Learning about Job Search: A Field Experiment with Job Seekers in Germany[*]

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January 18, 2016

Abstract

We conduct a large-scale field experiment in the German labor market to investigate how information provision affects job seekers' employment prospects and labor market outcomes. Individuals assigned to the treatment group of our experiment received a brochure that informed them about job search strategies and the consequences of unemployment, and motivated them to actively look for new employment. We study the causal impact of the brochure by comparing labor market outcomes of treated and untreated job seekers in administrative data containing comprehensive information on individuals' employment status and earnings. While our treatment yields overall positive effects, these tend to be concentrated among job seekers who are at risk of being unemployed for an extended period of time. Specifically, the treatment effects in our overall sample are moderately positive but insignificant. At the same time, we do observe pronounced and statistically significant effects for individuals who exhibit an increased risk of long-term unemployment. For this group, the brochure increases employment and earnings in the year after the intervention by roughly 4%. Given the low cost of the intervention, our findings indicate that targeted information provision can be a highly effective policy tool in the labor market.

[*]We thank Alberto Abadie, Björn Bartling, Stefano DellaVigna, Larry Katz, Eva Ranehill, Alexandra Roulet, Frédéric Schneider, Frederik Schwerter, and Roberto Weber as well as conference and seminar participants in Bonn, Edinburgh, Hamburg, London, Mainz, Nottingham, Paris, and Zürich for helpful comments and discussions. We also thank Susanne Koch, Steffen Künn, Christopher Osiander, Georgios Tassoukis, and the team of IAB-ITM for support with the data and the setup of the experiment. Maren Erdmann, Nicolas Kaufung, Patrizia Odyniec, and Thomas Wasilewski provided excellent research assistance.

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1 Introduction

Job search is a complex and daunting endeavor. Besides the substantial economic losses that unemployment typically entails, job seekers face a variety of non-trivial informational problems when looking for new employment opportunities. They need to inform themselves, for instance, about which firms or industries value their qualifications most and whether these are currently hiring new employees. Moreover, they have to decide how much search effort to exert, which search channels to use, and what kinds of jobs to target, e.g., in terms of occupations or geographical location. Job search is further complicated by the fact that there is relatively little information and feedback about important parameters of the search process, such as individual returns to search effort, path dependence, or the degree to which job arrival rates depend on the breadth of one's search. This opens up the possibility for a variety of biases in individuals' beliefs to affect their behavior.[1]

In addition to such informational challenges, job seekers also need to overcome general frustration, deterioration in life satisfaction, discouragement from rejected applications, and further personal setbacks that unemployment and the job search process often bring about. Clark and Oswald (1994), Krueger et al. (2011), and Krueger and Mueller (2012b), for instance, document substantial unhappiness and dissatisfaction among the unemployed, in particular during times of active job search. The search process and the circle of trying and failing that job seekers often experience also puts a strain on their self-confidence, patience, and willpower (e.g., Falk et al. 2006). Finally, returns to job search typically accrue with considerable delays, which in turn can lead to sub-optimally low levels of search effort when people are present-biased (e.g., DellaVigna and Paserman 2005, Paserman 2008). Summarizing the challenges faced by job seekers, Babcock et al. (2012) note that "...looking for work is, in the first place, a substantial information problem. Workers have to understand labor market conditions, have knowledge of openings and applications processes, possess an accurate understanding of their own skill level and how firms and markets might value those

[1] While job seekers do not necessarily have to understand these concepts per se, biases in individuals' perceptions of job arrival rates and their determinants can cause inefficiencies in terms of search intensity or the use of different search channels (see, e.g., Belot et al. 2015, Caliendo et al. 2015, McGee 2015, Spinnewijn 2015).

skills, and determine the quality of matches with employers. Moreover, searching for work requires willpower, which can be costly for individuals to draw upon."

Motivated by the behavioral and informational challenges that job seekers face, we investigate in this paper whether providing unemployed individuals with information about job search and the consequences of unemployment can improve their employment prospects and later labor market outcomes. We study this question in a field experiment among roughly 54,000 job seekers in Germany. In our experiment, people who had recently entered unemployment were randomly allocated to a treatment or control condition. Individuals assigned to the treatment group were sent a letter that contained a short information brochure. Job seekers in the control group did not receive the brochure, while otherwise facing identical conditions in terms of employment services, job search assistance, etc.

The brochure was designed to operate through two main channels. First, we provided concise and easy-to-understand *information* about the current labor market situation and selected research results from behavioral, labor, and public economics related to unemployment and job search. Second, the brochure aimed at *motivating* job seekers and encouraged them to actively search for new employment. The content of the brochure can be summarized as follows: First, individuals in the treatment group were informed about stylized facts regarding the state of the economy and the labor market situation at the time of the experiment. Second, the brochure provided a simple illustration of duration dependence (e.g, Jackman and Layard 1991, Kroft et al. 2013, Schmieder et al. 2013) and emphasized the importance and effectiveness of active job search, building on Spinnewijn (2015). The third part of the brochure informed job seekers about research on the non-pecuniary consequences of (un)employment, e.g., with respect to overall life satisfaction, health, or the stability of personal relationships (e.g., Clark and Oswald 1994, Björklund and Eriksson 1998, Eliason 2012). Finally, the brochure featured information about job search strategies. In particular, it emphasized the relevance of using a variety of formal and informal search channels for finding new employment (e.g., Holzer 1988, Topa 2011, Kuhn and Mansour 2014).

To investigate how the brochure affects job seekers' labor market outcomes, we combine information on treatment assignment with administrative data from social security records. Our data set contains comprehensive information on individuals' labor market outcomes in terms of employment and earnings after the intervention. Comparing these outcomes between treated and untreated individuals in administrative data enables a clean identification of the average causal effects of the brochure untainted by measurement issues such as attrition bias. In addition, our data set contains extensive information on sociodemographic characteristics as well as on individuals' employment history, allowing us to identify treatment effects for subgroups of individuals that differ based on pre-determined characteristics. This is important given that different groups of job seekers are likely to vary systematically in the degree to which they need the type of information and encouragement that our brochure provides.

The treatment effects in the overall sample can be summarized as follows. Over the course of one year after the intervention, individuals in the treatment group are, on average, employed for approximately 1.2 to 1.4 additional days relative to the control group. The corresponding increase in cumulative earnings for treated individuals amounts to EUR 144 to 155. While the point estimates are positive in both dimensions, they are relatively imprecisely estimated, rendering coefficients insignificant.

In addition to the identification of average treatment effects, the randomized nature of our intervention allows us to obtain unbiased estimates of the impact of the brochure in specific subgroups of the population. There has been a growing interest to better understand potential heterogeneities in the treatment effects of policy reforms and other interventions, guided by both theoretical arguments (e.g., Bitler et al. 2006) as well as policy considerations. We focus on a subgroup of job seekers that is of particular interest in both of these respects: individuals who are at risk of being unemployed for an extended period of time. As documented in DellaVigna and Paserman (2005), Paserman (2008), Dohmen et al. (2009), and Spinnewijn (2015), there is a tight theoretical and empirical link between the prevalence of behavioral biases—specifically present bias as well as biases in probability judgment and confidence—and longer unemployment duration. As our brochure aims at tackling the informational and behavioral challenges that job searchers face, it is plausible that treatment effects are concentrated among individuals at risk of long-term unemployment, since the challenges themselves are likely to be particularly pronounced in this group. In addition to these conceptual reasons, the group of job seekers with a higher risk of long-term unemployment is also of particular interest from a policy perspective. Combatting long-term unemployment is a key policy objective that has attracted the attention of policy makers and researchers alike (see, for example, Black et al. 2003 and Blundell et al. 2004 for the evaluation of policies aimed at reducing long-term unemployment in the US and UK). In Germany, the Hartz reforms—arguably the most comprehensive post-war labor market reforms—were implemented with an explicit goal of reducing long-term unemployment (see Hartz 2002).

To evaluate the causal effects of our treatment among individuals at risk of long-term unemployment, we first identify a subsample of job seekers whose pre-determined individual characteristics lead to a long predicted non-employment duration. We then evaluate labor market outcomes for treated and untreated individuals in the at-risk group, documenting strongly positive and statistically significant treatment effects for this group. Specifically, we find that the brochure causes an increase in cumulative employment and earnings in the year after the intervention of about 4% (4.7 days and EUR 450, respectively), relative to the corresponding group of at-risk individuals in the control treatment. Notably, the employment effects are concentrated in jobs with monthly earnings of more than EUR 1000. This indicates that the increase in employment does not come at the cost of lower wages and suggests that the brochure improves the employment prospects of individuals at risk of long-term unemployment without having detrimental consequences for the quality of resulting matches.

