y} = \gamma_0 + f(r_{i,y-1}, PBD12_{y-1}) + \gamma_1 PBD12_{y-1} + year_y + u_{i,y} \quad (7)$$

- 2) Set the lag of the PBD dummy to one, predict the running variable for each county in the sample where the running variable is close to the cut-off, and add a randomly drawn residual to this deterministic prediction. This yields the simulated running variable in year 1 for each county for the path with a PBD increase.
- 3) Repeat the second step, but set the lag of the PBD dummy to zero. This yields the simulated running variable in year 1 for each county for the baseline path without a PBD increase.

- 4) Set the simulated PBD to 12 for those counties where the simulated running variable is above zero and to 6 for the other counties. This yields the simulated values of the PBD in year 1 for each county for the two time paths with and without a PBD increase in year 0.
- 5) For each county and for both time paths, use the simulated values of the running variable and PBD in year 1 to predict the response of the running variable in year 2,  $m \in [24, 35]$ , from equation (7). Add a draw of residuals to these predicted running variables.
- 6) For each county, set the PBD to 12 for year 2 if the simulated running variable in that year exceeds zero and to 6 otherwise.
- 7) Subtract the baseline time path for the PBD from the time path with a PBD increase in year 0.
- 8) Repeat steps 2-7 for 50 sets of draws of the residuals and calculate the average of the differences in the PBD time paths.

The impulse responses of our outcome variables of interest depend linearly on the expected difference between the two time paths of the PBD. Thus, to calculate the impulse response we simply insert these values into equation (6). The simulation yields that in counties with a PBD of 12 months in year 0 the probability that the PBD is 12 in year 1 is 2.9 percentage points higher than in counties with a PBD of 6 months in year 0. In year 2, the difference is 2.1 percentage points.<sup>18</sup> The simulation of the PBD is only a local approximation as it neglects the fact that changes in the unemployment rate of treated counties have an impact on the country average of the unemployment rate. This impact is small when simulating only two years.

**Standard Errors.** Standard errors are simulated via block bootstrap with 200 replications, i.e., we draw samples of counties with replacement, run regressions based on equation (1), simulate the PBD path, and calculate the impulse responses for every draw.

## 4.2 RD Diagnostics

In this subsection we provide support for the key identifying assumption on which an unbiased RD estimation hinges: Assignment to treatment must be random around the cut-off, such that the only factor that varies discontinuously around the cut-off is the treatment itself (Imbens and Lemieux, 2008). Hence, counties on either side of the cut-off must be equal conditional on the polynomial of the forcing variable<sup>19</sup> (and potentially other control variables). The only meaningful difference between those counties is that

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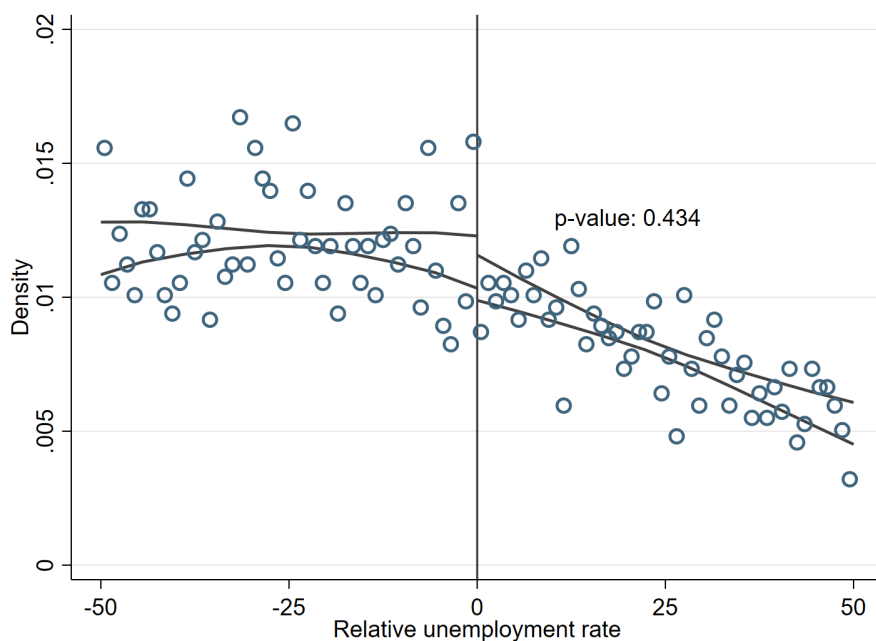
<sup>18</sup>Only including counties closer to the cut-off in the simulation slightly increases this difference and additionally including counties further away decreases it. These differences have a negligible impact on the resulting impulse responses.

<sup>19</sup>In section 7 we assess the sensitivity of our estimates to the specification of the polynomial.

eligible unemployed in counties just below the cut-off are entitled to a PBD of 6 months and those from counties that have passed the threshold can receive benefits for 12 months.

We provide two pieces of evidence in support of this assumption. Random assignment around the cut-off implies that the running variable cannot be manipulated. In our context this means that it is not possible for counties to strategically report unemployment rates that are just above (below) the cut-off in order to receive a higher (lower) PBD. For counties' relative unemployment rates, manipulation is implausible as the unemployment rate of each county is calculated from administrative records from which then the national unemployment rate is obtained. Manipulating the running variables requires knowledge of the national unemployment rate, which at that time is unknown, and the possibility to rig a county's unemployment rate.

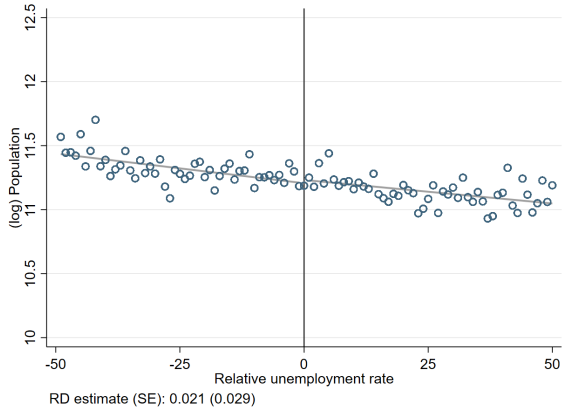
Figure 4: Density test



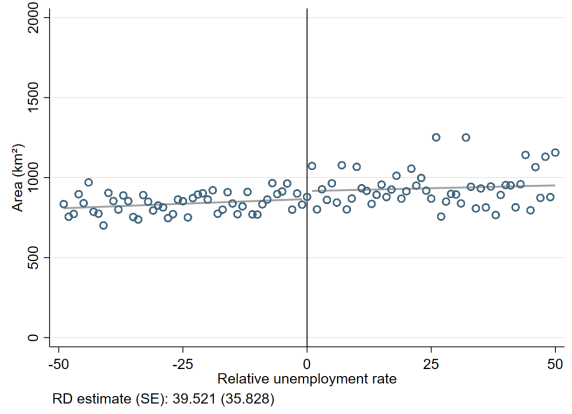
*Notes:* The figure shows a density test of the running variable (relative unemployment rate to the cut-off) at the annual county-level. The density test follows Cattaneo et al. (2020) with a bandwidth of 50 and a linear polynomial. Dots are density points in one-percent intervals, solid lines denote 95% confidence intervals. Sample period is 2005 to 2019.

In Figure 4 we display results from a density test of observations around the cut-off. If a discontinuity was identified, this would be indicative of strategic sorting around the cut-off, which could occur if, say, it would be politically advantageous to have a higher PBD and manipulation was possible. We collapse the data to the county-by-year level as the treatment assignment occurs on this level and use our preferred specification of a symmetric bandwidth of 50 and a linear polynomial in the estimation. The manipulation testing is based on the local polynomial density estimator by Cattaneo et al. (2020). To illustrate the density distribution of counties, we show one-percent bins. The density of

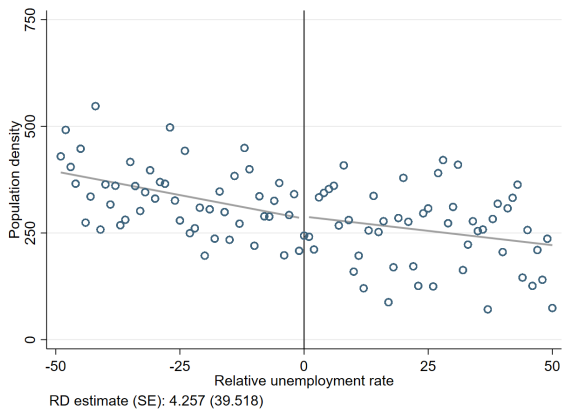
Figure 5: Distribution of county-level characteristics



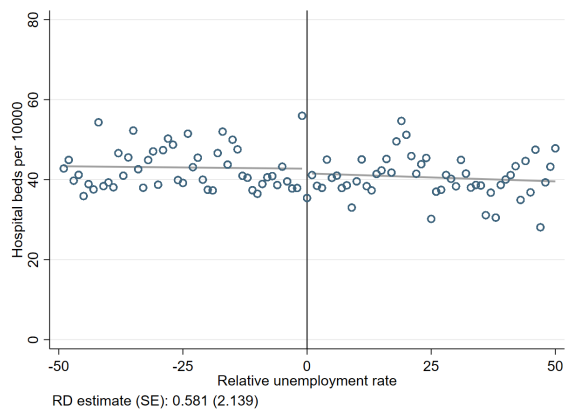
(a) Population



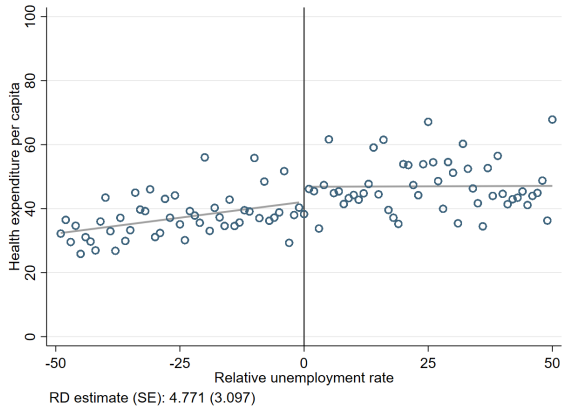
(b) Area



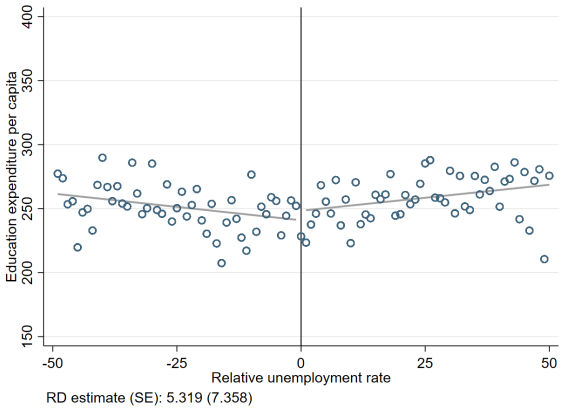
(c) Population density



(d) Hospital beds



(e) Health expenditure



(f) Education expenditure

*Notes:* The figure shows the distribution of county-level characteristics relative to the previous year's relative unemployment rate of counties. RD estimates are obtained following [Calonico et al. \(2014\)](#) with a symmetric bandwidth of 50 and a linear polynomial. Data from Statistics Poland. Sample period is 2005 to 2019.

county observations around the cut-off in Figure 4 appears smoothly distributed and the density estimator gives no indication of a discontinuity around the cut-off.

Another common approach to support random treatment assignment is to show that



exogenous characteristics are distributed smoothly around the cut-off. We consider the distribution of county-level characteristics which are highly unlikely to be related to the PBD in a given year.

