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# Back to work: The Long-term Effects of Vocational Training for Female Job Returners\*

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## Abstract

This paper studies how training vouchers increase the employment prospects of women with interrupted employment histories. Using the population of female job returners who receive a training voucher to participate in training courses and a randomly selected control group from German administrative data, we analyze the effectiveness of training on the employment probability, monthly earnings and job quality. The results suggest that the receipt of a training voucher translates into a higher employment probability and higher monthly earnings. We find a positive impact on the job quality, e.g. the probability to be full-time employed increases significantly. In contrast, the probability to be marginally employed decreases by 5 percentage points in the long-run. We count this as an indicator for employment stability. The investigation of effect heterogeneity reveals some interesting insights regarding the vocational degrees, and the different types of training courses. The effectiveness of vocational training increases with the provided human capital in the courses. Several robustness checks support a causal interpretation of the results and highlight the importance of vocational training for the very special sub-group of female job-returners.

JEL-Classification: J68, H43, C21

Keywords: Active Labor Market Policies, Treatment Effects Evaluation, Administrative Data, Voucher, Return to work, Job Return

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# 1 Introduction

Family-related employment interruptions negatively affect the working careers of women. Theory suggests that employment interruptions reduce the accumulation of human capital and can even lead to a reduction in human capital due to depreciation. Empirical investigations confirm this result (see for example, Drange and Rege, 2013, Beblo, Bender, and Wolf, 2009). There is controversial discussion regarding how to implement parental leave legislation and job protection schemes to make employment interruptions less attractive. However, little is known about how to re-integrate women once they decided to exit the labor market for shorter or longer times. In this paper, I investigate the impact of publicly funded vocational training for female job returners on their subsequent working careers. In particular, I analyze whether vocational training has positive effects on employment probability, monthly earnings, and job quality over the long run.

The labor force participation of women has increased over the last few decades due to female empowerment and family policies that consider gender equality issues. With females educational attainment, vocational qualifications and career perspectives became increasingly important. Nevertheless, mothers do have to interrupt their employment after giving birth for some amount of time. However, there are also other reasons for employment interruptions, e.g., caring for one's parents or relocation caused by a partner's job change. Regardless of the reasoning behind employment interruption, it does not stand in contrast with the desire to be occupationally engaged and financially independent. A modern society requires a gender role concept in which members of both genders have opportunities to care for family and be able to work.

In addition to the social justifications for female labor force participation, the re-integration of job returners is of high economic importance. First, due to recent skill shortages in the labor market by an aging economy and technological changes, there will be an increasing demand for skilled workers over the coming decades. Second, women face a remarkably higher risk of poverty at retirement if their working life is characterized by long interruptions. Third, re-integrated job returners contribute to social systems, e.g., by paying taxes. Fourth, empirical evidence shows that high female labor force participation will lead to macroeconomic gains, e.g., by increasing GDP (e.g., Elborgh-Woytek et al. 2013).

The importance of a successful re-integration of job returners is also recognized by German legislation that defines job returners as particularly eligible individuals for services. For example, if job returners need assistance or counseling, they can register at local employment agencies. The status of being a job returner requires that the woman has interrupted her employment, unemployment or apprenticeship for at least one year to care for her child (younger than 15 years) or other family members. Job returners can

potentially use all counseling services at local employment agencies. These includes the possibility of receiving a so-called training voucher to participate in vocational training courses, which are part of active labor market policy (ALMP) in Germany.

Vocational training programs appear to be particularly suitable to improve the likelihood of employment for job returners. These programs are designed to improve occupation-specific knowledge and job-related skills to participants. Long programs can even lead to a (new) vocational degree. Thus, participation in a vocational training course can be considered as an investment in human capital. Additionally, training courses can prepare women to manage their working life with respect to time management and childcare facilities. For job returners with long interruption durations, participation in training courses may provide the first contact with individuals working in a similar occupation since a long time. It is an opportunity to meet women in similar situations and to build networks that are found to positively affect labor market opportunities (e.g., Calvo-Armengol and Jackson, 2004).

