Benefit Duration, Job Search Behavior and Re-Employment

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Impressum:

CESifo Working Papers

ISSN 2364-1428 (electronic version)

Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH

The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute

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Editor: Clemens Fuest

<https://www.cesifo.org/en/wp>

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Abstract

This paper studies how the potential duration of unemployment benefits affects individuals' job search behavior and re-employment outcomes. We exploit an unexpected reform of the German unemployment insurance scheme in 2008, which increased the potential benefit duration from 12 to 15 months for recipients of age 50 to 54. Based on detailed survey data and difference-in-differences techniques, we estimate that one additional month of benefits reduces the number of filed applications by around 10% on average over the first two months of unemployment. Treatment effects on the reservation wage are positive but statistically insignificant. In a complementary analysis, we use social security data to investigate how the reform affected re-employment outcomes. The difference-in-differences estimates yield an elasticity of 0.24 (0.1) additional months in unemployment (nonemployment) per additional month of potential benefits. A cautious back-of-the-envelope calculation reveals substantial returns to early search effort.

JEL-Codes: D830, I380, J640, J680.

Keywords: unemployment insurance, job search, re-employment outcomes, natural experiment.

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This version: March 2020

An initial version of this paper circulated under the title "Benefit Duration and Job Search Effort: Evidence from a Natural Experiment" as IZA Discussion Paper No. 10264. We would like to thank Patrick Arni, Pierre Cahuc, Stefano DellaVigna, Simon Jäger, Michael Krause, Max Löffler, Robert Mahlstedt, Ulrike Malmendier, Andreas Peichl, Gerard Pfann, Sebastian Sieglöcher, Johannes Schmieder, Konstantinos Tatsiramos, Simon Trenkle, Josef Zweimüller as well as seminar and conference participants at IZA Bonn, ZEW Mannheim, SOLE, NBER TAPES, EALE, and the University of Mannheim for many valuable comments and suggestions. Amelie Schiprowski acknowledges funding by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) through EXC 2126/1-390838866 and CRC TR 224 (Project B04). The authors are further grateful to the data services of the IDSC of IZA.

1. Introduction

Unemployment insurance (UI) serves as the key policy instrument to protect workers against adverse income shocks due to job loss. The optimal design of UI, however, remains fiercely debated. This is mainly due to the robust empirical finding that increases in the generosity of UI schemes prolong job seekers' time to re-employment (see, for example, the overview by Schmieder et al., 2012). Job search theory suggests a simple mechanism behind this empirical link: a more generous UI scheme increases the value of unemployment, which in turn induces job seekers to lower their search effort and increase their reservation wage. Direct evidence on the empirical relevance of this proposed link, however, remains scarce.

In this paper, we provide quasi-experimental evidence on the relationship between individuals' potential benefit duration (PBD), job search behavior and re-employment outcomes. We exploit a reform of the German UI benefit scheme on January 1 2008, which raised the PBD for both the inflow and the stock of eligible job seekers aged 50 to 54 from 12 to 15 months. The reform was the unexpected outcome 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 desired size of the welfare state. It was first announced on November 12 2007, i.e., just a few weeks before its implementation. Coincidentally, the reform 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 dataset and the reform-induced variation in the PBD to study how a more generous UI scheme affects individuals' search effort and reservation wage at the beginning of their respective unemployment spells.¹ In a second step, we use comprehensive social security data from the German Federal Employment Agency to analyze the effect of the PBD reform on the duration of un- and nonemployment, as well as re-employment wages.

We estimate the causal effect of the benefit extension on individual search effort and reservation wages by means of a cross-sectional difference-in-differences (DiD) design. We compare the 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 job seekers aged 45 to 49. The younger cohort was not subject to the PBD extension, but closely resembles the treatment group in terms of past employment patterns and other observable characteristics. Importantly, we find no evidence of changes in the composition of both cohorts after reform implementation, nor of differential pre-reform trends in outcomes.

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

We show that the PBD extension significantly reduced individual job search effort, as measured by the total number of filed applications. On average, one additional month of potential benefits lowered individuals' search intensity during the first two months of unemployment 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. We further find a positive, albeit statistically insignificant, effect on individuals' (log) reservation wage.

We complement this evidence by analyzing the reform's effects on re-employment outcomes. We again set up a DiD design, but now use social security data on a 4.66% random sample of all German UI entries over the years 2006 to 2008. This larger dataset allows us to explicitly account for one particularity of the reform which becomes relevant when studying effects on the duration of un- and nonemployment spells: the extension of benefits applied to both the inflow and the stock of benefit recipients. Hence, the PBD extension not only affected eligible job seekers entering UI after the reform, but also applied ex post to those job seekers who entered UI in 2007 and were still eligible for benefits upon reform implementation.² The DiD results reveal that the duration of unemployment increased by around 10 days on average for the 2007 entry cohort, relative to the fully untreated 2006 cohort. At the same time, their duration of nonemployment remained unaffected. It appears that the retroactive application of the reform merely raised UI coverage among already unemployed individuals, but did not distort their job search incentives in a meaningful way.

