Short-Time Work Extensions*

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Abstract

Governments use short-time work (STW) schemes to subsidize job retention. A key policy lever during crises is the extension of potential benefit duration (PBD)—how long firms can receive STW subsidies. Using unique German administrative data from 2009 to 2021, we show that separations rise and employment falls when firms exhaust benefits. Yet the uptick in separations at exhaustion almost entirely reflects job-to-job moves rather than transitions into unemployment, even in slack labor markets, consistent with STW delaying reallocation rather than preventing unemployment. We develop a model to analyze exogenous shifts in PBD and show that wage flexibility can substitute for longer benefit duration. Exploiting a 2012 reform that doubled the PBD from six to twelve months, we find no evidence that the extension prevented unemployment and, if anything, reduced reallocation to other firms. In line with the model, firms without extensions reduced wages relative to those with extended support, and across labor market cells, larger wage adjustments coincide with smaller employment losses. Our findings imply that STW extensions operated mainly as wage subsidies rather than job-saving measures.

JEL classifications: J01, J08, J30, J41
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1 Introduction

Short-time work (STW) schemes are a widely used policy tool to preserve jobs during economic downturns (Giupponi, Landais, and Lapeyre, 2022; Giupponi and Landais, 2023; Cahuc, 2024). These schemes provide subsidies to firms so that they reduce employee hours instead of laying off workers. As such, STW could be a substitute for unemployment insurance by insuring jobs rather than workers. Both provide countercyclical insurance, but instrument choice varies geographically: US policymakers systematically extend the potential benefit duration (PBD) of unemployment insurance during recessions, while Germany and other European countries keep unemployment insurance duration relatively stable but instead adjust STW PBD as their primary crisis response.[1]

Existing work has studied the extensive-margin effects of STW (see, e.g., Cahuc, Kramarz, Nevoux, and Vieira, 2021; Kopp and Siegenthaler, 2021; Giupponi and Landais, 2023), but the (intensive-margin) effects of extending the PBD on employment and wages remain an open question. Importantly, little is known about whether and how STW interacts with risk- and rent-sharing arrangements (e.g., Guiso, Pistaferri, and Schivardi, 2005; Nimier-David, Sraer, and Thesmar, 2023) and decentralized wage bargaining institutions. Understanding this is key because if firms can flexibly trade off wages against employment stability through such channels, longer STW durations may have limited employment effects. This paper provides answers by leveraging novel microdata to study STW and PBD extensions in Germany, the homeland of short-time work.

We start by studying the exhaustion of STW benefits and changes in PBD using novel administrative data and institutional variation in Germany, where the government can adjust STW parameters by decree. Our analysis draws on newly assembled administrative records covering monthly STW receipt for the universe of establishments in Germany since 2009, the first representative dataset tracking firm-level participation in STW over time. We link these data to longitudinal employer-employee information from social security records, firm financial accounts, and local labor market information, yielding an integrated dataset that allows us to trace benefit exhaustion, employment dynamics, and wage adjustments within and across firms over multiple years, local labor market conditions, and firm-level financial conditions.

1 During the COVID-19 pandemic, for example, France introduced a new STW scheme with up to 48 months PBD, Switzerland extended PBD from 12 to 24 months, and Germany from 12 to 28 months, with the costs of short-time work in Germany in 2020 alone estimated at about 22.1 billion EUR (Bundesrechnungshof, 2022).

In a first step, we compare firms that exhausted STW benefits to firms that left the STW program before benefits ran out. Our exercise is similar in spirit to the analysis in Ganong and Noel (2019) who study unemployed job seekers who leave unemployment before or after exhausting benefits. At the end of STW, exhausting firms begin to shrink employment at a higher rate—both compared to their shrinkage rate before exhaustion and compared to firms who leave STW before exhaustion. This shrinkage is driven by a sharp increase in workers' exit rates from the firm at exhaustion rather than changes in the hiring rate. Importantly, the increase in workers' exit rates is largely accounted for by increases in job-to-job transitions rather than by increases in transitions into unemployment.

As governments extend PBD typically during labor market downturns, we probe this result further by considering "crisis" labor markets with particularly high unemployment rates. We find that workers at firms exhausting STW benefits during periods of high external unemployment experience no significant increase in their probability of becoming unemployed—either compared to their own pre-exhaustion risk or compared to workers whose firms exhaust benefits during low-unemployment periods. Our analysis of exhausters suggests that longer STW delays worker reallocation to other firms rather than preventing unemployment.

To understand the effects of exogenous shifts in STW PBD, we develop a stylized model with mutual-consent bargaining. In the model, firms have access to STW benefits in case of negative productivity shocks and can renegotiate wages to prevent layoffs. The model predicts that firms facing shocks can utilize STW to avoid wage cuts if benefit durations are extended, or they need to negotiate wage concessions to prevent layoffs if benefit durations are short. Put differently, flexible wage bargaining can substitute for a longer PBD through downward wage adjustments. Through the lens of the model, STW extensions constitute wage subsidies rather than job-saving measures.

To shed light on the actual employment and wage effects of extending PBD, we exploit a unique policy reform in Germany that unexpectedly doubled the PBD from 6 to 12 months in December 2012. The backward-binding nature of this reform generated quasi-experimental variation in STW PBD across firms that had already started using short-time work earlier in 2012. For firms that had started spells after June 2012, the December 2012 reform extended their benefit duration, while for those starting just before, it did not.

We exploit this sharp policy change in a regression discontinuity design comparing workers at

firms on either side of the reform's timing cutoff. Extending the PBD did not lower non-employment after one year for workers in treated firms with point estimates ruling out small effects (-0.3 p.p., s.e. = 1 p.p.). We find small though statistically insignificant effects on employment at the original firm (2.8 p.p., s.e. = 3 p.p.). As such, the probability of employment at other firms is lower (at 0.3 p.p.), consistent with the extension preventing some reallocation of workers to other employers rather than reducing non-employment. Across worker groups—by tenure, age, education, or wage level—we find at most small and statistically insignificant effects.

To test to what extent flexible wage setting and (efficient) bargaining may have prevented layoffs in control group firms with a shorter PBD, we investigate effects on wage growth. We find substantial and positive wage effects of STW extensions, with treated firms' wage growth exceeding that at control firms by up to 5.9 percentage points over the span of four years. To shed further light on the role of wage flexibility, we split our sample into cells based on sector, region and size, and calculate cell-specific treatment effects on wages and employment. We find a negative relationship between cell-specific treatment effects on wages and employment: firms that can reduce wage growth in response to negative shocks lay off fewer workers, in line with wage flexibility preventing layoffs.

Our results imply that control firms with a shorter PBD insure their employees (as they also do in other contexts, e.g., Guiso, Pistaferri, and Schivardi, 2005; Lagakos and Ordoñez, 2011; Ellul, Pagano, and Schivardi, 2017; Adamopoulou, Manaresi, Rachedi, and Yurdagul, 2025) at the expense of relatively lower wages. To bolster our evidence for this mechanism, we explore firm-level heterogeneity in firms' responses to exogenous variation in PBD. The wage declines are larger for firms in regions with higher local unemployment, and for those with worse access to liquidity. We also investigate the role of works councils in mediating employment effects, and find that they effectively substitute for firms' employment response under shorter PBD (see Budde, Dohmen, Jäger, and Trenkle, 2024, for the effect of works councils on employment protection).

In a final step, we use individual-level data from 2020 and 2021 to analyze the targeting of STW benefits and individual-level effects of take-up. Firms use STW for workers with lower predicted retention probabilities. STW take-up predicts higher retention, even conditional on predicted retention (based on lagged observables). However, rather than preventing unemployment, the additional retention comes with lower employment at other firms, consistent with hindered reallocation.

Overall, our evidence points to the crucial role of wage adjustments in shaping the employment

response to labor market policies. In the German context, wages regularly appear to be flexible enough to prevent layoffs and unemployment (Hartung, Jung, and Kuhn, 2025). In general, an institutional environment with decentralized wage setting can prevent layoffs if firms and workers can efficiently negotiate over wages (Jäger, Schoefer, Young, and Zweimüller, 2020; Jäger, Schoefer, and Zweimüller, 2023). The institutional environment thus appears to substitute for the policy response of STW extensions.

Our evidence on wage rigidity as a key mediator of the effect of STW extensions, as well as our model results, could also explain why the employment effects we estimate qualitatively differ from the ones in other contexts. For example, comparing Italy and Germany—the settings of Giupponi and Landais (2023) and our study, respectively—there are large differences in wage rigidity and decentralization of bargaining between the two countries (as documented by Boeri, Ichino, Moretti, and Posch, 2021). The intra-German heterogeneity in employment and wage effects of STW extensions that we document thus helps to understand the overall small employment effects as a consequence of greater wage flexibility.

Our paper contributes to several strands of literature. A recent set of design-based research has used policy reforms to estimate the employment effects of STW programs (Cahuc, Kramarz, Nevoux, and Vieira, 2021; Kopp and Siegenthaler, 2021; Giupponi and Landais, 2023). This literature has focused on the extensive margin of STW program introduction or eligibility. In contrast, our paper focuses on a key policy lever that governments use in crises—adjustments of potential benefit duration. We also present evidence of firms targeting STW towards workers with low retention probability in the absence of STW participation.

In addition, our paper relates to the macroeconomic literature evaluating the aggregate effects of STW policies, including Cahuc and Carcillo (2011), Hijzen and Martin (2013), Balleer, Gehrke, Lechthaler, and Merkl (2016), and Stiepelmann (2025). We provide the first quasi-experimental estimate of how changes in the PBD—a primary policy tool for regulating the generosity of STW schemes—affect employment and wage outcomes. While an extensive literature studies the effects of adjusting PBD for unemployment insurance (see Schmieder and Von Wachter, 2016, for an overview), including more recent evidence on heterogeneous effects of UI extensions across different initial durations (Acosta, Mueller, Nakamura, and Steinsson, 2024), ours is the first design-based estimate for the understudied yet quantitatively important policy lever of STW extensions.

Our work also contributes to the literature by providing the first comprehensive analysis of Germany's STW scheme combining novel administrative data on the universe of firms participating in STW matched with employer-employee data and firm financials. Despite Germany being the largest OECD economy with a significant STW scheme and, in fact, the birthplace of STW schemes (Cahuc, 2024), previous microeconometric work on the German STW program largely relied on surveys to measure STW take-up, with the exception of one innovative study drawing on administrative data on STW take-up from the city of Nuremberg (Tilly and Niedermayer, 2016).

The remainder of the paper is organized as follows. Section 2 describes the institutional context of STW in Germany. Section 3 introduces the datasets we use for our analysis alongside descriptive evidence on take-up and selection into short-time work both at the individual and at the establishment level. Section 4 provides further descriptive evidence on employment effects when firms exhaust their maximum benefit duration. Section 5 discusses a model of how shifts in STW PBD affect employment and wages. Section 6 presents evidence on employment and wage effects of exogenously varying the PBD of short-time work benefits, and the role of decentralized bargaining and wage flexibility. Section 7 presents evidence on within-firm targeting of STW benefits. The last section concludes.

2 Short-Time Work in Germany: Institutional Context

We provide institutional background information on the STW scheme in Germany. The German STW scheme allows firms to temporarily reduce working hours, while the employment agency replaces a significant share of the gap in wages for affected employees. The regular replacement rate is 60% of net wages (67% for employees with children). Once admitted to the program, firms decide every month on the reduction of working hours per employee, and pay wages for hours worked as well as STW benefits to employees. After handing in detailed documentation (*Abrechnungslisten*), firms are reimbursed for the STW benefits by the employment agency. Importantly, in firms with a works council, the works council has direct codetermination rights regarding the implementation of STW (§87 Abs. 1 Nr. 3 BetrVG).

Firms file an application for admission (*Anzeige*) to the STW scheme and need to meet certain eligibility criteria. First, the reduction in working hours must be temporary and due to economic

reasons or an unavoidable event. Second, other accommodating measures such as reducing working time accounts must have already been exhausted. Third, the shock must be sizeable enough such that at least one-third of the employees must each face a reduction in working hours of at least 10% (this was relaxed to 10% instead of one-third during the COVID-19 pandemic). Even after successful initial admission to the program, benefit claims are preliminary subject to a final examination at the end that determines whether all criteria were met (*Abschlussprüfung*).

The top panel of Figure 1 illustrates the share of all employees in establishments using STW and, as not all employees in STW establishments actually are in STW, also the share of employees in STW. At the height of the COVID-19 pandemic, approximately one-third of all employees worked in establishments using STW and half of them, i.e., about of one-sixth of all employees, were in STW. This is in line with Appendix Figure C.1, which depicts the intensity of STW use, as measured by the share of employees in STW in the starting month of a given establishment's STW spell. The respective distribution is almost uniform, and approximately one-quarter of all establishments used STW for at least 80% of their workforce.

Variation in the potential benefit duration (PBD). Firms may receive STW benefits for up until the PBD as part of one successful admission to the program (STW spell). Changes in the PBD are a key policy lever that governments use during economic downturns. Since 2009, the PBD has been adjusted multiple times, in particular during crises. We illustrate the PBD changes in the bottom panel of Figure 1 (see also our detailed description of the 2012 PBD reform in Section 6.1).[2]

3 Data

Our main data source is novel data on STW receipt at the establishment level starting in 2009, and on STW receipt at the individual level starting in 2020. We match the STW data to employer-

2 The government increased PBD during the financial crisis as well as during the COVID-19 pandemic. Formally, a law sets the default PBD (§104 SGB III); the federal government can temporarily increase PBD by federal ordinance "in case of exceptional circumstances in the labor market" (§109 (4) SGB III). Until the end of 2015 the default PBD set by law was 6 months. The government has temporarily increased PBD by executive ordinance multiple times (18m decided on November 26, 2008 (BGBl. I. S. 2332); 24m decided on May 29, 2009 (BGBl. I. S. 1223); 18m decided on December 8, 2009 (BGBl. I. S. 3855); 12m decided on December 1, 2010 (BGBl. I. S. 1823); prolongation extended on December 7, 2012 (BGBl. I. S. 2570); October 31, 2013 (BGBl. I. S. 3905) and November 13, 2014 (BGBl. I. S. 1749)). Since a change in the law in 2016, the default PBD has been 12 months. During the COVID-19 pandemic PBD has also been temporarily extended multiple times (final extension to 28m).

employee data based on German Social Security Records and supplement it with firm-level financial information from Bureau van Dijk (BvD) (see Jäger, Schoefer, and Heining, 2021; Moser, Saidi, Wirth, and Wolter, 2022; Kuhn, Luo, Manovskii, and Qiu, 2023, for recent work with BvD data matched with German administrative data) and regional labor market data (Kuhn, Manovskii, and Qiu, 2021). Below, we describe our four main data sources in detail.

Establishment-level information on monthly STW receipt. We use data on monthly STW receipt at the establishment level from 2009 to 2021. An establishment that has successfully been admitted to the STW program submits a detailed application every month for reimbursement by the employment agency. The data we use is compiled for statistical purposes by the Statistics of the Federal Employment Agency (*Statistik der Bundesagentur für Arbeit: Tabellen, Realisierte Kurzarbeit, Nürnberg, Oktober 2021, Daten mit einer Wartezeit von bis zu 5 Monaten (ohne Hochrechnung)*). The close link to the operational system upon which actual payment of benefits is based ensures high data reliability. The data includes monthly information on whether an establishment receives STW benefits, the number of short-time workers, and the wage bill gap, i.e., the difference between the regular wage bill and the reduced wage bill (incorporating hours changes due to STW).

We match this data with the Establishment History Panel (BHP, Ganzer, Schmucker, Stegmaier, and Wolter, 2022) which contains information on all establishments in Germany with at least one employee liable to social security as of June 30 each year. The match allows us to add information on the establishment's location, industry, and age. Details on the matching procedure are provided in Appendix A.2.

A STW spell is defined as the period of consecutive STW usage under the same application. A pause in STW receipt for one or two months is allowed and disregarded in the calculation of the spell's benefit duration. Throughout our analyses, we restrict attention to establishments that had not started another STW spell in the previous twelve months.

Individual-level information on monthly STW receipt. We additionally use novel data on individual-level STW receipt (*PKUG Personen in Kurzarbeit*). Since the employment agency reimburses employers for STW benefits paid to employees, the data compiled during the payment of benefits is at the establishment level, as described above. In their monthly applications (*Abrech-*

nungslisten), however, establishments list employees in STW and calculate their STW benefits step-by-step, documenting the wage gap and reduction in hours. In a unique data collection effort, these typically manual applications were digitalized for the period between March 2020 and December 2021 to link individuals in the applications to their employment biographies. To address challenges in the digitalization process, a thorough validation procedure cross-checked information with both establishment-level data and individual employment biographies for each month. The final dataset contains, for all individuals working at establishments using STW between March 2020 and April 2021, a monthly likelihood of being in STW after various cross-checks. The likelihood is categorized as 0%, small (0-20%), medium (20-50%), high (above 50%), and 100%. Details are provided in Appendix A.1. For our analysis, we consider an individual to be in STW if the likelihood is above 50%.

Matched employer-employee data. We combine the information on STW receipt with employee data based on German Social Security Records since 2008. The data stems from the Integrated Employment Biographies (IEB) database of the Institute for Employment Research. Specifically, the data is based on employers' reports to the German social insurance system and includes the start and end date of each job, employees' earnings up to the censoring limit at the social security maximum earnings limit, an indicator for part-/full-time employment, and data on education levels, occupation as well as demographic information. We use standard procedures to create cross sections of the data originally stored in spell format (Stüber, Dauth, and Eppelsheimer, 2023), transforming it into a monthly panel at the individual level (see details in Appendix A.3).

Firm-level financial information. We enrich our dataset on the policy variation of PBD with firm-level financial information from the commercial database Dafne, provided by Creditreform and Bureau von Dijk (BvD). Dafne contains financial information of German firms since 2008 and is the underlying source for data on German firms in BvD's Orbis dataset. Appendix A.5 summarizes how we assemble and clean the firm-level financial data. We draw on a link of establishments to firms using the record linkage key Orbis-ADIAB (Antoni, Koller, Laible, and Zimmermann, 2018) and focus on establishments that can successfully be matched. Appendix Table D.1 shows characteristics of matched and unmatched establishments. Restricting attention to establishments

that can be linked to the firm level primarily excludes very small establishments with fewer than five employees for which average wages are inherently volatile by construction. For analyses at the firm level, we aggregate the establishment data to the firm level, restricting the sample to firms with more than five full-time employees who are fully liable to social security. We provide details on the aggregation procedure in Appendix A.4.

Local labor market information. In a final step, we rely on the workplace information for each establishment in the data to bring in local unemployment rates. The regional information in the social security data is at the county (*Kreis*) level, and we aggregate counties to commuting zones to describe local labor markets following Kuhn, Manovskii, and Qiu (2021).

3.1 Take-Up and Selection into STW

Before our main analysis, we document take-up and selection into STW, first, using data at the establishment level from 2009 to 2021 and, second, using establishment and individual-level data from 2020 and 2021.

Establishment-level evidence on take-up and selection. Table 1 presents summary statistics for users and non-users of short-time work at the establishment level and over different time periods. In particular, we consider the total time period with available data, 2009-2021, and dissect it into subperiods of interest, specifically the aftermath of the Global Financial Crisis (2009-2010), the European sovereign debt crisis (2011-2012), the COVID-19 pandemic (2020-2021, which matches the time period for which we have individual-level data), and the remaining years in between (2013-2019).

The analysis of establishment-level data from 2009 to 2021 reveals distinct differences between establishments with and without STW take-up. Averaged over the entire sample, STW users tended to be larger (42 vs. 34 employees) and slightly older (19.6 vs. 18.6 years) compared to non-users. Average daily wages were the same (89.4 €). STW users consistently exhibited negative employment growth in the year *preceding* STW take-up (-2.8 vs. 1.2 percentage points), indicating that STW was often implemented in the presence of an ongoing employment decline.

The statistics further show that STW users during the COVID-19 pandemic were different.

While the size gap between STW users and non-users was substantial in earlier periods (e.g., 63 vs. 32 employees in 2009/2010), it narrowed significantly during the pandemic (36 vs. 35 in 2020/2021). The wage pattern also dramatically reversed: in pre-pandemic periods, STW users generally had higher average daily wages, but in 2020/2021 non-users had higher wages compared to users (107.9 €vs. 90.2 €).

Looking at the average worker characteristics in establishments with and without STW usage, we find that over time the education composition of STW-using establishments shifted. In earlier periods, STW users had higher shares of middle-educated workers and lower shares of low-educated workers. However, this pattern inverted in 2020/2021, with STW users showing higher shares of low-educated workers (25% vs. 20%) compared to non-users. The age-distribution differences that were prominent in earlier periods (with STW users having smaller shares of young workers) largely disappeared in 2020/2021.

Notably, the scale of STW usage increased dramatically during the pandemic. The number of STW-using establishments rose from 30,415 in the period from 2013 to 2019 to 402,008 in 2020 and 2021. This substantial increase, combined with the changes in establishment characteristics, demonstrates the nature of the much broader adoption of STW during the pandemic across various establishment types, reflecting the widespread economic impact of COVID-19 rather than the more selective use seen in previous economic downturns. Despite these significant changes in STW take-up patterns during the COVID-19 period, one feature remained consistent with earlier periods: STW users continued to exhibit negative establishment growth in the preceding year compared to non-users with positive growth (-2.2 vs. 1.9 percentage points in 2020/2021), suggesting that STW continued to be taken up by firms with longer-running employment declines regardless of the broader economic context.

Individual-level evidence on take-up and selection. In Table 2, we turn to individual-level data for the COVID-19 period, and focus on establishments with STW take-up and differentiate between workers on STW vs. workers with no take-up.[3] For the summary statistics, we consider separately establishments with short-time work in April 2020 vs. any start month from April to

3 For this analysis, we focus on establishments with a high quality of individual-level STW data (see Appendix A.1 for details).

December 2020, capturing heterogeneity in how the crisis unfolded.

Within their establishment, workers on STW earned 9% lower daily wages (113.8 €vs. 124.6 €) and were less likely to have high-level education (19% vs. 22%). They were similarly represented across occupations but less frequently engaged in highly complex tasks (13% vs. 16%). There are no clear age patterns, but there is a slight skew towards workers with shorter job tenure. Overall, these patterns of STW take-up during the COVID-19 period are consistent with those at the establishment level (in the last two columns of Table 1). This suggests that despite some small differences, our individual-level data is representative of the respective period and can inform us about which workers were on STW and which ones were not.

4 What Happens at STW Exhaustion?

Our first step toward understanding the effects of STW extensions is to document what happens when firms exhaust their STW benefit entitlements. This analysis focuses on how employment and worker flows evolve, and offers a natural benchmark against which to interpret the effects of policy-induced extensions of potential benefit duration (PBD).

We identify exhaustion events by tracking, for each firm, the timing of when its ongoing STW spell reaches the maximum duration allowed under the prevailing rules as well as when a firm exits from STW receipt. We compare firms that exhaust their STW benefits to firms that exit the program before exhausting their entitlement.

Figure 2 nonparametrically plots various firm-level outcomes around the time of leaving the STW program, comparing firms that exited "early," i.e., before exhausting benefits, to "exhausters," who leave the program when they reach the maximum PBD. All outcomes are normalized relative to employment levels at the time of STW take-up. Our approach mirrors the event study design used in analyses of unemployment-benefit exhaustion (e.g., Ganong and Noel, 2019), applied here to firm outcomes and worker flows around the end of STW.

Firm size is declining at a stable rate of about 0.5 p.p. per month before leaving the STW program, both for early exiters as well as for exhausters (Figure 2, Panel (a)). At the time of leaving the program, early exiters gradually move back towards stable employment while firm growth turns sharply more negative for STW exhausters. Figure 2, Panels (b) and (c) document that this change

in employment is driven by a sharp increase in the workers' exit rate at exhaustion rather than any changes in the hiring rate which is flat (and positive at 1%) around the time firms leave the STW program, both for early exiters and exhausters.

To distinguish whether separations at exhaustion represent reallocation to other employers or match destruction into unemployment, we decompose exit flows into transitions to new employers and transitions into unemployment. We find that the rise in separations is almost entirely driven by job-to-job moves (Figure 2, Panel (d)). In contrast, the probability that a worker employed at an exhausting firm becomes unemployed rises by less than 0.4 percentage points (Figure 2, Panel (e)). These results suggest that STW primarily delays worker reallocation to other firms rather than preventing unemployment.

Finally, we examine whether transitions into unemployment at exhaustion differ across low- and high-unemployment local labor markets. Using commuting-zone unemployment rates, we split commuting zones into terciles of local unemployment and compare firms exhausting STW benefits in low- vs. high-unemployment labor markets. Figure 2, Panel (f), documents that the rate at which workers enter unemployment at the time their firm exhausts STW is flat around exhaustion, in both low- and high-unemployment labor markets. Taken together, our event study results indicate that when STW benefits run out, firms begin to shed workers, but separations overwhelmingly reflect job-to-job reallocation rather than unemployment, even in the highest-unemployment labor markets.

The next sections complement our event study analysis by studying exogenous shifts in PBD.

5 Model: Effects of Exogenous Shifts in PBD

To analyze the role of exogenous shifts in PBD and to provide a framework for our analysis in the next section, we develop a stylized model of STW that builds on the framework in Cahuc, Kramarz, Nevoux, and Vieira (2021). Reflecting our empirical setting, we consider firm-level variation in the potential benefit duration (PBD) of short-time work.

Our model differs from Cahuc, Kramarz, Nevoux, and Vieira (2021) in four respects. First, we distinguish between firms with a short and a long PBD of STW. Second, we assume mutual-consent, rather than Nash, bargaining for wages, so the last bargained wage becomes an additional state

variable to the problem and transitory shocks can have persistent wage effects (Postel-Vinay and Turon, 2010). Third, we allow for fixed costs that can lead to separations of firms after transitory shocks. Fourth, we also allow for downward wage rigidity and study its effects in combination with STW usage.

In the model, firms' ability to adjust wages provides an important margin to preserve jobs. In particular, we show that firms with a shorter PBD can retain employees in most situations to the same extent as do firms with prolonged PBD. Only for very large negative shocks, possibly matching the extent of the COVID-19 pandemic, we find differences in separation decisions between firms with long vs. short PBD. The introduction of wage rigidity allows us to highlight the importance of wage adjustments. We show that limited wage adjustment ability attenuates firms' possibilities to trade off wages against employment stability.

5.1 Model Setup

We focus on a firm that experiences a negative productivity shock and then has to decide on short-time work usage, wage adjustments, and layoffs. The firm operates in the following three-period environment.

Firms. The firm employs one worker who, in period t, works h_t hours and receives the current wage w_t. As we assume mutual-consent bargaining, the state variables of the firm's problem are the negotiated wage w_t and the current level of productivity A_t. The associated per-period profits are $\Pi_t = (A_t - w_t)h_t - \kappa_t$ with κ_t denoting the per-period fixed costs of production. We assume that in $t = 1$, the firm experiences a negative productivity shock relative to its normal productivity level $\bar{A}$ and needs one or two periods to recover from the shock. Specifically, we assume that the firm starts in period 1 with the "normal" wage level $\bar{w}$ and a random productivity draw $A \in [0, \bar{A})$. In $t = 3$, productivity will always have recovered such that $A_3 = \bar{A}$. In $t = 2$, there is a probability π that productivity recovers, and with probability $1 - \pi$ productivity remains persistently low at $A < \bar{A}$, so π parameterizes the persistence of the shock. Firms and workers discount the future at a common rate $\beta \in (0, 1)$.

