Life Course Centre

WORKING PAPER SERIES

No. 2025-02

February 2025

Moral hazard among the employed

Evidence from regression discontinuity

Jonas Jessen

Robin Jessen

Andrew C. Johnston

Ewa Gałecka-Burdziak

The Australian Research Council Centre of Excellence for Children and Families over the Life Course
Phone +61 7 3346 7477 **Email** lcc@uq.edu.au
lifecoursecentre.org.au

Research Summary

Why was the research done?

We exploit policy discontinuities in Poland's unemployment insurance (UI) to examine the causal effect of changes to both benefit durations (how long a worker can receive benefits) and levels (how much a worker receives per month). We examine whether UI-induced moral hazard extends beyond job seekers to influence the behavior of employed workers as well.

What were the key findings?

Using a regression discontinuity approach, we uncover three findings: (1) Higher benefit levels distort employment more than benefit extensions. (2) Benefit durations and levels interact: Longer durations substantially increase the distortionary effect of more generous payments. (3) Higher payments increase the transition of employed workers into unemployment.

What does this mean for policy and practice?

We incorporate our findings into an extended Baily-Chetty model of optimal benefits, considering the social welfare implications of UI in the presence of endogenous inflows into unemployment (where the baseline Baily-Chetty model assumes that layoffs are exogenous) and the moral hazard interactions of benefit generosity and benefit duration. We conclude that including the effects of moral hazard among the employed significantly increases the understood fiscal costs of UI, in particular of increases in the benefit level.

Citation

Jessen, J., Jessen, R., Johnston, A.C., & Gałecka-Burdziak, E. (2025). 'Moral Hazard among the employed: Evidence from regression discontinuity', Life Course Centre Working Paper Series, 2025-02. Institute for Social Science Research, The University of Queensland.

The authors

Jonas Jessen
Berlin Social Science Center (WZB) and Institute for Employment Research (IAB)
Email: jonas.jessen@wzb.eu
https://wzb.eu/en/persons/jonas-jessen

Robin Jessen
RWI
Email: Robin.Jessen@rwi-essen.de
https://www.rwi-essen.de/en/rwi/team/person/robin-jessen

Andrew C. Johnston
Department of Economics, University of California and NBER
Email: acjohnston@ucmerced.edu
https://sites.google.com/site/andrewjohnstoneconomics/
Twitter: @acjohnston0

Ewa Gałecka-Burdziak
SGH Warsaw School of Economics
Email: eburdz@sgh.waw.pl

Acknowledgements/Funding Sources

We are grateful to Almut Balleer, Nathan Hendren, Xavier Jaravel, Justyna Klejdysz, Thomas Le Barbanchon, Lester R. Lusher, Steeve Marchand, Ella Mattinen, Christian Merkl, Bruce Meyer, Geoff Schnorr, David Seim, Gesine Stephan, Andrea Weber, Izabela Wnuk-Soares, Simon Trenkle and seminar participants at the Bank of Lithuania, University of Erlangen–Nuremberg, Rockwool Foundation Berlin, IZA, DIW, IfW, RWI, Warsaw School of Economics, IAAE 2023, IIPF 2024, EEA 2024, EALE 2024, LEW 2024 and VfS 2024 for helpful comments. We are grateful to the Ministry of Family, Labour and Social Policy of the Republic of Poland for providing access to the data. Access to the data can be from the ministry. 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 cofinanced by the Polish National Agency for Academic Exchange.

Moral Hazard among the Employed: Evidence from Regression Discontinuity*

Jonas Jessen†

Robin Jessen‡

Andrew C. Johnston§

Ewa Gałecka-Burdziak¶

February 4, 2025

Abstract

We exploit policy discontinuities in Poland's unemployment insurance to examine the causal effect of changes to both benefit *durations* and *levels*. Using a regression discontinuity approach, we uncover three findings: (1) Higher benefit levels distort employment more than benefit extensions. (2) Benefit durations and levels *interact*: Longer durations substantially increase the distortionary effect of more generous payments. (3) Higher payments increase the transition of employed workers into unemployment. We develop a model of optimal unemployment insurance that accounts for moral hazard among both employed and unemployed workers. Notably, for level increases, distortionary costs are larger among the employed than unemployed.

JEL: *H55, J20, J65*

Keywords: *Unemployment insurance, spell duration, regression discontinuity, endogenous separations*

*We are grateful to Almut Balleer, Nathan Hendren, Xavier Jaravel, Justyna Klejdysz, Thomas Le Barbanchon, Lester R. Lusher, Steeve Marchand, Ella Mattinen, Christian Merkl, Bruce Meyer, Geoff Schnorr, David Seim, Gesine Stephan, Simon Trenkle, Andrea Weber, Izabela Wnuk-Soares, and seminar participants at the Bank of Lithuania, University of Erlangen–Nuremberg, Rockwool Foundation Berlin, IZA, DIW, IfW, RWI, Warsaw School of Economics, IAAE 2023, IIPF 2024, EEA 2024, EALE 2024, LEW 2024 and VfS 2024 for helpful comments. We are grateful to the Ministry of Family, Labour and Social Policy of the Republic of Poland for providing access to the data. Access to the data can be from the ministry. 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.

†WZB; IAB; IZA and Berlin School of Economics: jonas.jessen@wzb.eu

‡RWI and IZA: robin.jessen@rwi-essen.de

§University of California, Merced; NBER; IZA; and J-PAL: acjohnston@ucmerced.edu

¶SGH Warsaw School of Economics and Life Course Centre, Australia: eburdz@sgh.waw.pl

1 Introduction

The optimal design of unemployment insurance (UI) depends on properly measuring the distortionary costs of moral hazard. While an excellent literature examines moral hazard among the unemployed (Bell et al., 2024; Card et al., 2015a; Cohen and Ganong, 2024; Dahl and Knepper, 2022; Karahan et al., 2022; Landais, 2015a), we demonstrate that these analyses capture only part of the distortionary effect of UI. We examine whether UI-induced moral hazard extends beyond job seekers to influence the behavior of employed workers as well.

This expanded moral hazard among the employed may manifest in reduced work effort, lower productivity, and increased transitions into unemployment (Ahammer et al., 2024; Ejrnæs and Hochguertel, 2013; Lusher et al., 2022). Consequently, the total distortionary cost of UI on employment, output, and welfare may substantially exceed estimates that focus on the behavior of the unemployed alone.

To investigate moral hazard's scope, we exploit discontinuities in the two core policy variables of unemployment insurance—potential benefit *duration* (how long a worker can receive benefits) and benefit *level* (how much a worker receives per month). These discontinuities allow us to measure UI effects on both employed and unemployed workers.

Our analysis leverages two intersecting discontinuities in Poland's UI system. First, claimants can receive 12 months of benefits instead of 6 if local unemployment exceeds a threshold, creating variation in potential benefit *duration*. Second, claimants receive a 25 percent increase in monthly benefits if they meet a threshold in years of covered employment, generating variation in benefit *levels*.[1] These policy features create quasi-experimental variation in UI generosity, enabling us to measure causal effects on labor market outcomes for both employed and unemployed workers.

The Polish setting offers several key advantages for empirical insight, in addition to quasi-random assignment. First, the setting provides discontinuities in both benefit level and potential benefit duration, allowing us to directly compare the effect of these two central policy choices.[2] Even when benefit increases and duration extensions have equivalent present discounted values, theory predicts different behavioral responses which have not been tested.[3] Second, these discontinuities *intersect* and are orthogonal to each other, enabling us to test for interaction effects between benefit level and potential benefit duration. Third, the policy variation is salient to

[1]The benefit level increase occurs when a worker has reached five years of covered employment. Because time is exogenous (and can neither be sped nor slowed), one's placement around the cutoff on any given day cannot be manipulated.

[2]This contrasts with previous estimates, which often arise piecemeal from different places, times, and labor markets, making direct comparisons difficult. An exception is Lalive et al. (2006) who find that in Austria the deadweight loss due to behavioral reactions is larger for benefit extensions than for increases in the replacement rate.

[3]There are two key reasons. First, while both policies increase total potential benefits, collecting the full value from extended durations requires remaining unemployed for longer. Thus, duration extensions might reduce search incentives relative to equivalent benefit increases. Second, benefit levels may be more salient since they represent an immediate and certain change rather than an uncertain one in the future. This heightened salience could generate stronger initial behavioral responses, particularly in separation decisions, compared to duration extensions that only materialize in future states for those still unemployed.

both employed and unemployed workers, allowing us to estimate moral hazard effects for both groups.[4] These features combine to provide a new vantage on the moral hazard effect of UI.[5]

Leveraging these discontinuities and high-quality administrative data that contain the universe of unemployment spells, we estimate the effects of UI benefit level and potential duration among the unemployed. Our findings reveal that a 10 percent increase in either policy leads to a 3 percent increase in unemployment duration. While both policy dimensions yield similar increases in unemployment duration, their fiscal implications differ significantly. Since an increase in the potential benefit duration has a much stronger impact on average benefit durations than an increase in the benefit level, extending the benefit duration proves more costly to taxpayers than increasing the benefit level by the same percent.

The effect of unemployment insurance level and duration might interact, where the level of one affects the impact of the other. To study this, we employ a two-dimensional regression discontinuity design that approximates the cross-randomization of two factors: benefit level and benefit duration. This approach allows us to estimate how benefit levels and durations *interact.* When potential benefit duration (PBD) is low, a 10 percent increase in benefit levels (BL) modestly increases unemployment duration by 0.06 months. When paired with longer PBD, the same 10 percent BL increase raises unemployment duration by 0.24 months. During the first 6 months of unemployment, when the unemployed in both high- and low-PBD counties receive benefits, the effect of BL increases on the probability to stay unemployed does not vary between counties with different potential benefit durations. Thus, during benefit receipt, the effect of the benefit level on labor supply does not vary with the PBD. Instead, the higher unemployment duration elasticity in 12-month PBD counties is simply due to the longer coverage, similar to the findings in Bell et al. (2024) for California.

We also examine how each policy dimension affects long-term job match quality by tracking unemployment status up to five years after the initial spell. Extended benefit durations increase the likelihood of long-run unemployment, while higher benefit levels show no systematic effect. We do not find evidence that either policy leads to decreased unemployment over the five-year horizon.

We then examine whether unemployment insurance creates moral hazard among employed workers. Using the discontinuities, we find that changes in benefit levels generate substantially larger effects on unemployment inflows than changes in benefit duration. While a 10 percent increase in potential benefit duration leads to a modest 2 percent increase in unemployment inflows, a 10 percent increase in benefit levels generates a much larger 13-17 percent increase. These increases in inflows around the policy threshold reflect permanent level shifts rather than intertemporal substitution. The marked difference in magnitudes between the inflow effects of benefit levels and durations suggests that raising benefit levels may generate significantly larger distortionary costs among the employed.

[4] This differs from some policy variation that only becomes clear to workers once they are unemployed and have applied for benefits. For instance, in Card et al. (2015a) and Johnston and Mas (2018), workers wouldn't know their benefit level and generosity until they became unemployed.

[5] For simplicity, we use the term *moral hazard* when referring to the overall disincentive effect of UI, which also includes the *liquidity effect* for the unemployed unable to smooth their consumption perfectly due to liquidity constraints (Chetty, 2008).

We study the characteristics of those who become unemployed in response to longer or more generous UI benefits.[6] For both policies, these marginal entrants tend to be older, more likely to be female, and less educated than the inframarginal unemployed population. Notably, despite these demographic differences, we find no corresponding differences in predicted unemployment durations across the cutoff. This suggests that the observed increase in unemployment duration is driven by behavioral responses to benefits rather than changes in the composition of the unemployed.

We interpret the welfare implications of our results by extending the Baily-Chetty model to include moral hazard among the employed (Baily, 1978; Chetty, 2006; Schmieder and von Wachter, 2016; Schmieder et al., 2012). In the model, workers maximize their individual utility, while the social planner maximizes aggregate welfare. The planner must balance the benefits of consumption smoothing against the cost of moral hazard, as reduced employment leads to lower total income. A key innovation in our approach is the treatment of unemployment transitions. Prior models typically assume exogenous separations, meaning that transitions from employment to unemployment occur independently of UI benefit generosity. In other words, these models posit that workers become unemployed due to exogenous factors only (like economic conditions or company restructuring) rather than in response to the UI system itself.

In contrast, our model incorporates our empirical finding that workers endogenously become unemployed in response to UI benefits. This means we account for the potential that more generous UI influences currently employed worker's decisions, potentially leading to increased unemployment inflows. We calculate the fiscal externality of redistribution to the unemployed following Chetty (2008) and Schmieder and von Wachter (2016). In the baseline exogenous layoff model (which is a special case of our model where the inflow channel is shut down), the cost of transferring \$1 to the unemployed in higher benefit levels is \$2.3 in behavioral distortions. When the model incorporates the social cost of endogenous inflows to unemployment, the calculated cost of transferring \$1 to the unemployed grows to more than \$10, since inflows respond strongly to benefit levels. The cost of endogenous inflows is smaller for potential benefit duration because extended durations cause a smaller inflow response. In a model without endogenous entry, a \$1 transfer to the unemployed through extended benefit duration costs \$2.5 in behavioral costs, similar to prior estimates (Centeno and Novo, 2009; Lalive, 2007, 2008; van Ours and Vodopivec, 2008).[7] When incorporating the behavioral costs among the employed, the cost of transferring \$1 increases to \$3.6.

We calculate the marginal value of public funds (MVPF) for benefits under a range of assumptions about risk aversion and self-insurance. The MVPF of BL or PBD increases is below one—unless a very high degree of risk aversion is assumed—implying that a reduction of UI generosity would improve social welfare. When accounting for endogenous inflows, the MVPF of

[6]To identify these characteristics, we first observe changes in the overall demographic composition of the unemployed at the cutoff, then calculate the implied demographic profile that would generate these compositional changes, given the increase in unemployment inflows.

[7]Behavioral cost calculations provided by Schmieder and von Wachter (2016).

BL increases is 0.4-0.5, even when assuming a relatively high coefficient of relative risk aversion of 5 and a large consumption drop at unemployment of 30 percent.[8]

Our work contributes to a long literature quantifying moral hazard from UI. While most of this research has focused on moral hazard among job seekers (Card et al., 2015a; Dahl and Knepper, 2022; Karahan et al., 2022; Landais, 2015b), some authors have also shown suggestive evidence of moral hazard among the employed. For instance, several studies have demonstrated layoff spikes when workers become UI eligible (see, for example, Albanese et al., 2020; Brébion et al., 2022; Christofides and McKenna, 1996 and Van Doornik et al., 2023). Another approach has been to show that workers and entrepreneurs reduce their effort when UI generosity increases (Ahammer et al., 2024; Ejrnæs and Hochguertel, 2013; Lusher et al., 2022). Most closely related to our paper are studies finding that cuts in benefit duration reduced unemployment inflows (Gudgeon et al., 2024; Hartung et al., 2024; Tuit and van Ours, 2010). Our study builds upon this literature by contributing: (1) transparent discontinuity evidence of moral hazard among the employed; (2) comparisons of benefit level and duration elasticities within the same labor market; (3) tests for moral hazard interactions between benefit level and benefit duration; and (4) interpretation through a welfare model that highlights the costs of moral hazard among the employed.

2 Institutional Background

2.1 Overview of the Polish Unemployment System

Poland's unemployment insurance (UI) system was established in 1989, shortly after the country's first free election in more than 40 years.[9] The program aims to provide temporary financial support to workers who have lost their jobs. Funded by a flat payroll tax, the system is administered by 340 county-level agencies known as *powiatowe urzędy pracy*, or public employment offices.[10]

Compared to other European Union countries, Poland's UI system is characterized by less generous benefits and shorter duration periods. For instance, while many EU countries offer benefits for 18–24 months or longer, Poland's typical duration is 6–12 months. The OECD reports an effective replacement rate of 44 percent for Poland, substantially lower than other EU countries such as Sweden (57 percent), Germany (59 percent), France (68 percent), and Switzerland (74 percent) (OECD, 2023). Poland's replacement rate more closely resembles English speaking countries like Australia (28 percent), the United Kingdom (33 percent), the United States (35 percent), and Canada (48 percent).

To be eligible for unemployment benefits in Poland, workers must have contributed to unemployment insurance for at least 12 of the previous 18 months. If workers themselves terminate

[8]For reference, Ganong and Noel (2019) find a 10 percent decline in consumption at unemployment.

[9]Under socialism, unemployment was officially non-existent, and thus the government did not provide unemployment insurance.

[10]Poland has a total of 380 counties. Some public employment offices are responsible for two neighboring counties.

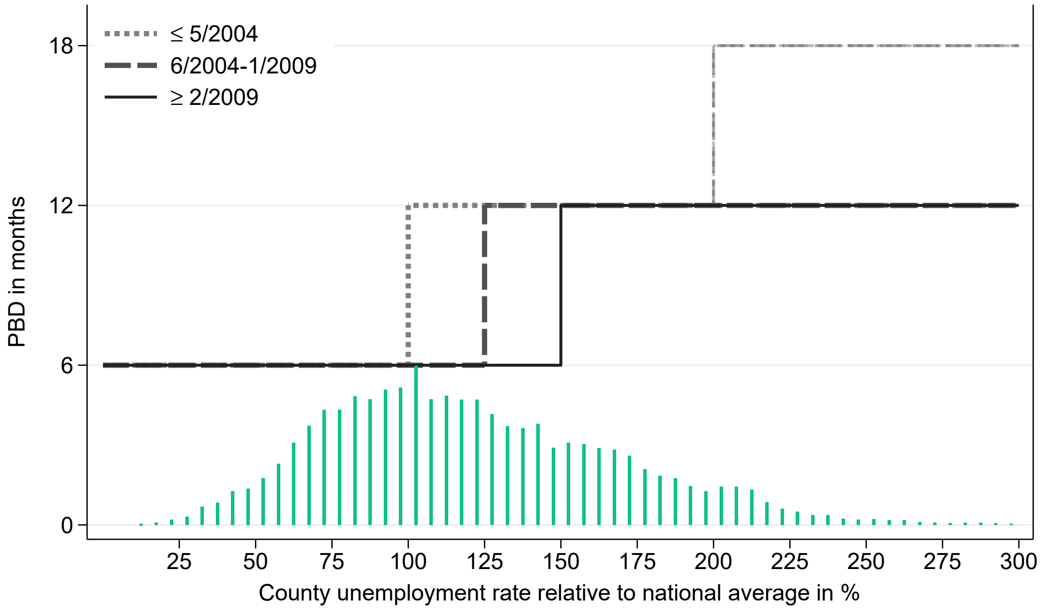
an employment relationship, rather than having been laid off, they only become eligible after 3 months and their benefit duration is shorter by this time period.

The application process for benefit involves two primary steps. First, workers register as unemployed at their local public employment office,[11] where eligibility is determined upon registration. Then benefits commence seven days after registration for eligible applicants.

When a worker becomes unemployed in Poland, the duration and level of their unemployment benefits depend on two key factors: (1) the unemployment rate in the county they live in and (2) their work history in UI-covered employment. These factors determine the worker's potential benefit duration (PBD) and the benefit level, respectively.

2.2 Determining Potential Benefit Duration

Figure 1: PBD rules



Notes: The figure depicts the potential-benefit-duration (PBD) rules for unemployed workers who are below the age of 50 (black lines). The vertical bars represent the distribution of relative unemployment rates of counties in the period 2000-2019. The dashed line at 18 months is a policy rule in place until 2009 which only affected the unemployed with at least 20 contributory years. Because just 0.32 percent (3 in 1,000) of the unemployed were eligible for 18 months, we focus on the other cutoffs in our empirical work.

Counties with higher unemployment rates relative to the national average offer a longer PBD to their unemployed residents. This policy, similar in spirit to the Extended Benefit program in the United States, aims to provide workers additional time to find new employment in less favorable economic conditions.

Our analysis focuses on unemployed workers younger than 50, as different rules apply to older workers. For instance, workers over 50 who have contributed to unemployment insurance for at

[11] If job termination occurs under mass layoff, the employer is obliged to inform public employment office about this fact.

least 20 years automatically receive extended durations, regardless of the local unemployment rate. This focus allows for a more straightforward analysis of the PBD rules.

For newly unemployed prime-age workers (under 50 years old) in a calendar year, workers normally receive 6 months of potential benefit duration. The PBD is extended to 12 months if the county's unemployment rate measured on June 30 of the prior calendar year is higher than 150 percent of the national unemployment rate measured at that time.

The unemployment-rate threshold triggering longer benefits has changed twice in our sample period as shown in Figure 1. Before May 2004, the threshold was 100 percent of the national average unemployment rate. From June 2004 to January 2009, the threshold was raised to 125 percent of the national average unemployment rate. And then since February 2009, the threshold has been 150 percent of the national average unemployment rate.[12]

An interesting feature of the PBD rule is its independence from absolute national unemployment levels. Whether the Polish unemployment rate is high (e.g., 20 percent in the mid-2000s) or at record-low levels (e.g., 3 percent in the early 2020s), a similar share of counties have extended PBD.[13]

The PBD for counties is determined annually based on the relative unemployment rate on June 30 of the previous year and remains constant for all UI claims submitted in the subsequent calendar year. Unlike to the US, changes to the benefit duration cannot occur within a year when economic conditions improve or deteriorate (see Jessen et al., 2024, for further details).

A potential threat to our identification strategy would be if workers could manipulate their benefit duration by changing their address to counties with more generous benefits. Both empirical and institutional evidence suggest this is not a concern. Empirically, we find no increase in cross-county moves at the PBD threshold (see Appendix Figure A.1). Institutionally, such manipulation is also less likely as simply renting in a different county is not sufficient to collect benefits there; the landlord must submit official documentation declaring the residence as permanent which then also increases tenants' rights.

2.3 Determining Benefit Level

Benefit levels in Poland are determined based on the duration of a worker's covered employment and are not affected by a worker's location or local unemployment rate. Unlike most UI systems elsewhere, benefit payments in Poland are not a fraction of previous earnings (up to a cap), but are fixed amounts that increase each year based on the consumer price index. Monthly benefits increase sharply at the thresholds of 5 and 20 contributory years.

For example, in 2023, if a worker has fewer than 5 contributory years, their monthly benefit amount is 1,192.90 PLN. If a worker has 5 to 19 contributory years, their benefit increases by 25 percent to 1,491.90 PLN. With 20 contributory years or more, benefits increases by 20 more

[12]In Appendix Figure A.2, panels (a)–(c) we plot the distribution of counties with PBDs of 6 or 12 months in years where different cut-offs were in place. Panel (d) illustrates that, in our sample period (2000–2019), very few counties have the same PBD throughout. Appendix Figure A.3 reports the average benefit and unemployment duration over time. Along with the improved macroeconomic environment, unemployment duration has dropped substantially over time.

[13]See Appendix Figure A.4.

percent to 1,790.30 PLN (European Commission, 2023). In 2010, an adjustment was introduced that increased the benefit level in the first three months of unemployment.

Together, these institutional features create multiple discontinuities that we can exploit in our empirical strategy. By examining behavior around these thresholds, we can isolate the causal effects of both benefit duration and benefit levels on various labor market outcomes.[14]

3 The Data

Our study uses comprehensive administrative data covering the universe of unemployment spells registered at the public employment offices in Poland. The dataset spans from January 2000 to July 2021, encompassing more than 40 million unemployment spells from 14 million individuals. This rich dataset provides a unique opportunity to examine the effects of unemployment insurance (UI) policies on labor market outcomes in Poland.

While our data cover all registered unemployment spells, we focus our analysis on claimants under 50 years of age, resulting in a sample frame of around 7 million unemployment spells. We choose this subset to avoid the fact that the thresholds do not result in the same treatment control comparisons for older workers. For instance, all workers older than 50 with at least 20 years of contributory years receive 12 months of PBD, regardless of their placement around the threshold.

Our dataset includes several key variables crucial for our analysis: precise start and end dates of unemployment and benefit receipt; cause of unemployment (e.g., layoffs, firings, quits); reason for leaving unemployment (e.g., resuming employment, starting old-age benefits); demographic information like date of birth, sex, county of residence; educational attainment; contributory years to unemployment insurance; and benefit level (available from 2004 onward). Of particular importance are the unemployment duration and benefit receipt variables, which allow us to accurately measure the effects of UI policies on spell length.

The assignment to a PBD regime is based on the relative unemployment rate of the county of residence of the unemployed. A county's unemployment being above the threshold, which has been adjusted twice in our sample period (see section 2), implies a PBD of 12 months. The benefit level, in contrast, depends on workers' contributory years to the UI system. While the benefit level itself is accurately reported in the data, there is some measurement error in contributory years (the running variable) as shown in Appendix Figure C.1; especially in close proximity to the cut-off, the assignment to a benefit level regime based on contributory years differs from the actual observed benefit level. The reason for this discrepancy is that upon registration the unemployed report their contributions which are recorded by the public employment offices. In some cases, the offices obtain additional information after initially recording the data, e.g. if some employment spells do in fact not count towards contributory years due to insufficient earnings. The benefit level is then accurately assigned based on the corrected contributory years, but the recorded contributory years in the data are not always updated. We discuss in Appendix C

[14] Appendix Figure A.5 shows the two intersecting discontinuities that allow us to test for interactions between benefit levels and benefit durations.

the consequences of this measurement error in the control variable. In short: it has almost no consequence on our RD estimates for benefit and unemployment duration and using a slightly adapted estimation strategy for inflows into unemployment we similarly find that the induced bias is negligible.

To observe full unemployment spells in our estimation, we use spells starting from 2000 to 2019 in our analysis sample. As we observe the end dates of unemployment spells until July 2021, none of the benefit spells and less than 1 percent of the unemployment spells are right-censored, minimizing bias from incomplete spells.

Table 1 presents summary statistics for our analytic sample of UI benefit recipients under 50 years of age. The average unemployed worker is 33 years old, with an even gender split (49.4 percent female). Mean unemployment duration (12.7 months) is about twice as long as the mean benefit duration (6.2 months), largely driven by a right tail of long unemployment spells. We see that more than half of recipients (54.6 percent) exhaust their benefits. And the average recipient resides in a county with an unemployment rate 18.7 percentage points higher than the national average.

These records provide several advantages for our study: (1) Comprehensive coverage of the Polish labor market over two decades; (2) precise measurement of unemployment spells and benefit receipt; and (3) a rich set of individual-level characteristics. These features enable us to examine the effects of UI policies on labor market outcomes in Poland.

Table 1: Summary statistics of spells of benefit recipients in analytic sample

Variable	Obs	Mean	Std. Dev.	P50
Age in years	7,129,048	32.77	8.505	32
Female (0/1)	7,129,048	.494	.5	0
Contributory years	7,129,048	9.127	7.678	6.649
Unemployment duration in months	7,129,048	12.662	15.729	7.951
Benefit duration in months	7,129,048	6.225	3.765	6
Benefits exhausted (0/1)	7,129,048	.544	.498	1
Months until entry into employment	4,020,888	6.303	4.593	5.421
Employment spell following unemployment	7,129,048	.656	.475	1
County unemployment rate relative to national average (%)	7,129,048	118.73	49.262	113.333

Notes: In this table, we present simple summary statistics describing the spells in the analytic sample.

4 Empirical Strategy

4.1 Estimating the Effect of Benefits on Unemployment Durations

Unemployment benefit claims are subject to rules that create sharp discontinuities in both potential benefit duration (PBD) and benefit level (BL). PBD discontinuities depend on county-level unemployment rates, while BL discontinuities depend on the duration of covered employment. To assess the effects of more generous UI on benefit and unemployment duration, we compare workers near each cutoff by estimating the following equation:

$$y_{ict} = \beta_0 + \beta_1 I(Treat = 1) + \beta_2 RU_{c/i,t} + \beta_3 RU_{c/i,t} \cdot I(Treat = 1) + county_c + year_t + \epsilon_{ict} \quad (1)$$

Here, y_{ict} is either the benefit duration or the unemployment duration of individual i in county c in year t.[15] On the right-hand side, RU is the running variable, representing either the relative county unemployment rate when estimating the effect of PBD (based on subscript c), or individual contributory years to unemployment insurance when estimating the effect of BL (based on subscript i). The indicator $I(Treat = 1)$ equals 1 if claimants have either a longer PBD or higher BL, and 0 otherwise.[16] β_1 is the coefficient of interest, capturing the estimated effect of additional UI generosity.

We center both running variables at zero by subtracting the cutoff value, such that positive values indicate that the worker has crossed the threshold for more generous or longer-lasting UI. We allow for different slopes in the running variable for treated and non-treated individuals and include controls for county and year fixed effects throughout the analysis. From an identification perspective, including these fixed effects is not necessary, but they increase precision. Standard errors are clustered at the county level. Our results are robust to a variety of alternative specifications which we discuss and present in subsection 6.4.

In our main specification, we use a linear model and employ the data-driven approach by Calonico et al. (2020) to determine the optimal bandwidth. Our findings are robust to using a quadratic polynomial, a wide range of bandwidths, and individual covariates. Due to substantial differences in benefit and unemployment durations across counties with different PBDs, we estimate the effects of higher BL separately for counties with PBDs of 6 and 12 months.

4.2 Estimating the Effect of Benefits on Unemployment Inflows

More generous unemployment insurance (UI) may not only reduce exits from unemployment but also increase the probability of workers entering unemployment. This suggests that moral hazard could affect both unemployed and employed individuals. Evidence on this inflow (or separation) margin is mixed, with some studies finding negligible effects (e.g. Nekoei and Weber, 2017; Schmieder et al., 2012) while others report larger impacts (e.g. Gudgeon et al., 2024; Hartung et al., 2024; Tuit and van Ours, 2010).

