# Benefit Duration and Job Search Effort: Evidence from a Natural Experiment[*]

ANDREAS LICHTER

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## Abstract

Findings of prolonged nonemployment spells due to more generous unemployment insurance (UI) schemes are commonly interpreted as an indication of reduced job search effort and UI-induced moral hazard. This paper exploits quasi-experimental variation in the potential benefit duration for one particular age group of workers in Germany paired with individual-level data on job search behavior to directly investigate the assumed relationship. The results of this study provide substantial support for strategic job search behavior in response to the generosity of the unemployment insurance scheme: extension of the potential benefit duration cause job search effort to significantly decrease, lowering the number of filed applications and the probability of applying for a job that requires moving. The results further suggests that the observed responses in job search effort are indeed due to moral hazard behavior.

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# 1    Introduction

A central challenge of unemployment insurance (UI) schemes is to allow unemployed individuals to actively search for suitable reemployment opportunities by partly compensating for income losses while at the same time repressing the unintended incentives to lower search intensity. Disincentive effects of UI systems, triggered by both the level of benefits as well as the potential benefit duration (PBD), have been, however, well identified by empirical research. In a nutshell, extensions of the PBD have been shown to significantly extend individuals' nonemployment duration, irrespective of personal characteristics or institutional regulations of the labor market (see, for example, Katz and Meyer, 1990; Card and Levine, 2000; Lalive et al., 2006; Van Ours and Vodopivec, 2006; Chetty, 2008; Schmieder et al., 2012, 2015).[1]

While standard job search theory shows that increases in the duration of nonemployment spells due to the extension of the PBD can be attributed to lower search effort and/or higher reservation wages, direct empirical evidence regarding the importance of reduced job search effort in contributing to this aggregate effect is sparse. Absent direct evidence, findings of prolonged spells of nonemployment are rather commonly interpreted as suggestive evidence of reduced search effort and the presence of moral hazard, although Chetty (2008) shows that liquidity effects must also be accounted for. To date, only the two recent studies by Baker and Fradkin (2015) and Marinescu (2015) provide empirical evidence in favor of the proposed mechanism by showing that U.S. state-level extension of the PBD caused significant reductions in aggregate Internet job search during the time of the recent recession.

The present paper adds to this evidence by using quasi-experimental variation in the PBD for one specific age group of the unemployed in conjunction with detailed, direct information on individuals' search effort and reservation wage choices to provide causal evidence of the effect of benefit duration on job search behavior. Variation in the PBD comes from an unexpected and rapidly implemented policy

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[1] Card et al. (2007) show that the *extent* of the observed spike in exit rates prior to the expiration of benefits significantly depends on the measurement of individuals' unemployment spells: reemployment hazards increase significantly less than unemployment exit rates. Given that unemployment registration is not mandatory in many countries after benefit exhaustion, spikes in unemployment exit rates may hence overstate the extent of a UI-induced moral hazard.

change in Germany in late 2007. The new legislation was motivated by concerns of social injustice and took place during times of stable-macro-economics conditions. On December 11, 2007, only two months after the initial reform proposal, the then acting coalition of the Christian Democrats (CDU) and Social Democrats (SPD) issued a law that enabled the extension of the PBD for eligible workers aged 50 to 54 by twelve weeks (from 12 to 15 months), while the PBD for younger workers remained unaffected.[2]

Using data from the IZA Evaluation Dataset Survey, which covers a large sample of individuals registering as unemployed at the German Federal Employment Agency between June 2007 and May 2008, I exploit this policy reform to investigate the effects of the PBD on job search behavior. Using unemployed individuals aged 45 to 49, who were not affected by the reform, as a control group allows for applying simple difference-in-differences techniques. Importantly, the swiftness of the political process and uncertainty about the design and scope of the reform until its public announcement by December 11, 2007 limit the scope of adaptive behavior. Moreover, the reform's detachedness from actual labor market conditions allows comparing the job search behavior of the two groups prior to and after the reform net of any endogenous policy bias.

The results of this study show that unemployed individuals entitled to an additional twelve weeks of unemployment benefits exerted substantially lower levels of job search effort at the beginning of the unemployment spell compared to their untreated counterparts: they filed less job applications and were less likely to apply for jobs in distant areas. The effects prove robust to the inclusion of a variety of personal and regional control variables and are of significant magnitude. The extension of the PBD by three months caused job applications to decrease by around 40% of a standard deviation, or around 1.5 applications per week. In contrast, the increase in the PBD had no effect on reservation wages, which is counterintuitive to theory but in line with recent empirical evidence demonstrating limited respon-

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[2] As detailed below, workers were subject to the reform in case having had contributed to UI for at least 12 months within the last two years (eligibility constraint) and for 30 months within the last five years. Note that the reform also extended the PBD for eligible workers aged 58 and above. Given that this study bases on data covering unemployed individuals aged 16 to 54 only, the effects of this change are, however, not investigated.

siveness of reservation wages with respect to changes in UI parameters (see, among others, Krueger and Mueller, 2014; Schmieder et al., 2015).

Exploiting the rich information of the dataset and using standard difference-in-differences techniques, I show that results are neither confounded by early adaption to the reform or strategic firm behavior nor driven by unobservable characteristics. The results further suggest that reductions in job search effort are mostly due to moral hazard behavior. Treatment effects are significantly stronger in regions with low local unemployment rates, i.e., areas where costs of leisure are significantly smaller. Moreover, individuals seem to postpone their job search effort in response to the extension of the PBD. Treatment effects are strongest at the very beginning of the unemployment spell but become smaller and insignificant over the course of unemployment.

Overall, the study offers considerable evidence for UI-induced moral hazard and strategic search behavior. Unemployed individuals respond to more generous UI by significantly reducing job search effort. These findings relate to early work by Barron and Mellow (1979), who report a negative relationship between UI payments and the time devoted to job search. Moreover, the results correspond to less direct evidence of moral hazard by Arni and Schiprowski (2015), who show that externally imposed changes in search effort affect job seekers' outcomes, and Black et al. (2003), who demonstrate that individuals leave unemployment upon receiving notice of required participation in reemployment services, i.e., in case costs of unemployment increase.

The paper proceeds as follows. Section 2 provides the theoretical foundation of this study by highlighting expected changes in job search behavior in response to an extension of the PBD. Section 3 offers a short overview about the key institutional characteristics of the German labor market and highlights the key features of the reform of interest. Information on the dataset are presented in Section 4, Section 5 provides the empirical model and details the underlying identification strategy. The results of this analysis are presented in Section 6, while Section 7 concludes.

# 2 Job search theory

According to the partial-equilibrium models of job search, increases in the PBD should lower job search effort and increase reservation wages. The theoretical framework by Schmieder et al. (2015) demonstrates the expected effects in a discrete-time setting.

Risk-neutral workers are assumed to become unemployed in period $t = 0$ and to maximize the present discounted value of income. They receive benefits $b_t$ and choose search effort $\lambda_t$, which is normalized to reflect the probability of receiving a job offer, in each period. Effort choices generate search costs $\psi(\lambda_t)$, which are assumed to be an increasing, convex and twice differentiable function of the search effort applied. Constant UI benefits $b$ are limited to $P$ periods and replaced by an indefinite second tier payment $\underline{b}$ thereafter ($\underline{b} < b$), generating non-stationarity in the spirit of Van den Berg (1990).

Jobs offer a wage $w_t^*$ that is drawn from a distribution with cumulative distribution function $F(w_t^*; \mu_t)$, assumed to vary over the spell of nonemployment $t$; for example, due to stigma effects. For simplicity, it is assumed that the distribution can be summarized by its mean in period $t$: $\mu_t$. In this case, $w_t^* = \mu_t + u_t$, where $E[u_t | t] = 0$, such that $u_t$ reflect random draws from the distribution. If a job is accepted, workers start at the beginning of the next period and are assumed to indefinitely stay with their new job.[3] Thus, the value of being employed, $V^e$, satisfies: $V^e(w^*) = \frac{1}{\rho} w^*$, with $\rho$ indicating the common subjective discount rate.

Given that $V^e(w^*)$ increases with $w^*$, the optimal search strategy of a job seeker thus comprises choosing effort to generate contact and specifying a reservation wage ($\phi_t$) in each period such that all wage offers $w^* \geq \phi_t$ are accepted. The corresponding Bellman equation is then given as follows:

$$V_t^u = b_t + \max_{\lambda_t} \left[ -\psi(\lambda_t) + \frac{1}{1+\rho} V_{t+1}^u + \frac{\lambda_t}{1+\rho} \int_{\phi_t}^{\infty} \left( V^e(w^*) - V_{t+1}^u \right) dF_t(w^*) \right].$$

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[3] Van den Berg (1990) acknowledges potential criticism regarding this assumption as rejecting a job offer may be suboptimal to accepting it and quitting immediately thereafter, given that the latter case may result in a new spell of unemployment and thus extended benefits. However, given legal boundaries prohibiting or punishing such behavior in reality, the validity of this assumption seems justifiable.