Workers. An employed worker receives utility $U_t = w_t h_t - \psi(h_t)$, where $\psi(\cdot)$ captures the disutility from working. In unemployment, the worker receives flow utility b, and we assume that unemployment is an absorbing state. We assume that working hours can be flexibly set each period and that wages are negotiated with mutual consent (Postel-Vinay and Turon, 2010). Mutual-consent bargaining is relevant in periods 1 and 2 of the model when productivity is low and wages might need to be reduced to safeguard positive continuation values of firms.

To study the role of wage flexibility, we allow for a form of downward wage rigidity: we assume that matches that wish to adjust wages downward can only do so with probability α (e.g., Galí, 2011). As we consider negative productivity shocks in periods 1 and 2, these are the only two periods where such wage adjustments are relevant. As a benchmark, we consider a model without wage rigidity by setting $\alpha = 1$. Wage adjustments after a negative shock allow workers and firms to trade off employment stability against wages, as lowering the wage preserves a positive surplus for workers and firms from continuing the match and, thus, prevents layoffs.[4]

Short-time work. The firm has access to short-time work benefits, modeled similarly to Cahuc, Kramarz, Nevoux, and Vieira (2021). It receives a wage subsidy σ per reduced hour whenever the current hours worked h_t are below a threshold value $\bar{h}$, i.e., $\sigma(\bar{h} - h_t)$. We consider two scenarios. The firm either has access to STW in periods 1 and 2 (*long PBD*) or access in period 1 only (*short PBD*). The eligibility status is a fixed institutional parameter, so that firms know their eligibility status in each period starting from period 1.

We split each period into two stages: a separation stage and a production stage. At the separation stage, the match observes the current productivity realization and decides on match continuation. If the joint match surplus is positive and there is mutual consent to adjust wages to preserve a positive firm surplus and, therefore, to preserve match continuation, this will happen at the separation stage, so the adjusted wage will be paid out in the current period already. If some wages cannot be adjusted downward because of wage rigidity, i.e., if $\alpha < 1$, the match separates when the surplus of the firm is negative.

Before entering the production stage, the firm further decides on the use of STW. In period 1

4 We consider a model in partial equilibrium, but the underlying assumption is that free entry leads to a continuation value of zero for the firm with a vacancy.

all firms can rely on STW benefits, whereas in period 2 only firms with long PBD will have this option. If the match enters the production stage, it will pay the fixed costs κ, production takes place, and wages are paid out. If STW is used, the firm receives the transfers for short-time work benefits at this stage. Exogenous separations take place at the end of each period with exogenous separation probability ρ. We solve the model by backwards induction.

We assume that decisions at the separation stage consider the joint surplus of the match at the current productivity level A. Only if wages cannot be adjusted and the firm surplus is negative, matches also separate at a positive joint surplus. The hours choice is taken within each period and depends only on current productivity A or, if the match uses STW, also on the parameters of the STW scheme. In Appendix B, we derive the following characterization of the hours choice:

$$\begin{aligned} h_{\text{STW}}^{L,*}(A) &= \min\left\{\max\{0, \psi'^{-1}(A-\sigma)\}, \bar{h}\right\} \\ h_{\text{no}}^{L,*}(A) &= \psi'^{-1}(A), \end{aligned}$$

where $\bar{h}$ denotes the maximum level of hours that can be worked when using STW. The maximum implies a minimum hours shortfall.

Consistent with the assumption on separation decisions, we also assume that the decision to take up STW, if available to the match, depends on the joint match surplus. If wages will be adjusted to $\hat{w}$ by mutual consent in periods 1 and 2, the adjusted wage will be set to make the firm indifferent between match continuation and separation, i.e., the adjusted wage $\hat{w}$ is characterized by a zero firm surplus given the current productivity level and eligibility for STW access. In the presence of wage rigidity, a share $1-\alpha$ of matches will not be able to adjust wages even if mutual consent for wage adjustment exists. The inability to adjust wages to preserve a positive surplus for the firm will lead to a separation. We assume that starting in period 3, wages recover to their normal level $\bar{w}$ with probability λ.[5] We relegate value functions and further details to Appendix B.

Parameterization. The model is highly stylized and we therefore abstain from calibrating it directly to the data. Instead, speaking to our empirical design, we parameterize the model to

5 This reduced-form wage recovery would in an extended model be related to outside offers from other firms and wage adjustments by mutual consent to avoid the worker leaving the current firm.

demonstrate whether differential wage adjustments absorb any employment differences between firms with long vs. short PBD.

For the (dis)utility function from work, we assume a standard functional form:

$$\psi(h) = \frac{h^{1+\phi}}{1+\phi}.$$

In the baseline, we abstract from wage rigidity and set $\alpha = 1$, so that wages can always be adjusted by mutual consent. We set the model parameters as shown in Table 3. The low discount factor β and the high wage relative to productivity, $\bar{w} = 0.9(\bar{A} - \kappa)$, imply that the stable employment situation starting in period 3 does not dominate the surplus in the first two periods. Effectively, the discount factor is a stand-in for the expected duration of the match that is affected by future job-to-job mobility, retirement, or quits of workers. We set the threshold for STW access $\bar{h}$ to 80 percent of the normal hours choice $h^*(\bar{A})$, i.e., the hours choice when productivity is $\bar{A}$. This choice constitutes an intermediate value between the two institutional threshold values of an hours reduction of 30% for at least 10% of all employees.

Finally, we set σ determining the STW transfer rate to 34.2%, which we rationalize as follows. The replacement rate on income for the worker is 0.67, but employers will have to pay on 80% of the earnings shortfall due to STW the full social security contributions of 41%. Subtracting these additional contributions from the 67% replacement rate, we obtain $\sigma = 0.67 - 0.41 \times 0.8 = 34.2\%$.

5.2 Model Results

To illustrate the effect of differences in the PBD on wages, we consider a cross section of firms that all start with wage $\bar{w}$ but face different productivity shocks in the first period, so that their productivity A in period 1 differs. We normalize $\bar{A} = 1$ and approximate a shock distribution with support between -0.3 and -0.6, a mode of -0.4, and a decreasing density around the mode towards the boundaries of the support.[6] We consider persistent shocks, and assume that productivity does not recover in period 2 but only when the firm enters period 3. Hence, we only consider relative to expectations a more persistent productivity shock.

6 Appendix Figure C.2 shows the density of the productivity distribution in the first period.

Figure 3, Panel (a), shows the average wage difference in percentage points between surviving firms with long vs. short PBD relative to the normal wage $\bar{w}$. The wage difference is positive, i.e., the firm with prolonged PBD lowers wages by less than the firm with the shorter PBD. The reason is that the option of longer access to STW benefits increases the value of the firm in period 1. Therefore, fewer of the firms with a long PBD have to enter into wage negotiations to keep their surplus positive after a shock compared to firms with a short PBD. The value of firms that only have access to STW benefits in the first period turns negative more often in the first period because of the risk that STW benefits will not be available when productivity is still low in period 2. Importantly, wage negotiations take place by mutual consent, and the wages of workers in firms with short access to STW benefits will be cut to preserve their jobs. Note that the wage renegotiation depends only on the expectations of the shock but not on its realization in period 2. The long-run wage effect in the model will always vanish because all surviving firms recover from the initial shock in period 3, as there will be mean reversion of wages at rate λ in the future.

Despite the wage adjustments, we also find small differences in employment for firms with long vs. short PBD (cf. red line in Figure 3, Panel (b)). The employment levels at these two groups of employers differ after three years by 1 percentage point and, hence, only by about a quarter of the wage effect. The ability to flexibly adjust wages and hoard labor leads to employment that largely evolves in parallel for employers with short and long maximum PBD. As in Cahuc, Kramarz, Nevoux, and Vieira (2021), the presence of STW leads to an hours distortion. We do not study this distortion as our data does not allow us to capture this variation in the intensive margin.

The wage effect in the model is the result of efficient negotiation between the worker and the firm to lower wages in an attempt to avoid layoffs. This flexible wage setting in case of productivity shocks provides insurance and employment stability to the worker. Period-by-period Nash bargaining of wages would also provide a mechanism to trade off wages and job stability, but only infrequent wage adjustments with mutual-consent bargaining yield persistent wage dynamics from transitory shocks and differences in future eligibility to STW benefits.

Inflexible wage adjustment. As an extension to the baseline model, we consider a model with wage rigidity ($\alpha < 1$) that prevents some wage adjustments that could preserve the employment relationship. As the considered firms in our model are all in a crisis state, as they were hit by

a negative productivity shock, the inability to adjust wages will increase separations. Note that this will have only negligible effects on wages as we only see wages in continuing employment relationships. We set $\alpha = 0.95$, meaning that in each period 5% of firms cannot adjust their wage even in the presence of large negative shocks as those in our simulation.[7]

Figure 3, Panel (b), shows the employment rate differences of firms with long vs. short PBD in an environment with rigid ($\alpha = 0.95$) and flexible ($\alpha = 1$) wages. For the case of rigid wages, we aggregate across the different wage adjustment paths, keeping the productivity path as in the case of flexible wages. Thus, some firms will differ only in their ability to adjust wages. As discussed before, we observe a small employment effect for the case of flexible wages of 1 percentage point relative to normal employment (red line). By contrast, we find much larger effects for the case of rigid wages where the employment difference between long and short PBD firms opens up strongly between period 1 and 2, and reaches close to 10 percentage points at the end of period 2 (blue line)—almost ten times larger than in the case of flexible wages. The differences in employment preservation between long and short PBD firms therefore become much larger in the presence of wage rigidity, with some firms lacking the ability to adjust wages to preserve employment.

6 2012 Reform: Effects of Extending the Potential Benefit Duration of Short-Time Work

Guided by our model, we estimate the effects of STW PBD and also assess to what extent wage rigidity amplifies employment effects.

The PBD of STW benefits is a key policy lever that governments use. The bottom panel of Figure 1 shows the PBD (left y-axis) for firms that started STW in the respective months since 2005. The figure also shows the unemployment rate in Germany (right y-axis) to illustrate the countercyclical nature of extensions, alongside the PBD of unemployment insurance (UI). Unlike in the US where UI PBD is, by design, countercyclical (see, e.g., Schmieder and Von Wachter, 2016), Germany has historically not changed UI PBD in response to crises and, instead, resorts to STW

7 If we consider our model to be at annual frequency, this implies that each quarter about 50% of firms can adjust their wage, so 6.25%—marginally more than 5%—of firms will never adjust the wage during the year. Estimates (often in quarterly terms) considering all firms, not only those hit by negative shocks, imply greater wage rigidity (e.g., Barattieri, Basu, and Gottschalk, 2014).

PBD changes as a key labor market policy lever in crises.

For the purpose of identifying the effect of PBD on employment and wages, we focus on a sharp and unexpected reform in 2012 that doubled the STW PBD from 6 to 12 months, which we describe next.

6.1 The 2012 Reform: STW Extension

In the wake of the global financial crisis, the German government had repeatedly extended the default PBD for STW, e.g., from 6 to 12 months. However, no further extension was planned beyond the end of 2011. For firms starting STW in January 2012, the PBD was reset to the default duration of 6 months.

Despite signs of an economic slowdown coinciding with the European sovereign debt crisis of 2012, Labor Minister Ursula von der Leyen publicly rejected any plans to alter the PBD as late as November 25, 2012. In a surprising policy reversal on December 7, 2012, she announced a doubling of the PBD from 6 to 12 months. This abrupt shift in policy, highlighted by contemporaneous newspaper coverage (see Appendix Figure C.3), underscores the unexpected nature of the reform. The extension applied retroactively to firms already receiving benefits and was backward-binding. Firms that had begun STW in 2012 could not have anticipated this change.

The extension's impact varied depending on when firms initiated STW: those whose benefits had expired by December were ineligible, while those still receiving benefits in December could claim an additional 6 months of support. This policy change provides a unique quasi-experimental setting for our research design, allowing us to examine the causal effects of extended STW PBD on various labor market outcomes.

Figure 4 illustrates the reform for starters in May and July of 2012. Firms that started STW receipt in July and still received STW benefits in December (last month of a 6-month spell under the old PBD regime) could benefit from the extension and continue using STW in 2013. For firms that had started in May 2012, the PBD for uninterrupted usage ended in October and, thus, before the reform.

When a firm's STW spell reaches the PBD limit, the firm has to pause STW receipt. Theoretically, it can apply for benefits again in the future. However, this requires a new STW application and can only occur after a mandatory moratorium of at least three months. In principle, gaps

in STW receipt of up to two months are allowed within one STW spell and prolong the PBD accordingly.[8]

6.2 Research Design: Regression Discontinuity Based on STW Start Date

Our design exploits the 2012 reform by comparing firms that started STW in the second half of 2012 and were, thus, ex-post eligible for the PBD extension to those that started STW earlier and were, thus, ineligible for the extension.

We estimate the following linear regression discontinuity model with outcome $y_{i,h}$ at horizon h for firm i that starts STW in start month $m(i) \in \{2011m1, 2011m2 \ldots, 2012m12\}$:

$$y_{i,h} = \beta_{1,h} D_{m(i)} + \beta_{2,h} D_{m(i)} \mathbf{1}(D_{m(i)} > 0) + \tau_h \mathbf{1}(D_{m(i)} > 0) + \alpha_m + \beta_{3,h} X_{m(i)} + \epsilon_{i,h}, \qquad (1)$$

where α_m denotes calendar-month fixed effects and (for each firm i)

$$D_m = (m - 2012m6) \cdot X_m$$
$$X_m = \mathbf{1}(m \in \{2012m1, \ldots, 2012m5, 2012m7, \ldots, 2012m12\}).$$

The specification is a regression discontinuity design with distance to the cutoff 2012m6 (D_m) as running variable. The coefficient of interest for horizon h is τ_h, which captures the treatment effect of the STW extension.

The design is estimated for firms that start in 2012 (X_m). However, we also include firms that start STW in 2011 to estimate calendar-month fixed effects, allowing us to account for seasonality in the STW usage pattern. We exclude establishments that start STW in the cutoff month itself as we only have start date information at the monthly level and firms starting in June 2012 may or may not be eligible for the extension depending on whether they started before or after June 7, 2012.

Our baseline specification furthermore includes industry-by-region fixed effects. Industries are

8 We address potential concerns for our research design arising from this institutional setup in two ways. First, among all STW spells that start in 2011 or 2012, 84% do not have any interruptions. Second, we ignore starters in June of 2012 whose PBD expired in November, but who may still receive STW benefits if they had a gap of one month.

defined at the 1-digit level as sections based on the Classification of Economic Activities (WZ 2008) and regions as states (*Bundesländer*). The core outcome variables of interest are employment (share of initially employed that are employed anywhere), employment at initial employer (share of initially employed that are still employed at the firm) as well wage growth in average daily wages relative to the start month of STW.

Summary statistics and descriptive evidence. For our analysis on the PBD as an important policy lever, we focus on firms that start STW in 2011 and 2012. Specifically, we define a firm based on its employees in the start month of STW, and follow their employment status as well as wages in the months following the start of STW. To reduce noise when studying the evolution of wages, we restrict attention to individuals that work full-time and are fully liable to social security.

Table 4 shows key summary statistics for firms that start STW in 2012. The median firm has 20 employees. The difference to the size of the average firm (67 employees) implies a skewed size distribution. While financial information based on balance sheets (assets, cash) is widely available, the availability of financial information based on income statements is substantially worse. This is due to German reporting requirements: small firms (*Kleinst-Kapitalgesellschaften* and *kleine Kapitalgesellschaften*)—defined based on a combination of thresholds for revenue, assets, and employees—are not required to publish information beyond their balance sheet.

To better interpret the magnitude of subsequent effects on wage growth, Table 4 also includes summary statistics of the growth rates of average (nominal) wages for different horizons. On average, wages increase by 3 (6, 9, 11) percent one (two, three, four) years after the start of STW and relative to the level at the start of STW. For at least 75% of firms, wage growth is non-negative in the first year since the start of STW.

Figure 5 shows the differences in consecutive use of STW and employment outcomes for our treatment and control groups non-parametrically. Treated firms are more likely to use STW for more than six but fewer than twelve months (Panel (a)), and it is during the same short time period that we would expect potential employment differences to emerge. Panels (b)-(e) indicate that is the case, but driven by retention at the initial employer, possibly at the cost of hindered reallocation rather than prevented unemployment.

6.3 RD Design: Balancedness, Take-Up, and Complier Characterization

In the following, we implement several robustness checks to probe the validity of our RD design and study predictors of extended benefits.

First, consistent with the fact that we leverage an unexpected and backward-binding reform, we find that characteristics of firms are smooth around the cutoff date. In Appendix Figure C.4, we show that firms are similar around the cutoff date in terms of (i) their total number of employees, (ii) average daily wage paid, (iii) the number of observations available, which reflects the number of firms starting STW in a given month, and (iv) the share of manufacturing firms, which faced particularly severe economic conditions.

Second, we consider firm-level determinants of using extended benefits among eligible firms (starting STW in the second half of 2012) in Appendix Table D.2. We include as covariates in the cross-sectional regressions firms' total number of employees, their average daily wage, their age, and the one-year growth rate in average wages (based on employees that were employed at the respective firm 12 months prior to the start of STW). As such, from this exercise one learns what types of firms would have desired a longer PBD than was available in the first half of 2012. We find that older firms, those with higher average wages, and smaller firms are more likely to take up short-time work benefits for more than six months when it is possible to do so, while the wage growth compared to the year prior to the start of STW bears no statistically significant effect. Note that by controlling for industry by region fixed effects, we also account for any potential differences in the severity of economic conditions across local sectors.

6.4 (No) Employment Effects of Short-Time Work Extensions

To provide an initial visual assessment of potential employment effects of the PBD extension, Figure 6 plots the probability of retention of workers by their establishment's STW start month.[9] To account for potential seasonality by STW take-up, the outcomes are differenced relative to the mean of establishments taking up STW in the same month in the year before (2011). The figure shows some evidence consistent with employment effects of about three percentage points at 12 months but no such evidence at longer horizons.

9 Starters in June are excluded from the regression, but are included here (in gray) for illustrative purposes.

To formally assess effect sizes and confidence intervals, we report estimation results for the RD design estimated at various horizons in Figure 7 and in Table 5. Panel (a) of Figure 7 confirms that firms in the treatment group indeed had substantially longer STW benefit receipt compared to firms in the control group—irrespective of whether we consider firms' consecutive or nonconsecutive use (Appendix Figure C.5) of short-time work.

Figure 7, Panel (b), reports effects on retention (employment at the initial employer); and Panel (c) reports effects on employment anywhere. Consistent with the visual evidence in Figure 6, we find a statistically insignificant effect of 0.028 (SE 0.03) for retention at 12 months. Effects at longer horizons are smaller and continue to be statistically insignificant (cf. Table 5, Panel (a)). We can further rule out positive effects on employment anywhere at all horizons (Table 5, Panel (b)). This implies that even the small positive (though not statistically significant) effect on retention at 12 months is due to a reallocation of employment from other firms rather than from reductions in non-employment. This chimes with the—if anything—negative effects on unemployment and employment elsewhere in Panels (d) and (e) of Figure 7.

We next investigate heterogeneity in several dimensions of worker-level characteristics. For the sake of compactness, we summarize our results graphically, and present the point estimates alongside confidence bands for the baseline effects and the respective interaction effects.[10] Regardless of whether we consider employment at the initial employer (Figure 8) or employment anywhere (Appendix Figure C.6), we find only small and never any statistically significant effects across all worker characteristics that we consider, ranging from tenure, age, education to the position in the wage distribution.

6.5 Wage Effects and the Role of Wage Flexibility

We have uncovered precisely estimated zero employment effects from a longer PBD, i.e., variation in the intensive margin of short-time work. At first glance, this is at odds with other design-based work that has found positive employment effects of STW, be it in France (Cahuc, Kramarz, Nevoux, and Vieira, 2021), Switzerland (Kopp and Siegenthaler, 2021), or—at least in the short run—also in Italy (Giupponi and Landais, 2023). In striking contrast to these countries, Germany

10 We include the full tables in the Appendix, in Tables D.3 to D.6.

has substantially more decentralized bargaining institutions (Boeri, Ichino, Moretti, and Posch, 2021), so that wage adjustments can be used to avoid separations in response to labor market changes (Hartung, Jung, and Kuhn, 2025). Wage rigidity, on the other hand, is a key friction that inhibits such efficient renegotiation. As highlighted in our model in Section 5, wage flexibility can preserve jobs where firm surplus would have been negative, leading to layoffs when wages are fixed but joint surplus remains positive (Jäger, Schoefer, and Zweimüller, 2023). This opens up the possibility that decentralized bargaining and wage flexibility are potential remedies, which we investigate next.

6.5.1 Effect of STW Extensions on Wage Growth

To test the role of wage flexibility, we study the effects of PBD variation on the wage trajectories of the initially employed over time. In particular, we now use as dependent variable the growth in average daily wages relative to a given firm's short-time work start month. In doing so, we consider workers' wages in the post-period, measured one to four years later, earned anywhere, possibly at another firm.[11] Due to the fact that wages in the first year upon receipt of short-time work are potentially mismeasured, we focus on longer horizons starting 24 months.[12]

Figure 9 shows that firms with shorter PBD adjust their wages downward relative to otherwise equivalent firms that are treated with extended benefits. We test this more formally in Figure 7, Panel (f), and Table 5, Panel (c), where the effect size is long-lasting and increasing in the horizon, leading to treated firms' wage growth exceeding that at control firms by up to 5.9 percentage points. Our empirical findings are qualitatively consistent with the model-implied paths of wage effects (in Figure 3, Panel (b)). Our empirical estimates correspond to the wage difference in $t = 2$ of the model when only long PBD firms still have access to STW.

As treated and control firms do not vary in employment outcomes—i.e., firms with a shorter PBD offer the same level of employment protection—our evidence is consistent with intra-firm insurance at the cost of a wage penalty incurred by employees at firms with a shorter PBD. Unless there are adverse effects on individual workers' matching in the labor market subsequent to working

11 We separately focus on wage effects among stayers and switchers below.

12 STW, albeit to a small extent, affects social security contributions and, thus, during STW receipt, contaminates wages as reported to the German social insurance system.

at a firm with a shorter PBD, it should be primarily employees remaining with the same firm that see relative wage cuts in exchange for employment protection in spite of shorter PBD. Across panels in Appendix Table D.7, we consider heterogeneous treatment effects for workers that are no longer with the same firm—i.e., switchers—one to four years upon said firm starting to use STW.

Switching mutes the treatment effect on wages (consistent with evidence in Di Addario, Kline, Saggio, and Sølvsten, 2023). Especially workers that switch within the first two years see no wage adjustments. Our estimates for the coefficient on the respective interaction effect become weaker for longer horizons when we consider switchers within three or four years, as the ex-post probability of having already switched by the time wage growth is measured decreases in the horizon.

6.5.2 Interdependence of Employment and Wage Effects

We next seek to characterize under what circumstances firms trade off wages against employment stability. In line with our theoretical prediction, our empirical findings suggest that, on average, firms with a shorter PBD retain their employees at similar rates compared to treated firms with a longer PBD, but they do so at the cost of lower wage growth. To shed light on potential heterogeneity in firms' responses and study the role of wage rigidity in mediating the effects, we split the sample into cells based on sector (manufacturing, wholesale and retail trade, other), region (East/West), and size (up to 5, 6-15, 16-50, more than 50 employees). We then calculate cell-specific treatment effects on employment and wages.

If the absence of an effect on employment across treated and control firms is due to wage flexibility—i.e., control firms with a shorter PBD insure their employees at the expense of the latter's wage growth—then one should detect an employment effect, but no wage effect, for control firms that do not, or cannot, insure their employees, even if this is not their average response in our data. Using all available establishments—i.e., without requiring firm-level data—Figure C.7 reveals for both employment-related outcomes and the shortest valid horizon (24 months) that positive wage effects go hand in hand with zero or negative employment effects, while positive employment effects are associated with zero or negative wage effects. Firms that lower wages more in response to shorter PBD (in comparison to the treatment group) preserve more employment. The elasticity is -0.86, i.e., a 10 percent decrease in wages is associated with an 8.6 percent increase in employment.

As our baseline sample is conditional on available firm-level data from Orbis, this also implies

an admittedly modest sample selection in terms of firm size, although even small and medium-sized companies are covered by Orbis. However, the sample is fairly representative as it covers 77% of all employees at establishments that made use of short-time work in 2012. To establish whether firms with balance-sheet data that populate our baseline sample are indeed focused on a different quadrant of the cell-level analysis, we split up the previous figure into the latter group and the remaining group without firm-level balance-sheet data coverage, comprising arguably smaller firms. Appendix Figure C.8 shows that in contrast to firms with balance-sheet data, those that do not make part of our baseline sample are indeed more likely to exhibit employment effects, but no (positive) wage effects. These results also hold for a longer horizon of 36 months (Appendix Figures C.9 and C.10).

6.5.3 Heterogeneity by Local Labor Market Conditions, Works Council Presence, and Liquidity

To further probe robustness and shed light on mechanisms, we next analyze heterogeneity in the effect of PBD by local labor market conditions, the presence of a works council, and measures of liquidity. For this analysis, we draw on the sample with firm-level balance-sheet data.

Heterogeneity by local labor market conditions. We first assess heterogeneity by local unemployment. A potential reason for the absence of employment effects that our research design indicates could be the fact that unemployment levels did not rise to, e.g., the levels experienced during the Great Recession or the COVID-19 pandemic. To shed light on whether a lower unemployment rate environment can account for the absence of effects, we zoom in into local labor markets with higher levels of unemployment, i.e., commuting zones with a high unemployment rate (in the top tercile of the respective distribution) in the month of the beginning of an STW spell.

Panel (a) of Appendix Table D.8 provides evidence against such a view at least in the short run (up to 12 months) where we find similar effects on retention in high-unemployment local labor markets. In addition, the sum of the coefficients on the treatment variable, the high unemployment indicator, and their interaction is not statistically significant in four out of six specifications. In Panel (b) of Appendix Table D.8, we find larger wage effects in slack labor markets, consistent with workers accepting larger wage concessions to retain employment when outside options are limited.

That wage adjustments substituted for longer benefit duration even under these conditions reinforces the role of decentralized bargaining institutions and suggests our findings generalize beyond the 2012 setting, which was a relatively contained downturn. Our event study evidence further supports generalizability: even in commuting zones in the top tercile of local unemployment, workers at firms exhausting STW benefits do not experience elevated transitions into unemployment (Figure 2, Panel (f)). We cannot rule out that employment effects would emerge in a more severe crisis, but the absence of differential effects across local unemployment conditions—combined with the theoretical logic that wage flexibility substitutes for longer PBD—points to a mechanism that operates wherever bargaining is sufficiently decentralized.

Role of works councils. We next investigate the role of works councils in mediating the effects we find. Works councils directly matter for STW as they have codetermination rights in the decision whether and how to implement STW. In addition, works councils matter as an institution for decentralized wage bargaining, e.g., by concluding local pacts for employment (agreements to lower wages in exchange for employment security, see, e.g., Jäger, Noy, and Schoefer, 2022). Works councils may complement STW measures, or may also substitute for them, by providing an alternative channel through which employee retention may be organized.

Information on works council presence is not directly reported in the administrative data, so we use data from an official survey (the IAB Establishment Panel) to predict the presence of works councils.[13] We then compare retention and wage effects across establishments with high or low (predicted) works council presence.

