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On government-subsidized training programs for older workers

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Mit der Reihe „IAB-Discussion Paper“ will das Forschungsinstitut der Bundesagentur für Arbeit den Dialog mit der externen Wissenschaft intensivieren. Durch die rasche Verbreitung von Forschungsergebnissen über das Internet soll noch vor Drucklegung Kritik angeregt und Qualität gesichert werden.

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Abstract

We analyze the impact of the German WeGebAU programs, which are government-subsidized training measures for employed workers over 45 years old. We apply a dynamic matching approach similar to Crépon et al. (2009) and exploit novel information contained in rich German registry data. We focus on the effects on survival probability in original employment and estimate the effects separately by gender, age, job status, and program duration. We find that WeGebAU training improves the probability of remaining in paid employment by 1.0 to 2.5 percentage points in the two-year period following treatment. The effect is more pronounced for part-time workers and longer-duration program participants. Our analysis suggests that postponed labor market withdrawal is the main driver of the positive effects and that there is selection into treatment at the firm level.

Zusammenfassung

Wir untersuchen die Auswirkungen des Sonderprogramms WeGebAU, in dessen Rahmen die Bundesagentur für Arbeit (BA) Weiterbildungsmaßnahmen für Beschäftigte über 45 Jahren subventioniert. Wir wenden dynamische Matchingmethoden nach Crépon et al. (2009) an und nutzen neue Informationen aus den Prozessdaten der BA. Wir konzentrieren uns auf die Verbleibswahrscheinlichkeit in Beschäftigung und schätzen getrennte Effekte nach Geschlecht, Alter, Berufsstellung und geplanter Programmdauer. Es zeigt sich, dass WeGebAU die Wahrscheinlichkeit zwei Jahre nach Programmstart noch abhängig beschäftigt zu sein, um ein bis 2,5 Prozentpunkte erhöht. Der Effekt ist stärker für Teilzeitbeschäftigte und Teilnehmer an längeren Weiterbildungsmaßnahmen. Unsere Analysen weisen darauf hin, dass die Effekte in erster Linie auf einen verzögerten Arbeitsmarktrückzug zurückzuführen sind, und dass die Teilnahme auf Ebene der Betriebe selektiv ist.

JEL classification: J18, J14, I21

Keywords: Further training for employees, government-funded programs, dynamic matching

1 Introduction

In the coming decades, aging populations and high levels of public debt are expected to exert increasing pressure on public budgets. This problem is exacerbated by skill shortages and less-than-desirable labor force participation rates. As part of their planned countermeasures, governments are encouraging additional training for the current workforce.

Public sector involvement in labor market training can be justified by several types of market failures, such as credit constraints and positive externalities. Another justification is related to equity concerns and the social costs of unemployment (Hollenbeck, 2008). Many governments consider market-based training to be insufficient, especially for certain disadvantaged groups, such as older workers, mothers of young children, and low-skilled employees. Although workers from these groups confront the highest risk of job loss and skill depreciation, their participation in training is disproportionately low. For instance, approximately 40 percent of all German firms offer further training, but only 6 percent provide training for employees over 50 (Bellmann and Leber, 2008).

There is a large body of literature on active labor market program (ALMP) evaluation. As both Kluve et al. (2007) and Card et al. (2010) demonstrate, a substantial share of the literature addresses the evaluation of German programs. Most of the programs (and the related literature) are devoted to improving the qualifications of the unemployed (see for example Greenberg et al., 2003; Heinrich et al., 2009). Evaluations of the corresponding German programs typically find negative lock-in effects in the short term but positive effects on employment and earnings over the medium and long term (Lechner and Melly, 2010; Lechner et al., 2011; Fitzenberger and Völter, 2007; Lechner et al., 2007; Fitzenberger and Speckesser, 2007; Fitzenberger et al., 2008).

In comparison, the body of literature on the effects of government-sponsored further training for employed workers is much smaller. Most of the available studies tend to focus on marked-based training in the European labor market and typically concentrate only on firm-level outcomes. The results are quite heterogeneous and vary between rather large returns (Almeida and Carneiro, 2009) and small figures (see Bassanini et al., 2005: for a review). For instance, Swiss training vouchers encouraged workers to increase training participation (Messer and Wolter, 2009), and Irish government subsidies improved training investments by firms (Görg and Strobl, 2005). In another study, Dutch tax incentives for employee training were analyzed by Leuven and Oosterbeek (2004), who found that the incentives failed to produce any measurable improvement in wages. By contrast, Swedish adult education schemes appeared to produce notable (albeit costly) positive effects (Albrecht et al., 2009; Stenberg, 2011).

Of the programs previously analyzed in the literature, perhaps the most similar to the German WeGebAU is the UK government pilot scheme "Train to Gain." This program aims to increase the level of training provided by employers and focuses on low-skilled employees by offering free or subsidized training for improving basic qualifications, paid time off for training, and wage compensation. Unfortunately, Abramovsky et al. (2011) find that the program failed to produce any notable increase in the proportion of firms that offer training

or in the proportion of workers participating. A German program closely linked to WeGebAU is a training voucher program that was implemented in January 2006 in the North Rhine-Westphalia Federal State. This program covered 50 percent of the direct training costs for small and medium-sized establishments. Görlitz (2010) finds that the program increased the share of firms investing in training by 4 to 6 percentage points but had no effect on training intensity.

Our study advances the small body of literature on the evaluation of publicly funded further training programs for employed workers in several ways. First, unlike the bulk of the existing studies (Abramovsky et al., 2011; Görlitz, 2010), we focus on outcomes at the individual level as opposed to the firm level. Thus, we provide evidence showing that training programs eventually generate benefits from the participants' perspective–in terms of earnings and employment rates. Second, we analyze a different demographic group, older workers, who confront particular labor market difficulties that may intensify in the future. Therefore, we contribute to the literature on the heterogeneous effects of active labor market programs and to the policy debate pertaining to employment of older workers. Finally, we evaluate the German WeGebAU program, a special job-training subsidy introduced in 2006 to target workers who are at least 45 years old in small and medium-sized firms.[1] This program has not previously been analyzed.

Our analysis focuses on post-program employment, and we use registry data from the German Federal Employment Agency, which offers a rich data set that integrates information from different government sources and provides extensive information on individual employment and unemployment histories and workplace characteristics. Because treatment is not randomly allocated, we employ the dynamic matching framework developed by Crépon et al. (2009). This method identifies the average treatment effect on the treated (ATT) for monthly strata of participants by comparing them with other eligible workers who did not participate in the program up to the moment of the outcome.

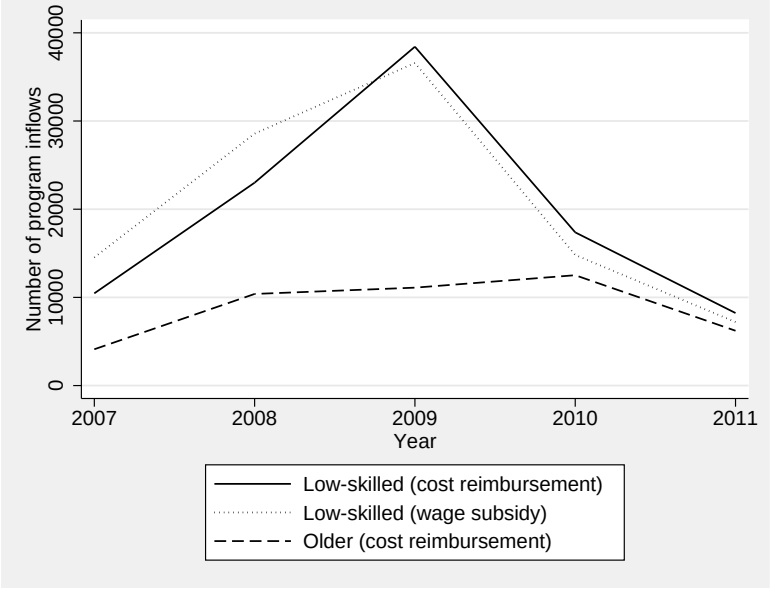
Our results show that WeGebAU increases employment stability by increasing the probability of remaining in paid employment by 1.0 to 2.5 percentage points for two years after the treatment; the effect on entry to insured unemployment is negative but small. The decreasing exit rate from employment is accompanied by a comparable increase in the number of days of employment, with a small and (in most cases) statistically insignificant wage improvement. The effect is more highly concentrated for part-time workers, for those workers in longer training programs, and over 50 years of age. This finding suggests that the positive effect primarily originates from a decreasing tendency to leave the labor force.