Figure 5 shows the distribution of a set of county-level characteristics. As is visible in Panels (a), (b) and (c), counties with a lower unemployment rate have a higher population on average, a smaller area and as a consequence a higher population density, indicating that counties with higher unemployment rates are more likely to be rural. None of the RD estimates with bias-corrected standard errors (Calonico et al., 2014) reveal a discontinuity around the cut-off. Panels (d)-(f) examine outcomes relating to health care provision and public expenditure of counties. These outcomes are related to the relative unemployment rate of counties, but the distribution around the PBD-determining cut-off is smooth for all variables considered. This is in stark contrast to the pronounced discontinuities in outcomes that are *directly* affected by whether a county is above or below the cut-off which we documented in Figure 3.

## 5 Results

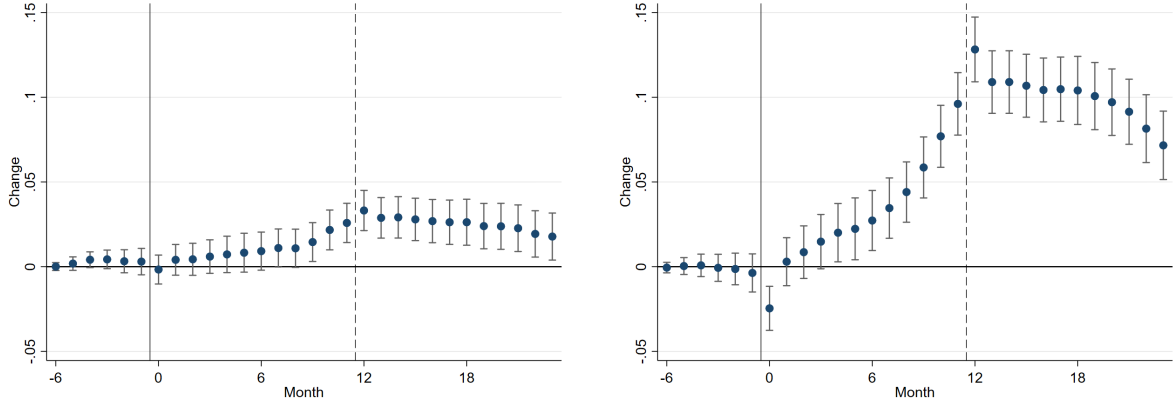
In this section we report the effect of a longer PBD on numerous labour market outcomes. We begin by presenting the effect on the stock of unemployed, before moving on to unemployment and benefit receipt duration. Next, we consider market externalities of a PBD increase by assessing whether indirectly affected unemployed respond and by directly looking at measures of labour market tightness. Following that, we analyse the effect of a longer PBD on inflows into unemployment and use our estimates to discuss the effect of an hypothetical permanent PBD increase on equilibrium unemployment.

**Stock of Unemployed.** The effect of a longer PBD on aggregate stocks is reported in Figure 6. Recall that the PBD increases in  $m = 0$  in counties above the cut-off and remains constant at that level for one calendar year (up to  $m = 11$ ) before it can potentially decrease again in the following year. We present monthly coefficients for the six months prior to the PBD raise to capture anticipation effects and then the cumulative effects for 24 months afterwards. 95% confidence intervals are obtained from block bootstrapped standard errors with 200 replications.

Panel (a) of Figure 6 shows the effect of a PBD increase from 6 to 12 months on the (log) stock of all unemployed, which includes both the directly affected unemployed and those indirectly affected. The stock of unemployed starts to gradually increase in the year of the PBD change peaking at 0.033 log points 12 months after the PBD increase. Effects are larger when focusing on eligibles in Panel (b). Point estimates turn statistically significant after four months and grow continuously up to 0.13 log points after a year.

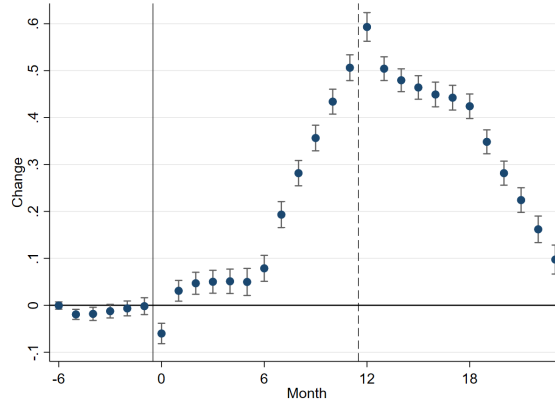
Table 3 reports the summary coefficients for the effect of a larger PBD for the period

Figure 6: Effect on (log) stocks of unemployed



(a) All unemployed

(b) Eligible unemployed



(c) Eligible benefit recipients

*Notes:* Figures show cumulative effects of a PBD increase from 6 to 12 months at the county-month level obtained from estimating equation (1). Panels (a) and (b) refer to the stock of unemployed, Panel (c) refers to the stock of benefit recipients. The forcing variable (relative unemployment rate) is specified linearly and we use a symmetric bandwidth of 50. Impulse response are constructed as described in section 4. The sample period is 2005 to 2019. Eligibles refer to unemployed eligible for benefit receipt who are under 50 years and were laid off. Whiskers indicate 95% confidence intervals obtained from block bootstrapped standard errors with 200 replications.

after the PBD increase (in 12-months bins) and for the six months before. The summary estimates are the averages of the monthly coefficients in the relevant time period as indicated in the table. We identify precisely estimated zeros for the anticipation period for all stock outcomes. In response to the PBD increase to 12 months, the effects in the following two years are a 0.01 and 0.026 log points increase for all unemployed, and 0.032 and 0.101 for eligibles.

Panel (c) of Figure 6 contains the effect on the (log) stock of benefit recipients. In contrast to unemployment, the duration of benefit receipt has an upper limit of 6 or 12 months, depending on the county of residence. Assessing the effect on the stock of benefit recipients is of particular interest from a fiscal perspective as it directly affects public expenditure. Consistent with Figure 3, the effect of a higher PBD on the stock of benefit recipients is much larger than that on the stock of unemployed. In year 0 the stock

Table 3: Summary estimates for effect on stocks of unemployed

Outcome	(Log) stock of								
	All unemployed			Eligible unemployed			Eligible benefit recipients		
Month $\in$	[-6, -1]	[0, 11]	[12, 23]	[-6, -1]	[0, 11]	[12, 23]	[-6, -1]	[0, 11]	[12, 23]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Coefficient	0.0027 (0.0024)	0.0101 (0.0050)	0.0255 (0.0064)	-0.0009 (0.0033)	0.0318 (0.0081)	0.1007 (0.0094)	-0.0099 (0.0060)	0.1681 (0.0118)	0.3725 (0.0117)
Observations	60,784			60,784			60,784		

*Notes:* The coefficients show summary coefficients for the time period indicated in the top row of the table. The sample period is 2005 to 2019 and observations are at county-month-level. Eligibles refer to unemployed eligible for benefit receipt who are under 50 years and were laid off. Block bootstrapped standard errors based on 200 replications in parentheses.

of recipients increases by 0.168 log points and in year 1, when a larger share of eligible unemployed receive a longer PBD, by a substantial 0.373 log points (Table 3).

The observed gradual increase of the stocks is expected as only the PBD of newly unemployed is prolonged.<sup>20</sup> At the end of the calendar year of the PBD increase ( $m = 11$ ), a larger share of the unemployed have a longer PBD than is the case at the beginning. If the only effect of a higher PBD was a change in the exit rate from unemployment, one might expect that the stock would only start to expand six months after the PBD increase. If, however, the inflow into unemployment also responds to a higher PBD, the stock may start to increase immediately, but still gradually. We decompose the effect on the stock of unemployed in section 6 and provide evidence that both the effect on the exit rate and the effect on inflows play important roles, but that initially the inflow effect drives the increase in the stock.<sup>21</sup>

We also show the effect on the log of the official county-level unemployment rate calculated by Statistics Poland in Appendix Figure A.5. We see a significant effect of a very similar magnitude to the effect on the log stock of all unemployed. The effect on the unemployment rate can be estimated with publicly available data on county-level unemployment rates.

**Unemployment Duration.** Next, we analyse unemployment and benefit durations, and the duration of joblessness. In contrast to stocks, these outcomes concern those who entered unemployment and started collecting benefits in the respective month, i.e., the effects are not cumulative in nature, so that we focus on the summarising annual coefficients (impulse responses are reported in Appendix Figure A.6). Despite being aggregated at the county-month-level, these estimates allow us to contrast our results

<sup>20</sup>Given that the relative unemployment rate in *June* determines the following year's PBD, this gradual expansion only leads to a slight increase in the probability to observe a PBD of 12 months in the year after the PBD raise.

<sup>21</sup>The curious reader may wonder why the stock of unemployed drops upon the PBD increase. We kindly ask for patience as we will return to this point when discussing the effects on inflows.

Table 4: Summary estimates for effect on durations of eligibles

Month $\in$	$[-6, -1]$	$[0, 11]$	$[12, 23]$	$[-6, -1]$	$[0, 11]$	$[12, 23]$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: (log) Durations						
	Unemployment			Benefit		
Coefficient	-0.0237	0.1943	0.0032	0.0036	0.4963	0.0094
	(0.0074)	(0.0078)	(0.0094)	(0.0040)	(0.0059)	(0.0125)
Panel B: Share duration $\leq 12$ months						
	Unemployment			Jobless spell		
Coefficient	0.0012	-0.2451	0.0015	-0.0040	-0.1622	0.0033
	(0.0047)	(0.0083)	(0.0081)	(0.0042)	(0.0070)	(0.0053)
Observations	60,784			60,784		

*Notes:* The durations in Panel A are in logs. Panel B denotes the share with unemployed or jobless duration of less than 12 months. Jobless duration only considers unemployed whose reason for de-registering from unemployed is subsequent employment (65%). See Table 3 for other notes.

directly to the duration estimates in the literature based on individual-level data. For the analysis of durations, we, for now, focus on benefit recipients and discuss the effects on the indirectly affected afterwards.

Panel A of Table 4 shows the results for continuous measures of durations. The effect in the year following the PBD increase from 6 to 12 months is 0.194 log points for unemployment duration (column (2)) and 0.496 log points for benefit duration (column (5)).<sup>22</sup> This yields an elasticity of the unemployment duration with respect to the PBD of  $0.194/(\log(12) - \log(6)) = 0.28$ , which is a little lower than other estimates of the micro elasticities discussed in Landais et al. (2018a), which range from 0.36 to 0.85. The elasticity of the benefit duration is 0.72.

Withdrawal from unemployment does not necessarily translate into job finding if a larger share of unemployed simply de-register from unemployment without returning to employment (see Card et al., 2007b, who show for Austria that only around 10% of exit are into re-employment). In the Polish context, Gałecka-Burdziak et al. (2021) find that around two-thirds of the unemployed directly re-enter into employment following their unemployment spell and the spikes in hazard rates for exit from unemployment or entry into employment are both sizeable.

In Panel B of Table 4 the dependent variables are the share of eligible unemployed who *exit unemployment* within 12 months (columns (1)-(3)) and the share who *enter*

<sup>22</sup>The small negative effect for unemployment duration in the anticipation period of -0.024 log points can be explained by selective inflow into unemployment as inflows into unemployment by those with a looser labour market attachment are more responsive to the PBD level. Those becoming unemployed just before the PBD increase are thus more likely to be positively selected with regard to their search effort.

Table 5: Market externalities—effect on (log) durations of indirectly affected

Group of unemployed:	Ineligibles			Benefit recipients, > 50 years and $\geq 20$ contributory years					
	Unemployment duration			Benefit duration					
Outcome									
Month $\in$	[-6, -1]	[0, 11]	[12, 23]	[-6, -1]	[0, 11]	[12, 23]	[-6, -1]	[0, 11]	[12, 23]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Coefficient	-0.0036 (0.0046)	0.0075 (0.0060)	0.0151 (0.0072)	-0.0023 (0.0106)	0.0076 (0.0103)	-0.0118 (0.0101)	0.0019 (0.0076)	0.0052 (0.0069)	-0.0076 (0.0054)
Observations	60,784			60,784			60,784		

*Notes:* All outcomes are in logs. Columns (1)-(3) concern unemployed workers under 50 years who are ineligible to receive benefits as they have not had sufficient contributory spells in the 18 months preceding unemployment. Benefit recipients over 50 years with at least 20 contributory years have a PBD of 12 months regardless of their county’s relative unemployment rate. See Table 3 for other notes.