The evidence in Appendix Table D.9, Panel (a), provides more support for the view of works councils as a substitute for STW: retention effects of extended STW are smaller in the presence of a works council (with a statistically significant negative interaction effect between the treatment

13 We predict the presence of works councils based on survey data. Specifically, we draw on the IAB Establishment Panel (2012 wave), a representative employer survey based on more than 15,000 establishments from all branches and sizes. We fit a logistic regression model for the presence of a works council using information on the establishment's size, region, industry as well as age, and use this model to predict the probability that an establishment in our sample has a works council. We present details in Appendix A.6. We align our data as closely as possible to the IAB Establishment Panel by considering establishments instead of firms for our analysis at this point. Appendix Figure C.11 shows the receiver operating characteristic curve (ROC curve) for the prediction exercise based on a random 15% subsample of the IAB Establishment Panel. For the prediction in our sample, we pick the threshold that maximizes the Area Under The Curve (AUC).

and the predicted presence of a works council). Turning to wage effects in Appendix Table D.9, Panel (b), we do not detect differences in wage effects of STW extensions by the presence of a works council.

Appendix Table D.9, Panels (a) and (b), also reveal the baseline effect of works council presence among the firms in our sample, with works councils associated with greater employment protection and negative wage effects. Overall, the heterogeneity of effects by (predicted) presence of a works council lends support to the idea that works councils independently provide insurance against layoffs and therefore substitute for STW policies in preventing layoffs during economic downturns.

Heterogeneity by liquidity. Previous work has pointed to liquidity as a key friction in preventing labor hoarding (see, e.g., Guiso, Pistaferri, and Schivardi, 2012; Giroud and Mueller, 2017; Giupponi and Landais, 2023). To test whether liquidity constraints mediate the effects of STW extensions, we consider firms in the top and bottom terciles of the distribution of their cash-to-assets ratio. Due to the resulting considerable drop in sample size (also because the respective variable is not available for all firms), we omit industry by region fixed effects in Appendix Table D.10, but our findings are qualitatively similar when not doing so (Appendix Table D.11).

Similar to the baseline effects of works councils, more liquid firms are more likely to retain employees, as the respective intercept effect is positive and statistically significant for all horizons starting at 24 months in Appendix Table D.10, Panel (a). Firms' liquidity might thus substitute for their response to shorter PBD. However, we do not find direct evidence for important interaction effects of PBD extensions with liquidity when it comes to employment. There is no differential treatment effect of prolonged PBD on retention irrespective of firms' cash-to-asset ratio. While the sum of the coefficients on firms' cash-to-asset ratio (indicator) and the respective interaction is borderline significant at the 10% level for only one horizon (24 months), the sum of the three coefficients (adding the coefficient on our main treatment to the previous two coefficients) is insignificantly different from zero throughout (the lowest p-value across all horizons/columns is 0.35).

We do see a negative interaction effect of liquidity and the PBD extension when focusing on wages as an outcome in Appendix Table D.10, Panel (b), where the treatment effect on wage growth is lower for firms with higher liquidity. As such, our results are consistent with the idea that corporate liquidity reduces the need to adjust wages to retain employees in spite of a shorter PBD.

7 Within-Firm Targeting of STW

Within-firm targeting of STW can explain the absence of employment effects, especially when establishments target STW towards workers that are likely to stay even in the absence of STW. To evaluate this possibility, we use our individual-level data from 2020/2021 and estimate a logistic regression model of retention at the same employer 12 months later on rich individual and establishment characteristics in a training sample in the pre-COVID-19 pandemic period, and use the coefficients to predict the retention probability for individuals in the sample (for details see Appendix A.1). The respective summary statistics are reported in the last row of Table 2.

Panel (a) of Figure 10 zooms in into establishments with STW take-up between April and December 2020, and shows that predicted retention is actually negatively correlated with individual STW take-up in a binned scatter plot.[14] A 10 percentage point decrease in the predicted retention probability increases STW take-up by 1.3 percentage points (with a standard deviation of 0.4 percentage points). This implies that STW was targeted towards individuals that were somewhat less likely to be retained in the absence of an STW-triggering event or STW take-up itself.

We additionally validate the prediction model in Figure 10, Panel (b), which demonstrates a remarkably linear relationship between predicted retention probability and actual retention in a binned scatter plot. A 10 percentage point increase in the predicted retention probability corresponds to a 9.6 percentage point increase in actual retention (with a standard deviation of 0.3 percentage points). The strength of this relationship is particularly noteworthy given that our prediction model was trained on pre-pandemic data, yet maintains its predictive power when applied to the pandemic period—a dramatically different economic context—and specifically in firms utilizing STW. This robust performance suggests that the underlying factors influencing employee retention remained relatively stable despite the unprecedented economic disruptions caused by the pandemic.

Panel (c) of Figure 10 further dissects this relationship by comparing individuals with and without STW take-up. We find a slope of close to one (1.10) between actual and predicted retention for non-STW individuals. For STW recipients, the slope is substantially lower at 0.79. At lower levels of predicted retention, STW recipients are more likely to remain with their firm. This disparity diminishes as the predicted retention probability increases. This pattern suggests that STW is

14 We focus on the first STW spell in case of multiple spells.

associated with a higher probability of actual retention, driven by employees who, based on pre-pandemic patterns, would have been at higher risk of separation (i.e., those with lower predicted retention probabilities).

Panel (d) adds nuance to this interpretation by considering employment elsewhere, which is negatively correlated with the predicted retention probability. The positive employment effect of STW take-up for individuals with lower predicted retention probabilities is mirrored by a negative effect, of similar size, on employment elsewhere.

These individual-level patterns align with our firm-level findings. While firms are somewhat more likely to use STW for workers with low baseline retention probabilities, this targeting is relatively weak. Moreover, when STW does affect retention—primarily for at-risk workers—it operates by preventing reallocation to other employers rather than preventing unemployment. This mechanism explains the absence of aggregate employment effects at the firm level.

8 Conclusion

Short-time work schemes are intended to subsidize job preservation, with some success at least in the short run (Kopp and Siegenthaler, 2021; Giupponi and Landais, 2023). Our paper provides novel evidence on the take-up and effects of STW schemes, with a focus on Germany's experience from 2009 to 2021. We leverage rich administrative data and quasi-experimental variation to study the intensive margin of potential benefit duration (PBD). In contrast to the US practice of extending UI duration during crises, Germany adjusts STW PBD as a countercyclical labor market policy.

In event study analyses comparing firms exhausting PBD with those leaving the STW program before benefits run out, we find an uptick in separations at the time of exhaustion, driven by workers moving to other employers rather than entering unemployment. Focusing on a 2012 reform that unexpectedly doubled the PBD from 6 to 12 months, we provide quasi-experimental evidence on this key but understudied policy lever. We find small albeit statistically insignificant effects on retention at the initial employer and precisely estimated zero effects on overall employment. This pattern indicates that any increased retention likely stemmed from hindered reallocation to other firms. We uncover substantial and persistent wage effects, with short PBD firms adjusting wages downward relative to long PBD firms in lieu of adjusting employment. We also find larger wage

effects corresponding with smaller employment effects across industry-region-size cells, consistent with wage flexibility preventing layoffs.

Overall, our findings suggest that the effects of STW policies depend critically on the underlying wage-setting institutions and the bargaining environment. While STW extensions did not preserve job matches on average in our setting, some firms with shorter PBD were able to sustain employment through wage adjustments, demonstrating that a wage-job stability trade-off can substitute for longer benefit durations.

Our evidence constitutes a puzzle for the notion of STW as a labor hoarding device as we find no employment effects of extending the PBD. The absence of employment effects lends support to the view that moral hazard plays a large role in STW schemes (cf. Lapeyre, 2023). STW appears to primarily benefit higher-paying, larger, but declining firms. While several aspects of the effects of STW schemes are beyond the scope of our study (e.g., the role of spillover effects), our evidence raises questions about the efficient allocation of resources in labor market policies. Our findings suggest that policymakers may need to carefully assess the design and targeting of STW programs to achieve their objectives, and to consider the balance of policies aimed at insuring jobs vs. workers (Giupponi, Landais, and Lapeyre, 2022).

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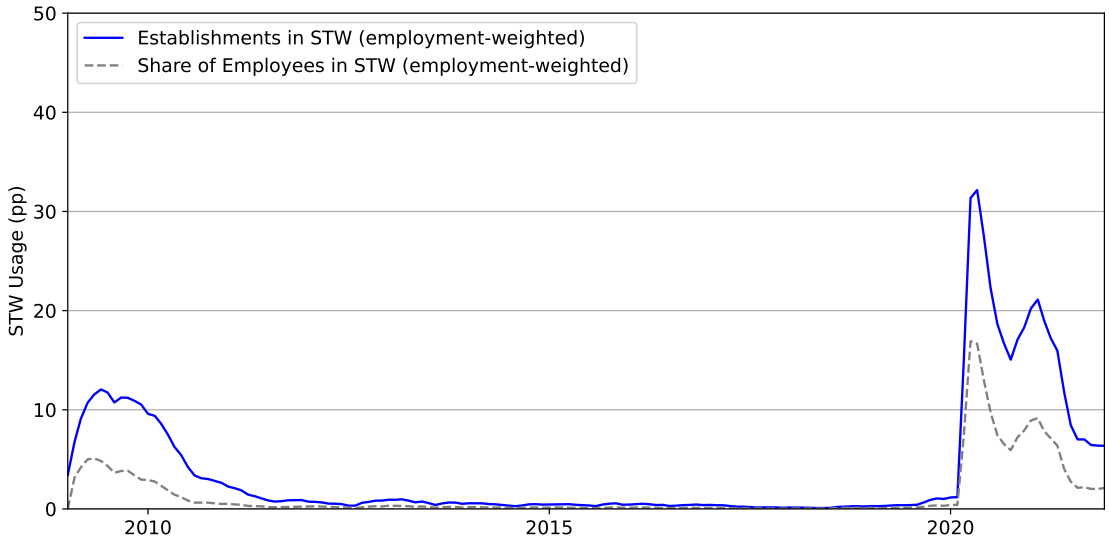
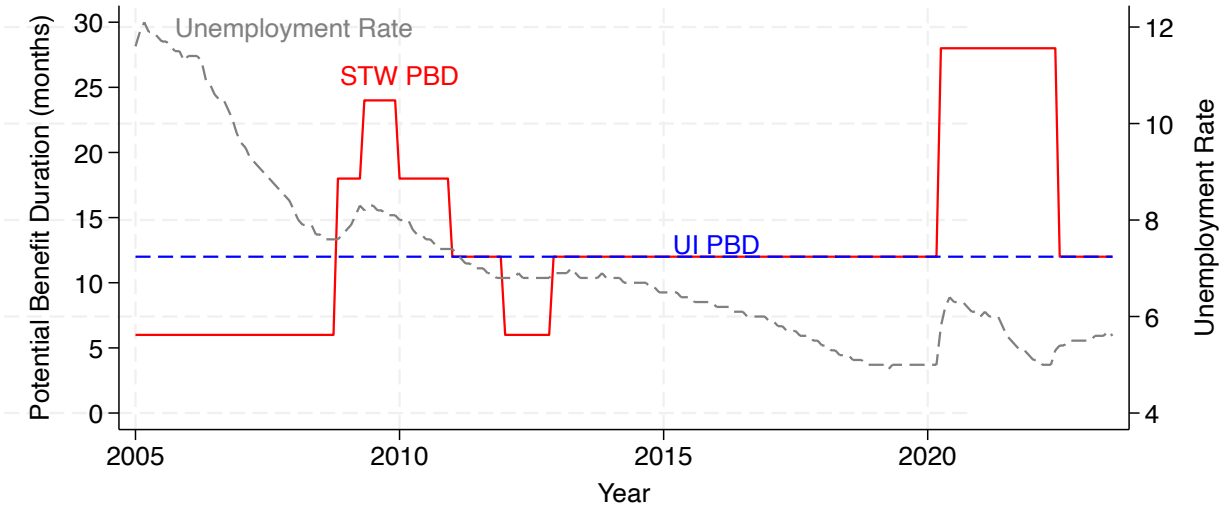
Figures

Figure 1: STW Take-Up Over Time and STW Potential Benefit Duration

(a) STW Take-Up Over Time

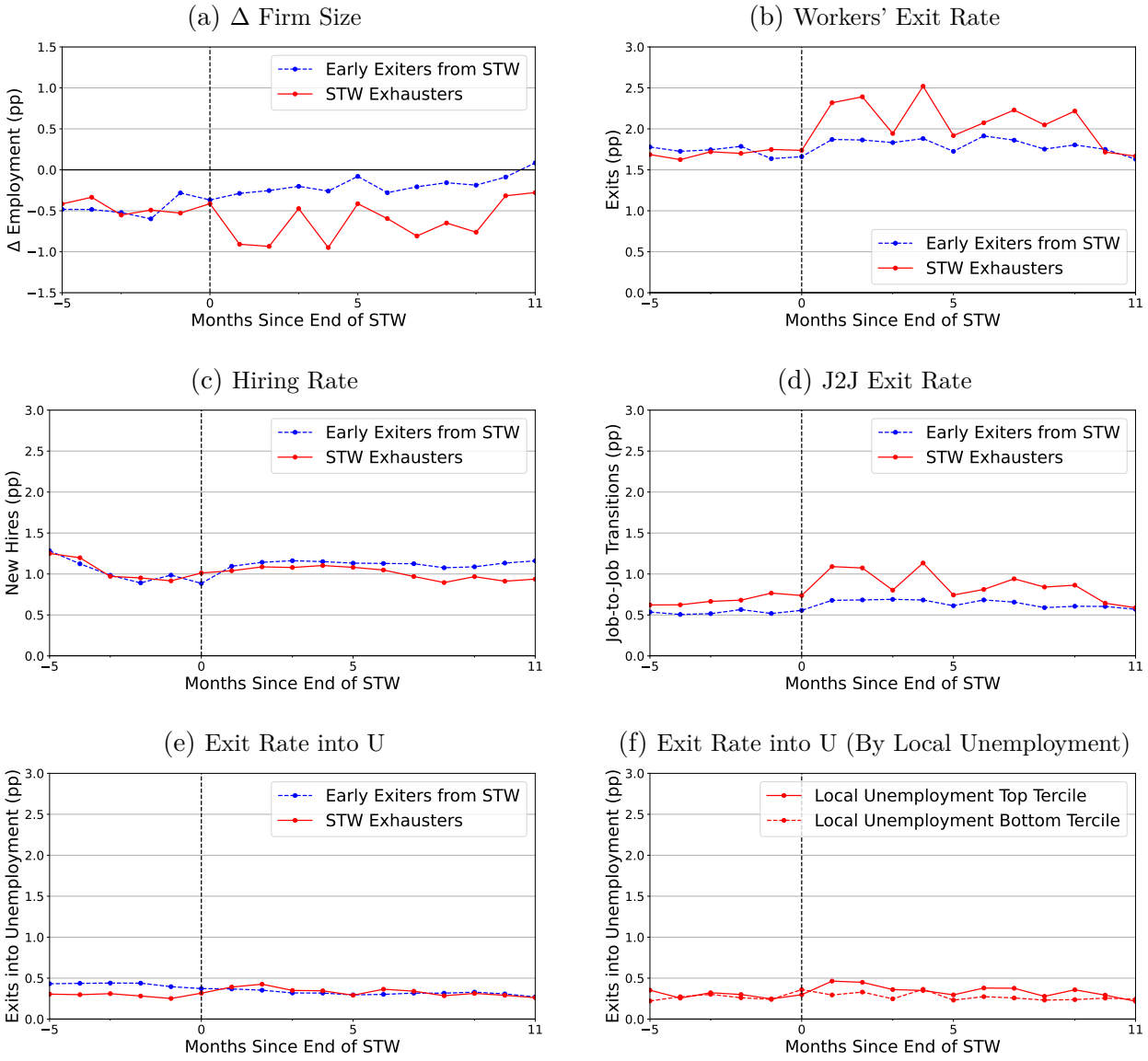


(b) Short-Time Work PBD, UI PBD, and the German Unemployment Rate Over Time



Notes: Panel (a) shows monthly STW usage since 2009. The solid line depicts the employment-weighted share of establishments in STW, the dashed line depicts the establishment-level share of employees in STW—again employment-weighted. We use the Establishment History Panel since 2009 as universe, and add information on STW receipt. Establishments with five employees or less as well as establishments that are eligible for seasonal STW (*Baugewerbetarif*) are excluded (see Appendix A.2 for details). Panel (b) plots STW potential benefit duration (PBD) (solid red, LHS scale). For comparison, we also plot the PBD for unemployment insurance (UI) (dashed blue, LHS scale) as well the monthly unemployment rate in Germany (dashed gray, RHS scale).

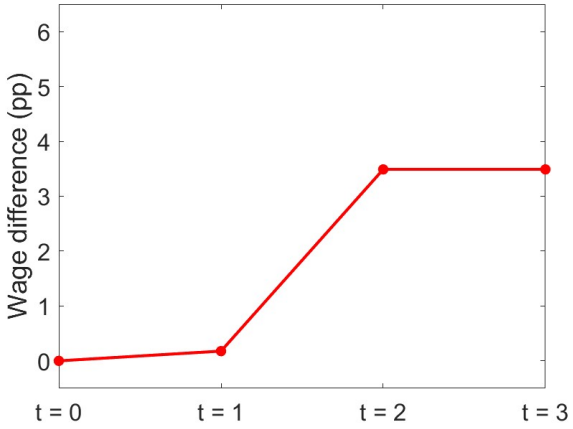
Figure 2: STW Benefit Exhaustion 2009-2018: Event Studies of Exhausters vs. Early-Exiters



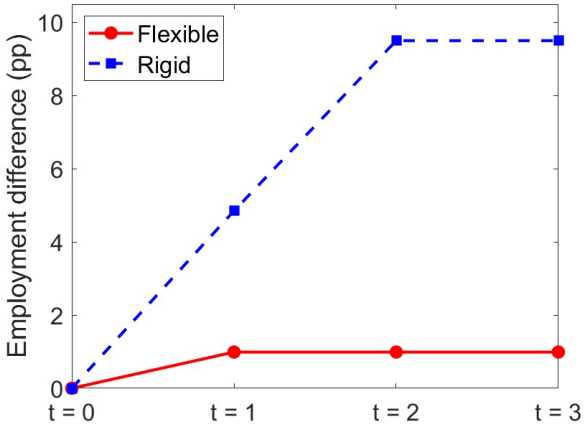
Notes: The figures plot establishment-level outcomes around the end of STW, pooled across STW spells from 2009 to 2018. Panels (a)-(e) are split into two groups: establishments with an STW take-up of less than the PBD applicable at the time, and those that exhaust STW PBD. Establishment-level outcomes are normalized to establishment-level employment in the start month of STW, and winsorized at the 1% level. Panels (a) and (c) plot the change in employment and hiring rate, respectively. The other panels zoom in on the exit margin (Panel (b)) and show the exit probability of incumbent workers to other employers (Panel (d)) and into unemployment (Panel (e)). Panel (f) focuses on the latter for exhausters, split into establishments in commuting zones in the top vs. bottom tercile (per year). Location information is based on the 2017 data-version (*Kreisschlüssel 2017*, SIAB 1975-2017). Unemployment (U) is defined based on data on unemployment-benefit receipt (*Leistungsempfängerhistorik*). There is a residual transition category which is omitted in the figure. The sample is restricted to STW spells without a gap and not exceeding 24 months that are present the year before they use STW. The sample is restricted to employees that are fully liable to social security.

Figure 3: Model Predictions

(a) Differential Wage Growth Between Firms With Long vs. Short PBD

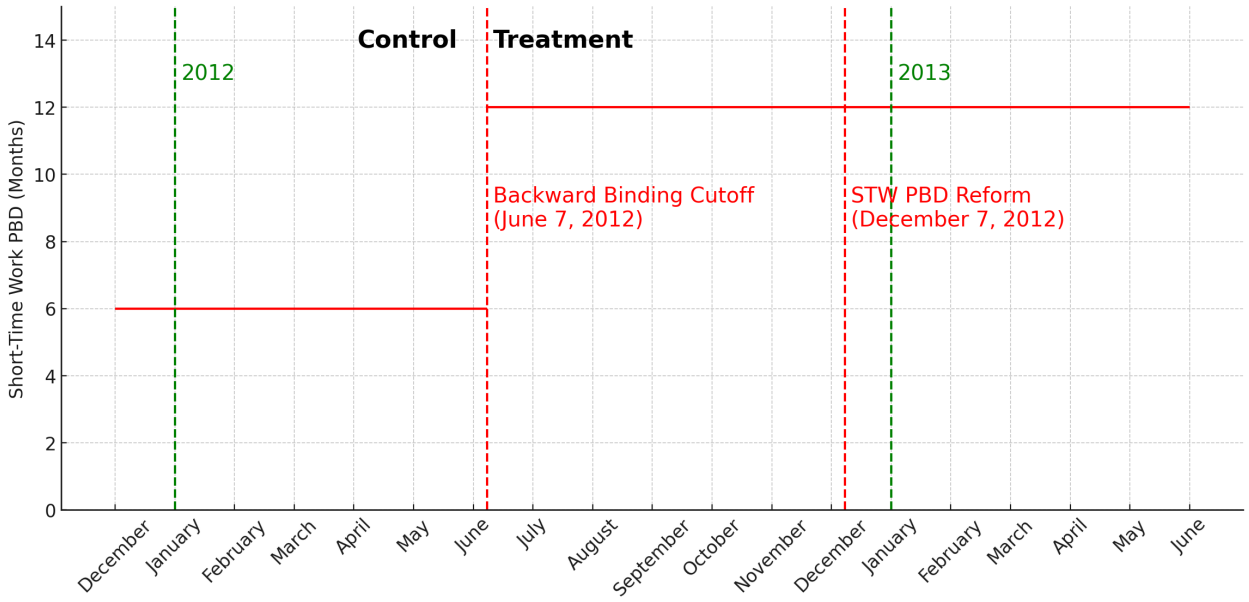


(b) Differential Employment Between Firms With Long vs. Short PBD by Wage Rigidity



Notes: Panel (a) plots the model-implied difference in average wages (in percentage points) relative to the normal wage ($\bar{w}$) for firms with long vs. short PBD after a productivity shock. Firms experience shocks in period 1, and the shocks persist in period 2. Starting in period 3, productivity has recovered for all firms. Wages are averaged across employed workers of all firms with different shocks using the shock probability distribution (cf. Appendix Figure C.2). Panel (b) plots the model-implied difference in average employment levels (in percentage points) for firms with long vs. short PBD in environments with and without flexible wage adjustments. Employment levels are expressed relative to the pre-shock "normal" employment level. Employment considers only that of workers who are initially employed at their current employer.

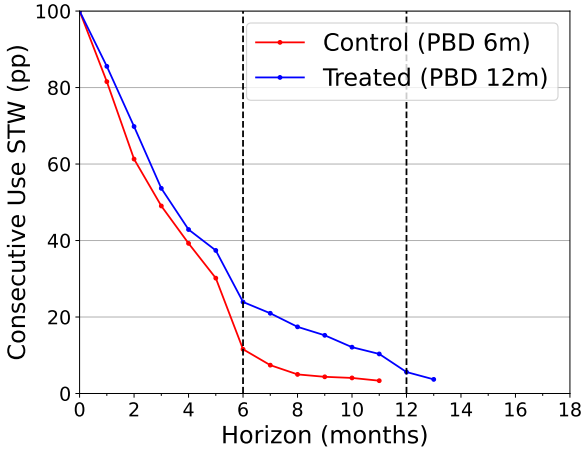
Figure 4: Illustration of 2012 Reform Research Design



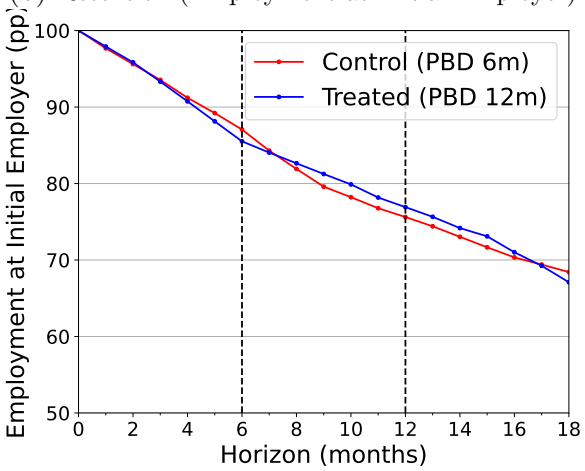
Notes: This figure illustrates the 2012 STW PBD reform that was announced by executive ordinance on December 7, 2012 and extended STW PBD from 6 to 12 months. It was backward-binding as it also applied to firms that had already been admitted to the program and were still receiving benefits (under the STW PBD of 6 months applicable until then). This splits firms that start STW in 2012 ex post into a treatment (PBD of 12 months) and control (PBD of 6 months) group as indicated by the red dotted lines.

