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

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Technische Universität Dortmund, Department of Economic and Social Sciences
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

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Jonas Jessen, Robin Jessen, Ewa Gałecka-Burdziak,
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The Micro and Macro Effects of Changes in the Potential Benefit Duration

Abstract

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 the 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 cut-off using regression discontinuity estimates on registry data containing the universe of unemployed from 2005 to 2019. For those 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 micro effect). The aggregate effect on unemployment (the macro effect, which includes equilibrium effects) is entirely explained by this increase. 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. We cannot reject that the micro effect equals the macro effect. 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 half of the overall increase in unemployment.

JEL-Codes: H55, J20, J65

Keywords: Unemployment benefits, extended benefits, spell duration, separation rate, regression discontinuity

November 2024

** Jonas Jessen, IZA, IAB, and Berlin School of Economics; Robin Jessen, RWI and IZA; Ewa Gałecka-Burdziak, SGH Warsaw and Life Course Centre, Australia; Marek Góra, SGH Warsaw and IZA; Jochen Kluve, HU Berlin, KfW Development Bank, and IZA.* *– We are grateful to Almut Balleer, Gabriel Chodorow-Reich, Philipp Jäger, Simon Jäger, Xavier Jaravel, Andy Johnston, Philip Jung, Johann(es) König, Helmut Lütkepohl, Ioana Marinescu, Kurt Mitman, Michael Oberfichtner, Arianna Ornaghi, Maximilian Pöhnlein, Roland Rathelot, Regina Riphahn, Enrico Rubolino, Laura Schmitz, Daphné Skandalis, Viktor Steiner, Simon Trenkle, Joanna Tyrowicz, Felix Weinhardt, Izabela Wnuk-Soares, and seminar participants at IZA, EALE 2020 and 2023, IAAE 2021, 2022, and 2023, LEW 2022, the 4th IZA/Higher School of Economics Workshop, IZA Summer School in Labor Economics 2024, the 12th CESifo Norwegian-German Seminar on Public Sector Economics 2021, IIPF 2022, Berlin Applied Micro Seminar, Free University of Berlin, Goethe University Frankfurt, Warsaw School of Economics, RWI, TU Dresden, IAB, DIW Berlin, IWH, the German Federal Ministry of Labour and Social Affairs, and the BSoE Postdoc and Junior Professor Workshop for helpful comments. We are grateful to the Ministry of Family, Labour and Social Policy of the Republic of Poland for giving access to the data. Access to the data can be obtained from the Ministry of Family, Labour and Social Policy of the Republic of Poland. Ewa Gałecka-Burdziak acknowledges funding within the project "Registered unemployment as a non-traditional route to non-participation of older workers. Recurrent event longitudinal data analysis" financed by the National Science Centre Poland, project no. UMO-2018/30/E/HS4/00335. The project is also co-financed by the Polish National Agency for Academic Exchange. –* *All correspondence to: Robin Jessen, RWI, Zinnowitzer Str. 1, 10115 Berlin, Germany, e-mail: robin.jessen@rwi-essen.de*

1 Introduction

What is the effect of a change in the potential unemployment benefit duration (PBD) on the level of unemployment? The PBD can impact unemployment through three channels. First, it impacts individual job search effort. This is often referred to as the micro effect. Second, it can impact 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,[1] the evidence on the effect on aggregate labour market outcomes is mixed (see, e.g., Acosta et al. (2023); Chodorow-Reich et al. (2019); Hagedorn et al. (2019b); Johnston and Mas (2018); Karahan et al. (2022); Lalive et al. (2015); Marinescu (2017) and, see Landais et al. (2018a) for an overview. 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 longer PBD on aggregate unemployment. We use rich registry data of the universe of unemployment spells in Poland covering 2005 to 2019 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 durations, 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 their 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.[2] The cut-off creates exogenous variation in the PBD between counties with essentially identical economic conditions. Treatment is then randomly assigned around the cut-off, due to which this natural experiment resembles a designed experiment (Fuchs-Schündeln and Hassan, 2016). We use this natural experi-

[1] 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; Schmieder et al., 2012), 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 Rodríguez-Planas, 2020) and Poland (Gałecka-Burdziak et al., 2021).

[2] 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%.

ment in a regression discontinuity (RD) framework to quantify the effects of a six months longer PBD. Controlling for the running variable, i.e., the relative unemployment rate of June of the previous year, accounts for remaining differences between counties due to differences in past economic conditions. 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.

The PBD of counties in proximity of the cut-off changes as the relative unemployment rate moves above or below the threshold. Many PBD increases are thus temporary. Our estimates therefore directly speak to the literature studying effects of temporary UI extensions, which have been introduced in every recession since 1973 in the US and in many countries during the COVID-19 pandemic. An important difference is, however, that such extensions occur in response to high *absolute* unemployment rates. In contrast, in Poland the PBD is determined by the *relative* unemployment rates of counties, and each year some counties are treated with a longer PBD. Consequently, our findings are not restricted to unfavourable economic conditions.[3]

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 quarter of all unemployed) and the *indirectly affected* (older workers and those ineligible to receive benefits). Estimating these labour market externalities allows us to assess the impact of PBD changes on labour market tightness. This is important for at least two reasons. First, it allows us to calculate the effect of a hypothetical change in the PBD for all workers, the macro effect. Second, if the PBD impacts labour market tightness, this has important policy implications. Labour market tightness is often at an inefficient level and varies over the business cycle. In principle, policy makers can vary the PBD over the business cycle to correct for this inefficiency (Landais et al., 2018b). In a final step, we quantify the impact of a longer PBD on inflows into unemployment. This allows us to decompose the total increase in unemployment into contributions of changes in inflows into and outflows from 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 the directly affected unemployed, corresponding roughly to

[3] As regression discontinuity estimates identify local average treatment effects around the cut-off, our results concern counties in proximity of 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%.

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 only indirectly affected unemployed, we identify no effect on their unemployment duration. These findings are in line with a decrease 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 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 (or firms) 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.[4] 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 in 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.[5] In

[4]This is illustrated in Hagedorn et al. (2019b) who decompose the job-finding rate into two elements:

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

where θ_t is labour market tightness.

[5]Abstracting from the effect of PBD changes on separations, 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).

contrast, our finding of no spill-overs of PBD changes in Poland is in line with a horizontal labour demand curve as in Hall (2005).

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 ($1 - \frac{\text{macro elasticity}}{\text{micro elasticity}}$). When the elasticity wedge is positive, i.e. the micro elasticity exceeds the macro elasticity, an increase in UI generosity 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 in order to correct inefficient tightness.

The literature on aggregate effects of PBD changes focuses on benefit extensions in the US and finds mixed results (Acosta et al., 2023; Boone et al., 2021; Chodorow-Reich et al., 2019; Dieterle et al., 2020; Hagedorn et al., 2019a,b; 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. (2019b) 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 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. Acosta et al. (2023) compare outcomes in states qualifying for the same "trigger rules" to extend the PBD, but which adopted different rules. They find that macroeconomic effects of extensions are only pronounced when initial unemployment durations are short and that general equilibrium effects are small overall.

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 the 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. (2024) 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 officially 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 2024, 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[6] in June of the previous year *relative to the country-wide mean.* If the previous years' county unemployment rate is above a certain threshold, the PBD for eligible prime-age workers is 12 months, and 6 months otherwise. The revised and PBD-determining June unemployment rates for Poland and each county are announced in September. From then onwards, workers and firms could act in anticipation of upcoming PBD changes. We illustrate the timing of PBD changes and the potential anticipation period in Appendix Figure A.2. The preliminary estimate of the unemployment rate for June is published towards the end of July. Therefore, agents might already update their beliefs from August onwards. However, the preliminary rate often differs from the revised one.

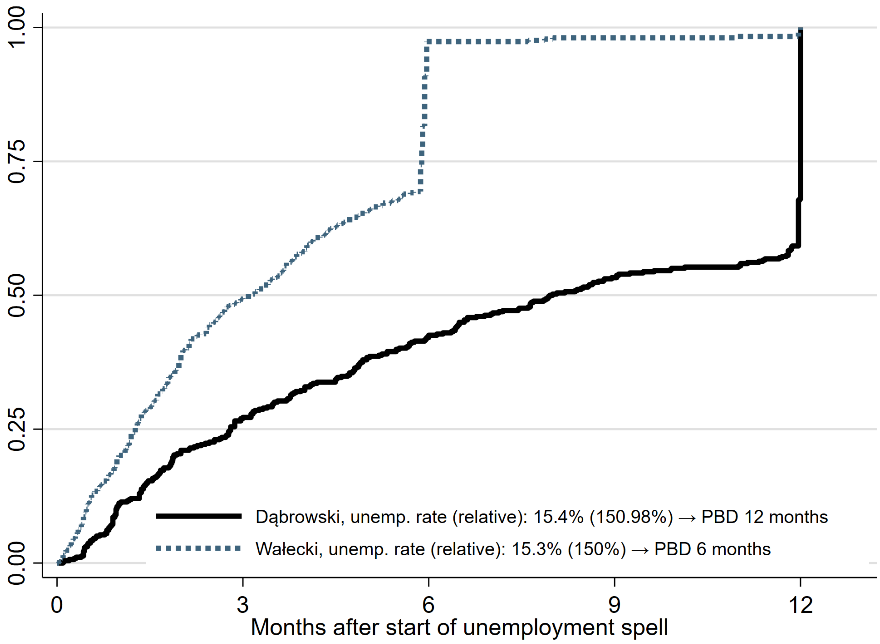
The system shares some similarities with the Extended Benefits programme in the US where the PBD can be extended in states when unemployment is high and growing (Rothstein, 2011), which led to state-level variation in PBD in the 2007-2009 recession. As shown in Landais et al. (2018b), it might indeed be optimal that the transfer system is more generous in economic slumps. A crucial difference between the US system and the Polish system is that in Poland, the threshold for receiving a longer 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 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 2024. Thus, since 2009 a PBD of 12 months is applied to a lower share of counties. Appendix Figure A.3 describes the set-up over time. For newly unemployed to receive 12 months of benefits, the unemployment rate in 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

[6] In principle, workers could move to a county with a longer PBD, but as a proof of residence is required, the cost of actually moving would typically exceed the gain of a 6 months longer PBD.

Figure 1: Exit from unemployment benefits in Dąbrowski and Wałecki

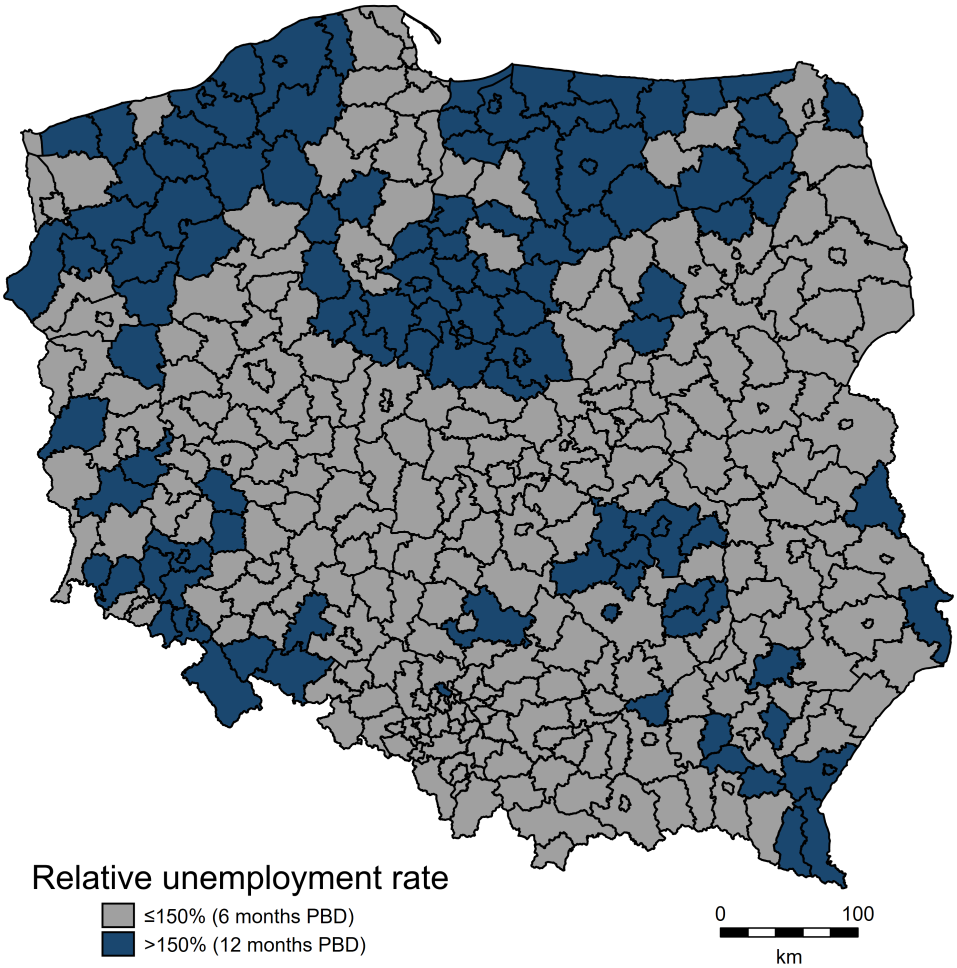


Notes: The figure shows Kaplan-Meier failure functions for exiting benefits in two counties in 2016. Unemployment rates refer to June 30, 2015. Benefit recipients in Dąbrowski could receive 12 months of benefits as their county had just exceeded the threshold of 150%. Recipients in Wałecki could receive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

that month. In the county Wałecki 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 Wałecki was 150% and the one in Dąbrowski 150.98%, benefit recipients in Wałecki 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 Wałecki 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 a map of Poland with the 380 counties coloured according to 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, but can be found throughout the country. Importantly, regional spill-overs of PBD increases are unlikely to play a relevant role in our context as 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.

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 ≤150% have a PBD of 6 months in 2016, county with a relative unemployment rate of >150% have 12 months.

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. As a result, benefit recipients registering in the same county in December or January may have different PBDs. While counties far from the cut-off are unlikely to cross the threshold regularly, leading to a change in the PBD, this can happen frequently for counties near the threshold. Over the time period we consider in our analysis, we find that 35% of counties (133 out of 380) have different PBDs over time. In Appendix Figure A.4 we show a map indicating the counties that, in our sample period, have different PBDs over time and those who constantly have a PBD of either 6 or 12 months. Appendix Figure A.5 plots the relative unemployment rate over time for each of the 133 counties whose PBDs change in our sample period to illustrate the identifying variation we are exploiting in our estimates. Year-to-year changes in the county-level relative unemployment rates roughly follow a symmetric distribution as shown in Appendix Figure A.6.

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.[7] 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%.[8]

Besides receiving benefits, the unemployed in Poland have additional incentives to be and remain registered, as it guarantees free health insurance (if ineligible otherwise) and access to job search assistance. To receive social assistance benefits, a proof of unemployment is required. These pecuniary and non-pecuniary benefits incentivise prompt registration after becoming unemployed although there is no time limit regarding registration. There are no formal search requirements except that the unemployed have to indicate their willingness to work in monthly meetings. Compared to other institutional settings, deregistering from the unemployment register as a transition into non-employment or inactivity occurs less frequently.[9] Of the unemployed eligible to receive benefits and under 50 years old, 63.6% directly enter into employment when leaving the unemployment registry (the shares are 49.5% for all unemployed and 41.8% for those ineligible to receive benefits).

There are some notable exceptions to the institutional rules which are relevant for our analysis.[10] First of all, not all newly unemployed are eligible to receive benefits. Of the 30.6 million unemployment spells we observe from 2005, 28% are eligible to receive benefits. 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 June 2004 these older workers have a PBD of 12 months irrespective of the relative unemployment rate of their county.[11] We use these two groups of unemployed for whom their counties' relative unemployment rates do not affect the PBD— those ineligible for benefits and older workers—to examine market externalities of a longer PBD.