To measure the effect of more generous UI on unemployment inflows, we adjust equation (1). The unit of observation is bins of the running variable and the dependent variable y is the log number of individuals who become unemployed in the running-variable bin. For the potential

[15] We present estimates for durations in both levels and logs to enable a straightforward comparison to other estimates in the literature. As the comparison of the effects of a longer PBD and a higher BL is a core element of our paper, we largely focus on estimates in logs which facilitate us to translate estimates into elasticities. This is of particular importance in this context, as at the threshold PBD increases by 100 percent and BL only by 25 percent.

[16] At the PBD discontinuity, $Treat = 1$ if $RU_{c,t} > 0$, i.e. the relative unemployment rate has exceeded the threshold. For benefit levels, $Treat = 1$ if the recorded benefit level is 25 percent higher, but—as described in section 3—due to some measurement error in contributory years $RU_{i,t} < 0$ holds for some observations close to the cut-off.

benefit duration (PBD) threshold, we calculate the total number of inflows by county and month. To account for inter-temporal substitution around the time of PBD increases as found by Jessen et al. (2024), we calculate inflows using three sample windows: (i) over the entire calendar year, (ii) excluding months with inter-temporal substitution (February to September only), and (iii) only for June, far from potential January PBD changes. All approaches lead to qualitatively similar conclusions.

For the benefit level (BL) threshold, where the running variable contains some measurement error, we employ a modified estimation strategy. We first aggregate inflows per year in bins of 0.01 contributory years, centered around the 5-year threshold. Given the relatively tight bandwidths of around one year yielded by the Calonico et al. (2020) procedure, this binning results in 2,880 and 3,744 bins for PBDs of 6 and 12 months, respectively. For each bin, we calculate the share of spells with a higher indicated benefit level (cf. Appendix Figure C.1). Akin to the minimum wage literature (Dube and Lindner, 2024), this continuous variable indicates the "bite" of being exposed to a higher benefit level in each bin. The estimation equation for inflows around the benefit level threshold reads:

$$y_{bt} = \gamma_0 + \gamma_1 HighShare + \gamma_2 RU_{bt} + \gamma_3 RU_{bt} \cdot HighShare + year_t + u_{bt} \tag{2}$$

Y_{bt} is the log number of inflows per year t in bin b and $HighShare$ is the share of spells with a 25 percent higher benefit level in each bin. The running variable RU is the initially recorded contributory years. The coefficient of interest is γ_1. In Appendix C we discuss the consequences of the measurement error in the running variable for the benefit level estimation and show in a simulation exercise that using this approach the consequences are minor. Note that in a special case without measurement error, equation (2) becomes a standard RD estimation with $HighShare$ jumping from 0 to 1 at the threshold.

The key identifying assumption for regression discontinuity (RD) designs is the continuity of potential outcomes around the threshold (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In other words, assignment to treatment by the threshold should be as good as random, conditional on the running variable. Our findings that benefit generosity affects entry into unemployment indicates selection across the threshold, potentially commingling selection and treatment effects when estimating moral hazard among the unemployed.

Remarkably, we find that accounting for this selection has minimal effect on our estimates of moral hazard among the unemployed. While differences in covariates across the threshold are statistically significant, these differences are small and do not predict variations in benefit duration or the length of an unemployment spell. This suggests that the selection occurring at the threshold does not change our main estimates of the effect of UI generosity.

To ensure the robustness of our results and address potential concerns about selection, we conduct a comprehensive set of tests. First, following Card et al. (2007a), we use individual characteristics to predict benefit and unemployment durations. These predicted durations evolve smoothly across the thresholds. Second, we add a rich set of individual characteristics in estimation. The stability of the coefficients across controls suggests the discontinuity approximates randomization from an experiment. Third, we test for balanced covariates around the threshold.

While some estimates are statistically significant, they are rather small economically. Fourth, for workers with previous UI spells, we examine the discontinuity in prior-spell outcomes. The estimated discontinuities are small and typically not significant, suggesting limited scope for selection bias based on past UI experience.

These tests collectively suggest that while selection into unemployment does change at the benefit thresholds, the dimensions of selection we observe cannot explain the vast majority of the estimated effects on benefit and unemployment duration.

5 Graphical Evidence

Before turning to formal econometric estimation in section 6, we begin with a visual exploration of unemployment outcomes and how they differ descriptively by potential benefit duration (PBD) and benefit level (BL) regimes.

Figure 2 illustrates the evolution of unemployment outcomes after job loss. Panel (a) shows the share in benefit receipt over time after job loss, and panel (b) shows the share remaining in unemployment. The figure distinguishes between four groups of unemployed workers: those with 6 months of PBD and low benefits (solid black lines); those with 6 months of PBD and high benefits (solid green lines); those with 12 months of PBD and low benefits (dashed black lines); and those with 12 months of PBD and high benefits (dashed green lines).

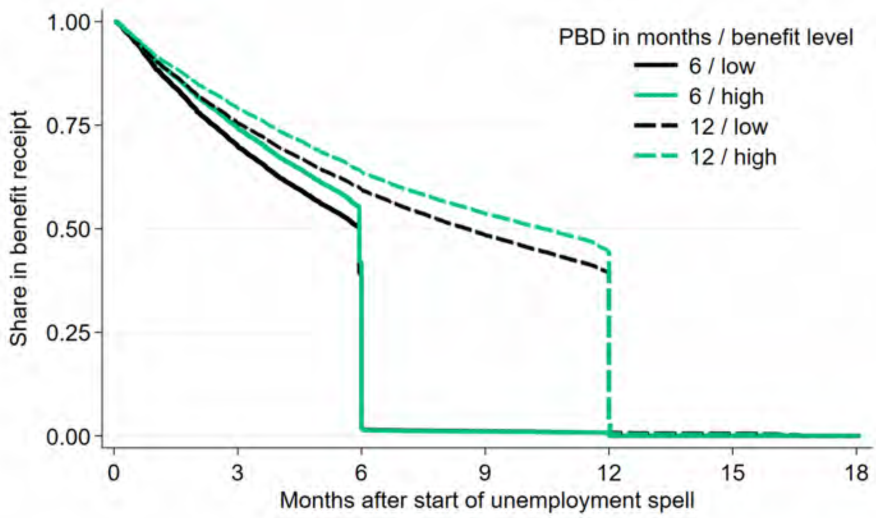
The figures reveal several noteworthy patterns. During the first 6 months, the two lines in the two panels are essentially identical. The reason is that all benefit recipients have a potential benefit duration of at least 6 months, such that exit from unemployment implies exit from benefit receipt and vice versa. During these first 6 months, those with 6-month PBD (solid lines) exit unemployment earlier than those with 12-month PBD (dashed lines), regardless of benefit level. Additionally, recipients with lower benefit levels (black lines) tend to have shorter durations of both benefit receipt and unemployment compared to those with higher benefit levels (green lines).

While these patterns suggest that more generous UI (both in terms of PBD and BL) is associated with longer benefit and unemployment durations, we cannot interpret this as a causal effect of UI generosity. This is because other factors may be at play. For instance, counties with a 12-month PBD face worse economic conditions, and individuals eligible for higher BL are more experienced and older on average—both demographics known to experience longer unemployment durations generally (see, e.g., Schmieder et al., 2012). These confounding factors necessitate a more rigorous analysis to establish causal relationships.

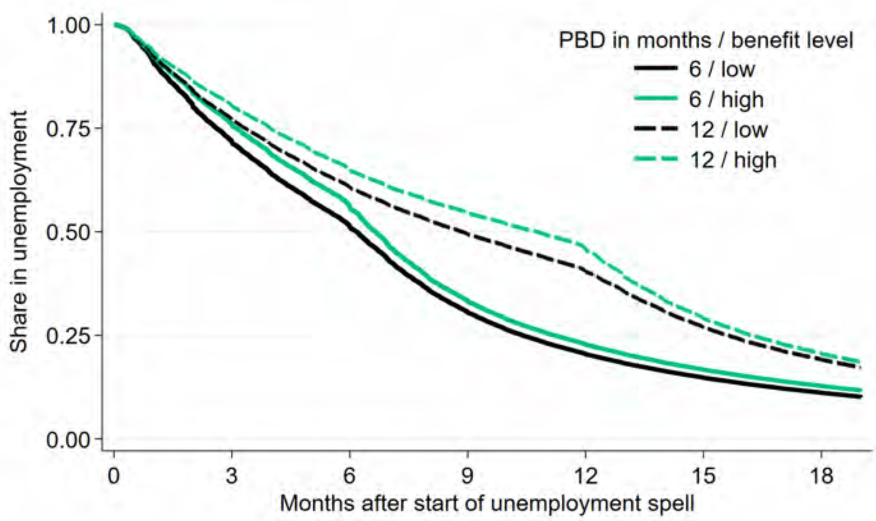
The relationship between unemployment spell duration and benefit regime is further demonstrated in panel (b) of Figure 2, which depicts overall unemployment duration, showing smoother curves compared to panel (a), which represents benefit receipt. This smoothness is a natural consequence of unemployment durations being, by definition, at least as long as the period of benefit receipt and not being mechanically censored at 6 or 12 months.

A closer examination of the data reveals persistent unemployment even after extended periods of benefit receipt. After 18 months, the proportion of individuals remaining unemployed ranges

Figure 2: Outcome evolution by PBD and BL regime



(a) Benefit receipt



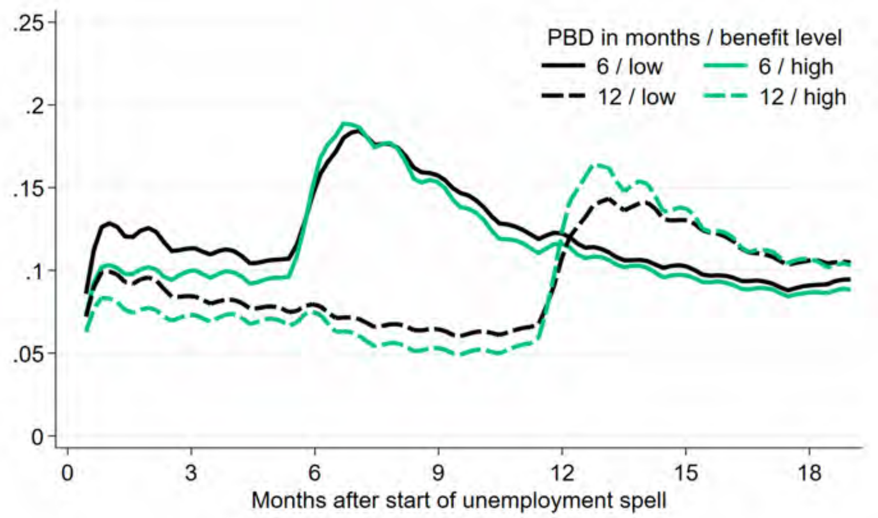
(b) Unemployment

Notes: Figure shows survival graphs for benefit receipt and unemployment for newly unemployed workers. The four lines distinguish by months of PBD (line pattern, determined by county unemployment rate) and by BL (line color, determined by contributory years). A higher level corresponds to an increase of 25 percent. Sample period is 2004 to 2019.

from 11.1 percent (for those with 6-month PBD and low BL) to 20.5 percent (for those with 12-month PBD and high BL). This observation, coupled with the high benefit exhaustion rate noted in Table 1, suggests a more complex dynamic than simple benefit exhaustion followed by rapid reemployment. Instead, it points to extended periods of uninsured joblessness for a significant portion of unemployed workers.

To further investigate the dynamics of unemployment exit, we present hazard rates for exiting unemployment in Figure 3.[17] For all four groups, we see pronounced spikes in exit when benefits end (cf. Card et al., 2007b; Lalive et al., 2006).

Figure 3: Hazard rates of unemployment exit by PBD and benefit level



Notes: Figure shows the hazard rates of unemployment exit. The four lines distinguish by months of PBD (line pattern, determined by county unemployment rate) and by BL (line color, determined by contributory years). A higher level corresponds to an increase of 25 percent. Sample period is 2004 to 2019.

To assess the causal relationship between UI generosity (in terms of PBD and BL) and job finding behavior, we now turn to regression discontinuity (RD) plots. These graphs illustrate average benefit and unemployment durations plotted against their respective running variables.

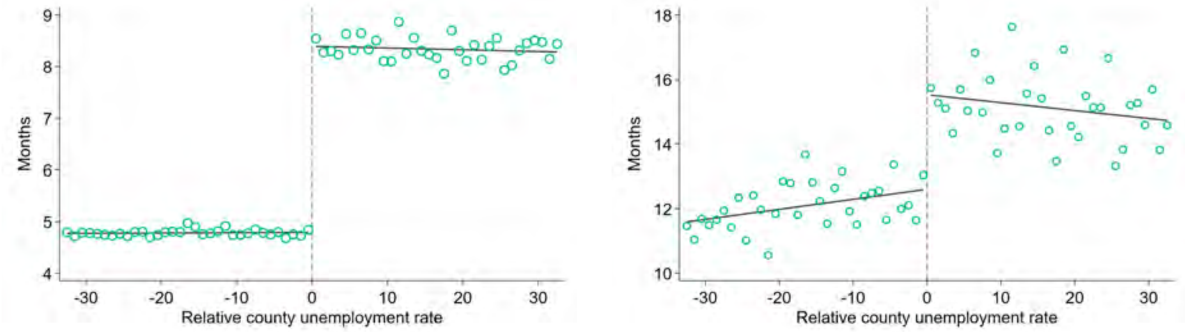
Our analysis utilizes the data-driven bandwidth selection procedure developed by Calonico et al. (2020). This method yields optimal bandwidths for benefit duration and unemployment duration that are very similar, differing by only 1.67 percent for the PBD estimation. To ensure consistency and facilitate direct comparisons, we use the average of these two optimal bandwidths. This approach results in identical estimation samples for both outcome variables. Specifically, we use a symmetric bandwidth of 32.47 percentage points for PBD estimation; 0.89 years for BL estimation in counties with a 6-month PBD; and 1.17 years for BL estimation in counties with a 12-month PBD.

We apply these averaged bandwidths consistently throughout our analysis, both in the RD plots presented in this section and in the formal econometric estimates detailed in Section 6. This uniform approach ensures comparability across our exhibits.

Figure 4 presents our first set of RD plots, illustrating the reduced-form relationship between our forcing variable for PBD—the relative unemployment rate—and two key outcomes: benefit duration and unemployment duration. These plots offer a visual representation of how these durations change at the PBD threshold.

[17] We focus on unemployment exit rather than benefit receipt exit, as the latter provides limited insight due to the majority of unemployed exiting benefit receipt precisely at the time of expiry (see panel (a) of Figure 2).

Figure 4: RD plots around PBD threshold



(a) Benefit duration

(b) Unemployment duration

Notes: Figures shows months in unemployment in bins of percentage point of county's relative unemployment rate. The bandwidth is determined using the automatic selection by Calonico et al. (2020). Solid lines linearly fit the scatters. Sample period is 2000 to 2019.

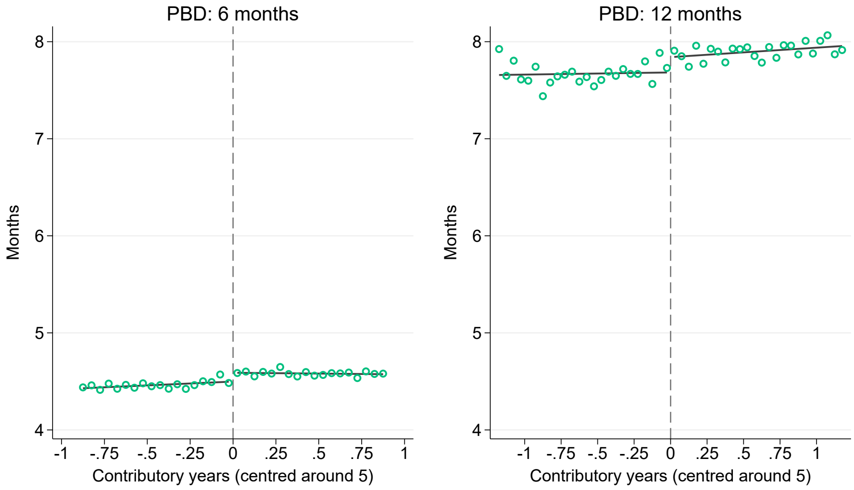
In Figure 4, each point represents a bin spanning one percentage point of the relative unemployment rate. We fit these points with linear regression, allowing for different slopes on either side of the cutoff. The vertical line denotes the threshold at which PBD increases from 6 to 12 months. The graph reveals a clear discontinuity at this cutoff: average benefit duration jumps by approximately three and a half months, while unemployment duration increases by nearly three months. These visual breaks in the fitted lines provide compelling preliminary evidence of the causal effect of extended PBD on both benefit receipt and overall unemployment duration.

Shifting our focus to the impact of benefit levels, Figure 5 illustrates how benefit and unemployment durations change around the BL threshold. In our analysis of the BL threshold, we stratify our sample based on the PBD regime of the claimant's county of residence (6 months vs. 12 months). This stratification is useful due to the substantial differences in average benefit and unemployment durations between these two types of counties. Such underlying differences could potentially interact with the effect of benefit levels, leading to heterogeneous impacts of higher BL across PBD regimes. By separating our analysis in this way, we can provide a more nuanced assessment of the BL impact on unemployment outcomes.

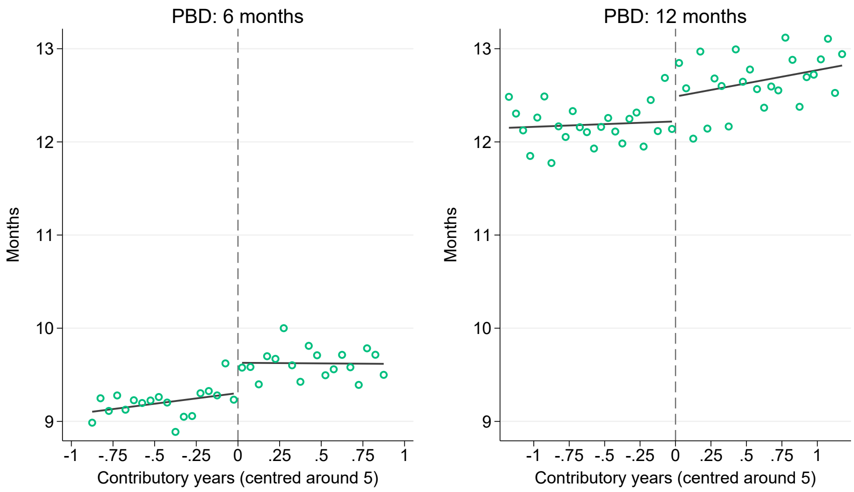
A clear pattern emerges that aligns with our PBD findings: more generous UI benefits, in this case manifested as higher benefit levels, are associated with extended durations of both benefit receipt and overall unemployment. This consistency across both dimensions of UI generosity—PBD and BL—strengthens the evidence for a causal relationship between more generous UI and longer unemployment spells.

These RD plots provide compelling visual evidence of the causal effects of both PBD and BL on unemployment outcomes. In the next section, we will formalize these relationships through rigorous econometric analysis, allowing us to quantify these effects precisely and test their robustness.

Figure 5: RD plots by BL



(a) Benefit duration



(b) Unemployment duration

Notes: Figures shows months in unemployment in bins of 0.05 contributory years. The bandwidth is determined using the automatic selection by Calonico et al. (2020). Solid lines linearly fit the scatters. Sample period is 2004 to 2019.

6 Regression Discontinuity Estimates

Our econometric analysis proceeds in four steps. First, we examine how more generous UI affects benefit receipt and unemployment durations. Second, we assess long-term effects on unemployment probability taking potential positive effects on job match quality into account. Third, we investigate the effects on unemployment inflows. Fourth, we demonstrate the robustness of our findings across a wide range of alternative specifications.

6.1 Effects on Benefit and Unemployment Duration

Table 2 presents our main regression discontinuity estimates based on equation (1). Columns (1)–(2) show the effects of extending PBD by 6 months, while columns (3)–(6) estimate the impact of increasing benefit levels by 25 percent.

We begin by examining the effects of extending PBD by 6 months. Panel A of Table 2 shows that this extension leads newly unemployed workers to collect benefits for an additional 3.46 months and increases their total unemployment duration by 2.44 months. When we consider these effects in logarithmic form (Panel B), we obtain elasticities of 0.62 for benefit duration and 0.29 for unemployment duration. These unemployment duration elasticities are somewhat smaller than the median elasticity of 0.4 reported for European studies in Schmieder et al. (2016), though more comparable to U.S. estimates (see also Landais et al., 2018a) and the average elasticity in the meta-analysis by Cohen and Ganong (2024) after correcting for publication bias.

Table 2: Effects of more generous UI on benefit and unemployment durations

Variation:	6 months longer PBD		25 percent higher BL			
Dependent variable:	Months of					
	benefit receipt	unemployment	benefit receipt		unemployment	
PBD:			6 mo	12 mo	6 mo	12 mo
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Levels						
RD estimate	3.4564***	2.4444***	0.1203***	0.3175***	0.2546***	0.5432***
	(0.0507)	(0.1395)	(0.0122)	(0.0295)	(0.0585)	(0.0982)
Panel B: Logs						
RD estimate	0.4304***	0.2006***	0.0496***	0.0685***	0.0557***	0.0746***
	(0.0072)	(0.0078)	(0.0049)	(0.0070)	(0.0060)	(0.0079)
Elasticity	0.621	0.289	0.222	0.307	0.250	0.335
Bandwidth	32.47	32.47	0.89	1.17	0.89	1.17
Observations	3,039,893	3,039,893	385,398	258,484	385,398	258,484

Notes: Estimates are based on equation (1). For the PBD estimates (columns 1-2) the running variable is the relative county unemployment rate and for BL estimates (columns 3-6) contributory years. All estimates include county and year fixed effects and a linear function of the running variable interacted with the treatment indicator. Sample period for PBD is 2000-2019 for PBD estimates and 2004-2019 for BL estimates. Standard errors clustered at the county-level in parentheses. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

The effects of increasing benefit levels by 25 percent, shown in columns (3)–(6), initially appear more modest. The resulting increase in benefit duration ranges from 0.12 to 0.32 months, while unemployment duration rises by 0.25 to 0.54 months. However, these smaller absolute effects must be considered relative to the size of the policy change—a 25 percent increase in benefit levels versus a 100 percent increase in benefit duration. When we examine the log specifications and corresponding elasticities, we can more easily make apples-to-apples comparisons of benefit level and benefit duration. While the elasticities of benefit receipt remain smaller than those for PBD (a benefit increase has no mechanical effect on benefit receipt duration), the unemployment duration elasticities are comparable and, notably, larger for workers with a 12-month PBD compared to those with a 6-month PBD. This suggests that unemployment durations are particularly responsive to benefit levels when workers have access to extended benefits. As with our PBD estimates, these benefit level elasticities are slightly smaller than the mean of those reported previously in the literature (Schmieder et al., 2016) but in the ballpark of publication-bias-corrected estimates (Cohen and Ganong, 2024).

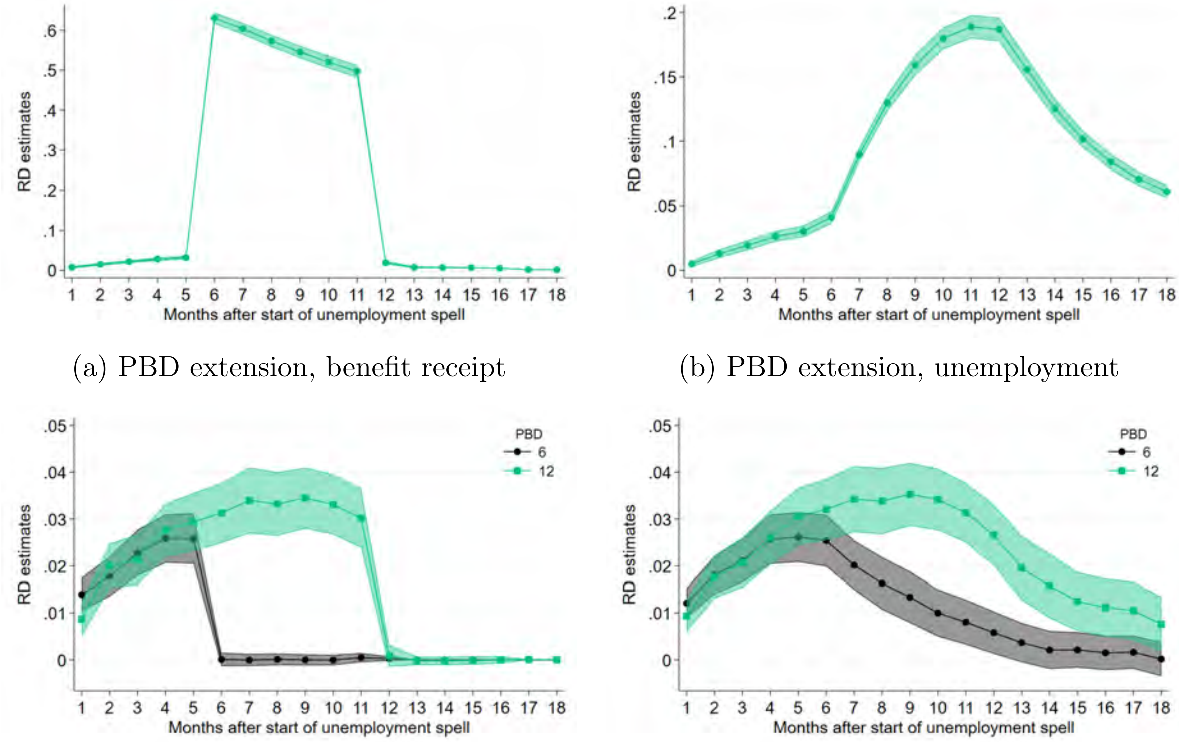
To facilitate direct comparisons between PBD and BL effects, we conduct additional analysis using a consistent sample of unemployment spells that appear in both our PBD and BL estimations (see Appendix Table B.1). In our main estimates (Table 2), the elasticities of both benefit receipt and unemployment duration with respect to benefit levels are 34–39 percent larger when workers have access to extended PBD. These interaction effects become more pronounced in our matched sample analysis, where the corresponding elasticities are 36–53 percent larger in the presence of longer benefit duration. This pattern suggests that the distortionary effects of more generous benefits are amplified when combined with longer potential benefit durations, highlighting important interactions between these two policy dimensions.

To dissect the summary duration estimates throughout the unemployment spell, we report monthly RD estimates for the probability to be in benefit receipt or in unemployment in Figure 6. The upper two panels concern PBD extensions. In the first six months, all unemployed can receive benefits, so any positive effects are solely due behavioral adjustments. After five months, the share receiving benefits and in unemployment has increased by around three percentage points for the treated individuals compared to the untreated. For benefit receipt, estimates between 6 and 11 months reflect a mix of a behavioral and mechanical effect. In absence of a behavioral effect, the point estimates would correspond to the share of individuals still in unemployment at the respective points in time and reflect the increase in coverage, which often constitutes a large part of PBD estimates of benefit durations (Schmieder et al., 2012).[18] In contrast, the treatment effect on unemployment probability has no mechanical component and peaks at an increased probability of around 19 percentage points after 11 months.

The bottom panels of Figure 6 show estimates for the BL increases, separately for counties with a 6 (black circles) or 12 (green squares) months PBD. The initially almost indistinguishable point estimates and overlapping confidence intervals in panels (c) and (d) reveal that the larger coefficients in the 12-month PBD regime shown in Table 2 are solely due to a so-called *"coverage effect"* as in Bell et al. (2024) and not due to a different behavioral response. During the first 6 months of the unemployment spell, while individuals in both high- and low-PBD counties receive benefits, the responses to BL increases do not differ across counties. Five months after the start of the unemployment spell, the effect of the benefit increase on the probability to still receive benefits is an increase of about 3 percentage points—irrespective of the PBD regime. Only when benefits expire in counties with a shorter PBD after 6 months, the curves diverge leading to the larger duration estimates in the more generous PBD counties.

[18] Gałecka-Burdziak et al. (2021) calculate that in a 2009 reform of the PBD threshold in Poland, the mechanical component of changes in the benefit duration was 96 percent and the behavioral component only 4 percent.

Figure 6: Monthly estimates of more generous UI on benefit receipt and unemployment



(a) PBD extension, benefit receipt

(b) PBD extension, unemployment

(c) BL increase, benefit receipt

(d) BL increase, unemployment

Notes: Figure shows monthly RD estimates of the effect of an increase in UI generosity on the probability to be unemployed. These are obtained from separate linear probability model estimations for every month. Panels (a) and (b) show effects of PBD extensions, panels (c) and (d) for benefit level increases. Estimates are based on equation (1) and correspond to the duration estimates shown in Table 2. Sample period is 2000 to 2019. Shaded areas show 95% confidence intervals obtained from standard errors clustered at the county-level.