The picture looks different for individuals who entered UI in 2008 and were subject to the PBD extension upon entry. On average, the PBD extension increased the duration in unemployment (nonemployment) for this group by 22 (10) days relative to the fully untreated 2006 entry cohort. Phrased differently, we find that one additional month of PBD raised the duration of unemployment (nonemployment) by around 0.24 (0.1) months on average. These estimates are close to the ones reported by Schmieder et al. (2012), who use age discontinuities in the German UI scheme and data on the universe of unemployed job seekers between 1987 and 2004. We further investigate whether the more generous UI scheme affected individuals' re-employment wages. The estimated coefficient is negative, but statistically insignificant.

Last, we use the given set-up to cautiously assess the returns to search effort. To avoid assumptions about the proportionality of effort changes over the unemployment spell, we focus on the impact of less effort (less applications) on early exit from nonemployment. We estimate that the reduction

² Note that this feature of the reform does not affect the analysis on search behavior. Before and at the time of the interview, individuals in the control group did not know that they will be treated in the future. Hence, they chose search effort and the reservation wage while expecting a PBD of 12 months.

in search effort by 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. 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 the scarce empirical evidence on the reaction of job search behavior to changes in the benefit environment. While previous studies have shown a positive relationship between UI generosity and the duration of un- and nonemployment (e.g., Schmieder et al., 2012, for Germany), little is known about the mechanisms underlying this effect. 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 aggregate job search effort, both using business cycle-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 their 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 (on-line and off-line) search activities. Our evidence is also complementary to recent studies in the sufficient statistics literature (e.g., Kolsrud et al., 2018), which derive the moral hazard effect of UI from the effect of changes in benefits on unemployment duration.

Our study further relates to evidence on the determinants and consequences of search effort choices. Based on cross-sectional time-use data, Krueger and Mueller (2010) show that job search intensity – as measured by the daily amount of time spent on searching for re-employment – increases prior to benefit exhaustion. Marinescu and Skandalis (2019) corroborate this finding, using information from a French on-line search platform and within-individual variation in job search effort around benefit exhaustion. They find that search effort – as measured by the number of filed applications – increases significantly in the period before benefit exhaustion. Faberman and Kudlyak (2019) use high-frequency data on individuals' application activity from a U.S. job vacancy website and find a decline in search effort over the course of the unemployment spell. Last, Arni and Schiprowski (2019) use Swiss administrative data to show that requirement-induced effort changes have a meaningful impact on job finding. We add to these insights by providing evidence on the relationship between the PBD, initial search effort and re-employment outcomes. Our results show that even early search

effort is highly reactive to the duration of benefit payments, which endorses policies that directly target the early effort provision of job seekers.

Finally, the results provide new insights on the duration dependence of individual responses to PBD extensions. When studying the reform’s impact on individuals who experienced the reform *during* their unemployment spell, we find that only the duration of unemployment increased, while the duration of nonemployment remained unaffected. This cohort was subject to extended UI coverage, but did not remain nonemployed longer. In contrast, we find positive effects on the duration of both un- and nonemployment for those individuals treated at the *start* of their unemployment spell. This may suggest that the moral hazard effect of PBD extensions is particularly large at the start of unemployment. This interpretation relates to evidence by Kolsrud et al. (2018), who investigate duration-dependent kinks in UI replacement rates in Sweden. Their results also support the idea that the moral hazard costs of benefits are larger at the beginning of the unemployment spell.

The paper proceeds as follows. Section 2 offers a brief overview about the institutional characteristics of the German UI scheme and highlights the key features of the reform under study. The data are presented in Section 3. In Section 4, we study the effects of the BPD extension on job search behavior. We first provide details on the empirical strategy in Section 4.1, before presenting the results in Section 4.2. In Section 5, we document the corresponding effects on re-employment outcomes and relate these findings to the observed changes in job search behavior. 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.³ 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 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

³ Benefit payments are generally rescinded for up to twelve weeks if workers terminate their job themselves, which lowers the maximum benefit duration accordingly. In this study, we only focus on individuals with the regular maximum benefit duration, i.e., drop individuals who terminated employment themselves.

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 institutions⁴, 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. It 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 the growing tensions, leaders of both parties eventually negotiated over the set of disputed policies⁵ 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 possible time”⁶ 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).⁷

Two groups of job seekers were directly 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

⁴ See, for example, Krause and Uhlig (2012) and Launov and Wälde (2013) for evaluations of these reforms.

⁵ Among others, the parties also negotiated about the introduction of minimum wages in the postal sector.

⁶ Volker Kauder, the then-acting leader of the CDU/CSU parliamentary group in German parliament, as quoted in “Spiegel Online” on November 13, 2007: <http://www.spiegel.de/politik/deutschland/einigung-bei-arbeitslosengeld-beitrag-sinkt-auf-3-3-prozent-laenger-geld-fuer-aeltere-a-516982.html>, last accessed in March 2020.

⁷ 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.

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 after the reform (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.

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 May 2008.⁸ 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.⁹

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 slightly younger individuals of age 45-49. To this end, we restrict the estimation sample to job seekers of age 45 to 54, who all meet the eligibility criteria for the extended PBD. These criteria require 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-employment¹⁰ and those who voluntarily quit their job. The baseline sample includes 963 individuals.