[7] https://data.oecd.org/benwage/benefits-in-unemployment-share-of-previous-income.htm, last accessed July 9, 2024.

[8] After benefit expiration or, if necessary, during the unemployment benefit spell, one can receive social assistance which are subjected to means-testing with regard to the family income. Maximum social assistance benefits are always lower than minimum unemployment benefits.

[9] Deregistering by the employment offices can occur if job seekers turn down job offers without an adequate justification, but such cases are uncommon in Poland.

[10] 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.

[11] Before June 2004, 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.

In some cases, the PBD can also be cut below the regular duration. For instance, when a person quits a job instead of being laid off by their employer, the eligibility period is cut by three months and payment only begins after three months of unemployment. In most analyses of directly affected unemployed, we restrict the sample to those who were laid off—around 85% of all benefit recipients. The motivation for this is 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 durations with respect to the PBD. We also include quitters in some analyses, as their PBD varies between 3 and 9 months depending on the relative unemployment rate. This results in the same 6-month difference based 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.[12] We exclude the year 2009 from our estimation sample, as the PBD threshold changed in February during that year. Nonetheless, observations from that year enter as leads or lags in our estimation.

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 deregister 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 longer PBD may affect

[12] Our analysis is restricted to unemployment spells which starting between January 2005 and December 2019, but we observe exits from unemployment up to July 2021.

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 for all individuals who have started their unemployment spell in a given month.

We additionally calculate some aggregate outcomes for benefit recipients only. As we do for all unemployed, we calculate 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 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) unemployed eligible for benefits under 50 who quit or were laid off, (iv) unemployed eligible for benefits under 50 who were laid off, (v) benefit recipients aged 50 or above with at least 20 contributory years.[13] 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 4669 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.[14] Inflows into and outflows from unemployment are 451 and 478 individuals per month, respectively. The exit rate, calculated as the monthly share exiting unemployment, is 11% on average and two percentage points lower in counties with a 12 months PBD.

Panel B focuses on eligibles. These are individuals whose PBD is directly affected

[13] In our sample, the shares of groups (ii) to (v) relative to the total stock of unemployed are 59.6%, 24.7%, 20.8% and 5.0%, 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 such that the exact PBD cannot be determined, as well as older unemployed ineligible for benefits or with fewer than 20 years of contributory spells.

[14] 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)
Panel A: All unemployed				
Unemployment duration (months)	8.95	8.56	9.78	67,836
Stock of unemployed	4668.55	4446.52	5127.37	67,836
Inflows into unemployment	451.06	451.66	449.80	67,836
Outflows from unemployment	478.31	477.36	480.27	67,836
Exit rate from unemployment	0.11	0.12	0.10	67,836
Panel B: Eligibles				
Unemployment duration (months)	9.97	9.08	11.80	67,836
Benefit duration (months)	5.40	4.41	7.45	67,836
Stock of unemployed	872.40	793.36	1035.73	67,836
Stock of benefit recipients	419.83	348.20	567.86	67,836
Inflows into unemployment	70.84	70.70	71.13	67,836
Outflows from benefit receipt	73.71	73.92	73.28	67,836
Outflows from unemployment	78.96	78.26	80.39	67,836
Exit rate from unemployment	0.10	0.11	0.08	67,836
Exit rate from benefit receipt	0.19	0.22	0.13	67,836
Panel C: Other outcomes				
Unemployment rate	0.14	0.11	0.20	67,836
New vacancies posted	263.84	297	184.05	40,716
Stock of vacancies	187.33	219.09	110.90	40,716
Wage of job offers in Złoty	2195.29	2220.86	2134.65	27,141
Panel D: Other variables				
Population	100748.13	115040.50	71213.68	67,836

Notes: The table shows summary statistics for county-level characteristics at the monthly level from January 2005 to December 2019. Monthly data on vacancies available for 2011-2019, and wages of job offers for 2014-2019. Eligibles refer to unemployed eligible for benefit receipt who are under 50 years and were laid off.

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 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 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.[15]

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 registry 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 longer PBD; a longer 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 at 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.[16]

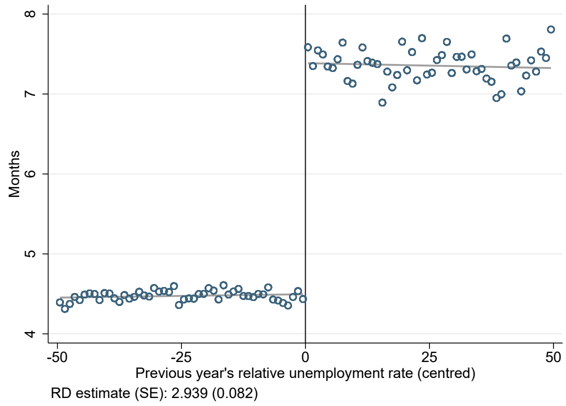
4 Empirical Strategy

To motivate our RD estimation, we plot the distribution of the average benefit and unemployment durations as well as the share of unemployed individuals eligible to receive benefits with unemployment durations up to 12 months in Figure 3. Each circle represents a one-percent bin of month-county observations of the running variable—the relative unemployment rate in June of the previous year, i.e., the variable that determines the PBD—centred around the cut-off and 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

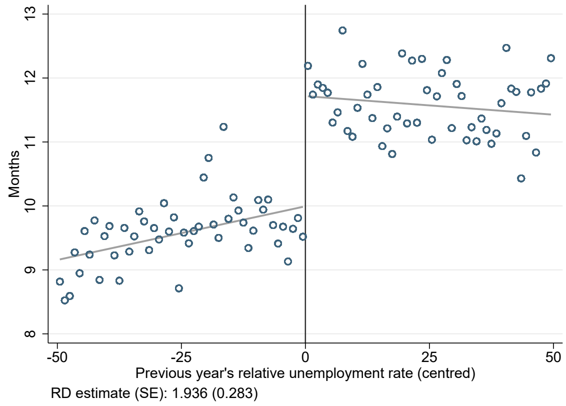
[15] Unfortunately, data on employment and labour force participation at the county-month level are not available.

[16] 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 July 9, 2024).

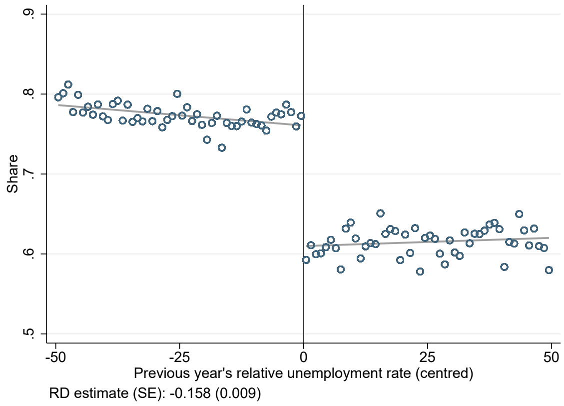
Figure 3: Benefit and unemployment duration of eligibles



(a) Benefit duration



(b) Unemployment duration



(c) Share with unemployment duration $\leq$ 12 months

Notes: The figure shows how benefit duration and unemployment duration relate to the relative unemployment rate in June of the previous year. The relative unemployment rate is centred around the threshold for a longer PBD (150% from February 2009 onward, 125% before that). Each circle represents a one-percent bin of month-county observations of the running variable. The RD estimate is 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.

(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 report results for a binary outcome that the unemployment duration is 12 months or lower and find a drop in the share of 16 percentage points. In Appendix Figures A.7 and A.8 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.

The basic intuition of RD estimates is that observations directly below and directly above the cut-off are very 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[17] 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 in June of the previous year 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.[18] Through the leads we are able

[17] 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 abundant literature on other countries referred to in footnote 1). In Gałecka-Burdziak et al. (2021), individual-level effects for Poland are analysed in more detail.

[18] If additional leads were included this would raise issues of reverse causality as next year's PBD is not yet determined at those points in time through the running variable and is thus influenced by the unemployment rate itself. Recall that the PBD depends on the relative unemployment rate in June of the previous year. Thus, e.g., at the beginning of June, the PBD of the following year is not yet determined. Naturally, workers and firms form expectations about the PBD of the following year, but these expectations will be the same for counties just below or just above the cut-off of the running variable. As soon as agents have information about the unemployment rate in June of the current year,

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. The official PBD-determining unemployment rates are announced in September so that it is implausible that responses to a potential upcoming PBD change occur beforehand. Therefore, including four leads—or five, to account for the possibility that agents react to the preliminary estimate of the unemployment rate published at the end of July—would in principle be sufficient in the estimation. The additional leads allow us to assess pre-trends before the announcement. We note that we obtain similar treatment effects after the PBD increase in a specification that omits leads from equation (1). In the robustness section we present these estimates and find that this has only a limited impact on the point estimates.

We additionally include 12 lags of the outcome variables (fifth sum). Lags of the outcome variable might be an important control variable, in particular for stock variables that only adjust gradually, such as unemployment. For these variables, we expect their first lags to explain much of the variance. If, in addition, the first lag is correlated with any of the explanatory variables, excluding it from the regression would lead to omitted variable bias. For instance, even when controlling for the running variable, the lag of the unemployment stock is positively correlated with the PBD as well as its lags and leads.[19] Therefore, failure to include lags of the unemployment stock leads to an upward bias in the coefficient of the leads of the PBD. In our preferred specification, we control for 12 lags of the dependent variable, covering a full year. In a robustness test, described in section 7, we show that varying the number of included lags has limited impact on the resulting impulse responses.

Finally, we control for time and county fixed effects. In doing so, we account for general time trends, business cycle effects, and time-invariant 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 Ravn (2012) and Christofzik et al. (2022). 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

they update their beliefs about next year's PBD. Conditional on next year's running variable, these beliefs do not depend on the outcome variable, such that reverse causality is not an issue.

[19]This might be at first glance surprising, since the running variable mechanically determines the PBD. However, the relationship between running variable and PBD is nonlinear and therefore some correlation remains.

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. Macroeconomic conditions would be correlated with the PBD and potentially the outcome variable of interest. However, they are controlled for by including the running variable in equation (1). The necessary assumption is that macroeconomic conditions are continuous and do not "jump" at the threshold. Lee and Lemieux (2010) provide an explanation of this assumption in the potential outcomes framework. Thus, macroeconomic conditions 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. In sum, the error term is not correlated with any of the regressors, fulfilling the standard OLS zero conditional mean assumption. We also find that for our main outcomes the error term exhibits no serial autocorrelation when at least two lags are included, see section 7.

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, where $m \in [0, 11]$ 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).[20] 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:

$$\tilde{\delta}_{-6} = \delta_{2,6} \tag{2}$$

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:

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

The last term in equation (3) captures that the outcome, for instance the log stock of unemployed, depends on its own lag via the coefficient $\delta_{3,1}$. The term must be included in equation (3), but not in equation (2), because in our specification a change in the PBD in $m = 0$ might have an impact on the outcome in $m = -6$, but not in $m = -7$. Thus, $Y_{i,-7}$ are the same in the path with a PBD increase and the baseline path. The impulse response captures the difference between these two time paths, which is zero in $m = -7$. $\tilde{\delta}_{-6}$ is the difference between the outcome variable in the path with a PBD increase and the baseline path in $m = -6$.

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:

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

The cumulative effect in the first month *after* the PBD increase additionally contains

[20] 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., 2024).

the contemporaneous effect of the PBD increase, $\delta_{1,0}$,

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

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

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

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 a dummy that is one if the previous year's PBD was 12 (and zero otherwise) as

well as time dummies and calculate residuals from the regression:

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

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 PBD dummy to one 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 dummy 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 dummy to one for year 2 if the simulated running variable in that year exceeds zero and to zero 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 2.8 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.[21] When interpreting the results, it is important to keep the relatively small impact on the PBD in years 1 and 2 in mind. Accordingly, it is plausible that the updating of beliefs about the PBD in years 1 and 2 only plays a minor role for the reaction to the PBD increase in year 0. 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.

[21] 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.

Standard Errors. Standard errors are obtained via panel bootstrap, also called block or cluster bootstrap, with 200 replications. We apply cross-sectional sampling with replacement, i.e. we draw samples of time-series of counties. We then use each of these resampled panel data sets to run our regressions based on equation (1), simulate the PBD path, and calculate the impulse responses for every draw. For each parameter of interest $\tilde{\delta}_m$, the standard error is simply the standard deviation of this parameter over all draws. A major advantage of the panel bootstrap is that it allows for heteroskedasticity and an arbitrary form of serial correlation as long as the number of counties is large (Cameron and Trivedi, 2005; Kapetanios, 2008). This is relevant in our application, as the error term might be serially correlated. As discussed by Hagedorn et al. (2019b), serial correlation is accounted for using the panel bootstrap procedure. In contrast to the panel bootstrap, the residual-based bootstrap, which is common in time-series econometrics, is based on i.i.d. errors and thus does not account for serial correlation.

4.2 RD Diagnostics

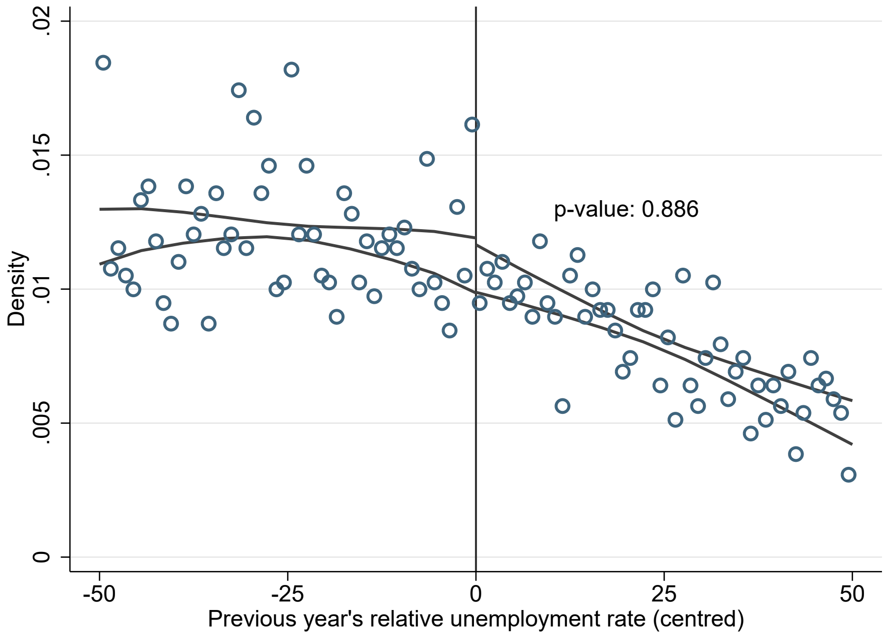
In this subsection, we provide support for the key identifying assumption on which an unbiased RD estimation hinges: Assignment to treatment must be random around the cut-off, such that the only factor that varies discontinuously around the cut-off is the treatment itself (Imbens and Lemieux, 2008). Hence, counties on either side of the cut-off must be equal conditional on the polynomial of the forcing variable[22] (and potentially other control variables). The only meaningful difference between those counties is that eligible unemployed in counties just below the cut-off are entitled to a PBD of 6 months and those in 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.

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 was politically advantageous to have a longer 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

[22] In section 7 we assess the sensitivity of our estimates to the specification of the polynomial.