2 Background

WeGebAU (Förderung der Weiterbildung Geringqualifizierter und beschäftigter Älterer in Unternehmen) is a special job-training program that was introduced in 2006 by the German Federal Employment Agency. The program is open to employed workers, including

[1] The other main group in the WeGebAU "high-risk" category includes low-skilled workers with no professional qualifications (see section 2).

Figure 1: Inflows to WeGebAU by eligibility criteria and subsidy type



Source: Statistics Department of the German Federal Employment Agency, July 2012.
Groups are partially overlapping because low-skilled participants may receive both wage subsidies and cost reimbursement.

new hires (in 2006, it was also open to the unemployed). The program's main objective is to provide further qualifications to those workers who are considered particularly vulnerable to unemployment and loss of employable skills. The government has identified two such groups: older employees and low-skilled employees in small and medium-sized firms. Since 2007, "older" ones must be at least 45 years of age, and "small firms" must be firms with fewer than 250 employees.

Most of the program participants belong to the low-skilled group. Figure 1 shows the enrollment of low-skilled and older workers over time; there is a rapidly increasing inflow after reforming the program in 2007. Largely unknown in the early years, the program was thereafter widely advertised by local employment agencies. During the recent economic crisis, its funding was increased, and its eligibility criteria were loosened, which caused participation numbers to peak in 2009. In the following years, the criteria were followed more closely again, and participation numbers thus declined substantially.

Both employees and employers can apply for the subsidy. However, during the early years, caseworkers typically initiated participation by personally visiting firms and promoting the program. After one of the parties files an application, the caseworker decides whether the employee fulfills the eligibility requirements. If the requirements are met, then the employee receives a voucher that guarantees reimbursement of training costs (direct program costs and an allowance for related expenditures, such as child care or transportation expenses) and can choose a certain schooling program offered by an educational institution. The program must be approved by the employment agency. Training must be conducted by an external certified institution, and courses must focus on improving general human capital and must apply to the wider labor market, as firm-specific training is not eligible for subsidies. In 2008, approved courses included the following: vehicle operation; health

Table 1: Length of program participation in days for the treatment group by quantiles

Percentiles	10%	25%	50%	75%	90%	Min	Max
Days	2	8	66	164	317	1	1472

care; metal construction; medical, mechanical, and automotive engineering; transportation; computer science and ICT; security; and production control. The employer must continue paying wages while the worker is participating in training.

On average, the enrollees in our sample were assigned to training for 115 days, with a minimum of one day and a maximum of 1,472 days. These figures do not necessarily indicate the actual length of full-time training but rather indicate a time span within which an individual is registered as a participant. These periods may include shorter or longer periods of actual participation. Table 1 provides an overview of the corresponding distribution quantiles.

3 Econometric Method

3.1 Dynamic Matching

This study analyzes the differences in labor market outcomes for WeGebAU participants and non-participants. The discontinuities in the program design (related to age and firm size) may initially appear to be an attractive identification strategy. However, during the observation period, the program enrolled only approximately 10,000 individuals out of a total labor force of 10 million in the relevant age range. Hence, at one side of the discontinuity, the probability of treatment is only approximately 0.1%, which is too small to noticeably affect the outcomes. Thus, we must rely on methods that are designed for analyzing non-experimental data. As is typical for employed workers, non-random selection may occur at both the individual and firm levels. At the individual level, those who receive WeGebAU subsidies may be more or less employable than other workers. Analogously, the performance of firms that choose to use the subsidies may be either better or worse than that of other similarly situated firms.[2] At the firm level, participants may originate either from the most profitable and high-performing firms that have management resources available for identifying and locating suitable programs and subsidies or from firms that are confronting difficulties and are seeking any type of support available, including WeGebAU.

Because the allocation of WeGebAU subsidies is not randomized, we can only base our inferences on observed differences. Matching methods are a popular approach to identify the average treatment effect on the treated (ATT) in this type of setting. Let D be the

[2] In the interpretation of our results, it is important to note that we do not have information on privately funded training in our data. However, with only approximately 6 percent of all firms offering training for employees over 50 years old, privately financed training activities for older workers are considered relatively unimportant in Germany (see Bellmann and Leber, 2008).

participation indicator, where $D = 1$ indicates participation and $D = 0$ refers to non-participation in a WeGebAU-sponsored program. Let $Y^D(\tau)$ be the outcome of interest, such as the survival rate in employment τ weeks after the beginning of the program, and $\boldsymbol{X}$ a vector of individual and employer-specific characteristics. Propensity score matching (Rosenbaum and Rubin, 1983) (which we use below) compares the outcomes of program participants with those of non-participants whose propensity scores $P(\boldsymbol{X}) = \Pr(D = 1|\boldsymbol{X})$ are similar.

With respect to WeGebAU, we do not have a stationary treatment arrangement with pre-determined treatment and control groups. Since the introduction of the program, certain workers are eligible for participation; from time to time, some of them choose to enter the program. Employed workers who are not treated now may be treated at any time in the future, and it would not be accurate to compare the outcomes of those who were treated to those who were never treated; because treatment in the case of WeGebAU is conditional on employment status, such a comparison would amount to selection based on the outcome. This type of policy analysis in a dynamic setting is discussed by Crépon et al. (2009); Sianesi (2004) and Fredriksson and Johansson (2008). As we are analyzing employment rather than unemployment, our approach differs slightly from that in those studies.

Let T_t be a random variable denoting the calendar time for treatment[3]. We are interested in the average treatment effect at time $t + \tau$, τ days after the treatment, which occurred at calendar month $T_t = t$ (obviously, we want to obtain these estimates for a number of different τ-s). Let $Y_t(t + \tau)$ denote the outcome of interest for a worker who is treated at t, and let $Y_\infty(t+\tau)$ denote the never-treatment (always-later-treatment) outcome of a worker treated at t. We want to estimate

$$ATT_t(t + \tau) = \mathbb{E}[Y_t(t + \tau) - Y_\infty(t + \tau)|T_t = t], \tag{1}$$

the difference in the post-treatment outcome at $t + \tau$ of those treated at $T_t = t$ and the non-treatment outcome of the same group in the hypothetical scenario in which they had not been treated. The first of these quantities, the outcome τ days after the treatment at t, is easily observable. The second term is the expected counterfactual, the never-treated outcome at the same point in time, for those treated at t. Note that ongoing participation may not prevent employers from firing an employee. However, if firms tend to postpone lay-offs until training is completed, then we observe a positive "lock-in" effect that corresponds to the program duration. The lock-in effect may cause participants to retain employment for the duration of the program. However, because 75 percent of the programs are less than half a year in duration, we are certain that the ATT at longer durations reflects the treatment effect.