To formally test for the PBD-BL interaction effect, we implement a two-dimensional regression discontinuity design that exploits both discontinuities simultaneously. This approach leverages two distinct running variables, centered around the relevant thresholds: contributory years (C_{it}), which determines eligibility for higher monthly benefits, and the relative county unemployment rate (U_{ict}), which determines extended PBD. For individual i in county c at time t, let y_{ict} represent our outcome of interest, G_i indicate whether the individual receives higher benefits, and D_{ict} indicate whether their county has crossed the threshold for extended PBD. We estimate local polynomial regressions of the form:

$$y_{ict} = \beta_0 + \beta_1 G_i + f(C_{it}) + \beta_2 D_{ict} + g(U_{ict}) + \beta_3(G_i \times D_{ict}) + county_c + year_t + \varepsilon_{ict} \qquad (3)$$

In this specification, $f(.)$ and $g(.)$ are continuous functions of their respective running variables, allowing for flexible relationships between each running variable and the outcome. The coefficients have natural interpretations: β_1 measures the effect of higher benefit levels holding duration constant, β_2 captures the effect of extended benefit duration holding benefit levels constant, and β_3 identifies the interaction between the two policies. The two-dimensional RD

Table 3: Two-Way Regression Discontinuity to Estimate Interaction Effects of Benefit Levels and Duration

Dependent variable:	Months of			
	benefit receipt	unemployment	benefit receipt	unemployment
	levels		logs	
	(1)	(2)	(3)	(4)
BL	0.0777***	0.1615*	0.0370***	0.0436***
	(0.0198)	(0.0937)	(0.0064)	(0.0081)
PBD	3.0169***	2.0674***	0.3843***	0.1821***
	(0.0572)	(0.1442)	(0.0108)	(0.0126)
PBD x BL	0.2629***	0.4490***	0.0344***	0.0345***
	(0.0322)	(0.1146)	(0.0085)	(0.0099)
Observations	292,208	292,208	292,077	292,208

Notes: The estimates include both forcing variables (relative unemployment rate and contributory years) interacted with the respective treatment indicator (PBD and BL, respectively), see equation (3). Additionally, the two forcing variables, and the two treatment indicators are interacted with each other. Sample period is 2004-2019. Standard errors clustered at the county-level in parentheses. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

design identifies these effects by comparing individuals just above and below each threshold while flexibly controlling for the other running variable.

The key parameter of interest is β_3, which tests whether the effects of benefit levels and durations are multiplicative rather than merely additive. A positive interaction term would indicate that the moral hazard effects of one policy dimension are amplified by the other. Table 3 presents these estimates.

Our estimates reveal substantial heterogeneity in the effect of benefit levels across PBD regimes. When potential benefit duration is short (6 months), a 10 percent increase in benefit levels leads to a modest 0.06-month increase in unemployment duration (0.1615/2.5, as levels increase by 25 percent). However, this same 10 percent benefit increase generates a much larger effect—a 0.24-month increase in unemployment duration—when combined with extended potential benefit duration ($(0.1615 + 0.449)/2.5$). The difference is economically and statistically significant: the elasticity of unemployment duration with respect to benefit generosity is 80 percent larger under extended PBD (0.35 vs. 0.2). This interaction suggests that the moral hazard costs of increasing benefit levels are substantially amplified when workers have access to longer benefit durations. In Appendix Figure A.6 we additionally report monthly estimates of the PBD-BL interaction term. This allows to test more explicitly than in Figure 6 whether the interaction of duration extensions and level increases is in fact due to the "coverage effect" where the amplifying effect materializes only after PBD benefits expire at the less generous side. We find this to be the case indeed.

6.2 Long-term Effects on Unemployment Probability

While we have established that more generous unemployment insurance extends unemployment durations, a key policy question remains: does longer job search lead to better job matches? Theory suggests two competing mechanisms. On the one hand, more generous UI could enable workers to search longer for higher-wage jobs and better employment matches, potentially reducing future unemployment risk. On the other hand, duration dependence in job search creates a countervailing force—longer unemployment spells might result in stigma or skill depreciation that reduce wages (Acemoglu and Shimer, 1999; Nekoei and Weber, 2017).

The empirical evidence on this question is mixed (Le Barbanchon et al., 2024). Schmieder et al. (2016) and Johnston and Mas (2018) find modestly negative effects of UI extensions on reemployment wages, while Nekoei and Weber (2017) document increased long-run earnings in Austria, attributing this to improved employment match quality. Similarly contrasting results emerge for employment stability: Caliendo et al. (2013) find more stable subsequent employment patterns, while Card et al. (2007a) and van Ours and Vodopivec (2008) find no effects on either wages or employment stability in Austria and Slovenia, respectively.

To assess match quality, we examine two key measures: the probability of subsequent unemployment spells and total days spent unemployed in the years following an initial claim.[19] While our previous results show that more generous UI increases the duration of initial unemployment spells, the long-term effects are theoretically ambiguous. If extended job search leads to better and more stable matches, we might expect lower unemployment rates in subsequent years, potentially offsetting the initial increase in unemployment duration

Our analysis requires a sufficiently long follow-up period to detect potential benefits of improved job matches. Therefore, we restrict our sample to individuals who began their unemployment spells before the end of 2014, allowing us to track each person for at least five years after their initial claim. Figure 7 plots unemployment rates over this five-year period (months 1-60), pooling observations across all PBD and BL regimes. The green area represents the share of individuals still in their initial unemployment spell (the standard survival curve), which declines steadily over time, consistent with the patterns observed in Figure 2. The gray shaded area shows the share of individuals who have entered subsequent unemployment spells. Notably, by the second year after the initial claim, subsequent spells account for a larger share of total unemployment than continuing initial spells.

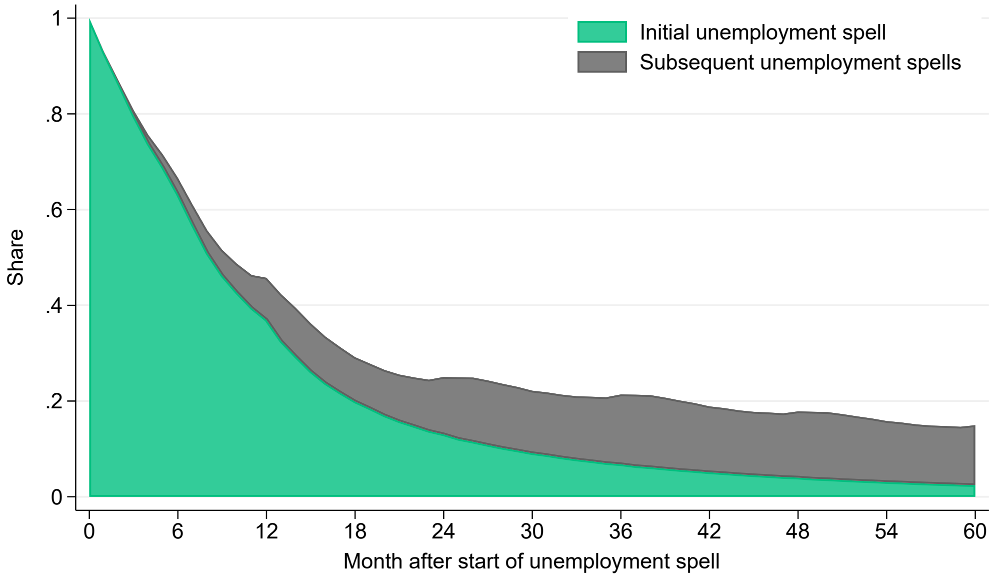
Figure 8 presents regression discontinuity estimates of how UI generosity affects unemployment probability over time.[20] We estimate separate regressions for each month using our baseline specification—the same bandwidth, linear polynomial, and fixed effects employed in our previous duration analysis.

Panel (a), which focuses on PBD extensions, reveals three key patterns. First, extended benefits increase the probability of unemployment from the start of the initial spell. Second, this effect peaks at approximately 12 months, when the unemployment probability is 20 percentage

[19] Unfortunately, we have no information on re-employment wages.

[20] Appendix Figures A.7 and A.8 provide complementary evidence on benefit receipt probabilities over the same five-year period.

Figure 7: Share of benefit recipients in unemployment



Notes: Figure depicts the share of benefit recipients in unemployment in months after begin of their unemployment spell. The green area is the initial unemployment spell and therefor corresponds to the averages of survival functions shown in panel (b) of Figure 2. The gray shaded area is the share unemployed in subsequent unemployment spells. The sample is restricted to benefit recipients who entered unemployment before the end of 2024 to ensure a sufficient post period of 5 years.

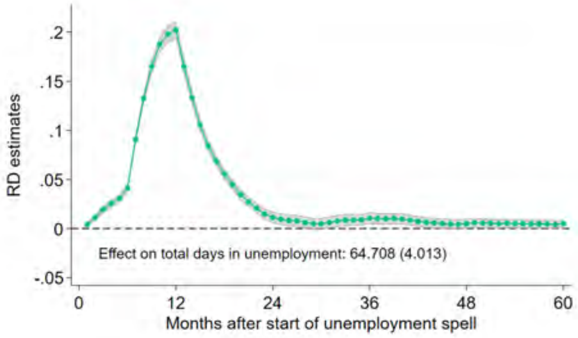
points higher for those receiving extended benefits. Third, while the effect moderates after this peak, extended benefits cause additional unemployment throughout the entire five-year follow-up period. In aggregate, the PBD extension increases total days in unemployment by 65 days in the five years following an unemployment claim (with a mean at the stingy PBD side of 553 days).

Further analysis in Appendix Figure A.9, which decomposes these effects into initial and subsequent unemployment spells, offers additional insight. While the probability of subsequent unemployment spells is lower in the second year, this reflects the mechanical effect of extended initial spells rather than improved job matching—more workers with extended PBD are still in their initial spell of unemployment.

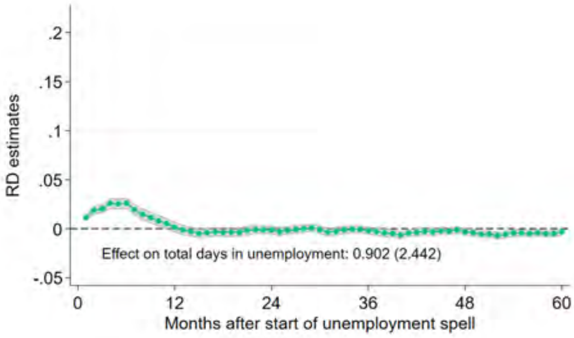
Panels (b) and (c) of Figure 8 present the effects of higher benefit levels, estimated separately for counties with 6- and 12-month PBD. While higher benefits initially increase unemployment probability, these effects are more modest and shorter-lived than the PBD effects. The impact becomes statistically indistinguishable from zero after 12 months, and occasionally turns slightly negative. Over the full five-year period, higher benefit levels increase total days in unemployment by only 0 to 6 days.

These patterns challenge the hypothesis that more generous UI improves job matching. Neither extended benefit durations nor higher benefit levels reduce future unemployment risk in Poland, suggesting that the initial increases in unemployment duration do not translate into more stable subsequent employment.

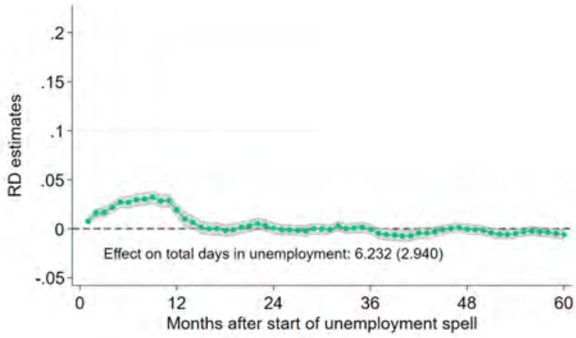
Figure 8: Effects of UI generosity on unemployment after initiation of unemployment



(a) PBD



(b) BL, 6 months PBD



(c) BL, 12 months PBD

Notes: Each coefficient stems from a separate regression and is estimated with the optimal bandwidth (see Table 2), linear function of the running variable and including county and year fixed effects. Gray shared areas indicate 95% confidence intervals.

6.3 Effects on Inflows into Unemployment

While the negative effects of unemployment insurance on job search are well-documented, far less attention has been paid to how UI generosity affects entry into unemployment. Early regression discontinuity studies (e.g. Card et al., 2007a; Nekoei and Weber, 2017; Schmieder et al., 2012) examined inflows primarily to verify their identifying assumption, finding little evidence of selection around UI generosity thresholds. However, recent research across multiple countries suggests that unemployment inflows from employment as a result of UI may be quantitatively important (Gudgeon et al., 2024; Hartung et al., 2024; Jessen et al., 2024).

For Poland specifically, Jessen et al. (2024) document substantial intertemporal substitution in unemployment inflows around changes in potential benefit duration (PBD). Because county-level PBD changes are announced in September and come into effect in January, workers and firms can time their separations to maximize benefit eligibility. When a county's PBD is set to increase from 6 to 12 months, unemployment inflows decline in the months just before the change (October-December) and spike immediately after. To account for this strategic timing, we measure inflows in three ways: across the full calendar year, excluding the months of apparent substitution (February-September only), and focusing on a single month (June) far from the PBD change. In contrast, changes in benefit levels (BL) occur at fixed contributory-year thresholds and thus do not generate similar seasonal patterns.

Columns (1)–(3) of Table 4 present our regression discontinuity estimates of how PBD extensions affect unemployment inflows. Using the discontinuity in county-level unemployment rates, we estimate the effect on log monthly inflows aggregated to the county-month level. The results reveal two key patterns. First, when we examine total annual inflows, a 6-month extension in PBD increases unemployment entry by approximately 13 percent. Second, this effect partially reflects strategic timing of unemployment claims: workers in counties approaching a PBD increase often delay their claims from November-December to January to qualify for extended benefits. When we exclude these months of apparent substitution, we still find substantial effects—PBD extensions increase unemployment inflows by 8 percent.

Table 4: Effects of more generous UI on inflows into unemployment

Variation:	6 months longer PBD			25 percent higher BL	
Dependent variable:	Log inflows into unemployment				
Sample:	Full year	Feb-Sep	June	6 mo PBD	12 mo PBD
	(1)	(2)	(3)	(4)	(5)
Inflow effect	0.1318***	0.0827***	0.0822***	0.3713***	0.2867***
	(0.0122)	(0.0113)	(0.0179)	(0.0281)	(0.0228)
Elasticity	0.190	0.119	0.119	1.664	1.285
Number of spells	5,748,171	3,466,842	401,636	385,398	258,484
Observations	35,904	23,936	2,980	2,848	3,712

Notes: Estimates of PBD extensions are based on equation (1) and estimates for level increases based on equation (2). The reported coefficients are β_1 and γ_1 for PBD extensions and level increases, respectively. For the PBD estimates (columns 1-3) the running variable is the relative county unemployment rate and for BL estimates (columns 4-5) contributory years. All estimates include a linear function of the running variable interacted with the treatment indicator. For the PBD estimation, the number of inflows into unemployment are aggregated at the county-month-level. PBD estimates include county and year fixed effects. For the BL estimation, number of inflows are aggregated in bins of 0.01 years at the annual level. Sample period is 2000-2019 for PBD estimates (excluding 2004 and 2009 where the PBD changed during the year) and 2004-2019 for BL estimates. For the PBD estimates, standard errors are clustered at the county level, for BL estimates, standard errors are robust. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

Columns (4)–(5) of Table 4 present the effects of higher benefit levels on unemployment inflows.[21] The response to benefit levels is substantially larger than to benefit duration: unemployment inflows increase by approximately 20 percent when workers become eligible for 25 percent higher benefits. Converting these effects to elasticities highlights this difference—the elasticity of inflows with respect to BL (1.29-1.66) are around times larger than the elasticity with respect to PBD (0.12-0.19).

Two factors might explain this stronger response to benefit levels. First, higher benefit levels affect all unemployed workers, whereas extended durations matter only for those who exhaust

[21] Appendix Figures A.10 and A.11 provide the corresponding reduced-form RD plots for both PBD and BL increases.

their benefits. The importance of this distinction may be even greater than suggested by observed exhaustion rates (see Table 1), as newly unemployed workers tend to strongly underestimate their unemployment duration (Caliendo et al., 2024). Second, benefit levels—which vary at the individual level based on work history—may be more salient to workers than the county-level unemployment rates that determine PBD.

More generous unemployment benefits may affect unemployment inflows through two channels. First, workers anticipating higher benefits might reduce their effort and attendance, leading to increased dismissals. Second, substantial inflow effects may reflect strategic behavior between workers and employers. Several institutional features of Poland's labor market facilitate such strategic responses. Foremost, employers do not internalize the fiscal costs that layoffs impose on the community (Johnston, 2021; Lester and Kidd, 1939). This misalignment of private and social costs creates incentives for workers and firms to collude at taxpayers' expense.

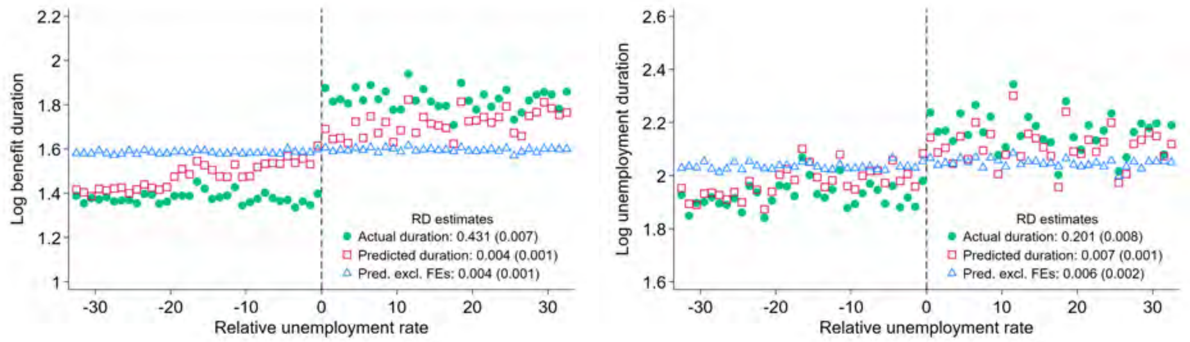
This strategic behavior could take three forms. First, when UI generosity increases, the surplus of some job matches becomes negative. In this case the worker could quit the job. Since this would shorten the duration of benefit receipt, the employer might agree to fire the worker. In many European countries, fired workers can sue employers for unjust dismissal and often the burden of proof is on the employer (OECD, 2019). Such a setting makes firing employees potentially costly. Instead, in Poland—as is the case in the US—the burden of proof was mostly on the employee when filing a complaint for unfair dismissal. This might facilitate collusion between employers and employees. Second, workers and employers might explicitly coordinate layoff timing to maximize UI benefits, similar to patterns documented by Van Doornik et al. (2023) in Brazil, where workers are often laid off upon UI eligibility and recalled once benefits expire. Third, employers might implicitly respond to UI generosity by concentrating necessary layoffs in periods when workers qualify for more generous benefits, thereby reducing the economic hardship their former employees face.[22]

6.3.1 Assessing the Importance of Selection for Duration Estimates

Our findings of substantial inflow effects raise a potential threat to our estimates of how UI generosity affects unemployment duration. If workers who are more likely to have longer unemployment spells are also more likely to enter unemployment when benefits are more generous, our duration estimates would conflate two effects: the true causal effect of UI generosity and compositional changes in the unemployed population around the threshold. This selection would violate the key regression discontinuity assumption that potential outcomes are continuous at the threshold. However, we present several pieces of evidence showing that while UI generosity affects the number of unemployment claims, it does not meaningfully change the composition of claimants in ways that would bias our duration estimates.

[22] As an alternative mechanism, more generous UI can lead firms to lay off more workers in response to lower demand, as a lower search effort of those workers increases the probability that firms could re-hire those workers when demand increases again (Feldstein, 1978).

Figure 9: Comparison of observed and predicted durations—PBD



(a) Benefit duration (b) Unemployment duration

Notes: Actual durations (circles) correspond to those shown in levels in Figure 4. Predicted durations (hollow squares) are obtained from regressing the observed durations on age, female indicator, number of contributory years, number of unemployment spells, education dummies, previous occupation, county FEs and year FEs. The predictions depicted as hollow triangles exclude county and year FEs. RD estimates as in Table 2.

Our first test follows Card et al. (2007a), examining whether observable characteristics predict discontinuities in unemployment durations at the benefit thresholds. We begin by regressing log benefit and unemployment durations on a rich set of covariates to generate predicted durations.[23]

Figure 9 compares actual and predicted durations around the PBD threshold, with corresponding evidence for the BL threshold presented in Appendix Figure A.12. The actual durations (green circles), which correspond to the estimates in Table 2, show clear discontinuities at the thresholds. While our covariates strongly predict duration—increasing the R-squared by 30 percent—the predicted durations (hollow red squares and blue triangles) show no meaningful discontinuities.[24] This smooth distribution of predicted durations suggests that changes in claimant composition are not driving our estimated effects in duration.

While the comparison of observed and predicted durations suggests only minor selection effects, we conduct three additional analyses to verify this conclusion. First, we examine the sensitivity of our estimates to increasingly rich controls. Appendix Figure A.13 presents RD estimates from four specifications: (1) A baseline specification with only the interacted running variable; (2) a specification adding county and year fixed effects, which increases precision but leaves point estimates unchanged; (3) a specification that includes individual demographic characteristics, which has little effect on the estimates; and (4) a specification adding controls

[23]For the PBD discontinuity, covariates include age, gender, education (six categories), previous occupation (42 categories), urban/rural status, contributory years, and county and year fixed effects. We exclude contributory years from the BL analysis as this defines the treatment threshold.

[24]Red squares represent predictions using all covariates. Blue triangles exclude county and year fixed effects, which determine treatment assignment. Excluding these fixed effects eliminates the gradual increase in predicted durations with relative unemployment rates, leading to a flat distribution. As an alternative specification, we also predicted durations based on a sample of unemployed unaffected by the PBD threshold (unemployed ineligible to receive benefits) and estimate slightly smaller point estimates and therefore qualitatively similar conclusions. We omit the specification excluding county and year FEs in Appendix Figure A.12 as fixed effects play no significant role at the BL threshold.

for previous unemployment history (number and duration of prior spells) to capture potential unobserved heterogeneity in unemployment risk.

The stability of our estimates across these specifications, even when controlling for detailed unemployment histories, suggests that selection on either observable or unobservable characteristics is not driving our results. To further verify this, Appendix Table B.2 documents the distribution of individual characteristics around the benefit thresholds. While we find statistically significant differences in claimant demographics across the threshold (as have also been found in other studies with comparable statistical power, see Bell et al., 2024; Schmieder et al., 2012), these differences are small in magnitude and do not predict meaningful variation in benefit or unemployment duration.

As a final test, we examine workers with previous unemployment spells, comparing RD estimates for their current and previous spells. If selection was driving our results, we would expect to see differences in previous spell durations across the threshold. However, estimates in Appendix Table B.3 show that differences in previous spell durations are both an order of magnitude smaller than our main effects and typically statistically insignificant.

Collectively, these analyses paint a clear picture: while more generous UI benefits do increase the number of workers entering unemployment, this selection does not meaningfully bias our estimated effects of UI generosity on unemployment duration. The stability of our estimates across specifications, the smooth distribution of predicted durations, and the absence of effects on previous spell duration all suggest that our main results reflect the causal effect of UI generosity rather than changes in claimant composition.

6.3.2 Demographics of the Induced Unemployed

Having established that more generous benefits increase unemployment inflows, we now ask: who are these additional claimants? Specifically, how do the characteristics of workers induced into unemployment by more generous benefits differ from those who would have entered unemployment regardless?

To answer this question, we develop a method to identify the demographic profile of these marginal claimants using our regression discontinuity design. The intuition is straightforward: if we know both the increase in unemployment claims at the threshold and the change in average claimant characteristics, we can recover the characteristics of the marginal group. This calculation relies on the assumption that the characteristics of inframarginal claimants (those who would enter unemployment anyway) evolve smoothly across the threshold.

Consider a concrete example to illustrate our approach. Imagine at the generosity threshold, the average age of claimants increases by 0.1 year. Intuitively, the marginal claimants must be older than the inframarginal claimants to generate this change, and the degree of difference between marginal and inframarginal claimants depends crucially on the size of the inflow effect: if the increase in claims is small, the marginal group must be substantially different to generate the observed change in average characteristics; if the increase is large, however, even small differences in the marginal group could shift the overall average.

To formalize this intuition algebraically, we define two key quantities related to unemployment inflows. First, let c_p represent the baseline probability of entering unemployment—specifically, the predicted inflow rate just below the RD threshold where benefits are less generous. We estimate this baseline rate without county and year fixed effects to capture the pure probability of entering unemployment. Second, let B_p represent the increase in unemployment probability at the threshold—that is, the causal effect of more generous benefits on unemployment inflows.

To characterize the marginal claimants, we need two additional pieces of information for each demographic characteristic (such as age, gender, or education). First, let c_d represent the average characteristic just below the threshold—this captures the demographic profile of inframarginal claimants who would enter unemployment even with less generous benefits. Second, let B_d represent the discontinuous change in this characteristic at the threshold. Combined with our earlier measures of unemployment inflows (c_p and B_p), these values allow us to solve for the characteristics of marginal claimants, those induced into unemployment by more generous benefits.

We can thus calculate what the demographic characteristics of the marginal group must be to rationalize the observed change in average characteristics at the threshold:

$$\underbrace{(c_p \times c_d)}_{\text{demographics of inframarginal}} + \underbrace{(B_p \times \mu_m)}_{\text{demographics of marginals}} = \underbrace{(c_p + B_p) \times (c_d + B_d)}_{\text{total demographics on generous side}} \tag{4}$$

By solving for μ_m (the mean characteristic of the marginals), we recover the average trait of workers induced into unemployment by more generous benefits. To illustrate this approach, consider a simple example: suppose the probability of claiming unemployment benefits doubles at the threshold from 10 percent to 20 percent, and the fraction of female claimants increases from 0 percent to 20 percent. For these changes to be consistent, the marginal claimants must be 40 percent female. We can verify this through our equation: $(0.10 \times 0) + (0.10 \times \mu_m) = (0.10 + 0.10) \times (0 + 0.20) \Rightarrow \mu_m = 0.40$, meaning that 40 percent of the marginal claimants must be female to generate the observed increase in average female share from 0 to 20 percent.

Table 5: Demographics of inframarginal and marginal claimants

	PBD Discontinuity		BL Discontinuity - 6mo		BL Discontinuity - 12mo	
	inframarg. mean (1)	marginal mean (2)	inframarg. mean (3)	marginal mean (4)	inframarg. mean (5)	marginal mean (6)
Age	32.147	35.478	29.641	31.922	29.837	31.927
Female (0/1)	0.458	0.663	0.569	0.550	0.523	0.493
Higher education (0/1)	0.497	0.535	0.663	0.588	0.550	0.420
Number of previous UI spells	1.377	1.342	1.393	1.683	1.539	2.028
Contributory years	8.814	11.893				

Note: We would like to know what kind of worker is drawn into unemployment by greater benefits. In this table, we compare the demographic profile of those induced to unemployment by more generous benefits (the *marginal* unemployed) to those that would have claimed even without more generous benefits (the *inframarginal* unemployed). The values in the inframarginal columns are recovered by estimating a regression discontinuity model where the outcome is the demographics of claimants and the running variable is that relevant for each generosity threshold. The inframarginal value is the constant on the stingy side of the cutoff. The values in the marginal columns are calculated. They are what the mean demographics of the marginals must be to rationalize how demographics change at the threshold, using equation (4).

Table 5 presents our estimates of marginal claimant characteristics. When PBD increases, the marginal claimants differ from the inframarginal group in several ways. First, they are notably older—the mean age of marginal claimants is 35.5 years compared to 32.1 years for inframarginal claimants. Second, they have substantially more labor market experience, with 11.9 years of contributory time versus 8.8 years for inframarginal claimants. Third, they are much more likely to be female—66.3 percent of marginal claimants are women compared to 45.8 percent of inframarginal claimants. However, marginal and inframarginal claimants show similar patterns in their prior unemployment history, with comparable numbers of previous UI spells.

The demographic profile of marginal claimants responding to higher benefit levels shows both similarities and striking differences compared to those responding to longer benefit duration. While marginal claimants are again older than the inframarginal group, the gender pattern reverses—these marginal claimants are slightly less likely to be women. Moreover, they differ from inframarginal claimants along two additional dimensions: marginal claimants from higher benefit levels have lower levels of education and have substantially more prior experience with unemployment insurance (prior unemployment experience could increase the knowledge of workers that their contributory years determine benefit levels). This distinct pattern of selection suggests that different policy instruments—benefit levels versus benefit duration—draw different types of workers into unemployment.

6.4 Robustness

Our main findings remain stable across a range of robustness checks.[25] We examine three key dimensions of robustness.

First, we test sensitivity to bandwidth selection. Appendix Figure A.14 compares our preferred estimates, which use optimal bandwidths from Calonico et al. (2020), to those using alternative bandwidths. The estimates remain very stable overall, with smaller bandwidths yielding slightly larger effects in the BL estimation in 12-month PBD counties (panels e and f).

Second, we find robustness to specification choices. Our results remain stable when using quadratic rather than linear specifications of the running variable (Appendix Figure A.15) and when excluding observations near the threshold to account for potential manipulation or measurement error ("donut RD", Appendix Figure A.16). Similar patterns emerge for our inflow estimates across different bandwidths (Appendix Figure A.17), polynomials (Appendix Figure A.18), and donut specifications (Appendix Figure A.19).[26]

Finally, we examine whether our results extend beyond our main sample of laid-off workers to include those who quit. While quitters face a three-month waiting period and shorter benefit durations (3 or 9 months versus 6 or 12 months), the difference in PBD remains six months. Appendix Tables B.4–B.7 show that including quitters (15 percent of potential claimants) leaves

[25] As established in the previous subsection, our estimates are insensitive to adding control variables, including unemployment history, while county and year fixed effects only improve precision (Appendix Figure A.13).

[26] For inflow estimates, very narrow bandwidths or large donut holes yield imprecise estimates. We omit these imprecise estimates from the figures for clarity but they are available upon request.

our estimates virtually unchanged. Moreover, focusing only on quitters yields similar effects on both unemployment duration and inflows.