Figure 4: Density test

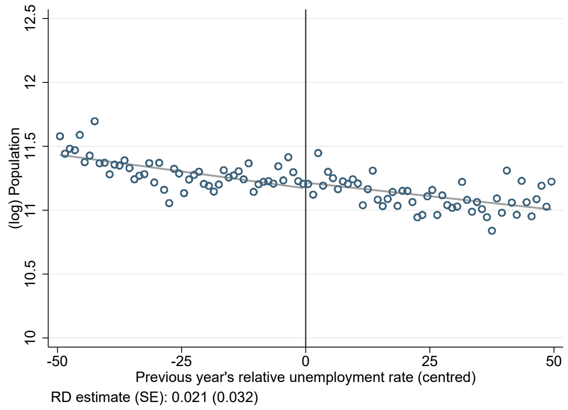


Notes: The figure shows a density test of the running variable (relative unemployment rate to the cut-off) at the annual county-level. The density test follows Cattaneo et al. (2020) with a bandwidth of 50 and a linear polynomial. Dots are density points in one-percent intervals, solid lines denote 95%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

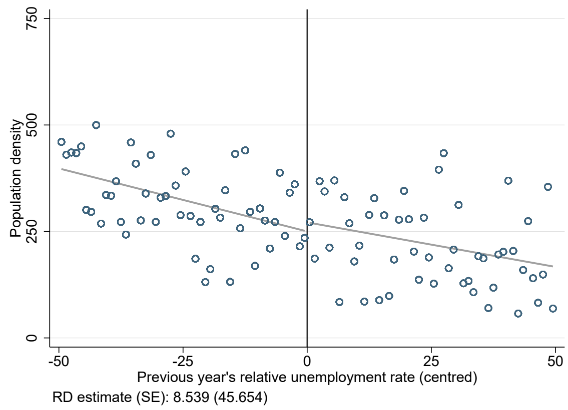
symmetric bandwidth of 50 and a linear polynomial in the estimation. The manipulation testing is based on the local polynomial density estimator by Cattaneo et al. (2020). To illustrate the density distribution of counties, we show one-percent bins. The density of county observations around the cut-off 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 the random treatment assignment assumption is to establish that exogenous characteristics are distributed smoothly around the cut-off. Figure 5 shows the distribution of such county-level characteristics which are highly unlikely to be related to the PBD in a given year. Panels (a) and (b) indicate that counties with a lower unemployment rate have on average a larger population and a higher population density, indicating that counties with higher unemployment rates are more likely to be rural. Panels (c) and (d) examine outcomes related to public expenditure per capita of counties. The distribution around the PBD-determining cut-off is smooth for all variables considered and none of the RD estimates with bias-corrected standard errors (Calonico et al., 2014) reveal a discontinuity around the cut-off. 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.

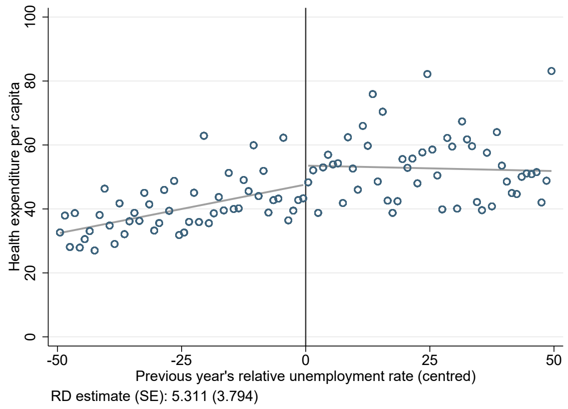
Figure 5: Distribution of county-level characteristics



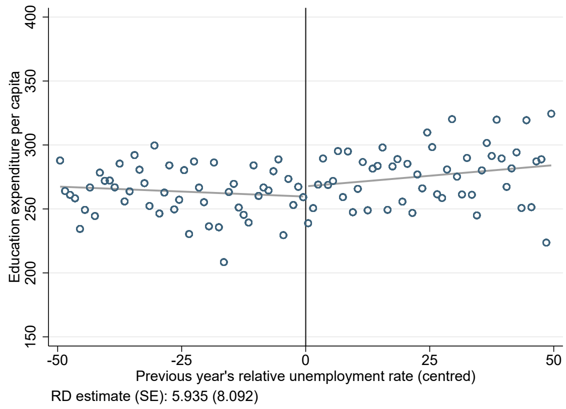
(a) Population



(b) Population density



(c) Health expenditure



(d) Education expenditure

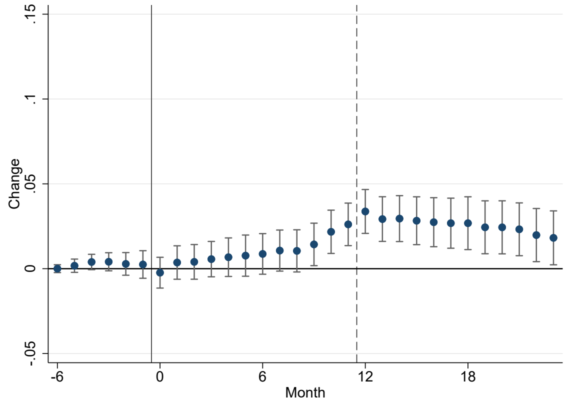
Notes: The figure shows the distribution of county-level characteristics relative to the centred previous year's relative unemployment rate of counties. RD estimates are obtained following Calonico et al. (2014) with a symmetric bandwidth of 50 and a linear polynomial. Data from Statistics Poland. Sample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

5 Results

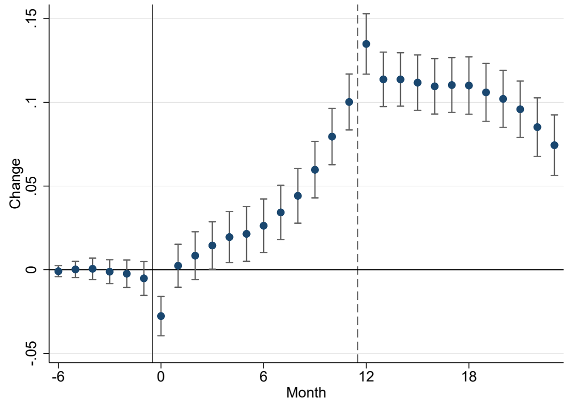
In this section we report the effect of a longer PBD on numerous labour market outcomes. We begin by presenting the effect on the stock of unemployed, before moving on to unemployment and benefit receipt durations. Next, we consider market externalities of a PBD increase by assessing whether the outcomes for the 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.

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 bootstrapped standard errors with 200 replications.

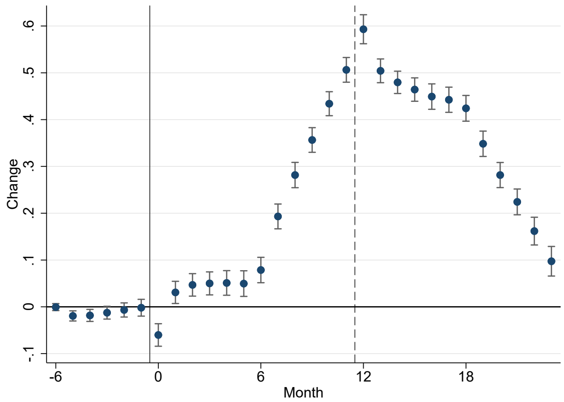
Figure 6: Effect on log stocks of unemployed



(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 panel 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 0.034 log points 12 months after the PBD increase.[23] Effects are larger when focusing on eligibles in Panel (b). Point estimates turn statistically significant after four months and grow continuously up to 0.135 log points after a year. Immediately after the PBD increase, the stock of unemployed drops notably before

[23] The quite modest increase in the stock of unemployed in the first months is a reason why a longer PBD does not have a strong effect on next year's PBD: the PBD is determined by the relative unemployment rate in June and at that point the stock of unemployed has only increased modestly with the confidence intervals including zero.

Table 3: Summary estimates for effect on stocks of unemployed

	Log stock of								
Outcome	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	0.0025	0.0098	0.0260	-0.0015	0.0319	0.1056	-0.0099	0.1682	0.3725
	(0.0024)	(0.0054)	(0.0073)	(0.0030)	(0.0071)	(0.0082)	(0.0057)	(0.0115)	(0.0118)
Observations		37,332			37,332			37,332	

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. Panel bootstrapped standard errors based on 200 replications in parentheses.

returning to its previous level in the following month. This pattern can be explained by the timing of inflows into unemployment, which we will turn to in Figure 7, Table 7, and the corresponding paragraphs.

Table 3 reports the summary coefficients for the effect of a larger PBD for the period 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 obtain 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 0.01 and 0.026 log points increases for all unemployed, and 0.032 and 0.105 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 longer PBD on the stock of benefit recipients is much larger than that on the stock of unemployed. In year 0 the stock 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 0.372 log points (Table 3).

The observed gradual increases of the stocks are expected as only the PBD of newly unemployed is prolonged.[24] 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 longer 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 longer PBD, the stock may start to increase immediately, but still gradually. We decompose the effect on

[24]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.

the stock of eligible 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.

We also show the effect on the log of the official county-level unemployment rate calculated by Statistics Poland in Appendix Figure A.9. 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, so that we focus on the summarising annual coefficients (impulse responses are reported in Appendix Figure A.10). Despite being aggregated at the county-month-level, these estimates allow us to contrast our results 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)).[25] 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 elasticity 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 deregister from unemployment without returning to employment (see Card et al., 2007b, who show for Austria a large spike in the unemployment exit hazard at benefit expiry with no corresponding spike for job-finding). 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 both exit from unemployment and entry into employment are 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*

[25] 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 4: Summary estimates for effect on durations of eligibles

Month ∈	$[-6,-1]$ (1)	$[0,11]$ (2)	$[12,23]$ (3)	$[-6,-1]$ (4)	$[0,11]$ (5)	$[12,23]$ (6)
Panel A: Log durations						
	Unemployment			Benefit		
Coefficient	-0.0237	0.1944	0.0033	0.0036	0.4963	0.0094
	(0.0076)	(0.0079)	(0.0093)	(0.0042)	(0.0062)	(0.0121)
Observations		37,332			37,332	
Panel B: Share duration $\leq$ 12 months						
	Unemployment			Jobless spell		
Coefficient	0.0012	-0.2451	0.0015	-0.0041	-0.1623	0.0032
	(0.0045)	(0.0080)	(0.0076)	(0.0043)	(0.0076)	(0.0061)
Observations		37,332			37,332	

Notes: The durations in Panel A are in logs. Panel B denotes the share with unemployment or jobless duration up to 12 months. Jobless duration only considers unemployed whose reason for deregistering from unemployed is subsequent employment (65%). See Table 3 for other notes.

employment within 12 months after starting their unemployment spell (columns (4)-(6)).[26] 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 longer PBD if the PBD was increased for all unemployed. 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

[26]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.

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

Group of unemployed:	Ineligibles			Benefit recipients, ≥ 50 years and ≥ 20 contributory years					
Outcome	Unemployment duration						Benefit duration		
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	-0.0035	0.0077	0.0152	-0.0022	0.0076	-0.0118	0.0008	0.0064	-0.0081
	(0.0052)	(0.0058)	(0.0073)	(0.0110)	(0.0107)	(0.0109)	(0.0075)	(0.0062)	(0.0058)
Observations		37,332			37,332			37,332	

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.

recipients and the second groups is more similar in terms of labour market attachment. In Appendix Table B.3 we also report estimates for more similar groups of unemployed and reach similar conclusions.[27]

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.

This lack of spill-overs on workers not directly affected by the PBD variation induced by the regional discontinuity 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 in order to affect labour market tightness 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 of labour market tightness, is affected and whether we find evidence for the job creation effect. 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 do not contain the universe of vacancies.[28]

[27] Specifically, we first look at ineligibles with a strong labour market attachment, i.e. those who contributed a larger share of their working life to the UI system. Additionally, we compare outcomes for workers around 50 years who are either just affected (44-49 years) or just not affected (50-54 years) by PBD changes. By focusing the analysis of market externalities on more similar groups, we alleviate concerns that the primary reason for the absence of spill-overs is that indirectly affected workers operate largely in different labour markets.

[28] In Appendix Figure A.11 we compare the number of vacancies posted at the public employment offices to the number of vacancies obtained from the annually conducted Labour Demand Survey by Statistics Poland. The number of vacancies are highly correlated and support the notion that vacancies from public employment offices track overall vacancies quite accurately and are thus a meaningful measure for our analysis.

Table 6: Effect on vacancies, vacancy filling rate and wages

Month ∈	$[-6,-1]$ (1)	$[0,11]$ (2)	$[12,23]$ (3)	$[-6,-1]$ (4)	$[0,11]$ (5)	$[12,23]$ (6)
Panel A: Vacancies						
	Stock of vacancies			Newly posted vacancies		
	0.0151	0.0054	-0.0258	0.0019	0.0031	0.0088
	(0.0433)	(0.0398)	(0.0482)	(0.0249)	(0.0230)	(0.0294)
Observations		18,513			18,513	
Panel B: Vacancy filling rate						
	$\frac{\text{Outflows}}{\text{Stock of vacancies}}$			$\frac{\text{Outflows}}{\text{Newly posted vacancies}}$		
Coefficient	-0.0061	0.0093	0.0409	0.0000	0.0120	0.0050
	(0.0495)	(0.0382)	(0.0394)	(0.0263)	(0.0244)	(0.0299)
Observations		18,513			18,513	
Panel C: Wages						
	Of job postings				At larger companies	
Coefficient	-0.0067	-0.0015	0.0048		0.0001	0.0010
	(0.0032)	(0.0036)	(0.0036)		(0.0016)	(0.0018)
Observations		11,448			954	

Notes: All outcomes are in logs. 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 and are available from 2015-2019. For other notes see Table 3.

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.

In Table 6, Panel A, we first report the effects on the number of vacancies. Columns (1)-(3) contain the estimates for the stock of vacancies and columns (4)-(6) newly posted vacancies, where the latter could be a more timely measure of labour demand. We move on to report effects on the vacancy filling rate in Panel B, where, mirroring Panel A, we calculate the filling rate using either the stock or newly posted vacancies. We find no impact on the vacancy filling rate, i.e. no change in labour market tightness, in response to the PBD increase.

Unchanged tightness implies that either aggregate search effort and vacancies remain constant or that both change in the same direction. In line with a plethora of papers in this literature, we find that that the unemployment duration increases for the directly affected unemployed. While this seemingly indicates a *drop* in aggregate search effort by the unemployed, as a contrasting effect we find that a PBD increase leads to more separations (we discuss these findings in more detail below), which, ceteris paribus, *increases* the number of unemployed and hence aggregate search effort by the unemployed.[29] These forces could balance each other out, leaving aggregate search effort roughly unchanged. Even if aggregate search effort changes, tightness might nonetheless stay unchanged if vacancies change accordingly. This is the case when the aggregate labour demand curve with respect to labour market tightness is horizontal (Hall, 2005; Landais et al., 2018b).

The PBD change might impact the number of vacancies through various channels. While we do not find a significant effect on vacancies, the confidence intervals are not very tight, such that we cannot rule out relevant effects. In our context, vacancies might increase in response to a PBD increase simply because the number of separations increases significantly. As pointed out by Hall (2005), a sudden increase in separations increases both unemployment and vacancies. On the other hand, wage effects due to improved outside options of workers could lead to a decrease in vacancies.

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. (2019b) 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 (1)-(3) of Panel C, 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 (5)-(6) display precisely estimated null effects on average wages at companies with at least 10 employees.[30] Our findings are in line with Jäger et al. (2020) who analyse wage effects of UI reforms of the benefit *level* in Austria

[29] In contrast, the findings in Lalive et al. (2015) are consistent with an increase in tightness in response to an increase in the PBD from 52 to 209 weeks for older workers in Austria. Like us, they find a large increase in the separation rate of treated individuals. However, the programme they study is effectively an early retirement schedule. Hence, the effect on aggregate search effort is unambiguously negative.