To identify the ATT, we require additional assumptions: a form of no-anticipation condition and a modified version of the conditional independence assumption (CIA). The former can be written as follows:

$$Y_{t^*}(t) = Y_{t^{**}}(t) \qquad \forall t < \min(t^*, t^{**}). \tag{A1}$$

[3] As in Crépon et al. (2009), we consider treatment to begin at the first entry into a WeGebAU-sponsored program.

Intuitively, this assumption establishes that the individual outcome today does not depend on whether or when the person will eventually be treated in the future. This assumption neither allows the anticipated future treatment to influence the current outcome nor allows the reverse scenario–the anticipated outcome may not influence current participation.

The adapted version of the CIA establishes that

$$Y_\infty(t) \perp\!\!\!\perp T_t | P(X). \tag{A2}$$

Intuitively, this assumption states that the never-treated outcome is independent of whether and when the treatment actually occurs.

Assumptions (A1) and (A2) allow us to write the counterfactual term in (1) as

$$\mathbb{E}[Y_\infty(t+\tau)|P(X), T_t = t] = \mathbb{E}[Y_\infty(t+\tau)|P(X), T_t > t+\tau]. \tag{2}$$

Effectively, we observe the never-treated outcome $Y_\infty(t)$ for all those workers who are not yet treated as of calendar time $t+\tau$.

This method allows us to consistently estimate the treatment effect on the exit rate from employment. However, for a number of potentially interesting variables, such as employment probability or earnings at a certain point in time, the results may be biased. The reason is simple: over time, an increasing number of eligible individuals in the control group are treated, whereas the ineligible individuals remain, including those who terminated their employment. Hence, the control group will increasingly contain non-employed individuals for whom we are unable to observe the (latent) time of treatment. However, all actual exits from the treatment and control groups are still observed. Hence, methods such as survival analysis that rely only on these events do not suffer from that bias. Thus, we select survival in employment as our main outcome variable. Because the actual number of controls entering treatment is not large, we expect the bias in the other estimates to be minor.

The employed members of the older population do not experience any event that fundamentally redefines their situation, such as leaving a job for the unemployed. Indeed, most of these individuals have been working for decades; thus, we do not expect their behavior and relevant outcomes to change rapidly. This expectation is similar to the assumption that the ATT does not depend on the exact date of treatment ($ATT \equiv ATT_t \quad \forall\, t$) within our two-year observation window. Hence, we may average the monthly estimates (we stratify the sample according to the month of beginning treatment below).[4] We choose a stratified matching approach in which we select controls only in the corresponding months. Finally, we also assume independent censoring for the baseline model. This assumption is harmless with respect to the fixed end date for the observation period.

[4] Most of the individual monthly estimates are positive but not statistically significant. No clear trend or other pattern is visible from these estimates.

3.2 Is the CIA Correct?

CIA, the assumption that the non-treatment outcome is independent of the treatment status conditional on observed characteristics, is one of the most crucial assumptions behind matching. To obtain more insight into the actual selection on the program, numerous interviews in 10 labor market agencies across Germany were conducted during the fall of 2011.[5]

This qualitative evidence indicates that in the first years after WeGebAU introduction (our observation period), the caseworkers typically contacted the firms and promoted the program directly by, for example, giving presentations on WeGebAU opportunities in the workplace. This initiative on the caseworker side ensures that the program information was disseminated independently of the performance of firms and workers, and it eliminates a potential source of bias, namely, that only a select group of workers may learn about the program. Thus, the results indicate that the CIA holds in our data unless the bias at the time of uptake is overly strong.

This evidence suggests that the stable unit treatment value assumption (SUTVA) also holds. SUTVA implies that the treatment effect of a single individual is independent of the participation decision of any other individual (Rubin, 1986). Both the explanation above and the small size of the program (in terms of inflow numbers) suggest that SUTVA is not violated in our data.

3.3 Operationalizing the Matching

Participation in WeGebAU involves decisions by at least two actors–employees and employers. Selectivity may occur at both levels. First, matching addresses the case in which participants constitute a selective subsample of workers. Second, the firms' choices to participate in WeGebAU may also be related to their business prospects, potentially introducing spurious correlations between workers' outcomes and participation. We address this bias by selecting control group individuals from only those firms that are already participating in WeGebAU—from firms in which at least one employee has previously received subsidized training by the month that we analyze (see section 4.1 below). We refer to this control group as the "Same Firm Sample." To assess the robustness of our results from that perspective, we also create another control group, henceforth referred to as the "All Workers Sample." This sample is created by merging the Same Firm Sample with 600,000 workers randomly drawn from firms not (yet) participating in WeGebAU in 2007 and 2008. In the All Workers Sample, individuals leave the control group as soon as their employer begins using WeGebAU subsidies. To correct for potential firm-level selectivity, we control for firm descriptors when computing the propensity score.[6]

Following our dynamic matching approach, we divide our observation period into monthly

[5] The interviews were conducted by IAB affiliates, permanently working in different local employment agencies.

[6] We also estimate a model in which firm descriptors are replaced by firm fixed effects in the Same Firm Sample. The results (which are available upon request) are qualitatively similar.

strata. This procedure yields 18 subsamples, one for each calendar month from July 2007 to December 2008. We employ a single matching procedure for each month using the psmatch2 module in Stata12 (Leuven and Sianesi, 2003). We apply Epanechnikov kernel matching, which uses the smoothed weighted average of all individuals in the control group that lie within a given bandwidth of the propensity score to construct the counterfactual outcome[7]. We use a bandwidth of 0.01 for the Same Firm Sample and 0.001 for the All Workers Sample.

We also conduct a placebo analysis by estimating the effect of the program on pre-program employment duration. These estimates are close to zero and are not statistically significant (see Appendix A.1).

4 Data and Variables

4.1 Data

We use registry data from the German Federal Employment Agency, the Integrated Employment Biographies Sample (IEB). The data originate from the IAB employment history and the IAB benefit recipient history, as well as the participants-in-measures history (MTH) and the job-seeking history (ASU). From the establishment history panel (BHP), we further add the characteristics of firms. We thus use information on all employment subject to social security tax, various benefits, and participation in ALMP-s.

However, we cannot clearly identify periods during which the individuals neither receive benefits nor pay social security contributions. In particular, we cannot differentiate between self-employment and non-benefit inactivity. Hence, we look at two types of periods: *non–paid employment* periods, during which individuals do not receive any wages, and *non–benefit* periods, during which individuals do not receive unemployment benefits. Technically, the non–paid employment periods amount to re-defining the unknown state as a non–paid employment period, while in the case of non-benefit periods we treat the unknown state as a non-benefit period.

Because the program inflow was small in early 2007, we limit our sample to those who entered WeGebAU between July 2007 and December 2008. We further restrict our analysis to individuals who were at least 45 years old when beginning the treatment and who were working in firms with fewer than 250 employees. To capture the relevant pre-treatment and post-treatment outcomes, we collect information on all periods that end after January 1, 2000. The latest date for which we have information available is December 31, 2009. Our final sample (Same Firm Sample) includes approximately 7 300 treated individuals, and the All Workers Sample includes approximately 8 000 treated individuals (Table A.2 presents a more detailed overview of the number of observations).[8]

[7] A summary of the advantages and disadvantages of this matching estimator is given in Caliendo and Kopeinig (2008). The main results remain robust when using different types of matching.

[8] Note that individuals may appear in the control group more than once as controls for those whose treatment begins at different points in time. The treated individuals may serve as controls as long as they have not entered treatment themselves.