7 Welfare Effects

In this section, we calculate the welfare effects of redistributing \$1 to UI claimants through either an increase in the potential benefit duration or an increase in the benefit level. As an easily interpretable measure of societal benefit from the \$1 spent by the government we focus on the marginal value of public funds (MVPF, see Finkelstein and Hendren, 2020; Hahn et al., 2024; Hendren and Sprung-Keyser, 2020). The MVPF is given by

$$MVPF^{\$1} = WTP/(1 + FE), \tag{5}$$

where WTP is the social willingness to pay for the redistribution of \$1 to benefit claimants and FE is the fiscal externality of redistributing \$1, i.e., the net effect of behavioral reactions on the government budget. The fiscal externality equals the behavioral cost of additional redistribution divided through the mechanical cost, an intuitive measure of the disincentive cost of UI (the BC/MC ratio, see Schmieder and von Wachter, 2017). We first calculate the BC/MC ratio (or fiscal externality) and use these estimates to calculate the MVPF as in equation (5). The social willingness to pay is the sum of the the mechanical transfer and the social value of additional insurance, $WTP = (1 + \text{social value})$, whereby the social value depends on risk aversion and consumption losses upon becoming unemployed. The fiscal externality can be calculated from two ingredients. 1) The changes in the stocks of workers, benefit claimants, and exhaustees due to behavioral reactions. Our empirical estimates of labor market effects of UI changes are sufficient statistics for these changes in stocks. 2) The ratio of taxes paid per worker and transfers received per benefit claimants, which can be calculated using publicly available information on the tax-transfer system.

7.1 The Model

To evaluate the welfare effects of unemployment insurance, we develop a continuous-time model of the labor market that incorporates two key features: workers' endogenous separations and their choice of search intensity while unemployed. The economy consists of a unit mass of workers who can be in one of three states: employed, unemployed with UI benefits, or unemployed without benefits. The number of employed workers is e, that of benefit claimants is u_b, and the number of unemployed without benefits is $u_x = u - u_b$, where u is the number of all unemployed. The unemployed receive UI benefits until exhaustion, after which they transition to lower social assistance payments. Workers exit the labor force at the end of their working life and are replaced by new entrants, maintaining constant population over time. When the value of unemployment exceeds that of employment, workers separate from their job. These separations are efficient, i.e. the net surplus of destroyed job matches are negative.

Within this framework, we analyze how social welfare responds to changes in both benefit levels (BL) and potential benefit duration (PBD).

7.1.1 The Individual's Problem

Building on the frameworks of Chetty (2008) and Schmieder et al. (2012), we formulate the worker's optimization problem recursively. Our key contribution is to endogenise job separations by allowing employed workers to become unemployed in response to UI generosity. Workers can smooth consumption through saving and borrowing, maintaining asset holdings A_t at time t that must remain above a lower bound L_t.[27] While workers can accumulate debt during their careers, they must fully repay any obligations by the terminal period ($A_T \leq 0$). For simplicity, we abstract from discounting.

The employed worker's value function is

$$V_t(A_t, \eta_t) = \max_{A_{t+1}} \left(\nu(A_t - A_{t-1} + w - \tau) - \eta_t + \mathop{\mathbb{E}}_{t} max\{V_{t+1}(A_{t+1}, \eta_{t+1}), U_{t+1}(A_{t+1})\} \right), \quad (6)$$

The value function captures two key components of worker welfare. First, $\nu(c_t^e)$, represents the flow utility from consumption during employment, where the consumption level equals the net change in assets plus the wage w earned and the tax τ on labor income. Utility is increasing and concave in consumption. Second, η_t represents the match-specific disutility of work, which varies independently across jobs, workers, and over time. Workers face uncertainty from two sources: their future disutility of work and their future employment status.

Workers endogenously separate from their jobs when the value of unemployment exceeds the value of continued employment. This can happen through two distinct channels: either the worker experiences an unusually high disutility of work (η_t), or changes in UI generosity raise the value of unemployment relative to employment.[28] The aggregate consequence of these individual separation decisions is captured by δ, the economy-wide job destruction rate, which measures the average probability that an employed worker transitions to unemployment.

When unemployed, workers choose both how much to consume and how intensively to search for jobs. Their value function is:

$$U_t(A_t) = \max_{A_{t+1}} \left(u(A_t - A_{t-1} + \mathcal{B} + \mathcal{A}) + J_{t+1}(A_{t+1}) \right), \quad (7)$$

where $u(c_t^u)$ represents flow utility from consumption during unemployment, with $u(\cdot)$ increasing and concave. The consumption level equals the net change in assets plus UI benefits $\mathcal{B}$ and social assistance $\mathcal{A}$. Workers make their search decisions according to:

$$J_t(A_t) = \max_{s_t} \left(s_t \mathop{\mathbb{E}}_{t} \max\{V_t(A_t), U_t(A_t)\} + (1 - s_t)U_t(A_t) - \psi(s_t) \right). \quad (8)$$

[27] An alternative approach to modeling self-insurance is through household production, as in Landais et al. (2018b).

[28] Alternatively, one could model separations as arising from productivity shocks, as in Mortensen and Pissarides (1994).

By searching more intensively (higher s_t), workers increase their chances of receiving a job offer, but incur greater search costs ($\psi(s_t)$) that rise at an increasing rate. The probability of transitioning to employment combines two terms: the probability of receiving an offer (s_t) and the probability of accepting it ($Pr(V_t(A_t) > U_t(A_t))$). We denote by f the average rate at which unemployed workers find jobs in the economy. We assume that the job finding rate per unit of job search effort is independent of aggregate job search.[29]

We can now work out the effects of changes of the unemployment insurance system on the transitions rates f and δ. Search intensity is implicitly defined by the difference in the value functions in employment and in unemployment, $\psi'(s) = V(A_t) - U(A_t)$. A raise in the benefit level $\mathcal{B}$ or the duration of benefit receipt increases the value function of the unemployed. This leads to a decrease in search intensity and to a decrease in the probability of accepting a job offer. Hence, it lowers the job finding rate f.

Workers separate from their jobs when $U(A_t) > V(A_t)$, i.e. when the disutility of work η_t is sufficiently high. Since a rise in UI generosity increases the value of unemployment, it also raises the separation probability.

7.1.2 The Steady State in the Labor Market

When transition rates naturally stabilize, the economy reaches a steady state where the unemployment rate converges to:

$$u = \frac{\delta}{\delta + f}, \tag{9}$$

The fraction of workers receiving UI benefits (u_b) also reaches a steady state when inflows equal outflows. Workers stop receiving benefits either because they find employment or because they exhaust their benefits, giving a total exit rate of f_b. In the steady state, $f_b u_b = \delta \times e$, which implies:

$$u_b = \frac{\delta \times e}{f_b} = \frac{\delta \times f}{f_b(f + \delta)}. \tag{10}$$

7.1.3 The Social Planner's Problem

The social planner maximizes steady-state welfare by choosing both UI benefit levels (b) and the potential benefit duration (P) to balance gains from consumption smoothing against moral hazard costs. The planner accounts for the welfare of three groups:

$$W = u_b \nu(c_b) + (u - u_b)\nu(c_x) + ev(c_e) - u\overline{\psi(s)}, \tag{11}$$

where c_b, c_x, and c_e represent consumption levels of UI recipients, benefit exhaustees, and employed workers. The last term in equation (11) captures the total search costs that unem-

[29]This arises in a model where firms' production function is linear and the wage is fixed as in Hall (2005).

ployed workers incur with search intensity s. $\overline{\psi(s)}$ indicates the average search cost among the unemployed.

To finance benefits, the government must satisfy its budget constraint:

$$\mathcal{G} + u_b\mathcal{B} + u_x\mathcal{A} = e\mathcal{T}, \tag{12}$$

In the budget constraint, $\mathcal{G}$ captures exogenous government spending, $\mathcal{B}$ represents UI benefit payments, $\mathcal{A}$ denotes social assistance to benefit exhaustees with $\mathcal{B} > \mathcal{A}$, and $\mathcal{T}$ is the taxes paid by employed workers to finance transfers. If we treat social assistance as a lump-sum transfer, we can define three simple terms $\tau = \mathcal{T} + \mathcal{A}$ (total tax liability), $b = \mathcal{B} - \mathcal{A}$ (UI premium over social assistance), and $G = \mathcal{A} + \mathcal{G}$ (total base government spending). Since the population adds to one ($1 = e + u_x + u_b$), the budget constraint simplifies to:

$$G + bu_b = e\tau. \tag{13}$$

Previous work by Chetty (2006), Chetty (2008), and Schmieder and von Wachter (2016) analyze UI through the lens of an individual worker who becomes unemployed and finances their own benefits through future taxes. We extend these canonical models by examining steady-state stocks in an economy where workers endogenously separate from jobs. When unemployment inflows remain fixed, our model nests these earlier approaches as a special case, see Appendix D.3.

7.1.4 The Welfare Effects of UI Changes

We now examine how marginal changes in UI generosity affect welfare, focusing on both benefit levels and potential benefit duration. We measure welfare changes in money-metric terms: how much welfare improves when we redistribute \$1 from employed to unemployed workers by raising benefits and funding them through higher taxes. The full derivation of the equations for the effects of marginal changes in UI on welfare can be found in Appendix D.

Both job separation and job finding rates adjust endogenously when we change UI policy parameters—either the potential benefit duration (P) or benefit level (b). To calculate how increasing benefits affects welfare, we differentiate the social welfare function (equation (11)) and budget constraint (equation (13)) with respect to b. This yields:

$$\frac{dW}{db}\frac{1}{u_b v'(c_e)} = \underbrace{\frac{\nu'(c_b) - v'(c_e)}{v'(c_e)}}_{\text{Social value of \$1 add. transfer}} - \underbrace{\left(b\frac{du_b}{db} - \frac{de}{db}\tau\right)\frac{1}{u_b}}_{\text{Behavioral cost per \$1 add. transfer}}. \tag{14}$$

To derive this expression, we apply the envelope theorem and assume separations occur efficiently—meaning worker-firm matches end only when their joint surplus becomes negative. If separations are inefficient, as is the case when firms make a surplus and wages are rigid, welfare costs are generally larger than in our model. Consider a match where both employer and employee earn a positive surplus. If UI becomes more generous, it may drive the worker's surplus negative while the total match surplus remains positive. While raising the wage could preserve such matches in principle, rigid wages prevent this adjustment such that worker and firm separate. These inefficient separations create larger welfare costs than our model predicts by imposing an additional externality on firms.[30]

When separations are efficient, behavioral responses affect welfare only through their fiscal impact on the government's budget. The welfare calculation thus reduces to a simple trade-off: higher benefits improve welfare if and only if the social value of redistribution exceeds its behavioral cost. This social value measures how much unemployed workers value an extra $ compared to employed workers. Since utility increases at a decreasing rate with consumption, this value remains strictly positive whenever benefits fall below after-tax wages ($b < w - \tau$) and other means of insurance besides the welfare state do not offer perfect insurance to workers.

Behavioral costs arise through two channels. First, workers receive benefits longer when benefits increase. Second, benefits lead to higher unemployment which shrinks the tax base as fewer people work. Following Schmieder and von Wachter (2017), we measure these costs using the BC/MC ratio—behavioral cost divided by the mechanical cost of transferring $1 to benefit recipients. We then use the BC/MC ratio and the calculated social value of $1 additional transfer to calculate the marginal value of public funds (MVPF) popularized by Finkelstein and Hendren (2020) and Hendren and Sprung-Keyser (2020), see equation (5).

To calculate these welfare effects, we express behavioral costs using empirical elasticities obtained from from our natural experiments. We begin by converting flow rates to durations: $f = 1/D$ and $f_b = 1/D_b$, where D measures how long unemployment spells last on average and D_b measures how long workers collect benefits. These relationships hold even when job-finding rates change over the unemployment spell, provided separations rates stay relatively stable (see Appendix D.2).

To capture how benefit receipt (u_b) and employment (e) respond to policy changes, we use three key elasticities: (1) $\eta_{D,b} = \frac{dD}{d_b}\frac{b}{D}$, which measures how unemployment duration responds to benefits; (2) $\eta_{D_b,b} = \frac{dD_b}{d_b}\frac{b}{D_b}$, which measures how benefit duration responds to benefits; and (3) $\eta_{\delta,b} = \frac{d\delta}{b}\frac{b}{\delta}$, measuring how separation rates respond to benefits, which we empirically approximate through the estimated change in log inflows.[31]

These elasticities let us express the behavioral-cost to mechanical-cost ratio as:

$$BC/MC^b = e\frac{\tau}{b}\frac{D}{D_b}(\eta_{\delta,b} + \eta_{D,b}) + \eta_{\delta,b} + \eta_{D_b,b} - \eta_{D,b}u_b. \tag{15}$$

Behavioral responses to benefits affect program costs through three distinct channels. First, when benefits become more generous, tax revenues fall because workers separate more often ($\eta_{\delta,b}$) and remain unemployed longer ($\eta_{D,b}$). Second, the government also pays out more in benefits because of higher separation rates ($\eta_{\delta,b}$) and longer durations ($\eta_{D_b,b}$). Third, and at first glance counterintuitively, longer unemployment spells ($\eta_{D,b}$) reduce the number of benefit recipients by

[30] Jäger et al. (2023) document evidence of such inefficient separations in Austria, consistent with wage rigidity.

[31] These elasticities capture both the direct effect of higher benefits and the indirect effect of financing them through higher taxes (τ). Our empirical estimates, which implicitly hold taxes fixed, slightly understate the theoretically relevant elasticities. However, since benefits require only small tax increases to finance them, this difference matters little. Chetty (2008) confirms this in numerical simulations (see footnote 32).

lowering employment. With fewer employed workers but the same separation rate, fewer workers start new benefit spells. In Appendix D.3 we derive that when we consider the special case of fixed inflows into unemployment, equation (15) equals the expression for the BC/MC ratio derived in Schmieder and von Wachter (2016). In that equation, e, $\eta_{\delta,b}$, and $\eta_{D,b}u_b$ drop out.

We now analyze how extending potential benefit duration (PBD) affects welfare. When we transfer \$1 to the unemployed by marginally increasing P, welfare changes by:

$$\frac{dW}{dP}\frac{1}{\left.\frac{du_b}{dP}\right|_M bv'(c_e)} = \underbrace{\frac{\nu'(c_x) - v'(c_e)}{v'(c_e)}}_{\text{Social value of \$1 add. transfer}} - \underbrace{\frac{1}{\left.\frac{du_b}{dP}\right|_M}\left(\left.\frac{du_b}{dP}\right|_B - \frac{de}{dP}\frac{\tau}{b}\right)}_{\text{Behavioral cost per \$1 add. transfer}}, \tag{16}$$

We decompose the changes in benefit receipt into two parts. The mechanical effect ($\left.\frac{du_b}{dP}\right|_M$) captures how many more workers receive benefits simply because benefits last longer, even holding constant the probability of remaining unemployed. The behavioral effect ($\left.\frac{du_b}{dP}\right|_B$) captures how workers adjust their job search when benefits last longer.

Longer potential benefit duration affects welfare much like higher benefit levels, but differs in one key way: who receives the additional transfers. When we extend PBD, the additional spending $\left.\frac{du_b}{dP}\right|_M b$ goes to workers who would otherwise have exhausted their benefits. Since these exhaustees typically consume less than other unemployed workers (as captured by $\nu'(c_x)$), the social value of transfers in equation (16) exceeds the value of raising benefit levels in equation (14).

Using observable durations and responses, we write the behavioral-cost to mechanical-cost ratio as:

$$BC/MC^P = \frac{1}{\left.\frac{dD_b}{dP}\right|_M}\left(e\frac{\tau}{b}\left(\eta_{\delta,P}\frac{D}{P} + \frac{dD}{dP}\right) + \eta_{\delta,P}\frac{D_b}{P}e + \left.\frac{dD_b}{dP}\right|_B - \frac{dD}{dP}u_b\right), \tag{17}$$

Equation (17) is very similar in structure to equation (15). When we extend PBD, benefit duration changes through two channels: $\left.\frac{dD_b}{dP}\right|_M$ measures the direct effect of longer available benefits, while $\left.\frac{dD_b}{dP}\right|_B$ measures how workers adjust their behavior. For small PBD changes, the mechanical effect ($\left.\frac{dD_b}{dP}\right|_M$) simply equals the share of recipients who exhaust their benefits, since only these workers automatically receive more benefits from extended PBD (Schmieder et al., 2012).

7.2 Parametrization

To quantify how UI changes affect welfare, we combine three types of parameters. First, we estimate empirically how benefit levels and PBD affect unemployment duration and inflows. Since these elasticities vary with UI regimes, we compute welfare effects separately for counties with 6- and 12-month PBD. Second, we draw descriptive statistics from our administrative data.

Third, we measure the tax-transfer system using OECD data on average-wage workers. Though the OECD reports tax rates and replacement rates that could yield $\mathcal{T}$ and $\mathcal{B}$ separately, we instead compute their ratio τ/b directly:

$$\frac{\tau}{b} = \frac{\mathrm{tr} + (1-\mathrm{tr}) \times \mathrm{rr}_{long}}{(\mathrm{rr}_{short} - \mathrm{rr}_{long}) \times (1-\mathrm{tr})} = \frac{.35 + (1-.35) \times .21}{(.41 - .21) \times (1-.35)} = 3.72, \tag{18}$$

Here, tr measures the average tax rate, while rr_{short} and rr_{long} measure replacement rates for two groups: the short-term unemployed (who receive UI benefits) and the long-term unemployed (who receive only social assistance). In Poland, UI recipients typically also receive social assistance, so rr_{short} includes both payment types. When multiplied by average gross earnings, equation (18) yields total tax liability ($\mathcal{T}+\mathcal{A}=\tau$), while the denominator yields the UI premium over social assistance ($\mathcal{B}-\mathcal{A}=b$).

The importance of accounting for social assistance payments becomes clear when we consider the alternative calculation. If we ignored social assistance ($\mathcal{A}$), we would compute τ/b simply as $\frac{tr}{(1-tr)\mathrm{rr}_{short}} = \frac{.35}{.41\times(1-.35)} = 1.31$. This approach would understate how much increasing UI generosity costs taxpayers by ignoring the transfers that all unemployed workers receive, regardless of UI status.

Table 6 shows all parameters we use to calculate welfare effects, combining our tax-transfer system parameters with our estimated behavioral responses.

Table 6: Parameters for BC/MC of increases in BL or PBD

	e	u_b	$\left.\frac{dD_b}{dP}\right\rvert_M$	$\left.\frac{dD_b}{dP}\right\rvert_B$	D	D_b	δ	$\eta_{D,b}$	$\eta_{D_b,b}$	dD/dP	$\eta_{\delta,b}$	$\eta_{\delta,P}$	τ/b
PBD extension	.9	.05	.62	.02	11.9	.	.01	.	.	.41	.	.12	3.72
BL increase at PBD=6	.92	.04	.	.	9.41	4.54	.	.25	.22	.	1.66	.	3.72
BL increase at PBD=12	.89	.07	.	.	12.66	7.92	.	.33	.31	.	1.28	.	3.72

Notes: Parameters used to calibrate the impact on welfare of marginal increases in UI generosity. Own calculations based on our sample and statistics provided by the OECD. Appendix Table B.8 provides a brief description of each parameter.

7.3 Results

We calculate the behavioral distortion of increases in potential benefit duration and benefit levels quantified as the BC/MC ratio and express welfare effects as MVPFs. These welfare effects require measuring how much unemployed workers value additional benefits compared to how much employed workers dislike tax increases. This valuation depends on consumption differences between groups, shaped by UI replacement rates and workers' access to other insurance through savings or family support. We calculate welfare effects for different constant relative risk aversion

(CRRA) coefficients[32] and consumption drops. With CRRA utility, the social value of a \$1 transfer is $\left(\frac{c_u}{c_e}\right)^{-\gamma} - 1$, where γ measures risk aversion.[33]

Since consumption drops during unemployment have not been quantified in Poland, we establish the MVPF for different consumptions drops and calculate what drop would make small benefit changes welfare-neutral (where social value equals behavioral cost and the MVPF equals one). Evidence from the U.S., which has similar replacement rates to Poland, shows about a 10 percent consumption drop at UI exhaustion, providing a useful benchmark.

Table 7 shows welfare calculations for PBD extensions. When we assume fixed inflows, the BC/MC ratio is 2.48 (column 1), in line but somewhat higher than the 1.78 median found in Schmieder and von Wachter (2016). Columns (2)–(7) show the MVPF for various assumptions. Most MVPFs range from 0.32 to 0.59—below many other social programs reported in Hendren and Sprung-Keyser (2020) but in line with MVPFs of European UI policies summarized in Le Barbanchon et al. (2024). Only extreme cases (30 percent consumption loss and CRRA of 5) yield higher MVPFs.

Table 7: BC/MC and MVPF of increases of potential benefit durations (PBD)

	BC/MC	MVPF					
CRRA coefficient:		1	2	5	1	2	5
Consumption loss		10%	10%	10%	30%	30%	30%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fixed inflows	2.48	.32	.36	.49	.41	.59	1.71
Endogenous inflows	3.64	.24	.27	.37	.31	.44	1.28

Notes: BC/MC ratios and MVPFs from extending potential benefit duration, comparing cases where workers' separation decisions are either fixed or respond to UI generosity. MVPFs are computed for different coefficients of relative risk aversion (CRRA) and consumption losses during unemployment. Authors' calculations.

Allowing for endogenous separations, the BC/MC ratio—calculated using the parameters of Table 6—reads as follows:[34]

[32]The social value of additional insurance has been found to be heavily influenced by the CRRA parameter (Le Barbanchon et al., 2024).

[33]For small consumption losses and low risk aversion, $\gamma\frac{c_u - c_e}{c_e}$ approximates this value well (see Baily, 1978).

[34]In Table 6 and equation (19)—see also Figure 6—it becomes apparent that the behavioral component of the PBD extension on benefit duration is quite small relative to the mechanical component (0.02 vs. 0.62). The exhaustion rate (the mechanical effect) is large in Poland and the social value of PBD extensions increases with the exhaustion rate (Schmieder et al., 2012).

$$\frac{BC}{MC}^P = \frac{1}{\underbrace{\left.\frac{dD_b}{dP}\right|_M}_{1.62}} \left(\underbrace{\eta_{\delta,P}}_{.12} \underbrace{\frac{D_b}{P}}_{.92} \underbrace{e}_{.9} + \underbrace{\left.\frac{dD_b}{dP}\right|_B}_{.02} - \underbrace{\frac{dD}{dP}}_{.41} \underbrace{u_b}_{.05} + \underbrace{e}_{.9} \underbrace{\frac{\tau}{b}}_{3.72} \left(\underbrace{\eta_{\delta,P}}_{.12} \underbrace{\frac{D}{P}}_{1.98} + \underbrace{\frac{dD}{dP}}_{.41} \right) \right) = 3.64 \tag{19}$$

Endogenous separations increase the behavioral cost increases by 46 percent to \$3.64 and reduce all MVPFs by around 25 percent.

Appendix Figure A.20 shows how the MVPF varies under different assumptions of CRRA coefficients and consumption losses. The MVPF exceeds one only when workers are highly risk averse or lose substantial consumption at benefit exhaustion. With a CRRA coefficient of 3, workers must lose 35 percent of consumption with fixed inflows, or 40 percent with endogenous inflows, for the MVPF to exceed one.

Appendix Table B.9, Panel A, reports what consumption losses make PBD extensions welfare-neutral at different CRRA values. With a CRRA coefficient of 2, consumption drops at exhaustion must exceed 50 percent. Even with highly risk-averse workers (CRRA of 5), consumption must drop by 26.4 percent. While evidence on consumption at exhaustion is scarce, Ganong and Noel (2019) find US consumer spending falls by only 12 percent when benefits end. If Polish workers lose similar amounts of consumption at exhaustion, our model suggests shorter PBD would increase welfare.

Table 8 compares BC/MC ratios and MVPFs for benefit level increases, separately for counties with 6-month PBD (Panel A) and 12-month PBD (Panel B). Using a canonical Baily-Chetty-type formula with fixed inflows, the BC/MC ratio equals around 2.2 regardless of PBD. This means transferring \$1 to the unemployed requires raising \$3.2 in taxes: \$1 for the transfer itself plus \$2.2 for behavioral costs from reduced employment. While these behavioral costs exceed the \$1.3 average found in Schmieder et al. (2016), they align with Card et al. (2015b), who find behavioral costs of \$2.8–\$5.6 per \$ transferred, similar to U.S. evidence from Card et al. (2015a).

With endogenous inflows and using the parameters from Table 6, benefit level increases with a 6-month PBD (and equivalently for 12 months) yield:

$$\frac{BC}{MC}^b_{PBD=6} = \underbrace{\eta_{\delta,b}}_{1.66} + \underbrace{\eta_{D_b,b}}_{.22} - \underbrace{\eta_{D,b}}_{.25} \underbrace{u_b}_{.04} + \underbrace{e}_{.92} \underbrace{\frac{\tau}{b}}_{3.72} \underbrace{\frac{D}{D_b}}_{2.08} \left(\underbrace{\eta_{\delta,b}}_{1.66} + \underbrace{\eta_{D,b}}_{.25} \right) = 15.44 \tag{20}$$

Incorporating endogenous separations dramatically changes the welfare calculations. The BC/MC ratio increases four- to seven-fold (in 6- and 12-month PBD counties, respectively) and the MVPFs similarly drop dramatically. These stark differences persist even under generous assumptions about risk aversion and consumption losses. With a high coefficient of relative risk aversion (CRRA of 5) and substantial assumed consumption losses (30 percent), the MVPF

Table 8: BC/MC and MVPF of increases of benefit levels (BL)

	BC/MC	MVPF					
CRRA coefficient:		1	2	5	1	2	5
Consumption loss		10%	10%	10%	30%	30%	30%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 6 months PBD							
Fixed inflows	2.15	.35	.39	.54	.45	.65	1.89
Endogenous inflows	15.44	.07	.08	.1	.09	.12	.36
Panel B: 12 months PBD							
Fixed inflows	2.3	.34	.37	.51	.43	.62	1.81
Endogenous inflows	10.17	.1	.11	.15	.13	.18	.53

Notes: This table shows BC/MC ratios and MVPFs from raising UI benefit levels, comparing cases where workers' separation decisions are either fixed or respond to UI generosity. MVPFs are computed for different coefficients of relative risk aversion (CRRA) and consumption losses during unemployment. Results reported separately for regions with 6- and 12-month PBD. Authors' calculations.

is 0.53 (identical to the average MVPF of seven estimates for benefit level extensions in EU countries reported in Le Barbanchon et al., 2024).

The strong differences in the welfare calculations with fixed or endogenous inflows highlight that the majority of these costs arise from employed workers becoming unemployed more frequently, rather than from longer unemployment durations. The findings for benefit level increases stand in contrast to those from extending PBD which increased behavioral costs much less with endogenous inflows. This difference arises in part because PBD extensions have smaller effects on employed workers' separation decisions, though they still substantially increase unemployment durations. These findings suggest that policymakers face fewer negative trade-offs when increasing benefit duration rather than benefit levels.

As for the PBD extensions, Appendix Figure A.21 shows MVPF of benefit level increases across CRRA coefficients and consumption losses, comparing fixed and endogenous inflows. When workers can respond to UI generosity by separating more often, MVPF remains below 1 even with a CRRA coefficient of 3.

Appendix Table B.9 reports what consumption losses would make benefit increases welfare-neutral (satisfying the Baily-Chetty condition). With endogenous separations, these required losses range from 38 percent (CRRA coefficient of 5, 12-month PBD) to 94 percent (CRRA coefficient 1, 6-month PBD)—far exceeding the consumption losses that Schmieder and von Wachter (2016) document in their survey. Clearly, accounting for how workers adjust their separations in response to UI dramatically changes how we evaluate unemployment transfers.

Table 9 summarizes how UI generosity affects labor market outcomes and program costs. Panel A shows the behavioral responses to a 10 percent increase in either benefit duration or benefit level. These estimates reveal a striking asymmetry: while both policies similarly increase unemployment duration (2.5–3.4 percent), they generate markedly different effects on

Table 9: Effects of an increase in UI generosity

Variation	PBD	Benefit level	
Sample:		6 Months PBD	12 Months PBD
	(1)	(2)	(3)
Panel A: Effects of a 10 percent increase in UI generosity			
Benefit duration	6.19%	2.22%	3.07%
Unemployment duration	2.89%	2.50%	3.35%
Inflows	1.19%	16.64%	12.85%
Panel B: Behavioral cost of $1 redistribution			
Fixed inflows			
Benefit payments	0.02	0.22	0.31
Tax revenues	2.44	1.93	1.99
BC/MC	2.46	2.15	2.3
Endogenous inflows			
Benefit payments	0.15	1.88	1.57
Tax revenues	3.48	13.56	8.6
BC/MC	3.64	15.44	10.17

Notes: Panel A shows how a 10 percent increase in UI generosity affects benefit duration, unemployment duration, and worker inflows to unemployment. Panel B decomposes the behavioral cost per $ of redistribution into changes in benefit payments and tax revenues, comparing cases where workers' separation decisions are either fixed or respond to UI generosity. Source: Authors' calculations using Polish administrative data.

unemployment inflows. A 10 percent increase in benefit levels leads to a 12.9–16.6 percent rise in unemployment inflows, compared to just a 1.2 percent increase from longer potential benefit duration.

Building on these behavioral responses, Panel B quantifies how these behavioral responses affect program costs, decomposing the total behavioral cost into changes in benefit payments and lost tax revenue. For all policies, the effect on tax revenues is much more important than the behavioral effect on transfer payments. As discussed above, for PBD increases, the behavioral effect on transfer payments is particularly small relative to the mechanical effect. With fixed inflows, the BC/MC ratios are quite similar across policies, ranging from 2.2 to 2.5. However, once we account for endogenous separations, the costs of benefit level increases rise dramatically. The BC/MC ratio for benefit level increases jumps to 10.2–15.4, driven primarily by substantial losses in tax revenue as more workers transition into unemployment. In contrast, the BC/MC ratio for extended benefit duration rises more modestly to 3.6, reflecting the smaller impact on separation decisions. This difference in fiscal costs suggests that policymakers face a lower distortionary cost when extending potential benefit duration rather than increasing benefit levels.