[30] 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.

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.

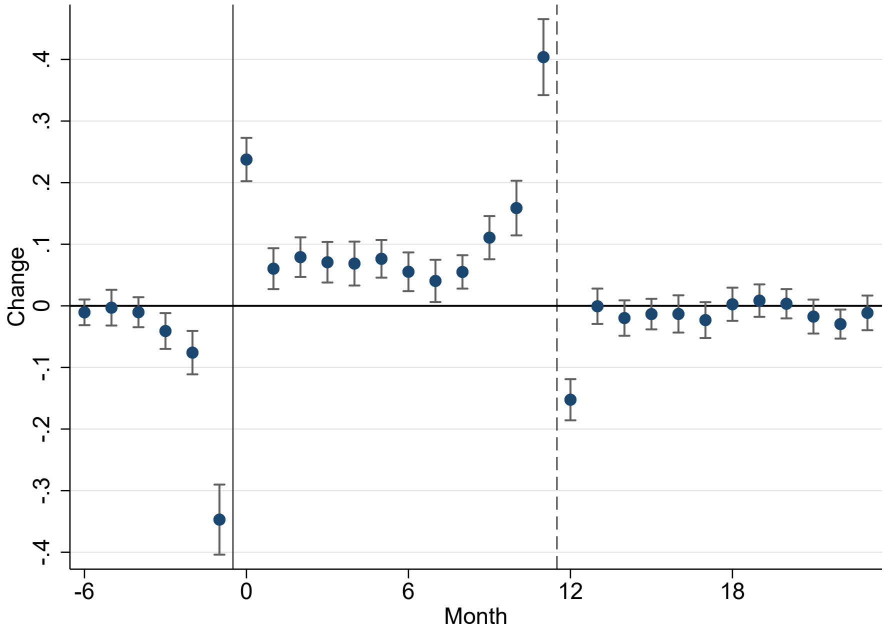
Inflows into Unemployment. A major advantage of having access to individual unemployment spells, from which we construct aggregate outcomes, is the ability to observe *flow* variables such as inflows into and outflows from unemployment. In many studies in this literature, the analysed outcomes are restricted to *stock* variables such as the unemployment rate or employment. We first analyse to what extent inflows into unemployment are affected by PBD changes. In section 6, we then decompose the effect on stocks into components driven by changes in the exit rate and changes in inflows, providing a comprehensive view 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 the 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 longer 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 depressed. In December, inflows are 0.347 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.[31] This shows that (i) workers can at least partially time their inflow into unemployment to receive higher benefits and (ii) a longer PBD increases overall inflows.

[31]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 inflows in this period is in fact less than half the size of

Figure 7: Effect on log inflows into unemployment of eligibles



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

Table 7: Effect on log inflows into unemployment of eligibles

Month $\in$	$[-6,-1]$ (1)	$[0,11]$ (2)	$[12,23]$ (3)
Coefficient	-0.0813	0.1181	-0.0222
	(0.0101)	(0.0095)	(0.0082)
Observations		37,332	

Notes: See Table 3 for notes.

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. A priori, the expectation is that socio-demographic groups with higher labour supply elasticities 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.4; the inflow effect in year 0 is 0.058 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 a longer PBD on unemployment durations for these groups in Poland (Gałecka-Burdziak et al., 2021).

the effect in the year after the PBD increase.

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 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.12 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, but there is no evidence for a substantial delay in registration in order to benefit from a longer PBD. 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.

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 the effect on the exit rate and a contribution based on the effect 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:

$$U_t = (1 - R_{t-1}) \times U_{t-1} + I_{t-1}, \tag{8}$$

where R_{t-1} denotes 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 are contained in Figures 6 and 7 above. We show the remaining ingredient, effects on the exit rate, in Appendix Figure A.13. To ensure that the decomposition adds up without residual in a finite sample,[32] 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. For each county, we normalise the level to the mean county population over the time period considered.[33] For the decomposition we calculate counterfactual stocks that are constructed using predictions from regressions in (normalised) levels based on equation (1).[34]

In equation (9) 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:

$$U_t^{I_{12},R_{12}} - U_t^{I_6,R_6} = \underbrace{U_t^{I_{12},R_{12}} - U_t^{I_{12},R_6}}_{\text{Exit rate}} + \underbrace{U_t^{I_{12},R_6} - U_t^{I_6,R_6}}_{\text{Inflows}} \tag{9}$$

The left hand-side of equation (9) 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 second element contains the effect of different inflows, holding the exit rate constant.[35]

The resulting decomposition of the change in the stock of eligibles, including workers who quit their previous jobs (around 15% of 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.

One would expect that the effect on the exit rate initially plays only a minor role, as newly unemployed individuals in counties both above and below the cut-off can still receive benefits during the first six months. Initially, the share of unemployed who receive longer benefits is small in 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 counties above the cut-off. After 12 months, changes in inflows and changes in the exit rate each account for about

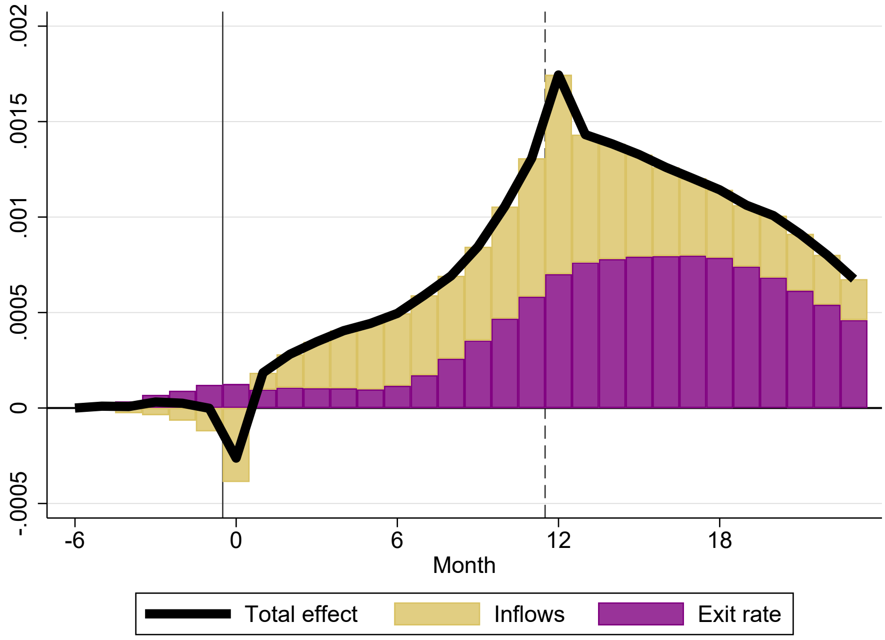
[32] This holds because stock, inflows and outflows are additively related, $U_t = U_{t-1} + I_{t-1} - O_{t-1}$, where O_{t-1} denotes outflows from unemployment.

[33] 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.

[34] 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.

[35] 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: Figure shows the effect of a longer 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.

half of the total increase in the stock of unemployed. 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.15 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.16 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 the unemployed eligible to receive benefits, we focused on 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 duration and benefit duration with respect to the PBD. In Appendix Figure A.17 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 between 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 around 85% of eligibles have been laid off, the impulse response functions in Appendix Figure A.17 are not notably different to the main estimates presented in section 5, although the patterns are slightly *more* pronounced.

As further robustness check, we add an additional control variable and include 12 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.18 are highly robust with, once more, no differences detectable by the naked eye.

In equation (1), both lags and leads are included in the estimation. The inclusion of leads allows us to assess pre-trends before the announcement of the revised unemployment rates in September and to tease out anticipation effects between then and the PBD change in January. However, the leads are not required for identification of the increases in stocks, durations, and inflows into unemployment following the PBD increase. We therefore also report the impulse responses based on regressions without leads in Appendix Figure A.19. In the six months before the PBD increase, the coefficients are mechanically zero, while the impulse responses following the PBD increase are in line with our preferred empirical strategy, which includes leads. An exception is the impulse response of inflows into unemployment. For this outcome, intertemporal substitution clearly plays a major role. The regression excluding leads still recovers the increase in inflows following the PBD raise. However, the intertemporal substitutions between the years visible in the impulse responses differ somewhat from the main results.

Our main results are obtained from a regression that includes 12 lags of the dependent variable. In Appendix Figure A.20, we show that our results are robust to this choice. We display the impulse responses obtained from estimations with various lag lengths of the dependent variable. For most outcomes, the various impulse responses lie within the 95-percent confidence band of the preferred specification with 12 lags. An exception is the impulse response of the log stock of all unemployed with zero lags of the dependent variable. While the impulse responses based on estimations with between two and 12 lags are all very similar, the impulse response with zero lags lies somewhat below the others. Nonetheless, it is qualitatively similar. Thus, at least the first two lags appear to be relevant control variables.

Appendix Table B.5 reports p-values of the test of the null-hypothesis of zero first-order auto-correlation in the error terms of specifications with various numbers of lags of the dependent variable for some of our main outcomes. As noted by Born and Breitung (2016), serial correlation in the residuals is not a problem *per se* in a setting like ours as we calculate standard errors that are robust to serially correlated errors. Nonetheless, serial autocorrelation may indicate that the model choice is not appropriate. We apply the bias-corrected Q(p) test proposed by Born and Breitung (2016). Wursten (2018) shows that this test performs well in Monte Carlo simulations. With two lags, we cannot reject the null hypothesis of no autocorrelation at conventional confidence levels for any of the main outcome variables.

8 Conclusion

In Poland, a county's PBD is 12 months if its 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 months 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 we cannot reject 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. Given that labour market tightness in Poland is unaffected by the PBD, variations in the PBD over the business cycle cannot be used to reduce fluctuations in tightness over the cycle as in 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 (Gudgeon et al., 2023; Hartung et al., 2024; Kuhn et al., 2021). The importance of job separations suggests that optimal UI is less generous than implied by the modified Baily-Chetty formula in Landais et al. (2018b), which accounts for the effects of UI on unemployment durations and labour market tightness.

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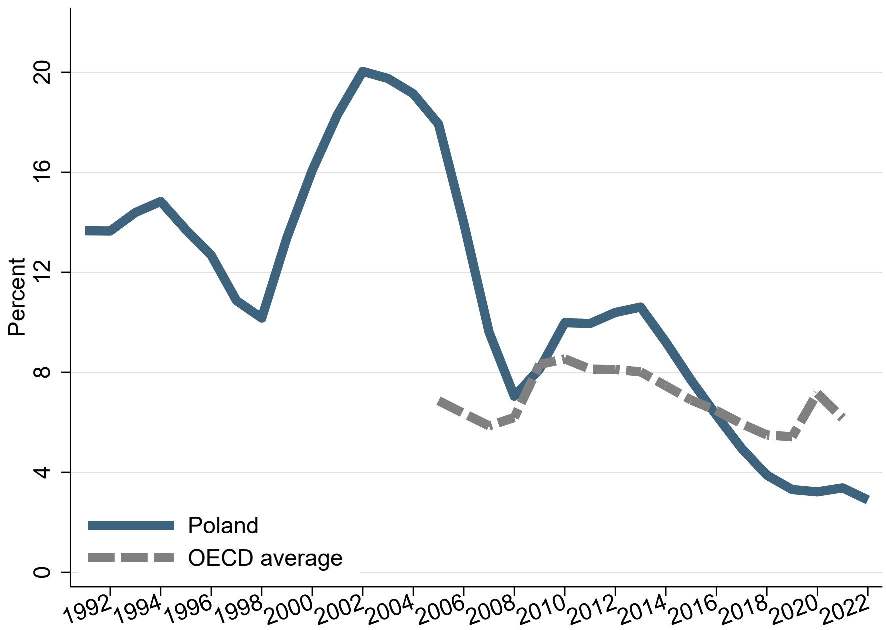
Appendix (for online publication)

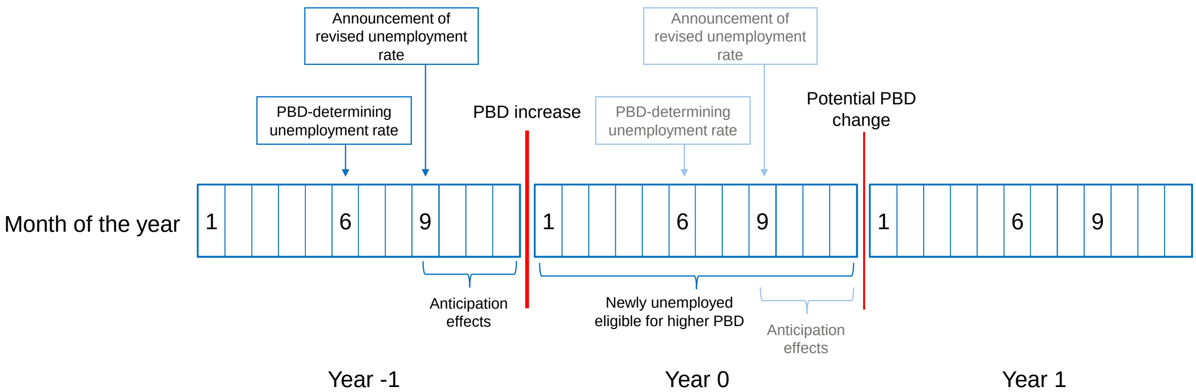
A Figures

Figure A.1: Unemployment rate over time



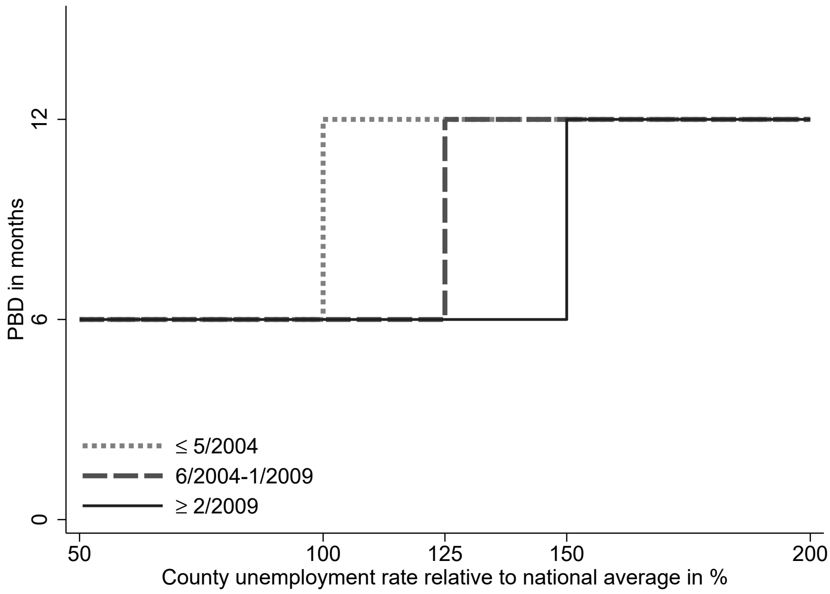
Notes: The figure shows how the unemployment rate of Poland and the OECD average over time. Sources: https://data.oecd.org/unemp/unemployment-rate.htm, accessed July 5, 2024, and Polish Labour Force Survey