The environment is assumed to become stationary for $t \geq T$: $b_t = \underline{b}$ and $F_t(w^*) = F_T(w^*)$. This in turn implies that the optimal search strategy is *constant* for $t \geq T$. Using that $V_t^u = V_T^u \; \forall \; t \geq T$, $\phi_t = \rho V_t^u$ holds true in stationarity. The optimal reservation wage can then be deduced from the Bellman equation:

$$\frac{\phi_T}{\rho} = b_T - \psi(\lambda_T) + \frac{1}{1+\rho}\frac{\phi_T}{\rho} + \frac{\lambda_T}{1+\rho} \int_{\phi_T}^{\infty} \left(\frac{1}{\rho}w^* - \frac{\phi_T}{\rho}\right) dF_T(w^*)$$

$$\phi_T = (1+\rho)(b_T - \psi(\lambda_T)) + \frac{\lambda_T}{\rho} \int_{\phi_T}^{\infty} \left(w^* - \phi_T\right) dF_T(w^*). \tag{1}$$

Optimal search intensity in stationarity is then obtained by differentiating equation (1) with respect to $\lambda_T$, yielding:

$$\psi'(\lambda_T)(1+\rho)\rho - \int_{\phi_T}^{\infty} \left(w^* - \phi_T\right) dF_T(w^*) = 0. \tag{2}$$

In the non-stationary segment $(t < T)$, it in turn holds true that $\phi_t = \rho V_{t+1}^u$. Knowledge about $\phi_t$ and $\lambda_t$ in period $t$, with the initial conditions resulting from equations (1) and (2) in $t = T$, allows derivation of the job seeker's optimal strategy in non-stationarity for period $t - 1$:

$$\frac{\phi_{t-1}}{\rho} = b_t - \psi(\lambda_t) + \frac{1}{1+\rho}\frac{\phi_t}{\rho} + \frac{\lambda_t}{1+\rho} \int_{\phi_t}^{\infty} \left(\frac{1}{\rho}w^* - \frac{\phi_t}{\rho}\right) dF_t(w^*)$$

$$(1+\rho)\phi_{t-1} = (1+\rho)\rho(b_t - \psi(\lambda_t)) + \phi_t + \lambda_t \int_{\phi_t}^{\infty} (w^* - \phi_t) \, dF_t(w^*). \tag{3}$$

Optimal search effort in period $t-1$ can then be deduced by differentiating equation (3) with respect to $\lambda_{t-1}$, which yields:

$$\rho(1+\rho)\psi'(\lambda_{t-1}) - \int_{\phi_{t-1}}^{\infty} (w^* - \phi_{t-1}) \, dF_t(w^*) = 0. \tag{4}$$

Based on equation (3), it can then be shown that reservation wages increase in response to an extension of the PBD,

$$\frac{d\phi_t}{dP} = \frac{dV_{t+1}^u}{dP}\rho > 0, \tag{5}$$

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in case there is some probability of remaining unemployed after the exhaustion of the PBD, in which case an extension of the PBD will increase the value of remaining unemployed in each period $t \leq P$: $\frac{dV_{t+1}^u}{dP} > 0$.

Using that

$$(1 + \rho)\rho\psi'(\lambda_t) - \int_{\phi_t}^{\infty} (w^* - \rho V_{t+1}^u)\, dF_t(w^*) = 0, [4]$$

it further follows that job search effort decreases in response to an increase in the PBD in the case where $\frac{dV_{t+1}^u}{dP} > 0 \; \forall \; t \leq P$:

$$(1 + \rho)\rho\psi''(\lambda_t)\frac{d\lambda_t}{dP} = -\rho\frac{dV_{t+1}^u}{dP}\big(1 - F_t(\phi_t)\big)$$

$$\frac{d\lambda_t}{dP} = -\frac{dV_{t+1}^u}{dP}\frac{1 - F_t(\phi_t)}{(1 + \rho)\psi''(\lambda_t)} < 0. \tag{6}$$

According to the model, an extension of the PBD as given in the empirical setting of this paper is thus expected to lower job search effort and to increase reservation wages of the individuals concerned. In the empirical part of this paper, these two hypotheses, i.e., relations (5) and (6), are tested.

# 3   The institutional setting

In Germany, all employees subject to social security contributions are covered by UI and are entitled to receive unemployment benefits if having had contributed to UI for at least twelve months within the last two years preceding their job loss. The duration of benefits is subject to the number of months employed within a given time frame and increases with age. Monthly benefits amount to 60% (67% for recipients with children) of the last net wage, which is capped at the upper ceiling of the social security contributions, and payments are generally rescinded for up to twelve weeks if workers terminate their job themselves, which lowers the maximum benefit duration accordingly. Each recipient of unemployment benefits is further obliged to actively search for a job and to be at the Employment Service's disposal, while

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[4] Note that this expression can be derived by differentiating equation (3) with respect to $\lambda_t$ and is equal to equation (4) in period $t$.

failure to comply with these requirements may result in benefit cuts.[5] Individuals who are not entitled for or exhaust their unemployment benefits may receive welfare benefits, which are granted for an unlimited period and designed to assure living at subsistence level.

**UI benefit extension for older workers in 2007** The extension of the PBD for older workers was the result of an unexpected policy reform under the grand coalition of Christian Democrats (CDU) and Social Democrats (SPD) in late 2007. The remarkably rapid implementation of the reform proposal, uncertainty about the design and scope of the reform until its public announcement, and its detachedness from the business cycle allows for the investigation of the effects of the PBD on job search effort and reservation wages in absence of (the common challenges of) avoidance behavior and endogenous policy bias. In the following, I detail the key features of this reform.

Since their implementation in the early 2000s, the Social Democrats were heavily divided about the evaluation of their large, structural reforms that had made the German labor market much more flexible (*Hartz IV, Agenda 2010*, among others) but had marked a significant shift in the party's policy agenda, resulting in electoral defeats and a challenge to the identity of the party. On October 1, 2007, the then acting party leader of the Social Democrats, Kurt Beck, marked the party's public turn from its (more) liberal policy by calling for an extension of the PBD for older workers. The reform proposal was motivated on the grounds of social injustice concerns – long periods of UI contributions were ought to be rewarded by extended PBD[6] – and was made during times of stable macro-economic conditions (see Figure A.1 in the Appendix).

The initial proposal was met with considerable skepticism, from politicians in both the Christian Democratic and the Social Democratic parties. Disagreement about the proposal, and hence uncertainty about the implementation of the suggested reform, lasted for several weeks and raised rumors about the collapse of the

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[5] Note that there is no general minimum number of applications required by law.

[6] The reform proposal followed claims of the German Trade Union Confederation (*DGB*), who initially suggested the extension of the PBD for all workers aged 45 and above to up to 24 months.

acting coalition. To ease the growing tensions[7], both parties negotiated over pending disputes in a coalition meeting on the night of November 12, and a general decision in favor of an extension of the PBD was announced by the following morning. However, details about the *actual* changes of the UI scheme did not become public until December 11, 2007, when the corresponding law was issued to parliament.

Ultimately, the reform affected those unemployed individuals aged 50 or above who fulfilled the given entitlement criteria. PBD for workers aged 50 to 54 was extended by twelve weeks (from 12 to 15 months) if having had contributed to UI for at least 12 months within the last two years (eligibility constraint) and for 30 months within the last five years.[8] Likewise, UI benefit duration was extended from 18 to 24 months for all workers aged 58 or above if they had fulfilled the eligibility constraint and had contributed to UI for at least four out of the last five years. The reform also contained a transitional agreement which extended the PBD for those respective workers who were unemployed prior to the reform, fulfilled the entitlement criteria highlighted above and whose eligibility period was not exhausted by December 31, 2007.[9] The reform was passed by parliament on January 26, 2008 and retroactively extended back to January 1, 2008. Table A.1 in the Appendix outlines the relationship between the claimant's age and length of UI contributions and the PBD prior to (upper panel) and after the reform (lower panel). However, as the data used in this analysis focuses on unemployed individuals aged 16 to 54, this study exploits information about the reform for the younger of the two age groups only.

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[7] The coalition also disagreed about other pending topics, such as the introduction of minimum wages in the postal sector, for example.

[8] Note that the reform extended the qualifying period from three to five years, too.

[9] Hence, the reform subsequently extended the PBD for all eligible individuals who had become unemployed before January 1, 2008 and were entitled to receive benefit payments on December 31, 2007 by three months (see §434r, SGB III). However, note that this only applied to those individuals who fulfilled both criteria (above the respective age threshold and sufficient contributions to UI) at the time of unemployment registration.

# 4    Data

In order to investigate the consequences of this reform, I use data from the IZA Evaluation Dataset Survey, which covers a large sample of individuals registering as unemployed at the German Federal Employment Agency between June 2007 and May 2008, i.e., prior to and after the reform (see Arni et al. (2014) for details). Designed to allow for the investigation of active labor market program (ALMP) effects, the dataset surveys prime-aged workers (aged 16 to 54) who enter unemployment, search for reemployment opportunities and qualify for participation in ALMPs. Individuals close to (early) retirement and all recipients of welfare benefits, who are thus not entitled for participation in ALMPs, are in turn not covered by the survey.

In order to obtain a representative sample of the unemployed population in this survey and to account for seasonal effects over one year, a random sample of unemployed individuals was drawn from the monthly unemployment inflow statistics of the German Federal Employment Agency in each month between June 2007 and May 2008. In total, 17,396 individuals were first interviewed around two months after becoming unemployed and were repeatedly questioned over time. For the present analysis, the first wave of the survey is exploited, which provides detailed information on individual job search behavior at the beginning of the unemployment spell. More precisely, the survey covers information on the number of applications, the filing of applications that require moving and the reservation wage, i.e., the indicated lowest wage rate at which an unemployed person would consider working. This information is supplemented by a large set of variables on the respondents' employment history, personal characteristics (e.g., the age, education or level of professional training) and personality traits, such as the locus of control or the Big Five. The data also include information on individuals' supervision intensity by the local Employment Agencies (the number of agency visits or received job offers, among others) and local labor market conditions, such as regional unemployment and vacancy rates.