Figure 5: 2012 Reform: STW Usage and Employment Outcomes By STW Start Date

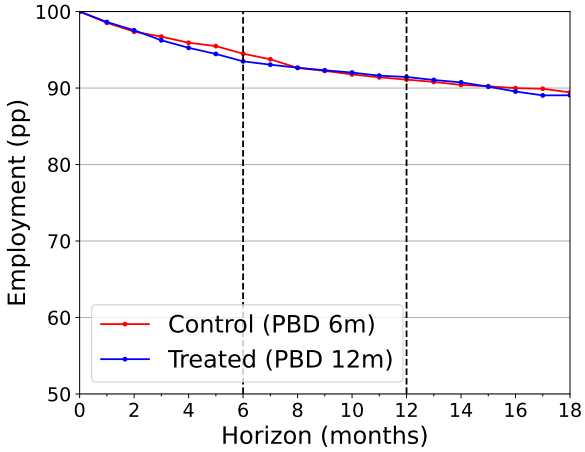
(a) STW Usage



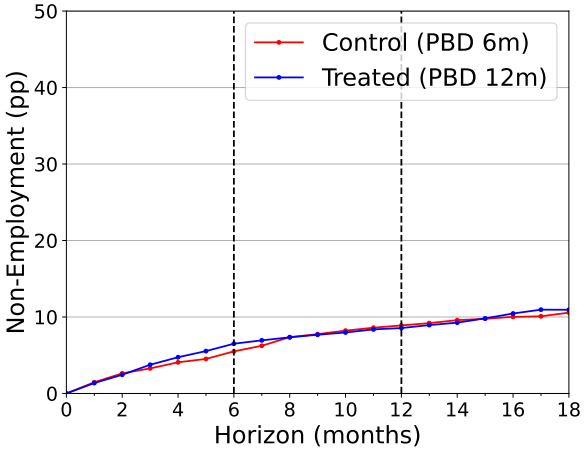
(b) Retention (Employment at Initial Employer)



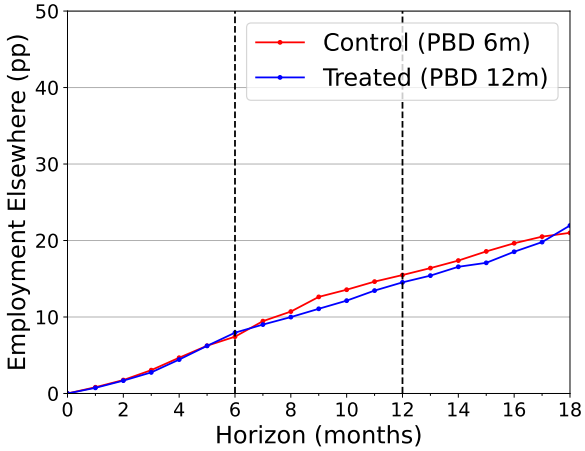
(c) Employment (Anywhere)



(d) Non-Employment

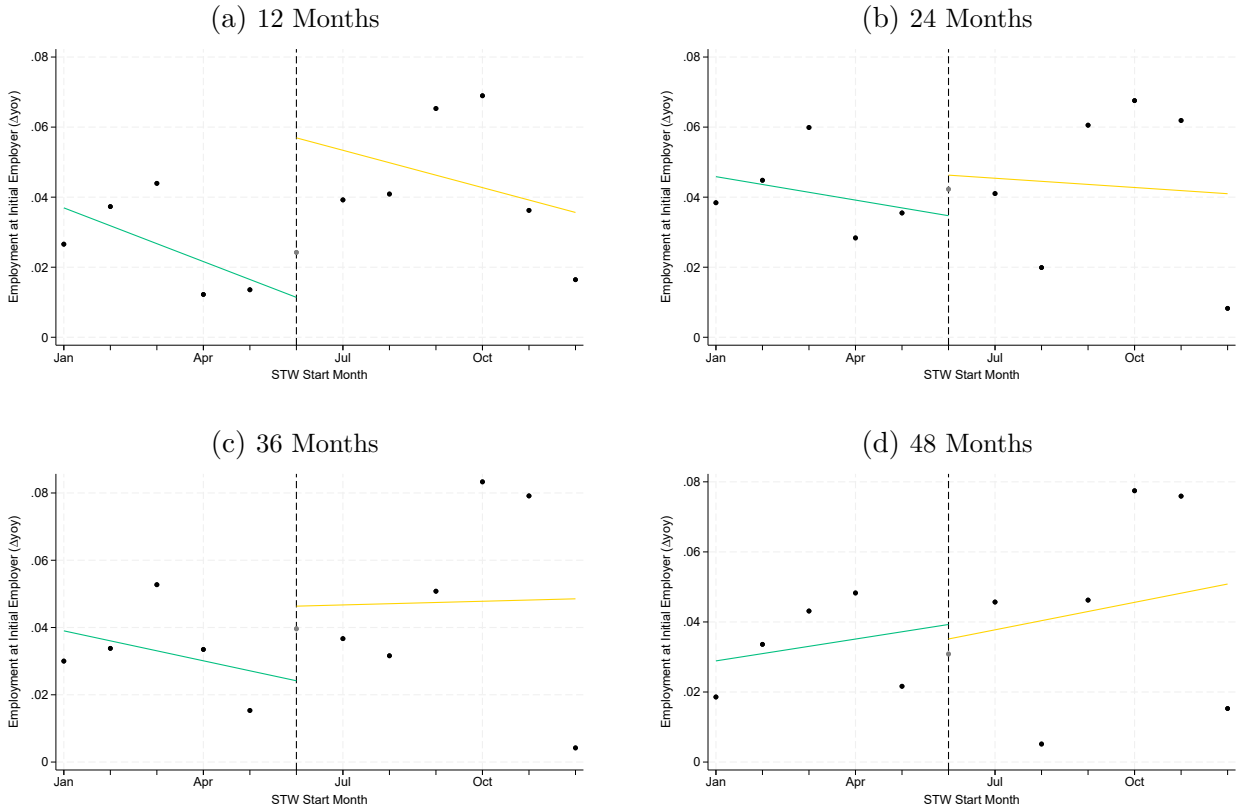


(e) Employment Elsewhere



Notes: The figures focus on firms that start STW in April, May, or June 2012 (red) or July, August, or September 2012 (blue). In Panel (a), we consider as outcome an indicator variable that is equal to one if the firm still receives STW benefits. Only STW receipt as part of the initial application is considered. The outcome variables in Panel (b)-(e) are the share of initially employed workers (i.e., employed at the start of the establishment's STW spell) who are still employed at the same firm (Panel (b)), employed anywhere (Panel (c)), not employed (Panel (c)), or employed at another employer (Panel (d)).

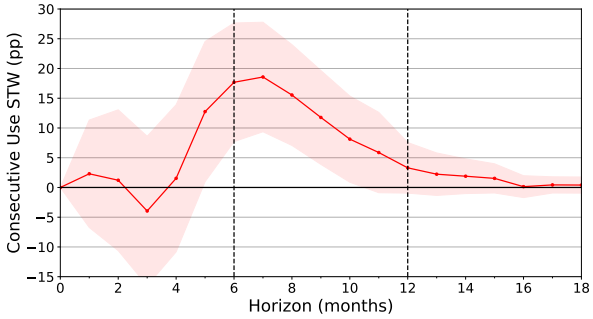
Figure 6: RD Design: Effect of PBD Extension on Retention (Employment at Initial Employer)



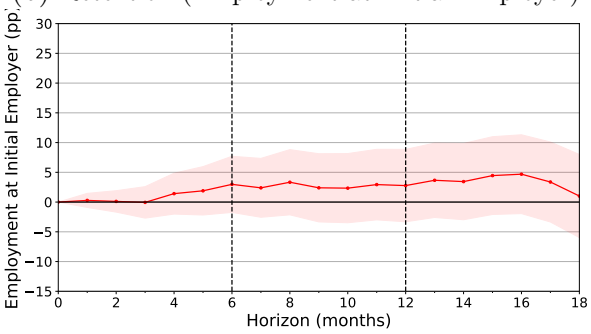
Notes: The figure plots the regression discontinuity design for the outcome variable considered 12, 24, 36 and 48 months after the start of STW. As outcome variable, we use for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the firm after the respective time horizon. Potential re-entries after an exit are ignored. To account for seasonality, we use the difference in cohort means per calendar month between 2012 and 2011. The cohort that starts STW in the cutoff month which we exclude from the analysis is shown in gray. The sample is restricted to firms that in the start month have more than five employees in full-time who are fully liable to social security.

Figure 7: RD Effects of PBD Extension

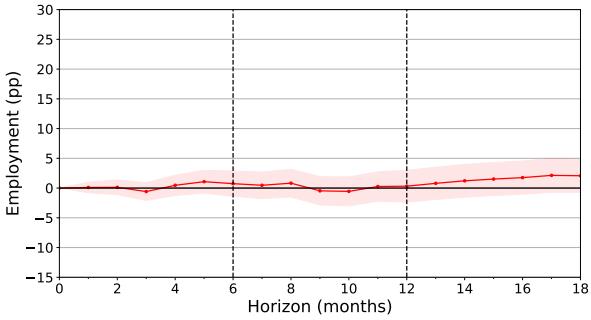
(a) Take-Up of STW Benefits in Extension Period



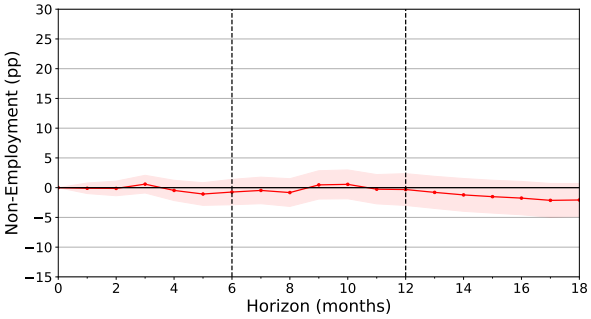
(b) Retention (Employment at Initial Employer)



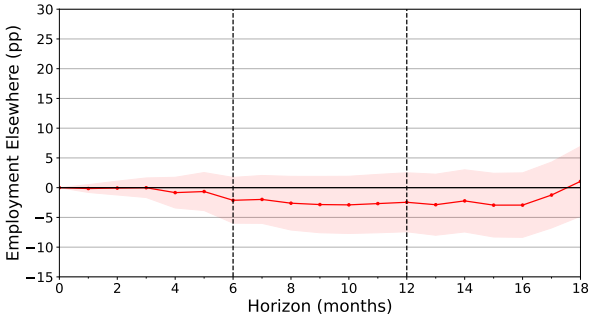
(c) Employment Anywhere



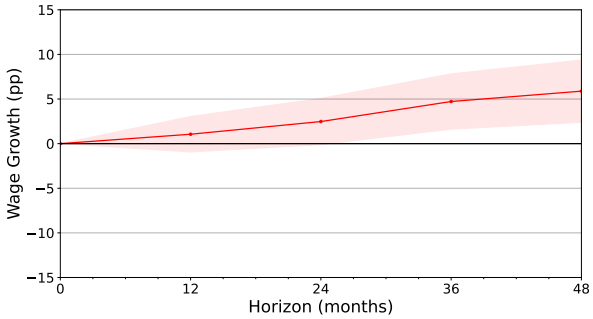
(d) Non-Employment



(e) Employment Elsewhere

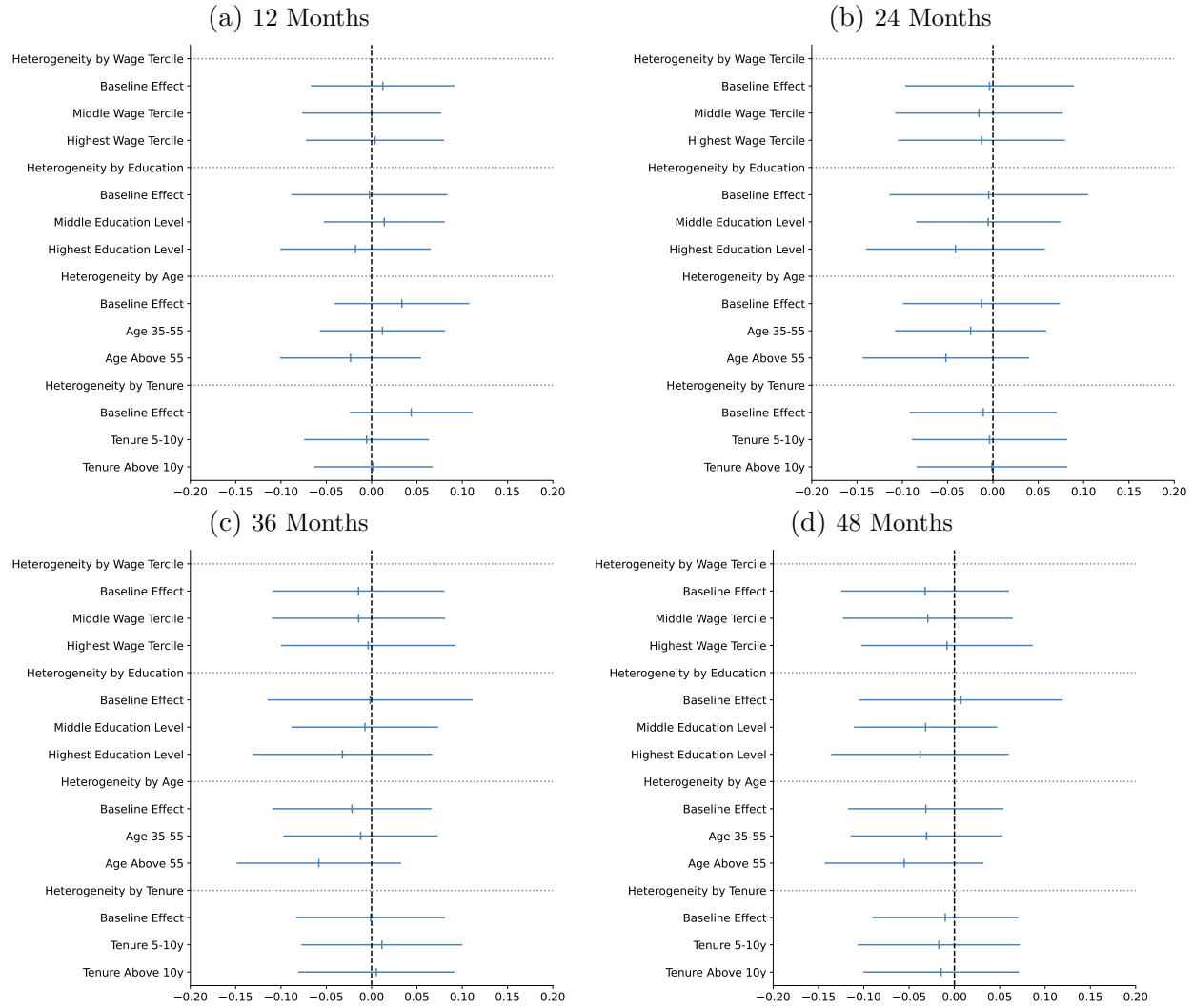


(f) Wage Growth



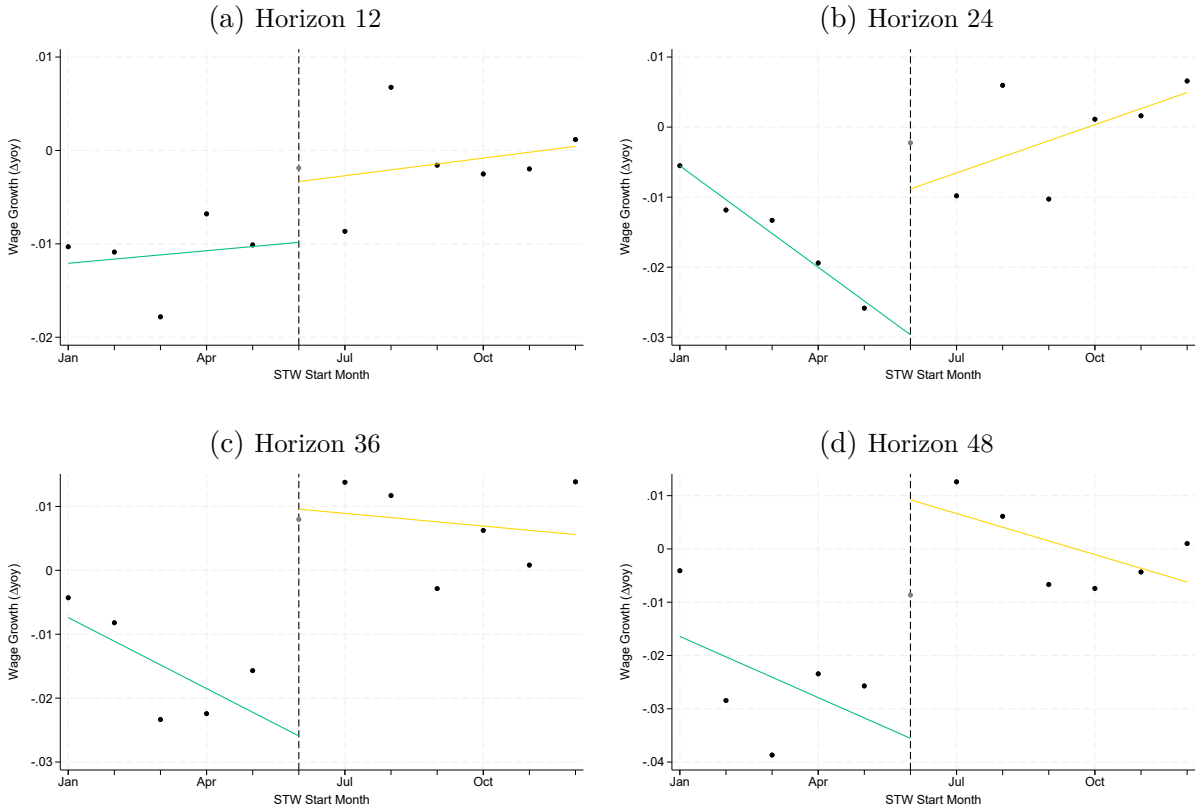
Notes: The figure plots the effect of the reform at different horizons after the start of STW (i.e., treatment effects using the regression discontinuity design specified in (1) including industry by region fixed effects). The data is at the firm-horizon level; a separate regression is run for each horizon. In Panel (a), the outcome is an indicator variable equal to one if the firm still receives STW benefits. Only STW receipt as part of the initial application is considered. Appendix Figure C.5 shows the analogous result with an indicator variable that is equal to one regardless of the STW spell as outcome variable. Panels (b)–(e) focus on employment outcomes. The outcome variables are for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm (Panel (a)), employed anywhere (Panel (b)), not in employment (Panel (c)), or employed with another employer (Panel (d)). Potential re-entries after an exit are ignored. In Panel (f), the outcome variable is the growth rate of average daily wages relative to the start of STW. Since in the majority of cases the administrative information on wages is based end-of-year reports, we consider coefficients at annual frequency. 95% confidence intervals based on robust standard errors are depicted. The sample is restricted to firms that in the start month have more than five employees in full-time who are fully liable to social security.

Figure 8: Heterogeneity in RD Effect of PBD Extension by Demographic Characteristics for Outcome: Retention (Employment at Initial Employer)



Notes: The figure plots heterogeneous employment effects by demographics at different horizons after the start of STW. We define groups within firms based on demographic characteristics at the start of STW (age, tenure at the firm, education level, wage tercile within the firm). The data is at the group-firm-horizon level. The coefficients shown are heterogeneous treatment effects of a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. As outcome variable, we use for each group-firm cell the share of initially employed (i.e. employed at the start of STW) who are still employed at the firm after the respective time horizon. Potential re-entries after an exit are ignored. The baseline education level is defined as no training or missing information, individuals with a middle (high) education level have a vocational training (hold a degree from an university of university of applied sciences). The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security.

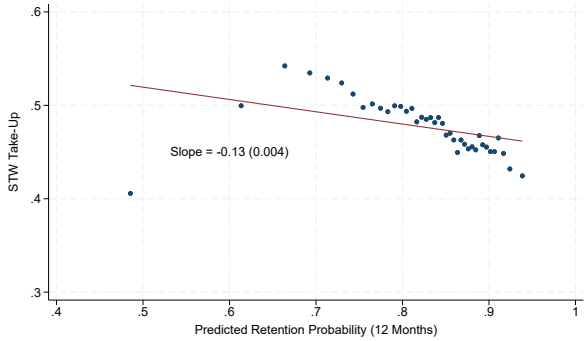
Figure 9: RD Design: Effect of PBD Extension on Wage Growth



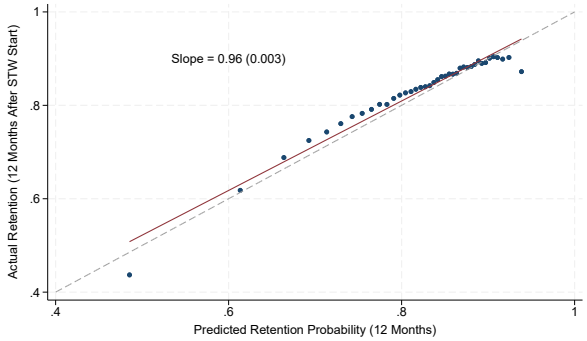
Notes: The figure plots the regression discontinuity design for the outcome variable considered 12, 24, 36 and 48 months after the start of STW. As outcome variable, we use the growth rate of average daily wages relative to the start of STW. To account for seasonality, we use the difference in cohort means per calendar month between 2012 and 2011. The cohort that starts STW in the cutoff month which we exclude from the analysis is shown in gray. The sample is restricted to firms that in the start month have more than five employees in full-time who are fully liable to social security.

Figure 10: Within-Firm STW Take-Up and Employment Outcomes By Predicted Retention

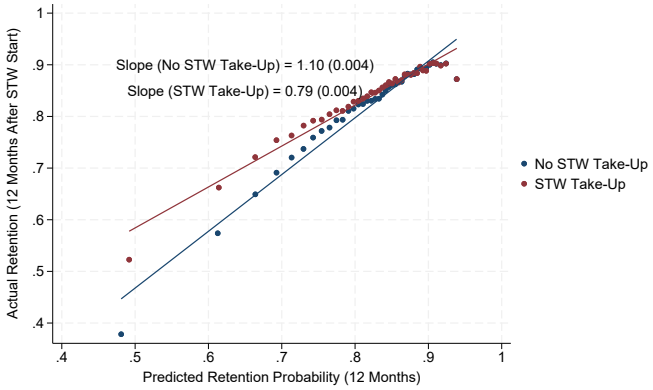
(a) Within-Firm STW Take-Up



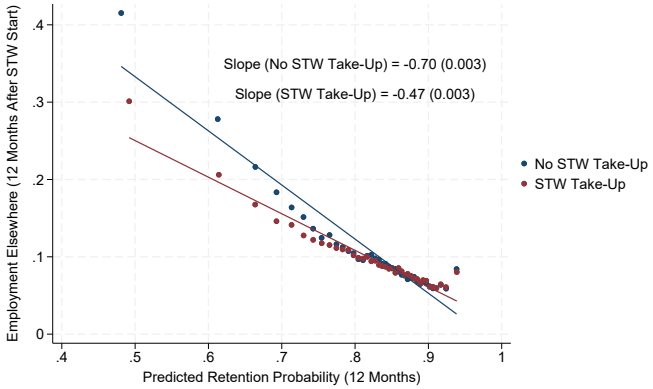
(b) Actual Retention 12m After STW Start



(c) Actual Retention 12m After STW Start (By Individual STW Take-Up)



(d) Employment Elsewhere 12m After STW Start (By Individual STW Take-Up)



Notes: The figures focus on establishments that start STW between April and December 2020 (first spell in case of multiple), and consider their employees in the start month of STW. Panel (a) plots individual STW take-up against the predicted retention probability. For the predicted retention probability, we estimate a logit regression model of retention at the same employer 12 months later on rich individual and establishment characteristics in a training sample in the pre-COVID-19 pandemic time period, and use the coefficients to predict the retention probability for individuals in the sample (for details see Appendix A.1). Panels (b) and (c) plot actual retention at the initial employer 12 months after the start of STW against the predicted retention probability. In Panel (c), we split the sample ex post by actual individual-level take-up of STW. The same split is used in Panel (d) where the outcome variable is employment at another employer 12 months after the start of STW. STW take-up is defined as high or 100% probability of STW receipt, and we restrict attention to establishments with high-quality information (see Appendix A.1 for details). The sample is restricted to employees in full-time that are fully liable to social security.

Tables

Table 1: Selection into STW Take-Up Across Establishments

	Time Periods									
	2009-2021		2009/2010		2011/2012		2013-2019		2020/2021	
	Nonuser	User	Nonuser	User	Nonuser	User	Nonuser	User	Nonuser	User
Number of Employees	33.65	42.41	31.89	62.70	33.42	56.71	33.95	64.53	34.55	36.19
	(59.17)	(71.78)	(56.66)	(91.11)	(59.12)	(85.97)	(59.57)	(93.57)	(60.05)	(63.33)
Average Daily Wage (Imp.)	89.44	89.35	75.99	84.24	81.03	83.64	91.26	96.66	107.92	90.16
	(36.00)	(32.77)	(32.33)	(29.18)	(33.16)	(28.56)	(35.48)	(30.98)	(37.12)	(33.62)
Establishment Age	18.61	19.62	16.51	18.45	17.30	19.34	19.00	22.32	20.90	19.67
	(12.64)	(13.40)	(11.23)	(11.40)	(11.71)	(11.91)	(12.79)	(13.29)	(14.09)	(13.80)
Employment Growth Previous Year (pp.)	1.23	-2.76	1.85	-4.88	-0.78	-3.11	1.49	-3.59	1.88	-2.23
	(43.00)	(39.99)	(41.19)	(27.56)	(43.30)	(26.69)	(43.30)	(24.40)	(43.16)	(43.49)
Education (Establishment-Level Shares)										
Low (Neither or Missing)	0.19	0.22	0.19	0.15	0.17	0.14	0.19	0.14	0.20	0.25
	(0.19)	(0.21)	(0.18)	(0.13)	(0.18)	(0.13)	(0.19)	(0.13)	(0.20)	(0.22)
Middle (Vocational Training)	0.68	0.65	0.71	0.74	0.71	0.75	0.68	0.73	0.64	0.63
	(0.23)	(0.23)	(0.21)	(0.18)	(0.21)	(0.19)	(0.23)	(0.19)	(0.24)	(0.24)
High (Degree from University/FH)	0.13	0.12	0.10	0.11	0.12	0.12	0.13	0.13	0.16	0.13
	(0.19)	(0.17)	(0.16)	(0.15)	(0.17)	(0.16)	(0.19)	(0.16)	(0.21)	(0.17)
Age (Establishment-Level Shares)										
Younger Than 35	0.36	0.35	0.37	0.31	0.36	0.28	0.35	0.28	0.34	0.36
	(0.21)	(0.21)	(0.22)	(0.18)	(0.22)	(0.18)	(0.21)	(0.17)	(0.20)	(0.21)
35-54	0.45	0.44	0.48	0.52	0.47	0.52	0.45	0.49	0.43	0.42
	(0.18)	(0.17)	(0.19)	(0.15)	(0.18)	(0.15)	(0.17)	(0.14)	(0.16)	(0.17)
55 and older	0.19	0.21	0.15	0.16	0.16	0.20	0.20	0.24	0.23	0.22
	(0.15)	(0.16)	(0.14)	(0.12)	(0.14)	(0.13)	(0.16)	(0.14)	(0.17)	(0.16)
Minimum Number of Observations	7,833,554	536,920	1,105,604	84,187	1,226,735	20,310	4,588,408	30,415	912,807	402,008

Notes: The table reports establishment-level summary statistics. Standard deviations are reported below the means in parentheses. We use the Establishment History Panel since 2009 as universe, and add information on STW receipt. Establishments with five employees or less as well as establishments that are eligible for seasonal STW (*Baugewerbetarif*) are excluded (see Appendix A.2 for details). We pool observations in the establishment-year panel for the time periods considered. An establishment is defined as a user in some year if it receives STW benefits at some point during that year. Number of employees, average daily wages (based on imputed wages for wages above the contribution ceiling) and employment growth are winsorized at the 1% level. We use the symmetric growth rate for calculation of the employment growth.

Table 2: Individual-Level Summary Statistics: Selection into STW Take-Up Within Establishments

	Start Months			
	2020m4		2020m4-2020m12	
	No STW	STW	No STW	STW
Wage				
Daily Wage	124.61	113.84	127.87	115.71
	(53.65)	(48.26)	(53.51)	(47.93)
Education Level				
Low (Neither or Missing)	0.10	0.11	0.10	0.11
	(0.31)	(0.32)	(0.30)	(0.31)
Middle (Vocational Training)	0.68	0.69	0.67	0.70
	(0.47)	(0.46)	(0.47)	(0.46)
High (Degree from University/FH)	0.22	0.19	0.23	0.19
	(0.41)	(0.40)	(0.42)	(0.39)
Occupation (Horizontal)				
Production	0.39	0.39	0.41	0.43
	(0.49)	(0.49)	(0.49)	(0.49)
Personal Service	0.10	0.12	0.09	0.11
	(0.29)	(0.33)	(0.29)	(0.31)
Commercial Service	0.28	0.30	0.27	0.28
	(0.45)	(0.46)	(0.44)	(0.45)
IT Service	0.06	0.04	0.06	0.05
	(0.24)	(0.21)	(0.24)	(0.21)
Other Service	0.18	0.14	0.16	0.14
	(0.38)	(0.35)	(0.37)	(0.34)
Occupation (Vertical)				
Unskilled/ Semiskilled Tasks	0.13	0.14	0.12	0.14
	(0.33)	(0.35)	(0.33)	(0.35)
Skilled Tasks	0.54	0.55	0.53	0.55
	(0.50)	(0.50)	(0.50)	(0.50)
Complex Specialist Tasks	0.17	0.18	0.18	0.18
	(0.38)	(0.39)	(0.38)	(0.38)
Highly Complex Tasks	0.16	0.13	0.17	0.13
	(0.37)	(0.33)	(0.37)	(0.33)
Age				
Younger 35	0.29	0.31	0.28	0.30
	(0.45)	(0.46)	(0.45)	(0.46)
35-54	0.49	0.48	0.49	0.48
	(0.50)	(0.50)	(0.50)	(0.50)
Older 54	0.23	0.21	0.23	0.21
	(0.42)	(0.41)	(0.42)	(0.41)
Tenure				
Less Than 5y	0.47	0.50	0.46	0.48
	(0.50)	(0.50)	(0.50)	(0.50)
5-10y	0.19	0.19	0.19	0.20
	(0.39)	(0.39)	(0.39)	(0.40)
More Than 10y	0.34	0.31	0.35	0.32
	(0.47)	(0.46)	(0.48)	(0.47)
Predicted Retention Probability	0.82	0.81	0.82	0.82
	(0.10)	(0.09)	(0.09)	(0.09)
Observations	797591	821581	1247020	1124674

Notes: The table reports individual-level summary statistics for workers at establishments that used STW in 2020, differentiating between employees on short-time work vs. all other workers. We focus on establishments that start STW in April 2020 (columns 1 and 2) and between April and December 2020 (columns 3 and 4, first spell in case of multiple), and consider their employees in the start month of STW. For columns 3 and 4 we then pool across start months. STW take-up is defined as high or 100% probability of STW receipt, and we restrict attention to establishments with high-quality information (see Appendix A.1 for details). For the predicted retention probability, we estimate a logit regression model of retention at the same employer 12 months later on rich individual and establishment characteristics in a training sample in the pre-COVID-19 pandemic time period, and use the coefficients to predict the retention probability for individuals in the sample (for details see Appendix A.1). The sample is restricted to employees in full-time that are fully liable to social security. Standard deviations are reported below the means in parentheses.