Figure A.2: Timing of PBD changes



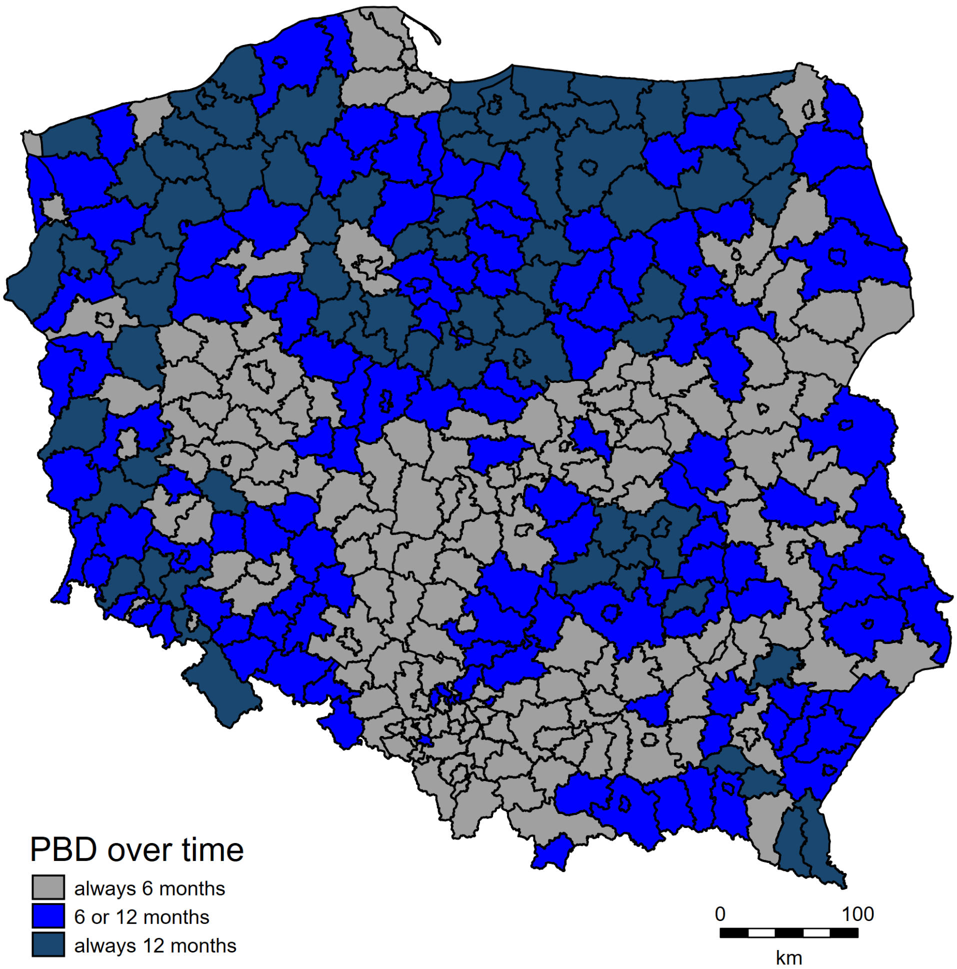
Notes: The figure delineates the timing of a PBD increase and the potential anticipation period. The PBD is increased in year 0 due to the unemployment rate in June (month 6) in the previous year 0 having passed the threshold. While the June unemployment rate determines next year's PBD, the revised and PBD-determining unemployment rates are announced in September. Although no PBD change has yet occurred, workers and firms could adjust their behaviour in anticipation of the upcoming changes. In year 0 all newly unemployed are eligible to the longer PBD. In June of year 0, the unemployment rate determines the PBD in year 1. As the PBD could potentially change again between year 0 and year 1, in September to December of year 0 anticipation effects could again play a role.

Figure A.3: Relative unemployment rate and PBD



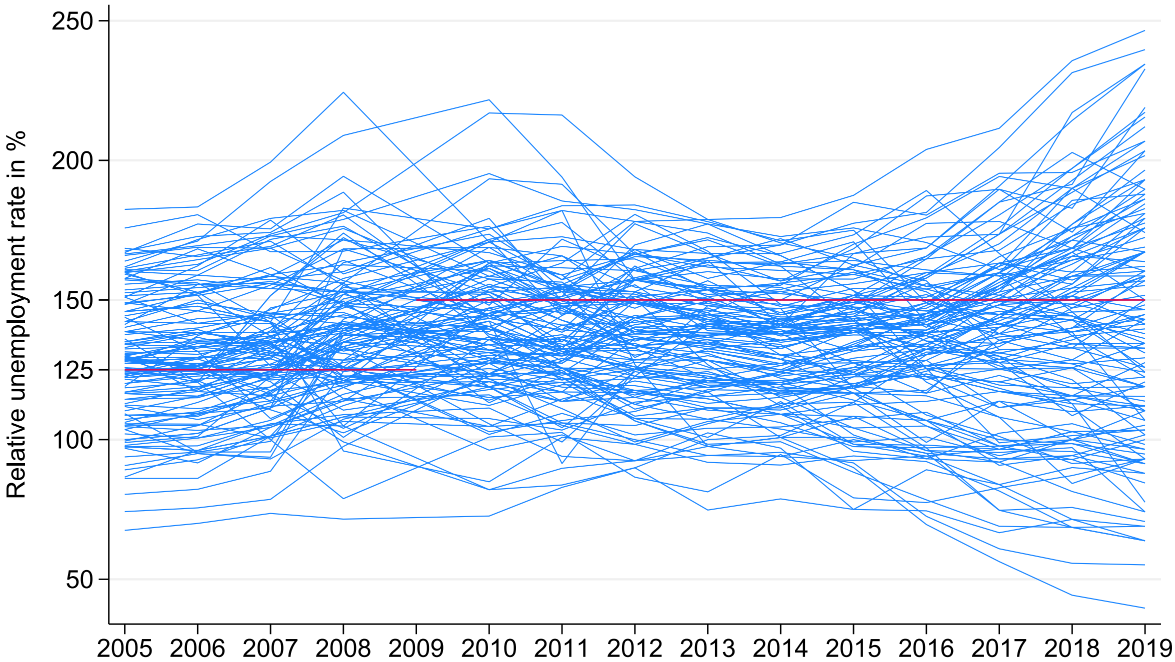
Notes: The figure shows how the PBD in a county depends on the relative unemployment rate of a county over time. Before June 2004, the threshold for 12 months PBD was 100%. Between June 2004 and January 2009, it was at 125%, since then the threshold is 150%. For 12 months PBD the county must have exceeded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

Figure A.4: County with different PBDs over time



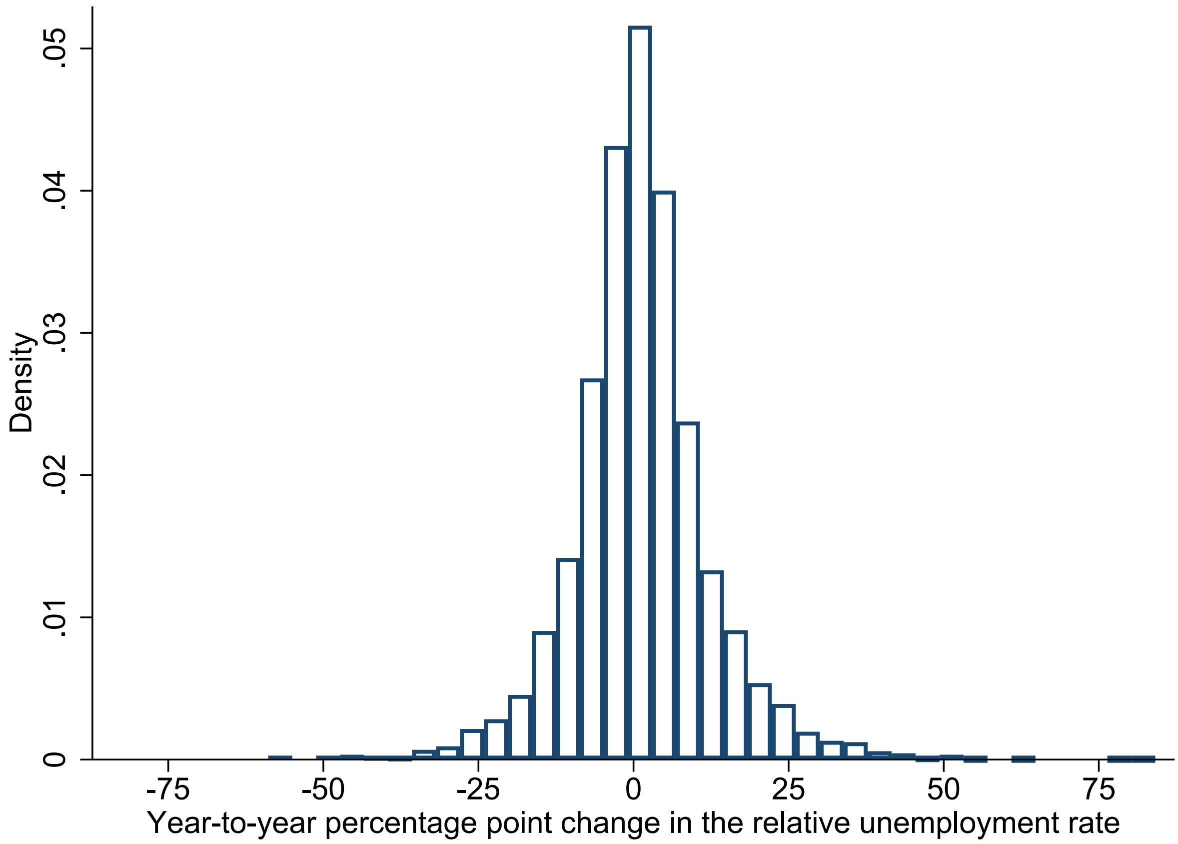
Notes: The map plots the counties which in our sample period have a constant PBD of 6 (light grey) or 12 months (dark grey), or different PBDs over time (light blue).

Figure A.5: Variation in the relative unemployment rate of counties with different PBDs



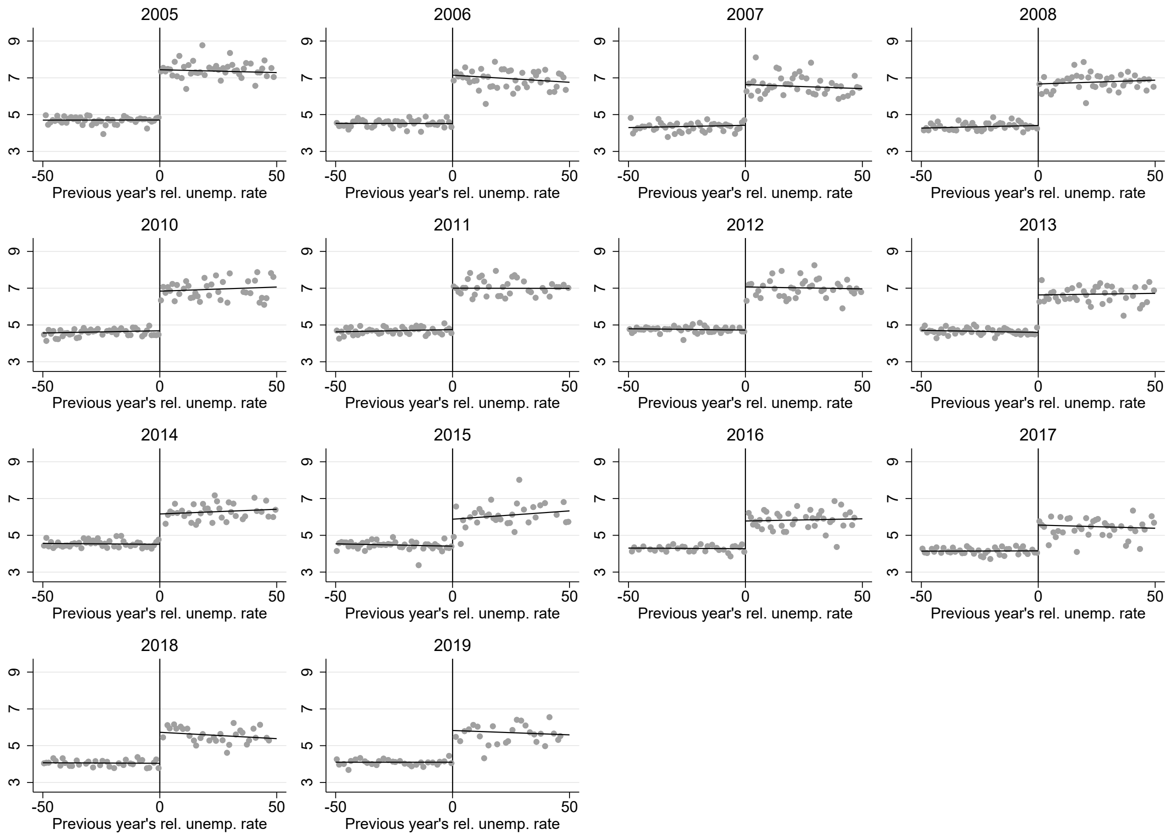
Notes: Each line is one of the 133 counties which has different PBDs over time. The red horizontal lines denote the thresholds, counties above have a PBD of 12 months in that calendar year and those below a PBD of 6 months.

Figure A.6: County-level changes in the relative unemployment rate



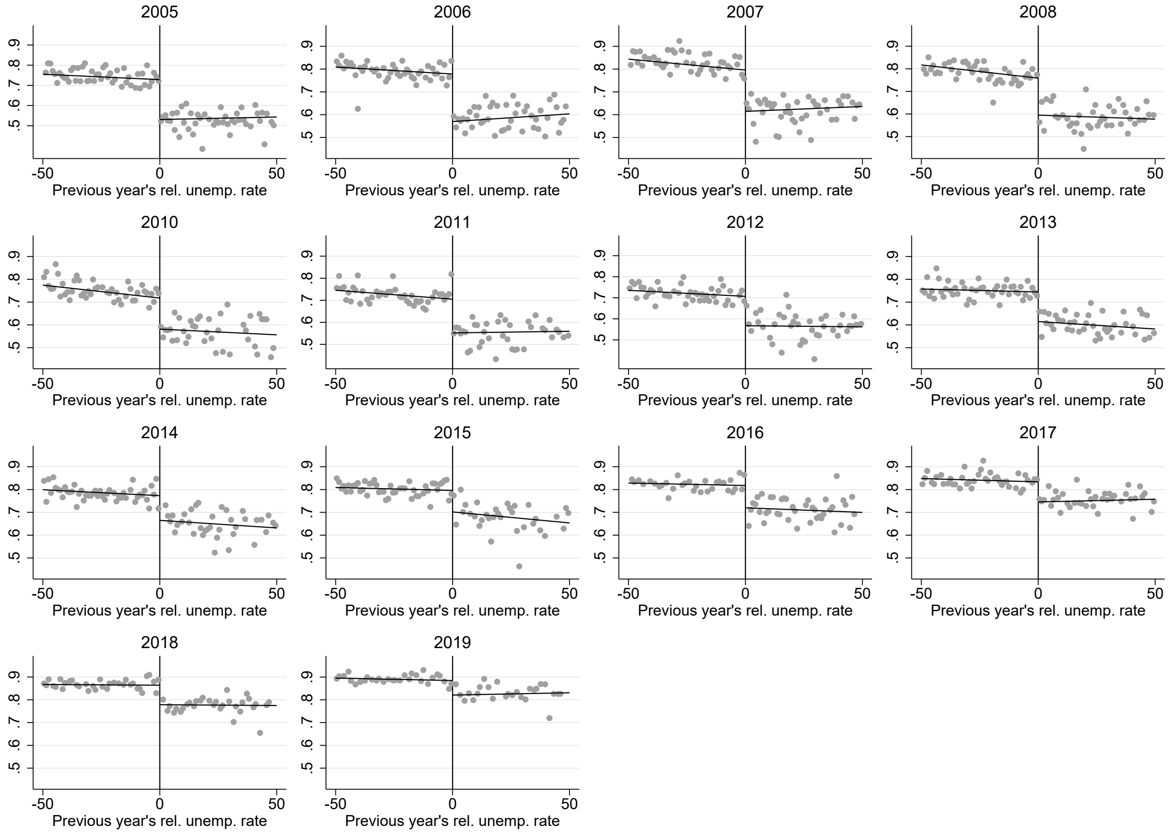
Notes: The histogram shows the densities of the year-to-year changes in counties' relative unemployment rates.

