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

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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. The effects of our treatment tend to be positive, but concentrated among job seekers who are at risk of being unemployed for an extended period of time. Specifically, treatment effects in our overall sample are moderately positive on average but mostly 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.

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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. They need to think about how much effort to exert, which search channels to use, and what kinds of jobs to target, e.g., in terms of industry, occupations or geographical location. The job search process 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. In addition to such informational challenges, the search process also puts a strain on individuals' self-confidence and willpower. Besides the general unhappiness and frustration caused by the loss of a job, a job seeker needs to overcome discouragement from rejected applications and further personal setbacks that unemployment and the job search process often bring about.[1]

Motivated by these challenges, we investigate whether providing unemployed individuals with information about the job search process 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. Our brochure was designed to operate through two main channels. First, we provided concise and easy-to-understand *information* about the current labor market situation, the non-pecuniary consequences of (un)employment and effective job search strategies. We also provided a simple illustration of duration dependence and emphasized the importance of active job search. Second, the brochure aimed at *motivating* job seekers and encouraged them to actively search for new employment.

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 individuals' employment history, allowing us to identify treatment effects for subgroups of individuals who differ in terms of pre-determined characteristics. This is important given that different groups of job

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

seekers are likely to vary systematically in the degree to which they need the type of information and encouragement that our brochure provides.

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 139 to EUR 147. While the point estimates are positive in both dimensions, the estimated effects are relatively small and coefficients are generally not statistically significant. Specifically, our estimates allow us to rule out that the brochure increased average earnings and days of employment by more than about 3% or reduced these outcomes by more than 1% relative to the control group. As a robustness check, we also report the results of median regressions. These indicate somewhat stronger treatment effects of roughly 5 to 7 additional days of employment and around EUR 300 to EUR 500 higher earnings. The estimated treatment effects at the median are statistically significant at the 5% [10%] level in specifications without [with] controls.

The randomized nature of our intervention also 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. It is plausible that the effects of our treatment are concentrated among individuals at risk of long-term unemployment, since the challenges that are addressed by the brochure are likely to be particularly pronounced in this group (see, e.g., DellaVigna and Paserman 2005, Paserman 2008, Dohmen et al. 2009, and Spinnewijn 2015). 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. In the case of Germany, for example, the Hartz reforms—arguably the most comprehensive post-war labor market reforms in the country—were implemented with an explicit goal of reducing long-term unemployment (see Hartz, 2002, as well as Price, 2016, for a recent evaluation).

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 average 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. As for the overall sample, the effect sizes in median regressions tend to be larger than the OLS estimates. Our results also indicate that the increase in employment for the at-risk group does not come at the cost of lower wages, suggesting 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. (2017) point out that endogenous stratification approaches like the one we employ 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. (2017). Reassuringly, the estimated increases in employment and earnings are almost identical in specifications using the repeated split-sample estimator, suggesting that the positive treatment effects observed in the at-risk group are not spurious. The results are also qualitatively robust to considering various alternative definitions of the at-risk group, e.g., based on previous unemployment durations or the wage level prior to the intervention. In a final step, we also analyze the effects of the brochure over a longer time horizon. While most of our findings are at least qualitatively robust, standard errors become relatively large, rendering the precise estimation of long-run treatment effects impossible. Taken together, our results suggest that targeted information provision can be an effective policy tool to improve the 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 amounts to less than EUR 1 per brochure—the findings suggest a highly positive cost-benefit ratio of the intervention.

Our results contribute to the literature examining the labor market effects of job search assistance, counseling, training, and activation (see Heckman et al. 1999 and Card et al. 2010 for comprehensive overviews). We follow a recent strand of the literature that uses explicitly randomized interventions to study the causal effects of labor-market policies (e.g. Crépon et al. 2013, Behaghel et al. 2014, Belot et al. 2015).[2] Our intervention is inspired by and adds to an emerging body of literature analyzing the consequences of information provision in a variety of economic applications, including job search and labor supply (Belot et al. 2015, Chetty and Saez 2013, Liebman and Luttmer 2015), 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 ours is the contemporaneous paper by Belot et al. (2015), who invite job seekers to the laboratory to search for jobs via a specifically designed online tool. Through the online interface, they administer information on suggested alternative occupations for job seekers, as well as information on geographical areas where vacancies for a given set of occupations are available. Belot et al. (2015) find that the additional information indeed leads job seekers to con-

[2] 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. 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.

sider a broader set of job ads. Additional survey evidence indicates that this also translates into a higher number of interviews, especially among longer-term unemployed individuals who had previously searched rather narrowly.

Our paper complements a number of theoretical models and observational studies that investigate unemployment and job search through the lens of behavioral economics.[3] For instance, DellaVigna et al. (forthcoming) 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. Other papers suggest that certain groups of job seekers might exert suboptimally low levels of search effort, for instance, due to 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 being mainly determined by external vs. internal factors (see Caliendo et 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 economic 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—not only advance our understanding of important labor market phenomena, but that they can also directly help people in making complex economic decisions.

The remainder of this 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 information about the job search process and the consequences of unemployment can improve job seekers' labor market outcomes, we implemented a large-scale field experiment in the German labor market. The experiment was conducted between October 2010 and January 2011 with job seekers who had recently entered unemployment. 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* needed to find new employment.

[3] See also Thaler and Sunstein (2008) and Chetty (2015) for a detailed discussion and a recent overview of policy interventions in other domains that are inspired by research in behavioral economics.

2.1 The Brochure

The brochure comprised four parts, each containing information to help people overcome the informational challenges and foster their motivation for the job 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 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 of the economy 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% in 2009, growth rates in 2010 and 2011 were 4.0% and 3.3%, 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 1994, Kroft et al. 2013, Schmieder et al. 2016). 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, Eliason 2012), and overall life satisfaction (e.g., Clark and Oswald 1994, Kassenboehmer and Haisken-DeNew 2009).