4.2 Treatment and Outcome

The current analysis focuses on the individual effects of WeGebAU subsidies. More specifically, we ask whether the measures (i.e., public subsidies for certain training courses) have any effect on the employability of the participants. Accordingly, treatment in the current analysis indicates participation in a training program sponsored by WeGebAU, and non-treatment indicates that a person is not participating in a WeGebAU-sponsored program. Note that "non-treated" workers may also participate in training, even in the same programs, when the costs are not covered by WeGebAU because we are interested in the effects of the public training subsidies rather than the actual programs that the individuals choose.

Our first outcome of interest is employment status, particularly survival in uninterrupted employment. This outcome corresponds to the original idea behind WeGebAU, namely, to improve the "employability" of workers in certain risk groups. In particular, more employable individuals should be less likely to leave employment for a period of non-employment, either because they cannot find a job or for other reasons. As stated above, we focus on paid employment as we cannot distinguish between self-employment and certain non-employment spells. This approach broadly answers the question "Does treatment increase the tendency to remain in paid employment?" Formally, we divide the individual labor force history into employment and non-employment periods and analyze the survival rate in initial employment. Note also that survival in employment allows for job changes. Analogously, we analyze survival in the non-benefit state. This approach addresses the question "Are the treated individuals less likely to begin claiming benefits?"

As explained above, we are able to provide unbiased estimates for survival in employment, but not for other relevant outcome variables. However, because only 1 764 controls out of 69 605 (Same Firm Sample) enter treatment during the entire observation period, we argue that the bias for other variables of interest is small and thus analyze other outcomes as well. First, we assess the effect on the median monthly earnings from the beginning of WeGebAU training until one year later.[9] Next, we analyze the employment rate over the period from zero to 360 days after the beginning of the program and then for the period from 360 to 540 days after the beginning of the program. Our last outcome variable is job mobility, which refers to leaving one employer for another employer or becoming unemployed. This approach is analogous to our first outcome variable, employment stability.

4.3 Explanatory Variables

CIA assumes that we condition on all relevant individual information. Because we employ registry data, we are able to include common individual descriptors, such as gender, age and education, firm characteristics, and individual labor market history; we also include a

[9] The income measure is top-coded, with the maximum income threshold set between 171 to 180 Euros per day for the 2004-2009 period in West Germany and set between 145 to 152 Euros in East Germany. The threshold affects approximately 206 participants and 2 763 non-participants in the Same Firm Sample. Because these figures are rather low, we ignore the top-coding in the estimations below.

Table 2: Variables used for propensity score matching

Variable group	Variables
Socio-demographic characteristics	Gender (dummy), nationality (dummy), age, attained degree of schooling (four dummies, reference: no degree), job status (three dummies, reference: unskilled blue-collar worker), interaction term (female×jobstatus), and wage at treatment start
Labor market history for four different periods: 12, 24, 36, and 24 months prior to beginning of treatment	Number of previous firms, recall (dummy), tenure with and without interruptions (months), employment (months), number of employment periods, mean duration of employment period, unemployment with receipt of benefits (months), number of unemployment (benefit) periods, mean duration of unemployment (benefit) period, job search (months), number of job search periods, mean duration of job search period, and daily wage
Firm characteristics	Firm size (three dummies, reference: 1-49 employees), firm age, sectoral affiliation (seven dummies, reference: construction), skill structure in the firm: share of low-skilled workers, share of middle-skilled workers, share of high-skilled workers, share of workers with unknown skill levels, share of unskilled blue-collar workers, share of skilled blue-collar workers, share of white-collar workers, and share of part-time workers
Regional characteristics	Unemployment rate in the month of beginning treatment at the district level, unemployment rate squared, and living in East or West Germany (dummy)

Notes: Individual, firm, and job characteristics are measured at the beginning of the employment period parallel to treatment.

set of variables describing the total amount of individual employment and unemployment during the past 12, 24, 36, and 60 months. As is typical for registry data, we do not observe individual desires and expectations, including those related to future job prospects. However, Wunsch and Lechner (2008) show that conditioning on this type of variables (such as health, benefit sanctions, and characteristics of the desired job) leaves the results for similar programs essentially unchanged. Table 2 explains the variables used for propensity score matching. Table 3 presents sample means for selected variables before matching (full statistics are available upon request).

Most indicators suggest that both treated and non-treated individuals tend to be similar. A few differences that arise, predominantly in the Same Firm Sample, merit further discussion. The treatment group slightly oversamples young eligibles and undersamples the older age groups. This difference is consistent with the life cycle investment in human capital. The treated group also appears to have completed more education than the control group. Relative to the control group, the larger share of skilled blue-collar and white-collar workers in the treated group indicates that more educated workers are more likely to participate in training (Fouarge et al., 2010; Fertig and Huber, 2010; Arulampalam and Booth, 1997). In the All Workers Sample, there are few educational differences, but part-time workers are undersampled in the treatment group. Past employment history is rather similar, but

Table 3: Sample statistics

Variables	Same Firm Sample Treated	Same Firm Sample Controls	All Workers Sample Treated	All Workers Sample Controls
Socio-demographic characteristics				
Female	0.4052	0.4219	0.4108	0.4793
Age	51.5266	52.8190	51.4993	52.3386
Unskilled blue-collar worker	0.1804	0.1995	0.1777	0.1755
Skilled blue-collar worker	0.2864	0.2565	0.2830	0.2096
White-collar worker	0.3501	0.3175	0.3565	0.3792
Part-time worker	0.1830	0.2265	0.1827	0.2357
Wage at treatment start (EUR/day)	80.2436	82.7232	79.0397	81.1900
Labor market history for the previous 730 days				
Tenure without interruptions (months)	22.5077	22.8506	22.3708	22.5218
Months in employment	24.0921	24.1480	24.0703	24.0772
Number of employment periods	1.0273	1.0246	1.0282	1.0259
Mean duration of employment period	82.0834	85.1924	81.3390	83.2691
Months unemployment (benefit receipt)	0.1669	0.1179	0.1777	0.1519
Number of unemployment periods (benefit receipt)	0.0556	0.0391	0.0594	0.0446
Mean duration of unemployment period (benefit receipt)	0.3968	0.2743	0.4168	0.3426
Firm characteristics				
1-49 employees	0.3686	0.1413	0.4171	0.4618
50-99 employees	0.3197	0.2647	0.2955	0.2139
100-149 employees	0.1477	0.2305	0.1361	0.1469
150-249 employees	0.1640	0.3635	0.1512	0.1774
Share of unskilled blue-collar workers	16.5935	18.4072	16.3593	15.5456
Share of skilled blue-collar workers	25.4814	23.2971	25.1200	18.4183
Share of white-collar workers	27.5602	28.6051	28.0433	32.9328
Share of part-time workers	25.7883	25.2446	25.9091	29.2916
Regional characteristics				
East Germany	0.3133	0.2714	0.3167	0.2132
Unemployment rate at district level	9.7186	9.3773	9.7479	9.1436
N	7,360	70,773	8,014	3,956,407

WeGebAU participants have experienced a slightly higher number of unemployment periods (benefit receipt) in the two years before beginning training, with a longer average benefit period duration. Accordingly, these participants spent less time in employment and had a larger number of shorter employment periods compared with non-participants. The participants also tended to remain with the same employer for a shorter period. The mean daily average earnings were slightly lower for WeGebAU participants than for non-participants. However, all of these differences are rather small.

5 Results

In this section, we present the estimates. First, we describe the effect of the program on survival in paid employment and on survival in the non-benefit period. Thereafter, we examine the yearly employment rate, earnings and job mobility. Finally, we analyze the effect of heterogeneity for different subgroups.