8 Conclusion

In this paper, we examine the implications of unemployment insurance (UI) on labor markets by exploiting a unique institutional setting. In Poland, benefit duration and generosity are quasirandomly assigned with sharp cutoffs and the two discontinuities intersect. That intersection allows us to estimate how the effects of benefit generosity and benefit duration interact to distort labor supply.

We estimate duration elasticities with respect to benefit generosity and benefit duration, and those estimates are in the range of prior work. Importantly, we also find significant moral hazard among the employed, where employed workers that are eligible for greater benefits are much more likely to become unemployed. Workers that enter unemployment because of more generous or longer lasting benefits tend to be slightly older, more female, and less educated than those that become unemployed under less generous benefit regimes.

We also find that the moral hazard from benefit duration and benefit levels interact: the elasticity of benefit and unemployment durations with respect to higher benefits is more than 80 percent larger in the presence of (randomly-assigned) longer benefit durations. Because both the labor supply distortion and the costs of insurance grow with an increasing PBD and BL, the interaction suggests temperance in policy design.

We incorporate these findings into an extended Baily-Chetty model of optimal benefits, considering the social welfare implications of UI in the presence of endogenous inflows into unemployment (where the baseline Baily-Chetty model assumes that layoffs are exogenous) and the moral hazard interactions of benefit generosity and benefit duration. This model weighs the benefits of consumption smoothing against the costs of moral hazard. We conclude that including the effects of moral hazard among the employed significantly increases the understood fiscal costs of UI, in particular of increases in the benefit level.

References

Acemoglu, D. and R. Shimer (1999): "Efficient unemployment insurance," *Journal of Political Economy*, 107, 893–928.

Ahammer, A., M. Fahn, and F. Stiftinger (2024): "Outside Options and Worker Motivation," *IZA DP No. 16333.*

Albanese, A., M. Picchio, and C. Ghirelli (2020): "Timed to say goodbye: does unemployment benefit eligibility affect worker layoffs?" *Labour Economics*, 65, 101846.

Baily, M. N. (1978): "Some aspects of optimal unemployment insurance," *Journal of Public Economics*, 10, 379–402.

Bell, A., T. Hedin, G. Schnorr, and T. von Wachter (2024): "Unemployment Insurance (UI) Benefit Generosity and Labor Supply from 2002 to 2020: Evidence from California UI Records," *Journal of Labor Economics*, 42, S379–S416.

Brébion, C., S. Briole, and L. Khoury (2022): "Unemployment Insurance Eligibility and Employment Duration," in *The 34th EALE Conference 2022.*

Caliendo, M., R. Mahlstedt, A. Schmeisser, and S. Wagner (2024): "The Accuracy of Job Seekers' Wage Expectations," *IZA DP No. 17198*.

Caliendo, M., K. Tatsiramos, and A. Uhlendorff (2013): "Benefit Duration, Unemployment Duration And Job Match Quality: A Regression-Discontinuity Approach," *Journal of Applied Econometrics*, 28, 604–627.

Calonico, S., M. D. Cattaneo, and M. H. Farrell (2020): "Optimal bandwidth choice for robust bias-corrected inference in regression discontinuity designs," *The Econometrics Journal*, 23, 192–210.

Card, D., R. Chetty, and A. Weber (2007a): "Cash-on-hand and competing models of intertemporal behavior: New evidence from the labor market," *The Quarterly Journal of Economics*, 122, 1511–1560.

——— (2007b): "The Spike at Benefit Exhaustion: Leaving the Unemployment System or Starting a New Job?" *American Economic Review*, 97, 113–118.

Card, D., A. Johnston, P. Leung, A. Mas, and Z. Pei (2015a): "The effect of unemployment benefits on the duration of unemployment insurance receipt: New evidence from a regression kink design in Missouri, 2003–2013," *American Economic Review*, 105, 126–130.

Card, D., D. S. Lee, Z. Pei, and A. Weber (2015b): "Inference on causal effects in a generalized regression kink design," *Econometrica*, 83, 2453–2483.

Centeno, M. and Á. A. Novo (2009): "Reemployment wages and UI liquidity effect: a regression discontinuity approach," *Portuguese Economic Journal*, 8, 45–52.

Chetty, R. (2006): "A general formula for the optimal level of social insurance," *Journal of Public Economics*, 90, 1879–1901.

——— (2008): "Moral hazard versus liquidity and optimal unemployment insurance," *Journal of Political Economy*, 116, 173–234.

Christofides, L. N. and C. J. McKenna (1996): "Unemployment insurance and job duration in Canada," *Journal of Labor Economics*, 14, 286–312.

Cohen, J. P. and P. Ganong (2024): "Disemployment Effects of Unemployment Insurance: A Meta-Analysis," *NBER Working Paper 32832*.

Dahl, G. and M. M. Knepper (2022): "Unemployment Insurance, Starting Salaries, and Jobs," Tech. rep., National Bureau of Economic Research.

Dube, A. and A. Lindner (2024): "Minimum wages in the 21st century," *Handbook of Labor Economics*, 5, 261–383.

Ejrnæs, M. and S. Hochguertel (2013): "Is business failure due to lack of effort? Empirical evidence from a large administrative sample," *The Economic Journal*, 123, 791–830.

European Commission (2023): "Poland - Unemployment Benefits," Accessed: 2024-10-16.

Feldstein, M. (1978): "The effect of unemployment insurance on temporary layoff unemployment," *The American Economic Review*, 68, 834–846.

Finkelstein, A. and N. Hendren (2020): "Welfare Analysis Meets Causal Inference," *Journal of Economic Perspectives*, 34, 146–67.

Gałecka-Burdziak, E., M. Góra, J. Jessen, R. Jessen, and J. Kluve (2021): "The effects of shortening potential benefit duration: Evidence from regional cut-offs and a policy reform," *IZA DP 14340*.

Ganong, P. and P. Noel (2019): "Consumer spending during unemployment: Positive and normative implications," *American Economic Review*, 109, 2383–2424.

Gudgeon, M., P. Guzman, J. F. Schmieder, S. Trenkle, and H. Ye (2024): "When Institutions Interact: How the Effects of Unemployment Insurance are Shaped by Retirement Policies," Tech. rep., Mimeo.

Hahn, R. W., N. Hendren, R. D. Metcalfe, and B. Sprung-Keyser (2024): "A welfare analysis of policies impacting climate change," *NBER Working Paper 32728*.

Hall, R. E. (2005): "Employment fluctuations with equilibrium wage stickiness," *American Economic Eeview*, 95, 50–65.

Hartung, B., P. Jung, and M. Kuhn (2024): "Unemployment insurance reforms and labor market dynamics," *mimeo*.

Hendren, N. and B. Sprung-Keyser (2020): "A Unified Welfare Analysis of Government Policies," *The Quarterly Journal of Economics*, 135, 1209–1318.

Imbens, G. W. and T. Lemieux (2008): "Regression discontinuity designs: A guide to practice," *Journal of Econometrics*, 142, 615–635.

Jäger, S., B. Schoefer, and J. Zweimüller (2023): "Marginal jobs and job surplus: a test of the efficiency of separations," *The Review of Economic Studies*, 90, 1265–1303.

Jessen, J., R. Jessen, E. Gałecka-Burdziak, M. Góra, and J. Kluve (2024): "The Micro and Macro Effects of Changes in the Potential Benefit Duration," *Ruhr Economic Papers #1119*.

Johnston, A. C. (2021): "Unemployment Insurance Taxes and Labor Demand: Quasi-experimental Evidence from Administrative Data," *American Economic Journal: Economic Policy*, 13, 266–293.

Johnston, A. C. and A. Mas (2018): "Potential Unemployment Insurance Duration and Labor Supply: The Individual and Market-Level Response to a Benefit Cut," *Journal of Political Economy*, 126, 2480–2522.

Karahan, F., K. Mitman, and B. Moore (2022): "Micro and Macro Effects of UI Policies: Evidence from Missouri," *mimeo.*

Lalive, R. (2007): "Unemployment Benefits, Unemployment Duration, and Post-Unemployment Jobs: A Regression Discontinuity Approach," *American Economic Review*, 97, 108–112.

——— (2008): "How do extended benefits affect unemployment duration? A regression discontinuity approach," *Journal of Econometrics*, 142, 785–806.

Lalive, R., J. van Ours, and J. Zweimüller (2006): "How Changes in Financial Incentives Affect the Duration of Unemployment," *The Review of Economic Studies*, 73, 1009–1038.

Landais, C. (2015a): "Assessing the welfare effects of unemployment benefits using the regression kink design," *American Economic Journal: Economic Policy*, 7, 243–278.

——— (2015b): "Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design," *American Economic Journal: Economic Policy*, 7, 243–278.

Landais, C., P. Michaillat, and E. Saez (2018a): "A Macroeconomic Approach to Optimal Unemployment Insurance: Applications," *American Economic Journal: Economic Policy*, 10, 182–216.

——— (2018b): "A Macroeconomic Approach to Optimal Unemployment Insurance: Theory," *American Economic Journal: Economic Policy*, 10, 152–181.

Le Barbanchon, T., J. Schmieder, and A. Weber (2024): "Job search, unemployment insurance, and active labor market policies," in *Handbook of Labor Economics*, Elsevier, vol. 5, 435–580.

Lee, D. S. and T. Lemieux (2010): "Regression Discontinuity Designs in Economics," *Journal of Economic Literature*, 48, 281–355.

Lester, R. A. and C. V. Kidd (1939): *The Case against Experience Rating in Unemployment Compensation*, Princeton, New Jersey: Princeton University Press.

Lusher, L., G. C. Schnorr, and R. L. Taylor (2022): "Unemployment insurance as a worker indiscipline device? Evidence from scanner data," *American Economic Journal: Applied Economics*, 14, 285–319.

Mortensen, D. T. and C. A. Pissarides (1994): "Job creation and job destruction in the theory of unemployment," *The Review of Economic Studies*, 61, 397–415.

Nekoei, A. and A. Weber (2017): "Does extending unemployment benefits improve job quality?" *American Economic Review*, 107, 527–561.

OECD (2019): "Detailed Description of Employment Protection, 2019: OECD Countries," .

OECD (2023): "Net Replacement Rate in Unemployment," OECD Data Explorer. Accessed: 2024-10-20.

Schmieder, J. F. and T. von Wachter (2016): "The effects of unemployment insurance benefits: New evidence and interpretation," *Annual Review of Economics*, 8, 547–581.

——— (2017): "A context-robust measure of the disincentive cost of unemployment insurance," *American Economic Review*, 107, 343–348.

Schmieder, J. F., T. Von Wachter, and S. Bender (2012): "The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over 20 years," *The Quarterly Journal of Economics*, 127, 701–752.

Schmieder, J. F., T. von Wachter, and S. Bender (2016): "The effect of unemployment benefits and nonemployment durations on wages," *American Economic Review*, 106, 739–777.

Tuit, S. and J. C. van Ours (2010): "How changes in unemployment benefit duration affect the inflow into unemployment," *Economics Letters*, 109, 105–107.

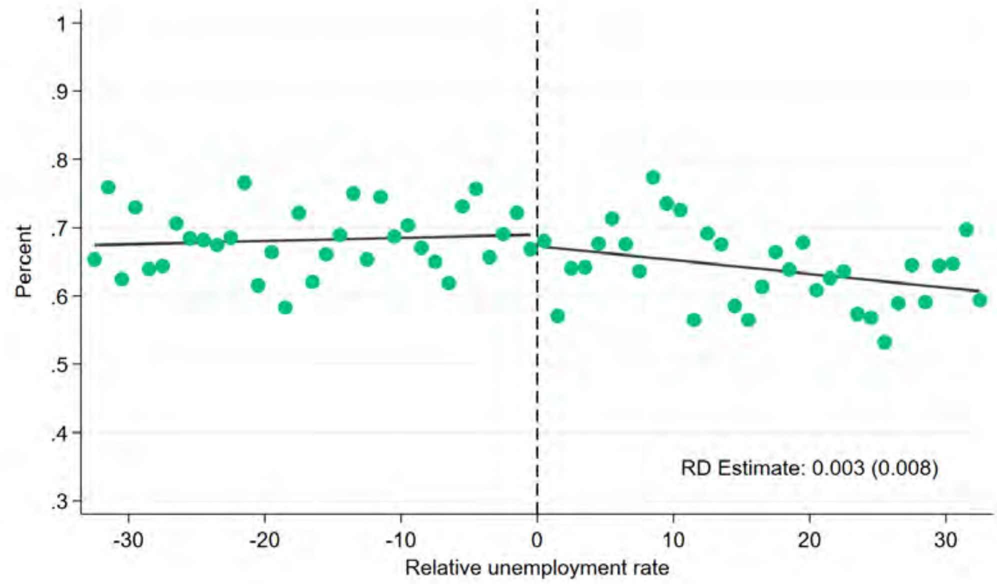
Van Doornik, B., D. Schoenherr, and J. Skrastins (2023): "Strategic formal layoffs: Unemployment insurance and informal labor markets," *American Economic Journal: Applied Economics*, 15, 292–318.

van Ours, J. C. and M. Vodopivec (2008): "Does reducing unemployment insurance generosity reduce job match quality?" *Journal of Public Economics*, 92, 684–695.

APPENDIX (FOR ONLINE PUBLICATION)

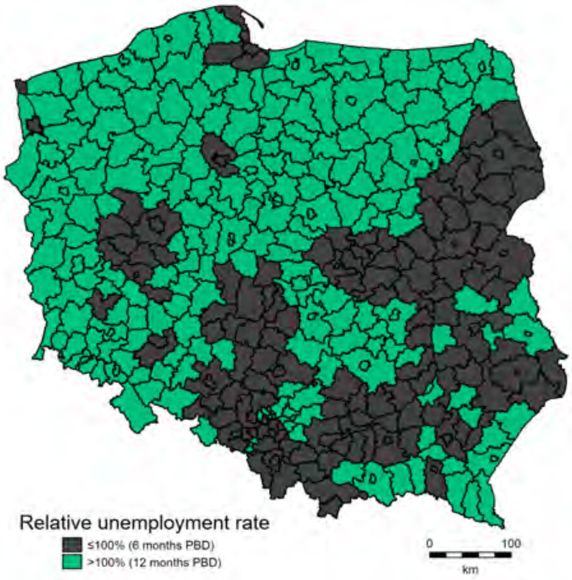
A Figures

Figure A.1: Cross-county moves by year

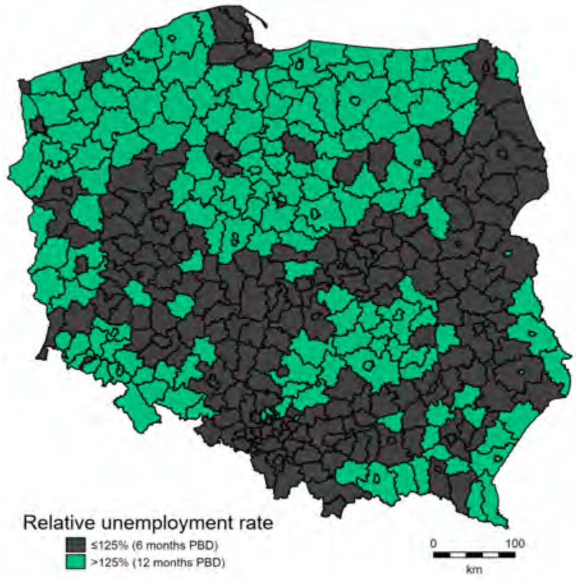


Notes: The figure shows the ratio of new registries for permanent residence to the given county over the end of the previous year population in the given county. Source: Statistics Poland https://bdl.stat.gov.pl/BDL/start, accessed October 26, 2024

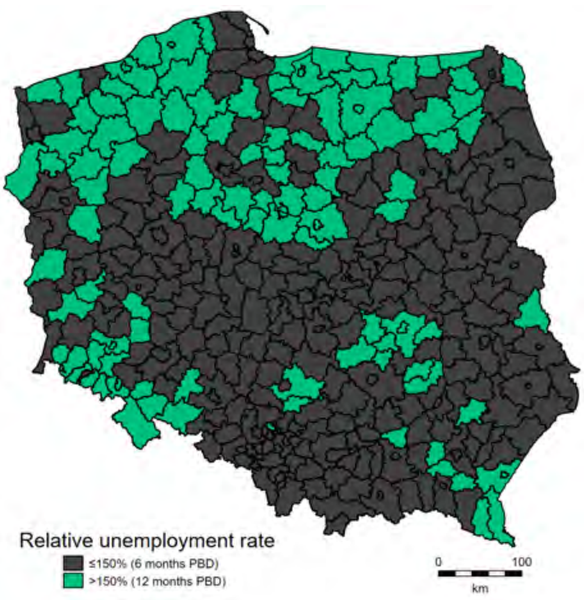
Figure A.2: Potential benefit duration with different cut-offs



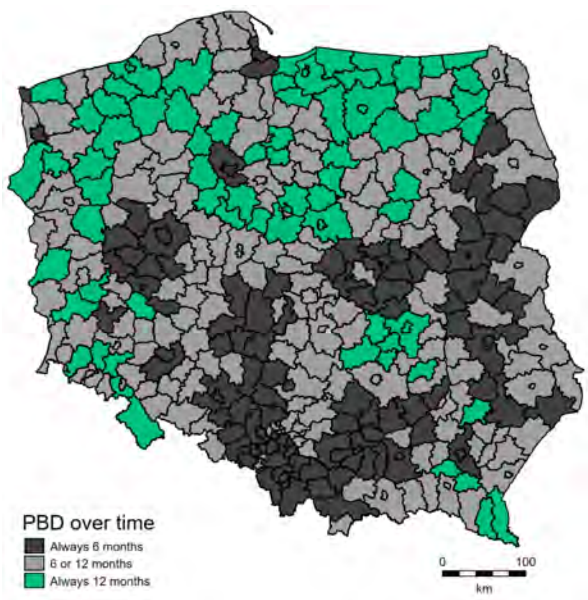
(a) 2003



(b) 2008



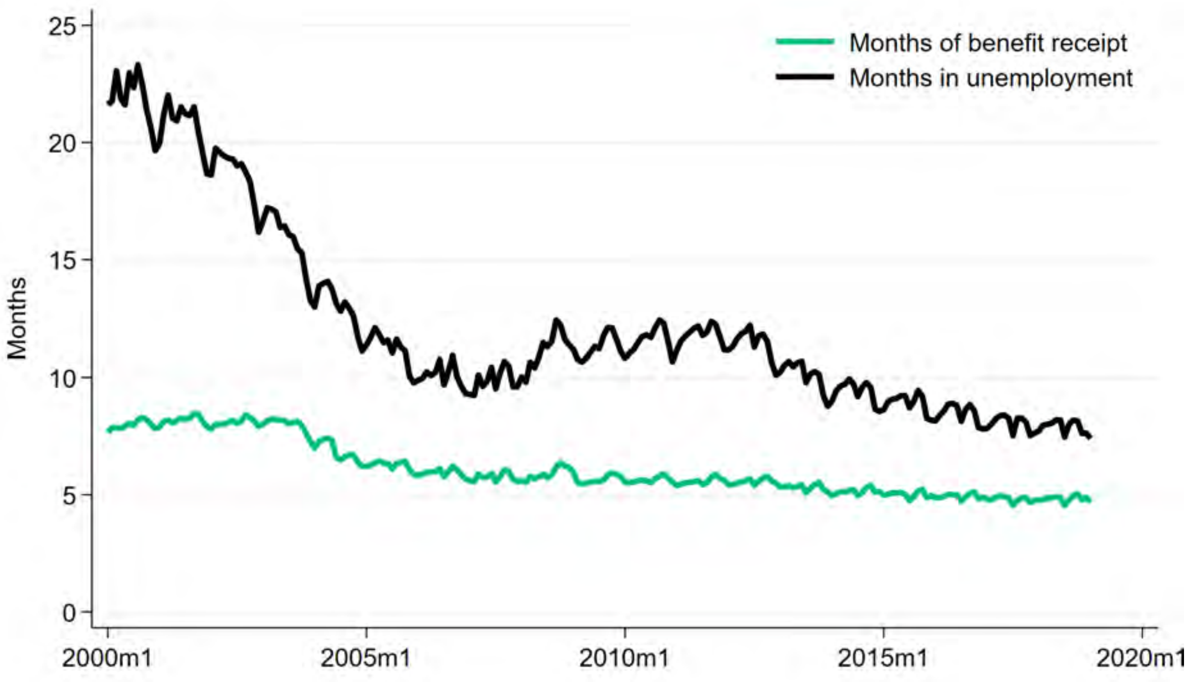
(c) 2013



(d) Changing cut-offs

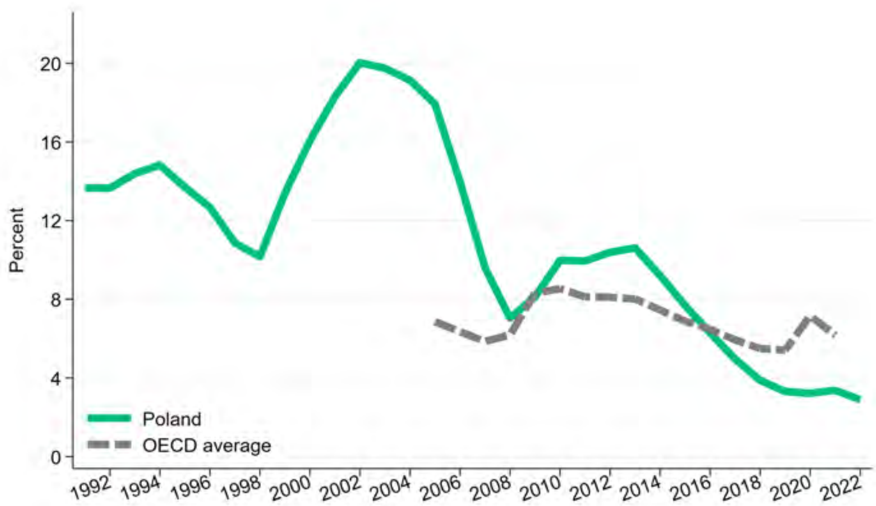
Notes: Panels (a)–(c) show the distribution of potential benefit durations in years with different threshold (100%, 125%, and 150%, respectively). Panel (d) shows the counties which always have a PBD of 6 or 12 months in our sample period, 2000–2019, and those with different PBDs over time (in gray).

Figure A.3: Months of benefit receipt and in unemployment over time



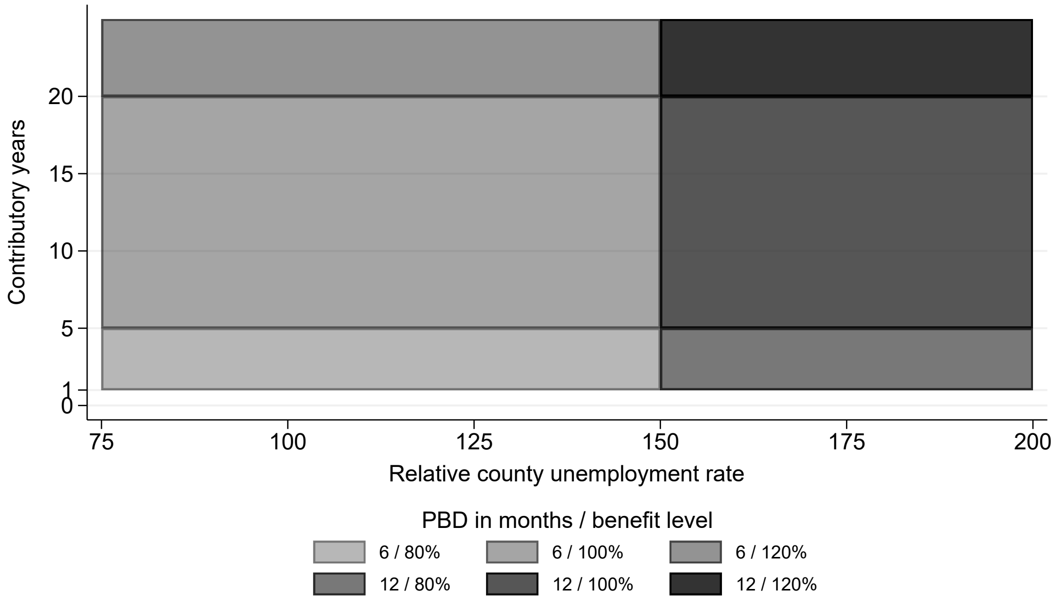
Notes: Figure shows the average months of benefit receipt (green line) and in unemployment over our sample period. The sample consists of benefit recipients under 50 and the time refers to the month of unemployment entry.

Figure A.4: Unemployment rate over time



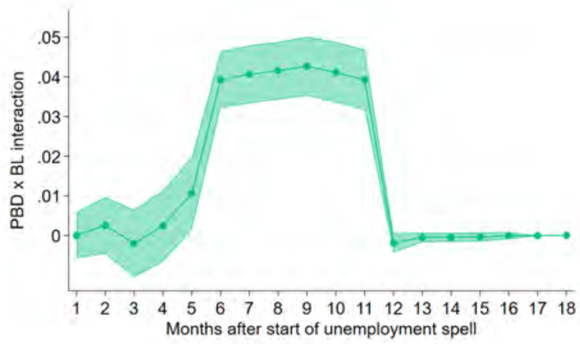
Notes: The figure shows how the unemployment rate of Poland and the OECD average over time. Sources: https://data.oecd.org/unemp/unemployment-rate.htm, accessed November 20, 2023, and Polish Labour Force Survey

Figure A.5: Benefit rules

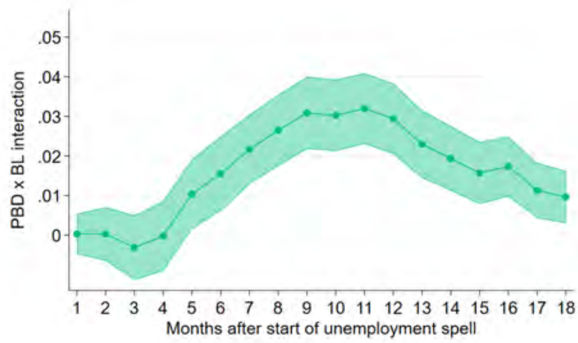


Notes: Rules for PBD concern the time period from February 2009 onward.

Figure A.6: Monthly estimates of the benefit-duration-level-interaction term of two-way regression discontinuity estimate



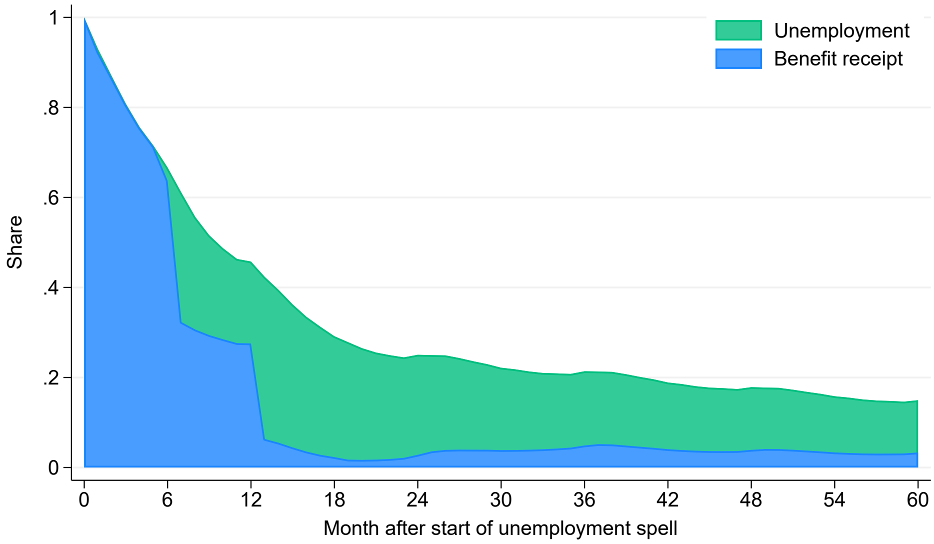
(a) Benefit receipt



(b) Unemployment

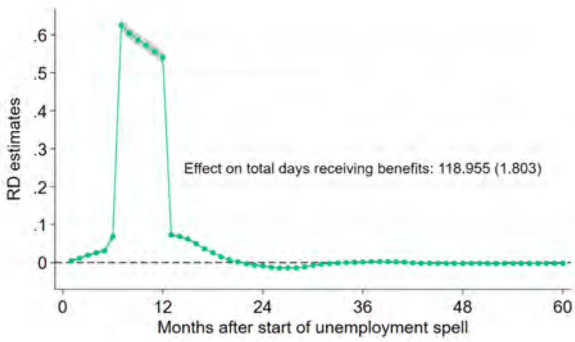
Notes: Figure shows monthly RD estimates of the interaction term of PBD extensions and level increases based on equation (3). Summary estimates are presented in Table 3. Sample period is 2004 to 2019. Shaded areas show 95% confidence intervals obtained from standard errors clustered at the county-level.

Figure A.7: Share of benefit recipients in unemployment and benefit receipt

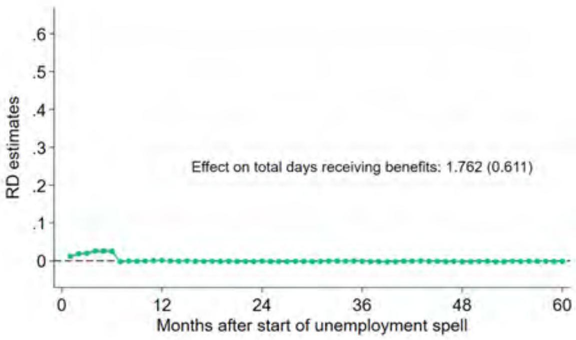


Notes: Figure depicts the share of benefit recipients in unemployment and the share receiving benefits. The sample is restricted to benefit recipients who entered unemployed before 2015 to ensure a sufficient post period of 5 years.