Figure A.7: Benefit duration of eligibles relative to the cut-off over time



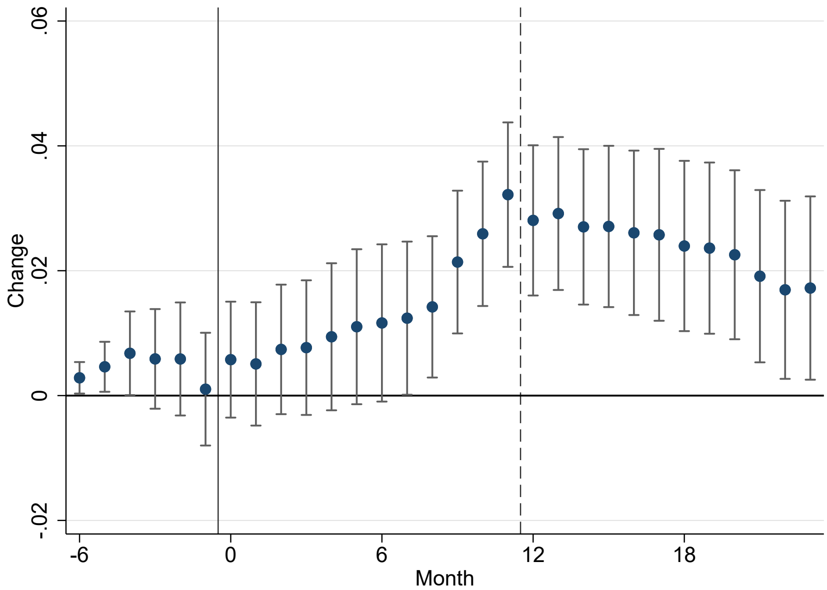
Notes: The figure shows the average benefit duration in months relative to the PBD determining cut-off, centred around zero, over time. Observations are at the county-month level and in bins of one percentage point.

Figure A.8: Share of eligibles with unemployment duration $\leq$ 12 months



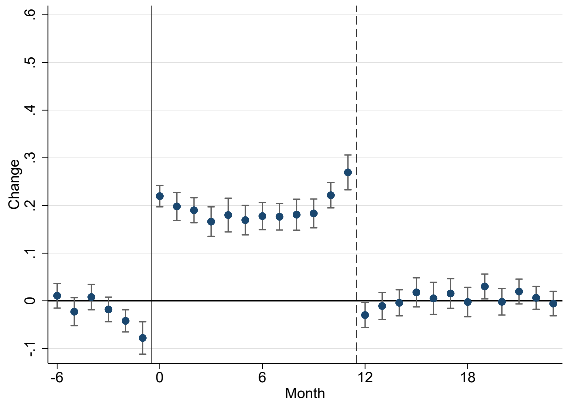
Notes: The figure shows the share of benefit recipients with an unemployment duration equal to or below 12 months relative to the PBD determining cut-off over time. Observations are at the county-month level and in bins of one percentage point.

Figure A.9: Effect on log unemployment rate

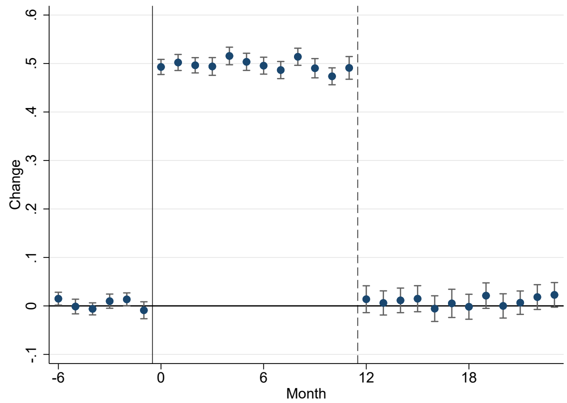


Notes: The outcome variable is the log of the official monthly unemployment rate calculated by Statistics Poland. See Figure 6 for other notes.

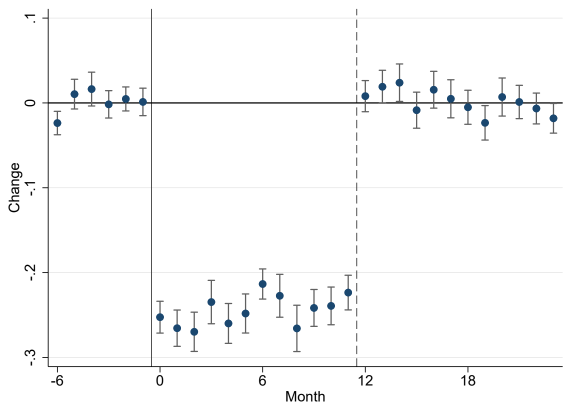
Figure A.10: Effect on log durations of eligibles



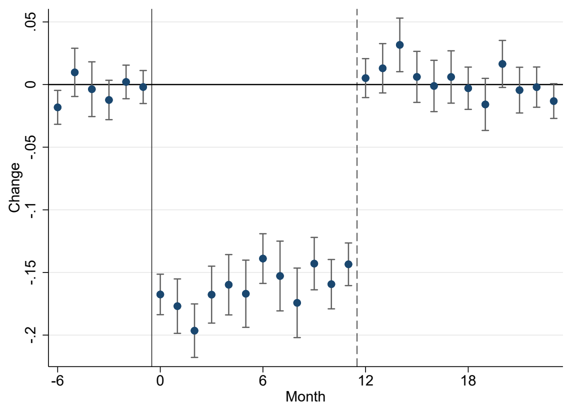
(a) Unemployment duration



(b) Benefit duration



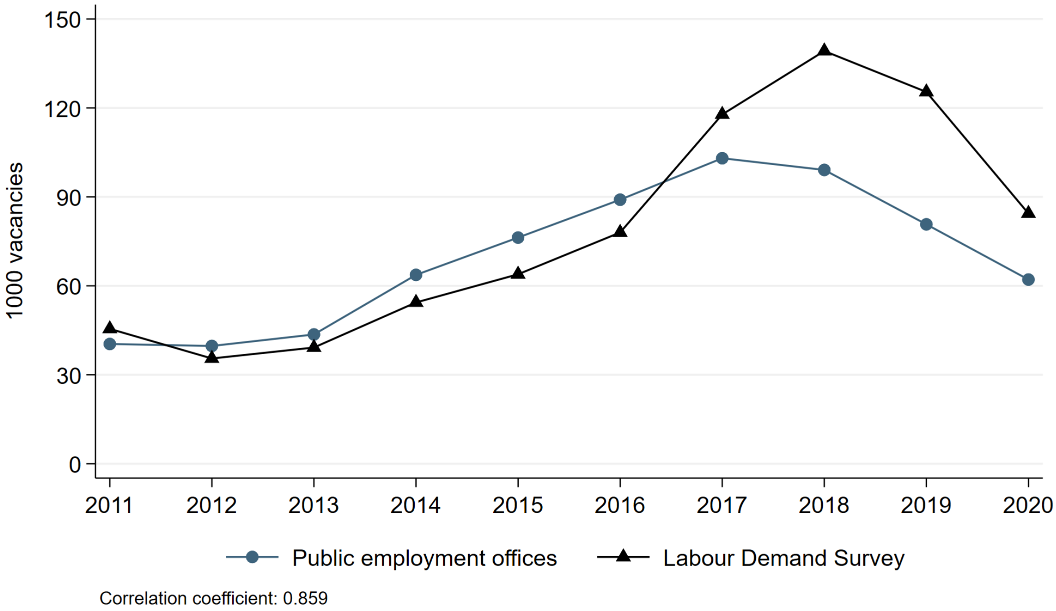
(c) Share unempl. duration $\leq$ 12 months



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

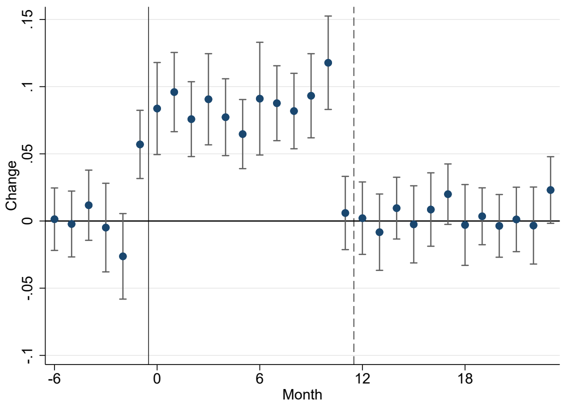
Notes: The figure shows the monthly estimates for log unemployment and log benefit duration for which annual summary estimates are contained in Table 4. See that table and Figure 6 for other notes.

Figure A.11: Number of vacancies from different sources

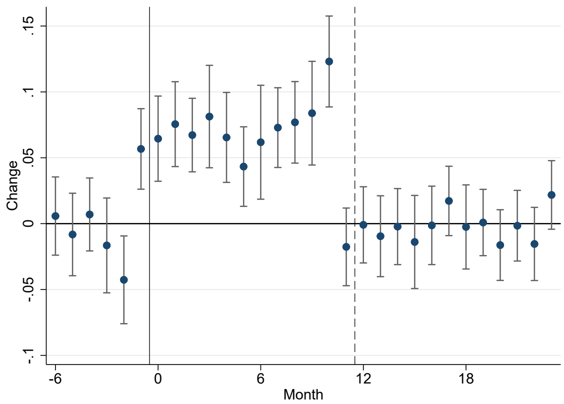


Notes: The figure compares the number of vacancies at the public employment office to the number of vacancies from the Labour Demand Survey. The survey is conducted annually and published by Statistics Poland under the following link: https://stat.gov.pl/en/topics/labour-market/demand-for-labor/the-demand-for-labour-in-2022,1,17.html (last accessed July 9, 2024).

Figure A.12: Log job endings of eligibles



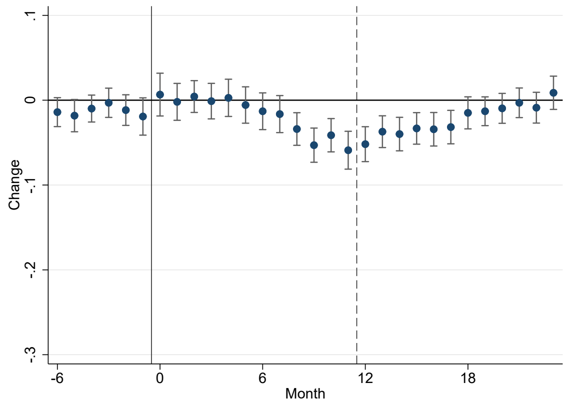
(a) Quitters and laid off



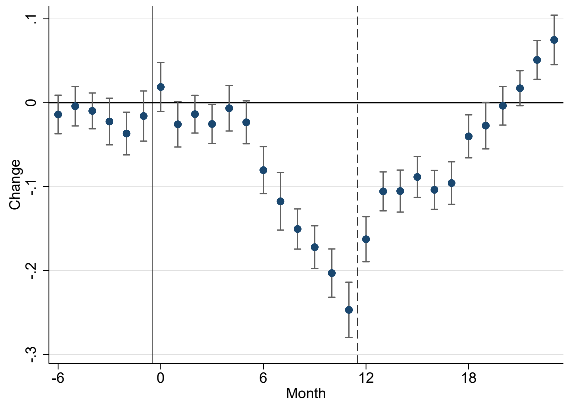
(b) Laid off

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

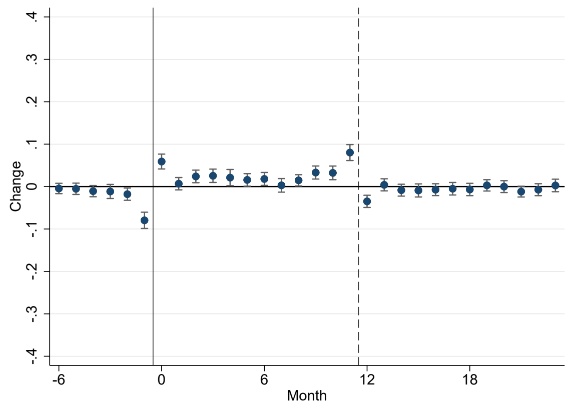
Figure A.13: Effect on log exit rates and on log inflows into unemployment for all unemployed



(a) Exit rate: All unemployed



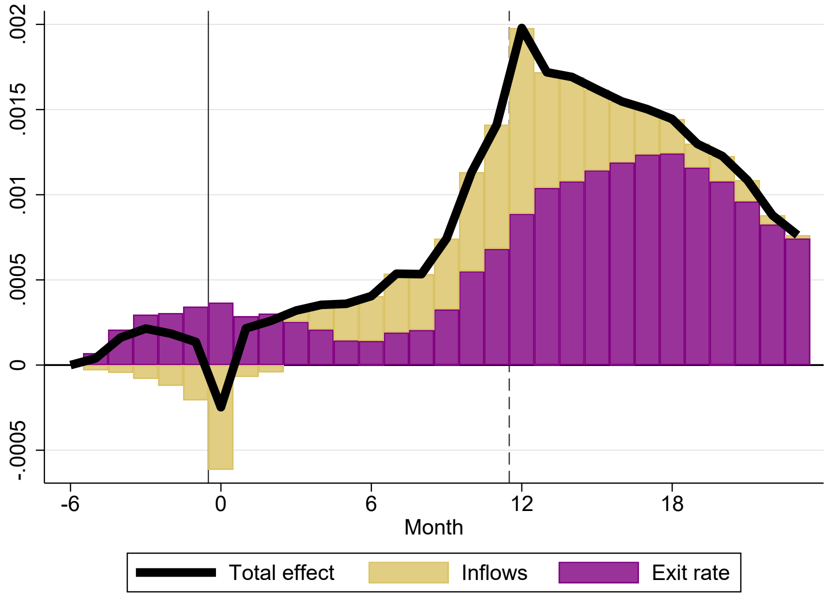
(b) Exit rate: Eligibles



(c) Inflows: All unemployed

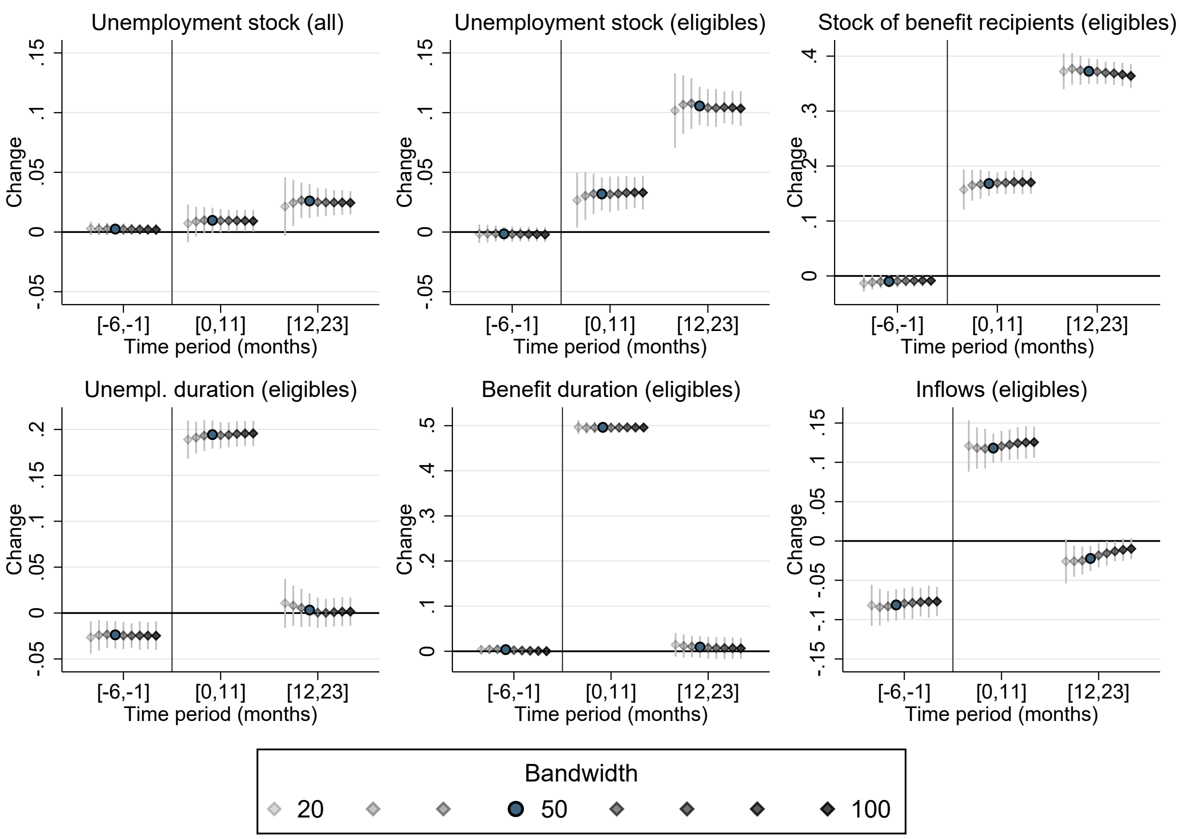
Notes: All outcomes are in logs. The figures show estimates for the exit rate, defined as the monthly outflows from unemployment divided by the stock, and inflows for all unemployed. These estimates (for outcomes normalised by the county population) are used for the decomposition in section 6.