For the empirical analysis presented below, all individuals who are already reemployed at the time of the first interview[10] – around 25% of the sample – or

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[10] On average, the interview is conducted around eight weeks after the individuals' unemployment

did not participate in the labor market are excluded.[11] Descriptive statistics for the estimation sample are provided in Table A.2 in the Appendix.

# 5 Identification

The dataset allows observing the job search behavior of unemployed individuals who were interviewed prior to or after the public announcement of the reform and its details on December 11, 2007. Variation in the date of unemployment registration, the policy reform and the date of the interview provide a clear quasi-experimental setting to identify the effects of the PBD on job search effort.

Figure 1: Unemployment entry, interview date and expected benefit duration
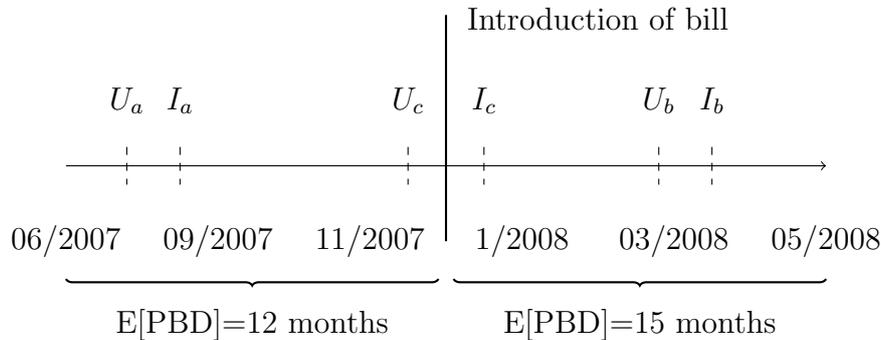


*Notes:* The figure plots the setting of this analysis. For example, individuals $i \in \{a, b, c\}$ registered as unemployed at $U_i$ and were interviewed at $I_i$. Expectations about the potential benefit duration change on December 11, 2007; the day the bill was introduced to the parliament.

Figure 1 illustrates the setting of the analysis. Individual $a$ registered as unemployed ($U_a$) and was interviewed about her job search behavior ($I_a$) prior to the reform, thus choosing her job search effort while expecting a PBD of twelve months. In turn, individual $b$ became unemployed and chose job search effort while knowing about the extension of the PBD. For individual $c$, expectations about the PBD were updated after unemployment registration but prior to the interview. Some part of the relevant job search period was thus subject to the new PBD regime, whereas initial job search effort was chosen while expecting a PBD of 12 months. The job search effort of individual $c$ may thus have converged towards the search effort level

registration.

[11] Note that both the probability of being reemployed at the time of the first interview as well as the probability of participating in the labor market are not affected by the reform of interest.

of individual $b$ after the extension of the PBD became public.[12]

Based on this setting and in line with the empirical strategies pursued by Kyyrä and Ollikainen (2008) as well as Van Ours and Vodopivec (2008), a simple difference-in-differences strategy is applied to compare pre- and post-reform outcomes. Unemployed workers aged 50 to 54, who were interviewed after the announcement of the reform and hence gained knowledge about the extension of the PBD prior to choosing their job search behavior, constitute the *treatment group*. Same-aged individuals interviewed prior to the introduction of the reform serve as the *comparison group*.[13] Unemployed workers aged 45 to 49, interviewed prior to or after the reform, serve as *control groups* in order to account for any seasonal aggregate effects.

**Eligible individuals** As highlighted before, benefit duration in Germany is subject to the claimant's age and length of UI contributions within a given qualifying period. The reform of interest thus changed the PBD for a subset of individuals aged 50 to 54 only. Individuals were entitled to extended PBD if having had contributed to UI for at least 12 months within the last two years (eligibility constraint) and for 30 months within the last five years (coverage constraint). For the purpose of this analysis, all unemployed individuals that did not fulfill the contribution criteria were thus excluded, irrespective of the claimant's age. Unfortunately, the present dataset provides information on the respondents' last employment *period* only, which limits the analysis to those claimants who have fulfilled both entitlement criteria without any interrupting period of non-employment. Compared to the entire eligible population, the individuals in this sample are thus positively selected with regard to their labor market history given that the sampled individuals were not subject to unemployment in the recent past. If the sampled individuals responded differently with regard to this reform compared to the eligible individuals not covered in the analysis, the estimates of this study may thus not provide the true treatment effect for the entire eligible population.

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[12] In the empirical analysis presented below, special attention is paid to those individuals whose expectations about the PBD updated after unemployment registration but prior to the interview.

[13] Note that the comparison group is equivalent to the treatment group observations measured pre-treatment.

In general, heterogeneous responses by these two groups may be due to consequences and causes of prior unemployment experience. First, UI-induced moral hazard may be less (more) pronounced among the group of those eligible individuals who have experienced unemployment prior to the current unemployment spell if these individuals had encountered net (dis)utility from unemployment and include past experiences in their current decision about job search effort. Second, unobservable and observable differences between both groups may have caused prior unemployment spells and could affect individuals' responses with respect to the reform of the PBD.

The results of my empirical analysis, however, suggest that past unemployment experience does not affect current choices about job search effort. As shown in Appendix Tables A.3–A.5, UI-induced reductions in job search effort are unaffected by (the length of) previous unemployment spells. Evidence of more pronounced effects for the low- and medium-skilled compared to the high-skilled unemployed further implies that the sample may underestimate the overall treatment effect for the entire eligible population if the covered sample is positively selected on (observed) skills.

**Empirical model and identification**    The present setting allows for testing the two hypotheses of the job search model presented in Section 2. Using *difference-in-differences* techniques, it is tested whether an extension in the PBD lowers the job search effort (cf. equation (6)) and increases the reservation wage (cf. equation (5)) of the individuals concerned. The underlying empirical specification reads as follows:

$$y_i = \alpha + \beta T_i + \gamma A_i + \delta(T_i \times A_i) + X_i'\rho + \varepsilon_i, \tag{7}$$

with the dependent variable $y_i$ indicating measures of job search effort or the reservation wage of individual $i$, $T_i$ being a dummy variable indicating whether the individual was interviewed after the reform, and $A_i$ indicating whether the individual is aged between 50 to 54. The treatment effect is given by $\delta$, $X_i'$ defines a vector of control variables and $\varepsilon_i$ the error term.

Identification of the model rests upon the assumptions that (i) no observable or unobservable individual characteristics determined the allocation to the treatment

or comparison group and (ii) potential changes in labor market conditions over the sampling period affected treatment and control groups to an equal extent. Put more precisely, except for differences in knowledge about the reform due to the timing of being interviewed/becoming unemployed, the comparison group should be highly similar to the treatment group. Moreover, changes in business cycle conditions should not have had asymmetric effects on treatment and control groups. The remainder of this section aims at validating these identifying assumptions.

**Voluntary quits and strategic layoffs** In order for the identifying assumptions to hold, layoffs have to be exogenous from the individual's perspective. As some workers may, however, potentially opt to become unemployed in response to the extension of the PBD, the treatment group may be self-selected in this respect. To account for potential selection, all workers that voluntarily quit their job or became unemployed by mutual agreement are therefore excluded from the sample. Excluding these individuals from the analysis further accounts for the fact that payments of UI can be suspended for up to twelve weeks if workers voluntarily opt out of employment, which lowers the PBD accordingly.

Strategic layoff decisions by firms may further violate the identifying assumption. If firms deliberately suspend dismissals of older workers (aged 50 or above) to allow for a longer PBD, allocation into the treatment and comparison group would be non-random. Due to the fast implementation of the reform, adaptive behavior of firms is highly unlikely, and strict dismissal laws impede strategic timing of layoffs in Germany. However, as a robustness check, the analysis is further limited to layoffs where strategic timing of terminations can be ruled out, focusing on those workers who became unemployed either due to plant closings or the expiration of a temporary contact. As detailed below, the results of the analysis remain unaffected in the cases where the analysis is limited to the respective subgroups.

**Concurrent ALMP reforms** Estimates would be biased if simultaneous reforms had occurred that asymmetrically affected treatment, comparison and control groups. Concurrent with the extension of the PBD, the government did indeed introduce labor market integration vouchers (*Eingliederungsgutscheine*). In

brief, these vouchers slightly modified eligibility criteria for unemployed individuals aged 50 or above so that they could receive employment integration subsidies (*Eingliederungszuschüsse*). These subsidies have long been used as an ALMP instrument in Germany, and all unemployed individuals are allowed to file for integration subsidies in general. Approval, duration as well as the amount of the subsidy are subject to the discretion of the local Employment Agency and are dependent upon applicants' work productivity limitations, with the scope and availability of integration subsidies being extended for individuals aged 50 or above (since May 2007).

