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This paper studies how the potential duration of unemployment benefits affects early job search behavior and re-employment outcomes. We exploit an unexpected reform of the German unemployment insurance (UI) scheme in 2008, which increased the potential benefit duration from 12 to 15 months for benefit recipients of age 50 to 54. Based on detailed survey data and a difference-in-differences design, we estimate that one additional month of potential benefits reduces early job applications by around 10%. Using social security data, we further find that the extension of benefits increases the average nonemployment duration of individuals entering UI after the reform. Among individuals who got treated at later stages of their unemployment spell, the increased UI coverage does not appear to come at the cost of longer nonemployment. A cautious back-of-the-envelope calculation reveals substantial job finding returns to early search effort.

JEL Classification: D83, I38, J64, J68

Keywords: Unemployment Insurance, Job Search, Re-Employment Outcomes, Natural Experiment

1. Introduction

Unemployment insurance (UI) serves as the key policy instrument to protect individuals against adverse income shocks due to job loss. Yet, the optimal design of UI remains fiercely debated — particularly due to the well-established finding that a longer potential benefit duration (PBD) prolongs job seekers' time to re-employment (see, for example, the overview by Schmieder and von Wachter, 2016). While this effect can be partly linked to a shifted "spike" in job finding at benefit exhaustion, a longer PBD has also been found to reduce job finding early in the unemployment spell (see, e.g., Card et al., 2007b; Schmieder et al., 2012). This has been interpreted as suggestive evidence that job seekers adjust their search behavior well in advance of reaching benefit exhaustion. However, to date there is little empirical evidence on how early search effort and reservation wages actually respond to the prospect of longer benefits. Such knowledge is yet important to understand and balance the trade-off between coverage and incentive when designing UI schemes. This is particularly true in light of the large insurance value of UI (see, e.g., Landais and Spinnewijn, 2019), which provides a rationale for policy responses that directly target job seekers' early search behavior instead of compromising on UI generosity.

In this paper, we provide quasi-experimental evidence on the relationship between individuals' PBD, early job search behavior and re-employment outcomes in a joint framework. We exploit a reform of the German UI benefit scheme, which raised the PBD for eligible job seekers aged 50 to 54 from 12 to 15 months. The policy change occurred on January 1 2008 and was the unexpected outcome of the government leaders' attempt to ease growing tensions within the then-acting coalition government of Christian Democrats and Social Democrats about the desired size of the welfare state. Coincidentally, it falls in the middle of the survey period of the IZA Evaluation Dataset, which provides detailed information on the job search behavior of individuals entering unemployment between June 2007 and May 2008. In a first step, we make use of the survey information and the reform-induced variation in the UI scheme to study how an extension of the PBD affects individuals' search effort and reservation wage at the beginning of their benefit entitlement period.[1] Second, to provide further interpretation to the observed changes in job search behavior, we estimate the reform's impact on re-employment outcomes using comprehensive social security data covering the period 2006-2008.

To derive causal effects, we set-up a cross-sectional difference-in-differences (Diff-in-Diff) design in which the control group consists of slightly younger job seekers aged 45 to 49. These job seekers were not affected by the reform — their PBD remained at 12 months — but are very similar to individuals

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[1] The vast majority of job seekers was interviewed around five to eleven weeks after UI entry.

in the treatment group with respect to their employment history and other observable characteristics. We show that the relative composition of the two cohorts did not change post reform, and that there is no evidence of differential pre-trends in the relevant outcome variables.

We find that the PBD extension significantly reduced individuals' early job search effort, as measured by the total number of filed applications during the first two months of benefit eligibility. On average, one additional month of potential benefits reduced search effort by around 1.8 applications, or 10% relative to the group's pre-reform mean. The effect is primarily driven by responses at the intensive margin, and reflects changes along the entire distribution of search effort. It is robust to different specifications of the treatment and various ways of drawing inference. The estimated effect of the reform on job seekers' stated reservation wage is positive — as predicted by standard job search theory — but lacks statistical power.

Using the more comprehensive social security data, we further show that the benefit extension lowered the chance of early job finding. We find that the probability to start a new job within the first three months after UI entry decreases by around two percentage points on average in response to the reform. We further detect a significant increase in both the duration of UI receipt and the duration of nonemployment, i.e., the time between unemployment entry and re-employment. When estimating these effects, we need to account for a notable particularity of the reform: the extension of benefits was granted to both the inflow and the stock of benefit recipients. Therefore, it not only affected eligible job seekers who entered UI in 2008, but also applied ex post to those who already entered UI in 2007 and were still eligible for benefits by December 31 2007. Job seekers aged 50 to 54 who entered UI in 2007 therefore chose early search behavior while expecting a PBD of 12 months.[2] However, their duration outcomes might have been affected by behavioral adjustments in response to the reform later during their spell.

We first compare fully untreated (UI entry in 2006) to fully treated job seekers (UI entry in 2008) and find that the PBD extension increased the duration of UI receipt by 22 days and the duration of nonemployment by 10 days on average. Phrased differently, we estimate that one additional month of PBD raises the duration of UI receipt (nonemployment) by around 0.24 (0.1) months. These estimated elasticities are in line with earlier studies, such as Card et al. (2007b), Lalive (2008) and Schmieder et al. (2012). When extending the analysis to job seekers treated after UI entry, i.e., to the 2007 entry cohort, we find that the PBD extension prolonged the duration of UI receipt for this group by around 10 days on average. At the same time, we find no meaningful effect on the duration of nonemployment.

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[2] We exclude job seekers who were interviewed between reform announcement and implementation when analyzing the effects on early search behavior.

It appears that the retroactive application of the reform raised UI coverage among already unemployed individuals, but did not distort their job search incentives. We corroborate this finding when explicitly focusing on job seekers still unemployed at reform implementation: the moral hazard costs of a PBD extension appear to be lower for the long-term than for the short-term unemployed.

In a final step, we use our estimates to cautiously assess the individual returns to early search effort. To avoid assumptions about the proportionality of effort changes over the unemployment spell, we focus on the impact of effort provided during the first two months of benefit eligibility on early job finding. We estimate that the reduction in search effort by around 10% per additional month of benefits is associated with a 7% reduction in the probability to exit nonemployment within the first three months. While this back-of-the-envelope calculation provides interesting new insights, we stress that it relies on estimates from two different samples and rather strong assumptions. For instance, we need to assume the irrelevance of the reservation wage and a constant application quality irrespective of quantity.

The results of this paper contribute to three strands of the literature. First, we add evidence to a small number of papers that analyze the response of job search behavior to changes in the benefit environment. Le Barbanchon et al. (2019) use French administrative data on reservation wages and changes in UI rules to explore whether a longer PBD increases individuals' stated reservation wages. They find a precise null effect against the prediction of standard job search theory. Marinescu (2017) as well as Baker and Fradkin (2017) analyze the relationship between the U.S. state level PBD and job search effort, both using business cylce-induced variation in the PBD within states and aggregate data on on-line job search behavior. Despite similarities in their set-ups and empirical designs, the findings of the two studies differ: while Marinescu (2017) reports a significant decline in aggregate search effort, Baker and Fradkin (2017) find no meaningful responses. We complement these studies by using exogenous between-individual variation in the PBD and estimating effects on total — both on-line and off-line — search activities at the beginning of the eligibility period.

Our study further relates to evidence on the dynamics of search effort over the unemployment spell. Using cross-sectional time-use data, Krueger and Mueller (2010) show that the time spent on job search increases prior to benefit exhaustion. Marinescu and Skandalis (2020) corroborate this finding, using individual panel data on the number of job applications filed via a French on-line job search platform. They find that search effort increases progressively in the year preceding benefit exhaustion, and remains high thereafter.[3] Most recently, DellaVigna et al. (2020) use information

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[3] Relatedly, Faberman and Kudlyak (2019) use high-frequency data on individuals' application activity from a U.S. job vacancy website. They find job search effort to be rather constant in the initial months of unemployment and to decline thereafter. However, they cannot test for a spike in search effort around benefit exhaustion in their set-up.

from a SMS survey of German job seekers to provide detailed evidence on the actual time spent on job search over the course of the unemployment spell. They show that job search effort is rather constant over the initial months of unemployment. In line with a reference-dependent job search model, they further measure a considerable spike in effort prior to benefit exhaustion and a decline thereafter. Our approach is highly complementary to these studies: we use exogenous cross-sectional variation in the PBD to study how individuals' job search behavior at the beginning of the eligibility periods reacts to a more generous PBD.

Last, we speak to the literature that analyzes the effect of UI generosity on job finding (e.g., Card et al., 2007b; Schmieder et al., 2012; Johnston and Mas, 2018; De Groot and Van Der Klaauw, 2019). In line with previous studies, we find that a longer PBD reduces early job finding and prolongs the job seekers' overall duration of actual UI receipt and nonemployment. Exploiting the retroactive application of the reform to job seekers who entered UI before January 1 2008, we are further able to cautiously assess the effects of prolonged benefits on job finding at different periods of the individuals' unemployment spell. While we find disincentive effects among job seekers who were subject to extended benefits upon UI entry, we detect no adverse labor supply effects among job seekers who experienced an increase in their PBD at later stages of their unemployment spell. This finding relates to Kolsrud et al. (2018), who show for the case of Sweden that the incentive costs of more generous benefit levels are lower when being implemented later during the unemployment spell.[4]

From a policy perspective, the results of this paper inform about the importance of interventions that target and support the early job search effort of UI benefit recipients. Previous evidence has shown that such interventions — e.g., the establishment of minimum effort requirements — increase job finding and can be implemented at low fiscal costs (see, e.g. Graversen and Van Ours, 2008; Cockx et al., 2018; Arni and Schiprowski, 2019). Altmann et al. (2018) further find that providing information about the importance of active job search to individuals at the beginning of their unemployment spell can increase job finding prospects. Relatedly, results from Belot et al. (2019) indicate that tailored advice for job seekers can broaden the search for re-employment and thereby increase the number of job interviews.

The paper proceeds as follows. Section 2 offers a brief overview about the German UI scheme and highlights the key features of the reform. The data are presented in Section 3. In Section 4, we study the effects of the PBD extension on early job search behavior. We first provide details on the empirical strategy, before presenting the results. In Section 5, we document the corresponding

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[4] The authors show that this finding holds true when taking into account forward-looking responses early in the spell to later increases in benefit levels.

effects on re-employment outcomes and cautiously assess the return to early search effort. Section 6 concludes.

2. Institutional Setting

In Germany, unemployed individuals are entitled to UI benefits if they have contributed to the social insurance scheme for at least twelve months within the two years preceding their job loss (eligibility constraint). The potential duration of unemployment benefits depends on the number of contribution months and the recipient's age (see below). Monthly benefits amount to 60% (67% for recipients with children) of the last net wage, but are capped at the ceiling of social security contributions.[5] Individuals who exhaust their unemployment benefits can claim means-tested welfare benefits.

The PBD extension. The reform of the UI scheme was the result of the government leaders' attempt to ease growing tensions within the then-acting coalition government of Christian Democrats (CDU/CSU) and Social Democrats (SPD) about the size of the welfare state in late 2007. Below, we provide details on the implementation and content of the reform.

Since the introduction of the so-called *Hartz* reforms in the early to mid 2000s, the Social Democrats were heavily divided about their own labor market policy agenda. The Hartz reforms had significantly altered German labor market institutions,[6] but also represented an important renunciation of the party's policy orientation and identity. Among others, the reform had substantially decreased the generosity of the benefit system. On October 1 2007, the then-acting party leader of the Social Democrats, Kurt Beck, marked the party's step back from its own policy towards favoring a more generous UI scheme by calling for an extension of the PBD for older workers. The reform proposal was motivated by social injustice concerns — long periods of social security contributions ought to be rewarded by a longer PBD — and occurred during times of stable macro-economic conditions.