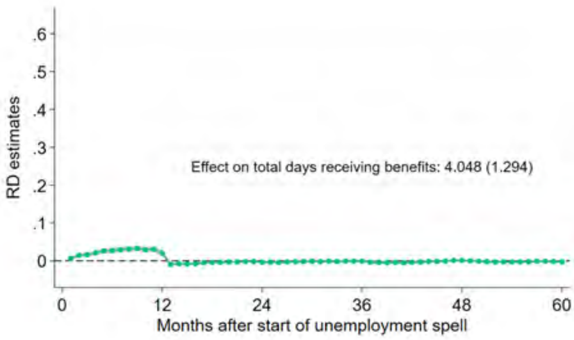
Figure A.8: Effects on benefit receipt in five years after begin of unemployment spell



(a) PBD



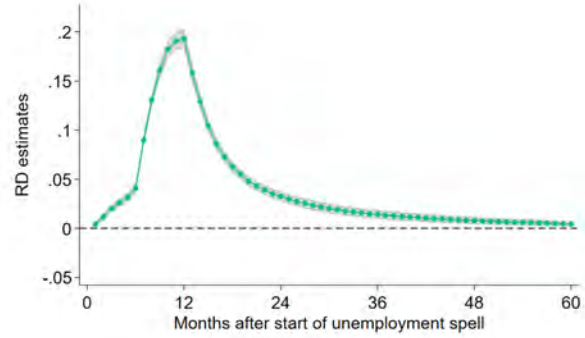
(b) BL, 6 months PBD



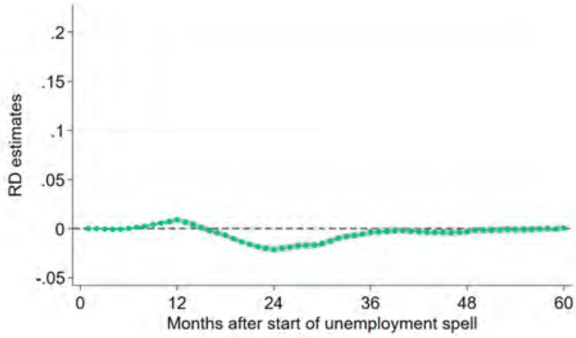
(c) BL, 12 months PBD

Notes: Each coefficient stems from a separate regression and is estimated with the optimal bandwidth (see Table 2), linear function of the running variable and including county and year fixed effects. Grey shared areas indicate 95% confidence intervals.

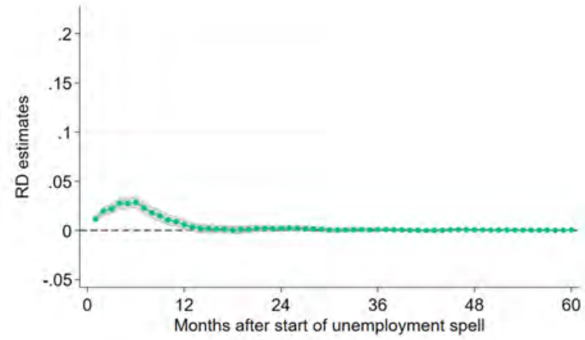
Figure A.9: Effects on unemployment in five years after begin of unemployment spell: Initial versus subsequent spells



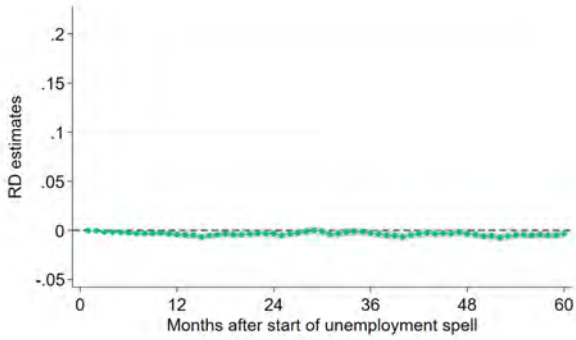
(a) PBD, initial spell



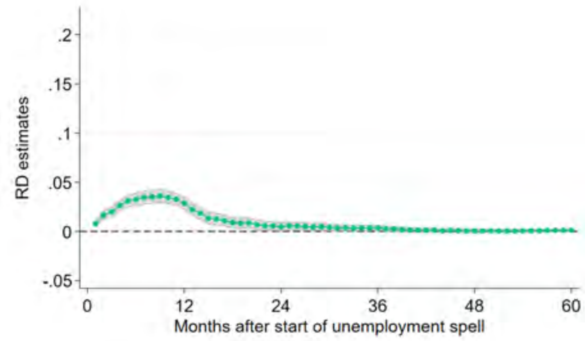
(b) PBD, subsequent spells



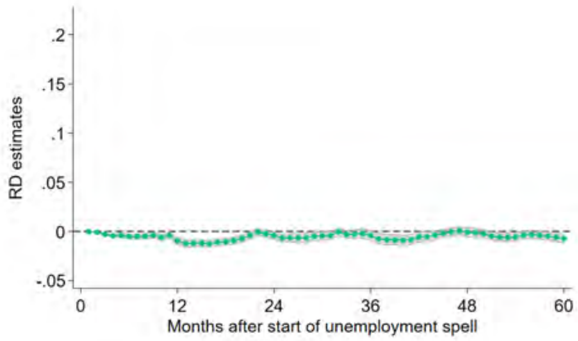
(c) BL, 6 months PBD, initial spell



(d) BL, 6 months PBD, subsequent spells



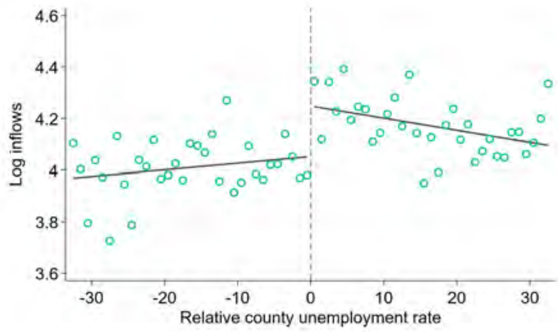
(e) BL, 12 months PBD, initial spell



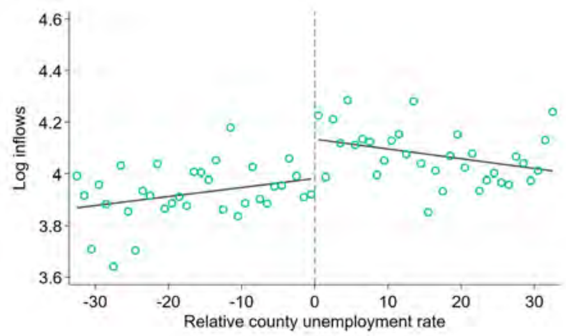
(f) BL, 12 months PBD, subsequent spells

Notes: Each coefficient stems from a separate regression and is estimated with the optimal bandwidth (see Table 2), linear function of the running variable and including county and year fixed effects. Lines indicate 95% confidence intervals.

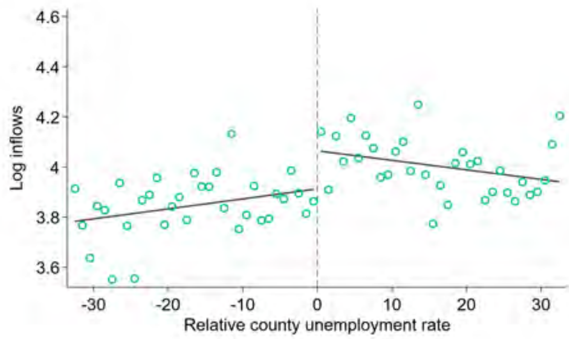
Figure A.10: Inflows into unemployment by potential benefit duration



(a) Entire calendar year



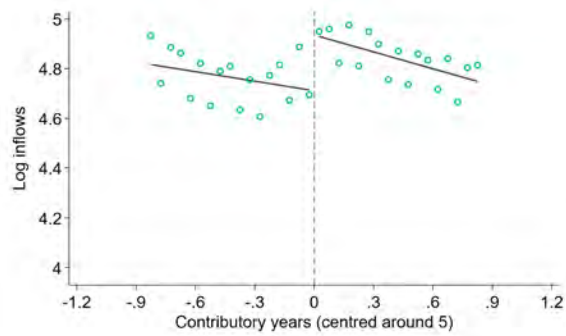
(b) February to September



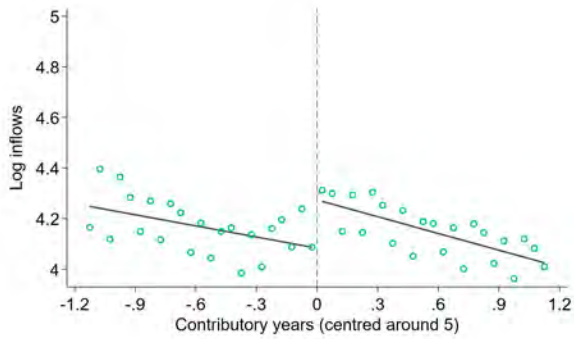
(c) June

Notes: Figures shows log inflows into in unemployment in bins of percentage point of county's relative unemployment rate. The number of inflows are calculated at they month-by-county-level. Solid lines linearly fit the scatters. RD estimates in Table 4 additionally absorb county and year FEs. Sample period is 2000 to 2019 (excluding 2004 and 2009 where PBD changes occurred during the year).

Figure A.11: Inflows into unemployment by benefit level



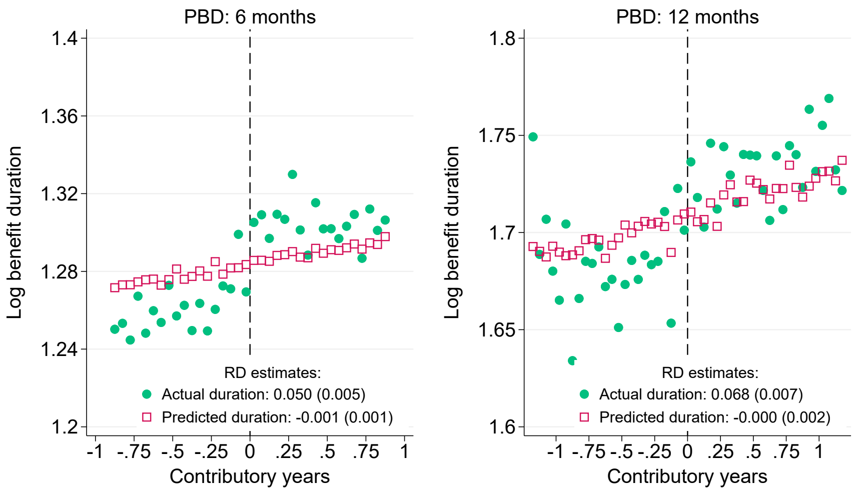
(a) PBD: 6 months



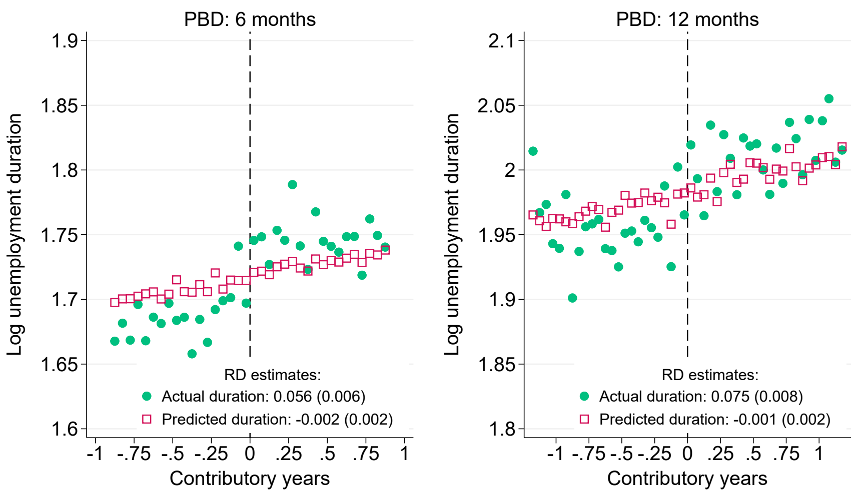
(b) PBD: 12 months

Notes: Figures shows log inflows into in unemployment in bins of 0.05 of contributory years. The number of inflows are initially calculated annually in bins of 0.01 contributory years. Solid lines linearly fit the scatters. RD estimates in Table 4 additionally absorb year FEs. Sample period is 2004 to 2019.

Figure A.12: Comparison of observed and predicted durations—BL



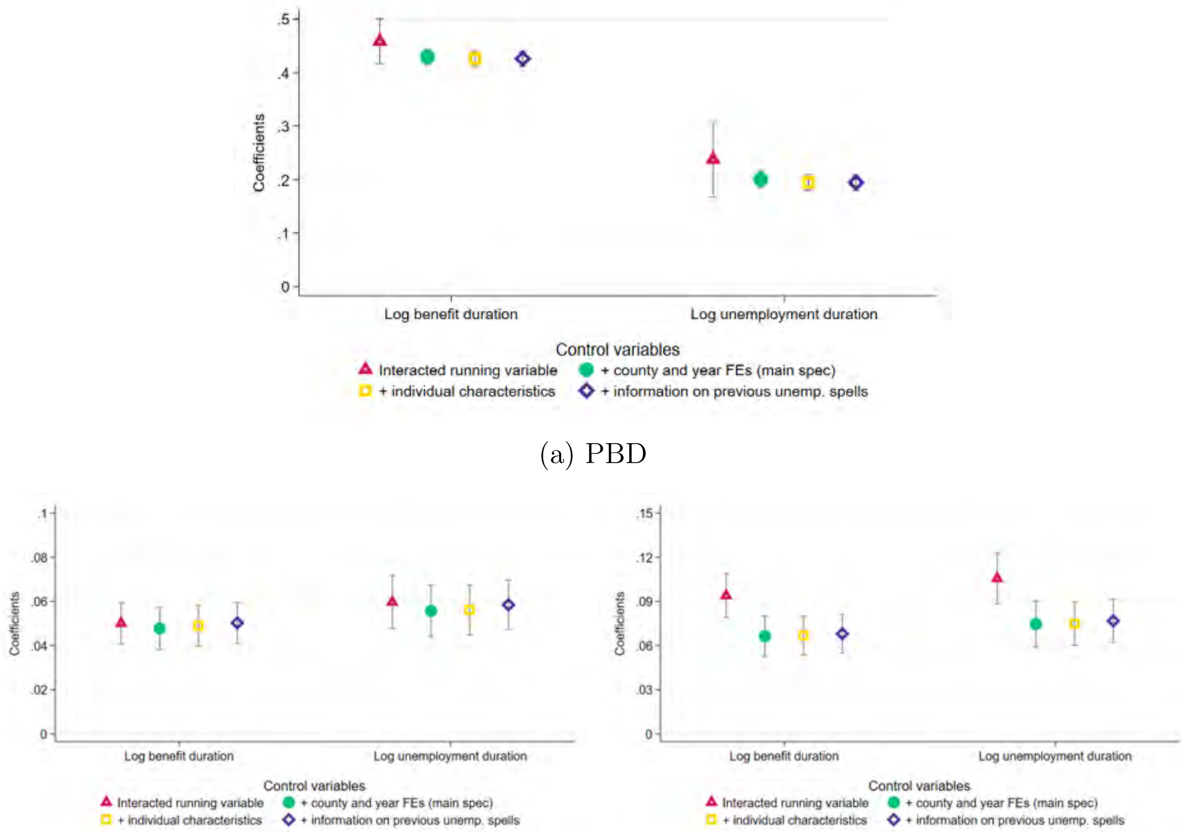
(a) Benefit duration



(b) Unemployment duration

Notes: Actual durations (circles) correspond to those shown in levels in Figure 5. Predicted durations (hollow squares) are obtained from regressing the observed durations on age, female indicator number of unemployment spells, education dummies, previous occupation, county FEs and year FEs. RD estimates as in Table 2.

Figure A.13: Stability of coefficients with control variables



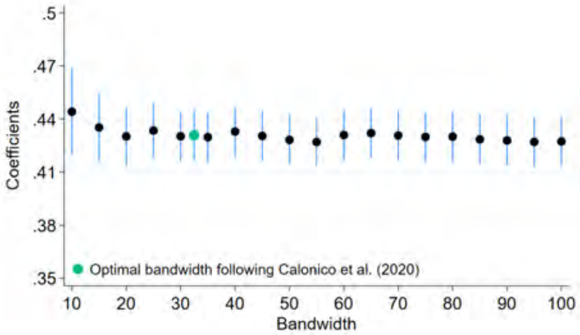
(a) PBD

(b) BL, 6 months PBD

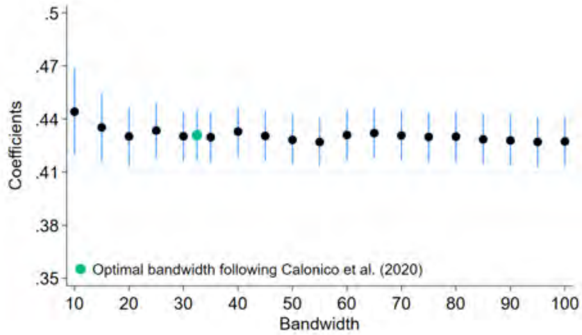
(c) BL, 12 months PBD

Notes: Red hollow triangles include only the interacted running variables in the estimation. Green circles correspond to RD estimates reported in Table 2. Individual characteristics are age, a female indicator, urban county, contributory years (PBD estimation only), education and previous occupation dummies. In the final specification additionally the number of previous unemployment spells and the length of previous unemployment spells (in 10 categories, including an indicator if the current one is the first). Whiskers indicate 95% confidence intervals.

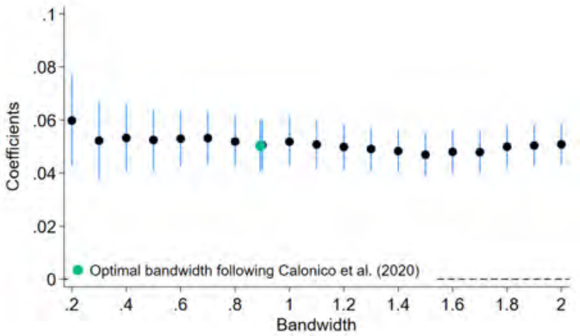
Figure A.14: Robustness of duration estimates to choice of bandwidth



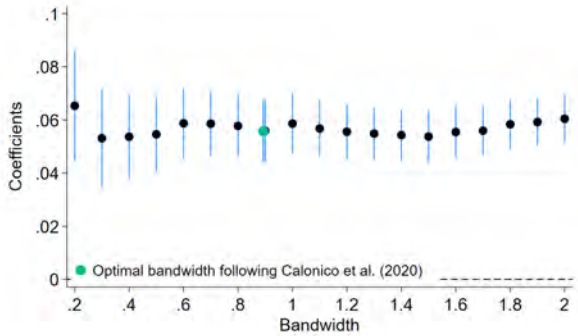
(a) PBD, log benefit duration



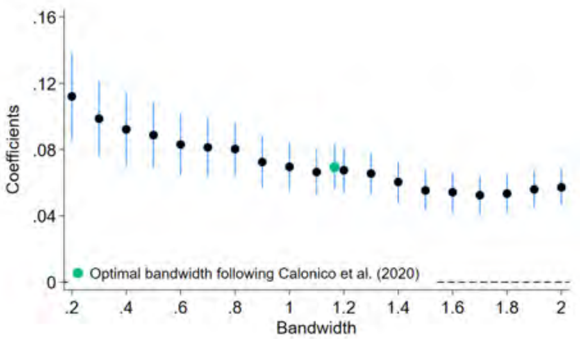
(b) PBD, log unemployment duration



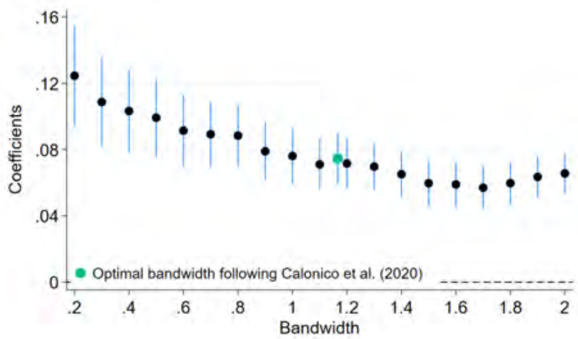
(c) BL, 6 months PBD, log benefit duration



(d) BL, 6 months PBD, log unemp. duration



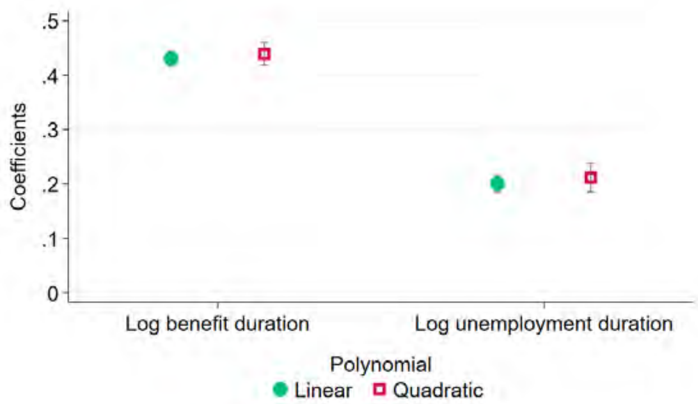
(e) BL, 12 months PBD, log benefit duration



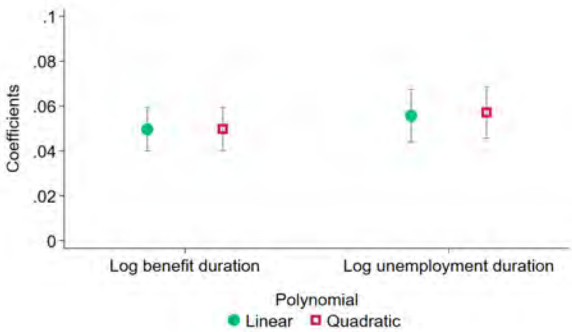
(f) BL, 12 months PBD, log unemp. duration

Notes: Green scatters correspond to those reported in Table 2. These use the optimal bandwidth based on Calonico et al. (2020). Whiskers indicate 95% confidence intervals.

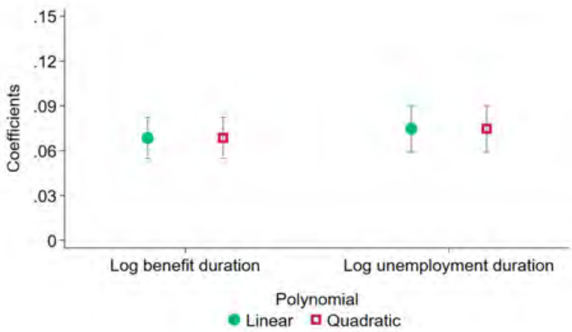
Figure A.15: Robustness of duration estimates to quadratic polynomial



(a) PBD



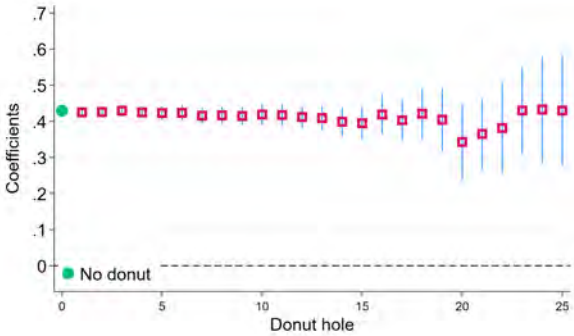
(b) BL, 6 months PBD



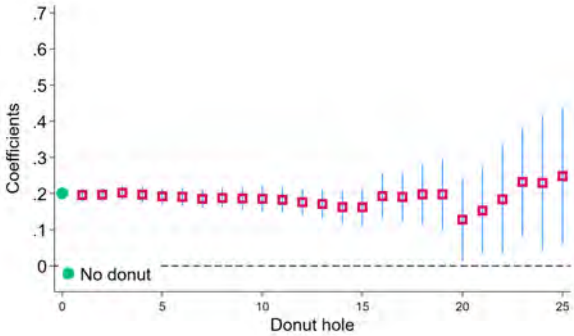
(c) BL, 12 months PBD

Notes: Green scatters correspond to those reported in Table 2. Whiskers indicate 95% confidence intervals.

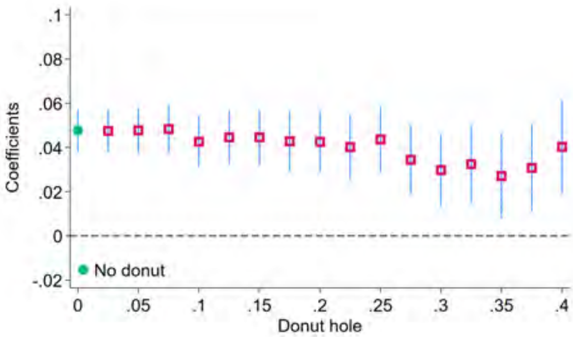
Figure A.16: Robustness of duration estimates to RD donut hole



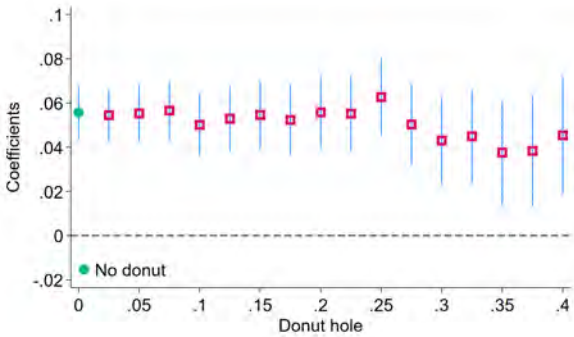
(a) PBD, log benefit duration



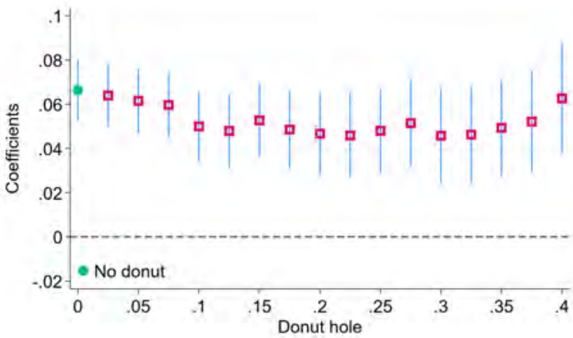
(b) PBD, log unemployment duration



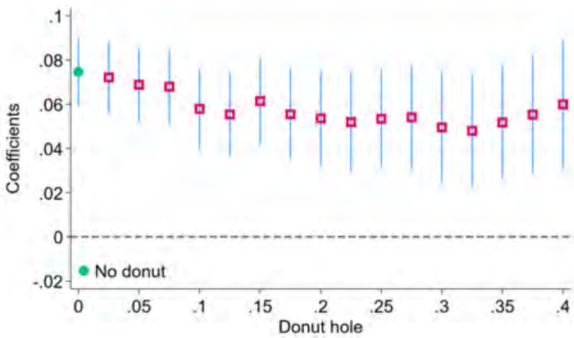
(c) BL, 6 months PBD, log benefit duration



(d) BL, 6 months PBD, log unemp. duration



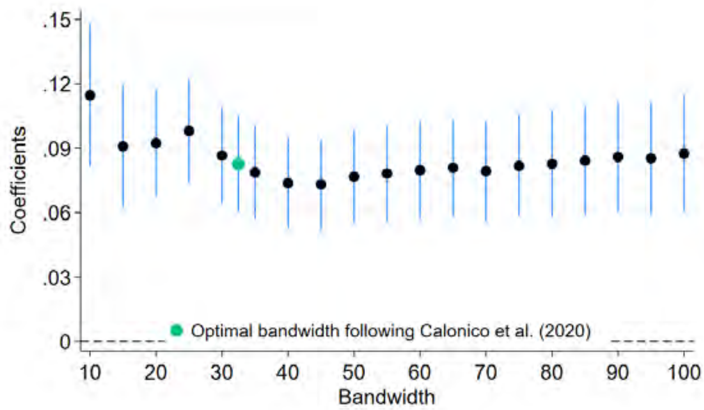
(e) BL, 12 months PBD, log benefit duration



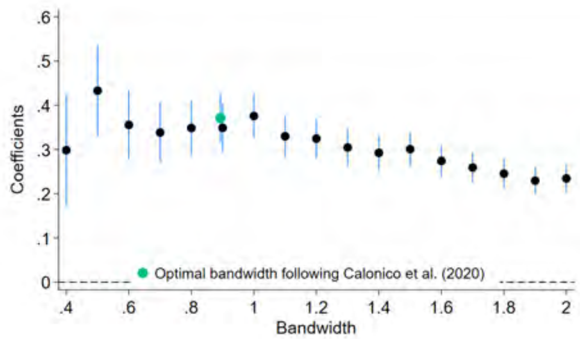
(f) BL, 12 months PBD, log unemp. duration

Notes: Green scatters correspond to those reported in Table 2. "Donut hole" means that observations around the threshold are omitted in the estimation sample. The bandwidth always uses the same bandwidth calculated following Calonico et al. (2020). Whiskers indicate 95% confidence intervals.