The existence of integration vouchers and extended subsidies for the unemployed aged 50 or above should, however, not impede the causal interpretation of the findings in my analysis. Given that all unemployed individuals in the treatment and the comparison group were potentially eligible for extended subsidies in general, potential effects arising from these subsidies should be captured by the parameter of the age group dummy and therefore not affect the treatment effect of interest. Moreover, the slight modifications in the eligibility criteria for subsidies invoked by the introduction of the integration voucher as of January 1, 2008 only had a marginal, negligible effect on take up rates. In 2008, the Federal Employment Agency granted 3,000 vouchers only, compared to more than 1.5 million ALMP measures in total (Statistics of the Federal Employment Agency).[14]

**Observable characteristics by age group and interview period**  As highlighted above, besides differences in knowledge about the reform and the timing of becoming unemployed, the comparison and treatment group should be highly similar in observable characteristics. Moreover, labor market conditions should be either constant over time or change to an equal extent for the treatment and control group. The IZA Evaluation dataset allows for extensive testing of both identifying assumptions. Table 1 shows (differences in) mean characteristics by age groups and within the treatment and control group prior to and after the reform.

Columns (1) to (3) show means for the two age groups and the results of a sim-

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[14] By April 2012, the voucher program was stopped. Over the course of its existence, a total of around 20,000 vouchers had been issued. The total number of subsidies granted was quite constant over the period of interest. Figure A.2 in the Appendix shows the annual number of subsidies granted from 2006 to 2010.

ple t-test (p-values) on the equality of the means for a large set of variables. Besides expected differences in age, it becomes apparent that both groups of individuals are not systematically different from each other. On average, individuals from both groups are married, completed an apprenticeship and generated a monthly net labor income of around 1,400 euros prior to unemployment, for example. Evaluated at the mean, both groups of workers further come from comparable regions across Germany, with differences in local unemployment and vacancy rates being small and insignificant. Moreover, the individuals in both groups received equal supervision by local Federal Employment Agencies, for example, by means of the number of agency visits or job offers. Lastly, both groups are similar with respect to personality traits, measured by means of individuals' locus of control, extroversion or openness, among others.

I further test whether mean characteristics within one age group differ before and after the reform. Columns (4) to (9) show the corresponding results. Both of the two control groups as well as comparison and treatment group are highly similar in terms of observable personal characteristics. Most importantly, the comparison and treatment group neither differ in terms of personal characteristics nor personality traits when being compared at the mean. The only notable exception is the share of respondents that has been unemployed prior to this current spell, which is higher in both the control and treatment group after the reform but only significantly different in the latter group.

When focusing on regional characteristics and individual ALMP measures, differences in some variables become apparent. However, changes over time occur for both age cohorts symmetrically and to a similar extent. In detail, the data suggest local active labor market intensities, measured by means of the share of ALMP participants over the number of total unemployed individuals, to be higher after the reform, yet for both treatment and control group. Local unemployment rates, in turn, remain constant. The same pattern applies to individual-level measures of support by the local Federal Employment Agencies. On average, the number of visits at the local agency is slightly lower after the reform. These small difference may, however, be explained by the fact that the mean number of weeks elapsed

Table 1: Observable characteristics by age and unemployment entry

| | Individuals | | | Within Control Group | | | Within Treatment Group | | |
|---|---|---|---|---|---|---|---|---|---|
| | Control group | Treatment Group | p-value | pre treatment | post treatment | p-value | pre treatment | post treatment | p-value |
| Personal characteristics | | | | | | | | | |
| Age | 47.31 | 52.57 | 0.00 | 47.18 | 47.36 | 0.32 | 52.71 | 52.51 | 0.31 |
| Male (no/yes) | 0.45 | 0.42 | 0.46 | 0.39 | 0.48 | 0.14 | 0.39 | 0.44 | 0.47 |
| Education | 3.83 | 3.63 | 0.11 | 3.99 | 3.76 | 0.24 | 3.61 | 3.64 | 0.90 |
| Skill level | 2.09 | 2.11 | 0.62 | 2.12 | 2.08 | 0.53 | 2.16 | 2.09 | 0.31 |
| Last log wage | 7.07 | 7.10 | 0.58 | 7.10 | 7.06 | 0.58 | 7.10 | 7.10 | 1.00 |
| Unemployed Before | 0.65 | 0.62 | 0.48 | 0.60 | 0.67 | 0.27 | 0.52 | 0.66 | 0.04 |
| Regional characteristics | | | | | | | | | |
| State of residence | 8.11 | 8.36 | 0.45 | 7.57 | 8.32 | 0.14 | 7.94 | 8.54 | 0.23 |
| Local unemployment rate | 9.26 | 9.19 | 0.83 | 9.14 | 9.31 | 0.74 | 9.11 | 9.23 | 0.82 |
| Local ALMP intensity | 15.70 | 16.34 | 0.17 | 14.42 | 16.19 | 0.01 | 15.59 | 16.66 | 0.18 |
| Individual ALMP measures | | | | | | | | | |
| Number of agency job offers | 1.83 | 1.90 | 0.78 | 2.04 | 1.74 | 0.36 | 1.74 | 1.96 | 0.60 |
| Number of agency visits | 1.70 | 1.78 | 0.16 | 1.93 | 1.61 | 0.00 | 1.91 | 1.72 | 0.04 |
| Personality traits | | | | | | | | | |
| Internal locus of control | 5.90 | 5.90 | 0.99 | 5.88 | 5.91 | 0.81 | 5.99 | 5.86 | 0.29 |
| Consciousness | 6.39 | 6.26 | 0.10 | 6.38 | 6.40 | 0.89 | 6.41 | 6.20 | 0.09 |
| Openness | 4.95 | 4.91 | 0.72 | 4.91 | 4.97 | 0.66 | 4.97 | 4.89 | 0.64 |
| Extraversion | 5.00 | 5.03 | 0.75 | 4.90 | 5.04 | 0.31 | 5.13 | 4.99 | 0.31 |
| Neuroticism | 3.83 | 3.85 | 0.79 | 3.80 | 3.84 | 0.79 | 3.76 | 3.89 | 0.39 |
| Weeks b/w UE and interview | 7.67 | 7.83 | 0.39 | 9.32 | 7.03 | 0.00 | 9.95 | 6.93 | 0.00 |
| Dependent variables | | | | | | | | | |
| Number of filed applications | 15.58 | 16.65 | 0.62 | 12.96 | 16.59 | 0.18 | 21.95 | 14.39 | 0.04 |
| Applying for distant jobs | 0.15 | 0.15 | 0.96 | 0.10 | 0.17 | 0.08 | 0.24 | 0.11 | 0.01 |
| Log reservation wage | 7.00 | 7.00 | 0.93 | 7.01 | 6.99 | 0.78 | 7.00 | 7.00 | 0.94 |
| Number of observations | 324 | 274 | | 90 | 234 | | 82 | 192 | |

*Notes*: The table provides information on (differences) in means for (a) control and treatment group; (b) the control group before and after the reform; and (c) the comparison and treatment group. The total number of observations is 598. Note that the number of observations is slightly smaller for the reservation wage (N=559).

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between the individuals' unemployment registration and the interview decreased for both age groups from nine weeks prior to the reform to seven weeks afterwards.

Against the background of these similarities, it is further investigated whether treatment and control group would have followed the same trend in the outcome variables over time absent treatment. In order to investigate this identifying assumption, respondents are grouped according to their interview date and trends in the average job search intensity and the reservation wage are compared between treatment and control group.[15] For example, Figure 2 visualizes the mean number of applications for the treatment and control group over the course of the survey period.[16] First, the graph provides evidence in favor of a common trend for both groups absent the treatment. Average job search intensity is higher for the treatment than for the control group (cf. Table 1), but trends are highly similar for both groups prior to the reform. The same applies to the other outcomes of interest (see Panels (a) of Figures A.4 and A.5).[17]

In addition to the visual evidence in favor of a common trend prior to the reform, Figure 2 also provides first insights about the treatment effect of the reform. While the mean number of job applications increases slightly for the control group after the reform, mean job applications for the treated unemployed significantly decrease. Similar trends can be observed for the second measure of job search effort, the probability of applying for a job in distant areas (see Panel (a) of Figure A.4). In contrast, reservation wages for both the control and treatment group remain unaffected by the extension of the PBD (see Figure A.5).

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[15] Recall that the underlying dataset is based on monthly-drawn random samples from the unemployment inflow statistics of the German Federal Employment Agency over the course of one year, such that interview dates vary for the respondents in the treatment and control groups.

[16] The corresponding figures for the other outcomes of interest are provided in the Appendix of this paper (see Panel (a) of Figures A.4, A.5)

[17] Panel (b) of Appendix Figures A.3– A.5 further demonstrates that this result holds true when controlling for differences in observable characteristics for the respondents covered in each of the three periods prior to treatment.