Finally, the fourth text block as well as the back side of the brochure provided information on various alternative search channels. It emphasized the relevance of social networks for finding a new job (see 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 largely an informational problem. Accurate beliefs about various aspects of the job search process are key to searching effectively, while misperceptions—for instance, regarding the returns to search effort—can lead to suboptimal search behavior. The information provided in our brochure thus aimed at communicating relevant research findings related to job search, with the ultimate goal of facilitating individuals' search for a job. At the same time, the provision of encouragement and motivation

[4] A picture of the brochure is provided in Figure 1; 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.

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 result from present bias, 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

Our experiment was conducted between October 2010 and January 2011. Sampling and randomization were carried out by the Institute for Employment Research at the German Federal Employment Agency (IAB). Each month, the IAB drew a random sample from the country-wide inflow of recently registered job seekers, and randomly assigned individuals to the treatment or control group. Due to administrative processes within the German labor market authorities, there was a gap of 4-8 weeks between the point in time when a person entered unemployment and the date at which he/she received our information brochure. This gap is mainly due to the fact that job seekers in Germany registered unemployed at their local job center, whereas our randomization is based on registry data that was updated only once a month at the IAB. More specifically, towards the end of the first week of a given month, we received information on all individuals who 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.

In each of our four waves, we drew a sample of 10,000 individuals for our treatment group and 30,000 for the control group.[5] 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 50, 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 inform-

[5]Note that due to the gap described above, many of these individuals were already back in employment at the time when we drew 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).

ing 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. 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.[6]

It is important to bear in mind that all analyses reported in Section 3 capture intention-to-treat (ITT) effects. In other words, we do not know the extent to which individuals in the treatment group actually "digest" the information that 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.[7] While from a policy perspective the treatment differences that we are identifying are the relevant ones, one might argue that we are only measuring a lower bound of the treatment "reading and understanding the contents of the brochure". To gain a sense of the potential magnitude of the difference between treatment-on-the-treated (TOT) and ITT effects, it is instructive to consider the results of a representative panel study among postal mail recipients in Germany (Nielsen Dialogmarketing Panel, 2016). According to this study, on average 72.6% of personally addressed letters are opened and read by recipients. Taking this number at face value, our ITT effects would have to be scaled up by 37.3% (i.e., dividing the ITT effect by 72.6%), to arrive at TOT effects for the treatment of reading the contents of the brochure.

It is also worth noting that, upon registering unemployed, job seekers in Germany obtain an information and counseling package from the employment agency. While the exact contents of this package can vary between local job agencies, a common theme is that the focus lies primarily on explaining job seekers the rights and duties arising from registering as unemployed (e.g., benefit entitlements and duration, possible penalties, etc.). At the same time, the package contains very little information on job search strategies or the job search process, more generally. Consequently, the information provided by the employment agencies to newly registered job seekers may be viewed as complementary to our brochure.

2.3 Data and Empirical Approach

In our empirical analysis in Section 3, we estimate 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.[8] Specifically, due to the time gap in the reporting system described above, 66.3% of the originally sampled individuals in the

[6] Relatively few participants (a total of 183 over the course of the experiment) contacted us with questions or feedback. Most inquiries related to questions about data privacy and could be addressed relatively easily by explaining that the study was fully in line with privacy laws and that the empirical analysis was exclusively based on anonymized data.

[7] In addition, about 1.9% of letters (749 in total) were returned to sender as undeliverable, presumably due to changes in the recipient's 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.

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

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[9]). The IEB 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. It thus avoids problems that can plague studies based on voluntary surveys, e.g., attrition and misreporting.

The population for which the IEB is recorded includes 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. Information in the IEB is reported in spells. Our data set comprises spells from 2001 until 2012 and includes detailed information on employment status, earnings, or occupations, as well as personal characteristics such as year of birth, gender, or education. Table 1 provides summary statistics for key variables that we use in our study.

Throughout our empirical analysis in Section 3, we estimate models of the following kind:

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

In our main specifications, we focus on two key measures of individuals' labor market performance after the intervention. The first, $Cumulative\_Employment_i$, captures the total number of days that individual i has been employed, measured from the the week in which the brochure was sent out in a given wave 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.[10] T_i is a dummy for treatment 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). As treatment status is randomly assigned, T_i is by construction orthogonal to X_i and ϵ_i, so that the estimated β coefficient from OLS regressions

[9]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 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.

[10]$Cumulative\_Earnings_i$ 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 and East vs. 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.

identifies the average causal effect of the treatment. In addition to the OLS specifications, Section 3 also reports the results from median regressions as a robustness check.

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 had been unemployed for almost 900 days during the last ten years before the intervention and earned about EUR 1,580 per month in his/her last job before registering as unemployed, illustrating that our intervention was conducted in a sample with relatively low 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.[11] 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 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. The results of the regression are reported in Table 7 in Appendix C 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.

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 in the year after the intervention. In a second step, we analyze the consequences of our treatment for job seekers who are at risk of remaining 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.

[11] 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.

3.1 Results for the Overall Sample

Table 2 reports the treatment effects of the brochure on cumulative earnings and employment for our overall sample. During the year after the intervention, treated job seekers worked, on average, for about 1.2 days more than individuals who did not receive the brochure. Furthermore, job seekers in the treatment group accumulated roughly EUR 140 of additional earnings over the year after the intervention (see Column (1) of Table 2). While the point estimates suggest a generally positive influence of the brochure, the employment and earnings effects in our overall sample are relatively small and turn out to be statistically insignificant in OLS specifications. Specifically, the estimated confidence intervals allow us to rule out that the brochure on average increased earnings and days of employment by more than 3%, or reduced these outcomes by more than 1% relative to the control group.

The table also presents additional specifications that control for individual-level as well as market-specific characteristics (see Column (2) of Table 2). 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 his/her unemployment spells during the past ten years), local labor market characteristics, and state as well as treatment-wave fixed effects. For both outcomes, the inclusion of control variables has only minor effects on the estimated average treatment effects.

We also report the results of median regressions (see Columns (3) and (4) of Table 2). These indicate larger effect sizes of about 5 to 7 additional days of employment and around EUR 300 to EUR 500 of additional earnings in the treatment group. The point estimates of the median regressions are statistically significant at the 5% (10%) level in specifications without (with) additional controls.