Beck's initial proposal was met with considerable skepticism from politicians in both ruling parties. Disagreement about the reform lasted for several weeks and even prompted rumors about the collapse of the coalition government. To ease these growing tensions, leaders of both parties eventually negotiated over the set of disputed policies[7] during the night of November 12 2007. The following morning, a decision in favor of an extension of the PBD for older job seekers "at the earliest

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possible time"[8] was reached and communicated to the public. Following this agreement, the coalition rapidly implemented the legislative process. On December 11 2007, the corresponding law was issued to parliament. It was eventually passed on January 26 2008 and retroactively put into effect from January 1 2008 onward. Notably, the law also contained a transitional agreement, which extended the PBD for those job seekers who had become unemployed prior to the implementation of the reform, but fulfilled the eligibility criteria and were still entitled to receive unemployment benefits as of December 31 2007. This feature will become relevant in our empirical strategy (see Sections 4 and 5).[9]

Two groups of job seekers were affected by the reform. First, the reform increased the PBD for unemployed job seekers of age 50 to 54 from 12 to 15 months if they fulfilled the eligibility constraint and had contributed to UI for at least 30 months out of the last five years prior to their job loss. Second, it raised the PBD for job seekers of age 58 and above from 18 to 24 months if they fulfilled the eligibility constraint and had contributed to UI for at least 48 out of the last 60 months prior to unemployment. Appendix Table A.1 depicts the relationship between a claimant's age, the length of her UI contributions and the potential benefit duration prior to the reform (upper panel) and thereafter (lower panel). As the survey data only cover individuals until the age of 55, we exclusively focus on the policy change for individuals of age 50 to 54.

3. Data

We use information from two distinct data sources to analyze the consequences of the PBD extension on search effort, reservation wages, and re-employment outcomes.

3.1. Survey data

Information on individual search effort and reservation wages stems from the *IZA/IAB Linked Evaluation Dataset* (see Arni et al., 2014, for a detailed description). The dataset covers around 15,000 unemployed job seekers of age 16 to 54, who all qualify for unemployment benefits (of varying lengths). The first wave of the survey was conducted over the course of one year and comprises random samples of job seekers drawn from the monthly pool of newly registered unemployed between June 2007 to

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[8] Volker Kauder, the then-acting leader of the CDU/CSU parliamentary group in German parliament, as quoted in "Spiegel Online" on November 13 2007. For details, follow the embedded link.

[9] Along with the extension of the PBD, the government introduced so-called "labor market integration vouchers" (*Eingliederungsgutscheine*). These vouchers enabled job seekers of age 50 and above to receive additional funds for re-employment measures, which could have had an effect on individuals' job search behavior. However, the scope of this program was tiny. In 2008, the Federal Employment Agency issued these vouchers to only around 0.1% of the unemployed individuals aged 50 and above. Hence, we do not expect this policy to affect our estimates.

May 2008.[10] Hence, the PBD extension occurred roughly in the middle of this first wave. Participants in the first wave were surveyed around 5 to 16 weeks after their unemployment registration at the German Federal Employment Agency.[11]

In the empirical analysis, we will compare changes in the search behavior of job seekers aged 50 to 54 to changes in the job search behavior of job seekers aged 45 to 49. We therefore restrict the estimation sample to job seekers of age 45 to 54 who all met the eligibility criteria for the extended PBD. These criteria required that individuals held a job subject to social security contributions for at least twelve months within the previous two years and for at least 30 months within the previous five years. We further exclude individuals who immediately found re-employment, i.e., got re-employed in the month of unemployment registration, and individuals who voluntarily quit their job.

The first wave of the survey provides comprehensive information about individuals' job search effort and reservation wage. We measure search effort by the self-reported *number of applications* filed between the time of unemployment registration and the survey interview, i.e., during the first 5 to 16 weeks of benefit eligibility. Our measure of the *reservation wage* refers to the minimum net monthly wage an individual would be willing to work for at the time of the interview. The information on job search behavior is supplemented with a large set of variables on respondents' employment history and personal characteristics. Notably, information on past employment and wages stem from administrative sources, the Integrated Employment Biographies (IEB) of the Institute for Employment Research (IAB). The IEB covers all individuals in Germany who are either in regular/marginal employment or receive benefits (unemployment benefits or means-tested welfare). We thus observe past wages and (un)employment spells for all surveyed individuals in a very precise way, which allows determining whether individuals are eligible for (extended) unemployment benefits. In addition, the survey part offers information on the respondents' gender, nationality, training and location of residence; variables serving as controls in some specifications. Appendix Table A.2 provides descriptive statistics for all outcome and control variables from the survey data.

Figure 1 plots the respective distributions of the two main measures of job search behavior for both the survey population and the estimation sample.[12] Panel (A) shows the distributions of the number of job applications. We observe very similar means (indicated by the horizontal lines) and distributions for the survey population and the estimation sample. A further common feature of both distributions is the presence of outliers. Throughout the paper, we pay special attention to their

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[10] We abstain from using waves two and three of the survey due to survey attrition. In wave two, information on job search behavior is available for only 10% of the initial sample; either because of exit to employment or non-response.

[11] However, note that 95% of the respondents were interviewed 5-11 weeks after unemployment entry.

[12] The estimation sample accounts for around 8% of the survey population.

Figure 1: Overall Distribution of Job Applications and Log Reservation Wages
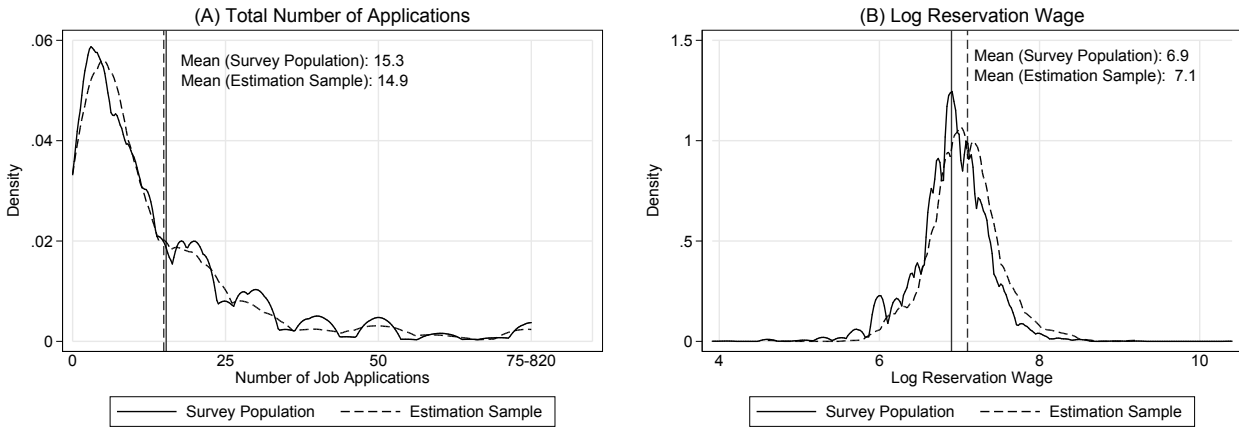
Notes: This figure plots the overall distribution of the total number of job applications and the log reservation wage for the full survey population and the estimation sample, respectively. In Panel (A), we winsorize the number of applications at the 98% level. Overall, 12,389 respondents state their amount of job search effort between unemployment registration and the survey interview. The estimation sample comprises 963 respondents. In Panel (B), we winsorize the variable at the bottom and top 2%. 8,292 respondents of the survey state their reservation wage. The estimation sample covers 790 respondents. Note that differences in the two estimation samples are due to non-response. Mean values for both variables are indicated by the solid and dashed vertical lines for the full survey population and the estimation sample, respectively.

impact on the estimated treatment effects. In the baseline regressions, we winsorize the number of job applications at the 98% level.

Panel (B) plots the two distributions of the (log) reservation wage. As for job search effort, we see considerable variation in stated reservation wages across job seekers in both samples.[13] Moreover, we note that the mean reservation wage is around 185 EUR higher in the estimation sample than in the overall survey population. This could be due to the fact that job seekers usually anchor their reservation wage to past earnings (Krueger and Mueller, 2016), which are considerably higher in the estimation sample (around 2,056 EUR) than in the overall survey population (around 1,410 EUR) on average.

In Appendix Figure A.1, we investigate the age profile in job search behavior in more detail. Focusing on individuals of age 35 to 54, we observe that the mean reservation wage is quite stable within this age window. Reservation wages are generally lower for job seekers below the age of 35 (not reported). At the same time, the average number of filed applications increases modestly with age within the age window of 35 to 54.[14] This result stands in contrast to Faberman and Kudlyak (2019), who find search effort to decrease with age. One possible explanation is that their study covers on-line job search behavior only, while our measure refers to both on- and off-line search.

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[13] To account for outliers, we winsorize the variable at the bottom and top 2% of the distribution in the baseline sample.

[14] Average search effort is highest among individuals below the age of 35.

3.2. Social security data

We complement the survey information with social security data from the German Federal Employment Agency, the *IZA/IAB Administrative Evaluation Dataset (AED)*. The dataset is of substantially larger scale and covers a 4.66% random sample of all individuals registering as unemployed between January 2001 and December 2008. Among others, it provides detailed information on job seekers' employment history, UI receipt and re-employment outcomes.

In line with the survey analysis, we restrict the estimation sample to job seekers of age 45 to 54 who met the eligibility criteria for the extended PBD. Unfortunately, the data only report the year, and not the exact date, of birth. To this end, we need to drop all individuals for whom we cannot clearly assign eligibility with respect to age (i.e., age $\geq$ 50), which results in the under-representation of job seekers aged 49 and 50. We further limit the sample to entries that occurred between February 2006 and December 2008. December 2008 is the last inflow month covered by the dataset. February 2006, in turn, refers to the first month in which both treatment and control group were subject to a PBD of 12 months. Beforehand, the PBD varied between 18 and 26 months within the sampled cohort, subject to the job seeker's exact age.

Throughout the paper, we focus on four different re-employment outcomes. First, we estimate the effect of the PBD extension on the probability to start a new job within the first three months after UI entry. Second, we consider the duration of UI benefit receipt as an outcome. We thereby measure how much longer job seekers actually receive benefits on average when entitled to an additional three months of benefit payments. However, this estimate does not inform about the disincentive effect of the benefit extension because it may merely reflect the effect of additional coverage for individuals who stay out of employment for more than 12 months. Therefore, we also estimate effects on the duration of nonemployment, i.e., the time to re-employment.[15] This outcome variable allows measuring how much the PBD extension actually distorted job seekers' incentives to re-enter employment (Card et al., 2007a). Last, we estimate how the PBD extension affected the average re-employment wage.

Appendix Table A.3 provides detailed descriptive statistics for all four outcomes variables and the set of covariates. We note that the treatment group, which is the older of the two cohorts, receives UI benefits and remains nonemployed for a longer period on average. However, in terms of observable characteristics, control and treatment group look fairly similar.[16] Appendix Figure A.3 further shows monthly exit rates from nonemployment by PBD. As expected, exit rates from nonemployment peak

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[15] The maximum duration of UI benefit receipt amounts to 15 months by definition. We also top-code nonemployment spells at 15 months, which is the period of interest in this study.

[16] By comparing Appendix Tables A.2 and A.3, we also see that the set of job seekers covered in both the administrative and survey sample are quite similar in terms of observable characteristics.

at the respective exhaustion points. Differences in the hazard rates can, however, not be interpreted as causal, as they may be influenced by cohort and time effects.

4. Potential Benefit Duration and Early Job Search Behavior

In this section, we study how the prospect of longer UI benefit payments affects two key variables of individuals' job search behavior at the beginning of their eligibility period: the number of filed applications (as a measure of effort) and the (log) reservation wage. As around 60% of job seekers in the treatment group reached exhaustion prior to the reform, the PBD extension from 12 to 15 months can be expected to affect a substantial share of job seekers.[17] We therefore expect that average early search behavior reacts to the benefit extension if job seekers are to some degree forward-looking.

4.1. Empirical Design

We exploit the age-specific change in the PBD by means of a cross-sectional difference-in-differences (Diff-in-Diff) design. More precisely, we compare post- to pre-reform changes in the job search behavior of individuals aged 50 to 54 to changes in the job search behavior of slightly younger individuals of age 45 to 49, who were unaffected by the reform. The corresponding empirical model is given by:

$$Y_i = \alpha + \lambda \, \mathbb{1}\,(Post_i) + \gamma \, \mathbb{1}\,(Age_i \geq 50) + \beta \, [\mathbb{1}\,(Post_i) \times \mathbb{1}\,(Age_i \geq 50)] + X_i'\rho + \varepsilon_i, \qquad (1)$$

with term Y_i referring to individual i's number of job applications since entry into unemployment or her log reservation wage. Term $\mathbb{1}\,(Age_i \geq 50)$ indicates whether a job seeker is 50 years or older, term $\mathbb{1}\,(Post_i)$ turns one if individuals are interviewed after reform implementation (see next paragraph for details). The coefficient of interest is β, the average treatment effect of the PBD extension on the search behavior of eligible individuals. Our preferred specification further controls for job seeker characteristics, denoted by vector X_i. It includes variables on job seekers' socio-demographics, such as their gender, type of training or location of residence, and past employment; see Figure 3 and Appendix Table A.2 for details. The error term is denoted as ε_i.