In a recent study, Abadie et al. (2013) have pointed out that endogenous stratification approaches like the one employed by us to determine the at-risk group can lead to substantial biases in the estimated treatment effects. To address this potential concern, we explore the robustness of our results using repeated split-sample estimators as suggested by Abadie et al. (2013). Reassuringly, the estimated increases in employment and earnings are almost identical in specifications using the repeated split-sample estimator, suggesting that the strong positive estimates for the treatment effects in the at-risk group are not spurious. Results are also qualitatively robust to considering alternative definitions of the at-risk group, e.g., based on previous unemployment durations or the wage level prior to the intervention. Taken together, our findings thus suggest that targeted information provision can be an effective policy tool to improve employment prospects of job seekers, at least in important subgroups of the population. Moreover, in light of the low costs of our treatment—the total cost for production and mailing amount to less than EUR 1 per brochure—the findings imply a tremendously positive cost-benefit ratio of the intervention.

Our findings contribute to the literature that examines the labor market effects of job search assistance, training and counseling programs, and other policies to encourage job search (see Heckman et al. 1999 and Card et al. 2010 for comprehensive overviews). Most directly related from a methodological perspective is the recent strand of the literature that uses randomized controlled interventions to study the causal effects of labor-market policies (e.g., Crépon et al. 2013, Behaghel et al. 2014, Belot et al. 2015). In the analysis of the welfare effects of such policies, a key consideration is whether negative displacement effects on untreated job seekers exist. For a large-scale labor market program in the UK, Blundell et al. (2004) find no evidence of such displacement effects. More recently, however, Crépon et al. (2013) document evidence for negative employment effects of a job placement program in the French labor market on untreated job seekers based on a clustered experiment.[2]

Our paper also adds to an emerging literature that analyzes the consequences of information provision in a variety of other economic applications, such as education and school choice (Jensen 2010, Hastings and Weinstein 2008), payday borrowing (Bertrand and Morse 2011), retirement saving (Duflo et al. 2006, Saez 2009), the choice of health insurance plans (Kling et al. 2012), compliance with rules, laws, and other regulations (Fellner et al. 2013, Apesteguia et al. 2013), or teenage sexual behavior (Dupas 2011). Perhaps most closely related to our study are the papers by Chetty and Saez (2013) and Liebman and Luttmer (2015).

[2] Crépon et al. (2013) also provide evidence that this externality is strongest in labor markets with high levels of unemployment. As our study was conducted in a time period of a tightening labor market in Germany, with the federal unemployment rate falling from 7.7% to 7.1% from 2010 to 2011, we would expect potential displacement effects to be limited in the context of our study.

Chetty and Saez (2013) conduct a field experiment in which EITC recipients are provided with personalized information about the EITC schedule. Although the overall effect of their intervention turns out to be limited, they find that individuals who were matched with tax professionals that comply more strongly with the treatment exhibit a significant change in earnings in the year after the intervention. Liebman and Luttmer (2015) test if providing older workers with information about the social security system affects their labor supply behavior. Studying a field experiment that involves an information brochure and the opportunity to participate in an online tutorial, they document a general increase in labor force participation in response to their treatment, with particularly pronounced effects among female participants.

Our paper complements a number of theoretical models and observational studies that investigate unemployment and job search through the lens of behavioral economics.[3] DellaVigna et al. (2014), for instance, analyze the implications of reference-dependent preferences for job search behavior. They demonstrate that reference dependence is consistent with a variety of stylized facts in individual search and job finding patterns, and can also account for the observed reactions of job seekers to benefit cuts and other changes in the unemployment insurance system. Krueger and Mueller (2010) and Krueger and Mueller (2012a) study the search process based on time-use data and document surprisingly low amounts of time devoted to job search among unemployed individuals. For instance, the percentage of unemployed workers who search for a job on a given day is typically below 20% and, conditional on searching, the amount of time devoted to job search is less than one hour per day in many countries. A number of papers suggest that these low amounts of time devoted to job search may indeed be suboptimal, for instance, because of biases in beliefs about job finding rates and returns to search effort (Spinnewijn 2015) or present-biased time preferences (DellaVigna and Paserman 2005, Paserman 2008). Biases in beliefs and resulting differences in search intensities might also be related to personality factors, such as people's tendency to consider life outcomes as mainly determined by external vs. internal factors (see Caliendo et

[3]See also Thaler and Sunstein (2008) and Chetty (forthcoming) for a detailed discussion and a recent overview of policy interventions in other domains that are inspired by research in behavioral economics.

al. 2015 as well as McGee 2015).

At a more general level, our intervention can be viewed as an attempt to study the effects of communicating economics research to a set of individuals whose decisions may be improved by having access to the findings from this research. In this spirit, our brochure condenses findings from research in labor, public, and behavioral economics aimed at investigating the causes and consequences of unemployment, and presents these findings in a simplified form to the very population that is the subject of this line of research. Our findings indicate that the insights from these studies—referenced above—do not only advance our understanding of important labor market phenomena, but that they can also directly help people make complex economic decisions.

The paper proceeds as follows. In the next section, we present the design of our field experiment. Section 3 discusses our empirical results and Section 4 concludes.

2 Design of the Experiment

To investigate whether providing unemployed individuals with information about job search and the consequences of unemployment—based on research in labor and behavioral economics—we conducted a field experiment among job seekers who had recently entered unemployment in the German labor market. Our treatment intervention consisted of an information brochure that aimed at addressing some of the key challenges that job seekers face in terms of the *information* and *motivation* that is needed to find new employment.

2.1 The Brochure

The brochure consisted of four parts, each containing information selected to help people overcome the informational challenges and to foster their motivation for the search process. The text blocks as well as accompanying illustrations were formulated in a concise and easily accessible manner. The four parts of the brochure can be summarized as follows:[4] The

[4] A picture of the brochure, a full English translation of the text blocks, and a detailed summary of the references we used for designing the brochure can be found in Appendix B.

first text block contained basic stylized facts about the economic environment and the labor market situation in Germany at the time of the intervention. It emphasized the positive state the economy was in and that "now is the ideal time" to successfully search for new employment. Specifically, job seekers were informed that the economy—which had experienced a substantial decline in 2009—had started to recover (while German GDP had declined by 5.1 percent in 2009, growth rates in 2010 and 2011 were 4.0 and 3.3 percent, respectively). Furthermore, it was mentioned that many companies were hiring new employees, and that "several hundred thousand vacancies" were available at the time of the experiment.

The second part of the brochure informed job seekers about duration dependence and returns to search effort. In particular, the text and two auxiliary figures illustrated in simple terms the negative association between unemployment duration and job finding rates (e.g., Jackman and Layard 1991, van den Berg and van Ours 1996, Kroft et al. 2013, Schmieder et al. 2013). Moreover, it emphasized the importance of personal search effort for successful job search, and mentioned evidence that many people tend to underestimate the returns to search effort (Spinnewijn 2015).

The third part of the brochure summarized evidence on the relationship between unemployment and life outcomes related to health, family, and life satisfaction. For instance, it mentioned evidence on positive associations between employment and health status (e.g., Björklund and Eriksson 1998, Gerdtham and Johannesson 2003, Eliason and Storrie 2009), stability of marriages and other personal relations (e.g., Jensen and Smith 1990, Kraft 2001, Eliason 2012), and overall life satisfaction (e.g., Clark and Oswald 1994, Winkelmann and Winkelmann 1998, Kassenboehmer and Haisken-DeNew 2009).

Finally, the fourth text block as well as the back side of the brochure provided information on the returns of using various alternative search channels. It emphasized the relevance of social networks for finding a new job (Calvo-Armengol and Jackson 2004, Dustmann et al. 2011, Burks et al. 2013, and Brown et al. forthcoming; see also Topa 2011 for a comprehensive overview of the literature), and pointed people to a number of other complementary search channels (e.g., direct unsolicited applications and the online job search platforms of the employment agency and private providers; see, e.g., Holzer 1988, Kuhn and Mansour 2014). The text also mentioned that feelings of frustration during the process of job search are normal and that job seekers should not be discouraged by rejected applications.

As emphasized, for instance, in Babcock et al. (2012), job search is to a large extent an informational problem. Accurate beliefs about various aspects of the job search process are key to search effectively, while misperceptions—for instance regarding the returns to search effort—can lead to suboptimal search behavior (see, e.g., Spinnewijn 2015). The information provided in our brochure thus aimed at communicating important research findings related to job search, with the ultimate goal to facilitate individuals' search for a job. At the same time, the provision of encouragement and motivation was an equally important feature of our intervention: the search process can lead to substantial frustration among job seekers, and the (repeated) experience of rejections may make it very difficult to maintain motivation. Low motivation and low levels of search effort can also be the consequence of present-biases, as demonstrated in DellaVigna and Paserman (2005) and Paserman (2008). In light of these challenges, the motivational components of our brochure were meant as a means to bolster individuals' motivation. In particular, throughout the brochure, we presented all information in a way that tries to encourage job seekers to actively search for new employment. For instance, rather than describing the negative impact of unemployment on health and life satisfaction, we highlighted the non-pecuniary *benefits* of finding new employment in terms of these dimensions.