Table 3: Model Parameterization

β	0.75	π	0.5
ϕ	2	σ	0.342
λ	0.33	$\bar{h}$	$0.8 \times h^*(\bar{A})$
ρ	0.07	$\bar{w}$	$0.9 \times (\bar{A} - \kappa)$
κ	0.1	α	1 (baseline)
b	0.4	α	0.95 (wage rigidity)

Notes: Model parameters of three-period model. See text for parameter description. The two different values of α show the parameter choices for the baseline model without wage rigidity and the model extension with wage rigidity.

Table 4: Firm-Level Summary Statistics for 2012 Reform Research Design

	Firms that Start STW in 2012						
	Mean	p10	p25	p50	p75	p90	N
Number of Employees (Start Month)	67.27	7.00	10.00	20.00	53.00	142.00	3683
Average Daily Wage (Start Month)	87.38	57.24	69.46	86.14	103.06	119.81	3683
Age	20.86	5.00	10.00	20.00	37.00	37.00	3683
Employment Growth Previous Year (pp)	-1.47	-20.69	-9.52	0.00	3.77	14.33	3682
Financial Information							
Assets (Mio EUR)	8.38	0.37	0.67	1.50	4.37	15.07	3125
Revenue (Mio EUR)	52.86	1.00	2.17	7.43	34.55	105.62	917
Cash-to-Asset Ratio (pp)	12.28	0.05	0.45	4.23	18.58	38.18	3078
Value Added per Employee (Mio EUR)	0.06	0.03	0.04	0.05	0.07	0.08	424
Wagebill-to-Value-Added Ratio (pp)	82.50	59.52	72.78	83.26	91.82	104.51	657
Wagebill-to-Revenue Ratio (pp)	31.97	13.32	20.55	30.32	39.64	53.78	517
Education (Firm-Level Shares)							
Low (Neither or Missing)	0.08	0.00	0.00	0.03	0.12	0.23	3683
Middle (Vocational Training)	0.79	0.55	0.72	0.83	0.92	1.00	3683
High (Degree from University/FH)	0.13	0.00	0.00	0.08	0.16	0.33	3683
Age (Firm-Level Shares)							
Younger Than 35	0.23	0.05	0.13	0.21	0.32	0.45	3683
35-55	0.56	0.38	0.47	0.57	0.66	0.74	3683
Above 55	0.20	0.00	0.11	0.19	0.29	0.39	3683
Tenure (Firm-Level Shares)							
Less Than 5y	0.38	0.07	0.15	0.29	0.54	1.00	3683
5-10y	0.22	0.00	0.08	0.17	0.29	0.50	3683
Above 10y	0.40	0.00	0.00	0.43	0.67	0.81	3683
Average Wage Growth							
Wage Growth Previous Year (pp)	0.94	-5.42	-1.75	1.23	3.72	7.00	3656
Wage Growth Within 1y (pp)	3.07	-4.10	0.15	3.14	6.11	9.96	3683
Wage Growth Within 2y (pp)	5.92	-3.76	1.82	6.13	10.14	15.25	3682
Wage Growth Within 3y (pp)	8.81	-2.97	3.29	8.68	13.91	20.75	3682
Wage Growth Within 4y (pp)	10.89	-3.54	4.58	10.64	16.89	25.19	3683

Notes: The table reports firm-level summary statistics. Firms that start in 2011 (3,559) which we include in the analysis to facilitate the use of calendar month fixed effects are not included. Number of employees, average daily wages in the start month, employment growth (symmetric growth rate), financial information as well as wage growth variables are winsorized as the 1% level. Age refers to the age of the largest establishment in case of multi-establishment firms (for details on the aggregation to the firm level see Appendix A.4). The sample is restricted to firms that in the start month have more than five employees in full-time who are fully liable to social security. Wage Growth Within 1y (2y, 3y, 4y) is the growth rate in average wages relative to the firm's start of STW after 1y (2y, 3y, 4y) based on employees that were initially employed at the respective firm – regardless of their future employer. Wage Growth Previous Year is the 1y-growth rate in average wages based on employees that were already employed at the respective firm 12 months prior to the start of STW. Financial information is based on 2012 information from the Dafne database by Creditreform/ BvD (see Appendix A.5 for details). Availability of financial information drops for items in income statements (revenue, value added, wagebill) rather than balance-sheet-items (cash, assets) since small firms in Germany need not publish information beyond their balance sheet.

Table 5: Effect of PBD Extension on Employment (RD Design)

(a) Retention (Employment at Initial Employer)

	Employment at Initial Employer, Horizon (months)					
	6	12	18	24	36	48
Running Variable	-0.005	-0.000	0.002	0.003	0.002	0.007
	(0.004)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
Treatment (12m PBD) × Running Variable	0.003	-0.003	-0.004	-0.002	-0.000	-0.003
	(0.006)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Treatment (12m PBD)	0.030	0.028	0.010	-0.011	0.004	-0.023
	(0.024)	(0.031)	(0.036)	(0.037)	(0.037)	(0.036)
Industry × Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	6,969	6,969	6,969	6,969	6,969	6,969
N Individuals	664,634	664,634	664,634	664,634	664,634	664,634

(b) Employment Anywhere

	Employment, Horizon (months)					
	6	12	18	24	36	48
Running Variable	-0.002	0.001	-0.004*	-0.004	-0.003	0.002
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Treatment (12m PBD) × Running Variable	0.002	0.000	0.004	0.005	0.004	0.001
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Treatment (12m PBD)	0.007	0.003	0.021	0.010	0.018	-0.011
	(0.011)	(0.014)	(0.014)	(0.014)	(0.015)	(0.016)
Industry × Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	6,969	6,969	6,969	6,969	6,969	6,969
N Individuals	664,634	664,634	664,634	664,634	664,634	664,634

(c) Wage Growth

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Running Variable	-0.000	-0.005**	-0.006*	-0.007**
	(0.002)	(0.003)	(0.003)	(0.003)
Treatment (12m PBD) × Running Variable	0.000	0.008**	0.004	0.004
	(0.003)	(0.003)	(0.004)	(0.005)
Treatment (12m PBD)	0.011	0.025*	0.047***	0.059***
	(0.010)	(0.013)	(0.016)	(0.018)
Industry × Region FE	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes
N Firms	6,969	6,968	6,968	6,969
N Individuals	623,638	605,768	592,361	579,913

Notes: The table reports the results of the regression discontinuity design specified in (1) at different horizons after the start of STW. The outcome variable is for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm (Panel (a)) or employed anywhere (Panel (b)), or the growth rate of average daily wages relative to the start of STW (Panel (c)). Potential re-entries after an exit are ignored. The data is at the firm-horizon level; a separate regression is run for each horizon. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown includes firms that start in 2011, which are included to facilitate calendar month fixed effects in order to account for seasonality. The number of individuals in Panel (c) refers to the number of individuals among all initially employed who are still in the labor market at this horizon and, thus, for whom wage growth can be calculated. The sample is restricted to firms that in the start month have more than five employees in full-time who are fully liable to social security. Robust standard errors are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix:
Short-Time Work Extensions

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Farzad Saidi, and Stefanie Wolter

A Data Appendix

A.1 Processing Individual-Level Data on Monthly STW Receipt

Information on individual STW benefits is extracted from establishments' monthly applications (*Abrechnungslisten*) using an automated optical character recognition (OCR) procedure. The procedure reads out the social security number, reduction in hours, regular remuneration, actual remuneration, and STW benefits per individual.

The OCR procedure faced several challenges, such as illegible handwriting and the discontinuation of information extraction for long applications after a certain number of pages. Additionally, for multi-establishment firms and temporary employment agencies, the establishment applying for STW may not coincide with the individual's employer in the Social Security Records.

Mapping individual STW benefits to employment biographies requires thorough cross-checks with both establishment-level data and Social Security Records. The key variable, indicating an individual's STW risk, is constructed as follows: an individual eligible for STW based on cross-checks with Social Security Records and found in the digitalized lists is assigned a 100% STW risk. Employees at an establishment are eligible for STW if they are below the statutory retirement age, not on parental leave, and either fully liable to social security or in vocational training (beyond the second month). If in a month the number of employees with a 100% STW risk coincides with the number of employees in STW from the establishment-level data, the remaining employees are assigned a STW risk of 0%. If there is a discrepancy, the remaining individuals are assigned a positive STW risk based on the share of eligible employees in STW per gender per establishment.

The upper panel of Table D.16 shows the results of the cross-checks at the establishment level for establishments starting STW in April 2020 (columns 1 and 2) and pooled across all establishments starting between April and December 2020 (columns 3 and 4). We define individual-level data as high quality if the individual works at an establishment for which the aggregated individual-level information on STW receipt coincides with the establishment-level data.

The bottom panel of Table D.16 shows the STW risk for individuals working at establishments under the same restrictions as in the upper panel.

We drop individuals with incalculable STW risk. This is often due to the fact that there is no 1:1 or 1:n mapping between the establishment that applies for STW and the employer from Social Security Records (often the case when a temporary employment agency is involved).

A.2 Processing Establishment-Level Data on Monthly STW Receipt

This section describes the procedure for combining the administrative data on STW receipt (BTR KUG) with the Establishment History Panel (BHP).

1) We create STW spells from BTR KUG, defining them as periods of STW usage with a maximum gap of two months, and transform the data into a monthly panel.

2) This unbalanced monthly panel is matched to the Establishment History Panel (BHP) expanded to a monthly frequency.

3) We drop all establishments that qualify for the seasonal STW scheme (*Baugewerbetarif*) at any point. This STW scheme targets establishments in the construction sector that are dependent on weather conditions and, thus, regularly face fluctuations in working hours in the winter.

4) We exclude establishments that cannot be successfully matched to BHP. For the RDD design this requires a successful match in 2011 and 2012. For the event study of exhausters vs. early-exiters this requires a successful match in the year in which the establishment starts with STW. For the analysis of within-firm targeting of STW this requires a successful match in 2020.

A.3 Processing Matched Employer-Employee Data

This section details on how we process excerpts of the Integrated Employment Biographies (IEB).

For the event studies of exhausters vs early-existers, we study entries and exists during and after STW spells. We construct an individual–month panel by converting IEB employment (*Beschäftigten-historik*) and unemployment-benefit receipt (*Leistungsempfängerhistorik*) spells to monthly status, following standard procedures Dauth and Eppelsheimer (2020). When multiple concurrent jobs are recorded, we retain (i) the job liable to social-security contributions and, if several, (ii) the job with the highest wage. We then take end-of-month snapshots to obtain the monthly individual-level panel.

For the RDD design, we restrict the sample to employees who are employed at establishments that use STW in the start month of STW, and we track their employment status and wages over time. We create a monthly panel from employment spells, treating months with zero reported daily wages as employment. These episodes mostly stem from parental leave or longer illness (*Unterbrechungsmeldung wg Entgeltersatzleistung (151), Erziehungsurlaub (152), gesetzliche Dienstpflicht (153)*).) For analyseson wages, we construct a second panel restricted to employment spells with positive daily wages. One-time payments are converted into daily payments for the reported period and added to the daily wage.

A.4 Aggregation to the Firm Level

This section describes the aggregation of establishment-level information to the firm level used in the RDD design.

1) We drop firms with establishments that started STW multiple times in the 12 months prior to a start of STW in 2012 (excludes 15% of the 6,416 firms that started in 2012).

2) In case a firm has multiple establishments that started STW, we keep the firm only if the starts happen either in the same month or one month apart. In the latter case, we define the earlier start months of the two to as the start month of the firm (fewer than 20 firms dropped).

3) If the remaining firms have an establishment that starts STW in 2012 and another establishment that starts in 2011, we exclude the firm in the reference group of firms that start STW in 2011 (78 firms dropped in the reference year 2011).

4) We assign each firm the industry, region and age of its largest establishment.

A.5 Processing Firm-Level Financial Data

This section explains how we assemble and clean the firm-level financial data from the Dafne database.

1) We start with the universe of firms in Dafne (as of May 2022) and use financial information from the lowest level of consolidation available.

2) To identify the lowest level of consolidation available we follow the following procedure. We use financial information at the unconsolidated level whenever possible. Some firms only report financial information at the group level (i.e., they are exempt by HGB 264 to report at both levels). If we can identify such a firm as the group head and thus identify other subsidiaries of the group, we use the consolidated information and drop other subsidiaries of the group. If we cannot identify the firm as the group head, the firm is dropped. If a firm reports both consolidated and unconsolidated information, we use the unconsolidated information of the group head as long as its revenues exceed 5% of the group revenue. Below this threshold, we assume that the group head is merely a financial holding and should not be treated as an individual firm (within the group).

3) We add balance sheet information and income statement data from 2008 until 2020.

4) We follow standard cleaning procedures but focus on balance sheet variables, since many firms in the sample are so small that they are not required to publish their income statement:

   a) We drop firms that have negative or zero total assets in any year.

   b) We drop firms that have larger equity than total assets in any year.

A.6 Predicting the Existence of a Works Council

This section contains details on the prediction exercise for the existence of a works council based on the IAB Establishment Panel (*IAB Establishment Panel 9319, DOI: 10.5164/IAB.IABBP9319.de.en.v1*).

We split the 2012 wave of the IAB Establishment Panel into a random test sample (15%) and a training sample (remaining 85%). We fit a logit model using information on industry (as in the IAB Establishment Panel), region (*Bundesland*), wages (average monthly wage per employee), size (1-4,5-10,11-19,20-49,50-99,100-199,200-499,500 employeed and more) and age (founded before/after

1990). Panel (b) of Figure C.11 shows the number of establishments per bin of length 0.1 of the predicted probabilities on the LHS and the actual share of establishments with a works council per bin on the RHS. This indicates that the predicted probabilities are of the right order of magnitude. The ROC curve is shown in Panel (b) of Figure C.11.

As a robustness check, we run a Lasso version of the logit model described above and an alternative specification of the logit model with also includes the share of employees with high and middle education level as well the share of female employees. The prediction quality remains similar in all cases.

We use the estimated coefficients to predict the existence of a works council for establishments that start STW in 2012.

A.7 Predicting the Retention Probability based on Individual Characteristics

We predict the probability of an employee remaining with the same employer 12 months later based on individual characteristics. For this prediction, we use the universe of employees in Germany who were working on June 30, 2018, at establishments that can be linked to the firm level.

We fit a logit model using the following information: industry of the employer (1-digit), size of the employer (1-4,5-10,11-19,20-49,50-99,100-199,200-499,500 employeed and more), wage tercile at employer, occupation (*Berufssegment*, *Anforderungsniveau*), education (low, middle, high), full-time dummy, gender, tenure (year bins capped at 40) as well as age (5 year bins).

B Model Appendix

In this section, we provide further details for the model in Section 5, and discuss the decision problem based on the value functions for each of the three model periods. We start from the fixed point problem in period 3 and work backward to period 1.

Period 3. In the third period, productivity has recovered, while wages have not if there have been wage adjustments in period 1 or 2. The problem of the firm becomes a fixed point problem with the firm having its "normal" productivity state $\bar{A}$ and a wage w from period 2 that for some firms is below their "normal" level $\bar{w}$. Wages will recover over time and converge back to their normal level $\bar{w}$ at some point. We assume as a reduced form that wages recover to the "normal" level $\bar{w}$ with probability λ, which we interpret as the probability of an outside offer from another firm. We abstract from firm heterogeneity, so wages in this case will be set by mutual consent to $\bar{w}$.[15]. As we only consider downward wage rigidity, this will not affect the wage recovery. The decision for the match in period 3 is to set hours. We assume that the hours choice is made so as to maximize the joint surplus of the match $S(A)$.[16]

The value function of an employed worker at the separation stage in period 3 is $W_3(w, A)$, and $\tilde{W}_3(w, A)$ denotes the value function at the production stage. The value functions differ because of the probability that an outside offer leads to a wage change at the production stage if the current wage is below the normal wage $\bar{w}$. The value function in unemployment in period 3 is U. The value functions are:

$$\begin{aligned} U &= b + \beta U \\ W_3(\bar{A}, w) &= \lambda \tilde{W}_3(\bar{A}, \bar{w}) + (1-\lambda)\tilde{W}_3(\bar{A}, w) \\ \tilde{W}_3(\bar{A}, w) &= wh^* - \psi(h^*) + \beta\Big((1-\rho)W_3(\bar{A}, w) + \rho U\Big), \end{aligned}$$

where h^* denotes the optimal hours choice and $\psi(h)$ is the disutility from work. The worker surplus

15 Postel-Vinay and Turon (2010) provide a model with on-the-job search where outside offers lead to renegotiations and wage increases of workers on the job.

16 Note that the total surplus of the match depends only on productivity A but not on the wage w that splits the total surplus.

at the production stage, $\tilde{\Delta}$, in period 3 is:

$$\tilde{\Delta}_3(\bar{A}, w) = \tilde{W}_3(\bar{A}, w) - U = wh^* - \psi(h^*) - b + \beta\Big((1-\rho)\Big(\lambda\tilde{\Delta}_3(\bar{A}, \bar{w}) + (1-\lambda)\tilde{\Delta}_3(\bar{A}, w)\Big)\Big).$$

The value function of the firm is $J_3(\bar{A}, w)$ at the separation stage and $\tilde{J}_3(\bar{A}, w)$ at the production stage:

$$\begin{aligned} J_3(\bar{A}, w) &= (1-\lambda)\tilde{J}_3(\bar{A}, w) + \lambda\tilde{J}_3(\bar{A}, \bar{w}) \\ \tilde{J}_3(\bar{A}, w) &= \bar{A}h^* - wh^* - \kappa + \beta\Big((1-\rho)J_3(\bar{A}, w) + \rho V\Big), \end{aligned}$$

with V being the value of a vacant job that we assume to be zero throughout. The total surplus of the match at the production stage in period 3 is then given by:

$$\begin{aligned} \tilde{S}_3(\bar{A}) &= \tilde{\Delta}_3(\bar{A}, w) + \tilde{J}_3(\bar{A}, w) \\ &= \bar{A}h^* - \kappa - \psi(h^*) - b + \beta(1-\rho)\tilde{S}_3(\bar{A}), \end{aligned}$$

where we use that given the recovery of productivity there will be no endogenous separations at the separation stage of period 3. To maximize the joint surplus, hours worked are set to satisfy the first-order condition, i.e., the hours choice in period 3 solves

$$h^*(\bar{A}) = \psi'^{-1}(\bar{A}).$$

Period 2. Productivity shocks are persistent and all firms start the first period with a below-normal productivity $A < \bar{A}$. In period 2, productivity recovers with probability π, so some firms have $A = \bar{A}$ and some firms have persistent realizations with $A < \bar{A}$ from the first period. We distinguish between two types of firms regarding their eligibility status $i \in \{S, L\}$. Long PBD firms ($i = L$) have access to STW benefits in period 2, whereas short PBD firms ($i = S$) cannot rely on STW benefits in period 2. The eligibility status is a fixed institutional parameter, so all firms always know their eligibility status. We use again tildes to distinguish value functions at the separation stage from those at the production stage, and we distinguish in addition if firms use short-time

work or not if eligible. The value functions for firms of each type in the second period are:

$$J_2^i(A,w) = \alpha \max\left\{\tilde{J}_2^i(A,w), \tilde{J}_2^i(A,\hat{w})\right\} + (1-\alpha)\max\left\{\tilde{J}_2^i(A,w), V\right\}, \qquad i \in \{L,S\}, \qquad (2)$$

where $\hat{w}$ denotes the adjusted wage in case of mutual-consent wage adjustment and the index i denotes the STW eligibility status. Mutual consent to adjust the wage exists if the firm surplus $\tilde{J}_2^i(A,w) - V$ is negative but the total match surplus is positive. In this case, the wage adjustment can preserve the match by redistributing the match surplus. Under mutual-consent wage adjustment, the wage will be set such that the firm is indifferent between continuing the match and separation in which case the firm will continue with the value of a vacancy $V = 0$; hence, we yield $\tilde{J}_2^i(A,\hat{w}) = V$, so that the value reduces to $J_2^i(A,w) = \max\{\tilde{J}_2^i(A,w), V\}$. Note, however, that the case with an adjusted wage $\tilde{J}_2^i(A,\hat{w}) = V$ differs in terms of labor market flows as it is not associated with a separation. For the value at the production stage, we obtain a value for the case when using STW and when no STW is used:

$$\begin{aligned}
\tilde{J}_{2,stw}^L(A,w) &= Ah_{stw}^{L,*} - wh_{stw}^{L,*} - \kappa + \sigma\max\{\bar{h} - h_{stw}^{L,*}, 0\} + \beta\left((1-\rho)J_3(\bar{A},w) + \rho V\right) \\
\tilde{J}_{2,no}^L(A,w) &= Ah_{no}^{L,*} - wh_{no}^{L,*} - \kappa + \beta\left((1-\rho)J_3(\bar{A},w) + \rho V\right) \\
\tilde{J}_2^S(A,w) &= Ah^{S,*} - wh^{S,*} - \kappa + \beta\left((1-\rho)J_3(\bar{A},w) + \rho V\right),
\end{aligned}$$

where $\hat{w}$ denotes an adjusted wage and $\bar{h}$ denotes the hours threshold to be eligible for STW benefits σ, i.e., it must hold that $h_{stw}^{L,*} \leq \bar{h}$. We follow Cahuc, Kramarz, Nevoux, and Vieira (2021) for the specification of STW benefits with subsidy σ for hours below the eligibility threshold $\bar{h}$. The STW choice is described below. The value functions of the worker at the separation stage is

$$\begin{aligned}
W_2^i(A,w) &= \alpha\Big(\max\{\tilde{W}_2^i(A,\hat{w}), U\}\mathbb{1}(\tilde{J}_2^i(A,w) < V) + \tilde{W}_2^i(A,w)\mathbb{1}(\tilde{J}_2^i(A,w) \geq V)\Big) \\
&\quad +(1-\alpha)\Big(U\mathbb{1}(\tilde{J}_2^i(A,w) < V) + \tilde{W}_2^i(A,w)\mathbb{1}(\tilde{J}_2^i(A,w) \geq V)\Big), \qquad i \in \{S,L\},
\end{aligned}$$

where $\mathbb{1}(\cdot)$ denotes the indicator function that is one if the surplus of the firm is positive, i.e., if $\tilde{J}_2^i(A,w) \geq V$ holds. The max operator in case of wage adjustment insures that if the worker surplus turns negative at the adjusted wage, then the match will separate. At the production stage, we

distinguish by the usage of STW $j \in \{stw, no\}$ for the case of long PBD ($i = L$):

$$\begin{aligned}
\tilde{W}^L_{2,j}(A,w) &= wh^{L,*}_j - \psi(h^{L,*}_j) + \beta\Big((1-\rho)W_3(\bar{A},w) + \rho U\Big), \qquad j \in \{stw, no\} \\
\tilde{W}^S_2(A,w) &= wh^{S,*} - \psi(h^{S,*}) + \beta\Big((1-\rho)W_3(\bar{A},w) + \rho U\Big). \\
U &= b + \beta U
\end{aligned}$$

The resulting worker surplus at the production stage is

$$\begin{aligned}
\tilde{\Delta}^L_{2,j}(A,w) &= wh^{L,*}_j - \psi(h^{L,*}_j) - b + \beta(1-\rho)\Big(\lambda\tilde{\Delta}_3(\bar{A},\bar{w}) + (1-\lambda)\tilde{\Delta}_3(\bar{A},w)\Big), \qquad j \in \{stw, no\} \\
\tilde{\Delta}^S_2(A,w) &= wh^{S,*} - \psi(h^{S,*}) - b + \beta(1-\rho)\Big(\lambda\tilde{\Delta}_3(\bar{A},\bar{w}) + (1-\lambda)\tilde{\Delta}_3(\bar{A},w)\Big).
\end{aligned}$$

When STW is used, we assume that the match maximizes the joint surplus. Given that the decision to implement STW is negotiated between the firm and the works council in Germany, matching our empirical setting, surplus maximization is a reasonable approximation. Only firms with long PBD have the option to use STW in the second period. Firms with short PBD will have exhausted their eligibility after having used it in the first period. The joint surplus from using and not using STW in period 2 is, respectively,

$$\begin{aligned}
\tilde{S}^L_{2,stw}(A) &= \tilde{\Delta}^L_{2,stw}(A,w) + \tilde{J}^L_{2,stw}(A,w) = Ah^{L,*}_{stw} - \kappa - \psi(h^{L,*}_{stw}) - b + \sigma\max\{\bar{h} - h^{L,*}_{stw}, 0\} + \beta(1-\rho)\tilde{S}_3(\bar{A}) \\
\tilde{S}^L_{2,no}(A) &= \tilde{\Delta}^L_{2,no}(A,w) + \tilde{J}^L_{2,no}(A,w) = Ah^{L,*}_{no} - \kappa - \psi(h^{L,*}_{no}) - b + \beta(1-\rho)\tilde{S}_3(\bar{A}),
\end{aligned}$$

where we have used that $V = 0$. Hours conditional on using STW or not are chosen so as to maximize the joint surplus. First-order conditions for hours taking into account the eligibility condition for the hours reduction and the non-negativity constraint imply:

$$h^{L,*}_{stw}(A) = \min\Big\{\max\{0, \psi'^{-1}(A-\sigma)\}, \bar{h}\Big\} \tag{3}$$

$$h^{L,*}_{stw}(A) = \psi'^{-1}(A). \tag{4}$$

For the case of short PBD, the hours choice is characterized by $h^{S,*}(A) = \psi'^{-1}(A)$. The STW decision is assumed to maximize the joint surplus, so the match will rely on STW if $\tilde{S}^L_{2,stw}(A) >$

$\tilde{S}^L_{2,no}(A)$, and not otherwise. Using this STW decision, we can define:

$$\begin{aligned}\tilde{\Delta}^L_2(A,w) &= \mathbb{1}(\tilde{S}^L_{2,stw}(A) > \tilde{S}^L_{2,no}(A))\tilde{\Delta}^L_{2,stw}(A,w) + \mathbb{1}(\tilde{S}^L_{2,stw}(A) \leq \tilde{S}^L_{2,no}(A))\tilde{\Delta}^L_{2,no}(A,w)\\ \tilde{J}^L_2(A,w) &= \mathbb{1}(\tilde{S}^L_{2,stw}(A) > \tilde{S}^L_{2,no}(A))\tilde{J}^L_{2,stw}(A,w) + \mathbb{1}(\tilde{S}^L_{2,stw}(A) \leq \tilde{S}^L_{2,no}(A))\tilde{J}^L_{2,no}(A,w),\end{aligned}$$

where $\mathbb{1}(\cdot)$ denotes again an indicator function. The joint match surplus at the production stage is then $\tilde{S}^i_2(A) = \tilde{J}^i_2(A,w) + \tilde{\Delta}^i_2(A,w)$ for $i \in \{L,S\}$.