Figure 2: Trends in the number of job applications
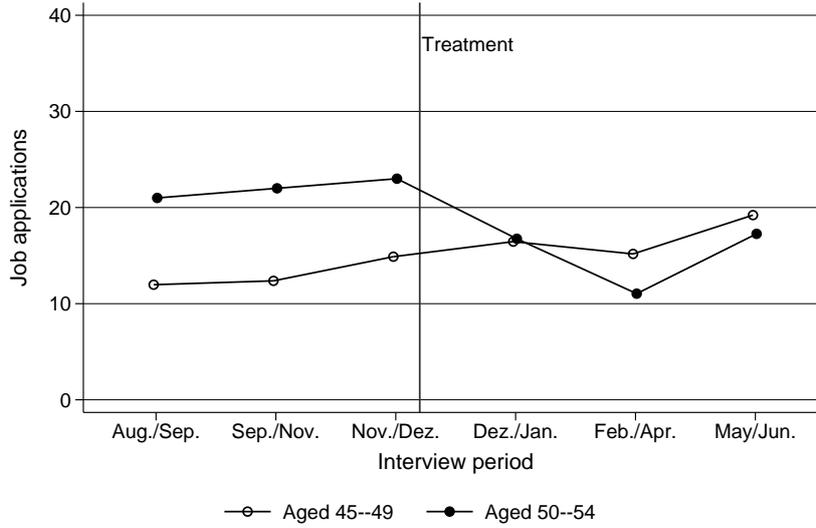


*Notes:* The graph plots variation in the mean number of job applications for treatment and control group over the survey period.

# 6 Results

## 6.1 Baseline estimates

Table 2 provides the corresponding treatment effect estimates for the three outcomes of interest, the number of filed applications, the probability of applying for jobs that require moving and the reservation wage.

Column (1) of Panel A shows that the PBD has a negative and significant effect on the total number of file applications. In this very simple model, the average number of filed applications drops by around 40% of a standard deviation in response to the reform. In columns (2) to (5), control variables are successively added to the model to check the robustness of this result. Adding personal characteristics, such as the individuals' gender, level of training or last wage prior to unemployment hardly changes the treatment effect (see column (2)). The same conclusions arise when adding individual-level controls of ALMP intensity (column (3)), or regional controls of the labor market to the model (column (4)). As it has been shown that personality traits may affect job search behavior (Caliendo et al., 2015), information on individuals' personality traits are added in the most comprehensive specification. As displayed in column (5), accounting for these variables, however, hardly affects

the estimate.

Panel B of Table 2 presents the corresponding results for my second measure of job search effort. The estimates show a statistically significant and robust negative effect of the PBD on the probability of applying for a job that requires moving. From the results of the simple model presented in column (1), it can be inferred that the probability decreases by around 20% in response to the reform. In line with the results of Panel A, the effect is very robust with respect to the inclusion of additional covariates. Estimates of the treatment effect provided in columns (2) to (5) do not change much when successively adding controls. Thus, the results for the two distinct measures of job search effort verify the prediction of the job search model presented in Section 2: an increase in the PBD lowers individual job search effort (cf. equation (6)).

The estimates presented in Panel C in turn provide no evidence in favor of higher reservation wages due to the increase in the PBD. The estimated treatment effect from the simple model presented in column (1) is close to zero and statistically insignificant. This holds true when successively adding control variables to the model. While this result is thus in contrast to the prediction of the job search model presented before (cf. equation (5)), it is still in line with recent evidence by Krueger and Mueller (2014) and Schmieder et al. (2015), who show that reservation wages respond little over the spell of unemployment and with respect to changes in UI parameters. For example, because job seekers may potentially "anchor their reservation wage on their previous wage" (Krueger and Mueller, 2014, p.31). Overall, the moderate increase in the PBD is thus found to lower job search effort but to keep reservation wages unaffected.

**Adjustment of job search behavior** Due to the setup of the reform, the treatment group comprises a subset of individuals who learned about the reform after registering as unemployed but prior to the interview.[18] These individuals therefore started their initial job search while expecting a PBD of 12 months but learned about the reform during the relevant search spell. In this section, it is hence tested

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[18] Note that this group accounts for around 15% of the sample.

## Table 2: The effects of benefit duration on job search

### Panel A – Number of job applications

|  | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Reform dummy | 3.638 | 6.410** | 6.969** | 7.419*** | 7.467*** |
|  | (2.685) | (2.804) | (2.874) | (2.863) | (2.790) |
| Aged 50-54 | 8.996** | 5.945 | 6.898 | 7.885* | 6.702 |
|  | (3.699) | (4.712) | (4.484) | (4.628) | (4.793) |
| **Treatment Effect** | -11.199** | -10.766** | -12.628*** | -12.914*** | -12.021*** |
|  | (4.528) | (4.456) | (4.818) | (4.786) | (4.606) |
| Adjusted-$R^2$ | 0.005 | 0.044 | 0.125 | 0.134 | 0.151 |

### Panel B – Distant applications

|  | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Reform dummy | 0.071* | 0.084* | 0.095** | 0.077 | 0.072 |
|  | (0.040) | (0.046) | (0.046) | (0.047) | (0.047) |
| Aged 50-54 | 0.144** | 0.185** | 0.193** | 0.197** | 0.194** |
|  | (0.057) | (0.079) | (0.079) | (0.081) | (0.080) |
| **Treatment Effect** | -0.205*** | -0.211*** | -0.223*** | -0.222*** | -0.218*** |
|  | (0.066) | (0.064) | (0.064) | (0.065) | (0.065) |
| Adjusted-$R^2$ | 0.013 | 0.152 | 0.155 | 0.146 | 0.151 |

### Panel C – (Log) reservation wage

|  | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Reform dummy | -0.017 | 0.008 | 0.011 | -0.002 | -0.002 |
|  | (0.061) | (0.034) | (0.035) | (0.035) | (0.035) |
| Aged 50-54 | -0.013 | 0.065 | 0.057 | 0.039 | 0.046 |
|  | (0.080) | (0.055) | (0.055) | (0.055) | (0.055) |
| **Treatment Effect** | 0.022 | -0.027 | -0.027 | -0.022 | -0.020 |
|  | (0.092) | (0.049) | (0.050) | (0.050) | (0.050) |
| Adjusted-$R^2$ | -0.005 | 0.707 | 0.710 | 0.709 | 0.710 |

|  | | | | | |
|---|---|---|---|---|---|
| Individual controls | No | Yes | Yes | Yes | Yes |
| ALMP measures | No | No | Yes | Yes | Yes |
| Regional controls | No | No | No | Yes | Yes |
| Personality traits | No | No | No | No | Yes |
| Number of observations | 598 | 598 | 598 | 598 | 598 |

*Notes:* The table provides the baseline results of the analysis based on equation (7). Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

whether there are different treatment effects for the fully and partly treated individuals. I further investigate whether the treatment effect remains stable over the survey period by allowing for different treatment effects from December 11 to January, February to April and May to June.

In a first step, all individuals who learned about the reform after unemployment registration but prior to the interview were dropped from the sample. Columns (1) and (2) of Table 3 indicate that the estimated effect remains virtually unchanged when using the reduced sample. In line with this result, columns (3) and (4) further show no evidence of heterogeneous treatment effects for the partly and fully treated. While the treatment effect may certainly depend on the relative time period between entry into unemployment, the reform and the interview date, a small sample size unfortunately precludes further analysis of this potential heterogeneity. Columns (5) and (6) in turn indicate that treatment effects do not differ with respect to the interview date, which further corroborates the baseline findings.

## 6.2  Sensitivity of results

The following section investigates the sensitivity of the previous results by (i) accounting for adaptive behavior, (ii) assessing potential biases due to strategic layoff decisions of firms, and (iii) testing the unconfoundedness assumption by means of a pseudo treatment test.

**Salience of the reform**   The fast implementation of the policy reform limits the potential of adaptive behavior. Knowledge about important aspect of the reform, such as the reform's date of inception and its retroactive implementation, only became public on December 11, 2007. In conjunction with exact knowledge about the interview date, this allows for the precise definition of treatment and control groups. However, as a general agreement about the reform was already reached by November 12, 2007, adaptive behavior to this news cannot be entirely ruled out.

Although early adaption to the reform would blur the control group and would bias estimates towards zero, given that the control and treatment group would be more similar, the sensitivity of the results is tested when redefining treatment and

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Table 3: PBD and the Number of Applications - Treatment Duration

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Reform dummy | 3.780 | 9.152*** | 3.780 | 8.304*** | 7.413 | 11.060 ** |
| | (2.968) | (3.350) | (2.971) | (3.161) | (6.268) | (4.801) |
| Aged 50-54 | 9.585*** | 5.306 | 9.585*** | 7.387 | 9.602*** | 7.975 * |
| | (3.701) | (4.645) | (3.705) | (4.759) | (3.699) | (4.677) |
| | | | | | | |
| **Treatment Effect** | -12.555*** | -13.000*** | | | | |
| | (4.802) | (4.931) | | | | |
| | | | | | | |
| $\times$ **Fully treated** | | | -12.555*** | -12.377 ** | | |
| | | | (4.807) | (4.806) | | |
| $\times$ **Partly treated** | | | -8.881 | -13.256 ** | | |
| | | | (5.955) | (6.024) | | |
| | | | | | | |
| $\times$ **Interview: Dec-Jan** | | | | | -10.539 ** | -12.247*** |
| | | | | | (4.541) | (4.495) |
| $\times$ **Interview: Feb-Apr** | | | | | -14.900*** | -13.813*** |
| | | | | | (5.421) | (5.286) |
| $\times$ **Interview: May-Jul** | | | | | -12.250 | -12.781 * |
| | | | | | (7.792) | (7.444) |
| | | | | | | |
| Adjusted-$R^2$ | 0.008 | 0.171 | 0.006 | 0.150 | -0.000 | 0.152 |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 505 | 505 | 598 | 598 | 598 | 598 |

*Notes:* The table shows the regression results of equation (7), focusing on differential effects due to the timing/duration of the treatment. The dependent variable is the number of applications. In Columns (1) and (2), all individuals who became unemployed prior to the reform but were interviewed thereafter are dropped. Standard errors (in parantheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

control groups by November 12, 2007. Table A.7 in the Appendix provides the corresponding results for the three outcomes of interest, obtained from the most simple and most comprehensive version of the empirical model. The estimates highlight that all qualitative results of the analysis remain robust to the redefinition of the reform's date, although point estimates are smaller and less precisely estimated, which suggests that the reform's date in the baseline regressions is correctly chosen.