The first wave of the survey provides comprehensive information about individuals’ job search behavior. 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

⁸ 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.

⁹ However, note that 95% of the respondents were interviewed 5-11 weeks after unemployment entry.

¹⁰ This relates to job seekers who got re-employed in the month of unemployment registration.

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' respective employment history and personal characteristics. Notably, information on past employment and wages stems 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 very precisely, which allows determining whether individuals are eligible for (extended) unemployment benefits. In addition, the survey part offers comprehensive information on individuals' nationality, level of education, and personality traits, which serve as controls in the regression analysis. Last, information on local labor market conditions are merged to the survey data. Appendix Table A.2 provides descriptive statistics for all outcome variables and controls.

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 our estimation sample, respectively. In Panel (A), we winsorize the number of applications at the 98% level. The full sample covers 12,389 respondents, the estimation sample 963. In Panel (B), we winsorize the variable at the bottom and top 2%. The full sample covers 8,292 respondents, the estimation sample 790. 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.

Figure 1 plots the distribution of the two main outcome variables for both the survey population and our estimation sample.¹¹ Panel (A) shows the respective 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 impact on treatment effects. In the baseline regressions, we winsorize the number of applications at the 98% level.

¹¹ The estimation sample accounts for around 8% of the survey population.

Panel (B) plots the corresponding two distributions of the (log) reservation wage. As for job search effort, we see considerable variation in stated reservation wages in both samples.¹² 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).

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 35 to 54.¹³ 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.

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, unemployment duration and re-employment outcomes.

In line with the survey analysis, we restrict the estimation sample to job seekers of age 45 to 54 who meet 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 drop all individuals for whom we cannot clearly assign eligibility with respect to age (i.e., age ≥ 50), which results in the under-representation of job seekers of age 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. In turn, February 2006 is 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. The estimation sample contains 59,076 individuals.

The main outcomes of interest are the duration of unemployment, i.e., the time of benefit receipt, and the duration of nonemployment, i.e., the time to re-employment.¹⁴ Additional outcomes are the

¹² To account for outliers, we winsorize the variable at the bottom and top 2% of the distribution in the baseline sample.

¹³ Average search effort is highest among individuals below the age of 35.

¹⁴ The maximum duration of unemployment amounts to 15 months by definition. We also top-code nonemployment spells at 15 months, which is the period of interest in this study.

probability to leave un- and nonemployment within three months, as well as (log) re-employment wages. Appendix Table A.3 contains descriptive statistics on these outcomes variables and covariates for the entire baseline sample, and split by treatment group status. The treatment group, which is of older age, stays un- and nonemployed longer on average. With respect to control variables, both groups look fairly similar.

4. PBD and Job Search Behavior

In this section, we study the effect of the PBD extension on two key variables of individuals' job search behavior: the number of filed applications (as a measure of effort) and the (log) reservation wage.

4.1. Empirical Design

We exploit the age-specific change in the PBD by means of a cross-sectional difference-in-differences (DiD) 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:

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

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, past employment and local labor market characteristics (see Figure 3 and Appendix Table A.2 for details). The error term is denoted by 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 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 already 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. Treatment effects slightly decrease 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 estimated by Equation (1) relies on two main assumptions, namely that: (i) the reform did not induce any changes in the composition of treatment and control group, and (ii) 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. 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).

Figure 3: Illustration of DiD 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 the equality of means t -test for each age cohort before and after treatment. See Appendix Table A.2 for more information on the set of covariates.

Second, we find no evidence of significant pre- versus post-reform differences in the composition of treatment and control group. Surveyed job seekers are very similar in terms of gender composition (Panel B), nationality (Panel C), educational attainment (Panel D), last wages prior to unemployment (Panel E), or labor market attachment (Panel F) before and after the reform.¹⁵ Simple t -tests for the

¹⁵ We measure job seekers’ labor market attachment via the time spent in regular employment within the last ten years

equality of means within groups before and after treatment confirm the visual evidence. 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 – shown at the bottom of each panel of Figure 3 – are small and statistically insignificant. We take this as further evidence against selection into treatment.

We draw similar conclusions when investigating potential differences in the local unemployment rate faced by the job seekers at the time of the interview (Panel G), or individuals’ internal locus of control (Panel I); a measure of job seekers’ personality traits, which have been shown to affect individual job search behavior (Caliendo et al., 2015). 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 some evidence on the plausibility of the parallel trend assumption, we expand the simple difference-in-differences strategy from Equation (1) to allow for varying treatment effects over the survey period. We estimate the following dynamic DiD specification:

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

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 covers the same amount of observations. 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 for 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).

prior to the observed unemployment spell.

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 laid out in Equations (1) and (2), respectively.

Graphical evidence. Standard models of job search theory 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 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 early search effort. The right figure of Panel (A) corroborates this interpretation. 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 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, there is no significant graphical evidence in favor of a reaction of (log) reservation wages 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 shows that patterns are qualitatively unaffected when not winsorizing the two outcome variables.