2.2 Procedures

The brochure was sent out in four consecutive waves between October 2010 and January 2011 to individuals who had recently registered as unemployed. There is a gap of 4-8 weeks between the point in time where a person enters unemployment and the date at which she receives our information brochure. This gap is mainly due to the fact that job seekers in Germany register as unemployed at their local job center, whereas our randomization is based on register data of the Federal Employment Agency. Depending on the cutoff dates in the reporting system, it can take several weeks until the local information is available in the central registry. More specifically, towards the end of the first week of a given month, we received information on all individuals that had registered as unemployed at their local job center in a time window of 3-7 weeks before that date. Subsequently, job seekers were randomized into the treatment and control group. Finally, the brochure was sent out via postal mail, such that treated job seekers received the brochure towards the end of the second week in a given month.[5]

For each of our four waves, we drew a random sample of 10,000 individuals for our treatment group and 30,000 for the control group.[6] When drawing samples for the experiment, we imposed a number of sample restrictions. First, to rule out language difficulties in understanding the content of the brochure, we focused on German citizens. Second, we excluded individuals younger than 25 and older than 55, to avoid potential contortions of our treatment effect due to peculiarities in the labor markets of people who are close to retirement or have just finished high school or college. Furthermore, we excluded individuals who re-registered as unemployed after having participated in a training program sponsored by the employment agency or other programs of active labor market policy. Finally, our data does not involve civil servants and self-employed individuals (see Section 2.3).

The brochure was sent out by regular mail, together with a short accompanying letter that informed the recipients that the brochure was designed in a collaboration between researchers at the University of Bonn and the Institute for Employment Research of the Federal Employment Agency (IAB). On the back side of the brochure, we provided an email address as well as a phone number that we had set up for the study in case recipients of the brochure had questions or feedback related to the brochure.[7]

[5]When registering as unemployed, job seekers in Germany are provided with other information leaflets, personal counseling services, etc. The specific information that a particular job seeker gets depends on her region of residence, education, and other variables. Our randomization procedure ensures that our treatment is orthogonal to these other services.

[6]Note that, because of the gap in the reporting system, many of these individuals were already back into employment at the time at which we draw samples. Our empirical analysis below focuses on those individuals in the treatment and control group who were still looking for a job at the time of our intervention (see also Section 2.3).

[7]Relatively few participants (a total of 183 over the course of the experiment) contacted us with questions or feedback. Most inquiries related to questions or concerns about data privacy and could be addressed rela-

It is important to bear in mind that all analyses reported in Section 3 capture an intention-to-treat (ITT) effect. In other words, we do not know to what extent individuals in the treatment group actually "digest" the information we provide. This is an inherent feature of information interventions via mailing. It is, for instance, conceivable that some people do not open the letter or do not read through the entire brochure.[8] While from a policy perspective the treatment differences we are identifying are the relevant ones, one might argue that we are measuring only a lower bound of the treatment "reading and understanding the contents of the brochure". Ultimately, the latter effect could only be examined with more invasive and costly interventions where it can be *ensured* that information is absorbed and understood by all treated individuals (e.g., counseling or training programs).

2.3 Data and Empirical Approach

In our empirical analysis in Section 3, we aim at identifying the causal impact of our information brochure on the employment prospects and labor market outcomes of unemployed individuals. We therefore exclude all individuals in both the treatment and control group who were already back in employment at the point in time at which we sent out our brochure.[9] Specifically, due to the time gap in the reporting system described above, 66.3% of the originally sampled individuals in the treatment group as well as 66.4% of individuals in the control group had already found a job again before our intervention started. After excluding these individuals, we are left with a total of 53,753 observations for our empirical analysis (13,471 and 40,282 in the treatment and control group, respectively).

An important feature of our study is that we are able to match data on treatment status with official registry data (so-called Integrated Employment Biographies or IEB[10]). The inte-

tively easily by explaining that the study was fully in line with privacy laws and that the empirical analysis was exclusively based on anonymized data.

[8]In addition, about 1.9% of letters (749 in total) were returned to sender as undeliverable, presumably because of changes in the recipients' address which were not yet updated in the register data. The corresponding individuals remained in our dataset, such that the treatment and control group are treated symmetrically and the reduced-form estimates of treatment effects remain unbiased.

[9]We also excluded a tiny fraction of individuals who were sampled more than once.

[10]Specifically, the registry data we use comes from the "IAB Integrierte Erwerbsbiographien (IEB)", Version 11.00, 2013, the "IAB Beschäftigtenhistorik (BeH)", Version 09.03, 2013, the "IAB Arbeitssuchendenhistorik (ASU)", Version 06.04, 2013 and the "IAB Leistungsempfängerhistorik (LeH)", Version 07.01, 2013. For a more

grated employment biographies are assembled by the German Federal Employment Agency for administrative purposes and are highly reliable as they are collected as part of administrative processes of the social insurance system. Using this data allows us to precisely track individuals' labor market status before, during, and after the intervention. Moreover, it avoids problems that can plague studies based on voluntary surveys, e.g., attrition and misreporting.

The population for which the IEB are recorded include employees with employment subject to social security contributions, marginal employment, and unemployed individuals. Importantly, data on civil servants or the self-employed is not included. Variables that are reported in the IEB include employment status, labor market data such as earnings or occupations, and personal characteristics such as year of birth, gender, or education. Table 1 provides summary statistics for key variables that we use in our study.

Information in the IEB is reported in spells. Our study is based on spells from 2001 until 2012. This allows us to observe individuals' labor market status up to ten years before the intervention and to track outcomes up to one year after the intervention. We estimate models of the following kind:

$$Y_i = \alpha + \beta \cdot T_i + X_i\gamma + \epsilon_i$$

As dependent variables, Y_i, we focus on two key measures of individuals' labor market performance after the intervention. The first, $Cumulative\_Employment_i$, measures the total number of days that individual i has been employed from the beginning of the intervention (i.e., the week in which the brochure was sent out) until one year (52 weeks) after the intervention. Correspondingly, $Cumulative\_Earnings_i$ measures the sum of an individual's labor market earnings during the year after the intervention.[11] T_i is a dummy for treatment

detailed introduction to the IEB data in general, see Jacobebbinghaus and Seth (2007) and Oberschachtsiek et al. (2009) who describe datasets based on the IEB.

[11] $Cumulative\_Earnings_i$ is generated from the gross daily wage during an employment spell, as reported in the IEB data ("Tagesentgelt"). The variable is set to zero for individuals who are not employed. The variable is capped at the maximum level of earnings upon which social security contributions are levied (higher levels are not reported in the social security data). Note that this maximum level differs from year to year and between East and West Germany. To be consistent across years 2010 and 2011 and East and West Germany, we cap the (monthly) wage at a level of EUR 4,625. We do not impute wages above this limit as the fraction of individuals earning above the maximum level in our sample is negligible.

status; β thus captures the effect of the treatment on outcome Y. In some specifications, we include a set of control variables, X_i (described in more detail in Table 1 and Section 3 below). Throughout our analysis, we allow for heteroskedasticity of the error term ϵ_i and estimate heteroskedasticity-robust standard errors (Huber 1967 and White 1980). As treatment status is randomly assigned, T_i is by construction orthogonal to X_i and ϵ_i, so that β identifies the average causal effect of the treatment even without controlling for X_i.

2.4 Balancing Tests

Table 1 provides an overview of participants' sociodemographic characteristics and a number of other summary statistics for the treatment and control group. The job seekers in our experiment are on average 37 years old, roughly 54% of them are male, and less than 15% have a university-level degree. The average participant in our sample was unemployed for almost 900 days during the last ten years before the intervention and earned about EUR 1,580 per month in her last job before registering unemployed, illustrating that our intervention was conducted in a sample with relatively bad labor market prospects. As an indicator for local labor market characteristics, we merged information on unemployment rates in individuals' districts of residence in 2010 to our data.[12] The average local unemployment rate for individuals in our sample is 8.3%.

The figures in Table 1 illustrate that sociodemographic characteristics as well as local labor market conditions are balanced across the treatment and control group. Balancing tests demonstrate that the experiment succeeded in achieving balanced groups across treatments (see column (3) of Table 1). We find no statistically significant differences in demographic characteristics between the treatment and control group, except for the case of one education category—individuals with upper secondary school leaving certificate and a vocational qualification—which is slightly less represented in the treatment group (the corresponding fractions in the treatment and control group differ by 0.6 percentage points, with a t-statistic of 2.35). To assess whether covariates significantly predict treatment status, we estimate a

[12]More precisely, we merge unemployment rates (Bundesagentur für Arbeit 2011) in a participant's last reported district of residence before the beginning of the intervention.

linear probability model in which we regress a dummy variable for treatment status on the set of covariates described above, as well as state and treatment-wave fixed effects. Results of the regression are reported in Table 2 and show that only one out of 29 regressors—the dummy variable for the education category described above—is statistically significant at the 5% level. Overall, an $F-$test does not reject the hypothesis that the regressors are jointly insignificant ($p = 0.76$), which further indicates that the covariates are balanced across treatment and control group.