5.1 Survival in Employment

Table 4 and Figure 2 present the estimated effects for survival in employment–the difference in employment survival rates between WeGebAU participants and non-participants. We repeat here that survival in employment is an indicator of uninterrupted employment, not overall employment. Table 4 displays the ATT (= survival rate for the treated − survival rate for the non-treated) for every 90 days for the first two years following the beginning of the WeGebAU program. Outcomes are estimated separately for paid employment periods which treat the unknown state as non–paid employment, and the non-benefit periods which treat the unknown state as a non-benefit period.

First, if unknown periods are treated as non–paid employment periods, then we can observe a clear positive effect. This indicates that the employment stability of the participants exceeds that of the non-participants. By the end of the second year, participants are approximately 2.5 percentage points more likely to remain in paid employment than non-participants (see the survival curves in Figure 2). The corresponding survival rates remain high. For instance, 720 days after the beginning of the program, these survival rates are approximately 82.7 percent and 80.0 percent for the treated and controls, respectively. Because our data cover a rather short time span, the standard errors become very large as they approach a duration of two years.

When treating unknown periods as non-benefit periods, we also find initial positive effects. However, these effects are smaller and become statistically insignificant before approximately two years. Participants are only 0.5 percentage point less likely to begin receiving benefits compared with non-participants. This indicates that the program mainly discourages workers from leaving paid employment; the influence on unemployment benefit claims is small.

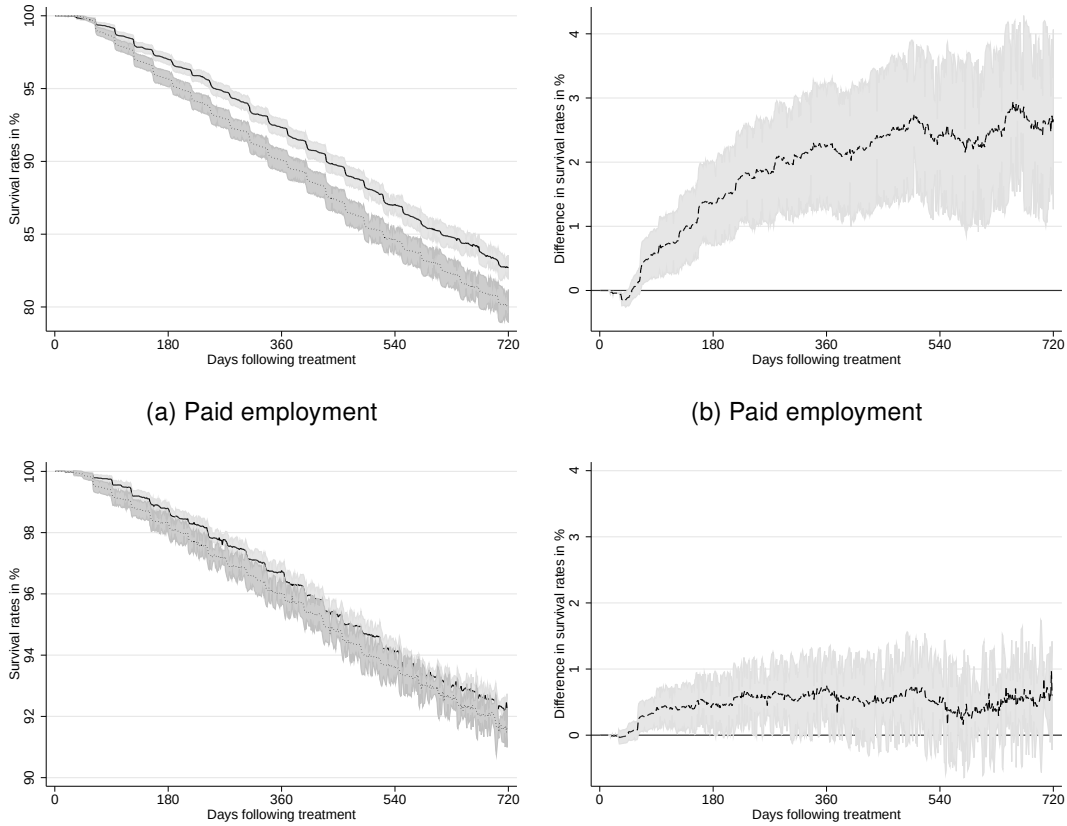
The estimates using individuals from non-participating firms as controls (All Workers Sample in Table 4) are smaller and become statistically insignificant after approximately one year. Note also that the confidence intervals for both samples largely overlap. However, lower point estimates still suggest that comparable employees from firms that have not (yet) participated in WeGebAU remain slightly longer in initial employment than controls from participating firms. The results lend some support to the idea that WeGebAU is used as a substitute for temporary layoffs during difficulties. Alternatively, the different estimates for the All Workers Sample may stem from firms with only a few older employees–recall that this sample also includes firms with only one worker. We will keep these considerations in mind when discussing further results. However, as we focus on individual effects, we primarily use the Same Firm Sample below.

Table 4: ATT estimates in percentage points for non–paid employment and non–benefit periods

	Impact on:					
	Paid employment			Non-benefit		
Duration	ATT	SE	N(Tr)	ATT	SE	N(Tr)
	Same Firm Sample					
90	0.58***	0.1928	7,154	0.40***	0.0841	7,193
180	1.35***	0.3321	6,998	0.48***	0.1425	7,125
270	1.86***	0.3457	6,835	0.56***	0.1871	7,051
360	2.25***	0.4086	6,661	0.76***	0.1894	6,975
450	2.40***	0.5196	5,834	0.48**	0.2400	6,182
540	2.42***	0.2732	4,312	0.58***	0.1931	4,649
630	2.52***	0.6399	3,447	0.36*	0.1473	3,759
720	2.66***	0.7145	2,139	0.60	0.4188	2,368
	All Workers Sample					
90	0.29**	0.1402	7,807	0.22***	0.0701	7,852
180	0.59***	0.2089	7,617	0.28**	0.1404	7,758
270	0.57**	0.2750	7,415	0.22	0.1986	7,658
360	0.61*	0.3351	7,213	0.26	0.2020	7,567
450	0.44	0.3887	6,294	-0.10	0.2304	6,681
540	0.25	0.4363	4,641	-0.31	0.2445	5,018
630	0.20	0.4842	3,713	-0.35	0.3063	4,061
720	0.19	0.5456	2,319	-0.29	0.3250	2,554

Notes: Duration in days after treatment. ATT displays the difference in the survival rate in the original period between participants and non-participants.
Standard errors obtained by bootstrapping (Same Firm Sample: 500 repetitions, All Workers Sample: 50 repetitions).
Significance level: *** 1%, ** 5%, * 10%.

Figure 2: Survival rates in percent (left panel) and ATT estimates in percentage points (right panel) for non-paid employment and non-benefit periods. Same Firm Sample specification.



(a) Paid employment

(b) Paid employment

(c) Non-benefit

(d) Non-benefit

In summary, our analysis suggests that uninterrupted employment periods are longer for treated workers than for workers in the control group. Up to two years after the beginning of treatment, the difference in the survival in paid employment is approximately 2.5 percentage points in favor of the participants, relative to the non-participants. However, the effect on benefit claims is smaller.

Next, we analyze the impact of the program on job mobility (Table 5 and Figure A.1). We observe a pattern that closely resembles that for employment: treated individuals are more likely to remain in the original firm than control individuals. The ATT estimates are larger than those for employment stability discussed above and are statistically significant in most cases, varying between 1 and 2 percentage points. For the All Workers Sample, we obtain positive but slightly smaller numbers. Together, these results suggest that participation lowers both employment–to–non-employment mobility and between-firm mobility.