Definition of treatment & baseline sample. As highlighted in Section 2, the PBD extension did not only apply to eligible individuals who registered as unemployed on or after the implementation of the reform on January 1 2008. It was also applied ex post to job seekers who entered unemployment in

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[17] This exhaustion rate is in line with other evidence for Germany, such as Schmieder et al. (2012) and Schmieder and Trenkle (2020).

2007 and were still eligible for benefits by December 31 2007. If aware of this particularity, individuals who were interviewed after the reform announcement on November 12 2007 might have adjusted their job search behavior in anticipation of the reform. As this group's behavior is rather difficult to predict a priori, we exclude individuals who were interviewed between November 11 and December 31 2017 from the baseline estimation sample. In Section 4.2, we perform robustness checks where we add these individuals to the treatment group. Estimated effects decrease slightly in magnitude, but remain qualitatively unaffected.

Awareness of treatment. We only expect to observe changes in job search behavior if individuals were aware of the PBD extension. Using data from Google Trends, we provide suggestive evidence of general awareness in the population. As displayed in Figure 2, individuals' on-line search volume for the term "Arbeitslosengeld I" (unemployment benefits) over the period July 2007 to July 2008 peaked during the week of the reform announcement (November 11-17 2007) and remained at a relatively high level until the end of January 2008. By contrast, search intensity for the term "arbeitslos" (being unemployed) remained remarkably constant over the given period, suggesting that the observed peak was indeed driven by individuals searching for information about the UI scheme rather than general advice for the unemployed.

Figure 2: Awareness of the Policy Reform
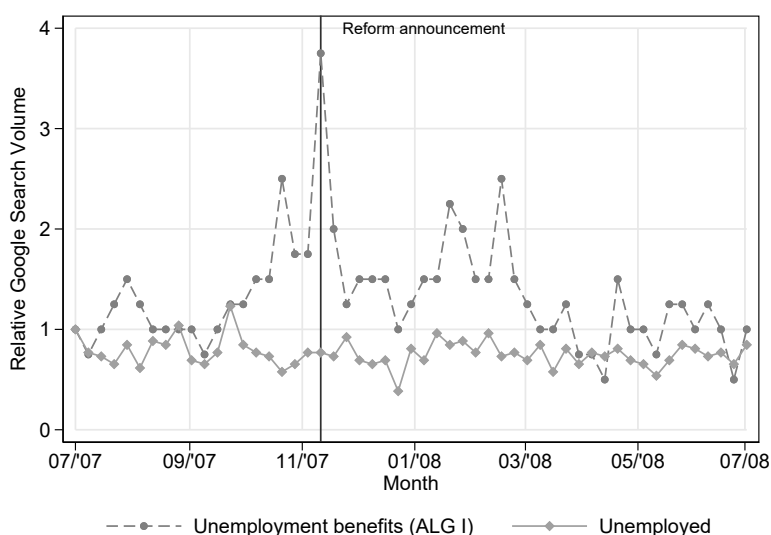
Notes: This figure presents the weekly Google search volume for the two terms "Arbeitslosengeld I" (unemployment benefits) and "arbeitslos" (unemployed) over the period from July 2007 to July 2008. Note that Google does not provide absolute numbers but normalizes queries to allow observing relative changes in search intensities for one term over time. To ease the interpretation of this graph, we follow Garthwaite et al. (2014) and divide the given weekly numbers by the respective value for the first observation in this graph, corresponding to the week of July 1-7 2007.

Identification. The causal interpretation of the treatment effect as estimated by Equation (1) relies on two main assumptions, namely that: (i) the reform did not induce any changes in the composition of both treatment and control group, and (ii) the job search behavior in control and treatment group would have followed parallel trends, had the PBD extension not been implemented ("parallel trend assumption"). To assess reform-induced changes in the composition of treatment and control group, we compare individuals from both age cohorts [45-49;50-54] before and after the PBD extension in terms of observable characteristics (vector X_i in Equation 1). Figure 3 shows the corresponding results, plotting the variables' means and 95% confidence intervals.

Figure 3: Illustration of Diff-in-Diff in Observable Characteristics
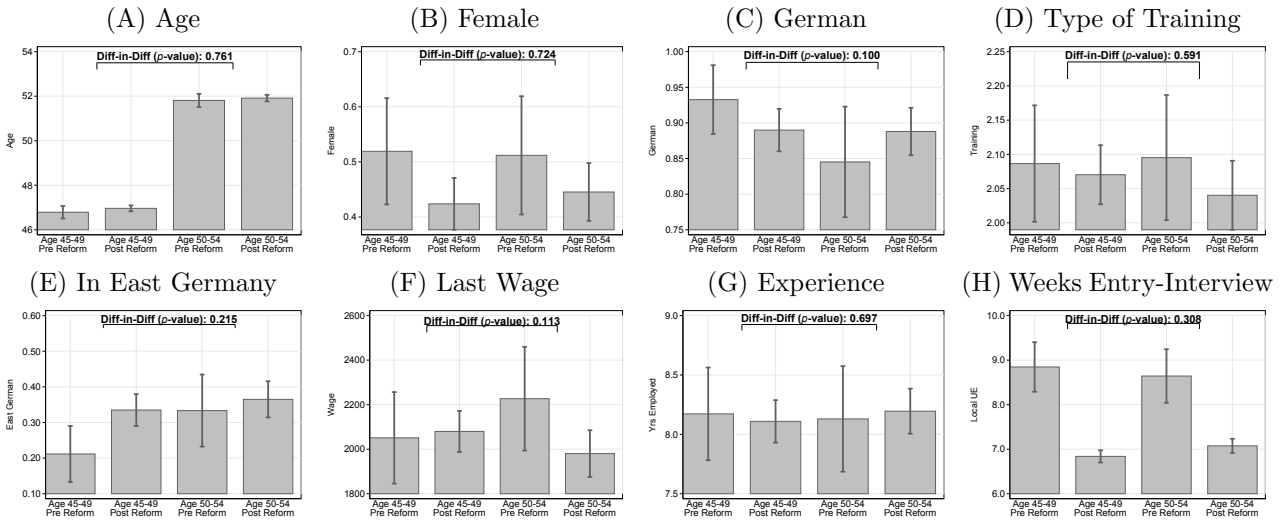
Notes: This graph illustrates differences in covariates for control and treatment group before and after treatment, respectively. The number of observations is 963. For each covariate, the figure provides the mean value and corresponding 95% confidence interval. Stated p-values relate to simple Diff-in-Diff regressions for each control variable as laid out in Equation 1. See Appendix Table A.2 for more information on the set of covariates.

We gain two key insights from this figure. First, individuals in the control and treatment group are, on average, very similar in terms of observable characteristics (see also Appendix Figure A.2). Second, we find no evidence of significant pre- versus post-reform differences in the composition of the treatment and control group. Surveyed job seekers are very similar in terms of individual (Panels B-E) and labor market characteristics (Panels F-G) before and after the reform.[18] We further run simple difference-in-differences regressions for each control variable; regressing the respective observable characteristic on the age group dummy, $\mathbb{1}(Age_i \geq 50)$, the post-reform dummy, $\mathbb{1}(Post_i)$, and the interaction of both variables, $\mathbb{1}(Post_i) \times \mathbb{1}(Age_i \geq 50)$, as laid out in Equation (1). The coefficient estimates are generally small and statistically insignificant. The corresponding p-values are shown at

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[18] We measure job seekers' experience via the time spent in regular employment within the last ten years prior to the observed unemployment spell. In the Diff-in-Diff specifications, we control for age (up to a third-order polynominal), gender, nationality, the type of training, the location of residence, the last wage prior to unemployment (in quintiles), experience (in quintiles), and the weeks between UI entry and the survey interview.

the top of each panel of Figure 3. We take this as further evidence against selection into treatment.

The only notable difference over time within both treatment and control group is the average number of weeks between an individual's entry into unemployment and the survey interview. After the PBD extension, respondents were interviewed around seven weeks after entry, compared to 8.5 weeks prior to the extension (Panel H). As this holds true for both age groups, treatment effects should, however, not be affected by these differences. Nevertheless, we control for the weeks between unemployment registration and the survey interview in our most comprehensive specification; as we do for all observable characteristics referred to.

To provide evidence on the plausibility of the parallel trend assumption underlying our empirical design, we further expand the simple difference-in-differences strategy from Equation (1) to allow for varying treatment effects over the survey period. The dynamic Diff-in-Diff specification reads as follows:

$$Y_i = \alpha + \sum_{k=1}^{n} \lambda_k \left[\mathbb{1} \left(Period_k \right) \right] + \gamma \mathbb{1} \left(Age_i \geq 50 \right) + \sum_{k=1}^{n} \beta_k \left[\mathbb{1} \left(Age_i \geq 50 \right) \times \mathbb{1} \left(Period_k \right) \right] + X_i' \rho + \varepsilon_i. \quad (2)$$

Essentially, we replace term $Post_i$ from Equation (1) with term $\sum_{k=1}^{n} Period_k$, which refers to dummy variables indicating the job seekers' respective period of interview. We create the periods in such a way that each period k approximately covers the same time interval. The period just before treatment serves as the baseline period. The coefficients of interest, β_k, measure the effect of being eligible for the PBD extension in every period k relative to this baseline. Evidence of flat pre-trends, i.e., the absence of significant effects of the PBD extension on job search behavior before the reform, would provide support of the parallel trend assumption. The specification also allows analyzing the dynamic nature of effects post treatment, albeit within a small time window (January to July 2008).

4.2. Results

In the following, we present the empirical results. We start by showing graphical evidence based on raw data and continue with presenting estimates from the simple and dynamic difference-in-differences specifications as laid out in Equations (1) and (2), respectively.

Graphical evidence. Standard models of job search predict that an increase in the PBD lowers an individual's job search effort and raises her reservation wage, because the value of unemployment increases when benefits are paid longer. Figure 4 provides a first graphical test of these predictions using raw data. Panel (A) shows distributions of the total number of applications filed between

entry into unemployment and the survey interview. The left figure in Panel (A) includes individuals aged 50 to 54. It shows that job seekers of this age cohort interviewed after reform implementation sent out substantially fewer applications on average than those interviewed prior to the reform; 12.2 compared to 17.5 applications (as indicated by the dotted and dashed horizontal lines, respectively). Importantly, this shift in the mean is not due to outlier observations. We rather observe a statistically significant shift of the entire distribution of applications to the left; the Kolmogorov-Smirnov test rejecting the equality of both distributions at the 1.2% level. We take this shift in the distribution as first suggestive evidence that the PBD extension had a direct effect on search effort at the beginning of the eligibility period. The right figure of Panel (A) corroborates this interpretation. Early search effort of individuals aged 45 to 49, who were unaffected by the PBD extension, was very similar before and after the implementation of the UI reform.