This is the first study that focuses on training effects for female job returners. There is a large body of literature about the effectiveness of vocational training for unemployed individuals (e.g., see Doerr et al. 2013, and Rinne et al., 2013 for evaluations after the labor market reform that took place in the year 2003, and Biewen et al. 2014, and Lechner, Miquel, and Wunsch, 2011, 2007, for evaluations prior to the reform). Nevertheless, all these studies exclude, by definition, the population of job returners because they restrict their sample to individuals transitioning from regular employment to unemployment. In recent years, the Federal Employment Agency initiated programs specially targeting female job returners. Those programs include information events, short training measures and intensive counseling (Diener et al. 2013), but they are not comparable to vocational training programs with regard to content or intensity.

Given the potential for positive effects discussed above, it is of high policy relevance to study the effectiveness of vocational training for the special sub-group of female job returners. The identification of causal effects requires that the treatment of interest (here, voucher award) is randomly assigned. In practice, it is unlikely that training vouchers are randomly assigned to job returners. To identify the causal effects of being awarded a training voucher, I apply a matching strategy which accounts for selection based on observable characteristics. I use detailed German administrative data with daily records that allow me to control for a large set of characteristics, including the employment and welfare histories of the women up to the 25 years before their labor market exit. Conditional on these determinants, I argue that job returners are randomly awarded a training voucher. I estimate the long-run effects of receiving a voucher to participate in vocational training on various labor market outcomes using radius matching with bias adjustment following Lechner, Miquel, and Wunsch (2011).

The results suggest that awarding a training voucher translates into a higher employment probability (10 percentage points) and higher monthly earnings (140 Euros/160 US-Dollars). Additionally, job quality increases. Job returners who receive training vouchers have a higher probability of receiving at least 90% of their previous earnings and of being employed full time. Moreover, the probability of experiencing unstable marginal employment decreases permanently. The investigation of effect heterogeneity reveals some interesting insights regarding vocational degrees and the effectiveness of different course types. The results strongly imply that the effects of training vouchers for job returners work through the human capital channel. I perform many robustness checks that support the identifying assumptions and the empirical approach.

The remainder of this study is structured as follows. Important background information and a description of the institutional setting follow in Section 2. The data and descriptive statistics are discussed in Section 3. I present the empirical strategy to identify the parameters of interest in Section 4. Finally, I present and discuss the empirical results with regard to the different outcomes and effect heterogeneity. The final section concludes.

## 2 Background

### 2.1 Definition of a job return

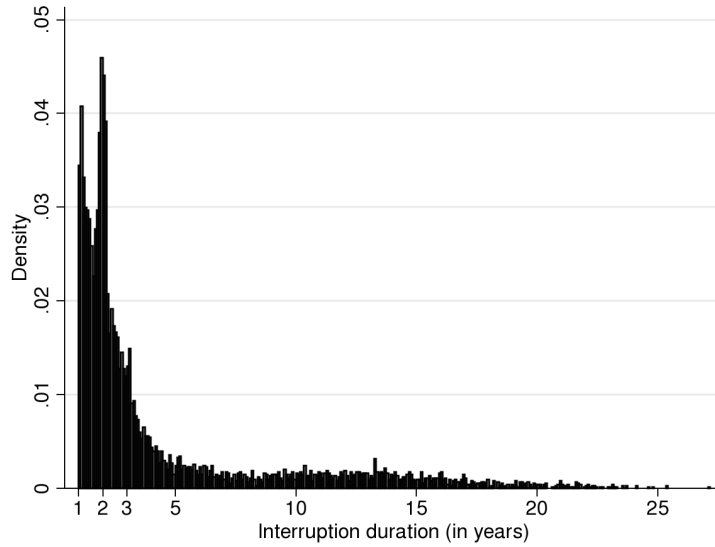
Formally, a job returner is defined as a woman (or man) who has interrupted her employment, unemployment or apprenticeship to care for her children (younger than 15 years) or other family members.<sup>1</sup> To obtain the status of a job returner, the interruption period must have lasted at least 12 months, and the job returner must show an intention to become employed within one year after registration. The distribution of the interruption duration is right skewed, indicating that the median interruption duration is below the average (see Figure 1). A large fraction of women (64%) returns to work within three years.

Once a woman decides to re-enter the labor market, she can return to her former employer (if the family-related employment interruption had a maximum duration of three years), she can search for a job herself or she can use the counseling and placement services of local employment agencies. In this study, I focus exclusively on those women who register as job returners at local employment agencies. The services available at local employment agencies depend critically on the registration status of the job returner. Different registration states correspond to different rights and obligations. In general,

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<sup>1</sup>Since the majority of job returners are women (98.2%), I restrict the analysis on the sub-group of female job returners.