Simple DiD. 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 5.2-5.4 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 unemployment spell by around 10% on average.¹⁶ In Appendix Table B.1, we show that this finding holds true when using alternative measures of 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.¹⁷

The results in columns (3)-(6) of Table 1 further suggest that the observed reduction in 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 are negative but statistically insignificant; see columns (3) and (4). In contrast, effects at the intensive 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 a reduction in search effort by 4.9-5.4 applications on average.

Table 1 also corroborates the graphical evidence on (log) reservation wages. While point estimates in columns (7) and (8) are positive, they are imprecisely estimated and not statistically different from zero. However, we do not want to draw too strong conclusions from this particular result as it

¹⁶ 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.

¹⁷ 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 survey 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 made.

has been noted that the precise estimation of wage effects in response to UI generosity requires large samples (e.g., Nekeoi and Weber, 2017).

Table 1: PBD and Job Search Behavior – Baseline Results

	Number of Job Applications						Log Reservation Wage	
	Overall		Extensive Margin		Intensive Margin		(7)	(8)
	(1)	(2)	(3)	(4)	(5)	(6)		
Reform Dummy	-0.046 (1.645)	1.430 (1.730)	0.035 (0.030)	0.049 (0.031)	-0.576 (1.736)	0.919 (1.851)	-0.005 (0.040)	-0.006 (0.040)
Aged 50-54	4.014* (2.394)	0.405 (3.517)	0.027 (0.038)	0.077 (0.050)	3.873 (2.502)	-0.396 (3.695)	-0.102** (0.050)	-0.091 (0.065)
Treatment Effect	-5.245** (2.606)	-5.450** (2.590)	-0.047 (0.042)	-0.050 (0.041)	-4.885* (2.726)	-5.428** (2.715)	0.082 (0.054)	0.062 (0.052)
Number of Observations	963	963	963	963	902	902	790	790
Mean value	13.38	13.38	0.94	0.94	14.32	14.32	7.06	7.06
Controls	Yes		Yes		Yes		Yes	

Notes: This table shows the effect of the PBD extension on our 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 our set of additional controls. 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 the log reservation wage. Note that the number of observations is lower in columns (7) and (8) due to non-response. 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 role of outliers. One possible limitation of the presented 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, it is still considerably larger than the median of 10 applications. In Figure 5, we plot treatment effects for the two main outcome variables when sequentially discarding observations from the upper and, for the reservation wage, lower end of the respective distributions.

From Panel (A) of Figure 5, we infer that the estimated treatment effect on the number of job applications remains of similar size, but becomes much more precisely estimated when successively discarding observations from the upper end of the respective distribution. This suggests that outliers do not drive the observed treatment effect on search effort but rather work against statistical pre-

cision; also see the estimate when not winsorizing search effort at all. We find a similar, albeit less pronounced pattern for the estimated treatment effect on (log) reservation wages in Panel (B).

Figure 5: PBD and Job Search Behavior - The Role of Outliers

Notes: This figure shows the estimated effect of the PBD extension on the total number of job applications (Panel A) and the log reservation wage (Panel B) when subsequently omitting observations from the upper and — for reservation wages — lower end of the distribution. All estimates base on Equation (1).

Dynamic DiD. Figure 6 presents the 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. To this end, 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.

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 assumption. In addition to testing for common pre-trends, the extended specification allows analyzing treatment effect dynamics between January and July 2008. Overall, we observe that – despite some degree of imprecision due to small sample sizes – treatment effects are stable over the time window, which hints at a rather persistent change in job search effort within the observed time period.

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.

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.

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 Table 2 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 10% level; see column (3) of Table 2. 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 adjustment of reservation wages.

Next, we run a set of placebo estimations. We follow Rosenbaum (1987) and run 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.2 presents the 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 a control group.

In column (3) of both panels, we compare job seekers of age 30 to 34 to job seekers aged 35 to 39. Again, point estimates are insignificant and close to zero in all specification. We take these findings as a further piece of evidence that the observed changes in the search effort of individuals in the main sample can be indeed attributed to the PBD extension.

Table 2: 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	1.397 (1.714)	0.889 (1.381)	1.660 (1.846)	-0.004 (0.038)	0.029 (0.029)	-0.009 (0.040)
Aged 50-54	0.484 (3.443)	-0.259 (2.831)	-1.002 (3.794)	-0.086 (0.063)	-0.003 (0.054)	-0.075 (0.070)
Treatment Effect	-5.393** (2.547)	-4.242** (2.085)	-4.386* (2.644)	0.059 (0.050)	-0.015 (0.041)	0.071 (0.053)
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 our measures of 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 results. In Column (2) and (5), we add those job seekers who were interviewed between 11/12-12/31/2007. In column (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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Last, we test the robustness of the results when using alternative ways of drawing inference; see Appendix Table B.3. 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. Effect of the PBD Extension on Re-Employment Outcomes

This section complements the previous evidence by documenting how the PBD extension affected re-employment outcomes. To this end, we run a separate analysis based on social security data covering a substantially larger sample of job seekers (see Section 3 for details).