3.2 Individuals at Risk of Long-Term Unemployment

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 likely to vary across different subgroups of the population. Based on such 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 is individuals at risk of long-term unemployment. The previous literature suggests a close link (both theoretical and empirical) between a number of behavioral biases 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.

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. 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. As explanatory variables, we include 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). For further details and estimation results, see Table 8 in Appendix C. In a second step, we use this model to predict the expected unemployment duration for each individual in our sample. Accordingly, 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. Finally, we repeat our analysis from Section 3.1 for the subsample of job seekers with above-median levels of expected unemployment duration according to the index. Table 9 in Appendix C reports summary statistics by treatment status for the at-risk group, documenting that pre-treatment characteristics are balanced across treated and untreated individuals, with only a minor difference of 0.1% in the local unemployment rate.

Columns (1) – (4) of Table 3 summarize the treatment effects for the subgroup of job seekers with a high risk of long unemployment. One year after the beginning of the intervention, treated job seekers in the at-risk group have, on average, accumulated more than four 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 roughly EUR 450 higher for job seekers who received the brochure ($p<0.05$). The treatment effects are highly robust to the inclusion of additional controls. Median regressions deliver qualitatively similar patterns compared to the OLS specifications for the at-risk group. Quantitatively, treatment effects again tend to be larger in magnitude when comparing the effect at the median to the average treatment effect in the corresponding subsample (see (3) – (4) of Table 3).

In columns (5) – (8) of Table 3, we also depict regression results for the counterfactual group of job seekers who exhibit a particularly low risk of long unemployment duration. The regressions for this subpopulation yield negative point estimates for the treatment effects on cumulated employment and earnings. However, these negative estimates are substantially smaller in magnitude than the positive effects for the at-risk group and are not statistically significant in any of the specifications.

3.3 Robustness Checks

In a recent paper, Abadie et al. (2017) raise an important methodological concern for research designs based on randomized experiments, in which researchers aim to estimate treatment effects for subgroups of individuals who 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.[12] Abadie et al. (2017) propose repeated split sample estimators as a remedy for this bias, as this 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 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, based on 100 repetitions of the Abadie et al. (2017) estimator. Results are reported in Table 4. Reassuringly, we find that our original results for both cumulative employment and earnings are almost identical to those 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. (2017) 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.

Another potential concern related to our subgroup analysis is that one could, of course, consider a large set of different subgroups, thus mechanically increasing the chances of getting significant results for some of these groups. While we in principle acknowledge this problem of multiple hypothesis testing, we assert that the subgroup we are considering (job seekers at risk of long-term unemployment) is of rather obvious economic relevance. Furthermore, our results are also highly robust to considering alternative definitions of subgroups at risk of long-term unemployment, based on individuals' pre-treatment characteristics. Specifically, as a robustness check of our results for the at-risk group, we estimate treatment effects in terms of employment and earnings for various alternative definitions of the at-risk group. We summarize estimation results for different subgroups in Table 5. For instance, columns (5) and (6) of Table 5 show the results for job seekers with a 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, albeit somewhat less pronounced, if we consider people's educational level as our indicator of the at-risk group (see columns (9) and (10) of Table 5).

[12] 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.

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, reported in Table 10 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 the outcome variable. We further find some evidence that the treatment could be particularly effective for older individuals: in particular, 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 meta-study, Card et al. (forthcoming) analyze the timing patterns in the treatment effects of active labor market policies. Using data from more than 200 different evaluations of various policy instruments, they demonstrate that policies that were effective in improving labor market prospects of job seekers often exhibit a substantial delay between the time of the intervention and the time at which positive effects of the treatment emerge.[13] Guided 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. As outcomes, we consider the dependent variables $Cumulative\_Employment_{it}$ and $Cumulative\_Earnings_{it}$, evaluated in week t after the intervention. As an additional measure for the strength of treatment effects at a given point in time, we also consider the binary variable $Employment_{it}$ that captures whether individual i has an employment spell at time t.

Figure 2 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. Qualitatively, this pattern is also observed in our overall sample, although the effects are generally weaker and not statistically significant (see Figure 3).

We suggest two possible mechanisms that may give rise to this pattern. First, DellaVigna et al. (forthcoming) show that job seekers with reference-dependent preferences tend to exert high levels of search effort at the beginning of the unemployment spell to overcome the losses in income that unemployment entails. By contrast, search efforts tend to be lower in later periods of the unemployment spell, once job seekers' income reference points have adjusted. Finally, search effort

[13] Relatedly, Osikominu (2012) analyzes the dynamic program effects of different active labor market policies in Germany and finds relatively short onsets of treatment effects with maximal exit rates around 60 days after program start.

rises again shortly before the date at which an individual's unemployment benefits expire. In such a setting, there could be more scope for any intervention to affect individuals' search behavior later in the unemployment spell, where the baseline search effort is relatively low. An alternative mechanism that is qualitatively in line with our findings relates to "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.[14] Consistent with both of these explanations, job seekers in our sample are typically eligible for receiving unemployment benefits for 6-12 months (depending on the length of their previous employment spells), indicating that there is scope for both reference-point adaptations as well as workers exploiting the possibilities of storable job offer.

To further explore these mechanisms, we plot hazard rates for exit into employment among treated and untreated individuals in our overall sample as well as in the at-risk group. Results are depicted in Figure 5 in Appendix C. For all groups considered, the hazard rates fall over time at the beginning of the unemployment spell and then increase again about 24 weeks after the treatment (roughly at the time at which benefits expire for individuals with a short benefit eligibility). Afterwards, hazard rates tend to decrease again, before we observe another pronounced spike in hazards after 52 weeks (around the time where unemployment benefits expire for another large subgroup in our sample). The figure also illustrates that differences in hazard rates between treated and untreated individuals tend to be larger during the second six months after the intervention, in particular in the at-risk group (see bottom panel of Figure 5). The combined timing patterns of exit rates and treatment effects are thus broadly consistent with a model in which treated job seekers become more active when benefits are beginning to run out, or manage to delay starting dates to that time period.