2.5 Behavioral Hypotheses

The hypothesis we want to test is simple: does our intervention improve the employment prospects and labor market outcomes of treated job seekers? As illustrated above, our brochure was designed to assist unemployed individuals in their search for a new job, by providing them with information about job search and the consequences of unemployment, and by motivating and encouraging them to actively look for new employment. Providing additional information on job search and different search channels should generally increase the effectiveness of people's search. Moreover, if individuals have biased beliefs about job arrival rates and their determinants, the information we provide should help attenuate such biases. For instance, Spinnewijn (2015) suggests that people spend too little effort on job search because they underestimate the returns to individual search effort. If the information provided in our brochure helps to "debias" such distorted beliefs, search effort should increase among treated job seekers. Both, higher levels of search effort and more effective search should manifest themselves in higher job finding probabilities and, consequently, higher cumulative employment levels in the treatment group.

The motivational features of the brochure should reinforce this effect. For instance, a number of studies document that present-biased time preferences are associated with suboptimally low levels of search effort (e.g., DellaVigna and Paserman 2005, Paserman 2008). Providing additional motivation might thus encourage present-biased individuals to overcome their tendencies to procrastinate the exertion of (costly) search effort. More generally, the search process and the repeated experience of "trying and failing" that job seekers often encounter might deplete their confidence, willpower, and motivation (e.g., Falk et al. 2006, Babcock et al. 2012, Caliendo et al. 2015). If the brochure succeeds in restoring their motivation and encourages them to continue searching for a new job, this should have positive effects on the labor market outcomes of treated individuals.

Note that different groups of job seekers are likely to differ systematically in the degree to which they need the type of information and encouragement that our brochure provides. As a result, the effects of the brochure are also likely to vary across different subgroups of the population. Based on similar considerations, Bitler et al. (2006) emphasize the importance of analyzing systematic heterogeneity in the treatment effects of labor market interventions and other policy reforms. One group of job seekers that naturally attracts attention in this regard are individuals at risk of long-term unemployment. The previous literature suggests a tight theoretical and empirical link between a number of behavioral biases—specifically present bias, biases in probability judgments, and overconfidence—and longer unemployment duration (e.g., DellaVigna and Paserman 2005, Paserman 2008, Dohmen et al. 2009, and Spinnewijn 2015). Furthermore, individuals at risk of long-term unemployment tend to have lower earnings and educational levels, both of which are associated with lower levels of cognitive skills. Low cognitive skills, in turn, have also been found to predict a high prevalence of behavioral biases (see, e.g., Dohmen et al. 2010, Benjamin et al. 2013). In sum, our brochure might have particularly strong effects for individuals at risk of long-term unemployment, since the behavioral and informational challenges that it addresses are likely to be especially pronounced in this group of job seekers.[13]

[13] Notice that the "at-risk" group is not only of particular interest because one would expect treatment effects to be particularly pronounced for this group. Rather, policy-makers and researchers alike have generally been worried about individuals at risk of long-term unemployment, for instance because of the loss of skills it entails, and many policy interventions have been specifically targeted to tackle long-term unemployment (see for example Black et al. 2003 and Blundell et al. 2004).

3 Results

In this section, we summarize the main results of our experiment. We first consider the overall sample described in Section 2.3 and analyze treatment effects for individuals' cumulative employment and earnings after the intervention. In a second step, we analyze the consequences of our treatment for job seekers that are at risk of being unemployed for an extended period of time. In the final part of our analysis, we explore the timing patterns of our treatment effects in more detail and report the results from a series of robustness checks. All figures and tables can be found in Appendix A.

3.1 Results for the Overall Sample

Column (1) of Table 3 reports the treatment effects of the brochure on cumulative earnings and employment for our overall sample. One year after the intervention, treated job seekers have, on average, worked for about 1.2 days more than individuals who did not receive the brochure. Furthermore, job seekers in the treatment group accumulated roughly 150 euros of additional earnings over the year after the intervention. While the point estimates indicate a generally positive influence of the brochure, the employment and earnings effects in our overall sample turn out to be insignificant. The table also presents additional specifications that control for individual-level as well as market-specific characteristics (see Column (2) of Table3). The set of control variables includes basic sociodemographic characteristics (gender, age, education categories), information on individuals' labor market history (the job seeker's last wage before the beginning of the intervention as well as the overall length of her unemployment spells during the past ten years), local labor market characteristics, and state as well as treatment-wave fixed effects. Note that, for both outcomes, the inclusion of control variables hardly affects the point estimates of treatment coefficients, in line with our earlier observation that randomization into treatments was successful.

3.2 Individuals at Risk of Long-Term Unemployment

In a next step, we analyze the impact of the brochure on the group of individuals who are at risk of being unemployed for a particularly long period of time. To identify treatment effects for this subgroup, we first estimate a summary index of predicted unemployment duration and classify job seekers who score high on this index as the "at-risk group". Specifically, we implement an estimation framework in which we use the data from the control group to estimate the determinants of an individual's overall unemployment duration in the year after the beginning of the experiment,[14] using basic sociodemographic characteristics (gender, age, education) as well as information on local labor markets (local unemployment rates) and individuals' employment history (previous wage, length of previous unemployment spells) as explanatory variables. We then use this model to predict the expected unemployment duration for each individual in our sample. That is, for both the treatment and control group, we predict the job seekers' expected overall unemployment duration in the absence of treatment, based on individual observables and local labor market characteristics.

The treatment effects for the subgroup of job seekers with above-median index values of predicted unemployment duration are summarized in columns (3) and (4) of Table 3. One year after the beginning of the intervention, treated job seekers have accumulated more than 4 additional days of employment compared to their counterparts in the control group. This effect is not only sizable, but also statistically significant ($p<0.01$). Similarly, accumulated earnings are significantly higher (EUR 450) for job seekers who received the brochure ($p<0.05$). The treatment effects are highly robust to the inclusion of additional controls. In columns (5) and (6) of Table 3, we also depict regression results for the counterfactual group of job seekers who exhibit a particularly low risk of long unemployment durations. The regressions for this subpopulation yield negative point estimates for the treatment effects on cumulated employment and earnings. These negative estimates, however, are substantially smaller in magnitude than the positive effects for the at-risk group and are not statistically

[14] More precisely, the dependent variable in our estimation framework is the total number of days in the year after the intervention for which a given individual in the control group has no employment spell. For further details and estimation results, see Table 5 in Appendix C.

significant in any of the specifications.

3.3 Robustness Checks

In a recent paper, Abadie et al. (2013) raise an important methodological concern for research designs based on randomized experiments, in which researchers aim to estimate treatment effects for subgroups of individuals that are "most in need of help". When such subgroups are endogenously created—as in our setting focusing on individuals with a high summary index of characteristics that predict the outcome in the absence of treatment—the estimated treatment effects for the subgroups can be substantially biased in finite samples.[15] Abadie et al. (2013) propose repeated split sample estimators as a remedy for this bias, as such a procedure does not lead to biased estimates of subgroup treatment effects. To implement this estimator, the sample in the control group is split such that observations that are used for the estimation of the index of characteristics are not used for the estimation of treatment effects.

We implement such repeated split sample estimators for our setting and report results in Table 4, based on 100 repetitions of the Abadie et al. (2013) estimator. Reassuringly, we find that our original results for both cumulative employment and earnings are almost identical to the ones based on the repeated split sample estimator. We conjecture that the number of observations in our control group ($N_C \simeq 40,000$) is so high that the finite sample bias documented by Abadie et al. (2013) for several other settings is quantitatively negligible in our experiment. In Table 4, we also report results for the group of individuals at low risk of unemployment. The analysis reveals again that the point estimate of the treatment is negative for the two main outcome variables. However, the absolute magnitudes are much smaller than the corresponding estimates for the high-risk group and, in addition, far from being statistically significant.

As a further robustness check, we also consider several other definitions of subgroups

[15] The intuition for the source of this bias is that unobserved shocks that affect the outcome variable will also affect the probability with which an individual in the control group will be classified as a member of a particular subgroup. Loosely speaking, individuals from the control group who are classified as "most in need of help" will be particularly negatively selected with respect to the outcome variable.

at risk of long-term unemployment, based on exogenous pre-treatment characteristics. The overall results we observe in terms of treatment effects on employment and earnings are very similar as in our previous analysis. We summarize estimation results for different subgroups in Table 6 in Appendix C. Columns (5) and (6) of Table 6, for instance, show results for job seekers with long overall unemployment duration prior to the experiment. Columns (7) and (8) display the corresponding findings for individuals who earned below-average daily wages (less than EUR 50) in their last job before the experiment. Effects are also similar, though somewhat less pronounced, if we consider people's educational level as our indicator of the at-risk group (see columns (9) and (10)).

Finally, we further explore the heterogeneity in treatment effects parametrically and report results for models in which we interact the variables reported in Table 1 with treatment status. The results in Table 7 in Appendix C document that almost all interaction terms are too imprecisely estimated to reject a null hypothesis of no interaction with treatment status for specific individual characteristics. In line with our previous analysis of subpopulations at risk of long-term unemployment, however, we find that the treatment is particularly effective for low-wage individuals: the interaction effect of an individual's previous daily wage with the treatment dummy on employment measured over 52 weeks after treatment is negative and statistically significant even when controlling for all other control variables interacted with treatment status. This result also holds true for cumulative earnings as outcome variable. We further find some evidence that the treatment could be particularly effective for older individuals: specifically, in the specification using employment as the outcome variable, high-age individuals exhibit significantly stronger treatment effects.