Figure 4: Changes in Search Effort and Reservation Wages
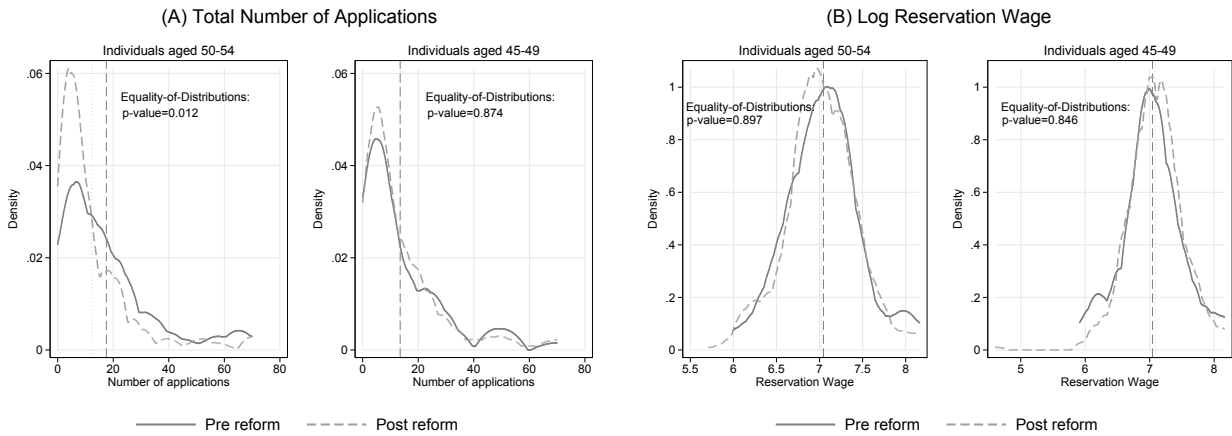
Notes: This figure compares the distributions of (i) the overall number of job applications and (ii) the log reservation wage before and after treatment for the two age cohorts, respectively. The number of filed applications is winsorized at the top 2% level, the (log) reservation wage at the top and bottom 2%. The given *p*-values refer to Kolmogorov-Smirnov tests on the equality of the two distributions per panel. The number of observations is 963 in Panel A, and 790 in Panel B.

In contrast, as shown in Panel (B) of Figure 4, we find no graphical evidence in favor of a reaction of the stated (log) reservation wage in response to the reform. Mean (log) reservation wages as well as the respective distributions are very similar, both across groups and within groups over time. Appendix Figure B.1 further shows that patterns for both outcome variables are qualitatively unaffected when not winsorizing the two outcome variables.

Simple Diff-in-Diff. We next present the results of the simple difference-in-differences strategy as laid out in Equation (1). The coefficients reported in Table 1 corroborate the graphical evidence. Columns (1) and (2) show that the PBD extension led to a statistically significant and substantial

reduction in the number of filed applications; irrespective of the inclusion of covariates. On average, the number of filed applications declined by around 5.2-5.7 applications, or 30% relative to the pre-reform treatment group average. Phrased differently, one additional month of benefits reduced overall job search effort at the beginning of the eligibility period by around 10% on average.[19] In Appendix Table B.1, we show that this finding holds true when using alternative measures of early job search effort: (i) the log number of applications, (ii) the number of filed applications per week, or (iii) the number of applications per week in nonemployment.[20]

Table 1: PBD and Job Search Behavior – Baseline Results

| | Number of Job Applications | | | | | | Log Reservation Wage | |
| | Overall | | Extensive Margin | | Intensive Margin | | | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform Dummy	-0.046	1.794	0.035	0.054*	-0.576	1.032	0.002	0.002
	(1.645)	(1.662)	(0.030)	(0.032)	(1.736)	(1.759)	(0.044)	(0.041)
Aged 50-54	4.014*	0.576	0.027	0.068	3.873	-0.166	-0.094*	-0.081
	(2.394)	(3.489)	(0.038)	(0.049)	(2.502)	(3.634)	(0.055)	(0.070)
Diff-in-Diff	-5.245**	-5.674**	-0.047	-0.049	-4.885*	-5.518**	0.062	0.038
	(2.606)	(2.554)	(0.042)	(0.041)	(2.726)	(2.664)	(0.059)	(0.055)
Number of Observations	963	963	963	963	902	902	790	790
Controls		Yes		Yes		Yes		Yes

Notes: This table shows the effect of the PBD extension on measures of job search behavior using the simple difference-in-differences design as laid out in Equation (1). Columns (1) and (2) show the corresponding effects on the total number of job applications excluding and including the set of additional controls, respectively. In columns (3) and (4) as well as (5) and (6), we report the effects of the PBD extension on job search effort at the extensive and intensive margin. Columns (7) and (8) show estimates of the PBD extension on individuals' log reservation wage. See Appendix Table A.2 for more details on the set of controls. Standard errors are heteroscedasticity robust. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results in columns (3)-(6) of Table 1 further suggest that the observed reduction in early search effort is mostly due to responses at the intensive rather than the extensive margin. Treatment effects on the probability of engaging in job search at the beginning of the eligibility period are negative but statistically insignificant; see columns (3) and (4). In contrast, effects at the intensive

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[19] We shy away from explicitly comparing the size of the treatment effect to the ones reported by Marinescu (2017) and Baker and Fradkin (2017), who study aggregate job search at the state level in response to business-cycle induced extensions of the PBD. By definition, the two studies' measures of job search effort comprise choices by employed and unemployed job searchers, as well as individuals with and without UI coverage. It is, therefore, not surprising that the two studies find a smaller reaction of job search effort to variation in the PBD.

[20] The *number of applications per week in nonemployment* accounts for the possibility of exit to nonemployment before the survey interview. It adjusts the number of filed applications for the time spent in nonemployment between unemployment entry and the interview. To this end, we divide the weekly number of applications by the share of months spent in nonemployment until the interview. As an example, the share amounts to $\frac{2}{3}$ if an individual exits nonemployment two months after unemployment entry and is interviewed three months after entry. For individuals who exit from nonemployment in or after the month of the interview (87%), the share is one and no adjustment is needed.

margin (excluding individuals without any application) are statistically significant and can explain most of the overall effect. Treatment effects, displayed in columns (5) and (6), indicate that the PBD extension from 12 to 15 months led to an average reduction in search effort by 4.9-5.5 applications during the first two months of benefit eligibility.

Table 1 also corroborates the graphical evidence on (log) reservation wages. While point estimates in columns (7) and (8) are positive, they are not statistically different from zero. Abstracting from statistical imprecision, an additional month of PBD would imply reservation wages to increase by around 1.3 to 2%. We can further rule out that an increase of the PBD by one additional month raises the average reservation wage by more than 4.8%. However, we shy away from drawing too strong conclusions from this particular result as it has been noted that the precise estimation of wage effects requires large samples (see, e.g., Nekeoi and Weber, 2017).

Figure 5: PBD and Job Search Behavior - The Role of Outliers
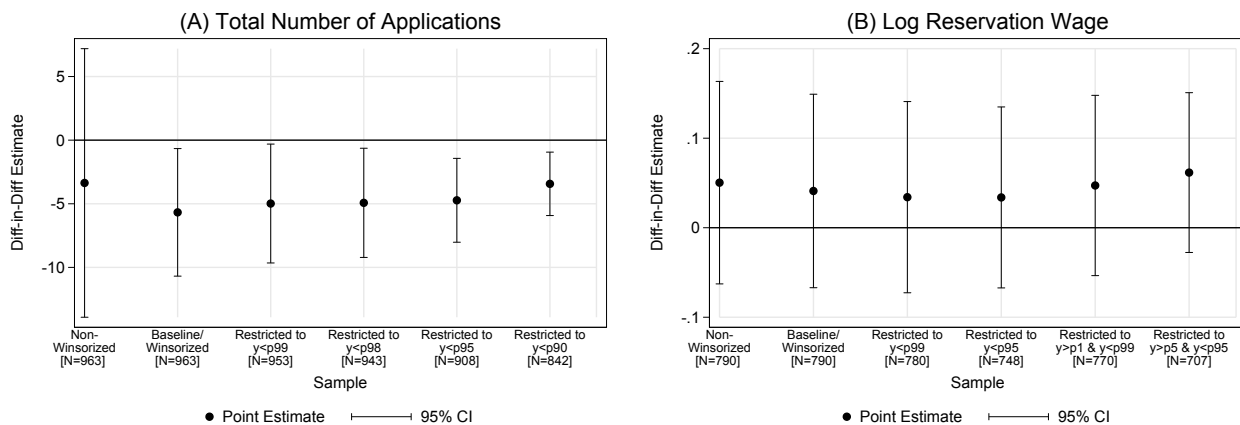
Notes: This figure shows the estimated effect of the PBD extension on early search effort (Panel A) and the log reservation wage (Panel B) when subsequently limiting the potential impact of extreme values in the two outcome variables, respectively. All estimates are based on Equation (1) and include the full set of controls.

The role of outliers. One possible limitation of the analysis is the rather small sample size, which gives rise to concerns about the role of outliers. While Figure 4 provides some first evidence that the estimates are not driven by the tails of the respective outcome distributions, we further analyze the role of outliers in the context of the regression analysis. Recall that in our baseline specification, we winsorize the two main measures of job search behavior — the overall number of job applications at the top two percent and the log reservation wage at the bottom and top two percent level. While this procedure limits, for example, the maximum number of job applications to 70, the winsorized maximum is still considerably larger than the median of 10 applications. In Figure 5, we therefore compare our baseline results to estimates we obtain when (i) not winsorizing the respective outcome

variable or (ii) sequentially discarding observations from the upper and, for the reservation wage, also the lower end of the respective distribution.

From Panel (A) of Figure 5, we infer that the estimated treatment effect on the number of early job applications slightly decreases in magnitude and becomes much more imprecise when not winsorizing the search effort outcome. In contrast, when successively discarding observations from the upper end of the distribution, point estimates remain close to the baseline coefficient and become even more precise. This implies that outliers do not drive the observed treatment effect on early search effort but rather work against statistical precision. We find a similar, albeit less pronounced pattern for the estimated treatment effect on (log) reservation wages in Panel (B).

Dynamic Diff-in-Diff. Figure 6 next presents results from the extended difference-in-differences design as laid out in Equation (2). For both outcomes, we report two different sets of estimates: in black, we plot the point estimates and the corresponding 95% confidence intervals when using the baseline sample. Statistical precision becomes even more of an issue when splitting the data into several periods. Therefore, we add — in grey — the results from a second specification where we discard the upper 5% of the respective outcome variable. In line with the evidence presented in Figure 5, coefficients become more precise when discarding outliers.

Figure 6: PBD and Job Search Behavior - Effects over Time
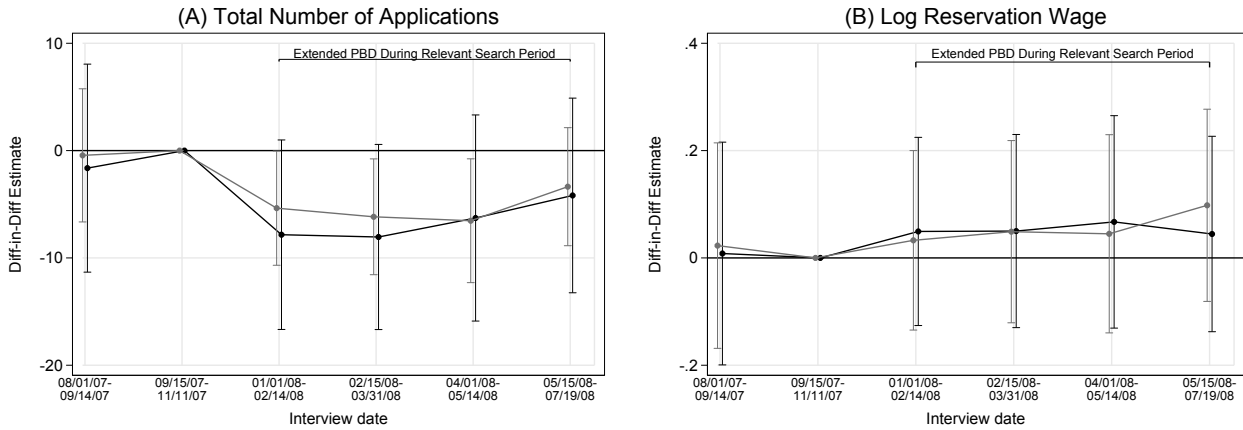
Notes: This figure shows the estimated dynamic effects of the PBD extension on the total number of job applications (Panel A) and the log reservation wage (Panel B). Estimates are based on Equation (2). Regressions include the full set of covariates as introduced in Section 4.1, as well as dummy variables for the month of the survey interview. The total number of observations is 963 (908) in Panel A, and 790 (748) in Panel B. See Appendix Table B.2 for the corresponding regression tables.

While we only observe a relatively small pre-reform period (August to November), the figures show no evidence of differential trends in job search behavior of treatment and control group before the implementation of the reform. We take this as suggestive evidence in favor of the parallel trend

17

assumption. In addition to testing for common pre-trends, this specification also allows analyzing treatment effect dynamics after reform implementation, i.e., between January and July 2008. Overall, we observe that – despite some degree of imprecision due to small sample sizes – treatment effects are rather stable over the time window, which hints at a persistent change in early job search effort within the observed time period (although estimates become less precise in later periods).