*employment* within 12 months after starting their unemployment spell (columns (4)-(6)). We use the second outcome to gauge the effect on employment because we only observe the reason for de-registration from unemployment, e.g., whether the unemployed transition into a job, an active labour market policy, retirement and so on. Thus, we do not observe the longer term outcomes of individuals. We find that not only are the durations of unemployment and benefit receipt prolonged by a longer PBD, but also the share of individuals with a jobless spell up to 12 months reduces by 16 percentage points.

**Market Externalities.** The *macro effect* is defined as the overall effect of a higher PBD. In Poland, not all unemployed are directly affected by PBD changes. Even when abstracting from the impact of UI generosity on inflows into unemployment, the *macro effect* cannot be deduced from the effect on the directly affected, as it is the sum of the *micro effect* and *market externalities* (Lalive et al., 2015).

To assess market externalities of job search in Poland empirically, we consider two distinct groups of unemployed not directly affected by the PBD increase. Unemployed below the age of 50 years who are ineligible to receive benefits due to insufficient contributory spells and benefit recipients above 50 years with at least 20 contributory years to the benefit system whose PBD is 12 months independent of their county’s relative unemployment rate. The first group is demographically closer to the directly affected benefit recipients and the second groups is more similar in terms of labour market attachment.

For both groups, the estimates for the unemployment duration for year 0 reported in Table 5 are precisely estimated zeros (columns (2) and (5)). The implied elasticities of 0.011 and tight confidence intervals allow us to rule out any economically meaningful effects with confidence. For the group of older workers, we also consider the benefit duration, where we similarly find no evidence for any spill-over.

We can use these estimates to recover the elasticity wedge, which is the difference between the micro- and macro-elasticity of unemployment duration with respect to the

PBD;  $W = 1 - \frac{e^M}{e^m}$ , where  $e^M$  denotes the macro elasticity and  $e^m$  the micro elasticity. This wedge can be used to calculate the optimal UI replacement rate over the business cycle Landais et al. (2018b). If the elasticities differ from one another, market externalities must play a role. We re-write the formula used by Lalive et al. (2015), their equation (5), to derive the wedge based on our estimates for the unemployment durations on directly and indirectly affected unemployed in Tables 4 and 5, respectively.<sup>23</sup> The obtained elasticities for the two groups are  $-0.11$  and  $-0.05$ , substantially smaller than the preferred estimate in Landais et al. (2018a) of  $0.4$  and the one obtained by Lalive et al. (2015) of  $0.21$ .

The lack of spill-overs on workers not directly affected by the PBD variation induced by the regional discontinuity and the associated small elasticity wedges point towards no effect of the PBD on labour market tightness. Hence, in the Polish context we find no support for the notion that PBD should be countercyclical from the perspective of optimal UI (Landais et al., 2018a,b).

As further measures of labour market externalities, we assess whether the vacancy filling rate, a direct indicator for labour market tightness, is affected and whether we find evidence for the job creation effect.<sup>24</sup> For this we leverage data provided by the public employment offices on vacancies, which are available monthly since 2011. From 2014 onward, job postings at the public employment offices also include the minimum earnings at the positions. Naturally, job postings at public employment offices are only a subset of all vacancies.

We calculate the vacancy filling rate by dividing the monthly outflows *into employment* by the monthly vacancy stock. As we take the log of the vacancy filling rate, for this outcome it is not problematic that we only observe a subset of vacancies. We only need to assume that the ratio of vacancies posted at employment offices to total vacancies is the same in counties with and without a PBD increase (differences over time are absorbed by time fixed effects). The vacancy filling rate is inversely related to labour market tightness (Karahan et al., 2022) and can thus be used as a direct measure of it. Columns (1)-(3) of Table 6 show no impact on the vacancy filling rate, i.e. no change in labour market

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<sup>23</sup>Lalive et al. (2015) estimate the effect of the Regional Extension Benefit Program in levels whereas our estimates are in logs. Re-writing the equation in terms of elasticities yields

$$W = \frac{1}{p} \times \frac{\epsilon_b D_b}{\epsilon_a D_a - \epsilon_b D_b}, \quad (8)$$

where  $p$  denotes the share of treated unemployed in a labour market,  $\epsilon_a$  is the elasticity of the unemployment duration of the directly affected with respect to the PBD and  $\epsilon_b$  the elasticity of unemployment duration of the indirectly affected groups (ineligibles or older unemployed) with respect to the PBD of the directly affected.  $D_a$  and  $D_b$  are the average unemployment durations of the respective groups. The shares of workers are reported in footnote 12, the average unemployment duration of eligible workers in counties with 6 months PBD is 9.15 months, that of ineligible workers is 7.95 months and that of older workers is 11.1 months. The elasticities can be calculated from the coefficients in Tables 4 and 5.

<sup>24</sup>Other outcomes that are related to market-level effects are the number of new vacancy postings and the stock of vacancies, on which we find no effect (result available on request). However, this finding is difficult to interpret because, as we report below, we find significant effects of PBD increases on separations. These, in turn, should have an impact on job postings as well.

Table 6: Effect on vacancy filling rate and wages

Outcome	Vacancy filling rate			Wages of job postings			Wages at larger companies	
	Month $\in$ [-6, -1] (1)	[0, 11] (2)	[12, 23] (3)	[-6, -1] (4)	[0, 11] (5)	[12, 23] (6)	[0, 11] (7)	[12, 23] (8)
Coefficient	-0.0061 (0.0472)	0.0093 (0.0435)	0.0409 (0.0474)	-0.0066 (0.0033)	-0.0015 (0.0037)	0.0048 (0.0034)	0.0000 (0.0016)	0.0009 (0.0020)
Observations	34,442			21,536			3,100	

*Notes:* All outcomes are in logs. Vacancy filling rate is defined as monthly outflows into employment divided by the stock of vacancies. Vacancies at public employment offices are available since 2011. Wages of job postings are only available from 2014 onward. Wages at larger companies are average annual wages at companies with at least 10 employees. For other notes see Table 3.

tightness, in response to the PBD increase. The result is consistent with a horizontal aggregate labour demand curve (Hall, 2005; Landais et al., 2018b).

In matching models with bargaining over wages, an improvement in workers' outside options increases their reservation wages (Burdett and Mortensen, 1998; Krusell et al., 2010), which, in turn, leads to less job creation and consequently a decrease in tightness (Pissarides, 2000). Hagedorn et al. (2013) provide evidence that PBD extensions in the US lead to a reduction in the number of vacancies created and an increase in wages. In contrast, Johnston and Mas (2018) and Marinescu (2017) find no impact of PBD changes on wages.

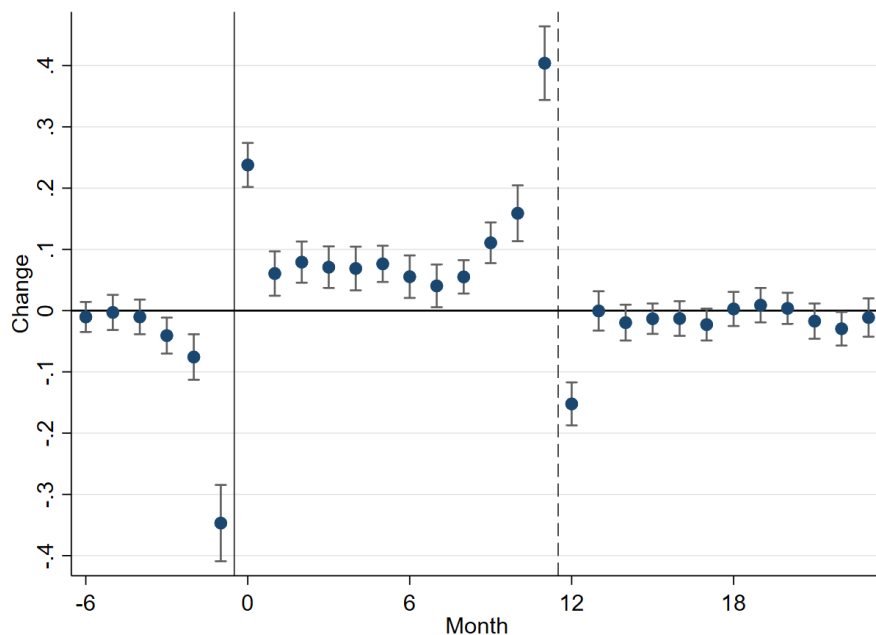
Using the information on lower bounds of wages of job postings at employment offices, we can look at the wage margin directly. Keep in mind that these lower bounds need not track actual wages of new hires perfectly. Columns (4)-(6) of Table 6 reveal precisely estimated null effects on wages of job postings, both in the pre-period, where wages could already change due to the anticipation of workers' improved outside options, and after the benefit change. Columns (7)-(8) display precisely estimated null effects on average wages at companies with at least 10 employees.<sup>25</sup> Our findings are in line with Jäger et al. (2020) who analyse wage effects of UI reforms of the benefit *level* in Austria and rule out economically significant effects as predicted by Nash bargaining models (see also Card et al., 2007a). Lalive et al. (2015) find no evidence of an effect of a PBD extension for older workers in Austria on re-entry wages of affected workers conditional on unemployment duration. These findings suggest that the generosity of UI plays a minor role for wage setting in European labour markets.

<sup>25</sup>Average wages are only available on an annual frequency. Using these annual data, we estimate a modified version of equation (1) including one lag and excluding leads. Due to the small number of periods we also have to drop the county fixed effects. For county  $i$  in year  $y$  we estimate

$$Y_{i,y} = \delta_0 + f(r_{i,y-1}, PBD12_{i,y-1}) + \delta_1 PBD12_{i,y-1} + \delta_{3,j} Y_{i,y-1} + time_y + \epsilon_{i,y}$$

and construct impulse responses.

Figure 7: Effect on (log) inflows into unemployment of eligibles



Notes: Figures shows the effect on monthly inflows into unemployment (separations). See Figure 6 for other notes.

**Inflows into Unemployment.** A major upside of having access to individual unemployment spells from which we construct aggregate outcomes is that *flow* variables such as in- and outflows into and from unemployment can be observed. In many studies of this literature, the analysed outcomes are restricted to *stock* variables such as the unemployment rate or employment. We first analyse to what extent inflows into unemployment are affected by PBD changes, before we decompose the effect on stocks in section 6 into a component driven by changes in the exit rate and changes in inflows to paint a rich picture of the dynamics underlying changes in the stocks.

The Polish setting is in many ways ideal to study the effect of a PBD increase on inflows; the PBD of a county depends on the unemployment rate of June in the previous calendar year. From September onward when revised and PBD-determining unemployment rates are announced, a potential PBD increase in January of the upcoming year is known and workers' inflow into unemployment could respond in anticipation of the change, as long as the timing of separations can be influenced at least to some degree. Specifically, if the PBD is about to increase from 6 to 12 months, workers would be expected to prefer becoming unemployed at the beginning of the upcoming year with the higher PBD rather than at the end of the current year with the lower PBD. One would expect such behaviour to be more prevalent among workers with a looser labour market attachment.