3.4 Timing and Margins of the Treatment Effects

In a recent metastudy, Card et al. (2015) analyze the timing patterns in the treatment effects of active labor market policies. Using data from more than 200 different evaluations of various policy interventions, they demonstrate that policies that were effective in improving labor market prospects of job seekers tyically exhibit a substantial delay between the time of the intervention and the time at which positive effects of the treatment emerge. Inspired by these findings, we further investigate the temporal dimension of the observed treatment effects for the at-risk group. In order to illustrate these effects, we consider differences in employment and earnings in the week immediately after the intervention and, consecutively, in four-week intervals from week 4 until week 52 after treatment. Thus, we estimate models of the following kind:

$$Y_{it} = \alpha_t + \beta_t \cdot T_i + X_i\gamma_t + \epsilon_{it}$$

As dependent variables, we consider the measures $Cumulative\_Employment_{it}$ and $Cumulative\_Earnings_{it}$, evaluated in week t after the intervention, as well as two additional measures that capture the strength of treatment effects at a given point in time. $Employment_{it}$ is an indicator for whether individual i has an employment spell at time t. Correspondingly, the variable $Wage_{it}$ measures labor market earnings of individual i in week t after the intervention.

Figure 1 depicts the timing effects for the at-risk population of job seekers. The figure indicates that the treatment effects on employment and earnings are positive throughout the observation period, but most pronounced in the second six months after the beginning of the intervention. Thus, consistent with the findings presented in Card et al. (2015), the effects of the brochure cumulate and only become visible with a delay (see panels in the right column of Figure 1). Qualitatively, this pattern is also observed in our overall sample, although effects are generally weaker and not statistically significant (see Figure 2). We suggest two possible explanations that are consistent with this pattern. First, DellaVigna et al. (2014) show that job seekers with reference-dependent preferences tend to exert particularly high levels of search effort at the beginning of the unemployment spell, in order to overcome the losses in income that unemployment entails. In contrast, search efforts tend to be lower in later periods of the unemployment spell, once job seekers' income reference points have adjusted.[16] In such a setting, there is more scope for any intervention to affect individuals'

[16]Finally, DellaVigna et al. (2014) show that search effort might rise again towards the date at which an individual's unemployment insurance benefits expire, consistent with the frequently observed spike in job finding rates around the exhaustion date.

search behavior later in the unemployment spell, where baseline search effort is relatively low. An alternative mechanism that is consistent with our findings are "storable job offers": Boone and van Ours (2012) provide and test a model in which workers might negotiate a delayed starting date for a newly found job if they can receive further unemployment benefits before taking up the job. As individuals in our data are only recorded as employed once they have actually started to work, the model in Boone and van Ours (2012) could explain a delay in the measured effect of the treatment on people's employment prospects.[17] Consistent with both of these explanations, the minimum duration of unemployment benefits for eligible individuals in Germany is 6 months, indicating that there is scope for both reference-point adaptations as well as for workers exploiting the possibilities of storable job offer.

In a final step, we want to shed further light on the margins of employment at which our treatment operates. To do so, we consider two additional outcome variables that help illustrate the type of jobs that individuals in the treatment group take up. First, we consider an outcome variable that is equal to 1 when an individual is employed at a job with monthly earnings of more than EUR 1,000. Comparing the treatment effect for this outcome variable (depicted in the middle panel of Figure 3) to the baseline outcome variable that is equal to 1 for any employment spell (depicted in the upper panel of Figure 3) reveals an almost identical pattern and quantitatively similar effect sizes. This holds both for our overall sample (see panels in the left column of Figure 3), as well as for the at-risk group with a long predicted unemployment duration (right column of 3). Importantly, this finding suggests that our treatment did not shift individuals disproportionally into low-wage jobs. Rather, treated individuals—in particular, those at risk of being long-term unemployed—-seem to have taken up jobs with salaries of more than EUR 1,000. Next, we consider an outcome variable that is equal to 1 when an individual is employed at a job with monthly earnings of more than EUR 2,000—which is substantially more than the average monthly earnings of

[17] Analogous to a model with storable job offers, mechanisms based on workers being rehired by their former employer would also be consistent with a delay in the onset of a treatment effect if the probability of being rehired rises with search effort, e.g., due to better outside offers. See, e.g., Katz and Meyer (1990) and Nekoei and Weber (forthcoming) for evidence documenting the importance of recall and temporary layoffs.

about EUR 1,500 that individuals in our sample earned before the intervention. Considering this high-wage employment outcome, we find effect sizes that are smaller in magnitude than the effects on the overall employment margin.[18] Taken together, these results indicate that the treatment increases the probability of finding employment in jobs with salaries that are similar to the ones that individuals earned before entering unemployment, in particular for job seekers who are at risk of being unemployed for a long time period. This suggests that, for the latter group of job seekers, the brochure indeed improves employment prospects without having detrimental consequences for the quality of the resulting matches.

4 Conclusion

In this paper we reported the results of a field experiment that investigated the impact of an informational brochure on job seekers' labor market outcomes. The brochure was designed to address some of the key challenges that job seekers face in terms of the information and motivation that is needed to find new employment. While we observe overall positive effects of our treatment on subsequent employment and earnings, these tend to be concentrated among job seekers who are at risk of being unemployed for an extended period of time. Within this group, we find pronounced and statistically significant treatment differences, corresponding to an increase in employment and earnings of about 4% in the year after the intervention. The fact that the brochure has particularly strong effects for individuals at risk of long-term unemployment is consistent with the hypothesis that informational or behavioral frictions impede the employment prospects of these individuals. In light of the extremely positive cost-benefit ratio of our intervention in the subgroup of individuals at risk of long-term unemployment (the total costs of production and mailing were less than 1 Euro per brochure), our findings suggest that targeted information provision can be an effective instrument in improving the labor market prospects of job seekers, at least in important subgroups of the population.

[18]Note, however, that the differences in effect sizes are relatively small compared to the standard errors of the estimates.

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Appendix A: Figures and Tables

Table 1: Summary Statistics by Treatment Status

Variable:	Control $[N = 40,282]$ (1)	Treatment $[N = 13,471]$ (2)	Difference $\lvert(2) - (1)\rvert$ (3)
Female	0.457	0.454	0.002
	(0.50)	(0.50)	[0.42]
Age	36.92	36.92	0.003
	(7.62)	(7.63)	[0.04]
Previous Daily Wage	52.38	52.53	0.162
	(38.36)	(38.51)	[0.42]
Days of Unemployment	873.06	881.90	8.44
(over ten years before treatment)	(847.1)	(852.8)	[1.05]
Education Category 1	0.148	0.152	0.003
	(0.35)	(0.36)	[1.11]
Education Category 2	0.590	0.591	<0.001
	(0.49)	(0.49)	[0.05]
Education Category 3	0.041	0.041	<0.001
	(0.20)	(0.20)	[0.09]
Education Category 4	0.076	0.070	0.006
	(0.26)	(0.25)	[2.35]
Education Category 5	0.041	0.041	<0.001
	(0.20)	(0.20)	[0.13]
Education Category 6	0.104	0.107	0.003
	(0.31)	(0.31)	[0.95]
Local Unemployment Rate	8.243	8.303	0.061
	(3.14)	(3.17)	[1.94]

Note: All variables are measured before the treatment. Standard deviations are reported in parentheses; absolute values of the t-statistics for differences between treatment and control group are reported in square brackets. The variable "previous daily wage" is censored at the maximum level of income upon which social security contributions are levied (EUR 150); wages above EUR 150 are not imputed. Days of unemployment are calculated from 2001 until the beginning of treatment. The local unemployment rate (Bundesagentur für Arbeit 2011) is measured at the level of the last district of residence reported before treatment. To measure an individual's education, we take the highest level of education reported before treatment. Education is measured in 6 categories: (1) Primary school/lower secondary school/intermediate school leaving certificate or equivalent school education, without a vocational qualification; (2) same as (1) but with a vocational qualification; (3) with upper secondary school leaving certificate (Abitur), but without a vocational qualification; (4) same as (3) but with a vocational qualification; (5) degree from a university of applied sciences (Fachhochschule); (6) university degree.

Table 2: Linear Probability Model - Treatment Status

Outcome Variable: Treatment	(1)
Female	-0.0001
	(0.004)
Education Category 2	-0.003
	(0.005)
Education Category 3	-0.003
	(0.011)
Education Category 4	-0.019**
	(0.008)
Education Category 5	-0.004
	(0.011)
Education Category 6	0.002
	(0.008)
Age	-0.00003
	(0.0003)
Local Unemployment Rate	0.001
	(0.001)
Previous Daily Wage	0.00003
	(0.00005)
Days of Unemployment	$2.00{\cdot}10^{-6}$
(over ten years before treatment)	$(2.41{\cdot}10^{-6})$
Wave 2 (Indicator)	0.002
	(0.005)
Wave 3 (Indicator)	-0.0002
	(0.005)
Wave 4 (Indicator)	0.004
	(0.005)
State Fixed Effects	Yes
N	53,753
R^2	0.0004
F-Statistic	0.81

Note: Linear probability models. Robust standard errors are reported in parentheses. Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. The model includes state fixed effects none of which are statistically significant. All regressors are measured before treatment. For an overview of the variables used in the regression see Table 1. An F-test for joint significance of all regressors is not significant (p=0.76).