5.1. Empirical Design

We again study the reform effects through the lens of a DiD framework. However, we slightly adapt the empirical design. First, we assign treatment based on the date of UI entry.¹⁸ Second, we need to explicitly account for the retroactive application of the PBD extension. Recall that 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 the initially expected 12 months of benefits (see Section 2). Thereby, they could become treated *during* their unemployment spell. Consequently, only job seekers who entered UI in 2006 constitute a fully untreated comparison group.¹⁹

5.1.1. Dynamic DiD

To document how (varying) exposure to treatment relates to re-employment outcomes, we first estimate a dynamic DiD specification over the years 2006, 2007 and 2008.²⁰ The empirical specification is given as follows:

$$Y_i = \alpha + \sum_{k=1}^n \beta_k [\mathbf{1}(Age_i \geq 50) \times \mathbf{1}(Period_k)] + \sum_{k=1}^n \lambda_k \mathbf{1}(Period_k) + \gamma \mathbf{1}(Age_i \geq 50) + X_i' \rho + \varepsilon_i \quad (3)$$

Outcome Y_i refers to job seeker i 's un- and nonemployment duration as well as her probability of exiting un- and nonemployment within the first three months. The term $\sum_{k=1}^n Period_k$ 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. To account for the retroactive application of the reform under study, we set the baseline period to the second half-year of 2006. This is the last inflow period in which job seekers could not become treated ex post.²¹ The dummy variable $Age_i \geq 50$ equals one if individual i was of age 50 to 54 (rather than 45 to 49) at the time of UI entry. The coefficients of interest, β_k , measure average outcome difference-in-differences relative to the baseline period (the second half of 2006). The vector X_i relates to a set of individual characteristics. We flexibly control for the job seekers' age, socio-demographics and measures of their employment history (see Appendix Table A.3 for the corresponding summary statistics). Last, ε_i refers to the error term.

¹⁸ By construction, there is no interview date in the social security data.

¹⁹ 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 a more generous, age-dependent PBD.

²⁰ Unfortunately, the AED dataset does not cover entries beyond 2008.

²¹ With a PBD of 12 months, any job seeker of age 50 to 54 who entered UI in the second half of 2006 had exhausted her eligibility period by the end of 2007.

5.1.2. Simple DiD

We further estimate a simple DiD specification, in which we exclude all job seekers who registered as unemployed in 2007. By discarding those who could become eligible for the PBD extension during their unemployment spell, this set-up summarizes effects on the fully treated vs. fully untreated cohort. The specification reads as follows:

$$Y_i = \alpha + \beta [\mathbb{1}Post_i \times \mathbb{1}(Age_i \geq 50)] + \lambda (\mathbb{1}Post_i) + \gamma \mathbb{1}(Age_i \geq 50) + X_i' \rho + \varepsilon_i \quad (4)$$

Term $\mathbb{1}(Post_i)$ equals one if job seeker i enters UI on or after January 1 2008. Term $\mathbb{1}(Age_i \geq 50)$ indicates whether a job seeker is aged 50 to 54. The coefficient of interest, β , measures the effect of the PBD extension on the respective outcome. As before, covariates are summarized by vector X_i and refer to a set of individual characteristics (see Appendix Table A.3 for details).

5.2. Results

5.2.1. Dynamic DiD

We first present estimates of the dynamic DiD specification to investigate how differential exposure to the PBD extension affected the average duration of un- and nonemployment spells, as well as the probability of leaving un-/nonemployment within three months.

Duration of un- and nonemployment. Panels (A) and (B) of Figure 7 plot the estimated treatment effects on un- and nonemployment duration. Recall that all individuals of age 50 to 54 who entered UI in 2006 under the old policy regime (with a PBD of twelve months) had exhausted their benefits by December 2007. They were fully unaffected by the extension of benefits. In contrast, job seekers of age 50 to 54, who entered UI in 2007 and were still unemployed by December 31 2007, were affected by the reform. Their PBD was prolonged from 12 to 15 months ex post. Individuals of age 50 to 54 who entered unemployment in 2008 were fully treated, facing a PBD of 15 months upon registration. We expect to see the strongest treatment effects for this entry cohort.

The results displayed in Panels (A) and (B) of Figure 7 are in line with this gradual treatment exposure. First, we observe that the PBD extension had no ‘effect’ on job seekers’ average duration in un- and nonemployment in the first half of 2006. We take these flat pre-trends as indicative support of the common trend assumption.²² In contrast, when focusing on individuals entering UI in 2007,

²² Recall that we cannot use a longer period to study pre-trends because of a different age-specific PBD before 2006.

we find positive and statistically significant treatment effects. On average, the retroactive extension of the PBD by three months led to an increase in the duration of unemployment by around 10 days (Panel A). Notably, treatment effects are of comparable size in the first and second half year of 2007. At first glance, this may be surprising because individuals who entered UI in the second half of 2007 got treated earlier during their respective unemployment spell. However, this finding may also suggest that the observed effects for the 2007 entry cohort are mostly driven by individuals who were close to benefit exhaustion. In line with this intuition, we observe no effect of the PBD extension on the average nonemployment duration for individuals registering as unemployed in 2007 (Panel B). This suggests that the retroactive treatment mostly affected individuals who would have stayed nonemployed longer than 15 months irrespective of the benefit extension. The retroactive implementation of the reform application increased their UI coverage – without any adverse labor supply effects.