Figure 7 provides strong evidence for strategic behaviour with regard to the timing of inflow into unemployment. Starting in October ( $m = -3$ ), inflows are substantially



Table 7: Effect on (log) inflows into unemployment of eligibles

Month $\in$	$[-6, -1]$	$[0, 11]$	$[12, 23]$
	(1)	(2)	(3)
Coefficient	-0.0812 (0.0108)	0.1182 (0.0099)	-0.0220 (0.0085)
Observations	60,784		

*Notes:* See Table 3 for notes.

depressed. In December inflows are 0.35 log points lower than in counties whose PBD is not about to increase. The large uptick in inflows after the PBD increase reveals that this does not imply overall lower inflows but is a matter of intertemporal substitution; inflows are especially high in January, but, importantly, they remain much higher throughout year 0 with an average increase of 0.118 log points (Table 7). Over the time period  $m \in [-6, 11]$ , the average effect is 0.052 log points.<sup>26</sup> This shows that (i) workers can at least partially time their inflow into unemployment to receive higher benefits and (ii) a higher PBD increases overall inflows.

The intertemporal substitution also explains the drop in  $m = 0$  of the stock of eligible unemployed observed in Panels (b) and (c) of Figure 6. We measure stocks at the beginning of each month—as inflows in December before the PBD increase are much lower, this reduces the stock of unemployed and benefit recipients at the beginning of the following year.

We investigate heterogeneities in the effect on inflows to shed light on the characteristics of workers that are more responsive in their inflow to the PBD. Ex ante, the expectation is that socio-demographic groups for which higher labour supply elasticities are commonly found in the literature, women and older workers (Keane, 2011), react stronger. This is indeed what we find and report in Appendix Table B.3; the inflow effect in year 0 is 0.059 log points larger for women and 0.048 log points larger for above median aged workers. This is also in line with larger effects of PBD on unemployment duration for these groups in Poland (Gałecka-Burdziak et al., 2021).

Given the heterogeneous inflow effects, composition effects—in addition to the immediate effect on the unemployment durations of individuals—might to some extent drive our estimated unemployment duration effect. We make three points. First, the composition effect is small. For instance, while the average unemployment duration of eligible women is 380 days and that of men only 272 days, their respective inflows increase by only 0.15 and 0.09 log points, respectively, such that the composition of inflows is only

<sup>26</sup>The almost equally sized coefficients with a reverse sign in the first two columns in Table 7 might, at first glance, give the impression that only the first finding of intertemporal substitution is empirically supported, while the overall level of inflows is unchanged. However, the summary coefficients average the coefficients over the respective time period. As the anticipation period before the PBD change only covers six months, the aggregate effect on the flow variable inflows in this period is in fact less than half the size of the effect in the year after the PBD increase.

weakly affected. Second, given that our outcome is the average unemployment duration on the county level, the composition effect does not impact the causality of our estimates, only the interpretation. Third, the existence of composition effects does not change the interpretation of the absence of spill-over.

Appendix Figure A.7 reports the effect on job endings for all eligible workers under 50 and for those who are laid off. For both groups, job endings contract two months before the PBD increase and substantially expand one month before the PBD increase. Thus, the intertemporal substitution, which we observe for inflows, occurs one month earlier as there is a slight delay between job endings and registration as unemployed. In year 0 job endings remain significantly increased by about 0.06 log points. The magnitude of the effect in most months in year 0 is similar to that of the increase in inflows into unemployment, i.e., increases in inflows into unemployment in response to PBD raises are due to larger numbers of separations.

**Effect on Equilibrium Unemployment.** While our natural experiment only allows us to directly quantify the effects of *temporary* PBD increases, we use our estimates for inflows into unemployment and unemployment durations to gauge the impact of a *hypothetical permanent* PBD increase on equilibrium unemployment of eligible workers under 50. A strong caveat to this exercise is that effects of a temporary increase may not be adequately informative to assess effects of a permanent increase, as estimated coefficients could partly reflect short-term adjustments and, especially in proximity to the cut-off, the awareness that the PBD of a county is likely to change again in the near future. We therefore recommend taking the calculation on equilibrium unemployment with a pinch of salt.

The equilibrium unemployment rate is given by  $u = \frac{s}{s+f}$ , where  $s$  and  $f$  denote the separation and job finding rates, respectively (Pissarides, 2000). For our back-of-the-envelope calculation we assume that exits from unemployment equal entries into employment and entries into unemployment equal job separations.

Based on the summary statistics reported in Table 1 and using the approximation that hazard rates are constant during an unemployment spell, the monthly job finding rate can be calculated as one divided through the average unemployment duration. This hazard rate is 9.94% ( $f = 0.0994$ ). We only have annual country-wide figures for total employment. We use these to calculate country-wide separation rates, which are plotted at the monthly level in Appendix Figure A.8. In our time period, the monthly separation rate declines from about 1.4% in 2005 to 0.6% in 2019. This level is about one fifth of that in the US (Shimer, 2012) and similar to the German one (Hartung et al., 2022). We use the mean value for this period of 1% ( $s = 0.01$ ) and assume that the separation rate of eligible workers is equal to the economy-wide separation rate. The equilibrium unemployment rate is then 9.14%.

A PBD extension by 6 months increases the average unemployment duration of young eligible workers by 0.19 log points, or approximately 19%, resulting in a decrease in the job finding rate to 8.32%. As the majority of the work force is employed, a percentage change in job separations corresponds roughly to the percentage change in the separation rate. Our estimates for changes in inflows into employment displayed in Figure 7 are clearly affected by intertemporal substitution at the beginning and at the end of the year of the PBD increase. These would play no role for the new equilibrium unemployment rate after a permanent PBD increase. The estimated mid-year effect should be less influenced by these intertemporal substitution effects and we assume that the estimate for the month  $m = 6$ , 0.055 log points, corresponds roughly to the permanent increase in inflows that we would observe in case of a long-term increase in the PBD. Thus, the separation rate would increase by about 6% to 1.06%.

In sum, a permanent PBD increase to 12 months would lead to an increase in the equilibrium unemployment rate for eligibles from 9.14% to 11.25%, a marked increase by 2.11 percentage points or 23%. Changes in both the separation rate and the job finding rate are quantitatively important. If only the separation rate increased, the equilibrium unemployment rate would increase by 0.46 percentage points and if only the job finding rate decreased, the equilibrium unemployment rate would increase by 1.58 percentage points.

The effect on the equilibrium unemployment rate is substantially larger than our estimate of the effect on the log stock of eligible unemployed, which is 0.1 log points twelve months after a PBD increase. The main reason is that, even if there were no intertemporal substitution effects, and job finding rates and separation rates changed immediately to the new levels, it would take about two years for the unemployment rate to reach its new equilibrium level. In contrast, in the US, where separation and job finding rates are much higher, the adjustment to equilibrium level is almost immediate.

## 6 Decomposition of the Effect on the Stock of Unemployed

What drives the increase in the stock of unemployed? The literature on optimal UI mostly assumes that changes in unemployment durations are the dominant channel and ignores the impact on separations. To quantify the relative contributions, we decompose the increase in the stock of unemployed into a contribution based on effects on the exit rate and a contribution based on the effects on inflows. The exit rate, which is directly related to unemployment durations, is defined as the share of monthly outflows from unemployment out of the total stock. It is useful to write the stock of unemployed  $U$  at month  $t$  as follows:

$$U_t = (1 - R_{t-1}) \times U_{t-1} + I_{t-1} \quad (9)$$

with  $R_{t-1}$  denoting the exit rate of the preceding month. The product on the right hand-side of the equation is thus the stock of unemployed of the previous month remaining unemployed in month  $t$ .  $I_{t-1}$  is the total inflow throughout month  $t - 1$ .

The effects of a PBD increase from 6 to 12 months on the stock and on inflows were contained in Figures 6 and 7. We show the remaining ingredient, effects on the exit rate, in Appendix Figure A.9. To ensure that the decomposition adds up without residual in a finite sample,<sup>27</sup> we construct the effect on the exit rate from a regression of outflows from unemployment. Further, we need to use the level instead of the log of stock, inflows, and outflows. We normalise the level to the mean county population over the time period considered.<sup>28</sup> For the decomposition we calculate counterfactual stocks that are constructed using predictions from regressions in (normalised) levels based on equation (1).<sup>29</sup>

In equation (10) we lay out how the difference in the stock of unemployed is decomposed. The first superscript refers to inflows under the respective PBD, the second superscript is the exit rate:

$$U_t^{I_{12},R_{12}} - U_t^{I_6,R_6} = \underbrace{U_t^{I_{12},R_{12}} - U_t^{I_{12},R_6}}_{\text{Exit rate}} + \underbrace{U_t^{I_{12},R_6} - U_t^{I_6,R_6}}_{\text{Inflows}} \quad (10)$$

The left hand-side of equation (10) is the difference in the stock of unemployed with a PBD of 12 vs. 6 months. We expand this expression by adding and subtracting  $U_t^{I_{12},R_6}$ , the hypothetical stock of unemployed if inflows were as with a PBD of 12 and exit rates as if the PBD was 6, and then obtain the two sums on the right hand-side. The first element refers to the difference in stocks with different exit rates and where inflows are held constant and the right element contains different inflows holding the exit rate constant.<sup>30</sup>

The resulting decomposition of the stock of eligibles, including workers who quit their previous jobs (13.4% of all eligibles), is plotted in Figure 8. In the anticipation period the cumulative effects on inflows and the exit rate are negligible. In the first month after the PBD increase to 12 months the stock is reduced, which is entirely attributed to the delay in inflows in the preceding month (see Figure 7). Then the stock of unemployed gradually increases and the effect is initially almost entirely caused by higher inflows.

That the effect on the exit rate at first plays a minor role is expected as in the first six months newly unemployed in counties both above and below the cut-off can still receive benefits. Furthermore, the share of unemployed who receive longer benefits is small in

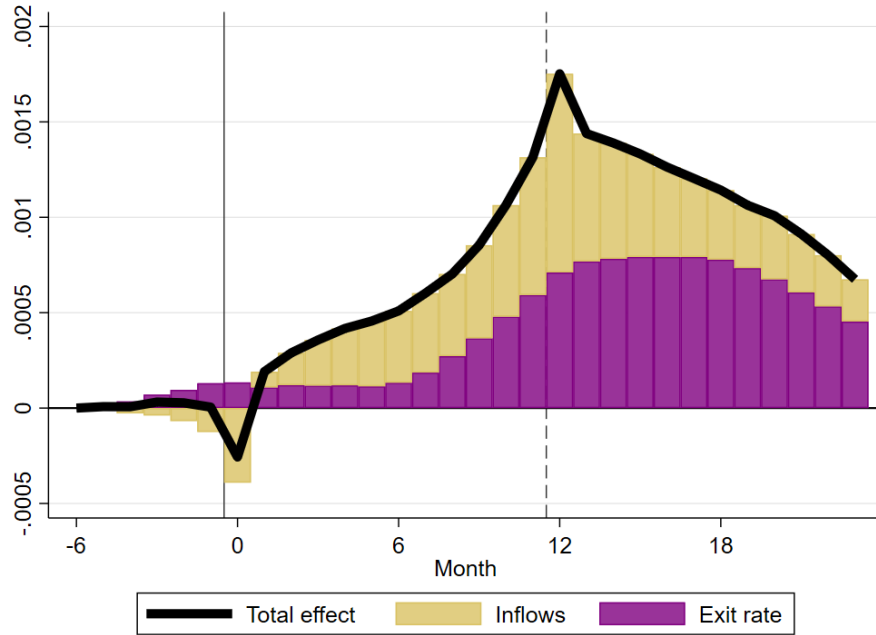
<sup>27</sup>This holds because stock, inflows and outflows are additively related,  $U_t = U_{t-1} + I_{t-1} - O_{t-1}$ , where  $O_{t-1}$  denotes outflows from unemployment.

<sup>28</sup>Although naturally scaled differently, estimates for the outcomes relative to the county population look qualitatively very similar to the estimates in logs as shown in the paper. Impulse responses for the normalised outcomes are available upon request from the authors.