Robustness Checks. Last, we investigate the sensitivity of our estimates along three dimensions: (i) using different definitions of the treatment and control group, (ii) running placebo difference-in-differences specifications, and (iii) using alternative ways of drawing inference.

First, we vary the exact definition of the treatment and control group. Recall that we exclude job seekers interviewed between November 12 and December 31 2007 from the baseline sample (see Section 4.1). From Appendix Table B.3 we infer that adding these job seekers to the treatment group does not qualitatively affect results (compare columns 1 and 2). The treatment effect on the number of job applications becomes smaller in magnitude, but remains statistically significant at the 5% level. The same holds true when choosing an even stricter sample definition than in the baseline analysis, comparing job seekers interviewed before November 12 2007 to those entering unemployment on or after January 1 2008. The treatment effect on the number of applications remains of substantial size and statistically significant at the 5% level; see column (3) of Table B.3. As shown in columns (4) to (6), estimates of reservation wage effects react more to the sample definition but remain statistically insignificant. The result supports the notion that the lack of statistical power forces us to remain inconclusive about the adjustments of reservation wages in this study.

Next, we run a set of placebo estimations. To this end, we follow Rosenbaum (1987) and estimate difference-in-differences regressions as laid out in Equation (1); but for two groups of job seekers that were not subject to the PBD extension. Appendix Table B.4 presents the corresponding results for three different specifications of this test. In column (1) of Panels A and B of the table, we first consider job seekers from the same two age cohorts as in our baseline analysis [45-49;50-54], but focus on individuals who were *not* entitled for the maximum PBD because they did not fulfill the contribution requirements. We find statistically insignificant and close to zero pseudo-treatment effects for both measures of job search behavior. We obtain similar estimates when focusing on younger age cohorts. In column (2), we assign individuals of age 45 to 49 a pseudo treatment group status and use individuals of age 40 to 44 as the control group. In column (3) of both panels, we compare job seekers of age 30 to 34 to job seekers aged 35 to 39. In both specifications of this test, point estimates of the pseudo treatment effect are insignificant and close to zero. We take these findings as further evidence

that the observed changes in the search effort of eligible job seekers aged 50-54 can be attributed to the PBD extension.

Last, we test the robustness of the results to alternative ways of drawing inference; see Appendix Table B.5. For each outcome, we compare p-values from the baseline specification, based on heteroscedasticity-robust standard errors, to p-values based on (i) jackknife bootstrapped standard errors, (ii) the wild-cluster bootstrap-t test (clustering at job seekers' exact age), and (iii) randomization inference in spirit of Fouka and Voth (2016). Effects on search effort remain statistically significant at the 5% level irrespective of the inference procedure applied.

5. Potential Benefit Duration and Re-Employment Outcomes

To provide further interpretation to the observed changes in early job search behavior, we now estimate the effects of the PBD extension on re-employment outcomes. Here, we rely on social security data that cover a substantially larger sample of job seekers (see Section 3.2 for details).[21] We focus on four different re-employment outcomes: (i) the probability of job finding within three months, (ii) the duration of UI receipt, (iii) the duration of nonemployment, i.e., the time between UI entry and the next employment spell, and (iv) the re-employment wage.

In line with Section 4, we estimate treatment effects by means of the simple and dynamic Diff-in-Diff models as laid out by Equations (1) and (2). However, two important differences apply. First, we now assign treatment based on the date of UI entry rather than the interview date, which is by construction unavailable in the social security data. Second, we need to explicitly account for the retroactive application of the PBD extension. As detailed in Section 2, all individuals who entered UI in 2007 and were still eligible for benefits by December 31 2007 became ex post entitled to 15 instead of 12 months of benefits. As duration outcomes (as opposed to early search behavior) may potentially be affected by the retroactive application of the reform, we need to rely on job seekers who entered UI in 2006 as a fully untreated comparison group.[22]

Simple Diff-in-Diff. We first present the results from the simple Diff-in-Diff model that compares fully treated to fully untreated job seekers. Thus, we discard all job seekers who became unemployed in 2007 and could have become treated ex post upon reform implementation. Table 2 presents the

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[21] Note that we derive very similar treatment effects on the two measures of job search behavior when weighting the corresponding regressions with the moments of the larger administrative dataset (see Appendix Table B.6).

[22] More precisely, we consider individuals who entered UI between February and December 2006 as the comparison group. Individuals who entered UI before February 2006 faced yet another PBD scheme.

corresponding results.

Columns (1) and (2) first report the estimated effects on the probability to start a new job within the first three months after UI entry.[23] In light of the substantial UI-induced reductions in early search effort (cf. Section 4), it is natural to expect a response in the probability of early job finding. Results confirm this intuition. Job seekers subject to the PBD extension are estimated to be around two percentage points less likely to start a new job within the first three months of unemployment, irrespective of the introduction of control variables (X_i).[24] The result that early job finding rates decline in response to a longer PBD is in line with previous evidence (see, e.g., Card et al., 2007b; Schmieder et al., 2012).

Table 2: PBD and Re-Employment Outcomes – Baseline Results

	Pr(Job Finding within 3 months)		Duration of UI Receipt		Duration of Nonemployment		Re-Employment Wage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform Dummy	0.038***	0.036***	-25.312***	-24.499***	-17.294***	-14.948***	0.039***	0.035***
	(0.006)	(0.006)	(1.567)	(1.540)	(2.210)	(2.154)	(0.007)	(0.005)
Age 50-54	-0.035***	0.013	19.081***	-9.881**	24.060***	-0.357	-0.011	0.017
	(0.006)	(0.013)	(1.960)	(4.130)	(2.591)	(5.247)	(0.008)	(0.013)
Diff-in-Diff	-0.017**	-0.021**	20.563***	22.169***	8.911**	10.860***	-0.012	-0.011
	(0.009)	(0.008)	(2.693)	(2.633)	(3.499)	(3.381)	(0.011)	(0.008)
Number of Observations	38,587	38,587	38,587	38,587	38,587	38,587	29,993	29,993
Controls		Yes		Yes		Yes		Yes

Notes: This table shows the effect of the PBD extension on re-employment outcomes. It only includes job seekers who registered as unemployed in either 2006 or 2008, thereby excluding partially treated individuals. Outcomes are the probability to start a new job within the first three months of benefit receipt in columns (1) and (2), the duration of UI receipt in columns (3) and (4), the duration of nonemployment in columns (5) and (6), and the log re-employment wage in columns (7) and (8). See Appendix Table A.3 for summary statistics on control variables. Standard errors are heteroscedasticity robust. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

From columns (3) and (4), we next infer that the PBD extension increased the duration of actual UI receipt by around 21 to 22 days on average. The effect corresponds to an elasticity of around 0.24 months of actual benefit receipt per month of potential benefit receipt.[25] To trace out how much of

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[23] For this outcome, we could in principle use all job seekers who entered UI before October 2007, i.e., three months prior to reform implementation, as a comparison group. For reasons of consistency, we refrain from this approach here. However, note that Panel A of Figure 7 shows that — in line with expectations — we observe no changes in early job finding for the 2007 cohort.

[24] In correspondence with Section 4, we control for age (up to the second-order polynomial), gender, nationality, the type of training, the location of residence (West vs. East Germany), the last wage prior to unemployment (in quintiles), and labor market experience (in quintiles).

[25] Note that the coefficients on the baseline terms are in line with expectations: the German unemployment rate was considerably lower in 2008 than in 2006, which explains why individuals have shorter spells post reform on average. Moreover, older job seekers take longer to find a job, which is in line with the positive age group dummy.

this response can be actually linked to UI-induced disincentives rather than additional coverage, we focus on the effect of the PBD extension on the duration of nonemployment, i.e., the time between UI entry and the start of a new job. The corresponding effects are reported in columns (5) and (6) of Table 2. We find that the PBD extension raised the duration of nonemployment by around 10 to 11 days on average, which implies an elasticity of around 0.1 months per additional month of PBD. Taken together, these estimates suggest that one extra day of effective UI coverage comes along with around 0.4 additional days of nonemployment. The elasticities are in line with the results from previous studies conducted in similar settings (see, e.g., the overview by Schmieder and von Wachter, 2016).[26]

Finally, we investigate the effect of the PBD extension on the (log) re-employment wage. From a theoretical perspective, the effect is ambiguous (Schmieder et al., 2016; Nekeoi and Weber, 2017). On the one hand, the PBD extension may potentially increase individuals' reservation wage, which pushes re-employment wages upward. On the other hand, a prolonged time in nonemployment may lead to lower wage offers due to stigma effects or skill depreciation. The corresponding point estimates reported in columns (7) and (8) of Table 2 are negative but statistically insignificant. In line with Schmieder et al. (2016), this may suggest that the negative effect of prolongued nonemployment dominates. However, very large samples are generally necessary to detect significant effects on re-employment wages in response to changes in UI generosity. We therefore refrain from drawing any strong conclusion from our comparatively small sample here.

Dynamic Diff-in-Diff. We next explicitly account for the retroactive application of the reform and study how the PBD extension at varying points of the individuals' eligibility period affected the four re-employment outcomes of interest. To this end, we estimate the dynamic Diff-in-Diff model as laid out in Equation (2) over the sampling period 2006-2008.[27] The term $\sum_{k=1}^{n} Period_k$ now refers to a set of dummy variables that indicate the date of entry into UI in half-year intervals, ranging from the first half of 2006 to the second half of 2008. Recall that all job seekers of age 50 to 54 who entered UI in 2007 were potentially eligible for the extended benefit duration. We therefore set the baseline period to the second half of 2006. This is the last inflow period in which job seekers could not become treated ex post as they had exhausted their eligibility period by the end of 2007 at the latest.

In Panel (A) of Figure 7, we first present the dynamic effects of the PBD extension on the

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[26] In Appendix Table B.7, we present additional Diff-in-Diff estimates on the duration of UI receipt and nonemployment when winsorizing these outcomes at three, six and twelve months, respectively. Results suggest that the nonemployment effect for job seekers treated at the beginning of the unemployment spells is almost entirely driven by behavioral responses during the first twelve months since UI entry, i.e., during the pre-reform coverage period. In turn, only half of the effect on the duration of UI receipt occurs during this period.

[27] Unfortunately, the social security data at hand do not cover entries beyond 2008. Moreover, note that we cannot use entries before February 2006 because a different age-dependent PBD scheme was in place until January 2006.

probability of exit from nonemployment within three months after UI entry. We first note that pre-trends are flat, which we take as indicative support of the common trend assumption. We also find no significant treatment effect on the probability of early exit from nonemployment for job seekers registering as unemployed in 2007. This is fully expected, as most of these job seekers were treated at a later point of their eligibility period.[28] In line with the results from Table 2, effects on the probability of early exit materialize for job seekers entering UI in 2008. The corresponding reduction amounts to around 1.5 to 3 percentage points, or 10 to 20 percent relative to the treatment group mean in 2006.