We conclude this part of our analysis by shedding further light on the margins of employment at which our treatment operates. For this purpose, we consider two additional outcome variables that help to 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 4) to the baseline outcome variable that is equal to 1 for any employment spell (depicted in the upper panel of Figure 4) reveals an almost identical pattern and quantitatively similar effect sizes. This holds for both our overall sample (see panels in the left column of Figure 4) and the at-risk group with a long predicted unemployment duration (right column of 4). Importantly, this finding suggests that our treatment did not shift individuals disproportionally into low-wage jobs; rather, treated individuals—in particular, those

[14]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 (2015) for evidence documenting the importance of recall and temporary layoffs.

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 about EUR 1,500 that individuals in our sample earned before the intervention. Considering this high-wage employment outcome, we find that effects are still positive, but smaller in magnitude than those on the overall employment margin.[15] In a final step, we also consider a range of further thresholds for individuals' minimum monthly earnings after 48 weeks $\{0, 250, ..., 1,750, 2,000\}$. Corroborating the results from Figure 4, the results in Figure 6 in Appendix C document relatively similar effects for all thresholds up to EUR 1,000 and then slightly lower effects on having earnings higher than EUR 1,250 and above. Notably, however, employment effects in the at-risk group are still positive even for these high-earnings jobs. Taken together, these results indicate that the treatment increases the probability of finding employment in jobs with salaries that are similar to those 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.

3.5 Treatment Effects in the Long Run

In a final step, we explore long-run treatment effects two years after the brochure was sent out. Table 6 summarizes results for the full sample and the different definitions of the at-risk group. A challenge that naturally arises in the analysis of such longer-run outcomes is that idiosyncratic shocks at the individual, regional, or country level accumulate over time, rendering it difficult to precisely estimate treatment effects in the long run. Comparing Table 6 to the corresponding results after one year (Table 5) illustrates this added imprecision in the long-run estimates. In all specifications, standard errors are roughly twice as high as for the corresponding one-year estimates. For the full sample (Columns (1) and (2) of Table 6), the analysis reveals point estimates close to zero in terms of employment and negative point estimates of around EUR 200 for cumulative earnings measured over the two-year time horizon. However, the standard errors for these long-term estimates are so large, and the treatment effects thus so imprecisely measured, that we cannot reject that the observed two-year treatment effects are identical to the positive point estimates after one year.

For the at-risk group (Columns (3) and (4) of Table 6), point estimates for the two-year treatment effects on employment and earnings are positive and comparable in magnitude to the corresponding estimates after one year. Results are again qualitatively robust to applying various alternative definitions of the at-risk group (Columns (5) - (10) of Table 6). In all specifications, the confidence interval for the treatment effect after two years includes the treatment effect after one year. Even though the point estimates for the two-year treatment effects in the at-risk group are rela-

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

tively large in magnitude, the increased standard errors imply that they are generally not or only marginally statistically significant. In summary, treatment effects after two years are too imprecisely estimated to draw a fully conclusive picture of how our intervention affected individuals' labor market outcomes in the longer run.

4 Conclusion

In this paper, we have reported the results of a field experiment investigating 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 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 are important factors impeding the employment prospects of these individuals. In light of the low cost of our intervention (the total costs of production and mailing were less than EUR 1 per brochure), our findings suggest that targeted information provision can be a cost-effective instrument in improving the labor market prospects of job seekers, at least among important subgroups of the population. It therefore also seems promising to examine in greater detail which type of information is particularly effective for different groups of job seekers, and what is the relative role of information and encouragement in enhancing the employment prospects of different subgroups. While a detailed assessment of the precise mechanisms through which our brochure worked is not possible in our present setting, we hope to further contribute to this interesting research agenda in future work.

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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: Overall Treatment Effects

	OLS		Median Regression	
	(1)	(2)	(3)	(4)
Outcome Variable: *Days of Employment (over 52 weeks)*				
Control Mean: 124.34				
Treatment	1.24	1.40	7.00**	4.96*
	(1.28)	(1.26)	(3.37)	(2.7)
Outcome Variable: *Cumulative Earnings (over 52 weeks)*				
Control Mean: EUR 11,450.71				
Treatment	146.94	138.98	478.15**	335.56*
	(138.89)	(134.86)	(228.7)	(172.0)
Controls	No	Yes	No	Yes
N	53,753		53,753	

Note: Each entry reports the treatment effect in a separate specification. Standard errors are reported in parentheses. Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Cumulative earnings correspond to the sum of an individual's labor market earnings during the year after the intervention. Control variables in Columns (2) and (4) 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 16 state and 4 wave-of-treatment fixed effects.

Table 3: Treatment Effects by Risk of Long-Term Unemployment

Sample:	Summary Measure High Risk				Summary Measure Low Risk			
	OLS		Median Regression		OLS		Median Regression	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome Variable: *Days of Employment (over 52 weeks)*								
Control Mean: 110.84 (High Risk) and 137.74 (Low Risk)								
Treatment	4.68^{***}	4.78^{***}	14.00^{***}	7.62^{**}	-1.89	-2.11	-6.00	-2.45
	(1.75)	(1.72)	(4.82)	(3.13)	(1.86)	(1.85)	(5.17)	(4.96)
Outcome Variable: *Cumulative Earnings (over 52 weeks)*								
Control Mean: EUR 10,197.20 (High Risk) and EUR 12,682.40 (Low Risk)								
Treatment	446.80^{**}	446.18^{**}	974.85^{***}	450.82^{*}	-122.79	-195.75	-229.65	-79.63
	(185.73)	(182.10)	(306.94)	(244.12)	(203.13)	(198.68)	(434.84)	(282.03)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	26,876		26,876		26,877		26,877	

Note: Each entry reports the treatment effect in a separate specification. Standard errors are reported in parentheses. Levels of significance: $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$. Cumulative earnings correspond to the sum of an individual's labor market earnings during the year after the intervention. 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 non-employment duration are included in the group "Summary Measure High Risk". Control variables in Columns (2), (4), (6), and (8) 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 16 state and 4 wave-of-treatment fixed effects.