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



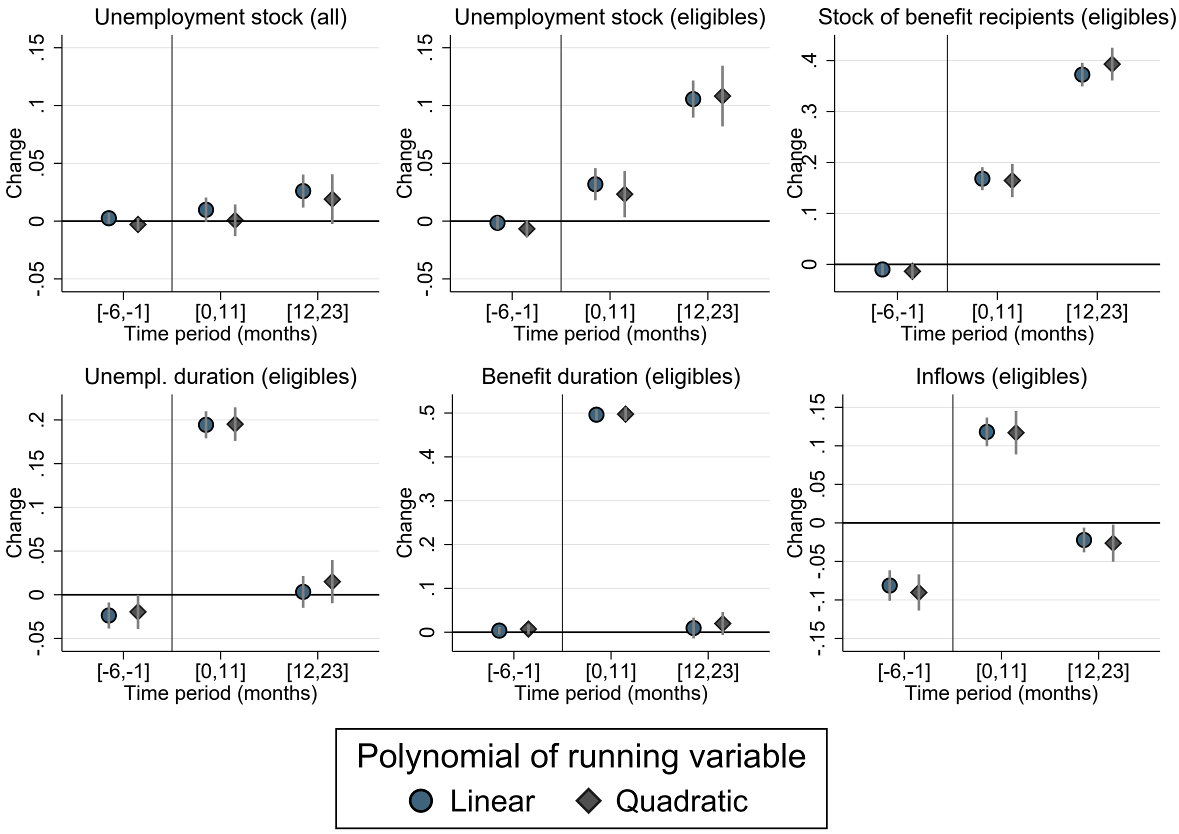
Notes: The figure shows a decomposition of the effect on the relative stock (normalised) by the county population of *all* unemployed into a share attributed to the effect on inflows and an effect on the exit rate.

Figure A.15: Robustness of coefficients to choice of bandwidths



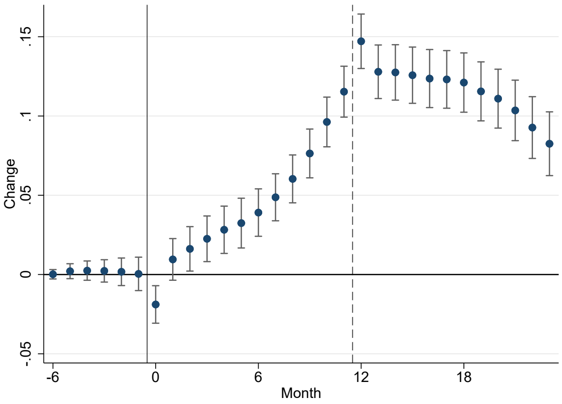
Notes: All outcomes are in logs. Coefficients are obtained from varying the bandwidth of the forcing variable (relative unemployment rate) in steps of 10 from 20 to 100. A bandwidth of 50—blue circles—is used in the main analysis. Grey spikes denote 95% confidence intervals.

Figure A.16: Robustness of coefficients to choice of polynomials

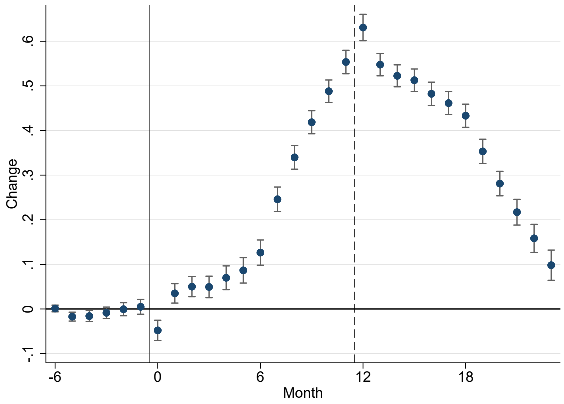


Notes: All outcomes are in logs. Coefficients are obtained from estimating equation (1) with either the function f_j either specified with a linear (blue circles, estimates in the main analysis) or a quadratic polynomial, in each case allowed to differ on either side of the cut-off. Grey spikes denote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

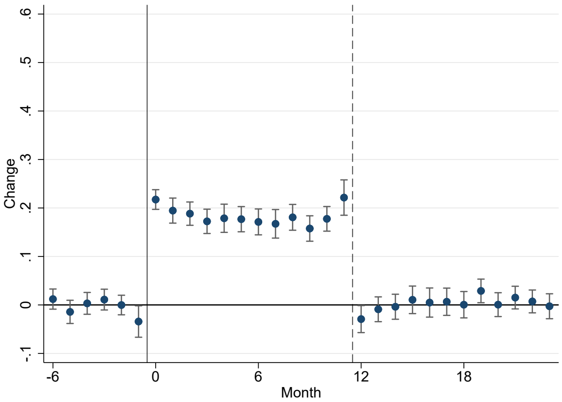
Figure A.17: Robustness—laid off and quitters



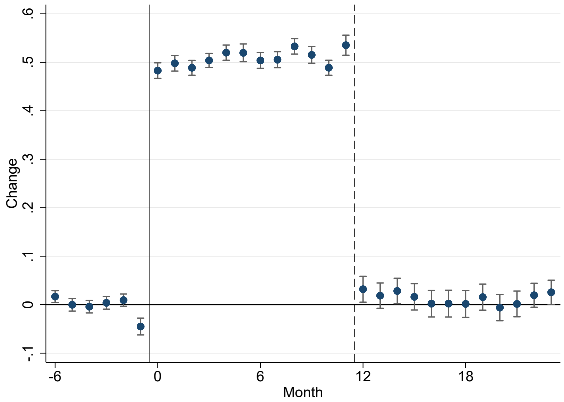
(a) Unemployment stock



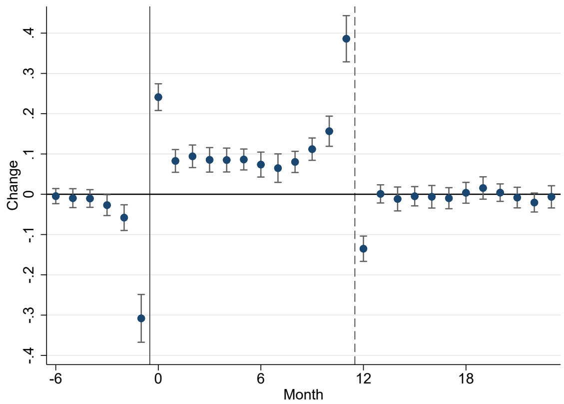
(b) Stock of benefit recipients



(c) Unemployment duration



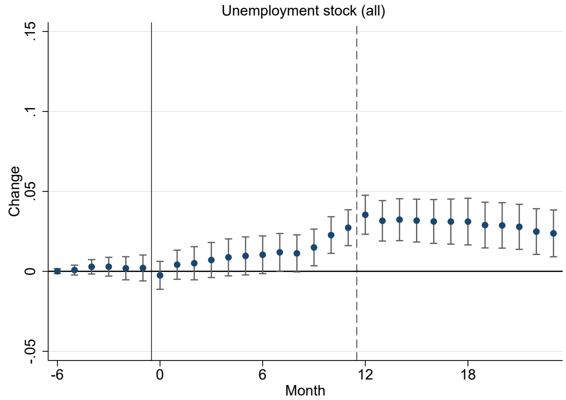
(d) Benefit duration



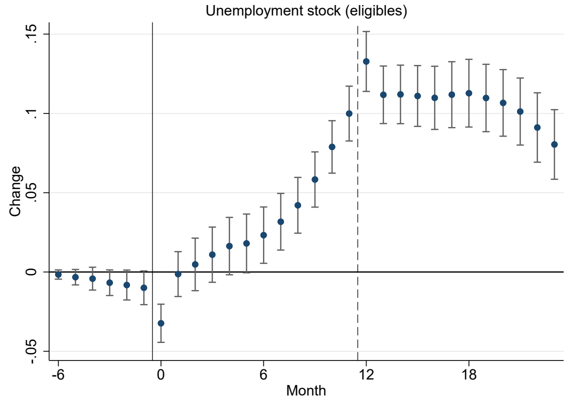
(e) Inflows

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). The estimates in this figure refer to all directly affected, i.e. eligibles under 50 who were laid off or have quit themselves.

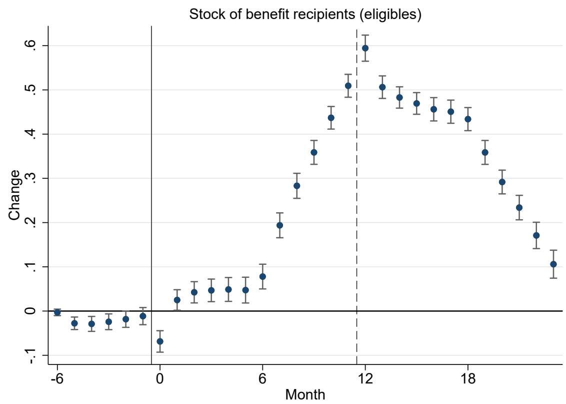
Figure A.18: Robustness of impulse responses to controlling for the unemployment rate



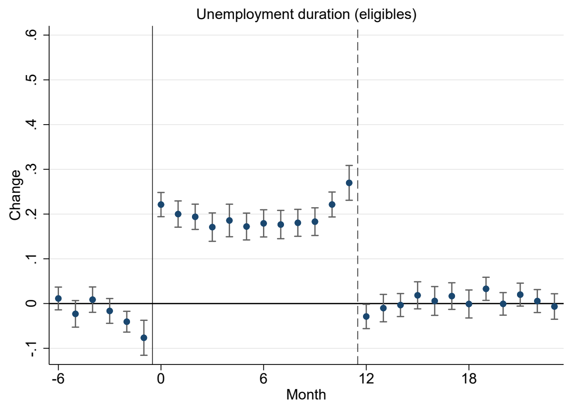
(a) Unemployment stock (all)



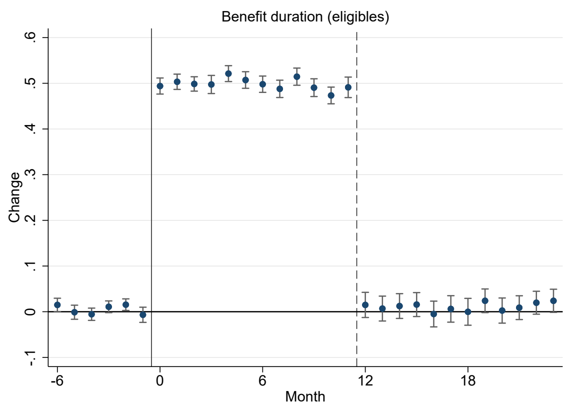
(b) Unemployment stock (eligibles)



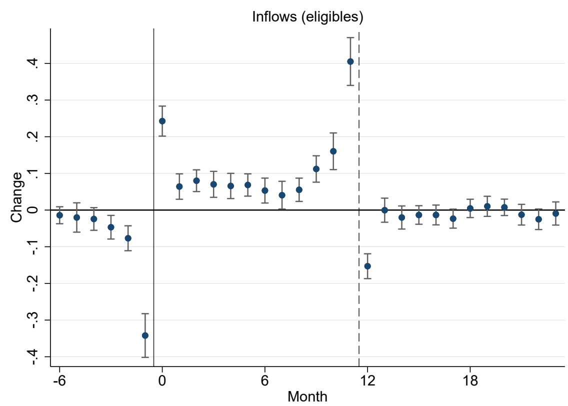
(c) Stock of benefit recipients (eligibles)



(d) Unemployment duration (eligibles)



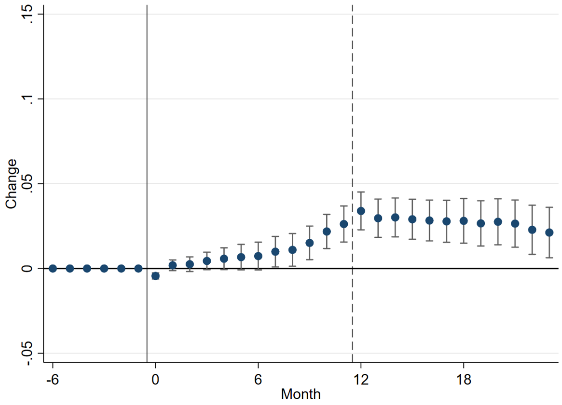
(e) Benefit duration (eligibles)



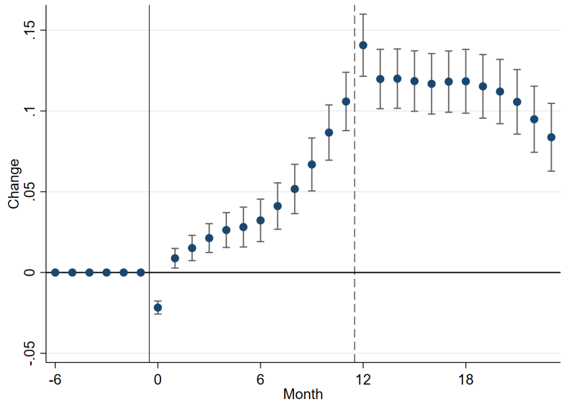
(f) Inflows (eligibles)

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.