Figure 7: PBD and the Duration of Un-/Nonemployment - Effects over Time

Notes: This figure plots the dynamic DiD estimates of β_k as defined in Equation (3). Outcomes are the duration of unemployment (Panel A) and nonemployment (Panel B). Job seekers in the treatment group (aged ≥ 50) who registered in 2006 were fully unaffected by the PBD extension. Those who registered in 2007 could get treated ex-post if they were still unemployed by December 31 2007. Those who registered in 2008 were treated upon registration. The number of observations is 59,076.

Last, we find the strongest treatment effects for job seekers who entered UI in 2008, i.e., were treated upon registration. As displayed in Panel (A) of Figure 7, treated individuals remained unemployed around 18 (second half of 2008) to 23 days (first half of 2008) longer than individuals in the baseline period. These effects correspond to around 9 to 11% relative to the 2006 mean unemployment duration in the treatment group. We also find statistically significant effects on the duration of nonemployment for this cohort. On average, the PBD extension increased the duration of nonemployment by around 7 (second half of 2008) to 12 (first half of 2008) days, or 3 to 4% relative to the 2006 treatment group mean.

Early exit probability. We next study the effect of the PBD extension on the probability of leaving un-/nonemployment within three months. This specification partly serves as a falsification exercise, because the retroactive application of the PBD extension should have had no effect on the probability of early exit for most of the eligible job seekers who entered UI in 2007. This is because most of these job seekers chose search behavior while expecting a PBD of twelve months during the first three months of their unemployment spell.²³ Panel (A) and (B) of Figure 8 show that this is indeed the case. We find no significant treatment effect on the probability of early exit into both un- and nonemployment for job seekers registering as unemployed in 2007. In contrast, effects on the probability of early exit materialize for job seekers entering UI in 2008. The probability of exiting unemployment within the first three months after UI registration decreases by around two to four percentage points, or 8 to 16 percent relative to the treatment group mean in 2006. The corresponding decrease in the probability of exit from nonemployment amounts to around 1.5 to 3 percentage points, or 10 to 20 percent relative to the treatment group mean in 2006.

Figure 8: PBD and the Probability of Early Un-/Nonemployment Exit - Effects over Time

Notes: This figure plots the dynamic DiD estimates of β_k as defined in Equation (3). Outcomes are the duration of unemployment (Panel A) and nonemployment (Panel B). Job seekers in the treatment group (aged ≥ 50) who registered in 2006 were fully unaffected by the PBD extension. Those who registered in 2007 could get treated ex-post if they were still unemployed by December 31 2007. Those who registered in 2008 were treated upon registration. The number of observations is 59,076.

5.2.2. Simple DiD (excluding entries in 2007)

In a second step, we present the results from the simple difference-in-differences design as laid out in Equation (4). This specification discards individuals who entered UI in 2007. Thereby, it summarizes the effect of the PBD extension on fully treated individuals. Table 3 presents the corresponding effects.

From columns (1) and (2), we first infer that the PBD extension increased the unemployment

²³ In fact, all job seekers entering UI before October 1 2007 were not treated when it comes to exit within three months.

duration of treated individuals by around 21 to 22 days on average, both unconditional and conditional on individual controls (X_i). The effect corresponds to an elasticity of around 0.24 months per additional month of PBD. Columns (3) and (4) further show that the PBD extension also raised the duration in nonemployment, by around 10 days on average. This result in turn implies an elasticity of around 0.1 months for one additional month of PBD.²⁴ Both elasticities are close to the findings by Schmieder et al. (2012), who use data on the universe of German job seekers over the period 1987-2004 and exploit the UI scheme's age discontinuities in the PBD by means of a regression discontinuity design. They show that the elasticity of nonemployment duration is quite stable over their sampling period and not subject to business cycle conditions (around 0.1, i.e., close to our estimate). In contrast, their estimates of the elasticity of unemployment vary considerably over time, from around 0.2 during times of low unemployment to 0.35 during times of high unemployment. Our estimates base on data from 2006 and 2008. In 2008, unemployment was rather low (around 8%) compared to the period covered by Schmieder et al. (2012). This may explain why our elasticity is at the lower end of the estimates obtained in their study.

Table 3: PBD, Duration Outcomes and Re-employment Wages: Diff-in-Diff 2006 vs. 2008

	UE Duration		NE Duration		Log Re-employment Wage	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-25.296*** (1.567)	-20.592*** (1.534)	-17.107*** (2.206)	-11.345*** (2.159)	0.039*** (0.007)	0.032*** (0.005)
Age 50-54	19.081*** (1.960)	-2.675 (3.925)	24.019*** (2.586)	12.461** (5.058)	-0.011 (0.008)	0.012 (0.013)
DiD	20.547*** (2.693)	22.024*** (2.596)	8.762** (3.493)	10.310*** (3.371)	-0.012 (0.011)	-0.011 (0.009)
Covariates	No	Yes	No	Yes	No	Yes
Outcome Mean	181.07	181.07	262.03	262.03	7.29	7.29
N	38,585	38,585	38,585	38,585	29,991	29,991