<sup>29</sup>We slightly modify equation (1) in this estimation and always control for lags of inflows and outflows, which ensures that all estimations contain the same set of control variables.

<sup>30</sup>The decomposition in reverse order, available on request, yields very similar effects.

Figure 8: Decomposition of the effect on the relative stock of unemployed of eligibles



*Notes:* Figures show the effect of a higher PBD on the stock of unemployed relative to the county population as well as which parts of the effect can be attributed to effects on inflows and on the exit rate. Eligibles include both workers laid off and those who have quit their jobs. See text for further details.

counties with a PBD increase. After six months, the effect on the exit rate starts to grow as benefits expire for the newly unemployed in counties below the cut-off and an increasing share receive the longer PBD. In year 1, the effect due to a change in the exit rate becomes larger than the part attributed to inflows. Nevertheless, a large share of the increase in the stock of eligible unemployed can be attributed to the effect on inflows throughout. Ignoring the role of inflows when studying labour market effects of PBD changes misses an important part of the picture.

In Appendix Figure A.14 we also present the decomposition for all unemployed. As the directly affected dominate the overall effect and externalities play no major role, the decomposition looks very similar (both stocks are identically normalised relative to the entire county population).

## 7 Robustness

In this section we provide a battery of robustness checks for our estimates. We first conduct standard RD tests (see Lee and Lemieux, 2010) where we present coefficients obtained with different bandwidths and using a quadratic rather than a linear polynomial.

In our main estimation we use a symmetric bandwidth of 50. Choosing a narrower bandwidth decreases the reliance on the functional form for the running variable, but

commonly decreases precision, and vice versa. In Appendix Figure A.10 we present annual summary estimates for bandwidths ranging from 20 up to 100 in intervals of 10. It is comforting to see that the coefficients are stable across this wide spectrum of bandwidths with no statistically significant differences detected.

In Appendix Figure A.11 we contrast annual summary estimates obtained using a linear (our preferred specification) and a quadratic polynomial of the forcing variable. As recommended by Gelman and Imbens (2019), we refrain from showing higher order polynomials. As is common, confidence intervals are somewhat wider for the quadratic specification, but all point estimates are very close to the ones obtained from the linear specification.

When considering unemployed eligible to receive benefits, we considered only workers who were laid off. The motivation for this was that for laid off workers the increase in the PBD is always from 6 to 12 months. With this uniform increase we can directly calculate the elasticities of unemployment and benefit duration to the PBD. In Appendix Figure A.12 we show the main estimates for all unemployed directly affected by the PBD cut-off, i.e. in addition to laid off workers also those who are recorded as having quit their jobs. For the latter group the PBD is 3 months shorter than for laid off workers, but the difference in PBD in counties above or below the cut-off is the same with 6 months. Hence this group can in principle also be included in the estimation as we have, in fact, done in the decomposition in section 6. As 86.6% of eligibles have been laid off, the impulse response functions in Appendix Figure A.12 are not notably different to the main estimates presented in section 5, although the patterns are slightly more pronounced.

As a final robustness check, we add an additional control variable and include the lags of the *monthly* unemployment rate to the estimation (in the main estimation we simply control for lags of the running variable, which can only change for every calendar year rather than monthly). Results for this estimation contained in Appendix Figure A.13 are highly robust with, once more, no differences detectable by the naked eye.

## 8 Conclusion

In Poland, a county's PBD is 12 months if the county's unemployment rate in June of the previous year exceeded 150% of the national average and 6 months otherwise. We have used this exogenous variation in the PBD to construct impulse responses of various county-level labour market outcomes to a temporary increase in the PBD. We find that, after 12 months, an increase in the PBD from 6 to 12 leads to an increase in the log stock of directly affected unemployed by 0.13. This increase in the stock of directly affected entirely explains the aggregate increase in unemployment by 0.03 log points. Moreover, there are no effects on the unemployment duration of workers, for whom the PBD does not change (older workers and those ineligible for unemployment benefits). The finding

of no spill-over effects suggests that labour market tightness is unaffected by changes in the PBD. In fact, we see no effect on wages of job offers and the vacancy filling rate.

The absence of spill-overs implies that the macro elasticity (the change in unemployment if the PBD for all unemployed was changed) equals the micro elasticity (the change if the PBD changes for a minuscule number of unemployed). Thus, extrapolating from the estimates on the directly affected, our findings imply that unemployment would increase by about 0.1 log points if the PBD for all Polish unemployed was increased temporarily by 6 months. A back-of-the-envelope calculation shows that a permanent increase in the PBD by six months would result in an increase in the unemployment rate by 23%. Moreover, given that labour market tightness in Poland is unaffected by the PBD, optimally, the PBD should not vary over the business cycle (Landais et al., 2018b).

Finally, we find that the the PBD has a large impact on job separations, which explain about half of the increase in unemployment after a PBD expansion. We thus contribute to an emerging literature on the importance of job separations to explain changes or regional differences in unemployment rates (Hartung et al., 2022; Kuhn et al., 2021). The importance of job separations suggests that UI should be less generous than implied by the modified Baily-Chetty formula in Landais et al. (2018b), which accounts for the effects on unemployment durations and labour market tightness.

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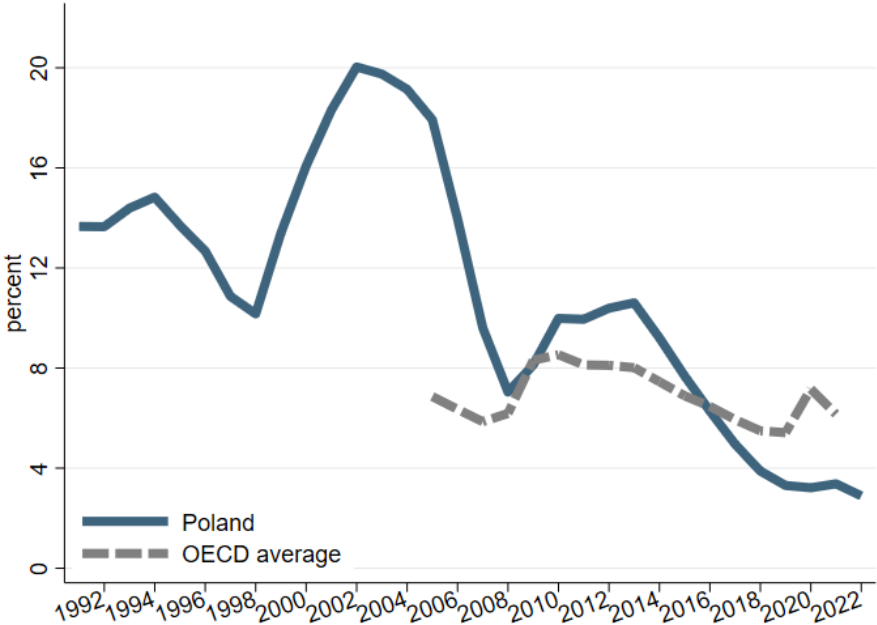
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Appendix (for online publication)

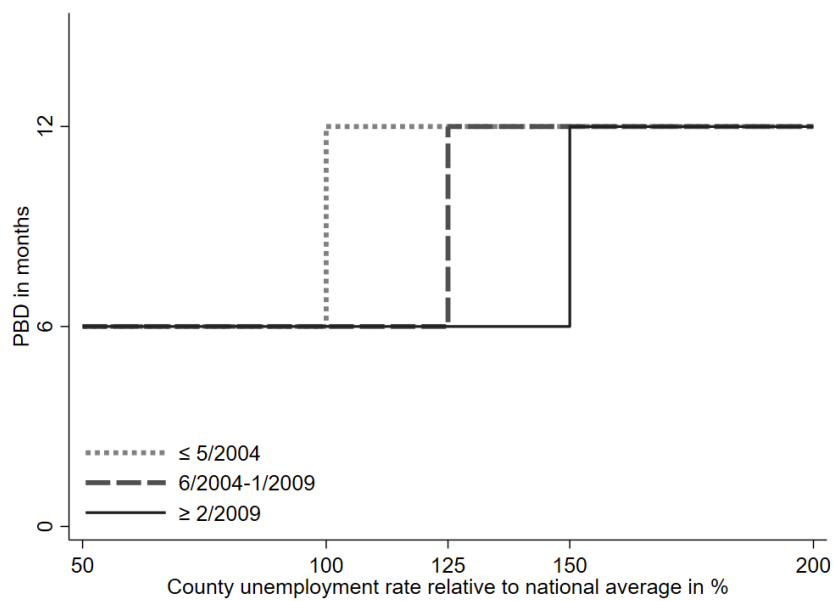
A Figures

Figure A.1: Unemployment rate over time



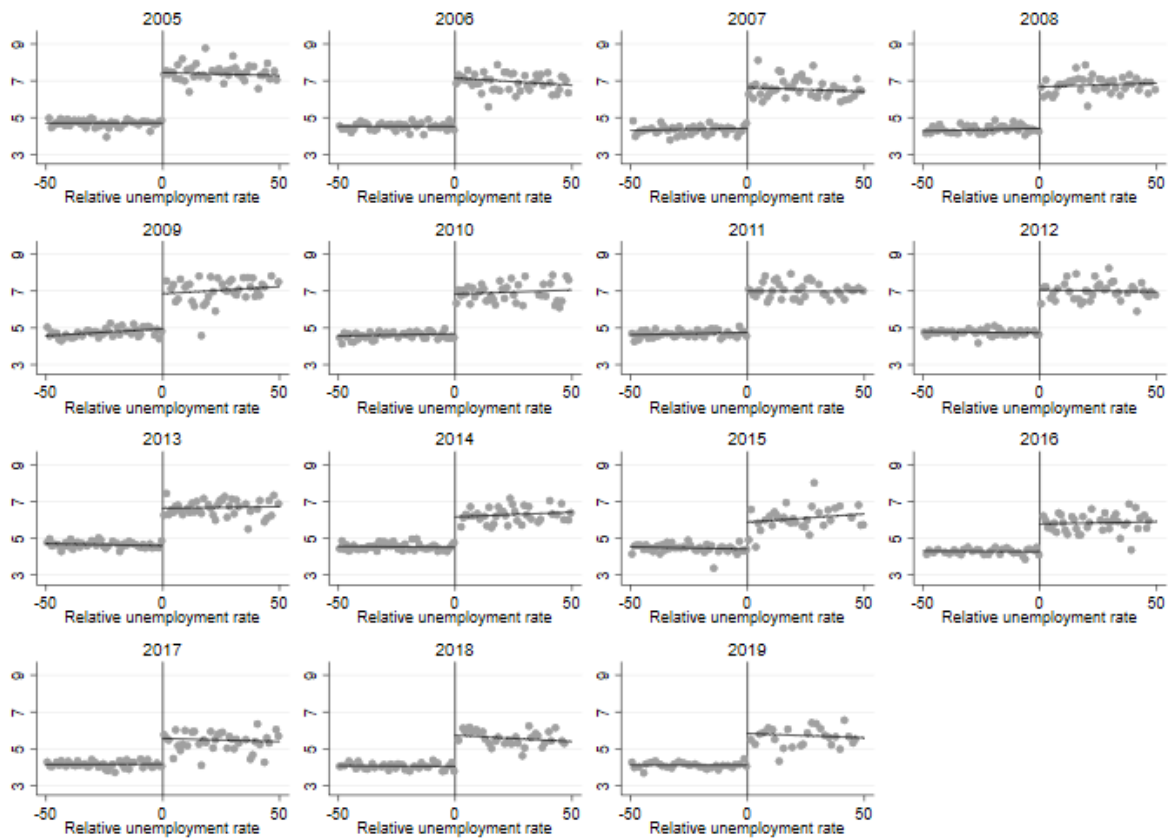
Notes: The figure shows how the unemployment rate of Poland and the OECD average over time. Sources: <https://data.oecd.org/unemp/unemployment-rate.htm>, accessed February 28, 2023, and Polish Labour Force Survey