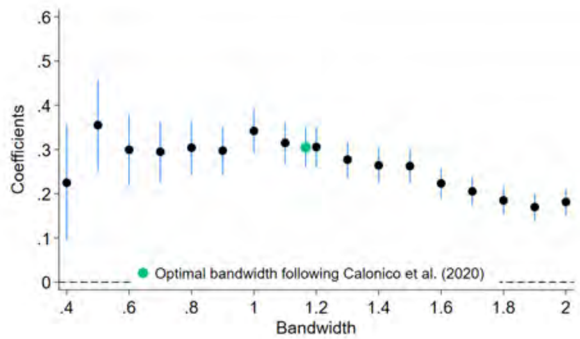
Figure A.17: Robustness of inflow effects to bandwidth



(a) PBD



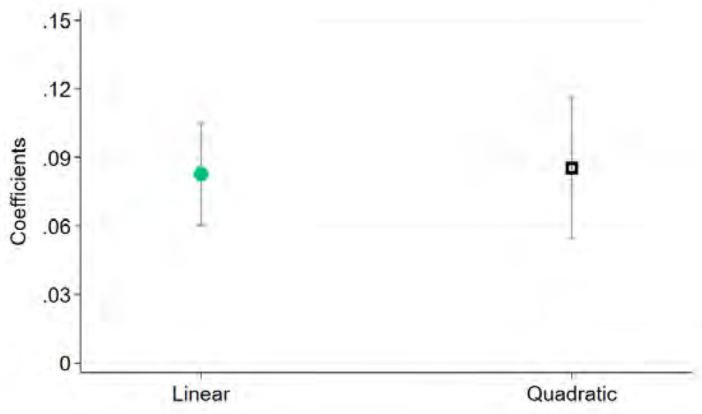
(b) BL, 6 months PBD



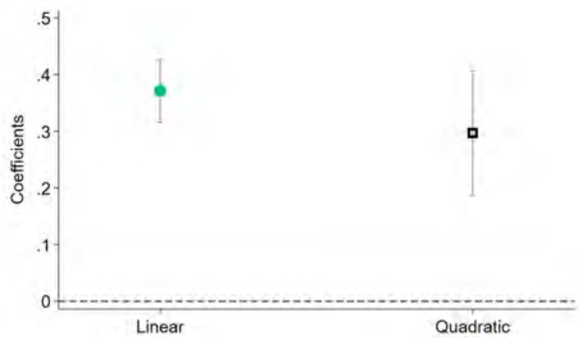
(c) BL, 12 months PBD

Notes: Green scatters correspond to those reported in Table 4. These use the optimal bandwidth for estimates on unemployment and benefit duration following Calonico et al. (2020). Whiskers indicate 95% confidence intervals.

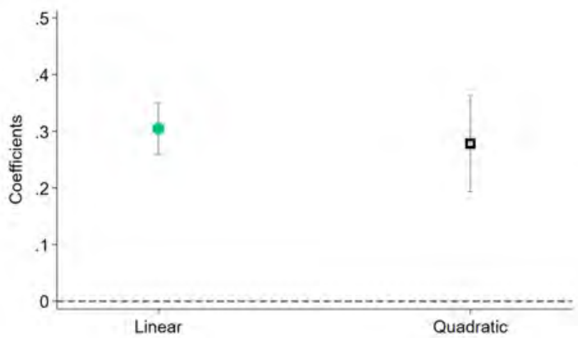
Figure A.18: Robustness of inflow effect to polynomial choice



(a) PBD



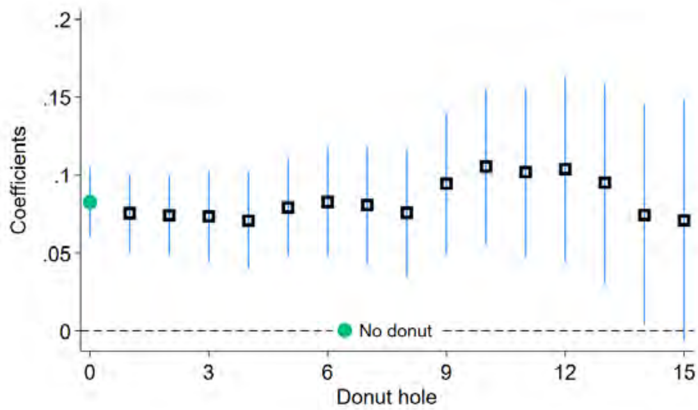
(b) BL, 6 months PBD



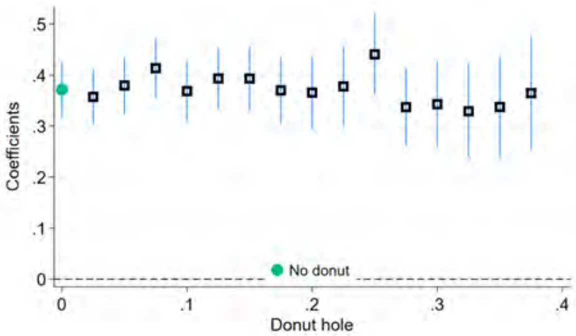
(c) BL, 12 months PBD

Notes: Green scatters correspond to those reported in Table 4. Whiskers indicate 95% confidence intervals.

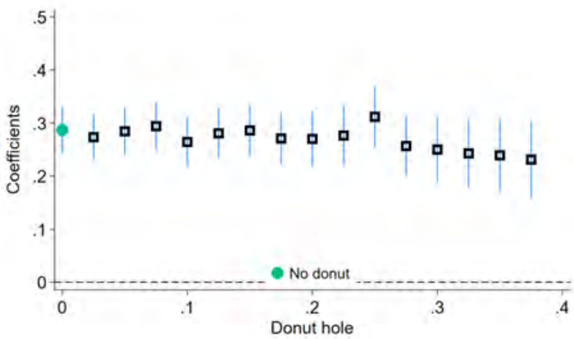
Figure A.19: Robustness of inflow effect to donut hole



(a) PBD



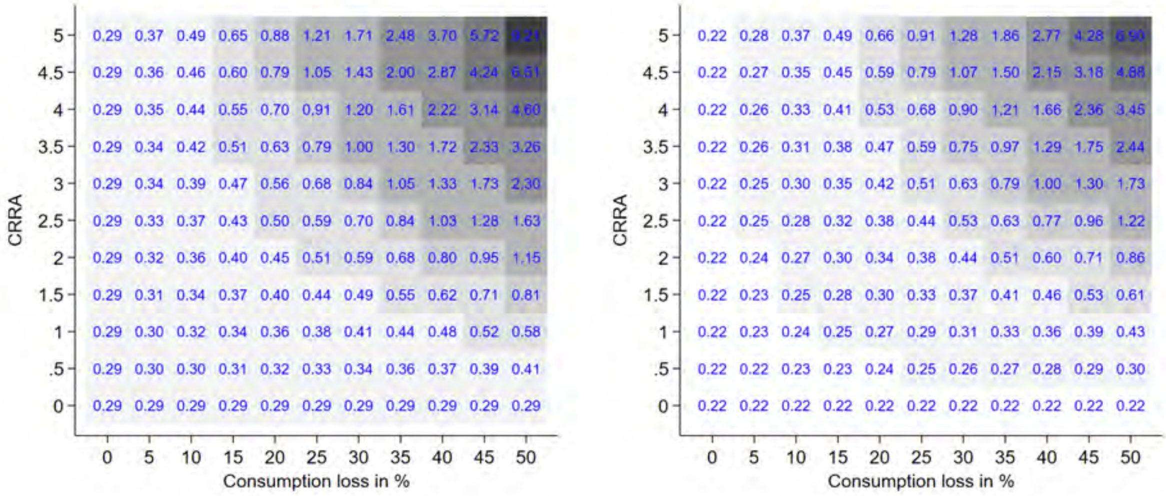
(b) BL, 6 months PBD



(c) BL, 12 months PBD

Notes: Green scatters correspond to those reported in Table 4. "Donut hole" means that observations around the threshold are omitted in the estimation sample. Whiskers indicate 95% confidence intervals.

Figure A.20: MVPF of PBD increases with different CRRA coefficients and consumption losses

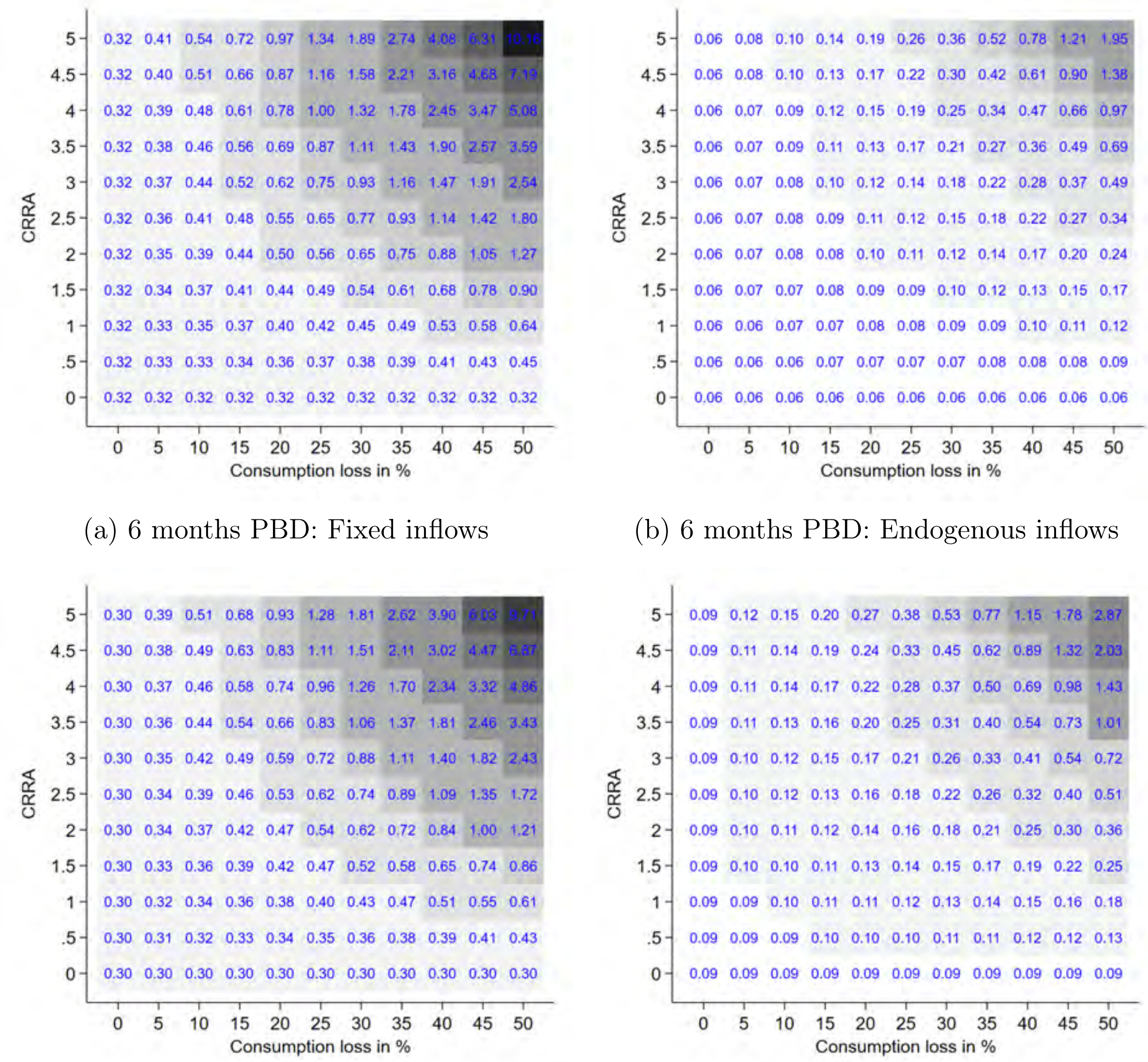


(a) Fixed inflows

(b) Endogenous inflows

Notes: Heatmap shows the marginal value of public funds (MVPF) of PBD increases for different values of coefficients of constant relative risk aversion (CRRA) and consumption losses in unemployment. Table 7 contains the MVPFs for CRRA coefficients of 1, 2 and 5, and consumption losses of 10 and 30%.

Figure A.21: MVPF of BL increases with different CRRA coefficients and consumption losses



(a) 6 months PBD: Fixed inflows

(b) 6 months PBD: Endogenous inflows

(c) 12 months PBD: Fixed inflows

(d) 12 months PBD: Endogenous inflows

Notes: Heatmap shows the marginal value of public funds (MVPF) of BL increases for different values of coefficients of constant relative risk aversion (CRRA) and consumption losses in unemployment. Table 8 contains the MVPFs for CRRAs of 1, 2 and 5, and consumption losses of 10 and 30%.

B Tables

Table B.1: Effects of more generous UI on benefit and unemployment durations—same estimation sample

Variation:	6 months longer PBD		25% higher BL			
Dependent variable:	Months of					
	benefit receipt	unemployment	benefit receipt		unemployment	
PBD:			6 mo	12 mo	6 mo	12 mo
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Levels						
RD estimate	3.1546***	2.3035***	0.1125***	0.2962***	0.2373**	0.4501***
	(0.0573)	(0.1348)	(0.0190)	(0.0383)	(0.1147)	(0.1301)
Panel B: Logs						
RD estimate	0.4026***	0.2005***	0.0425***	0.0649***	0.0504***	0.0685***
	(0.0104)	(0.0122)	(0.0070)	(0.0088)	(0.0098)	(0.0100)
Elasticity	0.581	0.289	0.190	0.291	0.226	0.307
Observations	292,217	292,217	120,332	134,587	120,332	134,587

Notes: Table corresponds to Table 2, but restricts the sample to observations contained in the bandwidths of both the PBD and BL estimates. Estimates are based on equation (1). For the PBD estimates (columns 1-2) the running variable is the relative county unemployment rate and for BL estimates (columns 3-6) contributory years. All estimates include county and year fixed effects and a linear function of the running variable interacted with the treatment indicator. Sample period is 2004-2019. Standard errors clustered at the county-level in parentheses. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

Table B.2: Distribution of characteristics of the unemployed around the threshold

Dependent variable:	Age	Female	Education	Number of prev. unemp. spells	Contr. years
	(1)	(2)	(3)	(4)	(5)
Panel A: PBD threshold					
RD estimate	0.2934***	0.0169***	0.0027	-0.0038	0.2749***
	(0.0516)	(0.0024)	(0.0020)	(0.0037)	(0.0558)
Relative effect	0.009	0.036	0.005	-0.003	0.030
Observations	3,039,893	3,039,893	3,035,247	3,039,893	3,039,893
Panel B: BL threshold—6 months PBD					
RD estimate	0.4087***	-0.0033	-0.0135***	0.0519***	
	(0.0480)	(0.0035)	(0.0031)	(0.0043)	
Relative effect	0.014	-0.006	-0.021	0.037	
Observations	385,398	385,398	385,130	385,398	
Panel C: BL threshold—12 months PBD					
RD estimate	0.3430***	-0.0049	-0.0212***	0.0803***	
	(0.0595)	(0.0045)	(0.0041)	(0.0064)	
Relative effect	0.012	-0.010	-0.040	0.051	
Observations	258,484	258,484	258,338	258,484	

Notes: Estimates are based on equation (1). Relative effects relate the RD estimate to the average of the estimation sample. Education is coded as a binary indicator for having at least secondary education. All estimates include county and year fixed effects and a linear function of the running variable interacted with the treatment indicator. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

Table B.3: RD estimates for current and previous unemployment spell

Dependent variable:	Log benefit duration		Log unemployment duration	
Spell:	Current (1)	Previous (2)	Current (3)	Previous (4)
Panel A: PBD threshold				
RD estimate	0.4015***	0.0075	0.2143***	0.0218**
	(0.0089)	(0.0103)	(0.0095)	(0.0095)
Elasticity	0.579	0.011	0.309	0.032
Observations	812,454	812,831	813,383	813,383
Panel B: BL threshold—6 months PBD				
RD estimate	0.0431***	0.0032	0.0545***	0.0063
	(0.0079)	(0.0081)	(0.0098)	(0.0096)
Elasticity	0.193	0.014	0.244	0.028
Observations	129,345	129,361	129,420	129,420
Panel C: BL threshold—12 months PBD				
RD estimate	0.0572***	-0.0027	0.0636***	-0.0033
	(0.0123)	(0.0085)	(0.0132)	(0.0096)
Elasticity	0.256	-0.012	0.285	-0.015
Observations	111,997	111,997	112,054	112,054

Notes: Estimates are based on equation (1). The analysis sample is restricted to unemployed who have had a previous unemployment spell. The running variable and treatment indicator is always based on the current unemployment spell. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

Table B.4: Effects of more generous UI on benefit and unemployment durations: PBD threshold including quitters

Dependent variable:	Months of			
	benefit receipt (1)	unemployment (2)	benefit receipt (3)	unemployment (4)
Panel A: Levels				
RD estimate	3.4256***	2.4067***	3.3265***	2.0551***
	(0.0501)	(0.1405)	(0.0594)	(0.1958)
Panel B: Logs				
RD estimate	0.4619***	0.1979***	0.6975***	0.1539***
	(0.0072)	(0.0076)	(0.0150)	(0.0086)
Elasticity			0.635	0.140
Sample	Quitters and laid off		Quitters only	
Bandwidth	32.47	32.47	32.47	32.47
Observations	3,505,639	3,509,847	469,259	469,954

Notes: Table shows RD estimates at the PBD threshold as in Table 2, but additionally includes unemployed who have quit their jobs. These have a PBD of either 3 or 9 months depending on the relative unemployment rate of the county. Table 2 only contained unemployed who were laid off. See that table for other notes. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

Table B.5: Effects of more generous UI on benefit and unemployment durations: BL threshold including quitters

Dependent variable:	Months of							
	benefit receipt		unemployment		benefit receipt		unemployment	
PBD:	3/6 mo	9/12 mo	3/6 mo	9/12 mo	3 mo	9 mo	3 mo	9 mo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Levels								
RD estimate	0.1546***	0.3426***	0.1474***	0.5156***	0.0307**	0.2566***	-0.0942	0.6307***
	(0.0114)	(0.0283)	(0.0538)	(0.0939)	(0.0154)	(0.0525)	(0.1412)	(0.1951)
Panel B: Logs								
RD estimate	0.0611***	0.0720***	0.0353***	0.0618***	0.0325***	0.0614***	0.0025	0.0516***
	(0.0048)	(0.0067)	(0.0053)	(0.0073)	(0.0101)	(0.0152)	(0.0085)	(0.0116)
Elasticity	0.274	0.323	0.158	0.277	0.146	0.275	0.011	0.231
Sample	Quitters and laid off				Quitters only			
Bandwidth	0.89	1.17	0.89	1.17	0.89	1.17	0.89	1.17
Observations	466,108	309,576	466,340	309,698	80,935	51,207	80,942	51,214

Notes: Table shows RD estimates at the BL threshold as in Table 2, but additionally includes unemployed who have quit their jobs. These have a PBD of either 3 or 9 months depending on the relative unemployment rate of the county, unemployed who were laid off have a PBD of 6 or 12 months. Table 2 only contained unemployed who were laid off. See that table for other notes. Significance levels: * < 10% ** < 5% *** < 1%.

Table B.6: Effects of more generous UI on inflows into unemployment: PBD threshold including quitters

Dependent variable:	Log inflows into unemployment					
Sample:	Full year	Feb-Sep	June	Full year	Feb-Sep	June
	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	0.1432***	0.0981***	0.0897***	0.1939***	0.1812***	0.1527***
	(0.0118)	(0.0109)	(0.0164)	(0.0176)	(0.0187)	(0.0347)
Elasticity				0.177	0.165	0.139
Sample	Quitters and laid off			Quitters only		
Bandwidth	32.47	32.47	32.47	32.47	32.47	32.47
Observations	35,904	23,936	2,980	35,713	23,808	2,962

Notes: Table shows RD estimates at the PBD threshold as in Table 4, but additionally includes unemployed who have quit their jobs. These have a PBD of either 3 or 9 months depending on the relative unemployment rate of the county. Table 4 only contained unemployed who were laid off. See that table for other notes. Significance levels: * < 10% ** < 5% *** < 1%.

Table B.7: Effects of more generous UI on inflows into unemployment:
BL threshold including quitters

Dependent variable:	Log inflows into unemployment			
PBD:	3/6 mo (1)	9/12 mo (2)	3 mo (3)	9 mo (4)
Inflow effect	0.3631*** (0.0290)	0.2628*** (0.0230)	0.2476*** (0.0360)	0.1727*** (0.0337)
Elasticity	1.627	1.178	1.110	0.774
Observations	2,492	3,248	2,492	3,248
Sample	Quitters and laid off		Quitters only	
Bandwidth	0.89	1.17	0.89	1.17
Observations	2,520	3,276	2,520	3,276

Notes: Table shows RD estimates at BL threshold as in Table 4, but additionally includes unemployed who have quit their jobs. These have a PBD of either 3 or 9 months depending on the relative unemployment rate of the county. Table 4 only contained unemployed who were laid off. See that table for other notes. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

Table B.8: Parameters and their meaning

Parameter	Description
e	Share employed
u_b	Share receiving benefits
$\left.\frac{dD_b}{dP}\right\|_M$	Share exhaustees of benefit recipients (mechanical effect)
$\left.\frac{dD_b}{dP}\right\|_B$	Behavioral component of marginal effect of benefit duration wrt PBD
D	Unemployment duration (in months)
D_b	Benefit duration (in months)
δ	Job destruction rate
$\eta_{D,b}$	Elasticity of unemployment duration with respect to benefit levels
$\eta_{D_b,b}$	Elasticity of benefit duration with respect to benefit levels
$\frac{dD}{dP}$	Marginal effect of PBD change on unemployment duration
$\eta_{\delta,b}$	Elasticity of job destruction rate with respect to benefit levels
$\eta_{\delta,P}$	Elasticity of job destruction rate with respect to PBD
τ	Tax liability + social assistance
b	Benefits - social assistance

Notes: Table contains a brief description of the parameters for the welfare calculations as reported in Table 6.

Table B.9: Required consumption losses in percent for 0 welfare effect

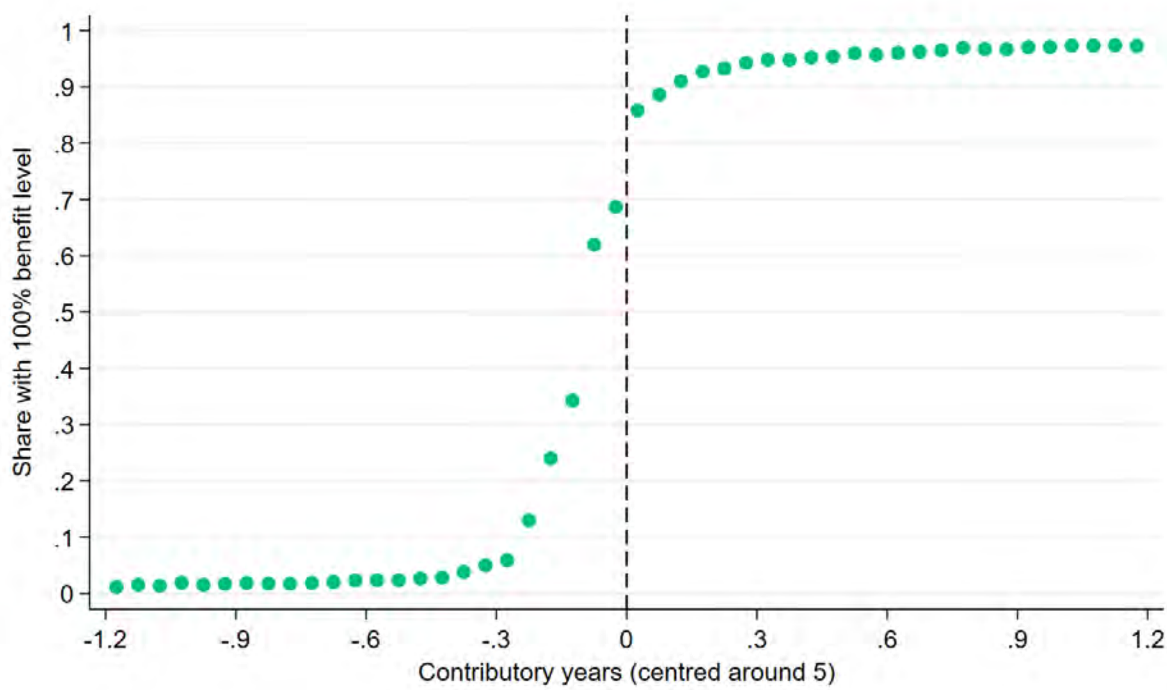
Coefficient of relative risk aversion (CRRA):	1	2	5
	(1)	(2)	(3)
Panel A: PBD variation			
Fixed inflows	71.13	46.26	22
Endogenous inflows	78.35	53.47	26.37
Panel B: BL variation (6 months PBD)			
Fixed inflows	68.24	43.64	20.5
Endogenous inflows	93.92	75.34	42.87
Panel C: BL variation (12 months PBD)			
Fixed inflows	69.65	44.91	21.22
Endogenous inflows	91.04	70.07	38.28

Notes: The table reports the required consumption losses at benefit exhaustion (Panel A) or when entering unemployment (Panels B and C) for the welfare effect of marginal PBD extensions or benefit increases to be zero. BC/MC ratios and MVPFs with different CRRA coefficients and consumption losses are reported in Tables 7 and 8.

C The Role of Measurement Error in Contributory Years

The registry data contain a variable that reports the benefit level and a second variable that contains a measure for contributory years. When comparing the two variables, it is apparent that the variable for contributory years contains measurement error. The reason is that employment offices in some cases obtain additional information from the benefit recipients and then update the benefit level of the recipient. When this happens, the benefit level in the data is updated, but sometimes the employment office does not update the first estimate of contributory years. Appendix Figure C.1 shows the share of individuals treated with a higher benefit level (100% instead of 80%) for bins of the running variable contributory years. If there was no measurement error, the shares would all be zero (to the left of the threshold) or one (to the right of the threshold). As one might expect, the share of wrongly assigned spells is higher close to the threshold. Moreover, there are more wrongly assigned spells on the left-hand side of the threshold. Thus, the measurement error appears to have a negative mean. This can be explained with the true data generating process: employment centers are more likely to correct contributory years, and hence the benefit level, upward instead of downward—perhaps because recipients only hand in later proof of some contribution spells.

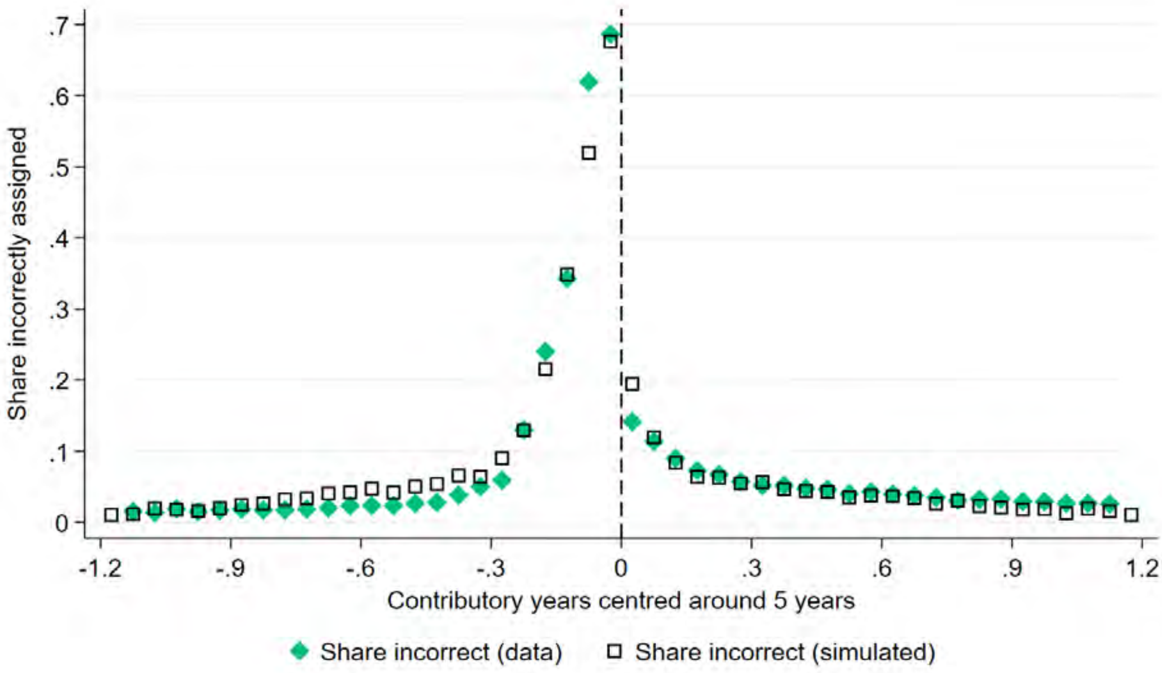
Figure C.1: Share of recipients with 100% benefit level along bins of contributory years



Importantly, the measurement error in contributory years only affects the estimates for the effects of changes in the benefit level and not those for the effects of changes in the potential benefit duration. However, since contributory years is the key control variable of the the regressions concerning the effect of changes in the benefit level, one might worry that measurement error in this variable biases the estimated treatment effects to a relevant degree.

In this Appendix, we present some simulations that demonstrate that this is not the case. For comparably simple cases, when distribution of the measurement error is known, it is, in principle, possible to calculate the bias induced by measurement error (see, e.g., Pischke, 2007). Our case is complicated considerably since, for the inflow estimates, we need to collapse the data into bins, leaving us with measurement error in the share of treated individuals and the running variable. Moreover, the bandwidth restricts the sample based on a variable that is measured with error. We do not know the exact distribution of the measurement error, but we use the share of individuals where treatment status and running variable are misaligned to generate simulated data with measurement error calibrated to these shares.