5.2 Analysis of Different Subsamples

Here, we divide the sample along various dimensions and estimate the effect on the respective sub-samples. We perform the analysis separately for men and women, for young and old workers, for full-time and part-time workers, and for short- and longer-duration programs. Because we have no information regarding the actual content of the programs that

Table 5: ATT estimates in percentage points for job mobility[1]

Duration	**Same Firm Sample**			**All Workers Sample**		
(days)	ATT	SE	N(Tr)	ATT	SE	N(Tr)
90	0.91***	0.2323	6,627	0.58***	0.1393	7,139
180	1.58***	0.3171	6,426	1.35***	0.2303	6,900
270	1.84***	0.4168	6,212	1.53***	0.2348	6,650
360	1.87***	0.5080	5,964	1.28***	0.2766	6,380
450	1.36***	0.4647	5,188	0.85***	0.3235	5,527
540	1.54***	0.3862	3,830	1.08***	0.3657	4,079
630	1.34***	0.4647	3,057	0.92**	0.4147	3,256
720	1.58***	0.4851	1,901	1.39***	0.4548	2,031

Notes: [1]Survival in the same firm where one worked at the beginning of WeGebAU training.
Standard errors obtained by bootstrapping (Same Firm Sample: 500 repetitions, All Workers Sample: 50 repetitions).
Significance level: *** 1%, ** 5%, * 10%.

the participants attended, the reported program duration serves as a proxy for the program type.

The results for paid employment are presented in Figure 3. All of the subsamples analyzed exhibit a substantial reduction in the tendency to leave paid employment. The largest and increasing effects are experienced by part-time workers and by participants of longer programs; by the end of the second year, the positive effects are larger for those over 50 than for those between 45 and 50 years of age. The effect on transitions to benefits is relatively smaller (Figure A.2). However, part-time workers show the greatest effect here as well. The estimates for the other groups are small and typically insignificant (except for men), and we conclude that the program prolongs the employment periods primarily by discouraging workers from exiting the labor force. For the older group, non–paid employment is likely a form of retirement, as self-employment at that age seems unlikely.

Table 6 presents the disaggregated estimates for the other outcome variables (for paid employment periods only). These estimates support the previous interpretation. The program appears to advance employment more for workers over 50 years old and for part-time workers. The employment duration for these workers increases by 1.5 to 4 percent, which is almost double or even triple the increase for full-time workers. As expected, we find stronger employment effects for the long-duration programs (more than 60 days). With respect to the latter, we also capture a positive effect for median monthly earnings (approximately 72€ monthly). The positive effect on earnings suggests that improved employment duration is not simply caused by lock-in effects; rather, participants actually profit from more productive jobs, at least after participating in a longer-duration program.

Figure 3: ATT estimates in percentage points for different subgroups for non-paid employment periods. The shaded area corresponds to the 95 percent confidence region.

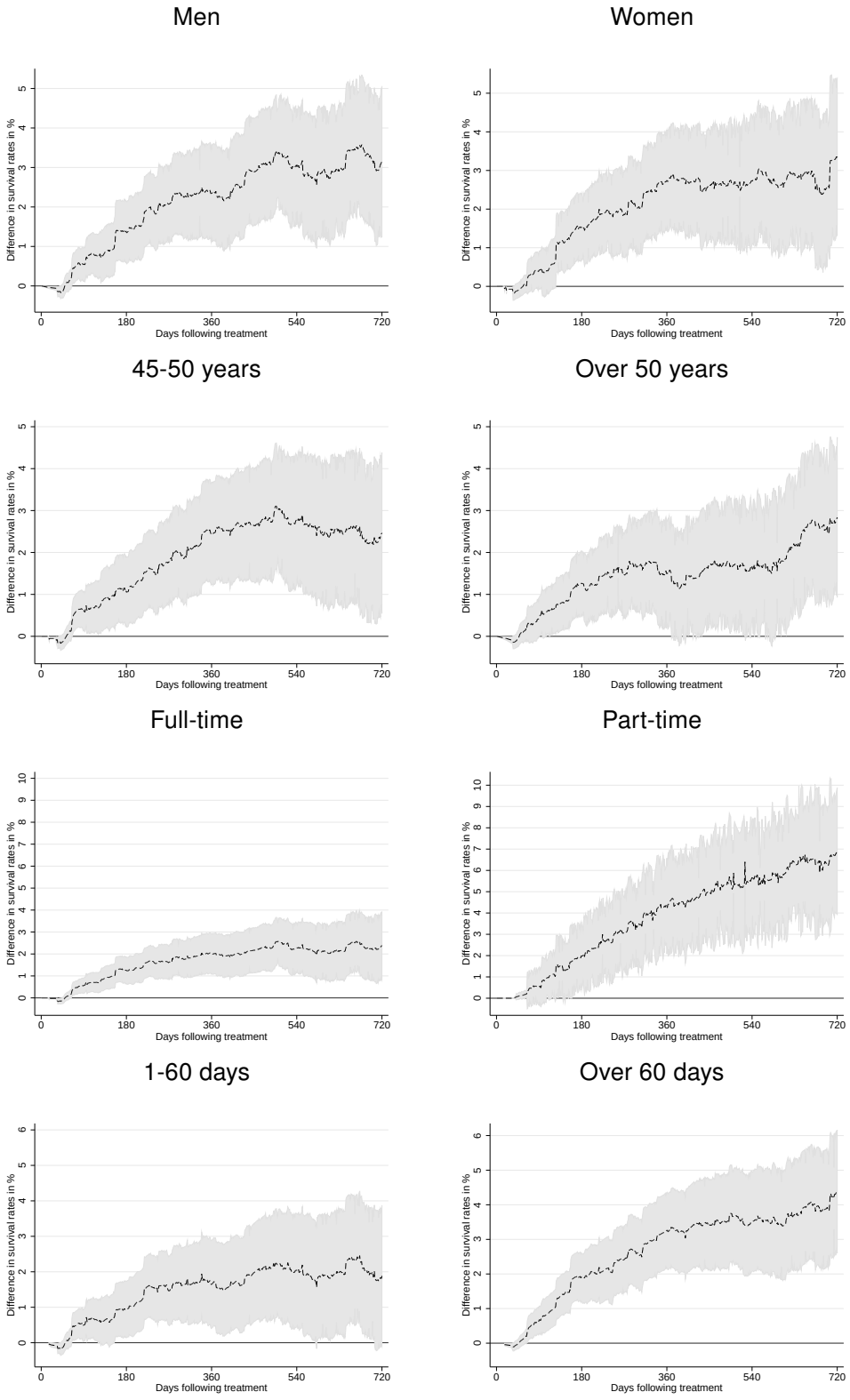


Table 6: ATT estimates in absolute numbers for different subgroups (Same Firm Sample)

Variable	ATT	SE	N (Tr)
Days in employment: 1-360 days after treatment start			
Women	4.2635***	1.1842	2,568
Men	4.0545***	0.9733	3,997
Days in employment: 360-540 days after treatment start[1]			
Women	3.6411***	1.3858	1,522
Men	3.0506***	1.1850	2,438
Median of monthly earnings one year after treatment[2]			
Women	22.91	38.54	2,568
Men	9.04	25.55	3,997
Days in employment: 1-360 days after treatment start			
45-50 years	2.7051***	1.0082	2,952
>50 years	3.9464***	1.0894	3,575
Days in employment: 360-540 days after treatment start[1]			
45-50 years	1.0530	1.3225	1,869
>50 years	4.0629***	1.2777	2,060
Median of monthly earnings one year after treatment[2]			
45-50 years	54.54*	31.18	2,952
>50 years	35.40	29.27	3,575
Days in employment: 1-360 days after treatment start			
Full-time	3.3743***	0.7725	5,694
Part-time	5.9875***	2.1501	853
Days in employment: 360-540 days after treatment start[1]			
Full-time	2.4044***	0.9817	3,407
Part-time	7.1519***	2.4102	495
Median of monthly earnings one year after treatment[2]			
Full-time	30.89	20.94	5,694
Part-time	28.01	46.76	853
Days in employment: 1-360 days after treatment start			
Short treatment	3.3500***	0.9931	3,341
Long treatment	4.8344***	0.9819	3,443
Days in employment: 360-540 days after treatment start[1]			
Short treatment	1.7151	1.2232	1,993
Long treatment	4.8651***	1.0771	2,098
Median of monthly earnings one year after treatment[2]			
Short treatment	24.78	28.06	3,341
Long treatment	71.94***	27.49	3,443

Notes: [1]We include only individuals whose data we actually observe in that period. Those treated after June 2008 are excluded.
[2]Periods of non-employment are treated as periods with zero earnings.
Standard errors obtained by bootstrapping in parentheses (500 repetitions).
Significance level: *** 1%, ** 5%, * 10%.