Period 1. The first period is similar to the second period except that now all firms are eligible to use STW and productivity has not yet recovered, so that all firms have a productivity level $A < \bar{A}$. We still need to distinguish between firms with short and long PBD because of their different continuation values in the second period.

Firms with a long PBD that are still eligible in the second period will have a higher expected firm surplus and, therefore, have less often a binding participation constraint in the first period. A binding participation constraint, i.e., $J_1(A,w) \leq 0$, will induce mutual consent to adjust the wage. The value functions of the two firm types at the separation and production stage are:

$$J^i_1(A,w) = \alpha \max\left\{\tilde{J}^i_1(A,w), \tilde{J}^i_1(A,\hat{w})\right\} + (1-\alpha)\max\left\{\tilde{J}^i_1(A,w), V\right\}, \qquad i \in \{L,S\}, \qquad (5)$$

where $\hat{w}$ denotes the adjusted wage in case of mutual consent for wage adjustment. The worker value function at the separation stage is

$$\begin{aligned}W^i_1(A,w) &= \alpha\Big(\max\{\tilde{W}^i_1(A,\hat{w}), U\}\mathbb{1}(\tilde{J}^i_1(A,w) < V) + \tilde{W}^i_1(A,w)\mathbb{1}(\tilde{J}^i_1(A,w) \geq V)\Big)\\ &\quad +(1-\alpha)\Big(U\mathbb{1}(\tilde{J}^i_1(A,w) < V) + \tilde{W}^i_1(A,w)\mathbb{1}(\tilde{J}^i_1(A,w) \geq V)\Big), \qquad i \in \{S,L\},\end{aligned}$$

where again the first max operator implies that if the adjusted wage $\hat{w}$ will be associated with a negative worker surplus, then the match will separate. The adjusted wage $\hat{w}$ implies $\tilde{J}^i_1(A,\hat{w}) = 0$, so that a negative worker surplus at that wage is equivalent to a negative total match surplus.

The value functions of firms at the production stage when using STW or not are, respectively,

$$\tilde{J}^i_{1,stw}(A,w) = Ah^{i,*}_{stw} - wh^{i,*}_{stw} - \kappa + \sigma\max\{\bar{h} - h^{i,*}_{stw}, 0\} + \beta(1-\rho)\Big(\pi J^i_2(\bar{A},w) + (1-\pi)J^i_2(A,w)\Big)$$

$$\tilde{J}^i_{1,no}(A,w) = Ah^{i,*}_{no} - wh^{i,*}_{no} - \kappa + \beta(1-\rho)\Big(\pi J^i_2(\bar{A},w) + (1-\pi)J^i_2(A,w)\Big),$$

and the corresponding value functions for the worker are

$$\begin{aligned}\tilde{W}^i_{1,j}(A,w) &= wh^{i,*}_j - \psi(h^{i,*}_j) + \beta(1-\rho)\Big(\pi W^i_2(\bar{A},w) + (1-\pi)W^i_2(A,w)\Big) + \beta\rho U,\ i\in\{L,S\},\ j\in\{stw,no\}\\ U &= b + \beta U.\end{aligned}$$

The worker surplus with and without STW at the production stage is

$$\tilde{\Delta}^i_{1,j}(A,w) = wh^{i,*}_j - \psi(h^{i,*}_j) - b + \beta(1-\rho)\Big(\pi\Delta^i_2(\bar{A},w) + (1-\pi)\Delta^i_2(A,w)\Big),\ i\in\{L,S\},\ j\in\{stw,no\},$$

with $\Delta^i_2(A,w) = W^i_2(A,w) - U$ denoting the worker surplus at the separation stage.

The total match surplus at the production stage is:

$$\begin{aligned}\tilde{S}^i_{1,stw}(A) &= \tilde{\Delta}^i_{1,stw}(A,w) + \tilde{J}^i_{1,stw}(A,w),\ i\in\{L,S\}\\ &= Ah^{i,*}_{stw} - \kappa + \sigma\max\{\bar{h} - h^{i,*}_{stw}, 0\} - \psi(h^{i,*}_{stw}) - b\\ &\quad +\beta(1-\rho)\Big(\pi(\Delta^i_2(\bar{A},w) + J^i_2(\bar{A},w)) + (1-\pi)(\Delta^i_2(A,w) + J^i_2(A,w))\Big)\\ \tilde{S}^i_{1,no}(A) &= \tilde{\Delta}^i_{1,no}(A,w) + \tilde{J}^i_{1,no}(A,w),\ i\in\{L,S\}\\ &= Ah^{i,*}_{no} - \kappa - \psi(h^{i,*}_{no}) - b\\ &\quad +\beta(1-\rho)\Big(\pi(\Delta^i_2(\bar{A},w) + J^i_2(\bar{A},w)) + (1-\pi)(\Delta^i_2(A,w) + J^i_2(A,w))\Big).\end{aligned}$$

Maximizing the surplus over the hours choice is an intratemporal decision, so that the optimal hours choice only depends on current productivity A and not on the eligibility status. The optimal choice is therefore characterized again by equations (3) and (4). The decision to rely on STW or not is taken to maximize again the total match surplus

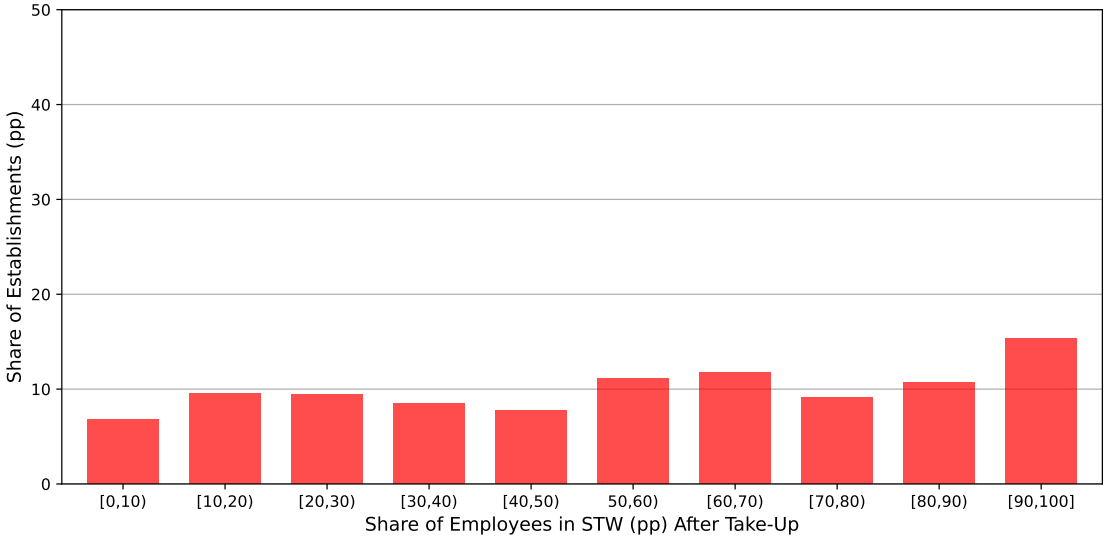
$$\tilde{S}^i_1(A) = \max\{\tilde{S}^i_{1,stw}(A), \tilde{S}^i_{1,no}(A)\} \tag{6}$$

and the worker and firm surplus follow accordingly. For wages, mutual-consent bargaining implies that at the separation stage wages will be adjusted if the firm has a binding participation constraint

$J_1^i(A, w) < 0$. The adjusted wage $\hat{w}$ is characterized again by the condition that the firm has a continuation value of zero $J_1^i(A, \hat{w}) = 0$, so it is indifferent between separation and continuation of the match. If such a wage will imply a negative surplus of the worker from continuing the match, the match will separate.

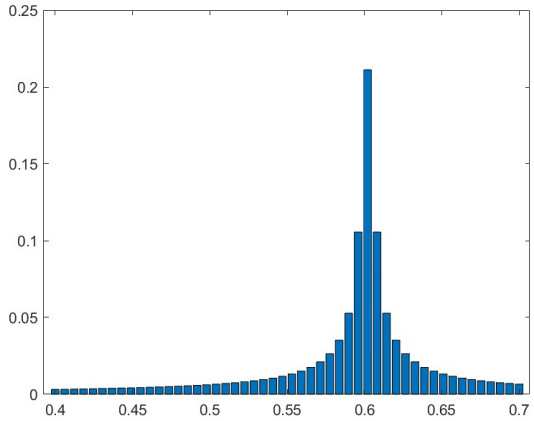
C Supplementary Figures

Figure C.1: Within-Firm Distribution of STW Take-Up



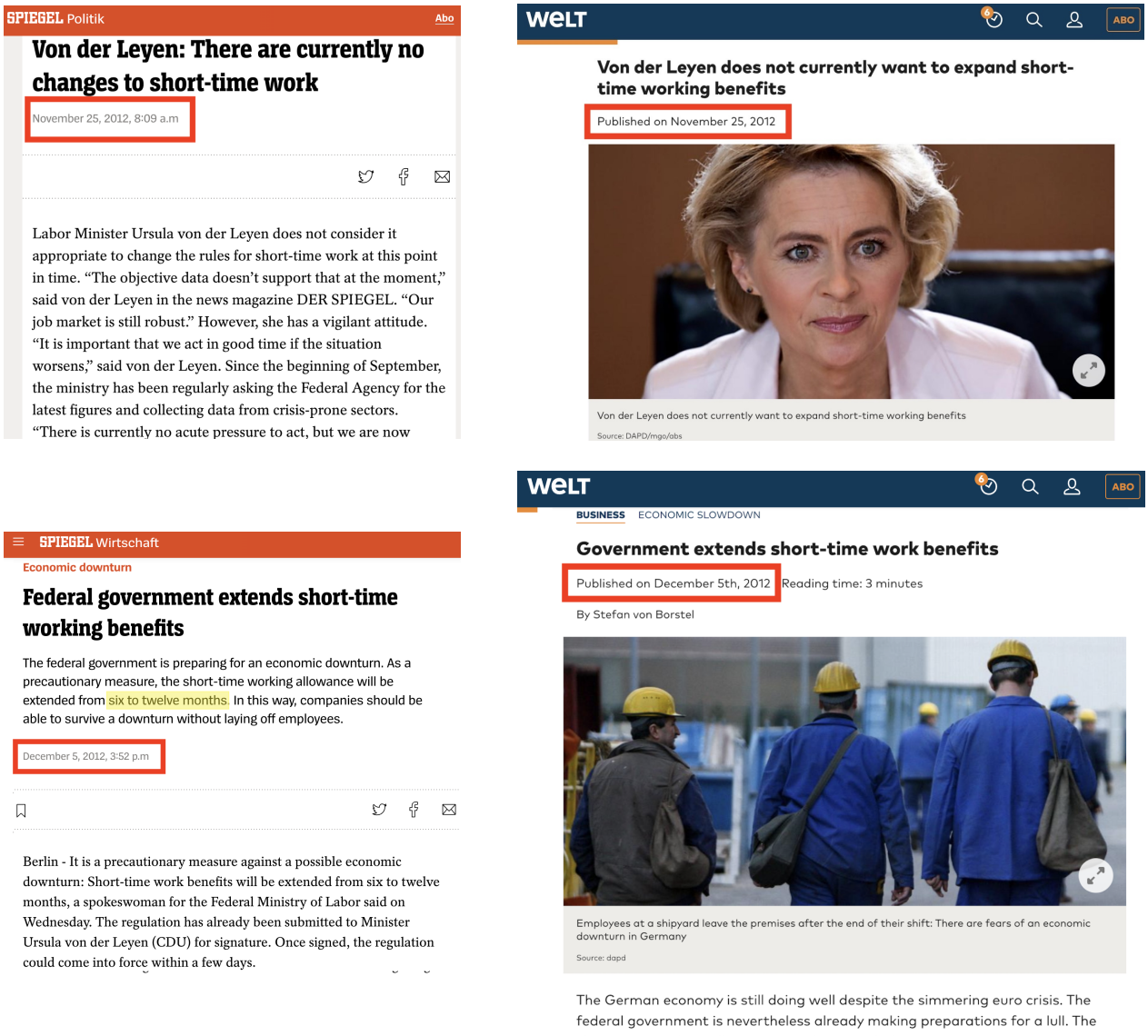
Notes: This figure shows the share of employees in STW per establishment in the start month of a STW spell. We consider all STW spells in Germany since 2009, with the same sample restrictions as in Panel (a) of Figure 1. In a small number of cases of multi-establishment firms (3,254 of 481,137), the reported number of employees in STW exceeds establishment-level employment and we set the share to 100%.

Figure C.2: Model Productivity Distribution



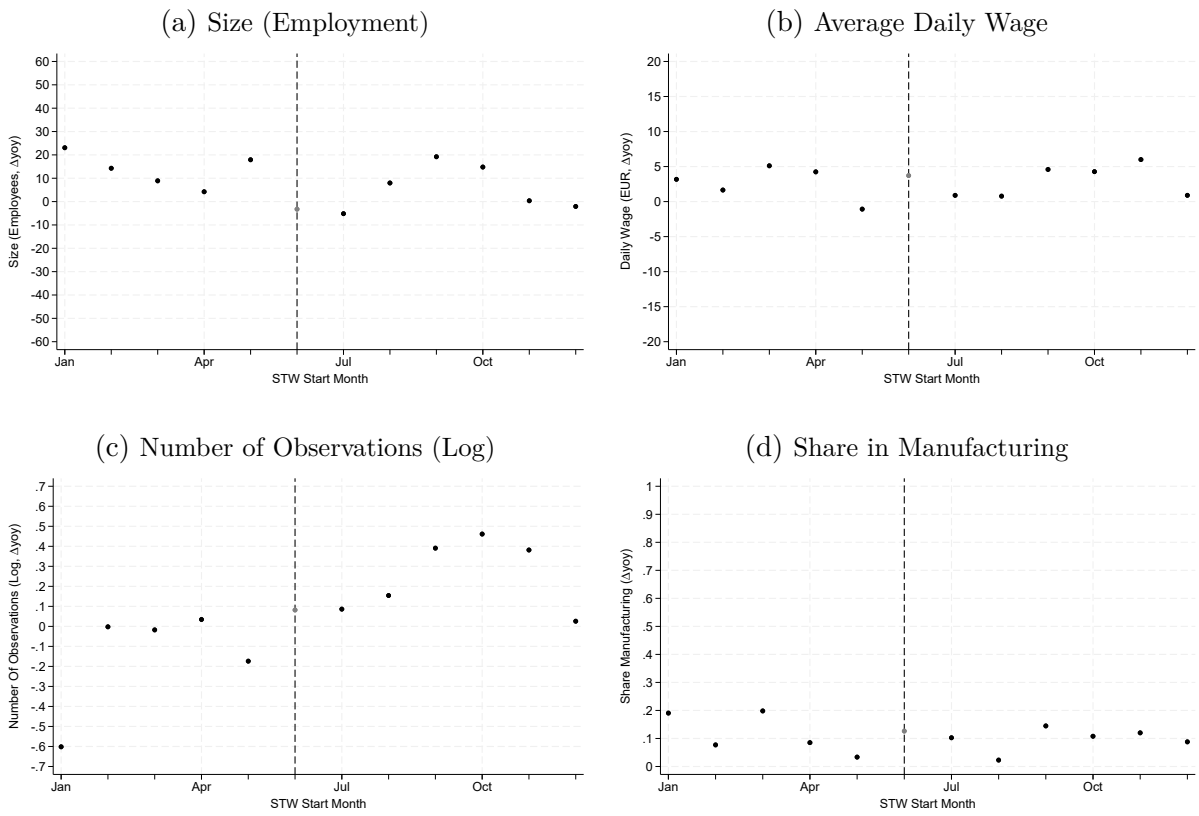
Notes: This figure plots the productivity distribution in our quantitative model after the shock. Normal productivity level is set to $A = 1$.

Figure C.3: Newspaper Coverage in November and December 2012 (translated)



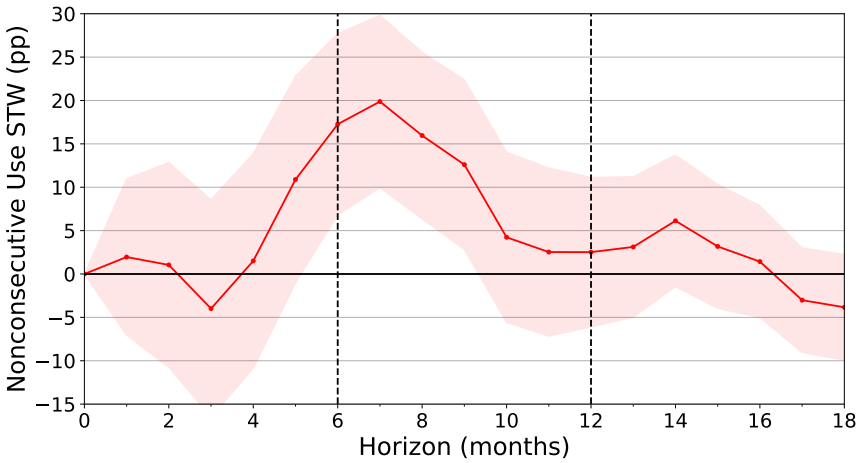
Notes: We include screenshots of newspaper coverage in two highly visible news outlets, the magazine *Der Spiegel* and the newspaper *Die Welt*, respectively. The top row shows news articles published on November 25, 2012, and highlights the stance of the Federal Labor Minister, Ursula von der Leyen, opposing STW extensions. The bottom row shows news articles from December 5, 2012, by which time the government had sharply reversed coures and announced a doubling of STW PBD. We translated the screenshots using Google Translate and added highlights in red around the dates as well as in yellow marking the policy change from 6 to 12 months of PBD.

Figure C.4: RD Design for 2012 Reform: Balancedness and Take-Up of Extended Benefits



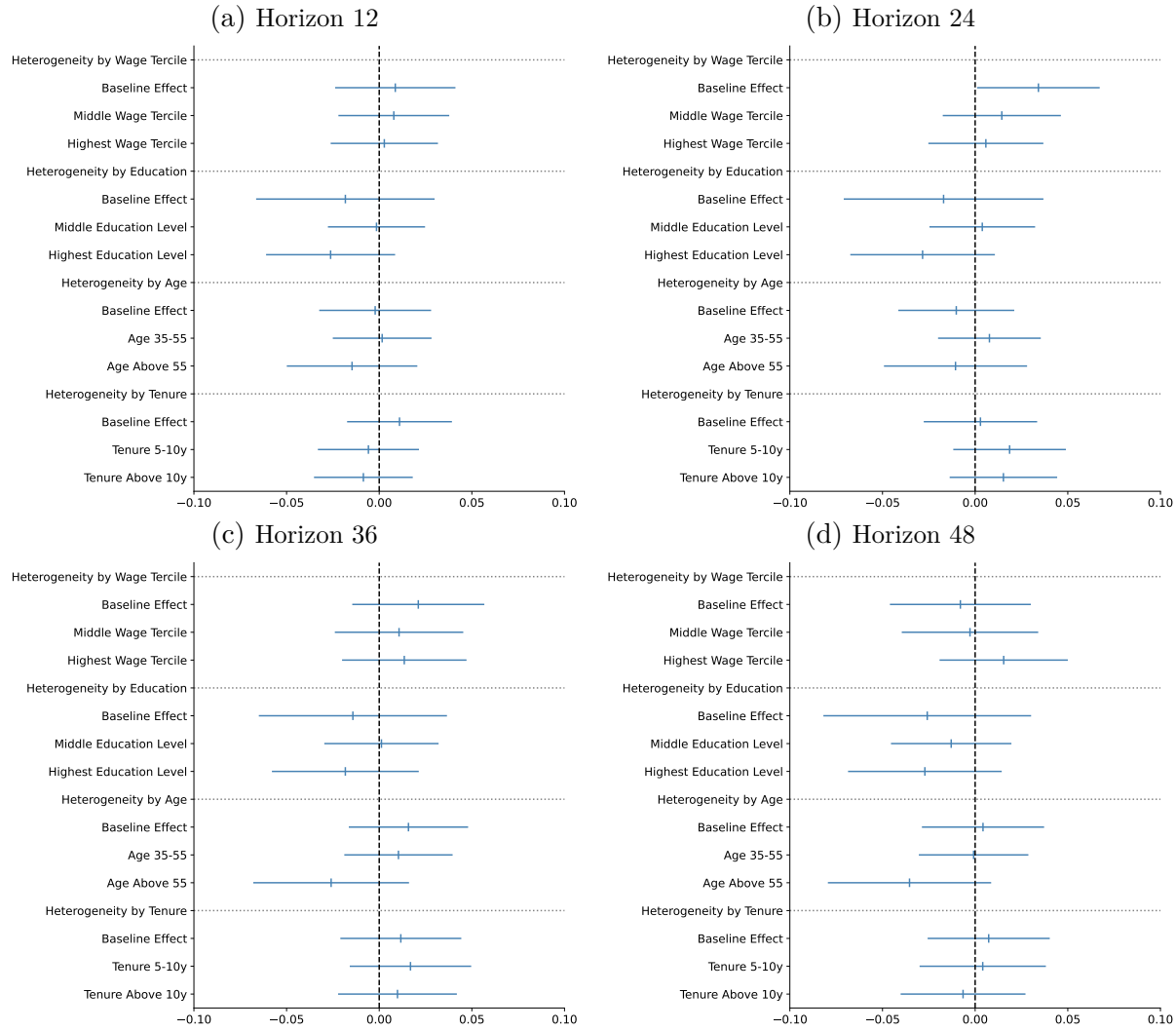
Notes: The figure plots firm characteristics by timing of the start of STW (x-axis). We compute the number of firms in each cohort of the same start month, as well as cohort means of employment in the start month, average daily wage in the start month, and a dummy whether the firm is in the manufacturing sector. Employment and wages are winsorized at the 1% level.

Figure C.5: Take-Up of STW Benefits in Extension Period (RD Estimates)



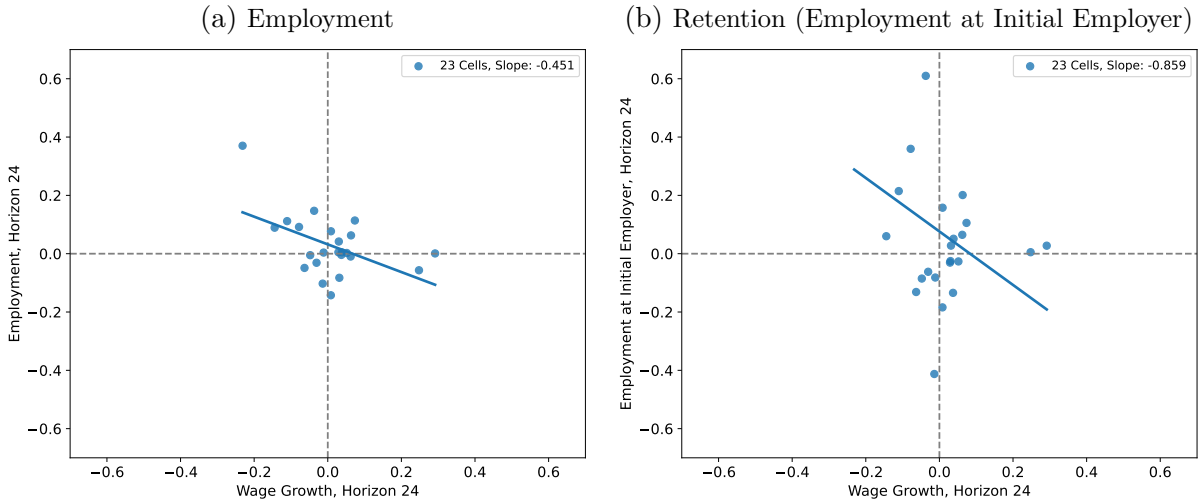
Notes: The figure plots the effect of the 2012 PBD reform at different horizons after the start of STW using as outcome variable an indicator variable that is equal to one if the firm still receives STW benefits regardless of the STW spell (*Nonconsecutive Use STW*). We report the treatment effects using the regression discontinuity design specified in (1) including industry by region fixed effects. The data is at the firm-horizon level; a separate regression is run for each horizon. 95% confidence intervals based on robust standard errors are depicted. The sample is restricted to firms that in the start month have more than five employees in full-time who are fully liable to social security.

Figure C.6: Heterogeneity by Demographics: Employment



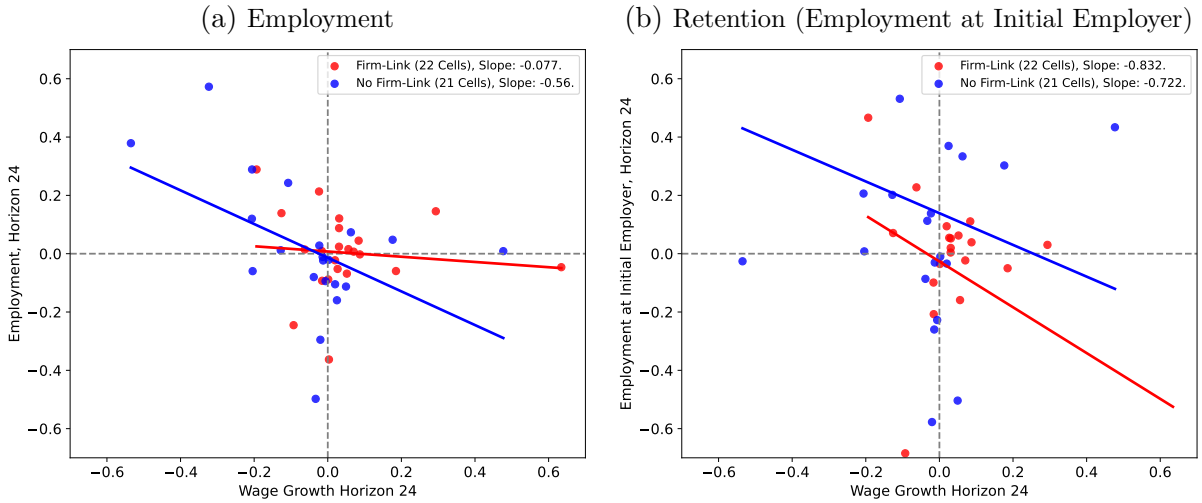
Notes: The figure plots heterogeneous employment effects by demographics at different horizons after the start of STW. We define groups within firms based on demographic characteristics at the start of STW (age, tenure at the firm, education level, wage tercile within the firm). The data is at the group-firm-horizon level. The coefficients shown are heterogeneous treatment effects of a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. As outcome variable, we use for each group-firm cell the share of initially employed (i.e. employed at the start of STW) who are employed anywhere. The baseline education level is defined as no training or missing information, individuals with a middle (high) education level have a vocational training (hold a degree from an university of university of applied sciences). The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security.