**Strategic timing of layoffs**  As highlighted above, strategic timing of layoffs may impede the causal interpretation of the findings provided. Although strict employment protection laws in Germany limit the scope for strategic firing decisions of firms[19], the robustness of the findings is tested by limiting the analysis to those individuals who became unemployed due to plant closure, the termination of a temporary contract and alike. Although the number of observations decreases significantly, the results presented in Table A.8 demonstrate that estimates remain robust to this constraint.

**Pseudo treatment**  Identification of the underlying model further relies on the assumption that individuals are randomly assigned to treatment and control group and are similar in terms of observable and unobservable characteristics. While observable characteristics are indeed similar among treatment, comparison and control groups (cf. Table 1), unobservable variables may still violate the unconfoundedness assumption. Following Rosenbaum (1987), this assumption is indirectly tested by estimating the causal effect of the treatment for two groups of individuals that were unaffected by the reform (workers aged 40 to 44 and 45 to 49, respectively); with one of the two groups (the older age group) being arbitrarily considered as pseudo-treated. No evidence of any pseudo treatment effect on the outcomes would strengthen the claim of unconfoundedness. Table A.9 shows support for the identifying assumption, given that pseudo-treatment effects for all three measures of job

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[19] Dismissal of regular workers is subject to a variety of legal regulations. Advanced notice of layoff is required by law, with the period of notice increasing with workers' tenure (§622, German Civil Code). Additional rules (*Kündigungsschutzgesetz*) apply for plants that employ at least ten full-time equivalent workers. Rates of job destruction and creation mirror these legislative features of the German labor market: job and worker flow rates are around 50% lower than in the US (Bachmann et al., 2013).

search effort are small and statistically insignificant.[20]

## 6.3 Explaining the mechanism

Against the backdrop of my previous findings, the following section aims at investigating whether the observed reductions in job search effort indeed reflect moral hazard behavior. For example, Chetty (2008) shows that UI-induced increases in unemployment duration cannot be entirely attributed to moral hazard behavior, but the role of liquidity effects must also be accounted for.

In order to better understand the potential mechanisms at work, I investigate whether UI-induced reductions in job search effort vary (i) with the tightness of the individuals' respective local labor market, (ii) the length of the current unemployment spell, and (iii) the individuals' financial situation. I account for differences in local labor markets as a reduction in job search effort may be less costly in regions with lower unemployment rates. Stronger treatment effects in prosperous regions may thus indicate moral hazard behavior. As indicated in columns (1) and (2) of Table 4, treatment effects are indeed strongest for individuals that live in regions with low or medium unemployment rates. In contrast, individuals subject to significant local unemployment do not reduce their job search effort.

As a second exercise, I exploit variation in the time period between individuals' unemployment registration and interview date, which varies from around 4 to 16 weeks. Columns (3) and (4) of Table 4 show that individuals seem to postpone their job search effort in response to the extension of the PBD, as the treatment effect is particularly strong at the very beginning of the unemployment spell but becomes small and insignificant over the course of unemployment. I take this postponement of search effort as suggestive evidence in favor of moral hazard behavior. In a final step, I account for the potential role of liquidity effects by using information on debts as a proxy for financial constraints. More pronounced reductions in job search effort by individuals subject to non-manageable debts (with relates to around 10% of the sample) might indeed provide evidence for the presence of liquidity effects.

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[20] Note that, except for the mean age, both groups are highly similar with regard to observable characteristics. The corresponding descriptive statistics are available upon request.

Table 4: Exploring the mechanism - PBD and the Number of Applications

| Dep. Var.: Job applications | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Reform dummy | 3.819 | 7.545*** | 4.907* | 7.434*** | 3.466 | 7.090** |
| | (2.695) | (2.797) | (2.585) | (2.867) | (2.729) | (2.788) |
| Aged 50-54 | 9.072** | 6.678 | 8.868** | 6.896 | 9.297** | 5.422 |
| | (3.686) | (4.721) | (3.527) | (5.037) | (4.083) | (5.245) |
| **Treatment** | | | | | | |
| | | | | | | |
| × **Low Local UE** | -12.789** | -12.955** | | | | |
| | (5.425) | (5.476) | | | | |
| × **Medium Local UE** | -13.377** | -14.885*** | | | | |
| | (5.575) | (5.718) | | | | |
| × **High Local UE** | -6.810 | -7.034 | | | | |
| | (5.075) | (5.029) | | | | |
| | | | | | | |
| × **Weeks(UE,Int.):4-6** | | | -12.026** | -11.555** | | |
| | | | (5.374) | (4.943) | | |
| × **Weeks(UE,Int.):7-9** | | | -10.702** | -13.294** | | |
| | | | (4.645) | (5.211) | | |
| × **Weeks(UE,Int.):10+** | | | -1.895 | -5.045 | | |
| | | | (8.853) | (6.804) | | |
| | | | | | | |
| × **Problematic debts** | | | | | -10.958** | -11.910** |
| | | | | | (4.523) | (4.722) |
| × **No problematic debts** | | | | | -13.755 | -7.960 |
| | | | | | (9.774) | (8.563) |
| | | | | | | |
| Adjusted-$R^2$ | 0.003 | 0.148 | 0.003 | 0.146 | 0.003 | 0.153 |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 598 | 598 | 598 | 598 | 593 | 593 |

*Notes:* The table shows the regression results of equation (7), allowing for heterogeneous treatment effects by (a) local unemployment rates, (b) the length between unemployment registration and the interview, and (c) individuals' debts. The dependent variable is the number of applications. Standard errors (in parantheses) are heteroscedasticity robust. $^*$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

However, columns (5) and (6) of Table 4 provide no evidence for stronger responses of the highly indebted.

# 7    Conclusion

To date, a large empirical literature has shown that more generous UI schemes significantly prolong spells of nonemployment. While this finding is usually attributed to UI-induced moral hazard, empirical evidence on the assumed relationship is scarce. Using quasi-experimental variation in the PBD for one specific age group of workers in Germany paired with direct information on the job search behavior of unemployed individuals, this paper complements the existing evidence by providing causal estimates of the effect of the PBD on job search effort and reservation wages.

The results of this analysis lend considerable support to the existence of UI-induced moral hazard, with the extension of the PBD leading to a considerable decrease in job search effort measured by the number of filed applications and the probability of applying for jobs that require moving. In line with recent evidence (see, among others, Krueger and Mueller (2014) and Schmieder et al. (2015)) but in contrast to standard job search theory, reservation wages, however, do not increase in response to the extension of the PBD.

Overall, the study provides comprehensive evidence of strategic search behavior. Unemployed individuals respond to more generous UI by reducing search effort, which highlights the trade-off faced by policy makers when designing UI schemes. While UI should allow individuals to actively search for suitable reemployment, disincentive effects arising from the generosity of UI need to be accounted for. Based on the findings of this study, future research might aim at estimating effort choices and reemployment probabilities due to changes in UI in one integrated framework.
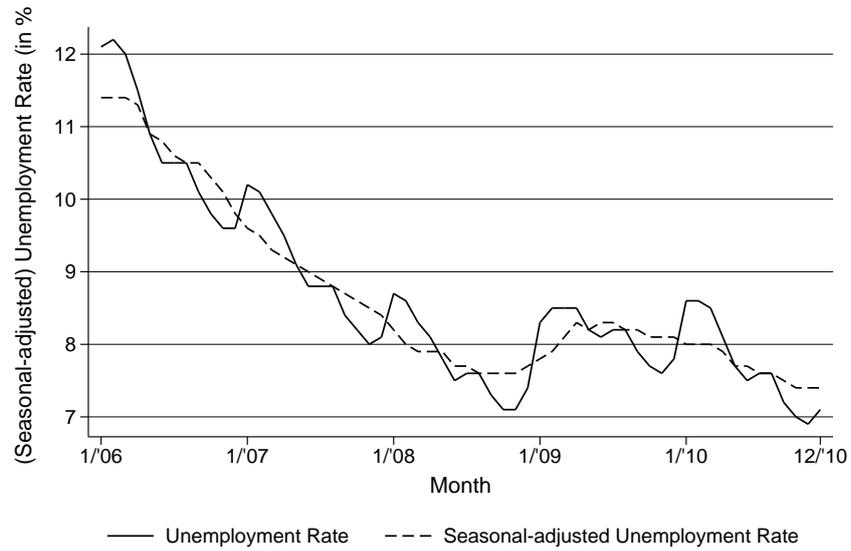
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# A   Appendix

Figure A.1: (Seasonal-adjusted) unemployment rate (2006–2010)

Figure A.2: Number of granted employment integration subsidies (2006–2010)