6 Summary and Concluding Remarks

Public sector training subsidies for employed workers constitute a widely used labor market policy in Europe (Bassanini et al., 2005). However, little is known about the efficacy of such programs. In this paper, we evaluate one of these measures, the German WeGebAU subsidized training programs for older employees (at least 45 years old) working in small and medium-sized firms (fewer than 250 employees), introduced in 2007. Because the implementation does not offer any suitable instrument, we rely on a dynamic version of propensity score matching (Crépon et al., 2009). The main idea of the method is to construct a control group of employees who have not (yet) entered treatment. We employ German registry data compiled by the German Federal Employment Agency that cover all workers who were employed on January 1, 2007, and participated in the years 2007 and 2008.

The results indicate that WeGebAU participation leads to improved job stability and survival in employment. More detailed analysis shows that this effect arises primarily from a declining likelihood of leaving paid employment, presumably into a form of retirement. The effect on insured unemployment is rather small. Part-time workers clearly exhibit strong gains from the program. This finding is encouraging, particularly given that part-time work is increasingly common at older ages and is often considered a form of early retirement. Although the sample size is small, this group appears to exhibit gains in all relevant dimensions, although the estimated effect on earnings is not significant, which suggests that subsidized training may be a means of increasing the labor market attachment to part-time workers and to older workers in general. In this matter, our results are not consistent with those of Boockmann et al. (2012), who show that part-time work as a specific measure for older employees does not prolong employment duration.

Not surprisingly, we find that long-duration training has a larger effect than short-duration training for both employment and earnings. The improvement in earnings suggests that the effect on employment is not simply an artifact of the lock-in effect. Increased training over a long period with the objective of acquiring in-depth knowledge is more effective in improving employability than shorter periods of training.[10]

Because we do not have detailed information regarding costs, we can perform only a rudimentary cost-benefit analysis. According to the Federal Employment Agency, the average cost per participant was approximately €1350 in 2007 and €2090 in 2008. We can also provide a simple figure for the benefits of the program. Our estimates suggest that the likelihood of paid employment increases by approximately 2.5 percent. Over a three-year period, this yields to approximately $2.5/100 \times 253 * 3 \approx 19$ more days of employment.[11] Given the assumption that the typical daily wage is close to the sample mean, €80, this approach would result in approximately €1500 of additional salary earned. Potential gains by employers and public gains through reduced benefits must also be added. Assuming

[10] In 2012, the Federal Employment Agency decided to subsidize only those training courses with a minimal duration of four weeks. Our results offer a partial ex-post justification for that decision, although a cost-benefit analysis should still be performed.

[11] 253 is the approximate yearly number of workdays in Germany.

that those are of similar magnitude as the private returns, we may conclude that our estimates point toward average gains in the order of €3000 per person over the three years following the beginning of employee training, given the lost domestic production is not big. These figures suggest that total social gains from the WeGebAU program may be positive. However, to pay for the average out-of-pocket costs for the government (€2090) in three years, the additional tax revenue from the 19 days of additional employment should be in the order of €2090/19 $\approx$ €110 per day. This figure is unrealistically large and suggests that in budgetary terms, the program's costs typically exceed its benefits. Even in the case of improving income, the program is not likely to be beneficial for the public budget. If the costs of €2090 are divided evenly over 36 months, then the additional tax income must be approximately €2090/36 $\approx$ €60 monthly. This figure appears to be unlikely, given that our typical estimates for monthly income gains were between €20 and €50.

When interpreting these results, we must consider that the positive effects that we find become much smaller or even vanish when using controls from firms that did not utilize the WeGebAU programs. This possibility might imply that the additional participants from firms with few older employees that we include in the All Workers Sample perform worse. Another explanation is that controls from participating firms perform worse than those from non-participating firms, which implies that firm selectivity is important and that WeGebAU is used as a substitute for lay-offs primarily by firms encountering difficulties.

It is instructive to compare our results with those of similar programs. Our estimated effects are dwarfed by those of the Workforce Investment Act, in which training increases the monthly salary by approximately €100 and employment probability by approximately 8 percentage points (Heinrich et al., 2009). However, our results offer a view that is more optimistic than that presented in a number of European studies. For example, Leuven and Oosterbeek (2004) do not find any effect on wages, and Abramovsky et al. (2011) offers no evidence that subsidized training for low-skilled workers increases their training participation rates. Additionally, Kristensen (2012), who analyzes the effect of cumulative life-cycle training on retirement decisions, concludes that training is not an adequate measure for prolonging employment careers.

More work is required to refine the estimates that are provided in this paper. In particular, a more detailed cost-benefit analysis would be valuable when more detailed expenditure data become available.

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Appendix

A.1 Placebo Analysis

As a further check, we estimate the "treatment effect" on a number of pre-treatment variables (Table A.1). We choose cumulative employment *before* the individuals actually entered WeGebAU. The measures are indistinguishable from zero at conventional levels of statistical significance.

Table A.1: ATT estimates of pre-treatment cumulated days in employment prior to 01/01/2007

	ATT	SE
Cumulated days one year before	0.0588	(0.2182)
Cumulated days two years before	0.0090	(0.1120)
Cumulated days three years before	-0.0252	(0.0676)

Notes: Standard errors obtained by bootstrapping in parentheses (500 repetitions).
Significance level: *** 1%, ** 5%, * 10%.

A.2 Number of Observations by Month

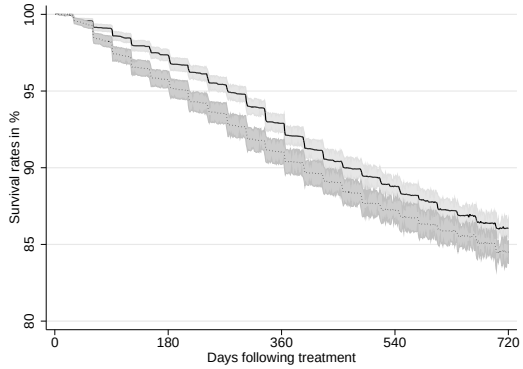
Table A.2: Number of treated (T) and controls (C) per month of treatment start