Table 4: Robustness Check - Abadie et al. (2017) 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. (2017)
High Risk of Long-Term Unemployment	4.68	4.66
	(1.75)	(0.85)
Low Risk of Long-Term Unemployment	-1.89	-1.87
	(1.86)	(0.99)

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

Subgroup:	OLS	Abadie et al. (2017)
High Risk of Long-Term Unemployment	446.80	440.70
	(185.73)	(95.62)
Low Risk of Long-Term Unemployment	-122.79	-120.14
	(203.13)	(105.5)

Note: We implement the repeated split sample estimator proposed in Abadie et al. (2017) to address bias in the estimation of treatment effects for endogenously stratified subpopulations. See Table 2 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 2); 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. (2017). 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.

Table 5: 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***	3.86**	3.75**	4.01**	4.22**	2.19	2.29
	(1.28)	(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	146.94	138.98	446.80**	446.18**	338.59**	380.87**	438.56***	399.17**	222.72	193.39
	(138.89)	(134.86)	(185.73)	(182.10)	(165.68)	(162.46)	(169.17)	(166.71)	(145.43)	(142.56)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	53,753		26,876		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 correspond to the sum of an individual's labor market earnings during the year after the intervention. 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 16 state and 4 wave of treatment fixed effects.

Table 6: Treatment Effect Two Years After Treatment - Whole Sample and At-Risk Samples

			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 104 weeks)*										
Treatment	-0.28	0.30	5.80*	6.38*	5.54*	5.47*	3.85	4.56	1.53	2.02
	(2.49)	(2.45)	(3.47)	(3.37)	(3.06)	(3.00)	(3.30)	(3.24)	(2.85)	(2.78)
Outcome Variable: *Cumulative Earnings (over 104 weeks)*										
Treatment	-190.84	-200.73	309.76	325.31	369.74	378.69	205.47	117.18	17.17	-48.95
	(272.83)	(264.41)	(369.00)	(359.48)	(321.11)	(312.67)	(331.53)	(326.34)	(284.27)	(276.77)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	53,753		26,876		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 correspond to the sum of an individual's labor market earnings during the year after the intervention. 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 16 state and 4 wave of treatment fixed effects.

Figure 1: Information Brochure

Wissen, Ideen, Perspektiven

Arbeitslos?

Was tun?

Jetzt ist der ideale Zeitpunkt

Sie wollen so schnell wie möglich wieder Arbeit finden. Jetzt ist der ideale Zeitpunkt, um erfolgreich nach einer neuen Stelle zu suchen! Im Jahr 2010 hat sich die deutsche Wirtschaft spürbar von der Wirtschaftskrise erholt. Unternehmen und Betriebe stellen wieder vermehrt Arbeitnehmer ein. Seit Beginn des Jahres haben bereits über zwei Millionen Arbeitslose eine neue Beschäftigung gefunden. Auch derzeit gibt es mehrere hunderttausend offene Stellen.

Chance auf Arbeit
Wenig
Viel
Eigeninitiative

Chance auf Arbeit
Kurz
Lang
Dauer der Arbeitslosigkeit

Wussten Sie eigentlich, dass…

… aktive Stellensuche ein Schlüssel zum Erfolg ist? Viele Leute unterschätzen den Einfluss ihrer eigenen Anstrengungen massiv. Wissenschaftliche Untersuchungen zeigen: Der Erfolg aktiver Arbeitssuche ist viel größer als die meisten Menschen glauben. Eigeninitiative und intensive Arbeitssuche erhöhen Ihre Chancen Arbeit zu finden weit mehr als Sie vielleicht denken. Die Initiative zu ergreifen zahlt sich also aus.

… die Chance Arbeit zu finden umso geringer ist, je länger die Arbeitslosigkeit dauert? Es ist wissenschaftlich belegt: Grundsätzlich verringert sich die Chance einen neuen Job zu finden mit jedem Monat, den man arbeitslos ist. Zögern Sie also nicht. Jeder Tag zählt.

Arbeitssuche lohnt sich – nicht nur finanziell

Arbeitssuche lohnt sich. Forschungsarbeiten zeigen, dass sich Arbeit positiv auf die persönliche Lebenszufriedenheit auswirkt. Erwerbstätigkeit sorgt häufig für stabilere familiäre Beziehungen und niedrigere Scheidungsraten. Berufstätige leiden darüber hinaus weniger häufig unter depressiven Verstimmungen und sind seltener krank. Außerdem haben sie ein durchschnittlich geringeres Sterberisiko und erfreuen sich insgesamt besserer Gesundheit. Ein neuer Arbeitsplatz schafft auch neue soziale Kontakte und Bekanntschaften.

Viele Wege führen zum Ziel

Offene Stellen finden Sie im Stellenmarkt Ihrer Tageszeitung, im Internet und bei der Jobbörse der Arbeitsagentur. Nutzen Sie auch die Möglichkeit, sich bei Unternehmen direkt zu bewerben.

Was Sie vielleicht noch nicht wussten: Viele Arbeitslose finden eine neue Stelle durch Verwandte, Freunde und Bekannte. Scheuen Sie sich also nicht, von Ihrer Arbeitssuche zu erzählen. Viele Berufstätige waren auch schon einmal arbeitslos und können Ihre Situation gut nachvollziehen.

Sie fühlen sich manchmal niedergeschlagen und zweifeln daran, dass Ihre Stellensuche erfolgreich sein wird? Diese Empfindungen sind ganz normal und treten bei den meisten Personen nach dem Verlust des Arbeitsplatzes auf. Bleiben Sie trotzdem am Ball – schon Ihre nächste Bewerbung könnte zur neuen Stelle führen!