Figure A.3: Trends in the number of job applications
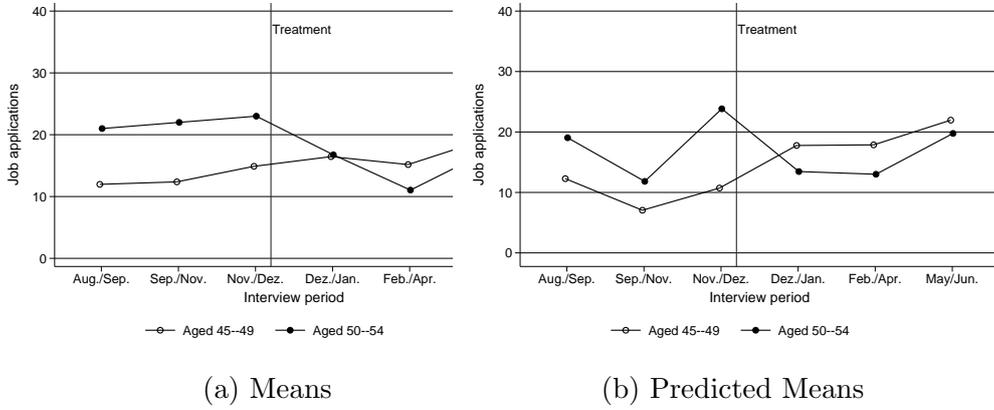


(a) Means

(b) Predicted Means

*Notes*: Panel (a) plots the variation in the mean number of job applications for treatment and control group over the survey period. Panel (b) plots the variation in predicted means, controlling for differences in observable characteristics across groups, over the same period.

Figure A.4: Trends in the probability of distant applications



(a) Means
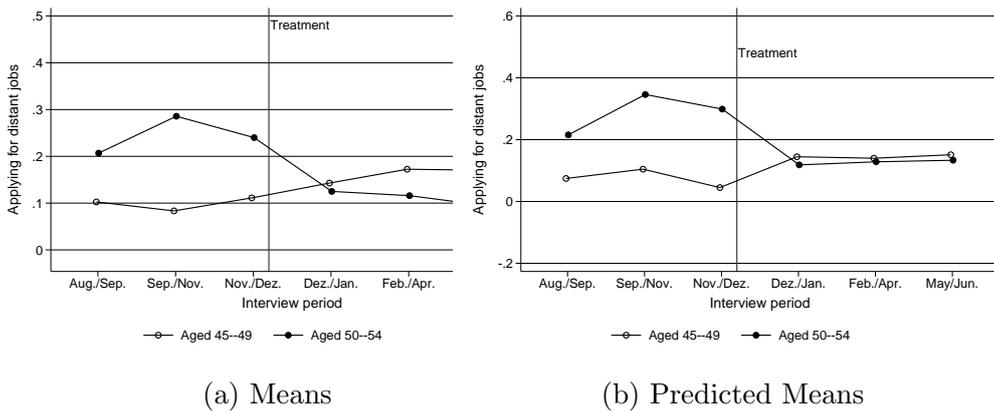
(b) Predicted Means

*Notes*: Panel (a) plots the variation in the mean probability of applying for distant jobs for treatment and control group over the survey period. Panel (b) plots the variation in predicted means, controlling for differences in observable characteristics across groups, over the same period.

Figure A.5: Trends in the reservation wage



(a) Means            (b) Predicted Means
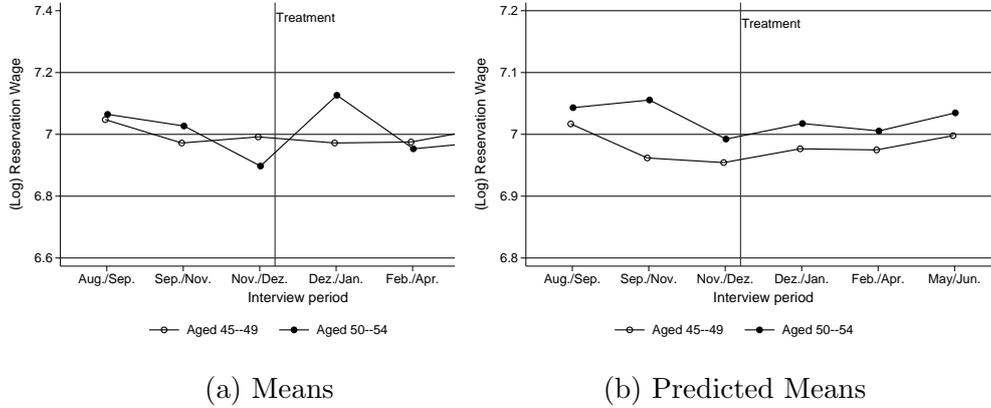
*Notes*: Panel (a) plots the variation in the mean reservation wage for treatment and control group over the survey period. Panel (b) plots the variation in predicted means, controlling for differences in observable characteristics across groups, over the same period.

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 | |

**Since 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:* The table shows the relationship between the claimant's age, length of UI contributions and the 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.

Table A.2: Descriptive statistics for the estimation sample

|  | Mean | Std Deviation | Minimum | Maximum | Observations |
|---|---|---|---|---|---|
| **Dependent variables** | | | | | |
| Number of filed applications | 16.07 | 26.82 | 0.00 | 400.00 | 598 |
| Applying for distant jobs | 0.15 | 0.36 | 0.00 | 1.00 | 598 |
| Log reservation wage | 7.00 | 0.47 | 5.30 | 8.99 | 559 |
| | | | | | |
| **Personal characteristics** | | | | | |
| Age | 49.72 | 3.00 | 45.00 | 55.17 | 598 |
| Male (no/yes) | 0.44 | 0.50 | 0.00 | 1.00 | 598 |
| Education | 3.74 | 1.50 | 0.00 | 7.00 | 598 |
| Skill level | 2.10 | 0.50 | 1.00 | 3.00 | 598 |
| Last log wage | 7.08 | 0.56 | 5.08 | 9.21 | 598 |
| Unemployed Before | 0.64 | 0.48 | 0.00 | 1.00 | 598 |
| | | | | | |
| **Regional characteristics** | | | | | |
| Local unemployment rate | 9.23 | 3.93 | 3.00 | 17.00 | 598 |
| Local ALMP intensity | 15.99 | 5.60 | 7.00 | 30.00 | 598 |
| State of residence | 8.22 | 4.07 | 1.00 | 16.00 | 598 |
| | | | | | |
| **Individual ALMP measures** | | | | | |
| Number of agency job offers | 1.86 | 3.00 | 0.00 | 25.00 | 598 |
| Number of agency visits | 1.74 | 0.70 | 0.00 | 4.00 | 598 |
| | | | | | |
| **Personality traits** | | | | | |
| Internal locus of control | 5.90 | 0.94 | 1.33 | 7.00 | 598 |
| Consciousness | 6.33 | 0.94 | 1.00 | 7.00 | 598 |
| Openness | 4.93 | 1.25 | 1.00 | 7.00 | 598 |
| Extraversion | 5.01 | 1.06 | 1.00 | 7.00 | 598 |
| Neuroticism | 3.84 | 1.20 | 1.00 | 7.00 | 598 |

*Notes:* The table provides descriptive statistics for the underlying estimation sample. The number of observations is 598, except for the reservation wage (N=559).

Table A.3: Heterogenous Effects - PBD and the Number of Applications

| Dep. Var.: Job applications | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Reform dummy | 3.854 | 7.468*** | 3.910 | 7.524*** | 3.675 | 7.100*** |
| | (2.830) | (2.795) | (2.897) | (2.904) | (2.672) | (2.717) |
| Aged 50-54 | 8.751** | 6.686 | 8.745** | 7.273 | 8.727** | 7.182 |
| | (3.810) | (5.003) | (3.876) | (5.097) | (3.699) | (4.753) |
| | | | | | | |
| **Treatment× Not UE before** | -13.286** | -12.100* | | | | |
| | (5.907) | (6.341) | | | | |
| **Treatment× UE before** | -9.787** | -11.975** | | | | |
| | (4.676) | (4.677) | | | | |
| | | | | | | |
| **Treatment× No UE experience** | | | -13.219** | -12.066* | | |
| | | | (5.984) | (6.456) | | |
| **Treatment× UE <= 12 months** | | | -8.208 | -9.398* | | |
| | | | (5.508) | (5.653) | | |
| **Treatment× UE > 12 months** | | | -11.771** | -16.053*** | | |
| | | | (4.817) | (4.895) | | |
| | | | | | | |
| **Treatment× Low-Skilled** | | | | | -11.590** | -11.585** |
| | | | | | (4.726) | (5.725) |
| **Treatment× Medium-Skilled** | | | | | -13.195*** | -14.454*** |
| | | | | | (4.684) | (4.760) |
| **Treatment× High-Skilled** | | | | | -1.155 | -3.257 |
| | | | | | (8.079) | (8.123) |
| | | | | | | |
| Adjusted-$R^2$ | 0.004 | 0.150 | 0.001 | 0.145 | 0.012 | 0.155 |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 598 | 598 | 585 | 585 | 598 | 598 |