	Same Firm Sample				All Workers Sample			
Month of	Before Matching		After Matching		Before Matching		After Matching	
Treatment	T	C	T	C	T	C	T	C
Jul 2007	133	1,231	121	1,199	154	267,185	154	245,738
Aug 2007	131	1,142	110	851	141	264,501	139	230,242
Sep 2007	547	6,114	546	5,860	578	256,592	564	229,001
Oct 2007	491	5,547	487	5,323	552	250,098	543	223,914
Nov 2007	483	4,963	478	4,780	527	243,792	520	219,027
Dec 2007	430	3,468	427	3,244	463	238,233	459	214,805
Jan 2008	395	4,512	386	4,348	424	232,790	419	212,729
Feb 2008	453	4,965	444	4,762	507	226,484	502	204,435
Mar 2008	557	5,248	552	5,089	599	220,025	587	200,455
Apr 2008	487	4,420	479	4,083	521	214,592	515	194,576
May 2008	326	3,682	317	3,472	349	210,521	332	187,552
Jun 2008	367	4,091	358	3,992	387	205,689	384	187,624
Jul 2008	232	2,517	224	2,315	259	201,909	252	183,554
Aug 2008	303	2,921	298	2,841	321	197,714	311	173,677
Sep 2008	695	5,789	684	5,632	783	190,958	767	172,755
Oct 2008	570	5,761	563	5,573	611	184,656	605	164,762
Nov 2008	582	5,041	574	4,859	642	178,618	631	160,388
Dec 2008	178	1,448	163	1,382	196	172,050	192	153,981
N	7,360	72,860	7,211	69,605	8,014	3,965,407	7,876	3,559,215

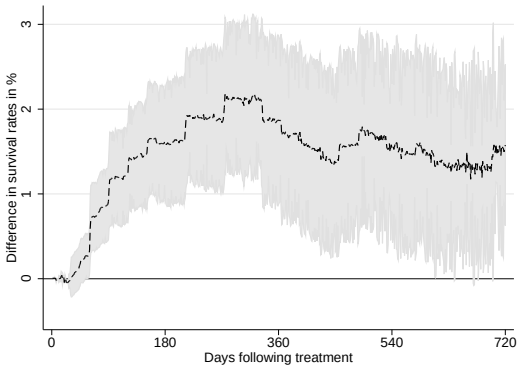
A.3 Alternative Transitions for the Aggregate Sample

A.3.1 Estimates for Firm-to-Firm Mobility

Figure A.1: Survival rates in percent (left panel) and ATT estimates in percentage points(right panel) for firm-to-firm mobility



(a) Same Firm Sample



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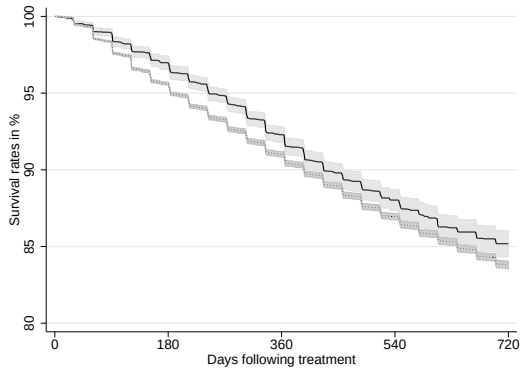
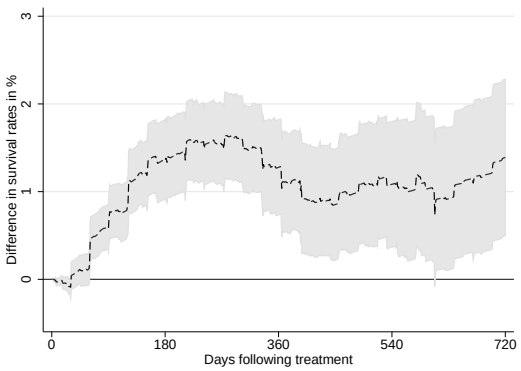


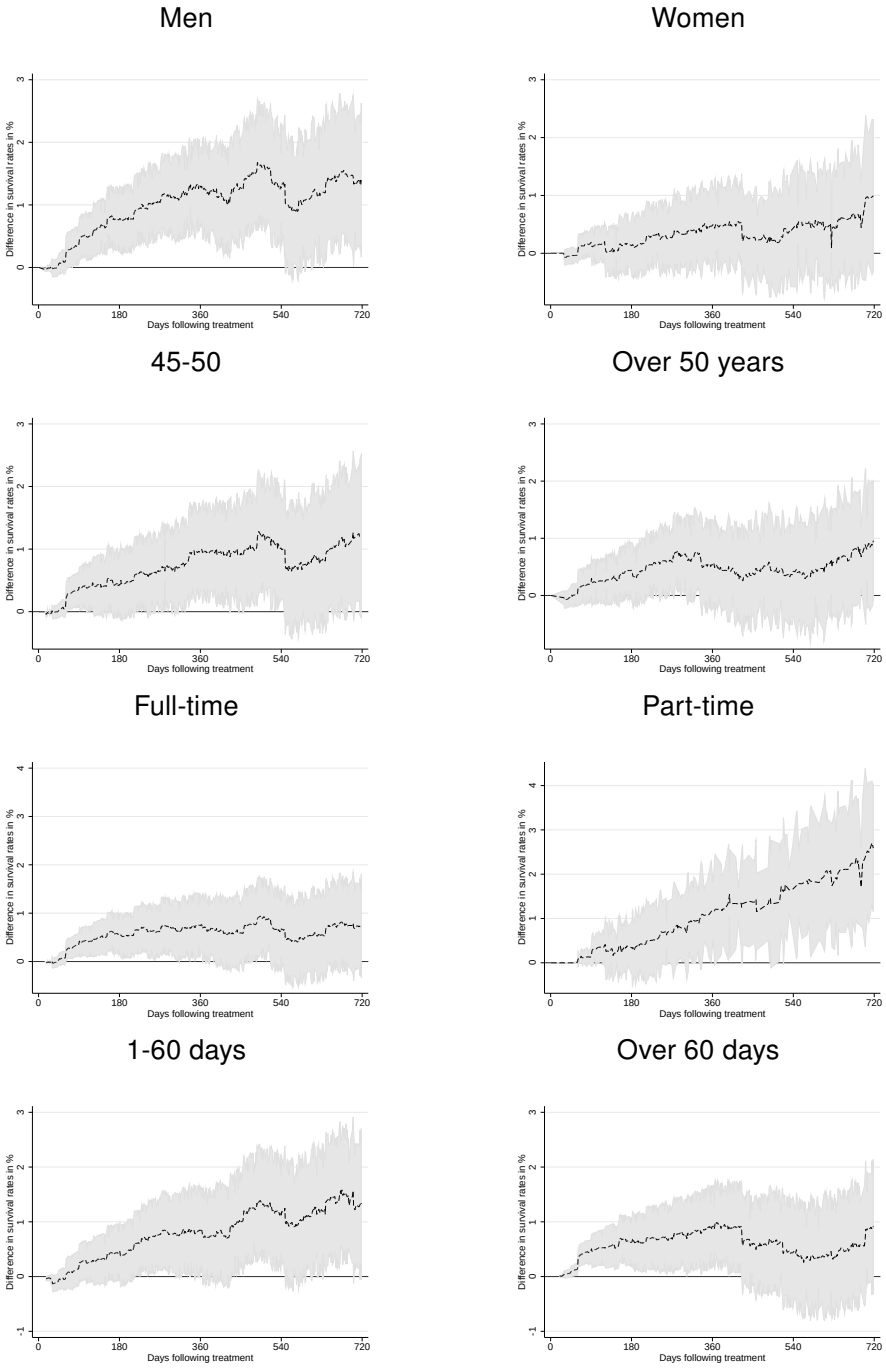
(c) All Workers Sample



(d) All Workers Sample

A.3.2 Different Subgroups: Non-Benefit Periods

Figure A.2: ATT estimates in percentage points for different subgroups for survival in non-benefit periods. The shaded area corresponds to the 95 percent confidence region.



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