Table 3: Main Treatment Effects

Sample:	All		By Risk of Long-Term Unemployment: Summary Measure High Risk		By Risk of Long-Term Unemployment: Summary Measure Low Risk	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome Variable: *Days of Employment (over 52 weeks)*						
Treatment	1.24	1.40	4.68***	4.78***	-1.89	-2.11
	(1.24)	(1.26)	(1.75)	(1.72)	(1.86)	(1.85)
Outcome Variable: *Cumulative Earnings (over 52 weeks)*						
Treatment	155.52	143.87	454.30**	446.20**	-113.02	-182.58
	(138.89)	(135.82)	(187.67)	(184.29)	(203.77)	(199.10)
Controls	No	Yes	No	Yes	No	Yes
N	53,753		26,451		26,742	

Note: Each entry reports the treatment effect in a separate specification. Robust standard errors are reported in parentheses. Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Cumulative earnings are calculated as the the sum of wages measured in 4-week intervals after the treatment (wages are reported as daily wages in the data; we scale wages up to correspond to monthly wages). As a summary measure for high risk of long-term unemployment, we regress days of employment in the control group on the set of variables reported in the summary statistics (Table 1). We then predict an individuals' non-employment duration based on the coefficients from this regression. Individuals with above-median levels of predicted nonemployment duration are included in the group "Summary Measure High Risk". Control variables in Columns (2), (4), and (6) include the variables reported in the summary statistics (gender, education categories, age, the last wage reported before the intervention, the number of days an individual was reported as unemployed before the intervention, as well as the local unemployment rate) as well as state and wave-of-treatment fixed effects.

Table 4: Robustness Check - Abadie et al. (2013) Correction for Endogenous Stratification

Estimates of Treatment Effects for Subgroups

Outcome Variable: Days of Employment (over 52 weeks after treatment)

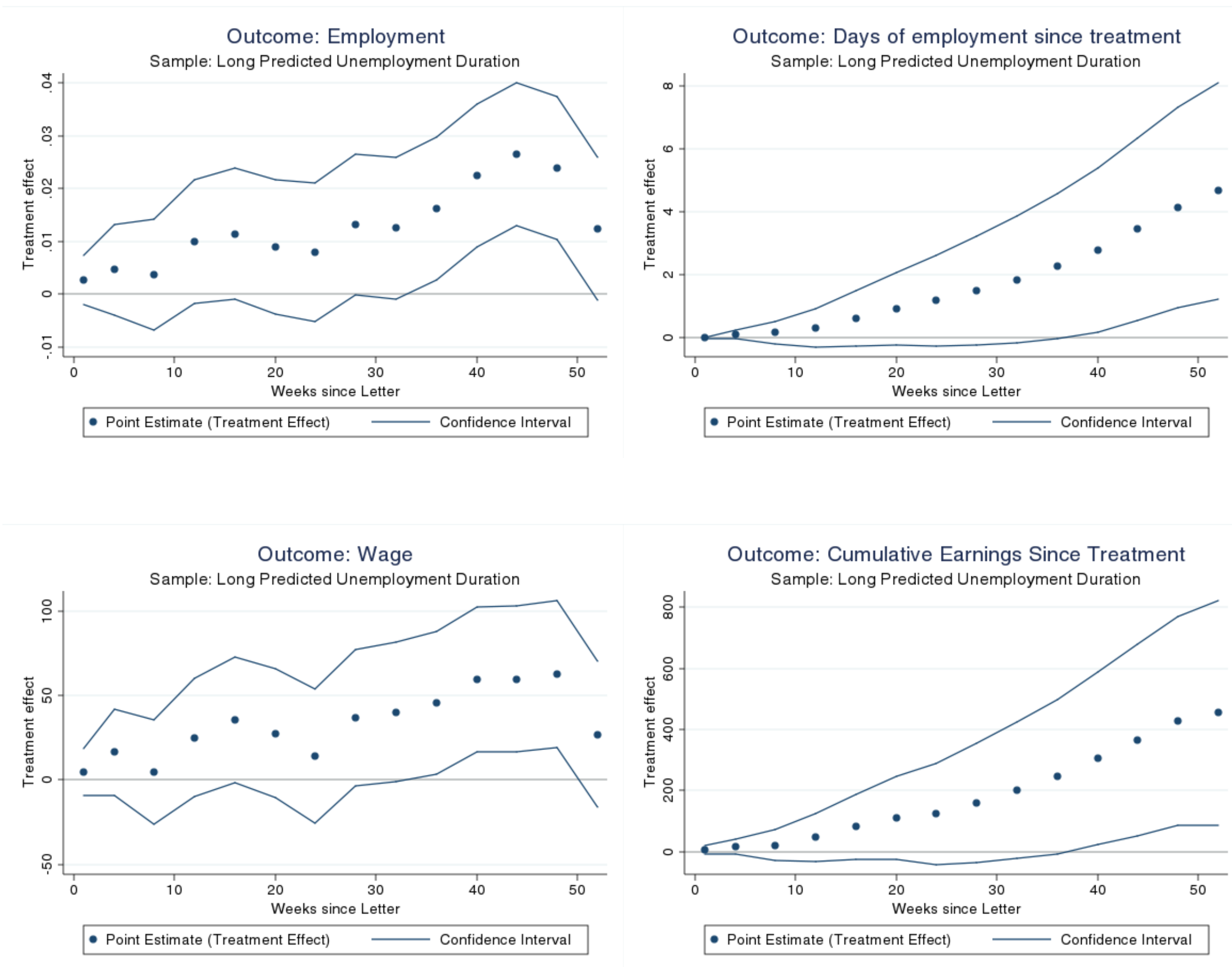
Subgroup:	OLS	Abadie et al. (2013)
High Risk of Long-Term Unemployment	4.68	4.72
	(1.75)	(0.95)
Low Risk of Long-Term Unemployment	-1.89	-1.96
	(1.86)	(1.07)

Outcome Variable: Cumulative Earnings (over 52 weeks after treatment)

Subgroup:	OLS	Abadie et al. (2013)
High Risk of Long-Term Unemployment	454.30	448.55
	(187.67)	(97.32)
Low Risk of Long-Term Unemployment	-113.02	-112.35
	(203.77)	(120.00)

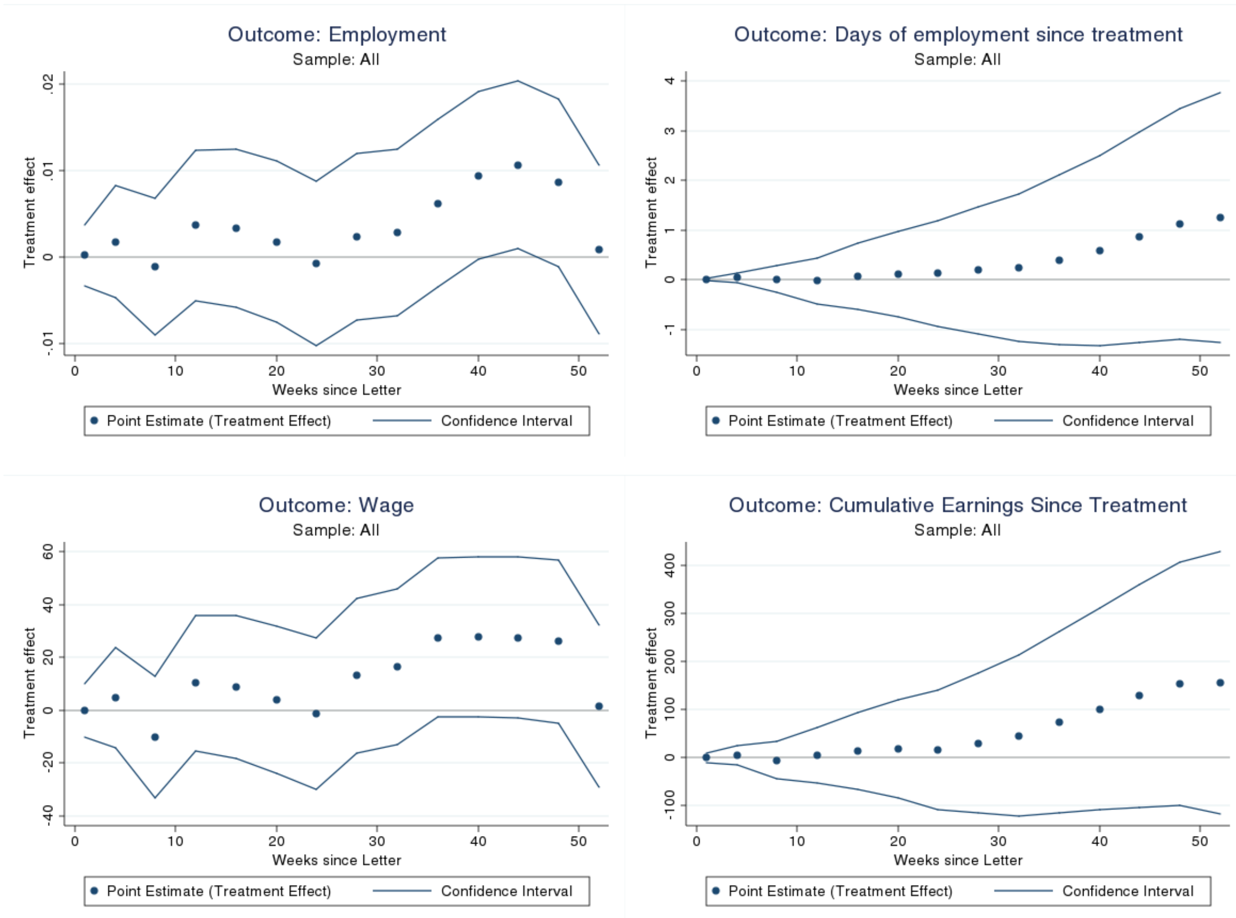
Note: We implement the repeated split sample estimator proposed in Abadie et al. (2013) to address bias in the estimation of treatment effects for endogenously stratified subpopulations. See Table 3 as well as Section 3.2 for further details on the definition of the high-risk vs. low-risk group. The first column presents OLS results (see also Table 3); robust standard errors are reported in parentheses. The second column reports the mean and standard deviation (in parentheses) of 100 repetitions of the split sample estimator in Abadie et al. (2013). In each repetition, we randomly draw half of the observations in the control group to estimate predictors of unemployment duration; we then predict unemployment duration in the treatment group and the remainder of the control group and split this sample at the median to estimate treatment effects in the two respective subgroups.