Notes: This table shows the effect of the PBD extension on re-employment outcomes using the simple difference-in-differences design as laid out in Equation (4). It only includes job seekers who registered as unemployed in either 2006 or 2008, thereby excluding partially treated individuals. The outcomes are the duration of unemployment in columns (1) and (2), the duration of nonemployment in columns (3) and (4), and the log re-employment wage in columns (6) and (7). Controls include age, gender, work history, log last wage and dummies for German nationality and residence in East Germany (see Table A.3 for summary statistics). Standard errors are heteroscedasticity robust. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

²⁴ Note that the large and significant 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 on average shorter un- and nonemployment spells post reform. Moreover, older job seekers take longer to find a job, which is in line with the positive age group dummy.

Finally, columns (5) and (6) report treatment effects on the (log) re-employment wage. Point estimates are negative — in line with the estimates in Schmieder et al. (2016) — but statistically insignificant. However, previous studies, such as Schmieder et al. (2016) and Nekeoi and Weber (2017), noted that very large samples are necessary to detect significant effects on re-employment wages in response to changes in UI generosity. Therefore, we do not want to draw too strong conclusions from our comparatively small sample.

The Returns to Search Effort Given the setting at hand, it is not trivial to relate the effects of the PBD extension on job search behavior to its effects on re-employment outcomes. This is due to two main 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 interview could 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 the unemployment spell. Second, we measure search effort around 5-16 weeks after registration. Extrapolating from the observed effects at the beginning of job seekers' unemployment spell to changes in search effort over the entire spell would require rather unrealistic assumptions.

Instead, we propose to tentatively relate the observed reductions in search behavior (Table 1) to the observed change in early nonemployment exit (Figure 8, Panel B). If we assume that the observed effect on early nonemployment exit in the first half of 2008 by around 20 percent was merely due to the reduction in search effort (-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, note that 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, several strong assumptions, such as a constant application quality irrespective of the number of filed applications and the irrelevance of reservation wages, are necessary to conclude that the estimated effect on exit is the direct result of a change in search effort.

6. Conclusion

This paper estimates the effect of UI generosity on individuals' job search behavior and re-employment outcomes in one joint framework. 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. At the same time, the PBD remained constant for slightly younger job seekers. Using survey data and a difference-in-differences approach, we find that the increase in the

PBD led to a significant reduction in individuals' job search effort. On average, treated job seekers filed around 30% less applications within the first two months of their respective unemployment spell. The estimated effect on reservation wages is positive but insignificant. Using social security data of a much larger scale, we show that the observed reductions in search effort are accompanied by increases in the average duration of un- and nonemployment. We estimate an elasticity of around 0.24 (0.1) additional months in unemployment (nonemployment) per additional month of PBD; estimates close to Schmieder et al. (2012). Last, we find no evidence that the more generous UI scheme had a positive impact on re-employment wages. The obtained point estimate is negative but imprecisely estimated.

Overall, the paper adds to a scarce empirical literature on the impact of UI generosity on job search behavior. To the best of our knowledge, we are the first to analyze the effect of UI generosity on job search effort at the individual level. We thus add to the evidence by Baker and Fradkin (2017) and Marinescu (2017), who both base their analyses on business cycle-induced variation in the PBD duration across U.S. states and aggregate measures of on-line job search behavior. We are also able to cautiously relate the observed reductions in search effort to the effects of un- and nonemployment duration. The findings suggest that the returns to search effort can be substantial. As this statement is subject to rather strong empirical assumptions, we consider a more detailed analysis of the returns to search effort as an important topic for future research.

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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) & Age of eligible person .. or above	12	16	20	24	30	36	
					55	55	
Potential Benefit Duration (PBD)	6	8	10	12	15	18	
After January 1 2008							
Period of UI contribution (months) & Age of eligible person .. or above	12	16	20	24	30	36	48
					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 Cohorts

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 for Baseline Sample (Survey Data)

	Mean	Std Deviation	Minimum	Maximum	Observations
A. Outcomes					
Total Number of Applications	13.378	14.73	0.00	70.00	963
Number of Applications (Intensive Margin)	14.323	14.95	1.00	72.00	902
Job Search (no/yes)	0.937	0.24	0.00	1.00	963
Log Number of Applications	2.200	0.99	0.00	4.28	902
Applications per Week	1.891	2.11	0.00	10.00	963
(Log) Reservation Wage	7.053	0.43	4.61	8.16	789
B. Controls					
Age	49.156	2.84	45.00	54.00	963
Male (0/1)	0.550	0.50	0.00	1.00	963
Born in Germany (0/1)	0.893	0.31	0.00	1.00	963
Level of Education	3.645	1.43	0.00	7.00	963
Last Wage before Unemployment	2,053.489	1,002.67	420.00	4,470.00	963
Years in Employment (last 10yrs)	8.149	1.89	2.50	10.00	963
High Local Unemployment Rate (0/1)	0.390	0.49	0.00	1.00	963
Weeks b/w Unemployment Entry and Interview	7.298	1.97	5.00	16.00	963
Internal Locus of Control Index	5.886	0.95	1.67	7.00	963

Notes: This table presents descriptive statistics for the outcome and control variables used in Section 4 Source: IZA Evaluation Dataset Survey.