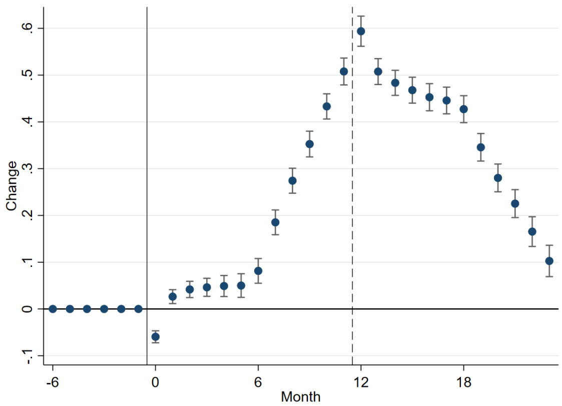
Figure A.19: Robustness—excluding leads



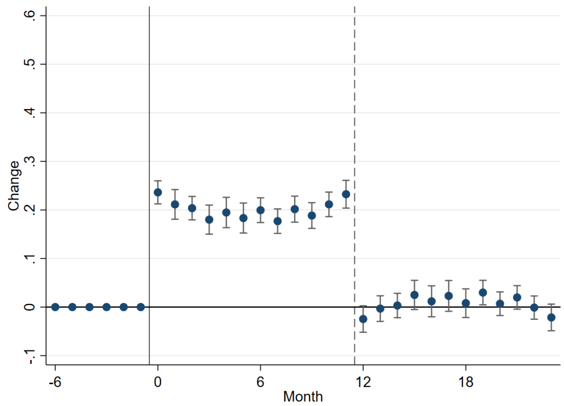
(a) Unemployment stock (all)



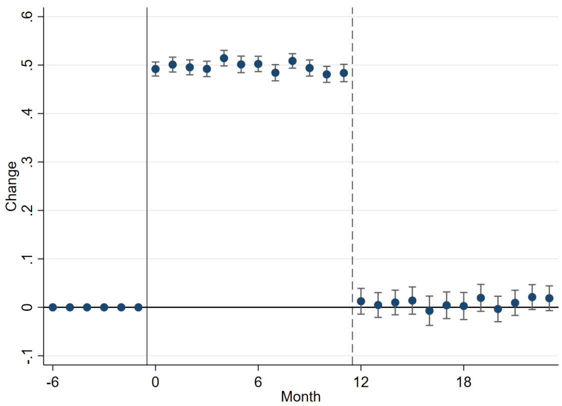
(b) Unemployment stock (eligibles)



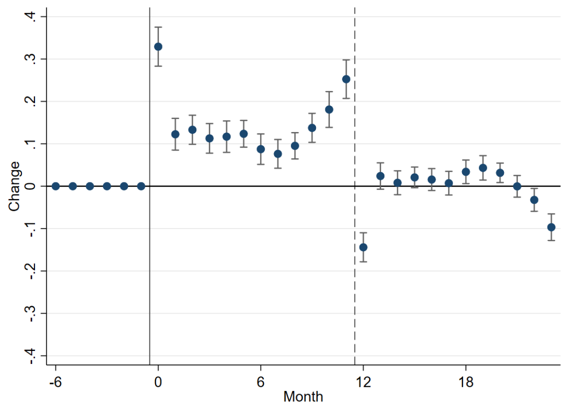
(c) Stock of benefit recipients (eligibles)



(d) Unemployment duration (eligibles)



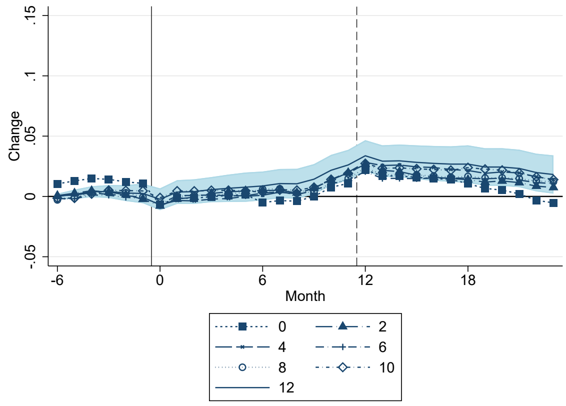
(e) Benefit duration (eligibles)



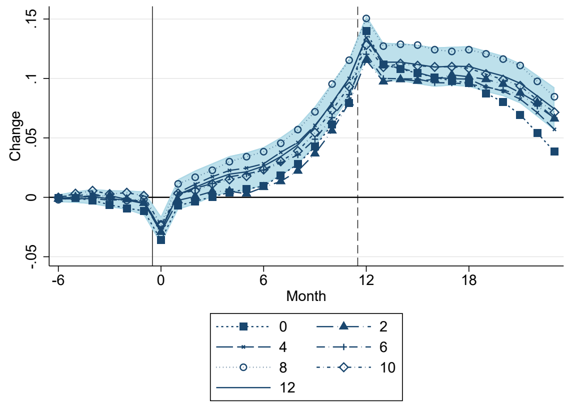
(f) Inflows (eligibles)

Notes: All outcomes are in logs. In contrast to the main estimates based on equation (1), the leads $(\sum_{j=1}^{6} f_{2,j}(r_{i,t+j}, PBD12_{i,t+j}) + \sum_{j=1}^{6} \delta_{2,j} PBD12_{i,t+j})$ are dropped from the estimation leading the point estimates in the months before the PBD increase to mechanically be zero.

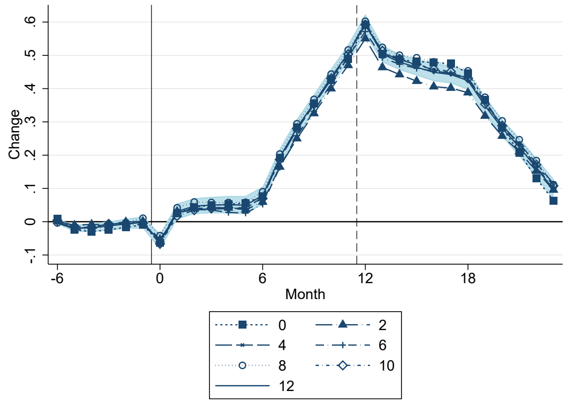
Figure A.20: Robustness—different numbers of lags of dependent variable



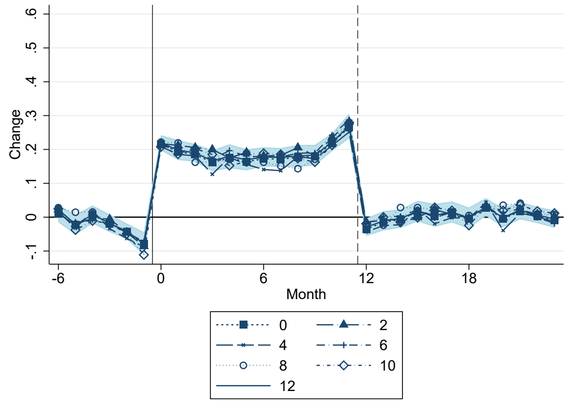
(a) Unemployment stock (all)



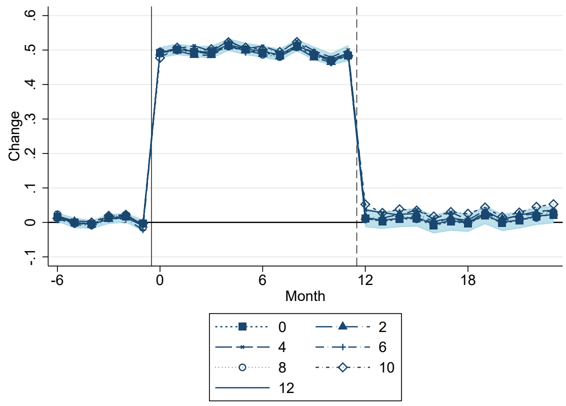
(b) Unemployment stock (eligibles)



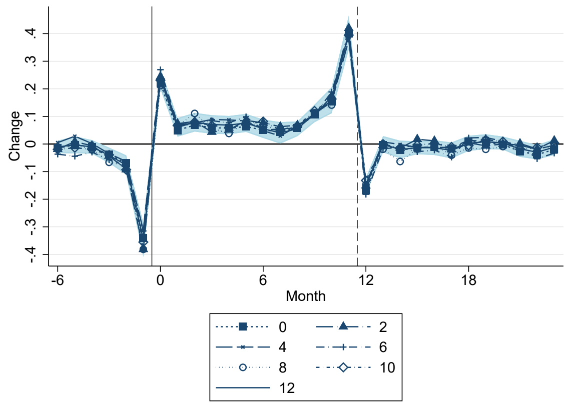
(c) Stock of benefit recipients (eligibles)



(d) Unemployment duration (eligibles)



(e) Benefit duration (eligibles)



(f) Inflows (eligibles)

Notes: All outcomes are in logs. Impulse response functions based on estimation of equation (1), but with different numbers of lags of the dependent variable. The areas shows the 95-percent confidence interval for our preferred specification with 12 lags.

B Tables

Table B.1: Summary statistics - individual level

Variable	Obs	Mean	Std. Dev.
All unemployed			
Age in years	30,608,367	33.579	11.998
Female	30,608,367	.466	.499
Contributory spells in years	30,608,367	6.647	8.944
Unemployment duration in months	30,608,367	9.049	13.078
Eligibles			
Age in years	4,807,510	33.006	8.185
Female	4,807,510	.525	.499
Contributory spells in years	4,807,510	8.567	7.421
Unemployment duration in months	4,807,510	10.204	12.526
Benefit receipt in months	4,807,510	5.489	3.463

Notes: The table shows summary statistics from calculated from individual unemployment spells from January 2005 to December 2019.

Table B.2: RD estimates - individual level

	Exit from			Entry into	
	benefits	unemployment		employment	
Sample:	Eligibles	All	Eligibles	All	Eligibles
	(1)	(2)	(3)	(4)	(5)
$\leq$ 3 months	-0.0218	-0.0037	-0.0212	-0.0079	-0.0357
	(0.0074)	(0.0090)	(0.0074)	(0.0085)	(0.0080)
$\leq$ 6 months	-0.5805	-0.0091	-0.0448	-0.0147	-0.0619
	(0.0069)	(0.0091)	(0.0092)	(0.0088)	(0.0095)
$\leq$ 9 months	-0.4820	-0.0311	-0.1511	-0.0353	-0.1363
	(0.0073)	(0.0085)	(0.0091)	(0.0082)	(0.0089)
$\leq$ 12 months	-0.0064	-0.0334	-0.1670	-0.0313	-0.1239
	(0.0007)	(0.0075)	(0.0088)	(0.0072)	(0.0082)
$\leq$ 15 months	-0.0040	-0.0156	-0.0827	-0.0123	-0.0540
	(0.0004)	(0.0064)	(0.0072)	(0.0059)	(0.0061)
$\leq$ 18 months	-0.0000	-0.0080	-0.0460	-0.0061	-0.0312
	(0.0000)	(0.0056)	(0.0061)	(0.0050)	(0.0049)
Observations	3,107,701	19,332,651	3,107,701	9,284,291	2,074,829

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. Results for entry into employment are based on the sample of individuals who enter employment immediately after the unemployment spell.

Table B.3: Market externalities—more similar groups of indirectly affected

Group of unemployed:	Ineligibles with strong labour market attachment			Eligibles, $44-49$ years			Benefit recipients, $50-54$ years, ≥ 20 contributory years		
Outcome	Log unemployment duration								
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	0.0169	0.0058	-0.0136	-0.0562	0.2199	0.0070	-0.0139	-0.0051	-0.0164
	(0.0138)	(0.0135)	(0.0124)	(0.0186)	(0.0160)	(0.0147)	(0.0188)	(0.0145)	(0.0143)
	0.0168	0.0058	-0.0140	-0.0562	0.2198	0.0069	-0.0139	-0.0052	-0.0163
	0.0131	0.0129	0.0128	0.0186	0.0153	0.0152	0.0170	0.0153	0.0134
Observations		37,332			37,332			37,332	

Notes: 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. Compared to the sample shown in the columns in Table 5, the sample is restricted to individuals with a strong labour market attachment. Assignment is based on the share of years since the age 20 that workers have contributed to the UI system. Those with above median values of *eligibles* are defined as having a strong labour market attachment. Columns (4)-(9) compare the effect on durations of workers in a similar age range (44-49 vs. 50-54), where the group in columns (4)-(6) are affected by the PBD change and the group in columns (7)-(9) is not as their PBD is 12 months regardless of their county's relative unemployment rate. See Table 3 for other notes.

Table B.4: Heterogeneous effects on inflows of eligibles

Month ∈	$[-6,-1]$ (1)	$[0,11]$ (2)	$[12,23]$ (3)	$[-6,-1]$ (4)	$[0,11]$ (5)	$[12,23]$ (6)
Gender of inflows:		Women			Men	
Coefficient	-0.1106	0.1512	-0.0132	-0.0637	0.0926	-0.0279
	(0.0146)	(0.0131)	(0.0097)	(0.0121)	(0.0116)	(0.0105)
Observations		37,332			37,332	
Age group of inflows:		Below median			Above median	
Coefficient	-0.0758	0.0953	-0.0132	-0.0835	0.1414	-0.0296
	(0.0111)	(0.0143)	(0.0087)	(0.0132)	(0.0113)	(0.0091)
Observations		37,332			37,332	

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

Table B.5: P-values of Q(p) test for serial correlation

Lags	Unemp. stock (all)	Unemp. stock (eligibles)	Stock of benefit recipients (eligibles)	Unemp. duration (eligibles)	Ben. duration (eligibles)	Inflows (eligibles)
0	0.000	0.000	0.000	0.000	0.000	0.000
1	0.000	0.000	0.000	0.428	0.118	0.478
2	0.908	0.535	0.365	0.874	0.713	0.261
3	0.722	0.575	0.905	0.867	0.693	0.095
4	0.949	0.543	0.476	0.759	0.597	0.087
5	0.723	0.802	0.656	0.755	0.661	0.114
6	0.825	0.885	0.454	0.769	0.677	0.090
7	0.700	0.893	0.679	0.750	0.706	0.084
8	0.726	0.620	0.970	0.718	0.708	0.094
9	0.878	0.498	0.965	0.660	0.776	0.134
10	0.610	0.520	0.968	0.711	0.870	0.262
11	0.991	0.816	0.855	0.826	0.924	0.866
12	0.000	0.247	0.672	0.803	0.946	0.258

Notes: The table shows p-values of the the bias-corrected Q(p) test proposed by Born and Breitung (2016) of the null-hypothesis of zero first-order auto-correlation in the error terms of specifications with various numbers of lags of the dependent variable. In our preferred specification, we include 12 lags of the outcome variable. As we use monthly data, this is a natural choice in order to match the seasonality. The test was implemented using the Stata package **xtqptest** by Jesse Wursten, see Wursten (2018).