Figure 1: Timing of Treatment Effects: Individuals with Long Predicted Unemployment Duration



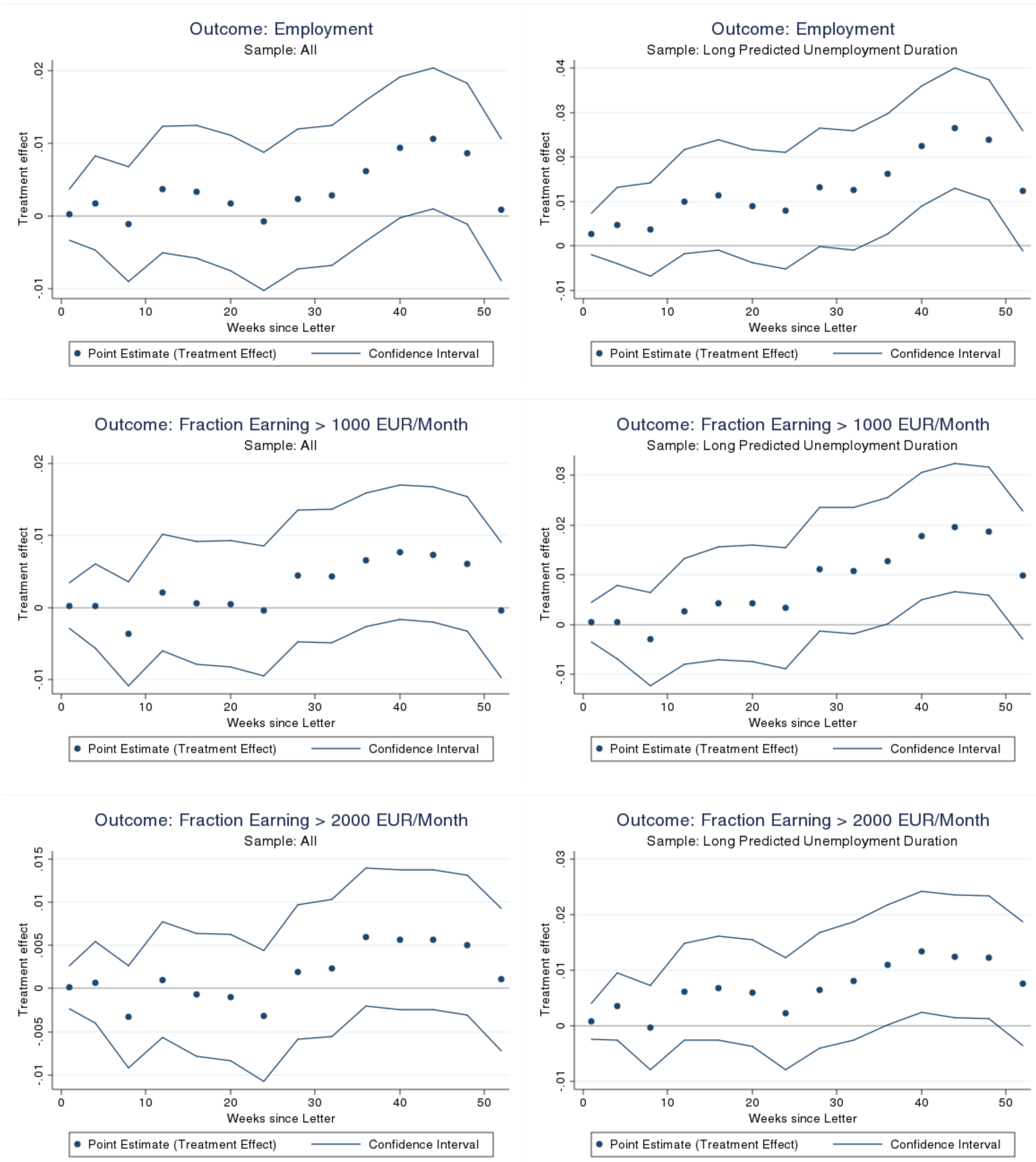
Note: Each blue dot denotes the point estimate for the treatment effect at a given time horizon based on OLS regressions. The sample in all specifications is restricted to individuals who were out of employment at the time of the intervention and who have an above-median level of overall predicted unemployment duration ($N = 26,875$). As a summary measure for a long predicted unemployment duration, we regress days of non-employment in the control group on the set of variables reported in the summary statistics (Table 1). We predict overall unemployment duration based on the coefficients from this regression. Individuals with more than a median-level of predicted unemployment duration are included in the group “Summary Measure High Risk”. The blue lines denote 95% confidence intervals and correspond to 1.96 time the robust standard error of the corresponding point estimate. The estimations do not include control variables. In the bottom left panel, the outcome variable for individuals without employment is zero.

Figure 2: Timing of Treatment Effects: Full Sample



Note: Each blue dot denotes the point estimate for the treatment effect at a given time horizon based on OLS regressions. The samples include individuals who were out of employment at the time of the intervention ($N = 53,751$). The blue lines denote 95% confidence intervals and correspond to 1.96 time the robust standard error of the corresponding point estimate. The estimations do not include control variables. In the bottom left panel, the outcome variable for individuals without employment is zero.

Figure 3: Treatment Effects on Different Margins of Employment



Note: Each blue dot denotes the point estimate for the treatment effect at a given time horizon based on OLS regressions. The blue lines denote 95% confidence intervals and correspond to 1.96 time the robust standard error of the corresponding point estimate. The regressions do not include control variables.

Appendix B: The Brochure

Picture of the Brochure

Figure 4: Information Brochure

Translation of the Text Blocks

Unemployed – What To Do?

Knowledge, Ideas, Perspectives

Now is the ideal time!

You'd like to find a new job as soon as possible. Now is the ideal time to successfully search for a new position! In 2010, the German economy has recovered noticeably from the economic crisis. Companies and businesses are increasingly hiring new employees again. Since the beginning of the year, more than two million people have already found a new job. Right now, there are several hundred thousand vacancies available as well.

Did you know that…

…active job search is a key to success? Many people greatly underestimate the impact of their personal initiative. Scientific studies show that active job search proves much more successful than most people think. Personal initiative and intensive job search increase your chances of finding a job much more than you might guess. Hence, taking the initiative pays off.

…the chance of finding employment decreases with the duration of unemployment? Research has shown that the likelihood of finding work decreases with every passing month of unemployment. So don't hesitate. Every day counts.

Chart:

Chances of Finding Work, Level of Personal Initiative (low, high)

Chances of Finding Work, Duration of Unemployment (short, long)

Job search pays off, not just financially!

Job search pays off. Scientific studies document a positive impact of working on personal life satisfaction. Employment is frequently associated with more stable family bonds and lower divorce rates. Moreover, employed individuals suffer less frequently from episodes of depression and don't fall ill as often. Furthermore, their average mortality rate is lower and their general health condition is better. In addition, a new job also comes with new social contacts and acquaintances.

There are Many Ways to the Goal

You can find job openings in your local daily newspaper, online or at the job platform of the Employment Agency. Also, don't miss the opportunity to send unsolicited applications to companies.