Figure A.2: Relative unemployment rate and PBD



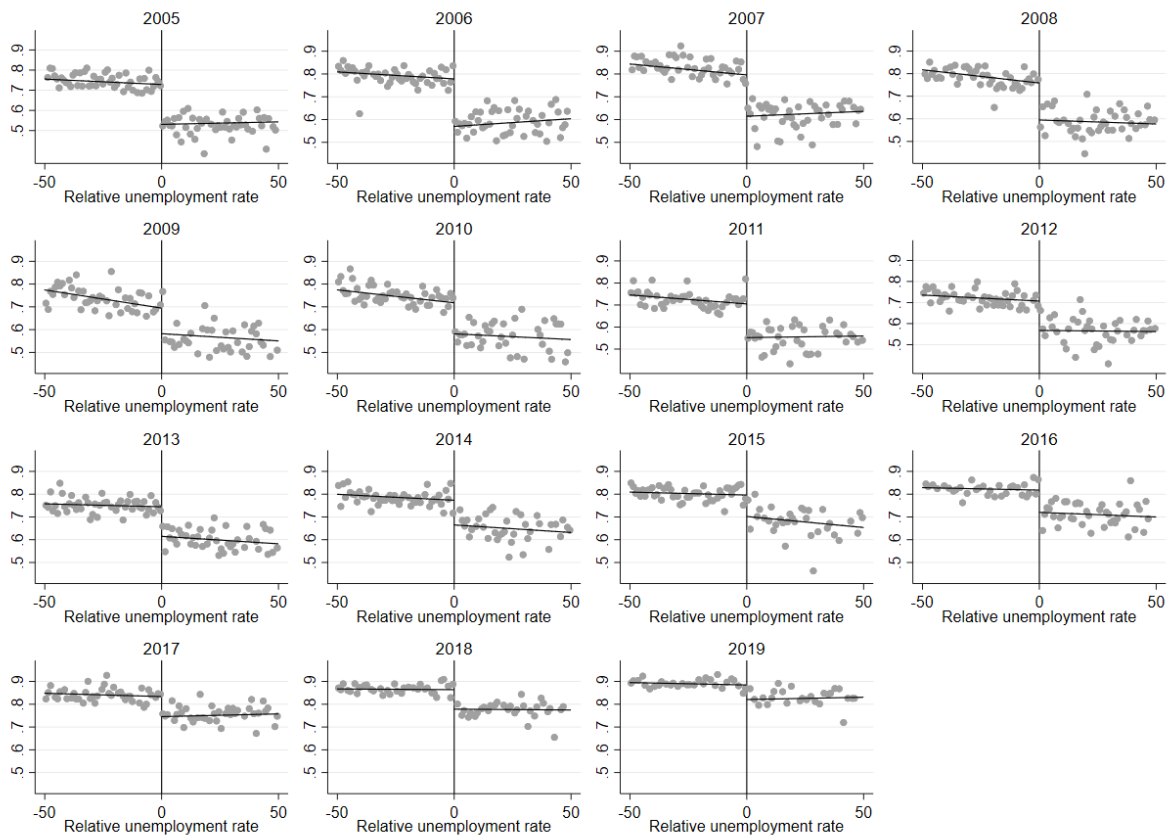
*Notes:* The figure shows how the PBD in a county depends on the relative unemployment rate of a county over time. Before June 2004, the threshold for 12 months PBD was 100%. Between June 2004 and January 2009, it was at 125%, since then the threshold is 150%. For 12 months PBD the county must have exceeded the threshold in June of the preceding year.

Figure A.3: Benefit duration of eligibles relative to the cut-off over time



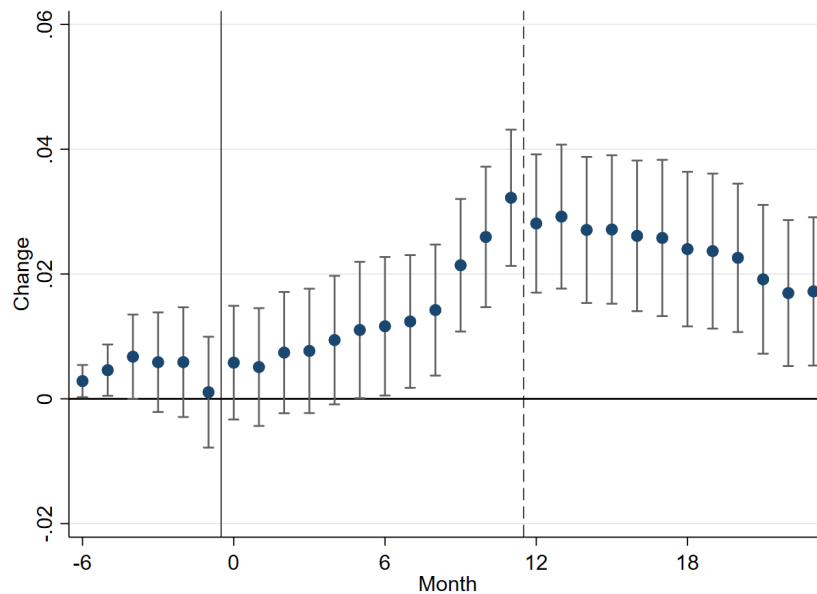
*Notes:* The figure shows the average benefit duration in months relative to the PBD determining cut-off, centred around zero, over time. Observations are at the county-month level and in bins of one percentage point.

Figure A.4: Share of eligibles with unemployment duration  $\leq 12$  months



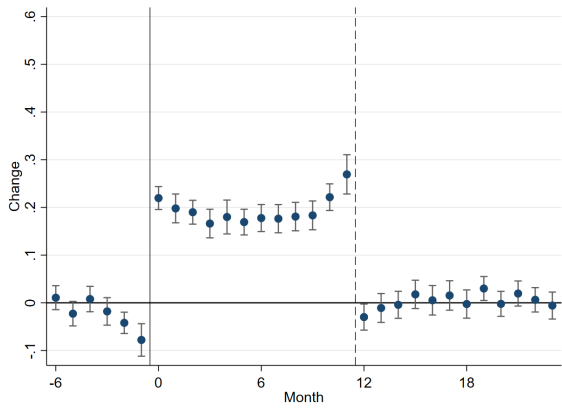
*Notes:* The figure shows the share of benefit recipients with an unemployment duration equal to or below 12 months relative to the PBD determining cut-off over time. Observations are at the county-month level and in bins of one percentage point.

Figure A.5: Effect on (log) unemployment rate

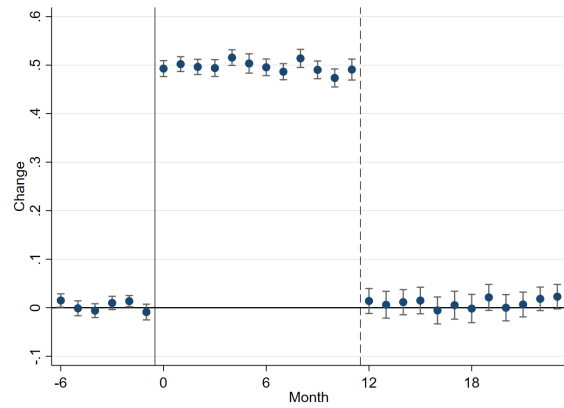


*Notes:* The outcome variable is the log of the official monthly unemployment rate calculated by Statistics Poland. See Figure 6 for other notes.

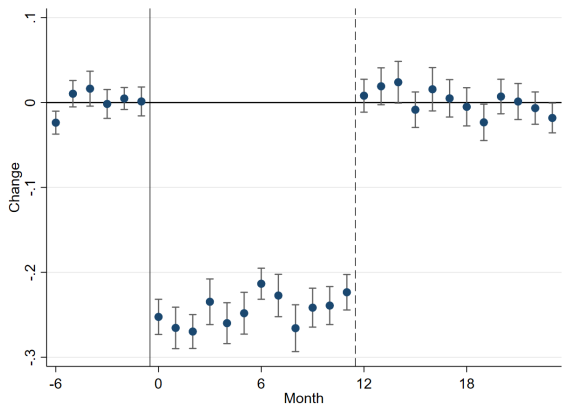
Figure A.6: Effect on (log) durations of eligibles



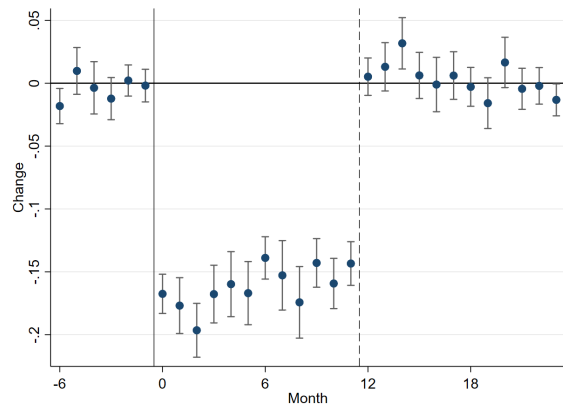
(a) Unemployment duration



(b) Benefit duration



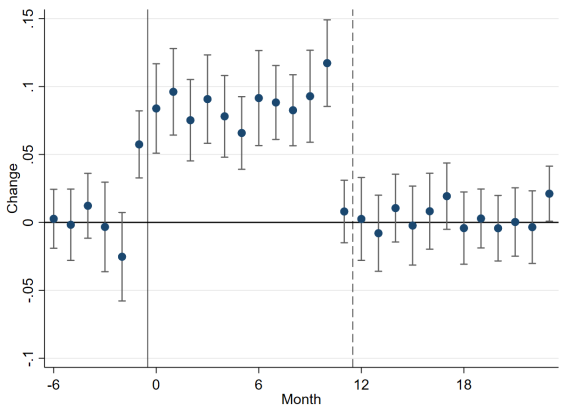
(c) Share unempl. duration  $\leq$  12 months



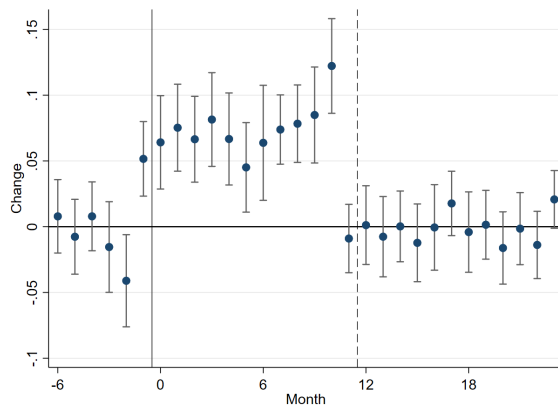
(d) Share jobless duration  $\leq$  12 months

*Notes:* The figure shows the monthly estimates for (log) unemployment and (log) benefit duration for which annual summary estimates are contained Table 4. See this table and Figure 6 for other notes.

Figure A.7: Log job endings of eligibles



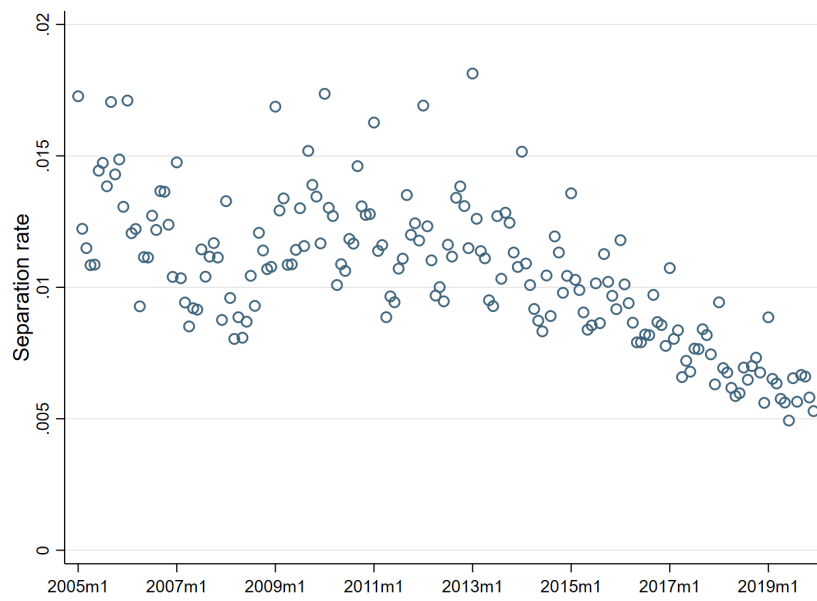
(a) Quitters and laid off



(b) Laid off

*Notes:* The graphs show the effect on (log) job endings per month and county.