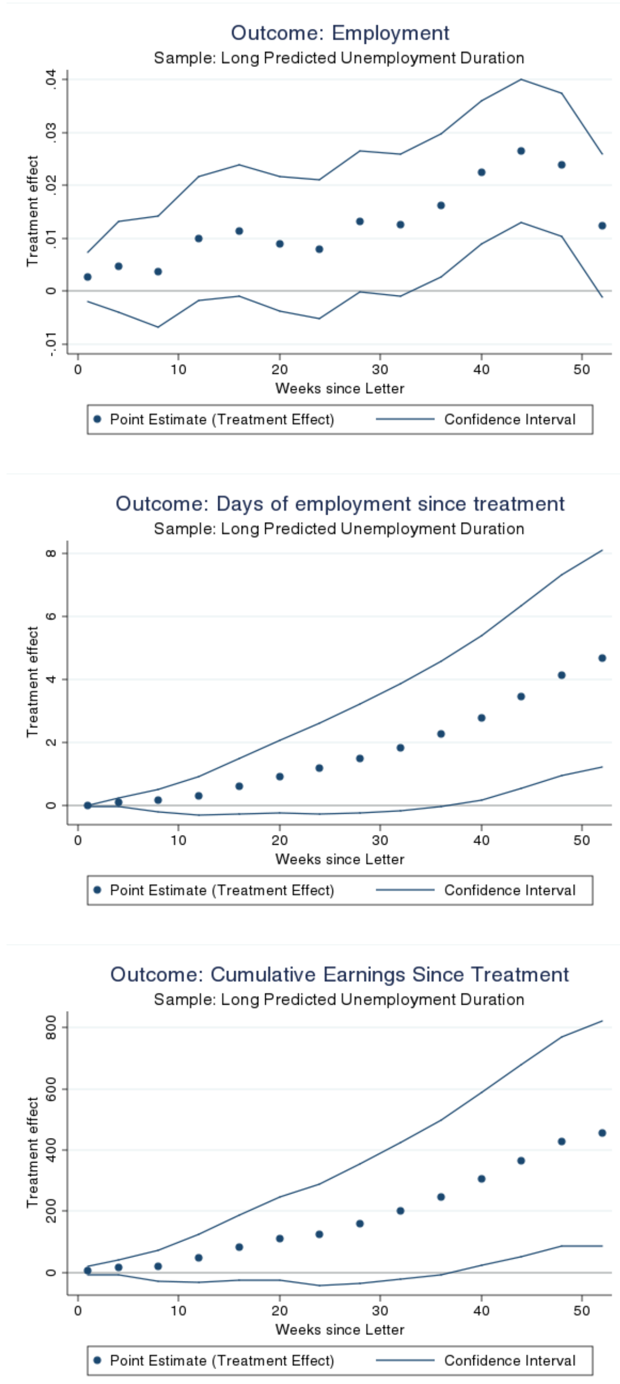
Bleiben Sie aktiv!

Beginnen Sie schon heute mit der Stellensuche:

- **Nutzen Sie die Jobbörse der Arbeitsagentur** http://jobboerse.arbeitsagentur.de
- **Informieren Sie sich im Internet** (einfach Stichwort „Jobbörse" in Ihre Suchmaschine eingeben)
- **Fragen Sie in Ihrem Freundes- und Bekanntenkreis nach offenen Stellen**
- **Sprechen Sie mögliche Arbeitgeber mit Ihrer Initiativbewerbung direkt an**

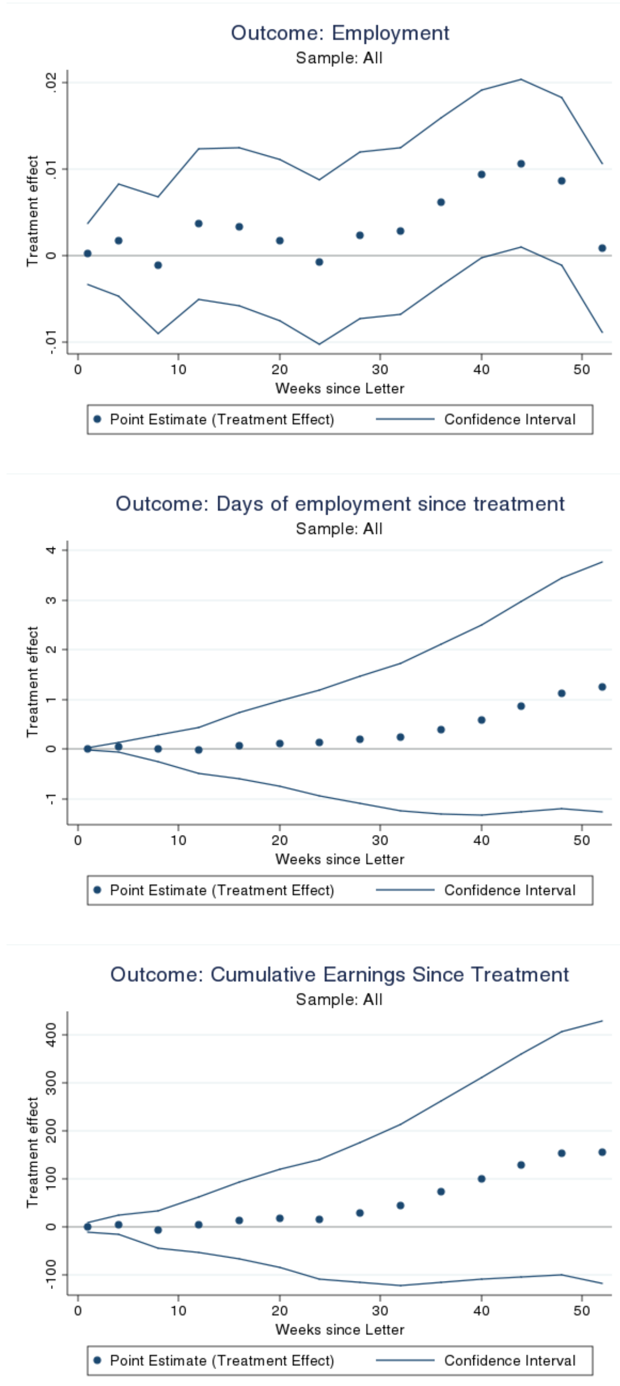
Kontakt: Universität Bonn | Abteilung für Empirische Wirtschaftsforschung | Adenauerallee 24-42 | 53113 Bonn | Tel.: 0228 823 69 456 | Email: wastun.info@uni-bonn.de
Bildnachweis: ©iStockphoto.com

Figure 2: Timing of Treatment Effects: Individuals with Long Predicted Unemployment Duration