Figure C.7: Interdependence of Employment and Wage Effects of STW PBD: Evidence Across Labor Market Cells 24 Months After Start



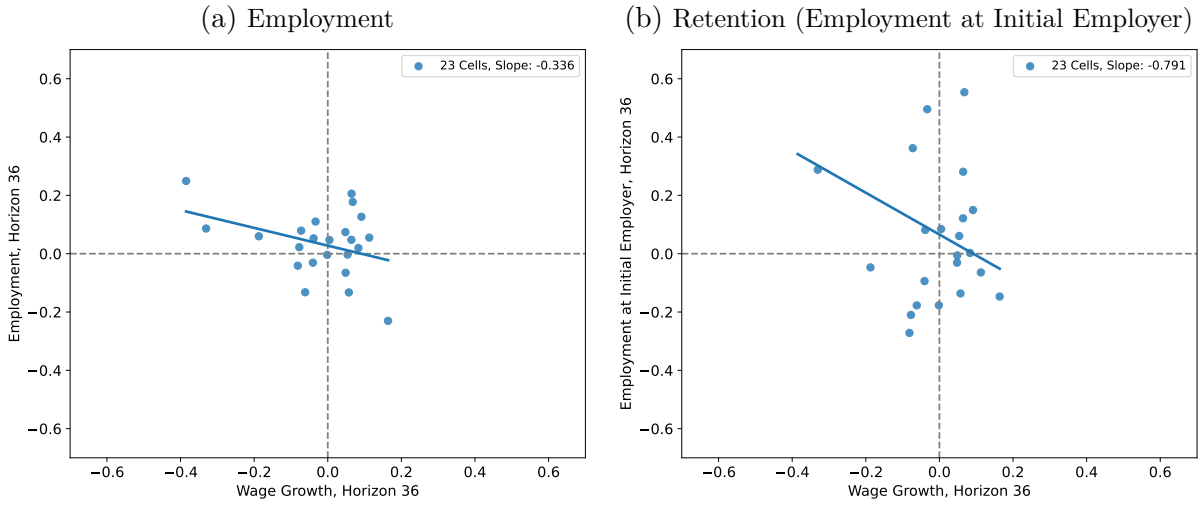
Notes: The figure plots the treatment effect on employment anywhere (Panel (a)) and retention (Panel (b)) on the y-axis against the treatment effect on wage growth on the x-axis in different cells 24 months after the start of STW at the establishment level. Establishments are assigned to cells based on their sector (manufacturing (43%), wholesale and retail trade (14%), rest (43%)), region (East (28%), West (72%)), and size (up to 5 (51%), 6-15 (23%), 16-50 (15%), more than 50 employees (11%)). One cell (wholesale and retail trade, east, more than 50 employees) is excluded because there are too few observations. In Panel (a), the outcome variable for employment is for each firm the share of initially employed (i.e., employed at the start of STW) who are employed anywhere. In Panel (b), the outcome variable for employment is for each firm the share of initially employed who are still employed at the same firm. Potential re-entries after an exit are ignored. Wage growth is the growth rate of average daily wages relative to the start of STW. We report treatment effects using the regression discontinuity design specified in (1) at the establishment level without industry by region fixed effects. The sample is restricted to employees in full-time that are fully liable to social security.

Figure C.8: Evidence Across Labor Market Cells 24 Months After Start: Establishments w/ and w/o Firm Link



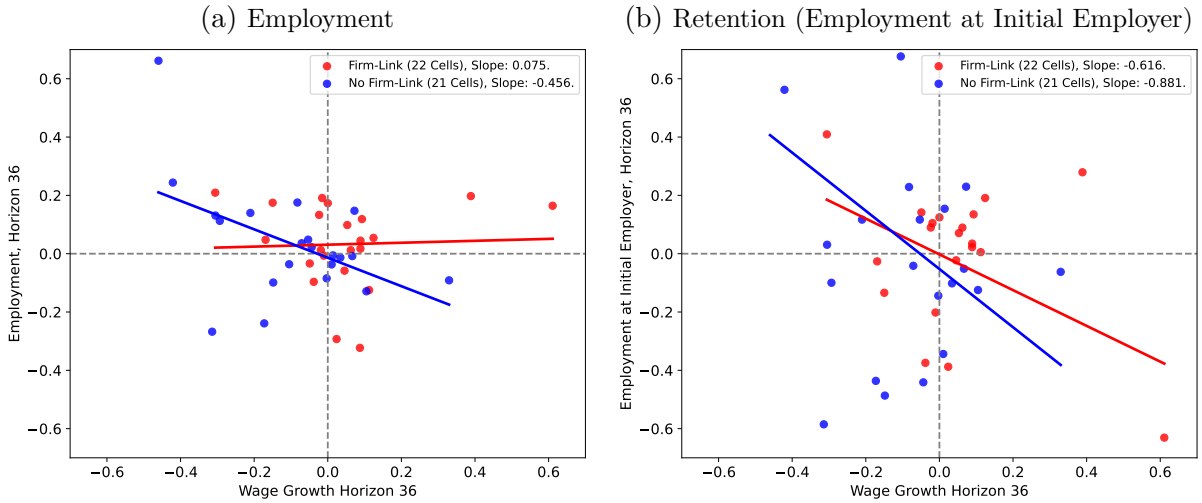
Notes: The figure plots the treatment effect on employment anywhere (Panel (a)) and retention (Panel (b)) on the y-axis against the treatment effect on wage growth on the x-axis in different cells 24 months after the start of STW, separately for establishments that can be linked to the firm level (red) or not (blue). Orbis-ADIAB (see Antoni, Koller, Laible, and Zimmermann, 2018, for details) is used for linking establishments to firms. Establishments are assigned to cells based on their sector (manufacturing (43%), wholesale and retail trade (14%), rest (43%)), region (East (28%), West (72%)), and size (up to 5 (51%), 6-15 (23%), 16-50 (15%), more than 50 employees (11%)). One cell (wholesale and retail trade, east, more than 50 employees) is excluded because there are too few observations. In Panel (a), the outcome variable for employment is for each firm the share of initially employed (i.e., employed at the start of STW) who are employed anywhere. In Panel (b), the outcome variable for employment is for each firm the share of initially employed who are still employed at the same firm. Potential re-entries after an exit are ignored. Wage growth is the growth rate of average daily wages relative to the start of STW. We report treatment effects using the regression discontinuity design specified in (1) at the establishment level without industry by region fixed effects. The sample is restricted to employees in full-time that are fully liable to social security.

Figure C.9: Interdependence of Employment and Wage Effects of STW PBD: Evidence Across Labor Market Cells 36 Months After Start



Notes: The figure plots the treatment effect on employment anywhere (Panel (a)) and retention (Panel (b)) on the y-axis against the treatment effect on wage growth on the x-axis in different cells 36 months after the start of STW at the establishment level. Establishments are assigned to cells based on their sector (manufacturing (43%), wholesale and retail trade (14%), rest (43%)), region (East (28%), West (72%)), and size (up to 5 (51%), 6-15 (23%), 16-50 (15%), more than 50 employees (11%)). One cell (wholesale and retail trade, east, more than 50 employees) is excluded because there are too few observations. In Panel (a), the outcome variable for employment is for each firm the share of initially employed (i.e., employed at the start of STW) who are employed anywhere. In Panel (b), the outcome variable for employment is for each firm the share of initially employed who are still employed at the same firm. Potential re-entries after an exit are ignored. Wage growth is the growth rate of average daily wages relative to the start of STW. We report treatment effects using the regression discontinuity design specified in (1) at the establishment level without industry by region fixed effects. The sample is restricted to employees in full-time that are fully liable to social security.

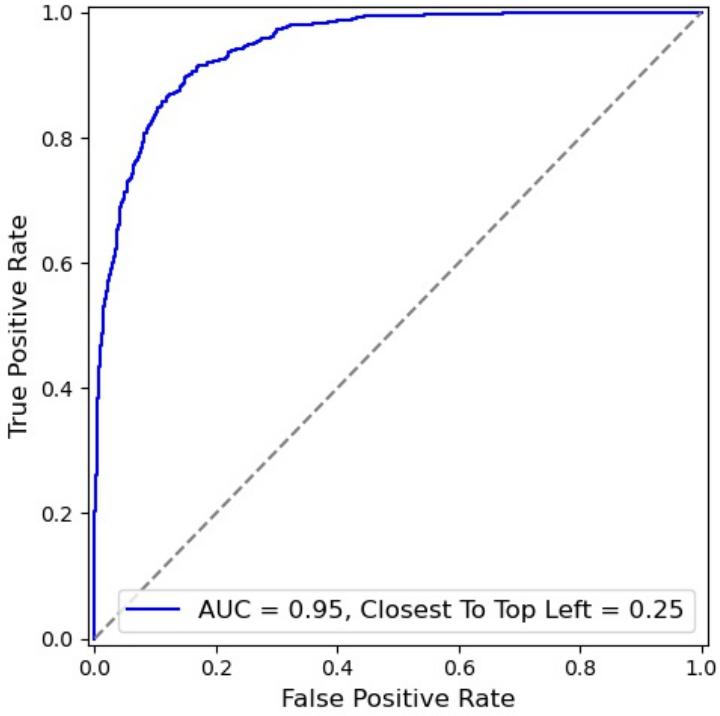
Figure C.10: Evidence Across Labor Market Cells 36 Months After Start: Establishments w/ and w/o Firm Link



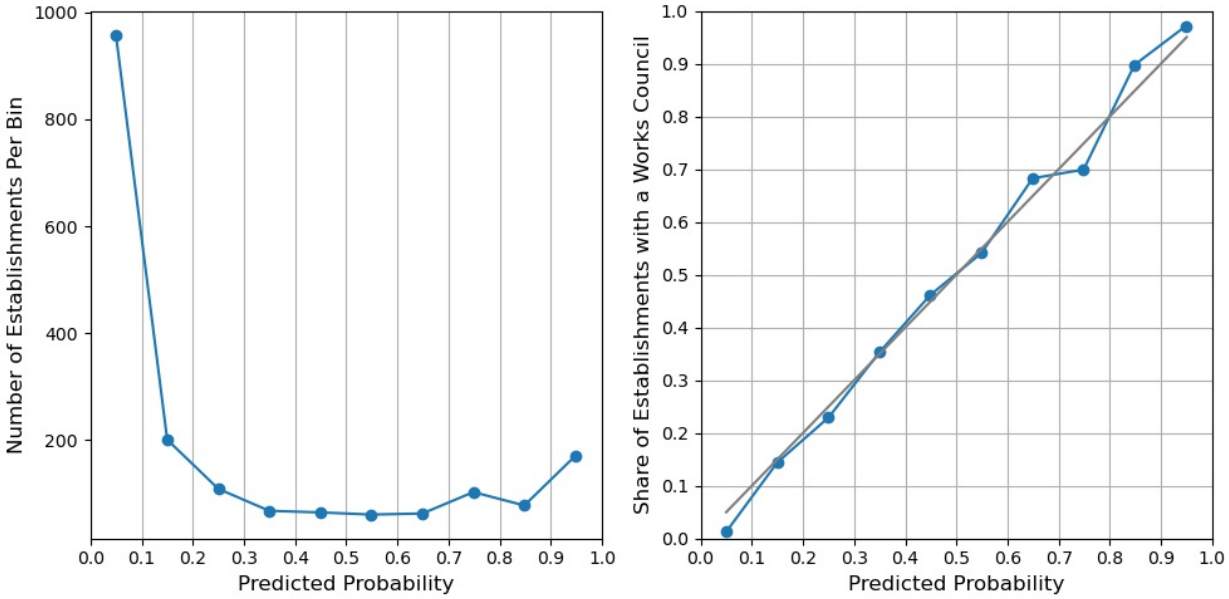
Notes: The figure plots the treatment effect on employment anywhere (Panel (a)) and retention (Panel (b)) on the y-axis against the treatment effect on wage growth on the x-axis in different cells 36 months after the start of STW, separately for establishments that can be linked to the firm level (red) or not (blue). Orbis-ADIAB (see Antoni, Koller, Laible, and Zimmermann, 2018, for details) is used for linking establishments to firms. Establishments are assigned to cells based on their sector (manufacturing (43%), wholesale and retail trade (14%), rest (43%)), region (East (28%), West (72%)), and size (up to 5 (51%), 6-15 (23%), 16-50 (15%), more than 50 employees (11%)). One cell (wholesale and retail trade, east, more than 50 employees) is excluded because there are too few observations. In Panel (a), the outcome variable for employment is for each firm the share of initially employed (i.e., employed at the start of STW) who are employed anywhere. In Panel (b), the outcome variable for employment is for each firm the share of initially employed who are still employed at the same firm. Potential re-entries after an exit are ignored. Wage growth is the growth rate of average daily wages relative to the start of STW. We report treatment effects using the regression discontinuity design specified in (1) at the establishment level without industry by region fixed effects. The sample is restricted to employees in full-time that are fully liable to social security.

Figure C.11: Evaluation of Prediction (based on IAB Establishment Panel)

(a) ROC Curve



(b) Distribution of Predicted Probabilities



Notes: The figure shows an evaluation of the prediction of existence of a works council based on the IAB Establishment Panel. We split the IAB Establishment panel into a random test sample (15%) and training sample, and present the results on the test sample. Panel (a) shows the receiver operating characteristic curve (ROC curve) for a logit specification described in Appendix A.6. Panel (b) shows the results of a simple evaluation whether the predicted probabilities are of the right order of magnitude. The LHS of Panel (b) shows the distribution of predicted probabilities. The chosen bin size is 0.1 and midpoints of bins are shown. The RHS of Panel (b) shows for each bin (x-axis) the true share of establishments with a works council (y-axis).

D Supplementary Tables

Table D.1: Summary Statistics by Match Outcome

	Starter in 2012		w/ Firm-Link		w/o Firm-Link	
	N	Mean	N	Mean	N	Mean
Share in East Germany	9813	0.26	5425	0.24	4388	0.28
Age	9813	18.13	5425	19.28	4388	16.71
Average Daily Wage (Start Month)	9813	78.18	5425	84.80	4388	70.01
Share in Manufacturing	9813	0.49	5425	0.58	4388	0.38
Size (Start Month)						
1-4 Employees	9813	0.42	5425	0.26	4388	0.63
5-9 Employees	9813	0.19	5425	0.20	4388	0.17
10-19 Employees	9813	0.14	5425	0.19	4388	0.08
20-49 Employees	9813	0.12	5425	0.17	4388	0.06
50-99 Employees	9813	0.06	5425	0.08	4388	0.03
100-199 Employees	9813	0.04	5425	0.05	4388	0.02
200-499 Employees	9813	0.02	5425	0.03	4388	0.01
More Than 500 Employees	9813	0.01	5425	0.01	4388	0.00

Notes: The table shows summary statistics of establishments that start STW in 2012. The first two columns include all establishments (no size restrictions), the middle two columns the subset thereof that can be linked to the firm level using Orbis-ADIAB and the last two the subset thereof for which no such link can be established. Size refers to employment in the start month of STW including only employees in full-time who are fully liable to social security.

Table D.2: Complier Analysis

	(1)	(2)	(3)	(4)
Log Employees (Start Month)	-0.004	-0.011	-0.015*	-0.015*
	(0.009)	(0.009)	(0.009)	(0.009)
Log Avrg Daily Wage (Start Month)		0.091*	0.089*	0.084*
		(0.049)	(0.049)	(0.050)
Age			0.004***	0.004***
			(0.001)	(0.001)
Pre-Period Wage Growth				0.033
				(0.192)
Start Month FE	Yes	Yes	Yes	Yes
Industry × Region FE	Yes	Yes	Yes	Yes
N Firms	1,762	1,762	1,762	1,750
R Squared	0.071	0.074	0.083	0.082
R Squared Adj.	0.024	0.026	0.035	0.033

Notes: The table shows the results of a regression of a dummy indicating a STW benefit duration that exceeds 6 months (*Complier*) on firm characteristics. The sample consists of firms that start STW between 2012m7 and 2012m12. *Pre-Period Wage Growth* is the 1y-growth rate in average wages based on employees that were employed at the respective firm 12 months prior to the start of STW. The sample is restricted to firms that in the start month have more than five employees in full-time who are fully liable to social security. Robust standard errors are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.3: Heterogeneity in the RD Effect of PBD Extension by Wage Terciles

	Employment at Initial Employer, Horizon (months)					
	6	12	18	24	36	48
Running Variable	-0.003	0.009	0.009	0.011	0.012	0.013
	(0.005)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Running Variable × Middle Wage Tercile	-0.002	-0.004*	-0.006**	-0.006***	-0.007***	-0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Running Variable × Highest Wage Tercile	-0.002	-0.005**	-0.005**	-0.006**	-0.007***	-0.006**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Treatment (12m PBD) × Running Variable	0.008	-0.010	-0.008	-0.013	-0.011	-0.011
	(0.008)	(0.010)	(0.012)	(0.012)	(0.012)	(0.012)
Treatment (12m PBD) × Running Variable × Middle Wage Tercile	0.001	0.005	0.004	0.007	0.005	0.004
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Treatment (12m PBD) × Running Variable × Highest Wage Tercile	-0.003	0.006	0.003	0.005	0.003	-0.000
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Treatment (12m PBD)	0.012	0.012	-0.002	-0.004	-0.014	-0.032
	(0.031)	(0.040)	(0.046)	(0.047)	(0.048)	(0.047)
Treatment (12m PBD) × Middle Wage Tercile	-0.013	-0.012	-0.005	-0.012	0.000	0.003
	(0.015)	(0.017)	(0.019)	(0.018)	(0.018)	(0.018)
Treatment (12m PBD) × Highest Wage Tercile	0.005	-0.008	-0.003	-0.009	0.011	0.024
	(0.016)	(0.018)	(0.020)	(0.020)	(0.020)	(0.019)
Middle Wage Tercile	0.053***	0.066***	0.069***	0.070***	0.069***	0.064***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Highest Wage Tercile	0.062***	0.083***	0.097***	0.102***	0.102***	0.100***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Industry × Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	11,801	11,801	11,801	11,801	11,801	11,801
N Individuals	639,228	639,228	639,228	639,228	639,228	639,228

Notes: The table reports heterogeneous treatment effects of the 2012 PBD reform by within-firm wage tercile on employment at different horizons after the start of STW. We define groups within firms based on demographic characteristics at the start of STW. The data is a balanced panel at the group-firm-horizon level. The results are from a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. The outcome variable is for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm. Potential re-entries after an exit are ignored. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown is the number of clusters including firms that start in 2011. The number of individuals refers to the number of individuals the calculation is based upon. Robust standard errors clustered at the firm level are reported in parentheses. The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.4: Heterogeneity in the RD Effect of PBD Extension by Education

	Employment at Initial Employer, Horizon (months)					
	6	12	18	24	36	48
Running Variable	-0.005	0.006	0.006	0.005	0.007	0.012
	(0.006)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Running Variable × Middle Education Level	-0.001	-0.004	-0.001	0.001	0.002	0.000
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Running Variable × Highest Education Level	0.001	-0.000	0.005	0.004	0.003	-0.000
	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Treatment (12m PBD) × Running Variable	0.008	0.002	0.001	-0.001	-0.003	-0.013
	(0.009)	(0.012)	(0.013)	(0.014)	(0.015)	(0.015)
Treatment (12m PBD) × Running Variable × Middle Education Level	-0.001	-0.005	-0.011	-0.008	-0.008	0.002
	(0.007)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)
Treatment (12m PBD) × Running Variable × Highest Education Level	0.000	-0.001	-0.010	-0.004	-0.004	0.004
	(0.009)	(0.011)	(0.012)	(0.012)	(0.013)	(0.013)
Treatment (12m PBD)	0.025	-0.002	-0.004	-0.005	-0.002	0.007
	(0.035)	(0.044)	(0.052)	(0.056)	(0.058)	(0.057)
Treatment (12m PBD) × Middle Education Level	-0.014	0.016	0.010	-0.001	-0.006	-0.039
	(0.029)	(0.033)	(0.040)	(0.042)	(0.043)	(0.043)
Treatment (12m PBD) × Highest Education Level	-0.017	-0.015	-0.022	-0.037	-0.030	-0.045
	(0.037)	(0.041)	(0.047)	(0.047)	(0.050)	(0.050)
Middle Education Level	0.013*	0.013	0.018*	0.013	0.021**	0.019*
	(0.007)	(0.009)	(0.010)	(0.010)	(0.011)	(0.010)
Highest Education Level	0.024***	0.034***	0.041***	0.030**	0.038***	0.039***
	(0.008)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)
Industry × Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	9,307	9,307	9,307	9,307	9,307	9,307
N Individuals	652,717	652,717	652,717	652,717	652,717	652,717

Notes: The table reports heterogeneous treatment effects of the 2012 PBD reform by education on employment at different horizons after the start of STW. The baseline education level is defined as no training or missing information, individuals with a middle (high) education level have vocational training (hold a degree from an university of university of applied sciences). We define groups within firms based on demographic characteristics at the start of STW. The data is a balanced panel at the group-firm-horizon level. The results are from a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. The outcome variable is for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm. Potential re-entries after an exit are ignored. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown is the number of clusters including firms that start in 2011. The number of individuals refers to the number of individuals the calculation is based upon. Robust standard errors clustered at the firm level are reported in parentheses. The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.5: Heterogeneity in the RD Effect of PBD Extension by Age

	Employment at Initial Employer, Horizon (months)					
	6	12	18	24	36	48
Running Variable	-0.007	0.001	0.006	0.008	0.007	0.009
	(0.004)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
Running Variable × Age 35-55	0.003	0.004	0.001	0.000	-0.000	-0.001
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Running Variable × Age above 55	0.003	0.005*	-0.001	-0.000	0.001	-0.000
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Treatment (12m PBD) × Running Variable	0.011	-0.001	-0.004	-0.002	0.003	-0.000
	(0.007)	(0.010)	(0.011)	(0.011)	(0.012)	(0.011)
Treatment (12m PBD) × Running Variable × Age 35-55	-0.008*	-0.006	-0.003	-0.003	-0.006	-0.003
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Treatment (12m PBD) × Running Variable × Age above 55	-0.003	-0.001	0.003	-0.001	-0.004	-0.005
	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)
Treatment (12m PBD)	0.022	0.033	0.012	-0.013	-0.022	-0.032
	(0.029)	(0.038)	(0.043)	(0.044)	(0.045)	(0.044)
Treatment (12m PBD) × Age 35-55	-0.003	-0.021	-0.026	-0.012	0.010	0.001
	(0.019)	(0.022)	(0.024)	(0.025)	(0.024)	(0.024)
Treatment (12m PBD) × Age above 55	-0.041*	-0.057**	-0.049	-0.039	-0.037	-0.024
	(0.024)	(0.027)	(0.030)	(0.030)	(0.029)	(0.028)
Age 35-55	0.070***	0.100***	0.115***	0.125***	0.134***	0.137***
	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
Age above 55	0.071***	0.087***	0.082***	0.075***	0.044***	0.007
	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Industry × Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	11,137	11,137	11,137	11,137	11,137	11,137
N Individuals	645,138	645,138	645,138	645,138	645,138	645,138

Notes: The table reports heterogeneous treatment effects of the 2012 PBD reform by age on employment at different horizons after the start of STW. We define groups within firms based on demographic characteristics at the start of STW. The data is a balanced panel at the group-firm-horizon level. The results are from a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. The outcome variable is for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm. Potential re-entries after an exit are ignored. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown is the number of clusters including firms that start in 2011. The number of individuals refers to the number of individuals the calculation is based upon. Robust standard errors clustered at the firm level are reported in parentheses. The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.6: Heterogeneity in the Effect of PBD Extension by Tenure

	Employment at Initial Employer, Horizon (months)					
	6	12	18	24	36	48
Running Variable	-0.006	0.001	0.003	0.006	0.005	0.007
	(0.004)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)
Running Variable × Tenure 5-10y	0.003	0.007**	0.006*	0.004	0.007**	0.005
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Running Variable × Tenure above 10y	0.003	0.006*	0.004	0.001	0.001	0.002
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Treatment (12m PBD) × Running Variable	0.012*	-0.004	0.000	0.000	0.000	-0.002
	(0.007)	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)
Treatment (12m PBD) × Running Variable × Tenure 5-10y	-0.005	0.000	-0.010	-0.008	-0.012*	-0.006
	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Treatment (12m PBD) × Running Variable × Tenure above 10y	-0.013**	-0.006	-0.014*	-0.011	-0.011	-0.008
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Treatment (12m PBD)	0.010	0.044	0.009	-0.011	-0.001	-0.010
	(0.027)	(0.035)	(0.040)	(0.041)	(0.042)	(0.041)
Treatment (12m PBD) × Tenure 5-10y	0.001	-0.049**	-0.003	0.007	0.012	-0.007
	(0.022)	(0.024)	(0.029)	(0.029)	(0.028)	(0.027)
Treatment (12m PBD) × Tenure above 10y	0.015	-0.042	0.003	0.010	0.006	-0.005
	(0.023)	(0.025)	(0.029)	(0.029)	(0.028)	(0.028)
Tenure 5-10y	0.087***	0.138***	0.152***	0.156***	0.156***	0.150***
	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Tenure above 10y	0.117***	0.183***	0.201***	0.207***	0.207***	0.199***
	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Industry × Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	10,387	10,387	10,387	10,387	10,387	10,387
N Individuals	649,531	649,531	649,531	649,531	649,531	649,531

Notes: The table reports heterogeneous treatment effects of the 2012 PBD reform by tenure on employment at different horizons after the start of STW. We define groups within firms based on demographic characteristics at the start of STW. The data is a balanced panel at the group-firm-horizon level. The results are from a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. The outcome variable is for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm. Potential re-entries after an exit are ignored. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown is the number of clusters including firms that start in 2011. The number of individuals refers to the number of individuals the calculation is based upon. Robust standard errors clustered at the firm level are reported in parentheses. The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.7: RD Effect of PBD Extension on Wage Growth by (Endogenous) Employee Job Switching Status

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Switch Within 1y				
Treatment (12m PBD)	-0.001	0.035**	0.055***	0.060***
	(0.011)	(0.014)	(0.017)	(0.019)
Treatment (12m PBD) × Switch Within 1y	-0.037*	-0.037*	-0.027	-0.030
	(0.022)	(0.022)	(0.027)	(0.028)
Switch Within 2y				
Treatment (12m PBD)	0.011	0.033**	0.053***	0.074***
	(0.011)	(0.015)	(0.018)	(0.020)
Treatment (12m PBD) × Switch Within 2y	-0.032**	-0.036*	-0.041*	-0.057**
	(0.013)	(0.019)	(0.022)	(0.023)
Switch Within 3y				
Treatment (12m PBD)	0.016	0.023	0.034**	0.051**
	(0.011)	(0.014)	(0.017)	(0.020)
Treatment (12m PBD) × Switch Within 3y	-0.027**	-0.022	0.008	-0.019
	(0.011)	(0.014)	(0.019)	(0.020)
Switch Within 4y				
Treatment (12m PBD)	0.012	0.012	0.047***	0.042**
	(0.011)	(0.014)	(0.018)	(0.018)
Treatment (12m PBD) × Switch Within 4y	-0.020**	-0.027**	-0.022	-0.019
	(0.010)	(0.013)	(0.016)	(0.020)
Industry × Region FE	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes

Notes: The table reports heterogeneous treatment effects by job switching status (defined in four different ways) of the reform on wage growth at different horizons after the start of STW. For the specification *Switch Within 1y (2y, 3y, 4y)* we define groups per firm based on whether an invidual has switched employer within 1y (2y, 3y, 4y) after the start of STW. The data is at the group-firm-horizon level. The coefficients shown are heterogeneous treatment effects of a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. As outcome variable, we use for each group-firm cell the growth rate of average daily wages relative to the start of STW. The table presented is a condensed version of the four specifications; the full tables can be found in Appendix Tables D.12, D.13, D.14, and D.15. The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security. Robust standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.8: Heterogeneity in the Effect of PBD Extension by Local Labor Market Conditions (RD Design)