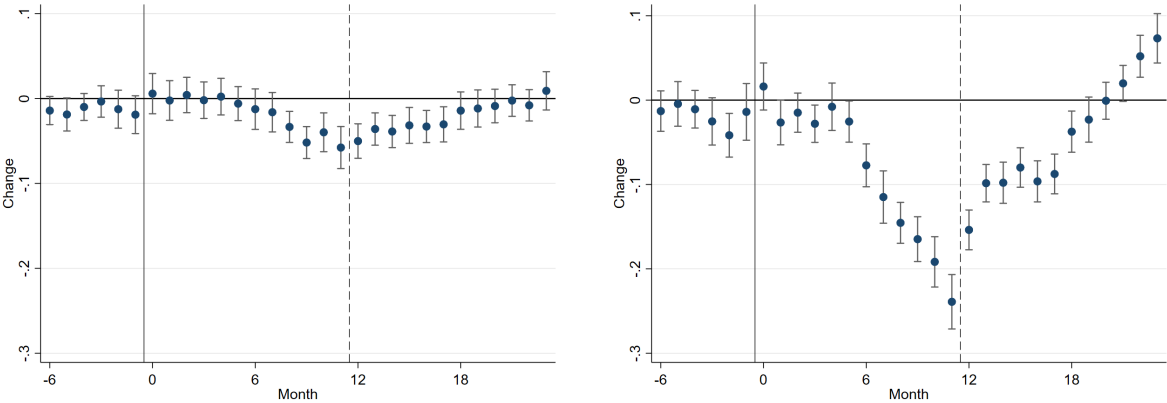
Figure A.8: Separation rates per month



*Notes:* The separation rate is calculated by dividing inflows obtained from our micro data through annual national employment calculated by Statistics Poland.

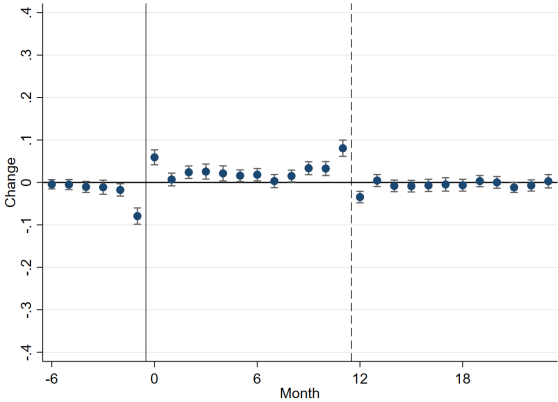


Figure A.9: Effect on (log) exit rates and on (log) inflows into unemployment for all unemployed



(a) Exit rate: All unemployed

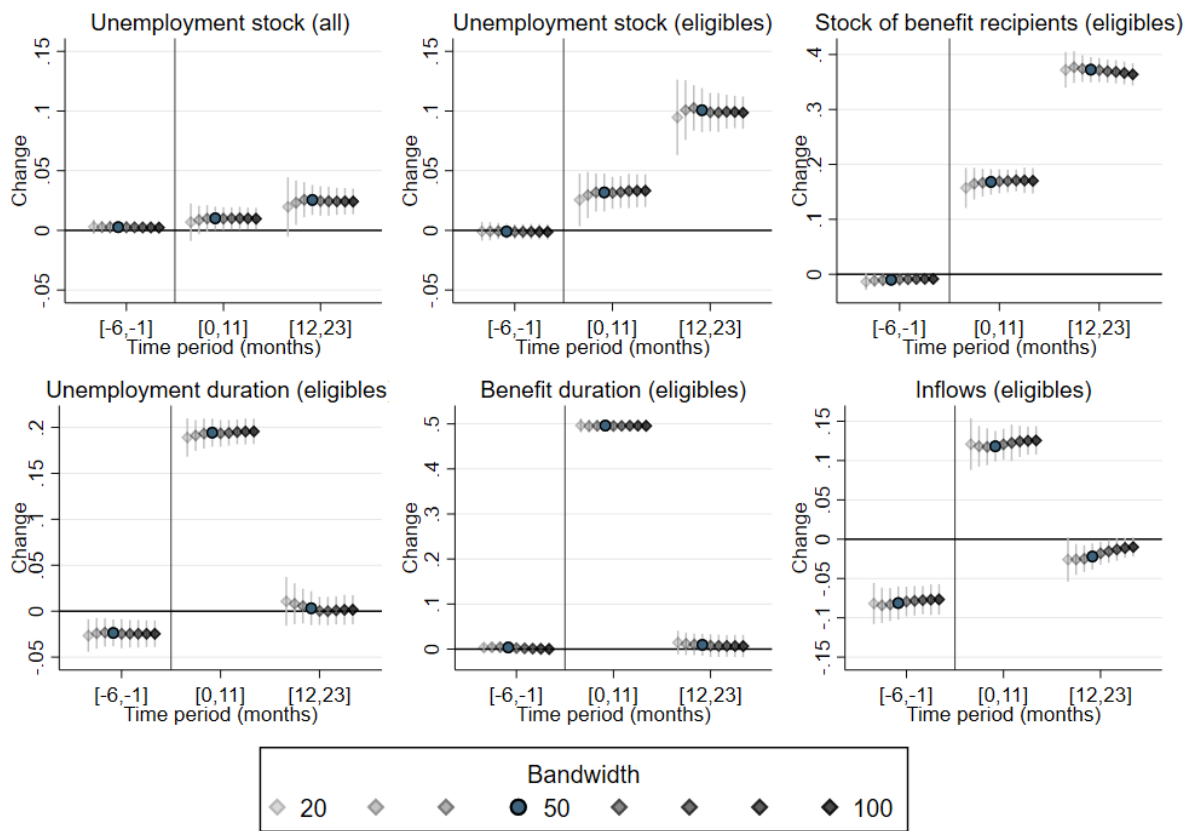
(b) Exit rate: Eligibles



(c) Inflows: All unemployed

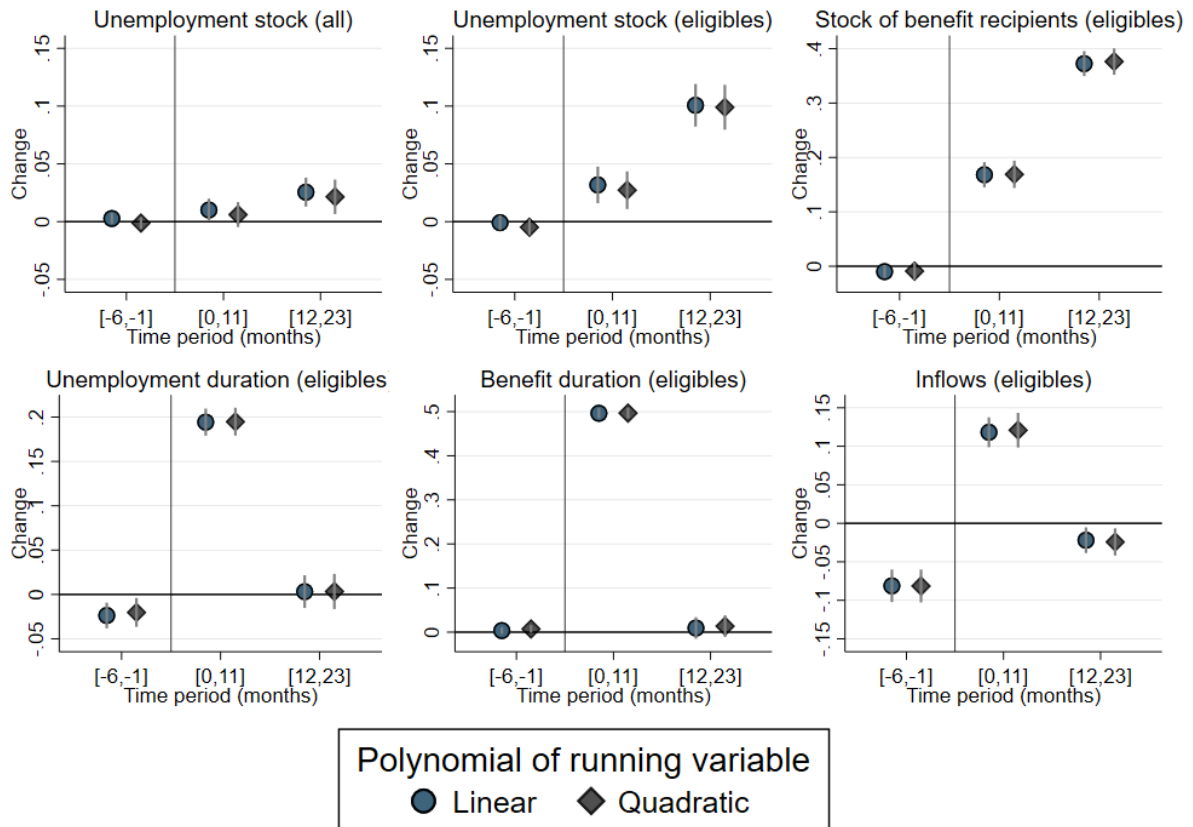
*Notes:* All outcomes are in logs. Figures shows estimates for the exit rate, defined as the monthly outflows from unemployment divided by the stock, and inflows for all unemployed. These estimates (for outcomes normalised by the county population) are used for the decomposition in section 6.

Figure A.10: Robustness of coefficients to choice of bandwidths



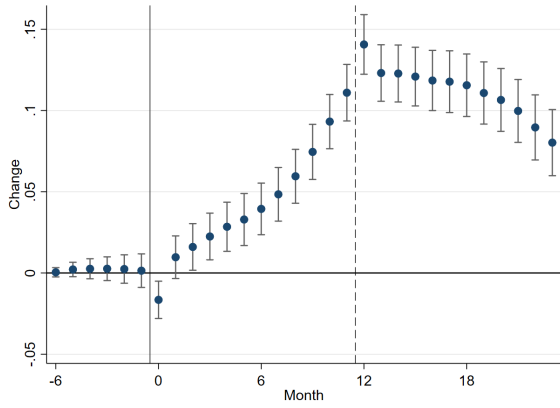
*Notes:* All outcomes are in logs. Coefficients are obtained from varying the bandwidth of the forcing variable (relative unemployment rate) in steps of 10 from 20 to 100. Bandwidth of 50—blue circles—is used in the main analysis. Grey spikes denote 95% confidence intervals.

Figure A.11: Robustness of coefficients to choice of polynomials

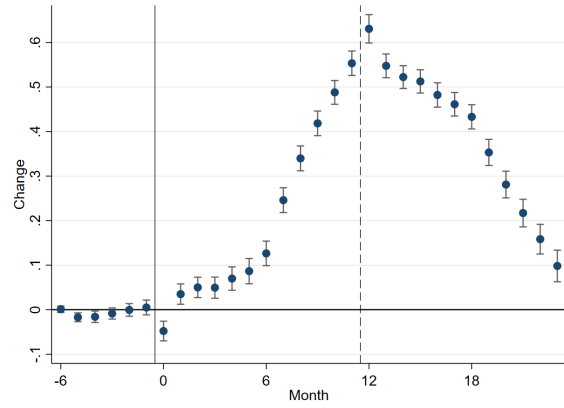


*Notes:* All outcomes are in logs. Coefficients are obtained from estimating equation (1) with either the function  $f_j$  either specified with a linear (blue circles, estimates in the main analysis) or a quadratic polynomial, in each case allowed to differ on either side of the cut-off. Grey spikes denote 95% confidence intervals.