Figure 1: Distribution of interruption duration



Note: Each bar represents one month. The abscissa is labeled in years. The minimum interruption duration according to the formal definition for job returners is 12 months. The maximum duration observed in the data is 326 months (27 years).

there is a distinction among three different states: seeking advice, searching for employment and being unemployed. When registered in the first state, job returners can use only information and counseling services. If they register as searching for employment, they can use information, counseling and placement services. Once they register as unemployed, they can take part in ALMP programs, may receive benefits and they are also registered in the pension system during unemployment periods. In contrast to regular unemployed individuals, job returners are typically not eligible to receive unemployment benefits. Nevertheless, they have the same responsibilities as other workers who are registered as unemployed. They must search actively for employment for at least 15 hours a week, attend regular meetings with a caseworker and be available if caseworkers present a suitable job offer. A recent study by Ebach and Franzke (2014) investigates the motivations behind the different registration states. They report that the registration in the pension system as well as the opportunity to participate in ALMP programs are driving forces of unemployment registration even without receiving benefits. Nearly two thirds of the job returners investigated in this paper registered as being unemployed at their return. The others registered as searching for employment. Because registration status determines the services that can be accessed and is therefore crucial to the identification strategy, I will discuss this point in further detail in Section 4.1.

Table 1: Vocational training programs for job returners

Program type	Average duration	Share	Description	Examples
Practice firm training	169 days	12%	Courses that took place in practice firms to simulate a work environment.	Training for commercial software, office clerks, data processing
Short training	129 days	36%	Provision of occupation specific skills (duration $\leq$ 6 months).	Training courses as medical assistants, office clerks, draftsman, hairdresser, lawyer
Long training	306 days	25%	Provision of occupation specific skills (duration $>$ 6 months).	Training as tax accountant, elderly care nurse, office clerks, physical therapist
Retraining	823 days	26%	Courses to obtain a first/new vocational degree.	Apprenticeship as elderly care nurse, physical therapist, hotel and catering assistant
Others	-	1%	e.g. courses for career improvement	

Note: I use the categorization of programs proposed by Lechner, Miquel, and Wunsch (2011). Additionally, the information on the training voucher with regard to the contents of the training courses is analyzed. The presented examples refer to training goals that are often denoted on the training voucher. The category "others" contains different types of training programs with very few participants.

## 2.2 Vocational training for job returners

Vocational training is intended to increase the human capital of participants by providing, maintaining and updating occupation-specific skills. Therefore, it appears to be a particularly suitable instrument to increase the re-employability of job returners. The provision of vocational training by local employment offices is organized through a voucher system in Germany.<sup>2</sup> Once the job returning woman and caseworker mutually decide that vocational training is the appropriate program to improve her employability, the woman is awarded a voucher that makes her eligible to participate in a training course. The caseworker notes the educational objective, maximum program duration and validity on the voucher.<sup>3</sup> The job returner may then choose a course offered by a certified training provider subject to the restrictions noted on the voucher.

There are different types of training programs with respect to content and duration. According to Lechner, Miquel, and Wunsch (2011), I distinguish among practice firm training, classical vocational training and retraining. Practice firm training programs are rather short and simulate a work environment in a practice firm. The educational aims and contents of these courses are to update the skills of job returners mainly in commercial software programs and MS Office applications. Classical training courses provide

<sup>2</sup>The voucher system was introduced in January 2003. Before, the provision of vocational training worked through a direct assignment system by caseworkers. For a detailed discussion of the reform of the provision of vocational training and the detailed institutional changes see Doerr et al. 2013.

<sup>3</sup>Beside this, there is information about the funding, the commuting zone and various other information denoted on the voucher. For a detailed description of the voucher system see Doerr et al. 2013 or Rinne et al. 2013.

occupation-specific skills and mainly take place in classrooms. I differentiate between short (maximum duration of 6 months) and long (duration of over 6 months) training programs. Typical examples of classical vocational training programs for job returners include courses for office clerks, draftsmen, and medical assistants to adjust knowledge and skills according to recent developments in the respective occupation. Retraining courses have long durations of up to three years (only the first two years are funded by local employment agencies). They lead to a (new) vocational degree within the German apprenticeship system and cover the full curriculum of a vocational training for such occupations as an elderly care nurse or physical therapist. These courses may be particularly relevant for job returners with former occupations in which they can no longer work given their current living and family circumstances (e.g., jobs with shifting working hours or dangerous working conditions).