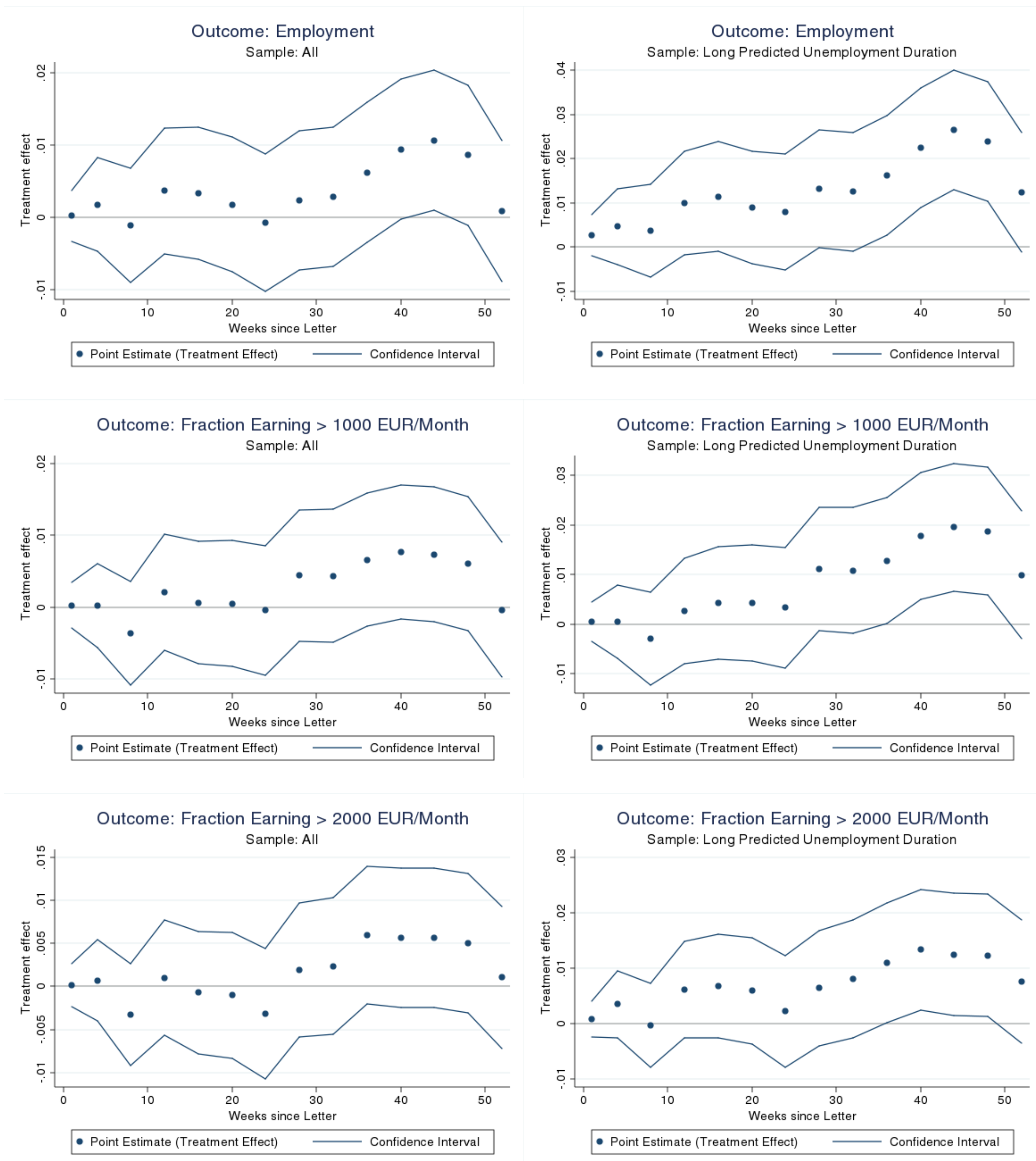
Note: Each blue dot denotes the point estimate for the treatment effect at a given time horizon based on OLS regressions. The sample in all specifications is restricted to individuals who were out of employment at the time of the intervention and who have an above-median level of overall predicted unemployment duration ($N = 26,876$). 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.

Figure 3: 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.

Figure 4: 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: Translation of the Brochure's 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 Photo credits: ©iStockphoto.com

Appendix C: Additional Figures and Tables (for Online Publication)

Table 7: 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 8: Determinants of Total Non-employment Duration in the Control Group

Outcome Variable: Days of Non employment (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 9: Summary Statistics by Treatment Status for Individuals at Risk of Long-Term Unemployment

Variable:	Control $[N = 20,070]$ (1)	Treatment $[N = 6,806]$ (2)	Difference $\|(2) - (1)\|$ (3)
Female	0.412	0.413	0.001
	(0.49)	(0.49)	[0.12]
Age	41.17	41.06	0.118
	(6.92)	(7.01)	[1.21]
Previous Daily Wage	45.92	46.02	0.11
	(36.51)	(36.47)	[0.21]
Days of Unemployment	1138.96	1149.65	10.69
(over ten years before treatment)	(969.0)	(973.2)	[0.79]
Education Category 1	0.296	0.300	0.004
	(0.46)	(0.46)	[0.58]
Education Category 2	0.502	0.501	0.002
	(0.50)	(0.50)	[0.22]
Education Category 3	0.019	0.019	<0.001
	(0.14)	(0.14)	[0.25]
Education Category 4	0.039	0.037	0.001
	(0.19)	(0.19)	[0.46]
Education Category 5	0.028	0.030	0.002
	(0.17)	(0.17)	[0.84]
Education Category 6	0.115	0.113	0.002
	(0.32)	(0.32)	[0.54]
Local Unemployment Rate	8.423	8.523	0.100
	(3.14)	(3.20)	[2.26]