You might not yet be aware that many unemployed people find work through their social network of relatives, friends, and acquaintances. So don't hesitate to tell them about your job search. Many people were unemployed at one point in their life and can relate well to your situation.

Do you sometimes feel depressed and doubt that your search for employment will eventually be successful? These feelings are perfectly normal and are experienced by most people after the loss of their job. Stay on top of things nonetheless—your next application could already get you a new job!

Stay Active!

Begin your job search already today:

- Use the job search platform of the Employment Agency.
- Search online (look for the keyword "Job Fair" on the internet).
- Ask your friends and acquaintances about vacant positions.
- Take the initiative and apply directly to potential employers.

Contact: University of Bonn | Department of Economics | Adenauerallee 24-42 | 53113 Bonn | Phone: +49 228 823 69 456 | Email: wastun.info@uni-bonn.de

Information on References Used in the Brochure

In the following, we provide a detailed overview on the references that we used for designing the information brochure.

Block 2: Duration dependence and returns to search effort. Classical references on this topic include Jackman and Layard (1991) who document a strong decline in the chance of reemployment with longer unemployment duration in the UK; they also note that both unobserved heterogeneity and duration dependence matter. van den Berg and van Ours (1996) examine duration dependence and unobserved heterogeneity in US data, finding no significant genuine duration dependence for black individuals, but significant effects for male white workers. Recently, Kroft et al. (2013) and Schmieder et al. (2013) have used data from field and natural experiments to examine the causal impact of unemployment duration on job seekers' labor market prospects, documenting negative effects of longer unemployment duration on employment prospects and wages, respectively. Using US survey-data, Spinnewijn (2015) provides indirect evidence that unemployed individuals underestimate the effect of their search effort on chances of reemployment.

Block 3: Non-pecuniary effects of unemployment. *Mortality and Health*: In terms of general health effects, the evidence suggests that unemployment is associated with deteriorating mental health (see Björklund and Eriksson (1998) for an overview). Gerdtham and Johannesson (2003) use individual level data from Sweden, finding that unemployment increases the risk of being dead at the end of the observation period by almost 50%, controlling for the initial health states of individuals. Similar evidence is found by Eliason and Storrie (2009) who use linked employer-employee data to study how establishment closures affect mortality of displaced workers. They show an increase in mortality risk of 44% for men within four years after job loss. Effect insignificant in the longer run and for female workers. But strong increase in suicides and alcohol-related deaths. *Family Outcomes*: Jensen and Smith (1990) use data from Danish married couples. Controlling for age, education, children and other factors, they find that unemployment leads marital instability; only unemployment of husband has effect. Kraft (2001) uses GSOEP data to examine effect of unemployment on married couples to move apart; finds significant effect which increases in duration of unemployment. Eliason (2012) uses data from Sweden to examine the impact of both husbands' and wives' job displacement on the risk that the marriage ends in divorce. Finds significant increase in case of husbands' job loss, effects sizable but insignificant for women. *Life Satisfaction/Happiness*: Several studies have examined the relationship between (entry into) unemployment and general life satisfaction, typically finding strong and negative associations (see, e.g., Clark and Oswald 1994, Winkelmann and Winkelmann 1998, Kassenboehmer and Haisken-DeNew 2009).

Block 4: Search channels. For instance, Holzer (1988) studies US data on job search strategies of young unemployed and finds that a large part of job offers are due to connections through friends or relatives. Furthermore, he documents that the returns to using these connections are very high. Montgomery (1991), nicely summarizes some empirical results; roughly 50% of employees found their job through friends or relatives. Kuhn and Mansour (2014) provide evidence for the effectiveness of internet job search. They show that unemployed that search for work online are re-employed about 25% faster.

Appendix C: Additional Figures and Tables

Table 5: Determinants of Total Nonemployment Duration in the Control Group

Outcome Variable: Days of Nonemployment (over 52 weeks after treatment)	(1)
Female	-4.33***
	(1.31)
Education Category 1	35.59***
	(3.65)
Education Category 2	-2.54
	(3.41)
Education Category 3	4.46
	(4.63)
Education Category 4	-3.03
	(4.06)
Education Category 5 (omitted)	-
Education Category 6	9.35**
	(3.88)
Age	1.25***
	(0.08)
Previous Daily Wage	-0.0035***
	(0.0006)
Days of Unemployment (over ten years before treatment)	0.006***
	(0.0008)
Local Unemployment Rate	-0.09
	(0.21)
N	40,282
R^2	0.018
F-Statistic	82.93

Note: Robust standard errors are reported in parentheses. Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. The sample is confined to the control group.

Table 6: Treatment Effect One Year After Treatment - Alternative Definitions of At-Risk Sample

			Subpopulations With Increased Risk of Long-Term Unemployment							
Sample:	All		Summary Measure High Risk		Long Previous Unemployment		Low Previous Wage		Low Education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome Variable: *Days of Employment (over 52 weeks)*										
Treatment	1.24	1.40	4.68***	4.78***	3.87**	3.75**	4.02**	4.22**	2.19	2.30
	(1.24)	(1.26)	(1.75)	(1.72)	(1.56)	(1.57)	(1.69)	(1.66)	(1.47)	(1.45)
Outcome Variable: *Cumulative Earnings (over 52 weeks)*										
Treatment	155.52	143.87	454.30**	446.20**	370.51**	367.71**	447.54***	410.19**	213.56	177.02
	(138.89)	(135.82)	(187.67)	(184.29)	(165.55)	(162.30)	(169.87)	(167.30)	(146.37)	(143.65)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	53,753		26,451		33,062		29,821		39,727	

Note: Each entry reports the treatment effect in a separate specification. Robust standard errors are reported in parentheses. Levels of significance: $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$. Cumulative earnings are calculated as the the sum of wages measured at 12 4-week intervals after the treatment (wages are reported as daily wages in the data; we scale wages up to correspond to monthly wages). The sample "Long previous unemployment" contains individuals with more than one year of reported unemployment spells in the ten year period before the intervention. "Low education" refers to individuals with less than a college-level education or Abitur (education categories 1 and 2). "Low previous wage" is a sample of individuals whose daily wage before the intervention was EUR 50 or less. As a summary measure for high risk of unemployment, we regress days of employment in the control group on the set of variables reported in the summary statistics (Table 1). We predict unemployment duration based on the coefficients from this regression. Individuals with more than a median-level of predicted unemployment duration are included in the group "Summary Measure High Risk". Control variables include the variables reported in the summary statistics (gender, education categories, age, the last wage reported before the intervention, the number of days an individual was reported as unemployed before the intervention, as well as the local unemployment rate) as well as state and wave of treatment fixed effects.

Table 7: Treatment Effect Heterogeneity - Interactions of Characteristics with Treatment

Outcome Variable:	Days of Employment *(over 52 weeks)* (1)		Cumulative Earnings *(over 52 weeks)* (2)	
	Main Effect	Interaction (∗Treatment)	Main Effect	Interaction (∗Treatment)
Female	5.79***	-0.51	-939.7***	57.7
	(1.31)	(2.63)	(140.46)	(282.9)
Education Category 2	35.8***	1.74	2207.0***	159.2
	(1.77)	(3.51)	(181.8)	(361.4)
Education Category 3	29.51***	10.66	3982.7***	1261.6
	(3.62)	(7.38)	(403.3)	(863.3)
Education Category 4	36.85***	-6.70	3749.6***	-243.1
	(2.89)	(5.87)	(313.5)	(644.9)
Education Category 5	34.17***	-1.74	4499.4***	-394.1
	(3.68)	(7.24)	(427.5)	(852.5)
Education Category 6	24.47***	2.63	5569.0***	384.6
	(2.67)	(5.29)	(333.2)	(655.9)
Age	-1.21***	0.38**	-109.7***	19.7
	(0.08)	(0.17)	(8.7)	(17.8)
Local Unemployment Rate	0.08	-0.43	27.2	7.7
	(0.21)	(0.41)	(21.6)	(43.5)
Previous Daily Wage	0.004***	-0.0038***	1.7***	-0.35**
	(0.006)	(0.0012)	(0.08)	(0.15)
Days of Unemployment	-0.005***	-0.0001	0.22**	0.03
(over ten years before treatment)	(0.0008)	(0.0015)	(0.08)	(0.17)
Wave 2 (Indicator)	-1.33	-0.87	85.8	-111.0
	(1.82)	(3.69)	(198.7)	(403.3)
Wave 3 (Indicator)	8.19***	2.12	996.4***	136.2
	(1.83)	(3.67)	(197.1)	(396.2)
Wave 4 (Indicator)	17.39***	-0.41	1370.5***	77.2
	(1.84)	(3.69)	(193.2)	(388.0)
N	53,753		53,195	
R^2	0.0218		0.0423	
F-Statistic	46.40		63.14	

Note: The regression model includes the main effect for the treatment indicator as well as the main effects for each of the variables displayed and interacted with treatment status. Robust standard errors are reported in parentheses. Levels of significance: * < 0.10, ** < 0.05, *** < 0.01.