Figure 7: PBD and Re-Employment — Effects over Time
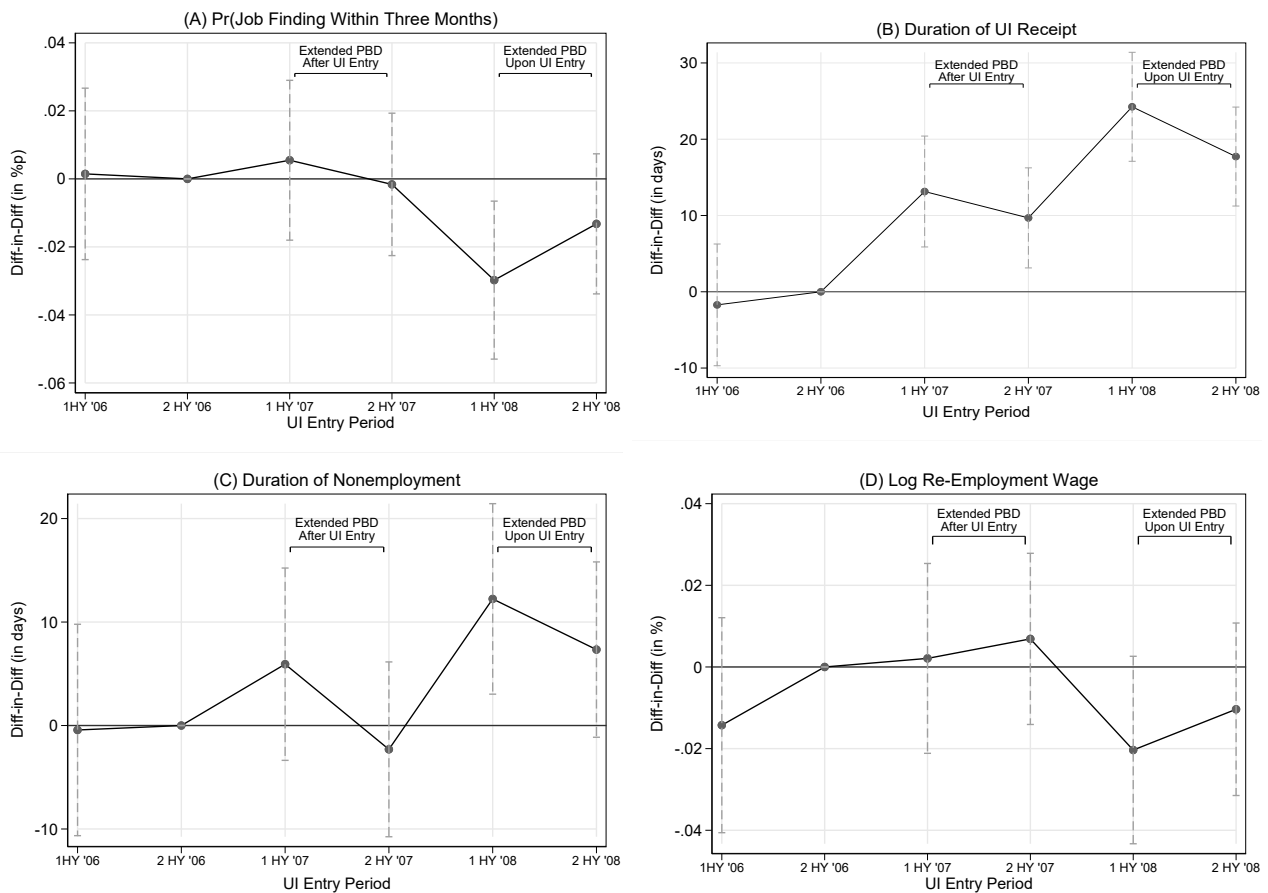
Notes: This figure plots the effects of the PBD extension on re-employment outcomes when using the dynamic Diff-in-Diff design as defined in Equation (2). Outcomes are the probability to start a new job within three months (Panel A), the duration of UI receipt (Panel B), the duration of nonemployment (Panel C), and the (log) re-employment wage (Panel D). Job seekers in the treatment group (aged 50-54) who registered in 2006 were fully unaffected by the PBD extension. Those who registered in 2007 were treated ex post if they were still eligible for benefits on December 31 2006. Those who registered in 2008 were treated upon registration. The number of observations is 59,078 in Panels (A)-(C), and 38,587 in Panel (D).

Panels (B) and (C) of Figure 7 next plot the estimated treatment effects for the duration of UI receipt and nonemployment, respectively. We find that the extension of the PBD led to an increase in

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[28] Appendix Figure B.2 shows that there is also no significant effect on the probability to exit from UI within the first three months of unemployment for the 2007 entry cohort.

the duration of UI receipt by around 10 days on average among job seekers who entered UI in 2007. Interestingly, treatment effects are of comparable size for individuals who entered UI in the first and second half of 2007. At first glance, this may be surprising because job seekers who entered UI in the second half of 2007 got treated earlier during their respective eligibility period. However, this finding may also suggest that the estimated effect is mostly driven by individuals who exhaust their benefits. In line with this intuition, it appears that the PBD extension did not significantly affect the average duration of nonemployment for individuals who entered UI in 2007 (Panel C). This suggests that individuals who were treated during an ongoing spell benefited from an increased UI coverage without exhibiting any meaningful adverse labor supply effect. Last, for job seekers entering UI in 2008 we find estimates in line with those presented in Table 2. Individuals subject to extended benefits upon registration remained in UI for an additional 18-23 days. In turn, the duration of nonemployment increased by around 7-12 days on average for this group of job seekers.[29]

Finally, Panel (D) of Figure 7 shows a slightly negative, yet statistically insignificant effect on (log) re-employment wages for the treated 2008 entry cohort (cf. Table 2). We do not find any effect for the 2007 entry cohort.

Effects over the unemployment spell. The previous set of results suggests that an extension of the PBD creates little disincentive effects among job seekers who were affected by the benefit extension during their eligibility period (the 2007 cohort). However, the observed pattern may simply reflect that a large share of the 2007 cohort had already left unemployment at the date of reform implementation and was thus de facto unaffected by the benefit extension. In the following, we therefore explicitly test for differential responses among the short-term versus the long-term unemployed. To this end, we condition on individuals still unemployed at reform implementation and allow for heterogeneous effects among job seekers treated at different points of their unemployment spell. Notably, this analysis relies on the assumption that the reform had no effect on exit before its implementation, i.e., did not cause dynamic selection.

Panel (a) of Figure 8 plots the estimated effects on the duration of UI receipt. We observe that the effect on UI coverage is most pronounced for the long-term unemployed. This is in line with expectations and partly mechanical, given that those individuals at the risk of reaching exhaustion should gain most from the extension of benefits. Panel (b) of Figure 8 next plots the corresponding effects on the duration of nonemployment. Here, point estimates suggest that the long-term unemployed react

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[29] We note that effects become slightly smaller and lose statistically precision in the second half of 2008. Unfortunately, the available data do not allow exploring whether this reflects changes in the salience of the UI reform, seasonal variation in the elasticity, or simply statistical noise.

much less than the short-term unemployed. Among the short-term unemployed, i.e., those treated upon UI entry or within in the first two months of unemployment, one extra day of actual bene-fit receipt leads to around 0.4 additional days of nonemployment. In contrast, the large additional UI coverage for the long-term unemployed does not seem to come at the cost of additional days of nonemployment. Although large standard errors warrant some extra caution when interpreting these coefficients, the observed pattern provides suggestive evidence that the moral hazard costs of PBD extensions may be lower for the long-term than the short-term unemployed.

Figure 8: PBD and Re-Employment – Effects over the Unemployment Spell

(a) Duration of UI Receipt

(b) Duration of Nonemployment
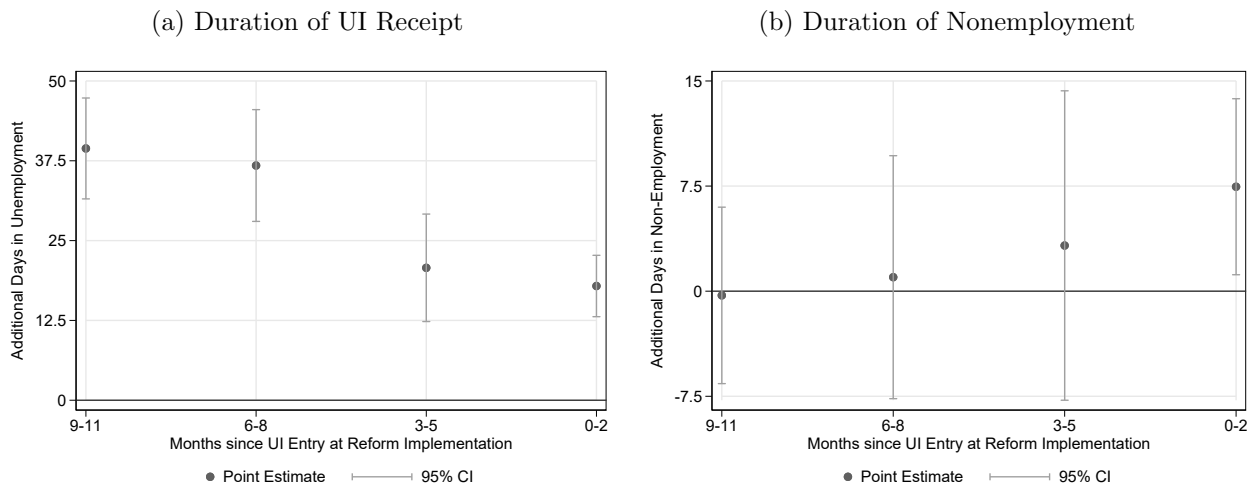
Notes: This figure plots the effect of the benefit extension on the duration of UI receipt (Panel A) and the duration of nonemploymnet (Panel B) when running separate difference-in-differences regressions as defined by Equation (1) for different UI entry cohorts and restricting the analysis to those eligble job seekers who were unemployed longer than nine/six/three/zero months, respectively. The cohort "0-2" includes job seekers treated upon UI entry (the 2008 cohort).

The Returns to Search Effort. In a final step, we make use of the estimated effects to carefully assess the individual returns to early job search effort. We acknowledge that this exercise is not trivial for at least two reasons. First, the retroactive application of the reform to job seekers unemployed before reform implementation implies that individuals who were untreated at the time of the survey interview could have become treated at a later stage of their unemployment spell. This feature of the reform makes it impossible to directly relate reform-induced changes in search behavior at the time of the survey interview to the overall duration of nonemployment. Second, we measure search effort around 5-16 weeks after UI entry. Extrapolating from the derived effects at the start of job seekers' eligibility period to changes in search effort over the entire spell would require unrealistic assumptions.

Instead, we propose to cautiously relate the observed UI-induced reductions in early job search effort (Table 1) to the observed change in the probability to exit from nonemployment within three

months. To best match the survey sample period, we focus on the effect on individuals who entered UI in the first half of 2008, as reported in Panel A of Figure 7. If we assume that the observed reduction in early nonemployment exit in the first half of 2008 by around 20 percent was merely due to the reduction in search effort ($\approx$-5.5 applications), we estimate that a decrease in search effort of around 10% per additional month of potential benefits is associated with a 7% reduction in the probability to exit nonemployment within the first three months. However, this result has to be taken with a grain of salt. It is based on estimates that come from different datasets and slightly varying specifications. Moreover, it relies on several strong assumptions, such as a constant application quality irrespective of the number of filed applications and the irrelevance of reservation wages.

6. Conclusion

This paper estimates the effects of an increase of the potential benefit duration (PBD) on early job search behavior and re-employment outcomes in a joint framework. For identification, we exploit an unexpected reform of the age-specific UI scheme in Germany. The policy change led to an increase in the PBD for eligible job seekers of age 50 to 54 from 12 to 15 months, while the PBD remained unchanged for slightly younger job seekers. Using detailed survey data and a difference-in-differences approach, we first show that the increase in the PBD led to a significant reduction in individuals' job search effort at the beginning of their respective eligibility period. On average, one additional month of potential benefits reduced early search effort by around 10% of the treatment group's pre-reform mean. Estimated effects on the reservation wage are positive, but lack statistical precision.

Using comprehensive social security data, we further show that the benefit extension decreased the probability of early job finding, as well as increased the overall duration of benefit receipt and nonemployment. We derive an elasticity of around 0.1 additional months in nonemployment per additional month of the PBD. The corresponding elasticity of UI receipt amounts to around 0.24, which reveals that one extra day of actual UI coverage comes along with around 0.4 additional days of nonemployment on average. Exploiting the retroactive implementation of the reform, we further provide suggestive evidence that job seekers exert little moral hazard behavior when they receive extended UI coverage during an ongoing unemployment spell.

By identifying sizable effort reductions in response to an increase of the PBD at the beginning of the eligibility period, our results highlight the importance of public policy measures that directly target early effort provision. Prior evidence has shown that suitable policies to reduce UI-induced moral hazard behavior exist and may be implemented at rather low cost. Such measures may help

policy makers to reconcile generous UI coverage with limited disincentive effects. This is especially relevant as the results of this paper suggest substantial returns to early job search.

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Appendix A   Additional Descriptive Statistics

Table A.1: Claimants' Age, Length of UI Contributions and PBD

Before January 1 2008

Period of UI contribution (months)	12	16	20	24	30	36	
& Age of eligible person .. or above					55	55	
Potential Benefit Duration (PBD)	6	8	10	12	15	18	

After January 1 2008

Period of UI contribution (months)	12	16	20	24	30	36	48
& Age of eligible person .. or above					50	55	58
Potential Benefit Duration (PBD)	6	8	10	12	15	18	24

Notes: This table shows the relationship between a claimant's age, length of UI contributions and her potential benefit duration. Note that prior to the reform, the qualifying period determining the length of coverage was three years. It was extended to five years by January 1 2008.