*Notes:* The table shows the regression results of equation (7), allowing for heterogeneous treatment effects by (the length) of prior unemployment spells and skills. The dependent variable is the number of applications. Standard errors (in parantheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Heterogenous Effects - PBD and Applying for Jobs in Distant Areas

| Dep. Var.: Distant Applications | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Reform dummy | 0.076* | 0.072 | 0.085** | 0.083* | 0.077* | 0.060 |
| | (0.039) | (0.047) | (0.039) | (0.046) | (0.041) | (0.046) |
| Aged 50-54 | 0.138** | 0.193** | 0.148** | 0.218*** | 0.145** | 0.194** |
| | (0.058) | (0.081) | (0.058) | (0.081) | (0.058) | (0.079) |
| | | | | | | |
| **Treatment× Not UE before** | -0.224*** | -0.220*** | | | | |
| | (0.078) | (0.074) | | | | |
| **Treatment× UE before** | -0.188*** | -0.217*** | | | | |
| | (0.071) | (0.071) | | | | |
| | | | | | | |
| **Treatment× No UE experience** | | | -0.234*** | -0.229*** | | |
| | | | (0.077) | (0.074) | | |
| **Treatment× UE <= 12 months** | | | -0.157* | -0.178** | | |
| | | | (0.082) | (0.081) | | |
| **Treatment× UE > 12 months** | | | -0.238*** | -0.292*** | | |
| | | | (0.074) | (0.077) | | |
| | | | | | | |
| **Treatment× Low-Skilled** | | | | | -0.303*** | -0.262*** |
| | | | | | (0.087) | (0.098) |
| **Treatment× Medium-Skilled** | | | | | -0.201*** | -0.224*** |
| | | | | | (0.066) | (0.064) |
| **Treatment× High-Skilled** | | | | | -0.180 | -0.197* |
| | | | | | (0.115) | (0.110) |
| | | | | | | |
| Adjusted-$R^2$ | 0.019 | 0.149 | 0.020 | 0.151 | 0.048 | 0.135 |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 598 | 598 | 585 | 585 | 598 | 598 |

*Notes:* The table shows the regression results of equation (7), allowing for heterogeneous treatment effects by (the length) of prior unemployment spells and skills. The dependent variable is the probabilty of applying for jobs in distant areas. Standard errors (in parantheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

## Table A.5: Heterogenous Effects - PBD and Reservation Wages

| Dep. Var.: Distant Applications | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Reform dummy | -0.010 | -0.002 | -0.014 | -0.007 | -0.000 | -0.001 |
| | (0.061) | (0.035) | (0.061) | (0.035) | (0.060) | (0.034) |
| Aged 50-54 | -0.023 | 0.047 | -0.019 | 0.052 | -0.008 | 0.051 |
| | (0.081) | (0.057) | (0.081) | (0.059) | (0.079) | (0.054) |
| | | | | | | |
| **Treatment× Not UE before** | -0.031 | -0.013 | | | | |
| | (0.106) | (0.056) | | | | |
| **Treatment× UE before** | 0.065 | -0.024 | | | | |
| | (0.099) | (0.056) | | | | |
| | | | | | | |
| **Treatment× No UE experience** | | | -0.026 | -0.011 | | |
| | | | (0.107) | (0.057) | | |
| **Treatment× UE <= 12 months** | | | 0.050 | -0.040 | | |
| | | | (0.114) | (0.062) | | |
| **Treatment× UE > 12 months** | | | 0.114 | 0.004 | | |
| | | | (0.107) | (0.072) | | |
| | | | | | | |
| **Treatment× Low-Skilled** | | | | | -0.171 | -0.092 |
| | | | | | (0.178) | (0.136) |
| **Treatment× Medium-Skilled** | | | | | 0.035 | -0.029 |
| | | | | | (0.093) | (0.049) |
| **Treatment× High-Skilled** | | | | | 0.048 | 0.018 |
| | | | | | (0.138) | (0.095) |
| | | | | | | |
| Adjusted-$R^2$ | 0.001 | 0.710 | 0.010 | 0.712 | 0.060 | 0.710 |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 559 | 559 | 546 | 546 | 559 | 559 |

*Notes:* The table shows the regression results of equation (7), allowing for heterogeneous treatment effects by (the length) of prior unemployment spells and skills. The dependent variable is the probablity of applying for jobs in distant areas. Standard errors (in parantheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: PBD and Applying for Jobs in Distant Areas - Treatment Duration

|  | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Reform dummy | 0.067 | 0.072 | 0.067 | 0.068 | 0.096 | 0.100 |
|  | (0.043) | (0.055) | (0.043) | (0.053) | (0.070) | (0.072) |
| Aged 50-54 | 0.131 ** | 0.186 ** | 0.131 ** | 0.177 ** | 0.131 ** | 0.179 ** |
|  | (0.058) | (0.087) | (0.058) | (0.081) | (0.058) | (0.081) |
|  |  |  |  |  |  |  |
| **Treatment Effect** | -0.192*** | -0.212*** |  |  |  |  |
|  | (0.069) | (0.068) |  |  |  |  |
|  |  |  |  |  |  |  |
| $\times$ **Fully treated** |  |  | -0.192*** | -0.204*** |  |  |
|  |  |  | (0.069) | (0.067) |  |  |
| $\times$ **Partly treated** |  |  | -0.163 * | -0.166 * |  |  |
|  |  |  | (0.094) | (0.095) |  |  |
|  |  |  |  |  |  |  |
| $\times$ **Interview: Dec-Jan** |  |  |  |  | -0.150 ** | -0.189*** |
|  |  |  |  |  | (0.075) | (0.072) |
| $\times$ **Interview: Feb-Apr** |  |  |  |  | -0.184 * | -0.182 * |
|  |  |  |  |  | (0.097) | (0.093) |
| $\times$ **Interview: May-Jul** |  |  |  |  | -0.242*** | -0.217 ** |
|  |  |  |  |  | (0.083) | (0.084) |
|  |  |  |  |  |  |  |
| Adjusted-$R^2$ | 0.011 | 0.150 | 0.006 | 0.144 | 0.003 | 0.139 |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 505 | 505 | 598 | 598 | 598 | 598 |

*Notes:* The table shows the regression results of equation (7), focusing on differential effects due to the timing/duration of the treatment. The dependent variable indicates whether individuals apply for jobs that require moving. In Columns (1) and (2), all individuals who became unemployed prior to the reform but were interviewed thereafter are dropped. Standard errors (in parantheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

## Table A.7: Benefit duration & job search: salience of reform

| | Job applications | | Distant applications | | Reservation wage | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform dummy | 4.291 | 7.213*** | 0.070 | 0.051 | -0.026 | -0.023 |
| | (2.626) | (2.611) | (0.044) | (0.046) | (0.069) | (0.039) |
| Aged 50-54 | 9.364*** | 4.339 | 0.150** | 0.172 * | 0.026 | 0.048 |
| | (3.467) | (5.298) | (0.068) | (0.091) | (0.093) | (0.059) |
| **Treatment Effect** | -10.399 ** | -7.996 * | -0.191** | -0.177** | -0.029 | -0.025 |
| | (4.311) | (4.191) | (0.075) | (0.075) | (0.103) | (0.054) |
| Adjusted-$R^2$ | 0.001 | 0.146 | 0.007 | 0.141 | -0.004 | 0.711 |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 598 | 598 | 598 | 598 | 559 | 559 |

*Notes:* The table shows the regression results of equation (7) when defining treatment and control groups by November 12, 2007. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

## Table A.8: Benefit duration & job search: accounting for selective layoffs

| | Job applications | | Distant applications | | Reservation wage | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform dummy | 8.148 | 13.306* | 0.095 | 0.205** | -0.034 | 0.006 |
| | (6.112) | (7.046) | (0.069) | (0.089) | (0.101) | (0.075) |
| Aged 50-54 | 3.206 | 3.888 | 0.063 | 0.335** | -0.066 | 0.012 |
| | (2.557) | (7.978) | (0.088) | (0.134) | (0.141) | (0.111) |
| **Treatment Effect** | -10.714 | -19.745* | -0.206** | -0.260** | 0.097 | 0.008 |
| | (6.587) | (10.082) | (0.102) | (0.110) | (0.160) | (0.108) |
| Adjusted-$R^2$ | -0.002 | 0.331 | 0.024 | 0.136 | -0.014 | 0.610 |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 200 | 200 | 200 | 200 | 190 | 190 |

*Notes:* The table shows the regression results of equation (7) when reducing the scope of strategic firm behavior. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Benefit duration & job search: pseudo treatment effects

| | Job applications | | Distant applications | | Reservation wage | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform dummy | -0.880 | 1.205 | 0.057 | 0.053 | -0.044 | -0.052 |
| | (2.354) | (2.230) | (0.045) | (0.044) | (0.058) | (0.041) |
| Aged 50-54 | -1.303 | -5.634 | -0.026 | -0.066 | -0.014 | -0.012 |
| | (2.646) | (4.000) | (0.049) | (0.068) | (0.072) | (0.053) |
| **Pseudo Treatment** | 3.621 | 4.597 | 0.009 | 0.025 | 0.018 | 0.047 |
| | (3.129) | (3.193) | (0.061) | (0.058) | (0.085) | (0.048) |
| Adjusted-$R^2$ | -0.001 | 0.113 | 0.002 | 0.140 | -0.004 | 0.716 |
| Controls | No | Yes | No | Yes | No | Yes |
| Number of observations | 639 | 639 | 639 | 639 | 595 | 595 |

*Notes:* The table shows the regression results of equation (7) when focusing on two groups of workers who were unaffected by the reform. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$