Figure C.2: Share of recipients with misaligned level benefit level and contributory years along bins of contributory years



We first generate artificial individual-level data, where the distribution of the running variable resembles that of the observed data. Since the impact of the measurement error in a control variable on the estimated treatment effect depends on the slope of the control variable, it is important to match it to the actual registry data. Following, we generate a jump in the distribution of the running variable at zero, our estimated inflow effect. We then add measurement error such that the share of individuals with misaligned treatment status and observed contributory years is similar to what we observe in the actual registry data. After generating the relevant outcome variables, these generated data can then directly be used to gauge the importance of measurement error for our duration estimates. For the estimation of effects on inflows, we need to generate bins and then estimate the model for inflows.

Appendix Figure C.2 shows the resulting distribution of individuals, where the treatment status does not 'match' the contributory years, i.e., individuals with 100% benefit level on the left side of the threshold and individuals with 80% benefit level on the right side for the observed and the generated data. The share of misclassified individuals is substantially higher near the cut-off but remains notable even farther from it. To match this feature of the distribution, it is necessary to generate measurement errors that follow a distribution that exhibits excess kurtosis. We do so by drawing the random component of measurement error ξ from a mixture of two normal distributions. A normal mixture provides a flexible way to approximate a non-Gaussian distribution (Guvenen et al., 2021):

$$\xi \sim \begin{cases} \mathcal{N}(-0.07, 0.1) & \text{with probability } 0.82 \\ \mathcal{N}(-0.07, 1) & \text{with probability } 0.18 \end{cases} \tag{C.1}$$

The mean of the measurement error is negative, ensuring that the share of individuals with misalignment of treatment status and running variable is larger on the left side of the cut-off.

Duration estimates — We use the generated individual-level data on contributory years and generate unemployment durations and benefit durations based on the coefficients reported in the main part of the paper in Table 2. We then estimate the model on the generated data with and without measurement error in contributory years and compare the coefficients to those obtained from actual data. For counties with a 12-month potential benefit duration, these estimates are reported in Appendix Table C.1. The results for 6-month PBD counties are similar.

For comparison, column (1) first reports the RD estimate also contained in Table 2 (column (4), Panel A) and additionally the coefficient for contributory years and its interaction with the treatment indicator (coefficients β_2 and β_3 in equation (1)). Looking at columns (2) and (3) for benefit duration, it becomes apparent that the measurement error in contributory years induces only a small bias in the treatment coefficient, well within the confidence interval. In this case the bias in the treatment coefficient is downward. The reason is that contributory years are negatively correlated with the outcome variable (and positively correlated with the treatment status). The coefficient of the generated running variable with measurement error is closer to zero than the coefficient of the generated true running variable due to regression dilution. Not fully accounting for the negative correlation between the running variable and the outcome variable, attributes some of this negative correlation to the treatment dummy, resulting in a small downward bias.

Columns (4)-(6) show estimates for unemployment duration. The coefficients are larger in absolute terms, such that the differences between the columns appear larger. It is, however, still the case that the bias induced by measurement error is well within the confidence interval. In this case the bias is upward since the coefficient of the running variable is strongly positive on the right-hand side of the threshold. Note that the coefficients derived from the generated data just represent one draw (instead of average values over many draws as in a Monte Carlo simulation) with the aim to demonstrate that measurement error does not heavily impact our estimates.

Inflow estimates — To test for the validity of the inflow estimates, we collapse the generated individual-level data into bins of the running variable and estimate the same model as in the main part of the paper. We present the results for the case of 12-month PBD counties in Appendix

Table C.1: Estimation results for the effects of 25% higher BL on durations in 12-month PBD counties with observed and generated data

Dependent variable:	Benefit duration			Unemployment duration		
	Actual data	Generated, no msmt. error	Generated, msmt. error	Actual data	Generated, no msmt. error	Generated, msmt. error
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.3175***	0.3492***	0.3160***	0.5432***	0.4670***	0.5141***
	(0.0295)	(0.0332)	(0.0292)	(0.0982)	(0.1032)	(0.0901)
Contributory years	-0.0614*	-0.0893**	-0.0655**	-0.1572	-0.1136	-0.0441
	(0.0352)	(0.0348)	(0.0318)	(0.1129)	(0.1082)	(0.0975)
Treated × contr. years	0.0557	0.0681	0.0565	0.3723***	0.4613***	0.1873
	(0.0416)	(0.0502)	(0.0427)	(0.1379)	(0.1556)	(0.1319)
Observations	258,484	258,484	258,484	258,484	258,484	258,484

Notes: Estimates for effects of benefit level increases on benefit duration and unemployment duration, equivalent to those reported in Table 2. Columns (1) and (4) equal the results reported in Table 2. Standard errors are clustered at the county-level for estimates based on actual data and robust for estimates based on generated data. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

Table C.2, which equivalently holds for 6-month PBD counties. The first 3 columns contain the estimates of the empirical model in the main part of the paper based on the actual data, those using the generated data without measurement error and the estimate based on generated data with measurement error. In case of no measurement error, the model for inflows is just a simple RD, as the share of treated individuals is one on the right-hand side of the cut-off and zero otherwise. The treatment coefficient is remarkably similar for the three estimations. The estimates using generated data are very close to those based on the real-world data. Hence, the simulation captures the key aspects of the actual data. Moreover, the results in columns (2) and (3) with and without measurement error are very similar, demonstrating that measurement error in contributory years does not bias the results in a relevant way. To demonstrate that simple regression discontinuity is not adequate for the estimation of the effect of benefit level increases on inflows in our setting, columns (4) to (6) show results from a simple RD, naively assuming that there is no measurement error and the share of treated in each bin jumps discontinuously from zero to one. Applying RD on the actual data leads to a substantial reduction in the estimated treatment effect displayed column (4). This is intuitive. Consider the case, where treatment shares in reality are either 0.05 or 0.95 and the researcher naively assumes that they are zero or one. In this case, the true treatment effect is rescaled by $(0.95 - 0.05) = 0.9$. As explained, column (5) shows equivalent results to column (2). Finally, column (6) based on the generated data with measurement error contains, once more, very similar results to those obtained with the actual data, once more demonstrating that the simulated data successfully capture the key aspects of the real-world data.

Table C.2: Estimation results for the effects of 25% higher BL on inflows in 12-month PBD counties with observed and generated data

Method:	Flexible Model			Naive RD		
Dependent variable:	Log inflows into unemployment					
	Actual data (1)	Generated, no msmt. error (2)	Generated, msmt. error (3)	Actual data (4)	Generated, no msmt. error (5)	Generated, msmt. error (6)
Share treated	0.2867***	0.2914***	0.2889***	0.1877***	0.2914***	0.1810***
	(0.0228)	(0.0079)	(0.0150)	(0.0160)	(0.0079)	(0.0109)
Contributory years	-0.2453***	-0.2514***	-0.2565***	-0.1475***	-0.2514***	-0.1574***
	(0.0233)	(0.0082)	(0.0139)	(0.0181)	(0.0082)	(0.0116)
Share treated $\times$ contributory years	0.0269	0.0063	0.0170	-0.0649***	0.0063	-0.0687***
	(0.0258)	(0.0118)	(0.0162)	(0.0245)	(0.0118)	(0.0163)
Observations	3,712	3,712	3,712	3,712	3,712	3,712

Notes: Estimates for effects of benefit level increases on log inflows, equivalent to those reported in Table 4. The number of inflows are aggregated in bins of 0.01 years at the annual level. The first three columns show results for the model estimated in the main part of the paper. Column (1) equals the result in Table 4. Columns (4)-(6) are based on a classic RD, where the share of treated individuals is assumed to be one on the right-hand side of the cut-off and zero otherwise. For the generated data without measurement error, columns (2) and (5), the two models are equivalent since the share of treated individuals within a bin of the continuous variable is zero or one in the absence of measurement error. Standard errors are robust. Significance levels: $^{*} < 10\%$ $^{**} < 5\%$ $^{***} < 1\%$.

D Derivation of Formulas for Welfare Effects

D.1 Welfare effects of changes in the UI system

Similar to Schmieder and von Wachter (2016), we derive the welfare effect of a small increase in the benefit level in the steady state by differentiating (11) with respect to b:

$$\frac{dW}{db} = u_b \nu'(c_b) - e v'(c_e) \frac{d\tau}{db} \tag{D.2}$$

Due to the envelope theorem, changes in e, u_b, and u_x have no first-order impact on welfare. Labor market behavior, i.e., separation and job finding rates, are a function of UI generosity defined by parameters P and b. Differentiating the government budget constraint (13) with respect to b and rearranging yields

$$-e\frac{d\tau}{db} + u_b = -(b\frac{du_b}{db} - \frac{de}{db}\tau), \tag{D.3}$$

where we assume that taxes are increased in order to balance the budget (instead of making social assistance less generous), i,e. $\frac{d\tau}{db} \neq 0$. Divide (D.2) through $v'(c_e)$, add $u_b - u_b$ on the right-hand side and substitute (D.3) to obtain

$$\frac{dW}{db}\frac{1}{v'(c_e)} = u_b \frac{\nu'(c_b) - v'(c_e)}{v'(c_e)} - \left(b\frac{du_b}{db} - \frac{de}{db}\tau\right). \tag{D.4}$$

Following Chetty (2008), Schmieder et al. (2012), and Schmieder and von Wachter (2016), we divide through u_b in order to obtain the marginal effect on welfare of increasing the transfers to benefit recipients by \$1:

$$\frac{dW}{db}\frac{1}{u_b v'(c_e)} = \underbrace{\frac{\nu'(c_b) - v'(c_e)}{v'(c_e)}}_{\text{Social value of \$1 add. transfer}} - \underbrace{\left(b\frac{du_b}{db} - \frac{de}{db}\tau\right)\frac{1}{u_b}}_{\text{Behavioral cost per \$1 add. transfer}},$$

which is equation (14) in the main paper.

This equation is essentially the Baily-Chetty-Formula, but accounts for the fact that not all unemployed receive UI benefits. Similarly, we can obtain the welfare effect of transferring 1\$ to transfer recipients by increasing the potential benefit duration P.

The formula for the welfare effect of an increase in the PBD, P, is similar in structure. It is obtained by differentiating the social welfare function and the government budget constraint w.r.t. P, the PBD:

$$\frac{dW}{dP} = \left.\frac{du_b}{dP}\right|_M b\nu'(c_{u,t>P}) - e v'(c_e)\frac{d\tau}{dP} \tag{D.5}$$

$$-e\frac{d\tau}{dP} + \left.\frac{du_b}{dP}\right|_M b = -\left(b\left.\frac{du_b}{dP}\right|_B - \frac{de}{dP}\tau\right), \tag{D.6}$$

where $\left.\frac{du_b}{dP}\right|_M = \left.\frac{dD_b}{dP}\right|_M \times \frac{u}{D}$ indicates the mechanical increase in the stock of benefit recipients, i.e. the increase in the stock of benefit recipients due to the change in P holding the survival function in unemployment constant. For marginal changes in P, $\left.\frac{dD_b}{dP}\right|_M$ is simply the benefit exhaustion rate.[35]

$\left.\frac{du_b}{dP}\right|_B$ indicates the increase in benefit recipients due to behavioral reactions. Similarly, Schmieder et al. (2012) decompose the increase in average benefit durations into a mechanical and a behavioral component.[36] For instance, when the PBD is increased from 6 to 7 months, it is the stock of unemployed who have been unemployed for more than 6 and up to 7 months. $\nu'(c_{u,t>P})$ is the marginal utility of consumption of exhaustees.

Add $\left.\frac{du_b}{dP}\right|_M bv'(c_e) - \left.\frac{du_b}{dP}\right|_M bv'(c_e)$ on the right-hand side of (D.5), divide through $\left.\frac{du_b}{dP}\right|_M bv'(c_e)$, and substitute (D.6) to obtain

$$\frac{dW}{dP}\frac{1}{\left.\frac{du_b}{dP}\right|_M bv'(c_e)} = \underbrace{\frac{\nu'(c_{u,t>P}) - v'(c_e)}{v'(c_e)}}_{\text{Social value of \$1 add. transfer}} - \underbrace{\frac{1}{\left.\frac{du_b}{dP}\right|_M}\left(\left.\frac{du_b}{dP}\right|_B - \frac{de}{dP}\frac{\tau}{b}\right)}_{\text{Behavioral cost per \$1 add. transfer}},$$

equation (16) in the main paper.

D.2 Relating job finding rates to durations

We want to express aggregate job finding rates—which determine the steady state stocks of unemployment, benefit receipt and employment—in terms of unemployment durations. Similarly to Schmieder et al. (2012), we first write the average unemployment duration in terms of survival functions. Denote by f_j the job finding rate in period j of an unemployment spell, i.e., unemployment spells start in period $j = 0$. Then the average unemployment duration is $D = \sum_{j=0}^{\infty} S_j$, where S_j is the survivor function at the start of period j, with $S_0 = 1$ and $S_j = \prod_{g=1}^{j}(1 - f_{g-1})$ for $j > 0$. Suppose that inflows into unemployment are somewhat constant. Then the aggregate job finding rate is

$$f \approx \sum_{j=0}^{\infty} \frac{S_j}{D} f_j, \tag{D.8}$$

i.e. the average over all f_j, weighted by the share of unemployed in their jth period of unemployment, $\frac{S_j}{D}$. Equation (D.8) can be written as $f \approx \frac{1}{D}\sum_{j=0}^{\infty} S_j f_j$. The term $\sum_{j=0}^{x} S_j f_j$ is the failure

[35]For instance, if the PBD is 6 months and one third of benefit recipients exhaust benefits, then increasing the PBD by one day, one third of benefit recipients will gain another day of receipt. The average benefit duration thus increases by one third of a day.

[36]The change in the average benefit duration caused by an increase in the PBD from P^0 to P^1 can be decomposed as follows:

$$\frac{dD_b}{dP} = \sum_{j=0}^{P^1} S_j^1 - \sum_{j=0}^{P^0} S_j^0 = \left(\sum_{j=0}^{P^1} S_j^1 - \sum_{j=0}^{P^1} S_j^0\right) + \left(\sum_{j=0}^{P^1} S_j^0 - \sum_{j=0}^{P^0} S_j^0\right). \tag{D.7}$$

The first term is the behavioral component $\left.\frac{dD_b}{dP}\right|_B$ and the second is the mechanical component $\left.\frac{dD_b}{dP}\right|_M$.

function in the xth period of unemployment. For $x = \infty$, it necessarily equals one. Therefore,

$$f \approx \frac{1}{D} \tag{D.9}$$

and by the same argument,

$$f_b \approx \frac{1}{D_b}. \tag{D.10}$$

f_b denotes the exit rate from benefit receipt either because of benefit exhaustion (it is one at the exhaustion point) or because of the end of the non-employment spell.

D.3 Relating steady-state equations to empirical estimates of effects on policy changes on durations and separations

In this subsection, we derive the fiscal cost of increasing UI generosity. To this end, we need to relate equations (14) and (16) to the effects of changes in UI generosity on the numbers of benefit recipients, exhaustees, and employed. We assume that changes in job search effort do not impact labor market tightness, such that the job finding rate per unit of search effort is constant as in Hall (2005), in line with empirical evidence for Poland (Jessen et al., 2024).

Special case with fixed inflows into unemployment Most of the literature abstracts from separations and considers the case of a worker who has become unemployed (Chetty, 2008; Schmieder and von Wachter, 2016). The resulting equations for welfare effects are equivalent to a special case in our model with exogenous inflows into unemployment, $i = \delta \times e$. In order to express welfare effects in terms of duration elasticities, we use the fact that on aggregate $f = 1/D$, where D is the average unemployment duration.

We denote the aggregate exit rate from benefit receipt as $f_b = 1/D_b$.[37] Using $\frac{df_b}{db} = -f^2 \frac{dD}{dB}$, the derivatives of the steady-state stocks are

$$\frac{du}{db} = -\frac{i}{f^2}\frac{df}{dB} = i\frac{dD}{db}, \tag{D.11}$$

$$\frac{de}{db} = -i\frac{dD}{db}, \tag{D.12}$$

$$\frac{du_b}{db} = i\frac{dD_b}{db}, \tag{D.13}$$

where D_b is the average duration of benefit receipt.

Now substitute the formulas for the steady state values as well as (D.11), (D.12), and (D.13) into (14) to obtain

$$\frac{dW}{db}\frac{1}{u_b v'(c_e)} = \underbrace{\frac{\nu'(c_b) - v'(c_e)}{v'(c_e)}}_{\text{Social value of \$1 add. transfer}} - \underbrace{\left(\eta_{D_b,b} + \eta_{Du,b}\frac{D}{D_b}\frac{\tau}{b}\right)}_{\text{Behavioral cost per \$1 add. transfer}}, \tag{D.14}$$

[37] Steady-state values can be written in terms of the unemployment exit rate and the inflow into unemployment as $u = i/f$, $e = (f - i)/f$, $u_x = i(1 - f)^P/f$, and $u_b = i(1 - (1 - f)^P)/f = i/f_b$.

where $\eta_{Du,b} = \frac{dD}{d_b}\frac{b}{D}$ and $\eta_{D_b,b} = \frac{dD_b}{d_b}\frac{b}{D_b}$. Reassuringly, (D.14) is equivalent to Schmieder and von Wachter (2016, eq. 7).

The changes in steady state stocks due to changes in the PBD are equivalent to those due to changes in the benefit level. In particular, $\frac{du_b}{dP}\Big|_M = \frac{dD_b}{dP}\Big|_M \times i$ and $\frac{du_b}{dP}\Big|_B = \frac{dD_b}{dP}\Big|_B \times i$. Then we can write

$$\frac{dW}{dP}\frac{1}{\frac{du_b}{dP}\Big|_M bv'(c_e)} = \underbrace{\frac{\nu'(c_{u,t>P}) - v'(c_e)}{v'(c_e)}}_{\text{Social value of \$1 add. transfer}} - \underbrace{\frac{1}{\frac{dD_b}{dP}\Big|_M}\left(\frac{dD_b}{dP}\Big|_B + \frac{dD}{dP}\frac{\tau}{b}\right)}_{\text{Behavioral cost per \$1 add. transfer}} . \tag{D.15}$$

Again, this formula is equivalent to Schmieder and von Wachter (2016, eq. 8).

General case with endogenous separations:

BC/MC of an increase in benefit level In the steady state, outflows from benefit receipt equal inflows, $f_b u_b = \delta \times e$ and thus the stock of benefit recipients is given by

$$u_b = \frac{\delta \times e}{f_b} = \frac{\delta \times f}{f_b(f+\delta)} = \frac{f}{f_b}u = \frac{D_b}{D}u. \tag{D.16}$$

The effects of an increase in b on u is obtained using the quotient rule and simplifying:

$$\frac{du}{db} = \frac{\frac{d\delta}{db}f - \frac{df}{db}\delta}{(\delta+f)^2} = \frac{\delta}{\delta+f}\frac{f}{\delta+f}\frac{\eta_{u,b}+\eta_{\delta,b}}{b} = u \times e\frac{\eta_{u,b}+\eta_{\delta,b}}{b}, \tag{D.17}$$

where $\eta_{\delta,b} = \frac{d\delta}{b}\frac{b}{\delta}$, and the effect on e is simply

$$\frac{de}{db} = -\frac{du}{db}. \tag{D.18}$$

The effect of an increase in b on u_b is given by:

$$\begin{aligned}
\frac{du_b}{db} &= \frac{\left(\frac{d\delta}{db}f + \frac{df}{db}\delta\right) f_b(f+\delta) - \delta f\left(\frac{df_b}{db}(f+\delta) + f_b(\frac{df}{db} + \frac{d\delta}{db})\right)}{(f_b(\delta+f))^2} \\
&= \frac{1}{(f_b(\delta+f))^2} \times \left(\frac{d\delta}{db}(ff_b(f+\delta) - \delta f f_b) + \frac{df}{db}((\delta f_b(f+\delta) - \delta f f_b) - \frac{df_b}{db}\delta f(f+\delta)\right) \\
&= \frac{1}{(f_b(\delta+f))^2} \times \left(\eta_{\delta,b}\frac{\delta}{b}(ff_b(f+\delta) - \delta f f_b) - \eta_{D,b}\frac{1}{bD}((\delta f_b(f+\delta) - \delta f f_b)\right. \\
&+ \left.\eta_{D_b,b}\frac{1}{bD_b}(\delta f(f+\delta))\right) \\
&= \eta_{\delta,b}\frac{\delta}{f_b b}\frac{f}{\delta+f}\frac{f}{\delta+f} - \eta_{D,b}\frac{1}{b}(\frac{f_b\delta^2 f}{(f_b(\delta+f))^2}) + \eta_{D_b,b}\frac{\delta}{\delta+f}\frac{D_b}{bD} \\
&= \eta_{\delta,b}\frac{\delta}{f_b b}e^2 - \eta_{D,b}\frac{D_b}{Db}u^2 + \eta_{D_b,b}\frac{u_b}{b}.
\end{aligned} \tag{D.19}$$

Note that the second term in the last line is negative; an increase in the unemployment duration, keeping the duration of benefit receipt constant, lowers the number of benefit recipients because it reduces the number of employees—who in turn become transfer recipients once they become unemployed.

Now substitute (D.18) and (D.19) into (14) to obtain the formula for the welfare effect of an increase in the benefit level

$$\frac{dW}{db}\frac{1}{u_b v'(c_e)} = \underbrace{\frac{\nu'(c_b)-v'(c_e)}{v'(c_e)}}_{\text{Social value}} - \underbrace{\left(b\left(\eta_{\delta,b}\frac{\delta}{f_b b}e^2 - \eta_{D,b}\frac{D_b}{Db}u^2 + \eta_{D_b,b}\frac{u_b}{b}\right) + u \times e(\eta_{D,b}+\eta_{\delta,b})\frac{\tau}{b}\right)\frac{1}{u_b}}_{\text{Behavioral cost}}. \tag{D.20}$$

The first part of the behavioral cost is the increase in transfers paid and the second part is the loss in tax revenue. Using (D.16), we can rewrite the behavioral cost as

$$BC/MC = \eta_{\delta,b} e\left(\frac{\tau}{b}\frac{D}{D_b}+1\right) + \eta_{D,b}\frac{D}{D_b}\left(\frac{e\tau}{b} - u_b\right) + \eta_{D_b,b}. \tag{D.21}$$

The equation can be rewritten as

$$BC/MC^B = \eta_{\delta,b} + \eta_{D_b,b} - \eta_{D,b} u_b + e\frac{\tau}{b}\frac{D}{D_b}\left(\eta_{\delta,b}+\eta_{D,b}\right),$$

equation (15) in the main paper. The first and second term capture the increase in the stock of benefit recipients due to an increase in the separation rate and the benefit duration. The third term is the decrease in the stock of benefit recipients due to an increase in the unemployment duration (given the average benefit duration). The final term captures fiscal cost because the share of employed decreases, both due to an increase in the unemployment duration and an increase in the separation rate.[38]

BC/MC of a PBD extension To obtain the behavioral cost of an increase in the PBD, again we allow for the separation rate and job finding rates to depend on the PBD:

$$BC/MC = \frac{1}{\left.\frac{du_b}{dP}\right|_M}\left(\left.\frac{du_b}{dP}\right|_B + u \times e(\eta_{D,P}+\eta_{\delta,P})\frac{1}{P}\frac{\tau}{b}\right) \tag{D.23}$$

[38] To fix ideas, consider the case, where all unemployed receive transfers infinitely, $u = u_b$. Then $\eta_{D_b,b} = \eta_{D,b}$ and $D = D_b$. We get

$$BC/MC^B = \eta_{\delta,b} e\left(\frac{\tau}{b}+1\right) + \eta_{D,b} e\left(\frac{\tau}{b}+1\right). \tag{D.22}$$

The change in the stock of benefit recipients due to behavioral adjustments is $\left.\frac{du_b}{dP}\right|_B = \eta_{\delta,P}\frac{\delta}{f_b P}e^2 - \eta_{D,P}\frac{D_b}{DP}u^2 + \left.\frac{dD_b}{dP}\right|_B \frac{u_b}{D_b}$, where $\left.\frac{dD_b}{dP}\right|_B$ is the change in the average benefit duration due to behavioral adjustments. Substituting into (D.23), we obtain

$$BC/MC^P = \frac{1}{\left.\frac{du_b}{dP}\right|_M}\left(\eta_{\delta,P}\frac{\delta}{f_b P}e^2 - \eta_{D,P}\frac{D_b}{DP}u^2 + \left.\frac{dD_b}{dP}\right|_B \frac{u_b}{D_b} + u \times e(\eta_{D,P}+\eta_{\delta,P})\frac{1}{P}\frac{\tau}{b}\right), \tag{D.24}$$

which, using $\left.\frac{du_b}{dP}\right|_M = \left.\frac{dD_b}{dP}\right|_M \times \frac{u}{D}$, can be rearranged to

$$BC/MC^P = \frac{1}{\left.\frac{dD_b}{dP}\right|_M}\left(\frac{d\delta}{dP}\frac{D_b e}{\delta} + \left.\frac{dD_b}{dP}\right|_B - \frac{dD}{dP}u_b + e\frac{\tau}{b}\left(\frac{d\delta}{dP}\frac{D}{\delta} + \frac{dD}{dP}\right)\right).$$

The structure of the equation is similar to (15). The first and second term in the outer parenthesis denote the increased benefit payments due to the increase in the stock of recipients due to an increase in the job destruction rate and a behavioral increase in the benefit duration. The third term is negative. It is the decrease in the stock of benefit recipients due to an increase in the average unemployment duration given the benefit duration. The final term captures the decrease in tax revenue due to a reduction in the stock of employed caused by an increase in the PBD. The equation can also be written in terms of elasticities of the job destruction rate, equation (17) in the main paper:

$$BC/MC^P = \frac{1}{\left.\frac{dD_b}{dP}\right|_M}\left(\eta_{\delta,P}\frac{D_b}{P}e + \left.\frac{dD_b}{dP}\right|_B - \frac{dD}{dP}u_b + e\frac{\tau}{b}\left(\eta_{\delta,P}\frac{D}{P} + \frac{dD}{dP}\right)\right)$$

D.4 Equations with two-step unemployment system

In Poland, the benefit level is higher in the first three months of benefit receipt since 2010. In this case the formulas for the welfare effects of changes in UI differ slightly from those in the standard case.

The government budget constraint can be written as

$$G + bu_b + b(1+\alpha)u_{b_A} = e\tau, \tag{D.25}$$

where u_{b_A} is the stock of recipients who receive benefits that are higher by a factor of $1+\alpha$.

The derivative of the budget constraint with respect to b is

$$-e\frac{d\tau}{db} + u_b = -\left(b\left(\frac{du_b}{db} + \alpha\frac{du_{b_A}}{db}\right) - \frac{de}{db}\tau\right). \tag{D.26}$$

We assume that the consumption level and utility functions of benefit recipients receiving higher benefit levels are the same as those for benefit recipients with lower levels. Moreover, we assume that the elasticities of the durations of receiving higher or lower benefit are the same,

$\eta_{D_b,b}$. Then we can write

$$\frac{dW}{db}\frac{1}{(D_b+\alpha D_{b_A})v'(c_e)} = \underbrace{\frac{\nu'(c_b)-v'(c_b)}{v'(c_e)}}_{\text{Social value of \$1 add. transfer}} - \underbrace{\left(\eta_{D_b,b} + \times \eta_{D,b}\frac{D}{D_b+\alpha D_{b_A}}\frac{\tau}{b}\right)}_{\text{Behavioral cost per \$1 add. transfer}}. \tag{D.27}$$

$(D_b + \alpha D_{b_A})b$ is simply the average benefit level times the average benefit duration. Write $(D_b + \alpha D_{b_A})b = (D_b + \alpha\beta D_b)b$ and then $(D_b + \alpha D_{b_A})b = D_b(1+\alpha\beta)b = D_b\bar{b}$. Then we have

$$\frac{dW}{d\bar{b}}\frac{1}{D_b v'(c_e)} = \underbrace{\frac{\nu'(c_b)-v'(c_b)}{v'(c_e)}}_{\text{Social value of \$1 add. transfer}} - \underbrace{\left(\eta_{D_b,b} + \times \eta_{Du,b}\frac{D}{D_b}\frac{\tau}{\bar{b}}\right)}_{\text{Behavioral cost per \$1 add. transfer}}. \tag{D.28}$$

References

Calonico, S., M. D. Cattaneo, and M. H. Farrell (2020): "Optimal bandwidth choice for robust bias-corrected inference in regression discontinuity designs," *The Econometrics Journal*, 23, 192–210.

Chetty, R. (2008): "Moral hazard versus liquidity and optimal unemployment insurance," *Journal of Political Economy*, 116, 173–234.

Guvenen, F., F. Karahan, S. Ozkan, and J. Song (2021): "What do data on millions of US workers reveal about lifecycle earnings dynamics?" *Econometrica*, 89, 2303–2339.

Hall, R. E. (2005): "Employment fluctuations with equilibrium wage stickiness," *American Economic Eeview*, 95, 50–65.

Jessen, J., R. Jessen, E. Gałecka-Burdziak, M. Góra, and J. Kluve (2024): "The Micro and Macro Effects of Changes in the Potential Benefit Duration," *Ruhr Economic Papers #1119*.

Pischke, S. (2007): "Lecture notes on measurement error," *London School of Economics, London.*

Schmieder, J. F. and T. von Wachter (2016): "The effects of unemployment insurance benefits: New evidence and interpretation," *Annual Review of Economics*, 8, 547–581.

Schmieder, J. F., T. Von Wachter, and S. Bender (2012): "The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over 20 years," *The Quarterly Journal of Economics*, 127, 701–752.