Figure A.1: The Distribution of Job Search Effort and Reservation Wages by Age Groups
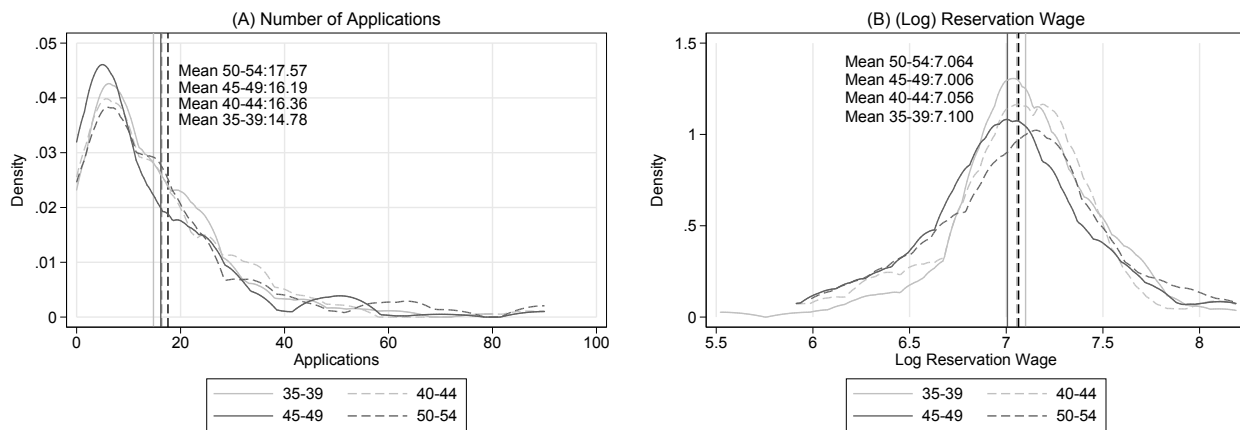
Notes: This figure plots the distribution of the number of job applications (Panel A) and the (log) reservation wage (Panel B) for different age cohorts. Vertical lines indicate the cohorts' corresponding mean values.

Figure A.2: Observable Characteristics by Age Groups
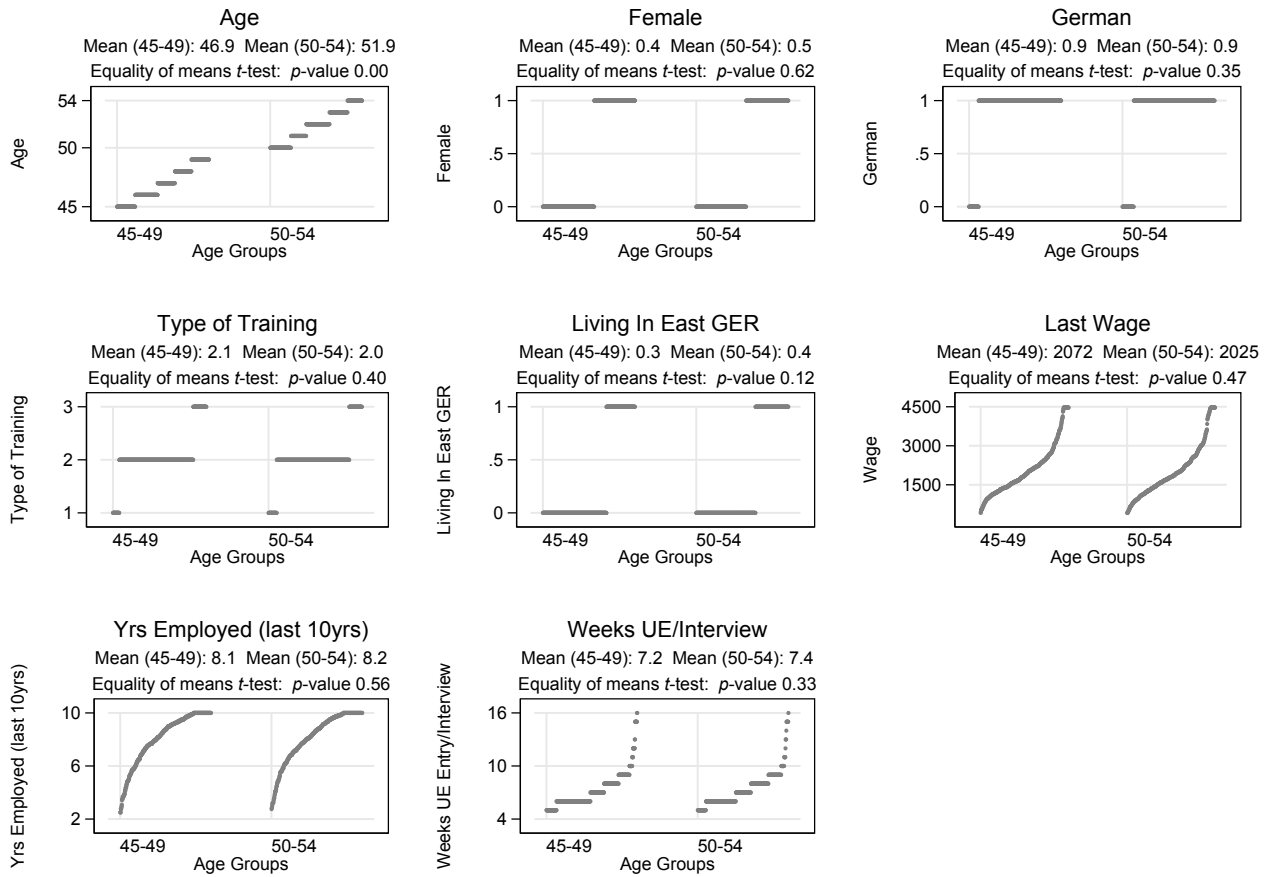
Notes: This figure plots the distribution of observable characteristics by age groups. For each covariate, we further state mean values for control and treatment group. The given *p*-value in each panel refers to a *t*-test on the equality of means between both groups. The number of observations is 963.

Table A.2: Descriptive Statistics – Baseline Survey Sample

|	Mean	Std Deviation	Minimum	Maximum	Observations
A. Outcomes					
Total Number of Applications	13.354	14.71	0.00	70.00	967
Number of Applications (Intensive Margin)	14.292	14.92	1.00	72.00	906
Job Search (no/yes)	0.937	0.24	0.00	1.00	967
Log Number of Applications	2.199	0.98	0.00	4.28	906
Applications per Week	1.887	2.11	0.00	10.00	967
(Log) Reservation Wage	7.052	0.43	4.61	8.16	793
B. Controls					
Age	49.156	2.84	45.00	54.00	967
Female	0.450	0.50	0.00	1.00	967
German	0.888	0.32	0.00	1.00	967
Type of Training	2.062	0.46	1.00	3.00	967
Last Wage	2,051	1,002	420	4,470	967
Years in Employment (last 10yrs)	8.146	1.89	2.50	10.00	967
Living in East Germany	0.331	0.47	0.00	1.00	967
Weeks b/w Unemployment Entry and Interview	7.304	1.98	5.00	16.00	967

Notes: This table presents descriptive statistics for outcome and control variables from the survey sample as used in Section 4. Source: IZA/IAB Linked Evaluation Dataset.

Table A.3: Descriptive Statistics – Baseline Admin Sample

	All			Treatment Group			Control Group		
	Mean	SD	Observations	Mean	SD	Observations	Mean	SD	Observations
A. Outcomes									
Duration of UI Receipt	180.0	127.9	59,078	198.5	140.4	23,817	167.5	117.1	35,261
Duration of Non-Employment	258.3	168.0	59,078	275.1	169.0	23,817	247.0	166.4	35,261
P(Exit from NE w/in 3 Mon)	0.228	0.42	59,078	0.203	0.402	23,817	0.245	0.43	35,261
Log Re-Employment Wage	7.292	0.47	46,199	7.281	0.46	17,459	7.299	0.47	28,740
B. Controls									
Age	49.269	3.04	59,078	52.721	1.01	23,817	46.936	1.14	35,261
Female	0.462	0.50	59,078	0.473	0.50	23,817	0.455	0.50	35,261
German	0.930	0.26	59,078	0.933	0.25	23,817	0.928	0.26	35,261
Type of Training	1.35	0.923	59,078	1.357	0.921	59,078	1.344	0.93	35,261
Last Wage	1,778	911	59,078	1,769	916	23,817	1,783	907	35,261
Years in Employment (last 10 yrs)	7.976	1.86	59,078	8.099	1.80	23,817	7.892	1.89	35,261
Living in East Germany	0.325	0.47	59,078	0.337	0.47	23,817	0.317	0.47	35,261

Notes: This table presents descriptive statistics for outcome and control variables from the admin sample as used in Section 5. The inflow period is February 2006 to December 2008. Source: IZA/IAB Administrative Evaluation Dataset (AED).

Figure A.3: Monthly Exit Rates from Nonemployment
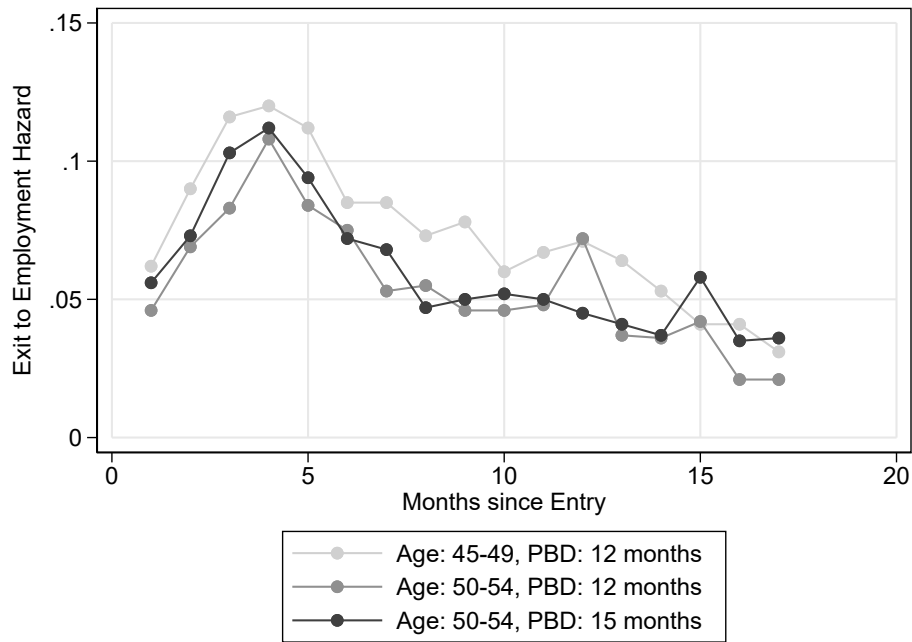
Notes: This figure plots the monthly exit rates from nonemployment for individuals subject to different lengths of the PBD. It only includes job seekers who registered as unemployed in either 2006 or 2008, thereby excluding partially treated individuals. Source: IZA/IAB Administrative Evaluation Dataset (AED).

Appendix B   Additional Results

Figure B.1: Changes in Job Search Behavior – Non-Winsorized Distributions
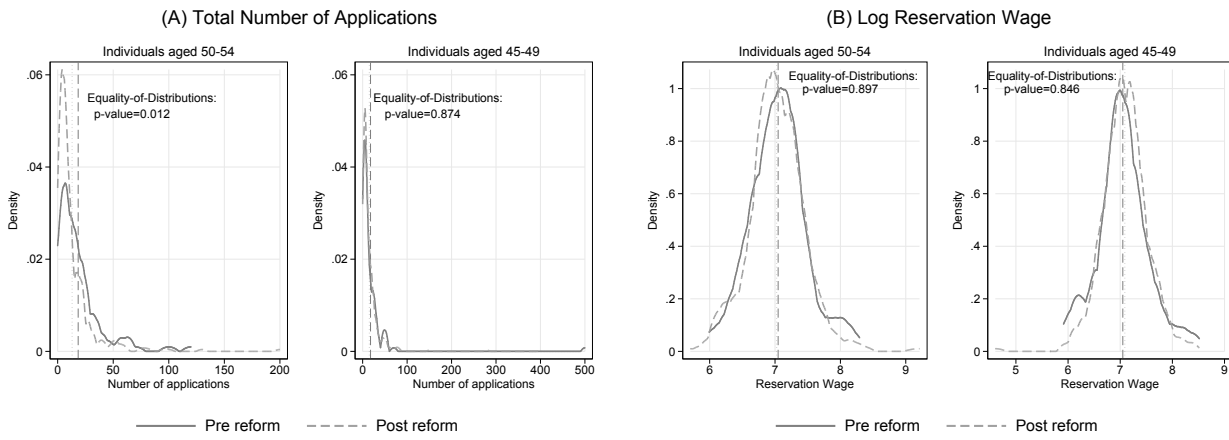
Notes: This graph compares the distributions of (i) the overall number of job applications and (ii) the log reservation wage before and after treatment for the two age cohorts, respectively. The given *p*-values refer to Kolmogorov-Smirnov tests on the equality of the two distributions per panel. The number of observations is 963 in Panel A, and 790 in Panel B.