Table A.3: Descriptive Statistics for Baseline Sample (Admin Data)

Variable	All			Treatment Group			Control Group		
	Mean	SD	Obs	Mean	Sd	Obs	Mean	SD	Obs
A. Outcomes									
UE Duration	180.001	127.88	59,076	198.473	140.40	23,817	167.523	117.05	35,259
NE Duration	258.876	167.75	59,076	275.627	168.75	23,817	247.561	166.12	35,259
P (Exit from UE w/in 3 Mon)	0.318	0.47	59,076	0.291	0.45	23,817	0.335	0.47	35,259
P (Exit from NE w/in 3 Mon)	0.216	0.41	59,076	0.192	0.39	23,817	0.233	0.42	35,259
Log Re-Employment Wage	7.292	0.47	46,197	7.281	0.46	17,459	7.299	0.47	28,738
B. Controls									
Days Employed, Prev 5 Yrs	1502.017	255.68	59,076	1506.428	254.96	23,817	1499.038	256.12	35,259
Days Employed, Prev 2 Yrs	593.768	107.83	59,076	589.603	109.32	23,817	596.581	106.72	35,259
Female (0/1)	0.462	0.50	59,076	0.473	0.50	23,817	0.455	0.50	35,259
Age	49.257	3.04	59,076	52.687	1.02	23,817	46.939	1.21	35,259
German (0/1)	0.930	0.25	59,076	0.933	0.25	23,817	0.928	0.26	35,259
East (0/1)	0.325	0.47	59,076	0.337	0.47	23,817	0.317	0.47	35,259
Log Previous Wage	7.353	0.54	59,076	7.342	0.56	23,817	7.360	0.52	35,259

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

Appendix B Additional Results

Figure B.1: Changes in Search Effort and Reservation without Winsorizing

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 cohorts. 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.

Table B.1: PBD and Job Search Effort – Alternative Specifications

	Log Number of Applications		Applications per Week		Application per Week in NE	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform Dummy	-0.002 (0.117)	0.135 (0.129)	0.353 (0.223)	0.186 (0.235)	0.392 (0.251)	0.388 (0.253)
Aged 50-54	0.306** (0.154)	0.176 (0.231)	0.497 (0.311)	0.050 (0.480)	0.427 (0.330)	0.509 (0.430)
Treatment Effect	-0.373** (0.170)	-0.432** (0.169)	-0.726** (0.346)	-0.655* (0.347)	-0.769** (0.368)	-0.732** (0.370)
Number of Observations	902	902	963	963	963	963
Mean value	2.20	2.20	1.89	1.89	2.02	2.02
Controls	Yes		Yes		Yes	

Notes: This table shows the effect of the PBD extension on alternative measures of job search effort 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 additional controls. In columns (3)-(6), we account for the varying length between individuals' unemployment registration and their survey interview. In columns (3) and (4), the outcome is the number of filed applications per week. In columns (5)-(6), the outcome is the number of applications per week in nonemployment. 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 – Pseudo Difference-in-Differences Regressions

	Treatment:50-54, Non-Eligible Control:45-49, Non-Eligible	Treatment:45-49 Control:40-44	Treatment:40-44 Control:35-39
	(1)	(2)	(3)
A. Number of Applications			
(Pseudo-)Treatment Effect	0.108 (2.972)	1.423 (2.208)	-0.863 (1.896)
Number of Observations	810	1,078	1,016
B. (Log) Reservation Wage			
(Pseudo-)Treatment Effect	-0.003 (0.083)	-0.032 (0.052)	0.011 (0.046)
Number of Observations	603	873	829
Controls	Yes	Yes	Yes

Notes: This table shows the effect of pseudo difference-in-difference regressions using two groups of job seekers not affected by the reform, and assigning one to be pseudo-treated. In column (1), we 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 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 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.3: Robustness Checks – Inference

	Number of Job Applications			Log Reservation Wage
	Overall	Extensive Margin	Intensive Margin	
	(1)	(2)	(3)	(4)
Baseline Estimate	-5.450	-0.050	-5.428	0.062
Heteroskedasticity-consistent p-values	[0.036]	[0.228]	[0.046]	[0.236]
Jackknife-robust p-values	[0.040]	[0.241]	[0.051]	[0.247]
Wild-Cluster Bootstrap-t p-values	[0.037]	[0.226]	[0.053]	[0.233]
Randomization Inference p-values	[0.014]	[0.120]	[0.022]	[0.228]

Note: This table presents robustness checks on inference for the baseline results as presented in Table 1. In the first line, we re-produce the baseline treatment effects along with the corresponding p -values when using heteroskedasticity-consistent standard errors. Below, we further provide p -values when using alternative ways of drawing inference: (i) jackknife standard errors, (ii) the wild cluster percentile bootstrap test (clustering at job seekers' age), and (iii) randomization inference in the spirit of Fouka and Voth (2016).