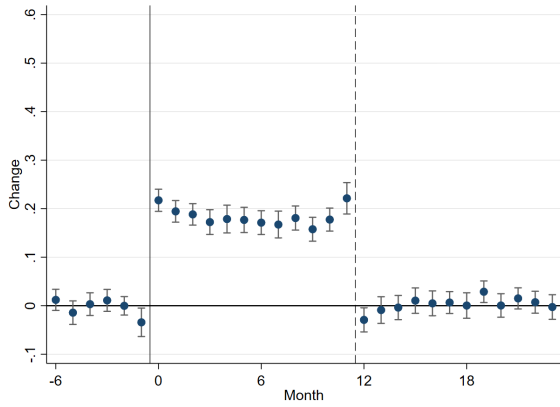
Figure A.12: Robustness—laid off and quitters



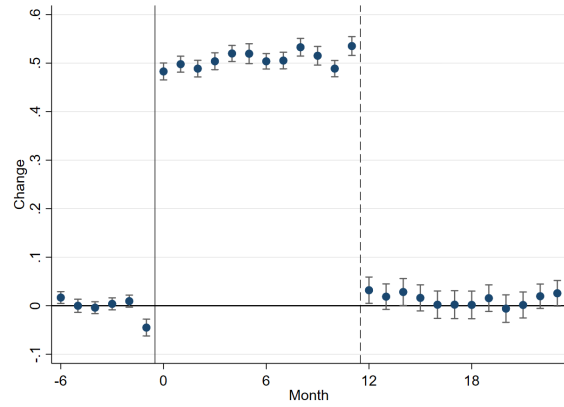
(a) Unemployment stock



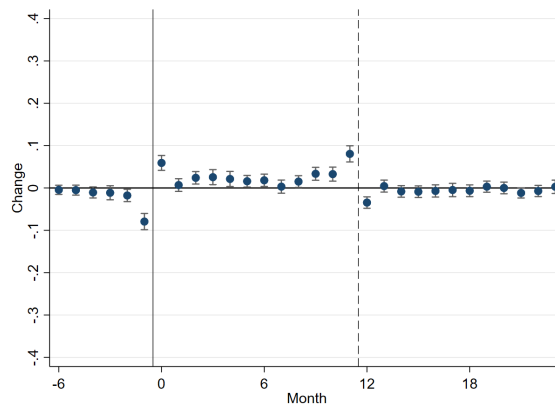
(b) Stock of benefit recipients



(c) Unemployment duration



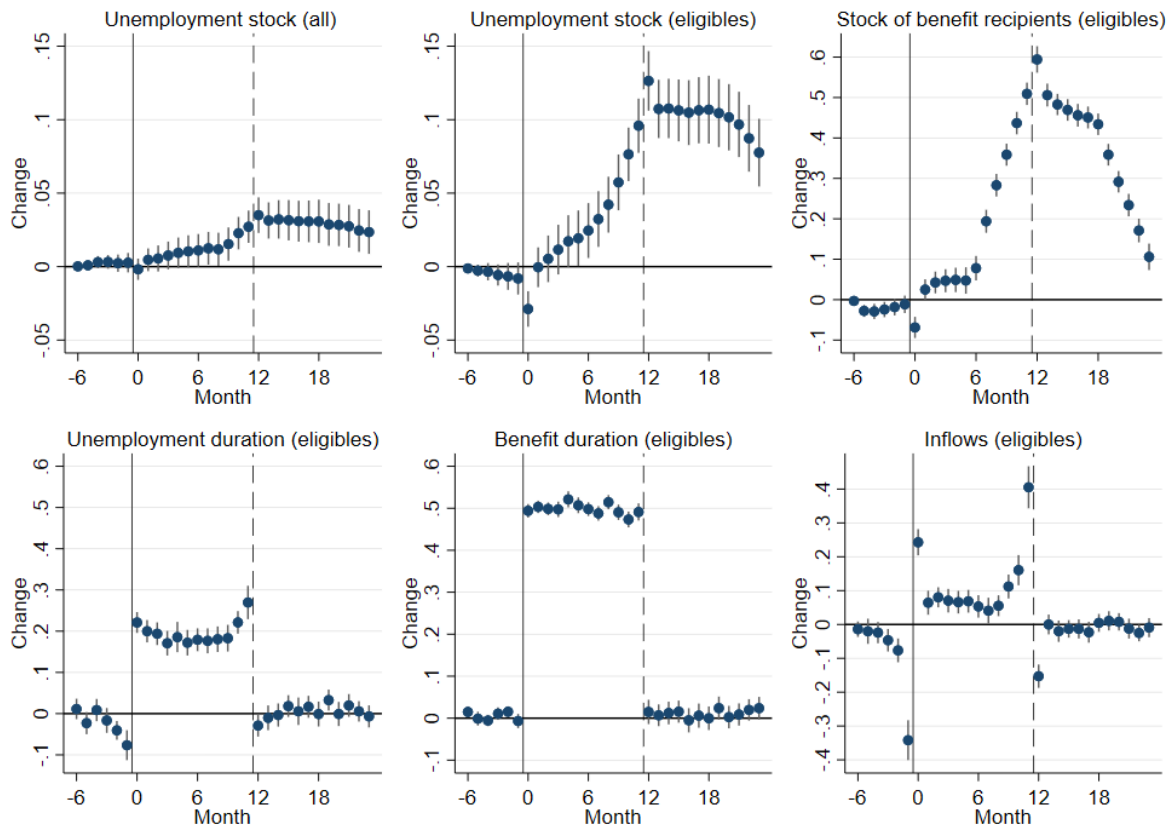
(d) Benefit duration



(e) Inflows

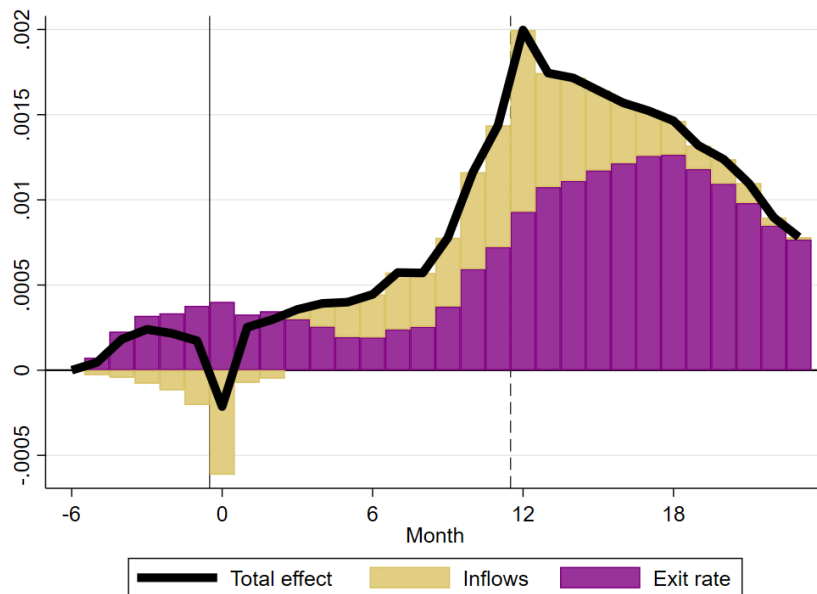
*Notes:* All outcomes are in logs. In the main paper the estimates for eligibles only refer to those who were laid off (86.6%) of all eligibles. Estimates in this figure refer to all directly affected, i.e. eligibles who were laid off or have quit themselves.

Figure A.13: Robustness of coefficients to controlling for unemployment rate



Notes: All outcomes are in logs. In this estimation we additionally control for 12 lags of the logged monthly unemployment rate of counties. Grey spikes denote 95% confidence intervals.

Figure A.14: Decomposition of the effect on the relative stock of all unemployed



Notes: The figure shows a decomposition of the effect on the relative stock (normalised) by the county population of all unemployed into a share attributed to the effect on inflows and an effect on the exit rate.

## B Tables

Table B.1: Summary statistics - individual level

Variable	Obs	Mean	Std. Dev.
<i>All unemployed</i>			
Age in years	33,598,257	33.962	11.947
Female	33,598,257	.465	.499
Contributory spells in years	33,598,257	6.651	8.926
Unemployment duration in months	33,598,257	9.172	13.126
<i>Eligibles</i>			
Age in years	5,407,796	33.457	8.262
Female	5,407,796	.518	.5
Contributory spells in years	5,407,796	8.641	7.49
Unemployment duration in months	5,407,796	10.386	12.593
Benefit receipt in months	5,407,796	5.6	3.512

*Notes:* The table shows summary statistics from calculated from individual unemployment spells from January 2004 to June 2020.

Table B.2: RD estimates - individual level

Sample:	Exit from			Entry into	
	benefits	unemployment		employment	
	Eligibles (1)	All (2)	Eligibles (3)	All (4)	Eligibles (5)
$\leq 3$ months	-0.0212 (0.0074)	-0.0037 (0.0089)	-0.0206 (0.0075)	-0.0075 (0.0085)	-0.0349 (0.0081)
$\leq 6$ months	-0.5781 (0.0070)	-0.0089 (0.0090)	-0.0437 (0.0093)	-0.0138 (0.0089)	-0.0607 (0.0097)
$\leq 9$ months	-0.4802 (0.0074)	-0.0310 (0.0084)	-0.1494 (0.0092)	-0.0345 (0.0083)	-0.1348 (0.0090)
$\leq 12$ months	-0.0064 (0.0007)	-0.0334 (0.0074)	-0.1652 (0.0088)	-0.0305 (0.0073)	-0.1225 (0.0083)
$\leq 15$ months	-0.0039 (0.0004)	-0.0152 (0.0064)	-0.0817 (0.0072)	-0.0117 (0.0060)	-0.0533 (0.0062)
$\leq 18$ months	-0.0000 (0.0000)	-0.0072 (0.0056)	-0.0452 (0.0061)	-0.0056 (0.0051)	-0.0306 (0.0050)
Observations	3,152,639	19,592,663	3,152,639	9,419,958	2,104,392

*Notes:* *Eligibles* denotes benefit recipients below the age of 50 who were laid off. *All* refers to all newly unemployed. The units of observation are individual unemployment spells. The estimation uses the *rdrobust* command by Calonico et al. (2017) with a linear polynomial and a symmetric bandwidth of 50. We do not include lags, leads or other control variables. Standard errors clustered at the county level in parentheses.

Table B.3: Heterogeneous effects on inflows of eligibles

Month $\in$	[-6, -1] (1)	[0, 11] (2)	[12, 23] (3)	[-6, -1] (4)	[0, 11] (5)	[12, 23] (6)
<i>Gender of inflows:</i>		Women			Men	
Coefficient	-0.1104 (0.0148)	0.1514 (0.0128)	-0.0129 (0.0107)	-0.0636 (0.0131)	0.0926 (0.0125)	-0.0278 (0.0108)
Observations	60,784			60,784		
<i>Age group of inflows:</i>		Below median			Above median	
Coefficient	-0.0753 (0.0102)	0.0961 (0.0123)	-0.0124 (0.0092)	-0.0840 (0.0135)	0.1440 (0.0113)	-0.0316 (0.0090)
Observations	60,784			60,784		

*Notes:* The table shows heterogeneous effects on (log) inflows stratified by gender and age corresponding to Table 7.