Figure B.2: PBD and the Probability of Exit from UI Receipt within Three Months
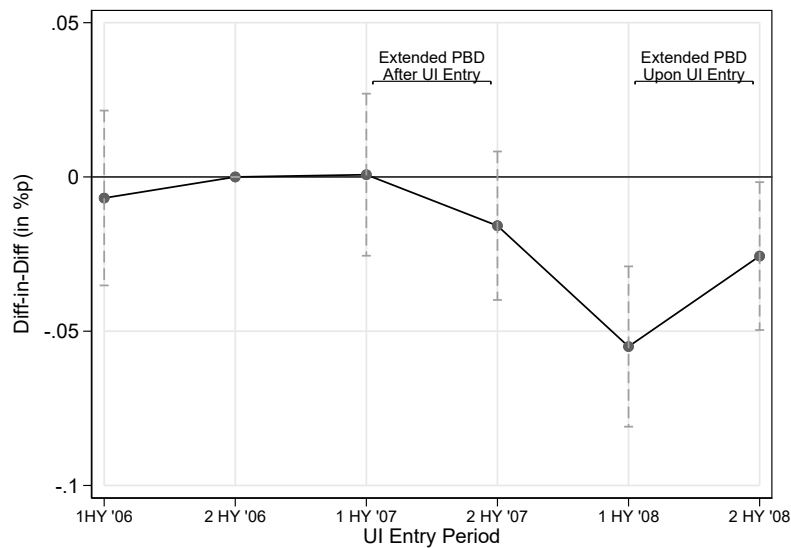
Notes: This figure plots the effect of the PBD extension on the probability to exit UI within three months when using the dynamic Diff-in-Diff design as defined in Equation (2). Job seekers in the treatment group (aged 50-54) who registered in 2006 were fully unaffected by the PBD extension. Those who registered in 2007 were treated during their unemployment spell if they were still eligible for benefits on December 31 2006. Those who registered in 2008 were treated upon registration. The number of observations is 59,078.

Table B.1: PBD and Job Search Effort – Alternative Specifications of the Outcome Variable

	Log Number of Applications		Applications per Week		Application per Week in NE	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform Dummy	-0.002	0.163	0.353	0.226	0.392	0.287
	(0.117)	(0.126)	(0.223)	(0.228)	(0.251)	(0.252)
Aged 50-54	0.306**	0.181	0.497	0.083	0.427	-0.064
	(0.154)	(0.231)	(0.311)	(0.483)	(0.330)	(0.518)
Diff-in-Diff	-0.373**	-0.448***	-0.726**	-0.706**	-0.769**	-0.757**
	(0.170)	(0.167)	(0.346)	(0.343)	(0.368)	(0.364)
Number of Observations	902	902	963	963	963	963
Controls		Yes		Yes		Yes

Notes: This table shows the effect of the PBD extension on alternative specifications of the job search effort outcome variable using the simple difference-in-differences design as laid out in Equation (1). Columns (1) and (2) show the corresponding effects on the log number of job applications, excluding and including the set of controls. In columns (3)-(6), we account for the varying length between the individuals' UI entry and their survey interview. In columns (3) and (4), we look at the number of filed applications per week. In columns (5)-(6), we look at the (log) number of applications per week in nonemployment. This variable explicitly accounts for potential differences in the duration of nonemployment between treatment and control group. See Appendix Table A.2 for more details on the set of controls. Standard errors are heteroscedasticity robust. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: PBD and Job Search Behavior – Treatment Effects over Time

	Number of Job Applications				Log Reservation Wage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Effect	-5.674**		-4.726***		0.038		0.028	
	(2.554)		(1.679)		(0.055)		(0.052)	
× Interviewed 08/01/07-09/14/07		-1.634		-0.445		0.008		0.023
		(4.938)		(3.161)		(0.106)		(0.098)
× Interviewed 01/01/08-02/14/08		-7.837*		-5.362**		0.049		0.033
		(4.499)		(2.712)		(0.089)		(0.085)
× Interviewed 02/15/08-03/31/08		-8.050*		-6.167**		0.050		0.049
		(4.395)		(2.750)		(0.092)		(0.086)
× Interviewed 01/04/08-05/14/08		-6.281		-6.535**		0.067		0.045
		(4.893)		(2.939)		(0.101)		(0.094)
× Interviewed 05/15/08-07/19/08		-4.180		-3.363		0.045		0.098
		(4.621)		(2.802)		(0.093)		(0.091)
Number of Observations	963	963	908	908	790	790	748	748
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of the PBD extension on job search behavior using the difference-in-differences designs as laid out in Equations (1) and (2). Columns (1) and (5) replicate the baseline Diff-in-Diff results for the number of job applications and the log reservation wage, respectively. In Column (2) and (6), we allow for varying treatment effects over time. In columns (3) and (7) as well as columns (4) and (8), we run the same regressions but discard individuals from the upper 5% of the respective outcome distributions. All regressions include the set of covariates as defined in Section 4.1. Standard errors are heteroscedasticity robust. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: PBD and Job Search Behavior – Different Sample Definitions

	Number of Job Applications			Log Reservation Wage		
	Base Sample (1)	Full Sample (2)	Restricted Sample (3)	Base Sample (4)	Full Sample (5)	Restricted Sample (6)
Reform Dummy	1.794	1.160	2.058	0.002	0.048	0.013
	(1.662)	(1.361)	(1.781)	(0.041)	(0.031)	(0.043)
Aged 50-54	0.576	-0.134	-0.843	-0.081	0.005	-0.060
	(3.489)	(2.804)	(3.774)	(0.070)	(0.055)	(0.074)
Diff-in-Diff	-5.674**	-4.534**	-5.257**	0.038	-0.042	0.031
	(2.554)	(2.085)	(2.604)	(0.055)	(0.042)	(0.056)
Number of Observations	963	1,125	733	790	926	595
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of the PBD extension on job search behavior using the simple difference-in-differences design as laid out in Equation (1) and different definitions of the estimation sample. Columns (1) and (4) replicate the baseline effects on the number of job applications and the log reservation wage, respectively. In columns (2) and (5), we add those job seekers who were interviewed between 11/12-12/31/2007. In columns (3) and (6), we only include those job seekers who were either interviewed before November 12 2007 or entered unemployment after 2008. All regressions include the set of covariates as defined in Section 4.1. Standard errors are heteroscedasticity robust. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: PBD and Job Search Behavior – Pseudo Difference-in-Differences Regressions

	[45-49,50-54] Non-Eligible	Treatment:45-49 Control:40-44	Treatment:40-44 Control:35-39
	(1)	(2)	(3)
A. Number of Applications			
(Pseudo-)Diff-in-Diff	1.029	1.686	-0.904
	(3.240)	(2.198)	(1.898)
Number of Observations	784	1,078	1,016
B. (Log) Reservation Wage			
(Pseudo-)Diff-in-Diff	0.035	-0.017	0.007
	(0.085)	(0.053)	(0.047)
Number of Observations	582	873	829
Controls	Yes	Yes	Yes

Notes: This table shows the effect of pseudo difference-in-differences regressions using two cohorts of job seekers not affected by the reform, and assigning one to be. In column (1) of Panels A and B, we consider job seekers from the same two age cohorts as in our baseline analysis [45-49;50-54], but focus on those individuals who were not entitled for the maximum PBD and hence not subject to the reform. In column (2), we consider individuals of age 45 to 49 to be treated and compare their job search behavior before and after the PBD extension to job seekers of age 40 to 44. In column (3), we compare job seekers of age 30 to 34 to job seekers aged 35 to 39. All regressions include the set of covariates as defined in Section 4.1. Standard errors are heteroscedasticity robust. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: PBD and Job Search Behavior – Alternative Ways of Drawing Inference

| | Number of Job Applications | | | |
| | Overall | Extensive Margin | Intensive Margin | Log Reservation Wage |
	(1)	(2)	(3)	(4)
Baseline Estimate	-5.674	-0.049	-5.518	0.038
Heteroskedasticity-consistent p-values	[0.027]	[0.231]	[0.039]	[0.494]
Jackknife-robust p-values	[0.029]	[0.240]	[0.042]	[0.504]
Wild-Cluster Bootstrap-t p-values	[0.028]	[0.240]	[0.046]	[0.504]
Randomization Inference p-values	[0.008]	[0.128]	[0.020]	[0.326]

Note: This table reports *p*-values for the four job search outcomes of interest as presented in Table 1 when using alternative ways of drawing inference. First, we re-produce the baseline treatment effects along with their respective p-values when using heteroskedasticity-consistent standard errors. We further state the corresponding *p*-values when using (i) jackknife standard errors, (ii) the wild cluster percentile bootstrap test (clustering at job seekers' age), and (iii) using randomization inference in the spirit of Fouka and Voth (2016).

Table B.6: PBD and Job Search Behavior – Using Weights from Moments of the Admin Data

| | Number of Job Applications | | | | | | | |
| | Overall | | Extensive Margin | | Intensive Margin | | Log Reservation Wage | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform Dummy	1.794	1.409	0.054*	0.066**	1.032	0.566	0.002	0.026
	(1.662)	(2.098)	(0.032)	(0.032)	(1.759)	(2.261)	(0.041)	(0.045)
Aged 50-54	0.576	3.104	0.068	0.066	-0.166	2.388	-0.081	-0.064
	(3.489)	(4.303)	(0.049)	(0.048)	(3.634)	(4.520)	(0.070)	(0.082)
Diff-in-Diff	-5.674**	-8.555***	-0.049	-0.055	-5.518**	-8.383**	0.038	0.038
	(2.554)	(3.297)	(0.041)	(0.042)	(2.664)	(3.468)	(0.055)	(0.065)
Number of Observations	963	963	963	963	902	902	790	790
Admin data weights		Yes		Yes		Yes		Yes

Notes: This table shows the effect of the PBD extension on job search behavior using the simple difference-in-differences design as laid out in Equation (1) and (not) using moments of the admin data as weights. Columns (1) and (2) show the corresponding non-weighted and weighted estimated treatment effect on the total number of job applications. In columns (3) and (4) as well as (5) and (6), we report the respective effects of the PBD extension on job search effort at the extensive and intensive margin. Last, columns (7) and (8) show non-weighted and weighted estimates of the PBD effect on individuals' log reservation wage. All regressions include the set of covariates as defined in Section 4.1. Standard errors are heteroscedasticity robust. Significance levels: $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table B.7: PBD and the Duration of UI Receipt and Nonemployment – Censored Outcome Windows

	Duration of UI Receipt			Duration of Nonemployment		
	Censored at			Censored at		
	3 Months	6 Months	12 Months	3 Months	6 Months	12 Months
	(1)	(2)	(3)	(4)	(5)	(6)
Diff-in-Diff	1.408***	5.249***	11.137***	1. 104***	4.398***	10.098***
	(0.495)	(1.225)	(2.474)	(0.420)	(1.123)	(2.648)
Number of Observations	38,587	38,587	38,587	38,587	38,587	38,587
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean (in days)	77.95	126.90	177.45	82.34	142.63	228.63

Notes: This table shows the effect of the PBD extension on the duration of UI receipt and nonemployment. The duration outcomes are winsorized at three months in columns (1) and (4), six months in columns (2) and (5), and twelve months in columns (3) and (6), respectively. All regressions include the set of covariates as defined in Section 5. Standard errors are heteroscedasticity robust. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.