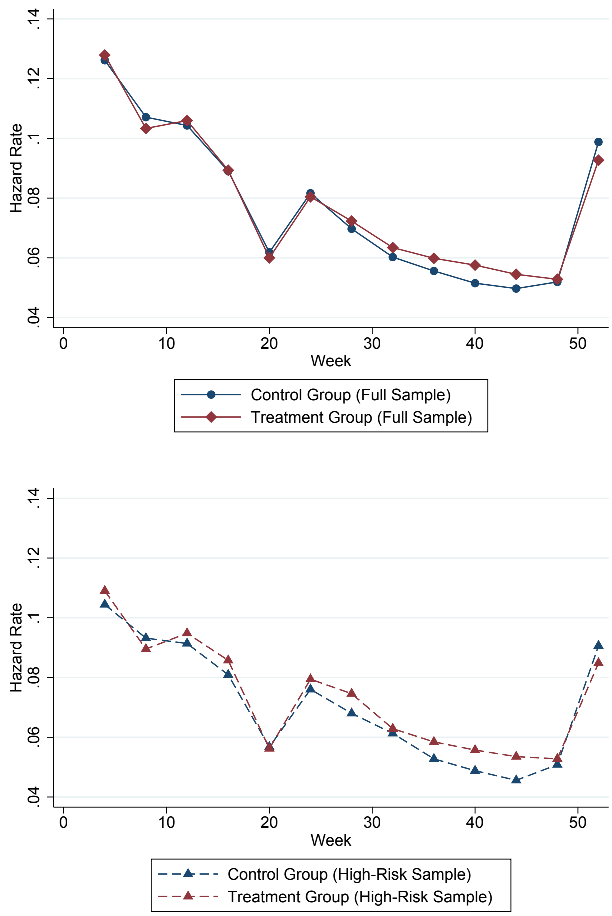
Note: All variables are measured before the treatment. The sample is restricted to individuals at risk of long-term unemployment (see Section 3.2 and Table 8). Standard deviations are reported in parentheses; absolute values of the t-statistics for differences between treatment and control group are 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 10: 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	-963.99***	24.16
	(1.31)	(2.63)	(139.31)	(280.7)
Education Category 2	35.8***	1.74	2577.19***	172.44
	(1.77)	(3.51)	(178.71)	(354.4)
Education Category 3	29.51***	10.66	4378.10***	1132.17
	(3.62)	(7.38)	(401.1)	(855.32)
Education Category 4	36.85***	-6.70	4165.76***	-246.53
	(2.89)	(5.87)	(310.7)	(639.32)
Education Category 5	34.17***	-1.74	4888.88***	-462.53
	(3.68)	(7.24)	(424.8)	(845.4)
Education Category 6	24.47***	2.63	5975.29***	247.77
	(2.67)	(5.29)	(328.8)	(645.2)
Age	-1.21***	0.38**	-108.7***	22.1
	(0.08)	(0.17)	(8.7)	(17.86)
Local Unemployment Rate	0.08	-0.43	20.2	9.0
	(0.21)	(0.41)	(21.6)	(43.3)
Previous Daily Wage	0.004***	-0.0038***	1.6***	-0.33**
	(0.006)	(0.0012)	(0.07)	(0.15)
Days of Unemployment	-0.005***	-0.0001	0.36**	-0.03
(over ten years before treatment)	(0.0008)	(0.0015)	(0.08)	(0.17)
Wave 2 (Indicator)	-1.33	-0.87	92.3	-54.5
	(1.82)	(3.69)	(197.2)	(400.4)
Wave 3 (Indicator)	8.19***	2.12	942.7***	167.0
	(1.83)	(3.67)	(195.6)	(393.0)
Wave 4 (Indicator)	17.39***	-0.41	1404.5***	61.0
	(1.84)	(3.69)	(192.2)	(385.5)
N	53,753		53,753	
R^2	0.0218		0.0420	
F-Statistic	46.40		66.29	

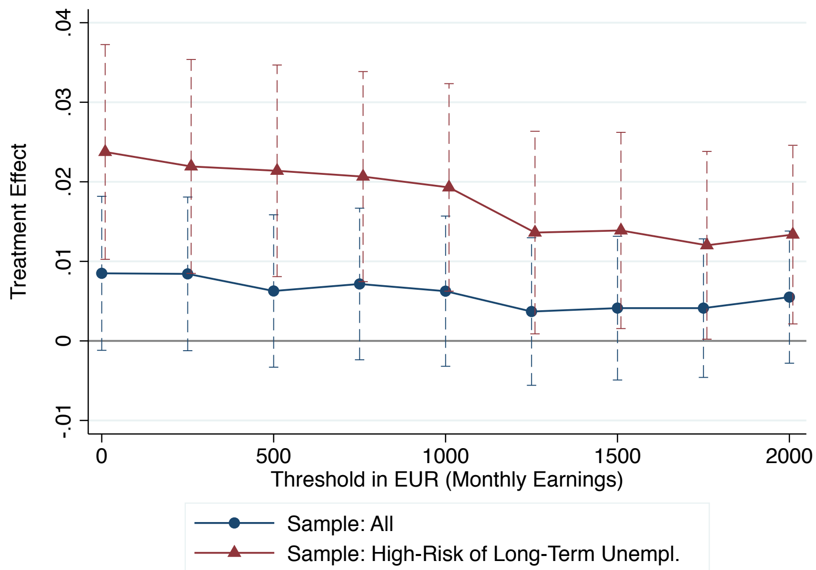
Note: The regression model includes the main effect for the treatment indicator as well as the main effects for each of the variables displayed and interacted with treatment status. Robust standard errors are reported in parentheses. Levels of significance: * < 0.10, ** < 0.05, *** < 0.01.

Figure 5: Exit Rates for Exit Into Employment



Note: The figures show hazard rates into employment for different samples. Exit rates are calculated as the fraction of individuals who are in employment in a given month conditional on not having been employed in the previous month. See Notes for Table 3 for more details on group definitions.

Figure 6: Treatment Effects on Probability of Employment at Different Margins of Monthly Earnings



Note: The figure shows the effects of the treatment on indicator variables for whether an individual holds a job with monthly earnings greater than the threshold amount indicated on the x-axis after a time period of 48 weeks after the treatment. For example, the maroon triangle at EUR 1,000 indicates a 2 percentage point effect on the probability of holding a job with monthly earnings of more than EUR 1,000. Navy circles indicate results for the whole sample and maroon triangles results for the sample at risk of long-term unemployment (see Notes for Table 3 for more details). Each circle and triangle stems from a separate regression. The vertical 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.