(a) Effect on Retention (Employment at Initial Employer)

	Employment at Initial Employer, Horizon (months)					
	6	12	18	24	36	48
Running Variable	-0.005	0.002	0.008	0.013	0.014	0.019**
	(0.005)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Running Variable × Local Unemployment Top Tercile	-0.001	0.000	-0.004	-0.004	-0.006	-0.007
	(0.003)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Treatment (12m PBD) × Running Variable	0.003	-0.004	-0.005	-0.007	-0.007	-0.013
	(0.008)	(0.010)	(0.012)	(0.012)	(0.013)	(0.012)
Treatment (12m PBD) × Running Variable × Local Unemployment Top Tercile	-0.006	-0.011	-0.015	-0.015	-0.012	-0.007
	(0.008)	(0.010)	(0.011)	(0.012)	(0.012)	(0.011)
Treatment (12m PBD)	0.041	0.025	-0.011	-0.052	-0.039	-0.064
	(0.033)	(0.041)	(0.048)	(0.049)	(0.050)	(0.048)
Treatment (12m PBD) × Local Unemployment Top Tercile	0.020	0.054	0.080*	0.096**	0.092**	0.096**
	(0.033)	(0.038)	(0.045)	(0.046)	(0.046)	(0.044)
Local Unemployment Top Tercile	0.019	0.007	-0.006	-0.016	-0.017	-0.017
	(0.012)	(0.017)	(0.018)	(0.019)	(0.020)	(0.019)
Industry × Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	4,699	4,699	4,699	4,699	4,699	4,699
N Individuals	457,728	457,728	457,728	457,728	457,728	457,728

(b) Effect on Wage Growth

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Running Variable	0.001	-0.007**	-0.008*	-0.010**
	(0.003)	(0.003)	(0.004)	(0.004)
Running Variable × Local Unemployment Top Tercile	-0.003*	-0.002	-0.004	-0.001
	(0.002)	(0.002)	(0.002)	(0.003)
Treatment (12m PBD) × Running Variable	0.002	0.013***	0.011**	0.011*
	(0.003)	(0.004)	(0.005)	(0.006)
Treatment (12m PBD) × Running Variable × Local Unemployment Top Tercile	-0.001	-0.006	-0.008*	-0.009
	(0.003)	(0.004)	(0.005)	(0.006)
Treatment (12m PBD)	0.001	0.013	0.034*	0.058**
	(0.013)	(0.017)	(0.020)	(0.023)
Treatment (12m PBD) × Local Unemployment Top Tercile	0.019	0.037**	0.048**	0.035
	(0.012)	(0.017)	(0.019)	(0.022)
Local Unemployment Top Tercile	0.001	0.006	-0.003	0.000
	(0.005)	(0.007)	(0.008)	(0.009)
Industry × Region FE	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes
N Firms	4,699	4,699	4,699	4,699
N Individuals	428,989	416,243	407,280	398,924

Notes: The table reports heterogeneous effects by local labor market conditions of the reform on employment and wage growth at different horizons after the start of STW. We report the results of the regression discontinuity design specified in (1) including industry by region fixed effects. In Panel (a), the outcome variable is for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm. Potential re-entries after an exit are ignored. In Panel (b), the growth rate of average daily wages relative to the start of STW is considered as outcome variable. The variable *Local Unemployment Top Tercile* takes the value one if the establishment's commuting zone in 2012 (2011) is in the top tercile. Establishments in the top and bottom tercile of the commuting-zone-unemployment-rate distribution are included. Definitions are based on the 2017 data-version (*Kreisschlüssel 2017*, SIAB 1975-2017). We assign the area of the largest establishment to a multi-establishment firm. The data is at the firm-horizon level; a separate regression is run for each horizon. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown includes firms that start in 2011, which are included to facilitate calendar month fixed effects in order to account for seasonality. In Panel (a), the data is a balanced panel with the number of individuals referring to the number of individuals the calculation is based upon. In Panel (b), the number of individuals per horizon refers to the number of individuals among all initially employed who are still in the labor market at this horizon and, thus, for whom wage growth can be calculated. A drop and subsequent increase in the number of firms can occur if, at some firm, all initially employed have gaps in their employment history (e.g., due to parental leave or sickness). Robust standard errors are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.9: Heterogeneity in the Effect of PBD Extension by Existence of a Works Council (RD Design)

(a) Effect on Retention (Employment at Initial Employer)

	Employment at Initial Employer, Horizon (months)					
	6	12	18	24	36	48
Running Variable	-0.005	-0.000	0.002	0.006	0.006	0.012*
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Running Variable × Works Council	0.005**	0.010**	0.010**	0.007	0.005	0.004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treatment (12m PBD) x Running Variable	0.001	-0.005	-0.008	-0.006	-0.009	-0.013
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treatment (12m PBD) x Running Variable × Works Council	0.008	0.005	0.004	0.002	0.013	0.014
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treatment (12m PBD)	0.035	0.036	0.026	-0.012	0.010	-0.016
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Treatment (12m PBD) × Works Council	-0.064**	-0.064**	-0.064*	-0.039	-0.072*	-0.072*
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Works Council	0.041***	0.063***	0.071***	0.073***	0.072***	0.072***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Industry x Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Establishments	7,378	7,378	7,378	7,378	7,378	7,378
N Individuals	461,120	461,120	461,120	461,120	461,120	461,120

(b) Effect on Wage Growth

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Running Variable	0.000	-0.004	-0.004	-0.006
	(0.00)	(0.00)	(0.00)	(0.00)
Running Variable × Works Council	-0.001	-0.002	-0.003*	-0.002
	(0.00)	(0.00)	(0.00)	(0.00)
Treatment (12m PBD) x Running Variable	-0.002	0.004	0.002	0.002
	(0.00)	(0.00)	(0.00)	(0.01)
Treatment (12m PBD) x Running Variable × Works Council	0.007***	0.006**	0.004	0.006
	(0.00)	(0.00)	(0.00)	(0.00)
Treatment (12m PBD)	0.012	0.025*	0.043**	0.055***
	(0.01)	(0.01)	(0.02)	(0.02)
Treatment (12m PBD) × Works Council	-0.010	-0.001	0.006	-0.006
	(0.01)	(0.01)	(0.02)	(0.02)
Works Council	-0.006***	-0.008***	-0.007**	-0.012***
	(0.00)	(0.00)	(0.00)	(0.00)
Industry x Region FE	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes
N Establishments	7,378	7,377	7,377	7,378
N Individuals	431,060	418,216	407,898	398,740

Notes: The table reports heterogeneous effects by existence of a works council of the reform on employment and wage growth at different horizons after the start of STW. We report the results of the regression discontinuity design specified in (1) including industry by region fixed effects at the establishment level. In Panel (a), the outcome variable is for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm. Potential re-entries after an exit are ignored. In Panel (b), the growth rate of average daily wages relative to the start of STW is considered as outcome variable. We predict the existence of a works council drawing on the *IAB Establishment Panel* for the prediction (for details see Appendix A.6). To match the level of observation of the *IAB Establishment Panel*, we run this analysis at the establishment level. The variable *Works Council* takes the value one if the predicted probability of the existence of a works council exceeds the threshold chosen to maximize the AUC in the prediction. The sample consists of establishments that can be matched to the firm-level using Orbis-ADIAB and, analogous to before, restricting to those establishments that in the start month have more than five employees in full-time who are fully liable to social security. The data is at the estblishment-horizon level; a separate regression is run for each horizon. The running variable is distance to the cutoff 2012m6. Treated establishments are those that start STW after the cutoff. The number of establishments shown includes establishments that start in 2011, which are included to facilitate calendar month fixed effects in order to account for seasonality. Robust standard errors are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.10: Heterogeneity in the Effect of PBD Extension by Firm-Level Liquidity (RD Design)

(a) Effect on Retention (Employment at Initial Employer)

	Employment at Initial Employer, Horizon (months)					
	6	12	18	24	36	48
Running Variable	0.003	-0.004	-0.003	-0.001	-0.003	0.004
	(0.005)	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)
Running Variable × Cash-to-Asset Ratio Top Tercile	-0.009***	0.000	0.001	-0.002	-0.003	-0.003
	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Treatment (12m PBD) × Running Variable	0.004	0.010	0.014	0.015	0.016	0.001
	(0.007)	(0.010)	(0.012)	(0.013)	(0.013)	(0.013)
Treatment (12m PBD) × Running Variable × Cash-to-Asset Ratio Top Tercile	0.009	0.000	-0.008	-0.006	-0.001	0.009
	(0.006)	(0.008)	(0.011)	(0.012)	(0.012)	(0.012)
Treatment (12m PBD)	-0.021	-0.017	-0.048	-0.055	-0.031	-0.020
	(0.027)	(0.038)	(0.047)	(0.049)	(0.052)	(0.051)
Treatment (12m PBD) × Cash-to-Asset Ratio Top Tercile	0.003	0.008	0.045	0.046	0.020	-0.043
	(0.027)	(0.033)	(0.043)	(0.045)	(0.047)	(0.046)
Cash-to-Asset Ratio Top Tercile	-0.010	0.004	0.017	0.027**	0.051***	0.063***
	(0.006)	(0.009)	(0.010)	(0.011)	(0.012)	(0.012)
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	3,924	3,924	3,924	3,924	3,924	3,924
N Individuals	299,701	299,701	299,701	299,701	299,701	299,701

(b) Effect on Wage Growth

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Running Variable	0.001	-0.007**	-0.005	-0.009**
	(0.003)	(0.003)	(0.004)	(0.005)
Running Variable × Cash-to-Asset Ratio Top Tercile	0.002	0.003	0.004	0.004
	(0.002)	(0.002)	(0.003)	(0.003)
Treatment (12m PBD) × Running Variable	-0.001	0.007	0.000	0.003
	(0.004)	(0.005)	(0.006)	(0.007)
Treatment (12m PBD) × Running Variable × Cash-to-Asset Ratio Top Tercile	0.002	0.007	0.006	0.005
	(0.003)	(0.004)	(0.005)	(0.006)
Treatment (12m PBD)	0.004	0.028	0.050**	0.065**
	(0.014)	(0.019)	(0.022)	(0.026)
Treatment (12m PBD) × Cash-to-Asset Ratio Top Tercile	-0.017	-0.030*	-0.038**	-0.038*
	(0.012)	(0.017)	(0.018)	(0.022)
Cash-to-Asset Ratio Top Tercile	0.003	0.004	0.009	0.011*
	(0.003)	(0.004)	(0.006)	(0.006)
Calendar Month FE	Yes	Yes	Yes	Yes
N Firms	3,924	3,923	3,923	3,924
N Individuals	280,869	272,334	265,805	260,033

Notes: The table reports heterogeneous effects by liquidity of the reform on employment and wage growth at different horizons after the start of STW. We report the results of the regression discontinuity design specified in (1). In Panel (a), the outcome variable is for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm. Potential re-entries after an exit are ignored. In Panel (b), the growth rate of average daily wages relative to the start of STW is considered as outcome variable. The cash-to-asset ratio is based on BvD data in 2012 (2011) for firms that start in 2012 (2011). Details on the cleaning procedures data can be found in Appendix A.5. The variable *Cash-to-Asset Ratio Top Tercile* takes the value one if the firm's cash-to-asset ratio is above the p66 among firms that start in the same year. The sample includes the bottom and top tercile. Due to the resulting drop in the number of observations we report the specification excluding industry by region fixed effects here (the results of the specification including industry by region fixed effects can be found in the Appendix Table D.11). The data is at the firm-horizon level; a separate regression is run for each horizon. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown includes firms that start in 2011. In Panel (a), the data is a balanced panel with the number of individuals referring to the number of individuals the calculation is based upon. In Panel (b), the number of individuals per horizon refers to the number of individuals among all initially employed who are still in the labor market at this horizon and, thus, for whom wage growth can be calculated. A drop and subsequent increase in the number of firms can occur if, at some firm, all initially employed have gaps in their employment history (e.g., due to parental leave or sickness). The sample is restricted to firms that in the start month have more than five employees in full-time who are fully liable to social security. Robust standard errors are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.11: Heterogeneity in the RD Effect of PBD Extension by Firm-Level Liquidity

(a) Effect on Retention (Employment at Initial Employer)

	Employment at Initial Employer, Horizon (months)					
	6	12	18	24	36	48
Running Variable	0.005	0.000	0.001	0.002	-0.000	0.007
	(0.005)	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)
Running Variable × Cash-to-Asset Ratio Top Tercile	-0.005*	0.004	0.005	0.002	0.001	0.001
	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Treatment (12m PBD) × Running Variable	0.001	0.005	0.010	0.011	0.013	-0.003
	(0.007)	(0.010)	(0.012)	(0.013)	(0.013)	(0.013)
Treatment (12m PBD) × Running Variable × Cash-to-Asset Ratio Top Tercile	0.007	-0.003	-0.011	-0.010	-0.006	0.004
	(0.006)	(0.009)	(0.011)	(0.012)	(0.012)	(0.012)
Treatment (12m PBD)	-0.026	-0.029	-0.062	-0.067	-0.041	-0.035
	(0.027)	(0.038)	(0.047)	(0.049)	(0.053)	(0.052)
Treatment (12m PBD) × Cash-to-Asset Ratio Top Tercile	-0.007	0.005	0.042	0.049	0.025	-0.039
	(0.028)	(0.034)	(0.043)	(0.046)	(0.048)	(0.046)
Cash-to-Asset Ratio Top Tercile	-0.001	0.015*	0.025**	0.037***	0.062***	0.075***
	(0.006)	(0.009)	(0.010)	(0.011)	(0.012)	(0.012)
Industry × Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	3,878	3,878	3,878	3,878	3,878	3,878
N Individuals	298,188	298,188	298,188	298,188	298,188	298,188

(b) Effect on Wage Growth

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Running Variable	0.002	-0.006*	-0.005	-0.010**
	(0.003)	(0.003)	(0.004)	(0.005)
Running Variable × Cash-to-Asset Ratio Top Tercile	0.001	0.002	0.003	0.002
	(0.002)	(0.002)	(0.003)	(0.003)
Treatment (12m PBD) × Running Variable	-0.002	0.006	-0.001	0.004
	(0.004)	(0.005)	(0.006)	(0.007)
Treatment (12m PBD) × Running Variable × Cash-to-Asset Ratio Top Tercile	0.001	0.006	0.006	0.006
	(0.003)	(0.004)	(0.005)	(0.006)
Treatment (12m PBD)	0.006	0.032*	0.056**	0.070***
	(0.015)	(0.019)	(0.023)	(0.026)
Treatment (12m PBD) × Cash-to-Asset Ratio Top Tercile	-0.011	-0.022	-0.033*	-0.032
	(0.012)	(0.016)	(0.019)	(0.022)
Cash-to-Asset Ratio Top Tercile	-0.001	-0.001	0.002	0.002
	(0.003)	(0.004)	(0.005)	(0.006)
Industry × Region FE	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes
N Firms	3,878	3,877	3,877	3,878
N Individuals	279,490	271,045	264,530	258,801

Notes: The table reports heterogeneous effects by liquidity of the 2012 PBD reform on employment and wage growth at different horizons after the start of STW. We report the results of the regression discontinuity design specified in (1). In Panel (a), the outcome variable is for each firm the share of initially employed (i.e., employed at the start of STW) who are still employed at the same firm. Potential re-entries after an exit are ignored. In Panel (b), the growth rate of average daily wages relative to the start of STW is considered as outcome variable. The cash-to-asset ratio is based on BvD data in 2012 (2011) for firms that start in 2012 (2011). Details on the cleaning procedures behind the BvD data can be found in Appendix A.5. The variable *Cash-to-Asset Ratio Top Tercile* takes the value one if the firm's cash-to-asset ratio is above the p66 among firms that start in the same year. The sample includes the bottom and top tercile. The data is at the firm-horizon level; a separate regression is run for each horizon. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown includes firms that start in 2011, which are included to facilitate calendar month fixed effects in order to account for seasonality. In Panel (a), the data is a balanced panel with the number of individuals referring to the number of individuals the calculation is based upon. In Panel (b), the number of individuals per horizon refers to the number of individuals among all initially employed who are still in the labor market at this horizon and, thus, for whom wage growth can be calculated. A drop and subsequent increase in the number of firms can occur if, at some firm, all initially employed have gaps in their employment history (e.g., due to parental leave or sickness). The sample is restricted to firms that in the start month have more than five employees in full-time who are fully liable to social security. Robust standard errors are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.12: RD Effect of PBD Extension on Wage Growth by (Endogenous) Employee Job Switching Status (Switching Within 1 Year)

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Running Variable	-0.001	-0.006**	-0.006*	-0.007**
	(0.002)	(0.003)	(0.003)	(0.004)
Running Variable × Switch Within 1y	0.005*	0.005*	0.007**	0.006*
	(0.003)	(0.003)	(0.003)	(0.003)
Treatment (12m PBD) × Running Variable	0.005*	0.008**	0.003	0.005
	(0.003)	(0.004)	(0.004)	(0.005)
Treatment (12m PBD) × Running Variable × Switch Within 1y	-0.013**	-0.012**	-0.015**	-0.013*
	(0.006)	(0.006)	(0.007)	(0.007)
Treatment (12m PBD)	-0.001	0.035**	0.055***	0.060***
	(0.011)	(0.014)	(0.017)	(0.019)
Treatment (12m PBD) × Switch Within 1y	-0.037*	-0.037*	-0.027	-0.030
	(0.022)	(0.022)	(0.027)	(0.028)
Switch Within 1y	0.008	0.076***	0.099***	0.122***
	(0.005)	(0.006)	(0.006)	(0.007)
Industry × Region FE	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes
N Firms	7,849	7,934	7,938	7,937
N Individuals	617,349	599,940	586,636	574,209

Notes: The table reports heterogeneous treatment effects of the 2012 PBD reform by job switches within 1y on wage growth at different horizons after the start of STW. We define groups within firms based on whether an individual has switched employer within the respective horizon after the start of STW. The data is at the group-firm-horizon level. The results are from a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. As outcome variable, we use for each group-firm cell the growth rate of average daily wages relative to the start of STW. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown is the number of clusters including firms that start in 2011. The number of individuals per horizon refers to the number of individuals among all initially employed who are still in the labor market at this horizon and, thus, for whom wage growth can be calculated. A drop and subsequent increase in the number of firms can occur if, at some firm, all initially employed have gaps in their employment history (e.g., due to parental leave or sickness). Robust standard errors clustered at the firm level are reported in parentheses. The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.13: RD Effect of PBD Extension on Wage Growth by (Endogenous) Employee Job Switching Status (Switching Within 2 Years)

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Running Variable	-0.002	-0.005**	-0.008**	-0.011***
	(0.002)	(0.003)	(0.003)	(0.004)
Running Variable × Switch Within 2y	0.004**	0.003	0.009***	0.010***
	(0.002)	(0.002)	(0.002)	(0.003)
Treatment (12m PBD) × Running Variable	0.004	0.008**	0.008*	0.009*
	(0.003)	(0.004)	(0.005)	(0.005)
Treatment (12m PBD) × Running Variable × Switch Within 2y	-0.005	-0.006	-0.012**	-0.009
	(0.004)	(0.005)	(0.006)	(0.006)
Treatment (12m PBD)	0.011	0.033**	0.053***	0.074***
	(0.011)	(0.015)	(0.018)	(0.020)
Treatment (12m PBD) × Switch Within 2y	-0.032**	-0.036*	-0.041*	-0.057**
	(0.013)	(0.019)	(0.022)	(0.023)
Switch Within 2y	0.026***	0.005	0.084***	0.113***
	(0.004)	(0.005)	(0.005)	(0.006)
Industry × Region FE	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes
N Firms	8,447	8,215	8,449	8,450
N Individuals	616,800	598,364	585,425	573,025

Notes: The table reports heterogeneous treatment effects of the 2012 PBD reform by job switches within 2y on wage growth at different horizons after the start of STW. We define groups within firms based on whether an individual has switched employer within the respective horizon after the start of STW. The data is at the group-firm-horizon level. The results are from a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. As outcome variable, we use for each group-firm cell the growth rate of average daily wages relative to the start of STW. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown is the number of clusters including firms that start in 2011. The number of individuals per horizon refers to the number of individuals among all initially employed who are still in the labor market at this horizon and, thus, for whom wage growth can be calculated. A drop and subsequent increase in the number of firms can occur if, at some firm, all initially employed have gaps in their employment history (e.g., due to parental leave or sickness). Robust standard errors clustered at the firm level are reported in parentheses. The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.14: RD Effect of PBD Extension on Wage Growth by (Endogenous) Employee Job Switching Status (Switching Within 3 Years)

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Running Variable	-0.002	-0.002	-0.005	-0.007*
	(0.002)	(0.003)	(0.003)	(0.004)
Running Variable × Switch Within 3y	0.001	-0.002	-0.002	0.003
	(0.001)	(0.002)	(0.002)	(0.003)
Treatment (12m PBD) × Running Variable	0.004	0.006	0.008*	0.010*
	(0.003)	(0.004)	(0.004)	(0.005)
Treatment (12m PBD) × Running Variable × Switch Within 3y	0.001	-0.001	-0.009*	-0.011**
	(0.003)	(0.004)	(0.005)	(0.005)
Treatment (12m PBD)	0.016	0.023	0.034**	0.051**
	(0.011)	(0.014)	(0.017)	(0.020)
Treatment (12m PBD) × Switch Within 3y	-0.027**	-0.022	0.008	-0.019
	(0.011)	(0.014)	(0.019)	(0.020)
Switch Within 3y	0.018***	0.026***	0.002	0.088***
	(0.003)	(0.004)	(0.005)	(0.005)
Industry × Region FE	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes
N Firms	8,745	8,734	8,414	8,716
N Individuals	616,459	598,928	584,909	572,862

Notes: The table reports heterogeneous treatment effects of the 2012 PBD reform by job switches within 3y on wage growth at different horizons after the start of STW. We define groups within firms based on whether an individual has switched employer within the respective horizon after the start of STW. The data is at the group-firm-horizon level. The results are from a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. As outcome variable, we use for each group-firm cell the growth rate of average daily wages relative to the start of STW. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown is the number of clusters including firms that start in 2011. The number of individuals per horizon refers to the number of individuals among all initially employed who are still in the labor market at this horizon and, thus, for whom wage growth can be calculated. A drop and subsequent increase in the number of firms can occur if, at some firm, all initially employed have gaps in their employment history (e.g., due to parental leave or sickness). Robust standard errors clustered at the firm level are reported in parentheses. The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.15: RD Effect of PBD Extension on Wage Growth by (Endogenous) Employee Job Switching Status (Switching Within 4 Years)

	Wage Growth Since Start, Horizon (months)			
	12	24	36	48
Running Variable	-0.000	-0.003	-0.008**	-0.008**
	(0.002)	(0.003)	(0.003)	(0.003)
Running Variable × Switch Within 4y	0.002	0.003	0.003	0.003
	(0.001)	(0.002)	(0.002)	(0.002)
Treatment (12m PBD) × Running Variable	0.001	0.008**	0.011**	0.013***
	(0.003)	(0.004)	(0.004)	(0.005)
Treatment (12m PBD) × Running Variable × Switch Within 4y	-0.002	-0.004	-0.008*	-0.010*
	(0.003)	(0.003)	(0.004)	(0.005)
Treatment (12m PBD)	0.012	0.012	0.047***	0.042**
	(0.011)	(0.014)	(0.018)	(0.018)
Treatment (12m PBD) × Switch Within 4y	-0.020**	-0.027**	-0.022	-0.019
	(0.010)	(0.013)	(0.016)	(0.020)
Switch Within 4y	0.019***	0.034***	0.044***	0.013***
	(0.003)	(0.004)	(0.004)	(0.005)
Industry × Region FE	Yes	Yes	Yes	Yes
Calendar Month FE	Yes	Yes	Yes	Yes
N Firms	8,941	8,933	8,920	8,484
N Individuals	616,241	598,825	585,460	572,571

Notes: The table reports heterogeneous treatment effects of the 2012 PBD reform by job switches within 4y on wage growth at different horizons after the start of STW. We define groups within firms based on whether an individual has switched employer within the respective horizon after the start of STW. The data is at the group-firm-horizon level. The results are from a regression discontinuity design analogous to the one specified in (1) at the group-firm level, including industry by region fixed effects. As outcome variable, we use for each group-firm cell the growth rate of average daily wages relative to the start of STW. The running variable is distance to the cutoff 2012m6. Treated firms are those that start STW after the cutoff. The number of firms shown is the number of clusters including firms that start in 2011. The number of individuals per horizon refers to the number of individuals among all initially employed who are still in the labor market at this horizon and, thus, for whom wage growth can be calculated. A drop and subsequent increase in the number of firms can occur if, at some firm, all initially employed have gaps in their employment history (e.g., due to parental leave or sickness). Robust standard errors clustered at the firm level are reported in parentheses. The sample is restricted to group-firm cells that in the start month contain more than five employees in full-time that are fully liable to social security. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.16: Additional Data on Individual-Level STW Receipt

	Start Month 2020m4		Start Months 2020m4-2020m12	
	All	High Quality	All	High Quality
	Mean	Mean	Mean	Mean
Establishment-Level				
Cross-Check: Aggregated Individual-Level With Establishment-Level				
Coincide	0.74	1.00	0.73	1.00
Divergence (Number of Employees in STW)	0.08	0.00	0.08	0.00
Divergence (Month of STW Receipt)	0.10	0.00	0.10	0.00
Divergence (Both)	0.00	0.00	0.00	0.00
Incalculable	0.09	0.00	0.09	0.00
Observations	86513	11598	119923	88059
Individual-Level				
STW Risk (pp)				
Ineligible	0.13	0.15	0.12	0.14
Confirmed 0	0.26	0.35	0.29	0.38
Estabishment-Level Gender-Specific Share in STW 0-20	0.05	0.01	0.07	0.02
Estabishment-Level Gender-Specific Share in STW 20-50	0.07	0.02	0.07	0.02
Estabishment-Level Gender-Specific Share in STW 50-100	0.14	0.04	0.12	0.04
Confirmed 100	0.26	0.36	0.25	0.34
Incalculable	0.08	0.05	0.07	0.05
Observations	4689821	2528057	6775140	3636864

Notes: The table shows the results of various cross-checks of the individual level data on STW receipt. The grand total of the individual-level data includes individuals working at an establishment in STW in the month of STW receipt, for April 2020 (columns 1 and 2) and pooled across the months April to December 2020 (columns 3 and 4). Columns 1 and 3 consider all establishments, while columns 2 and 4 restrict attention to establishments with high quality data, defined as coinciding numbers of short-time workers between aggregated individual-level and establishment-level data. The top panel shows the results of cross-checking the individual-level data, aggregated to the establishment level, with the establishment-level information on monthly STW receipt (maximum of the variable *Qualitätsklasse* per establishment). The number of individuals in STW can either match (first row) or diverge for a given month. Divergence can occur if the number of individuals differs (second row) or if the establishment is not found in both datasets for that month (third row). Cross-checks may be infeasible (last row) if there is no 1:1 or 1:n mapping between the establishment applying for STW and the employer in the Social Security Records, often due to the involvement of a temporary employment agency. The bottom panel shows the individual-level risk of being in STW after cross-checks with the establishment-level data and Social Security Records (variable *Kug-Status*). An individual can be classified as ineligible (e.g., above the statutory retirement age, first row), confirmed not in STW (second row, e.g., the establishment-level number of individuals matches the aggregated individual-level information, and the individual is not in the digitized list), or confirmed in STW. If there is a discrepancy between the establishment-level data and individual-level data, the individual is assigned the gender-specific share of eligible employees at the establishment in buckets (third to fifth row).