## 3 Data description

### 3.1 The data

This analysis is based on administrative data from the German Federal Employment Agency, which contains information on *all* female job returners in Germany who received a training voucher between 2003 and 2005. I observe the exact return dates of the women, the award and redemption dates for the vouchers and the precise start and end dates of participation in training courses. The individual data records are collected from the Integrated Employment Biographies (IEB) sample.<sup>4</sup> The data contain detailed information on individuals' entire employment histories subject to social security contributions, receipt of transfer payments during unemployment periods, job search information, participation in various active labor market programs and rich individual information. The sample of control persons originates from the same database. It is constructed as a three percent random sample of individuals who did not receive a training voucher between 2003 and 2005 and registered at the local employment agency between 1999 and 2005. I restrict this sample to women who are registered as job returners.<sup>5</sup> Using these data, I am able to consider a large set of personal characteristics and labor market histories for all the

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<sup>4</sup>The IEB is a rich administrative data base and source of the sub-samples of data used in all recent studies that evaluate programs of German ALMP (e.g., Biewen et al., 2014, Lechner, Miquel, and Wunsch, 2011, Lechner and Wunsch, 2013, among others). The IEB is a merged data file containing individual data records collected in four different administrative processes: The IAB Employment History (*Beschäftigten-Historik*), the IAB Benefit Recipient History (*Leistungsempfänger-Historik*), the Data on Job Search originating from the Applicants Pool Database (*Bewerberangebot*), and the Participants-in-Measures Data (*Maßnahme-Teilnehmer-Gesamtdatenbank*). IAB (*Institut für Arbeitsmarkt- und Berufsforschung*) is the abbreviation for the research department of the German Federal Employment Agency.

<sup>5</sup>I account for the fact of different sampling probabilities in all calculations whenever necessary.



women in the evaluation sample.

An enormous advantage of these data is the availability of voucher information. In addition to the exact start date of the training course, I observe the date when the assignment into the program took place. These data allow for the estimation of intention to treat effects, i.e., estimating the effect of being awarded a voucher (not the effect of participating in a training course). The voucher data contain information about the educational contents and planned duration of the training courses, which provides evidence of the intensity of human capital provided in each course type. Another advantage is the availability of information about other labor market programs, which makes the definition of treatment and control groups straightforward. The sample includes only those women who decide to register as job returners in one of the three states at local employment offices. It does not include women who return to their former employers or who search for employment by themselves. Additionally, women who are self-employed or civil servants are not included in the data. I argue that missing these subgroups is only a minor problem because I am interested in estimating the effects of receiving a training voucher. Because eligibility for a voucher depends critically on registration at local employment agencies, those who do not register are not relevant for the effect estimates of interest in this study.

### **3.2 Treatment and outcome definitions**

The treatment of interest is the award of a training voucher in 2003-2005 within the first year of return. Using this treatment definition, I estimate the intention effect of assigning vocational training to a job returner. Regarding the outcome measures, I follow all individuals over a period of 76 months to allow for the estimation of long-term effects. I use various outcomes to measure the effectiveness of awarding training vouchers to job returners. The two standard outcomes measures in the literature are non-subsidized, non-marginal employment (henceforth called employment) and monthly earnings. When considering job returners, it is particularly interesting to know whether vocational training alters job quality and employment stability. Therefore, I consider the probabilities of being marginally employed, being full-time employed and earning at least 90% of the previous earnings as additional outcome measures. The award of a training voucher represents an intention to invest in human capital that involves direct and indirect costs. Direct costs of the programs are not observed in the data. Nevertheless, I can measure the indirect costs in terms of earning and employment losses during the participation period. Following Lechner, Miquel, and Wunsch (2011), I use accumulated employment and earnings as additional outcome measures to assess the effectiveness of vocational training for job returners in terms of net benefits over the long run.

### 3.3 The evaluation samples

The sample definition is restricted to all registered job returners with interruptions of at least 12 months who register as such in one of the three states (seeking advice, searching for employment or unemployed) between January 2003 and December 2004. I do not restrict the evaluation sample with respect to employment status before the interruption. Nevertheless, I restrict the sample with regard to age (25-49 years at return). The definition of the treatment and control groups depends critically on the evaluation framework that is used. The decision to implement a dynamic or static evaluation approach is mainly driven by data limitations (e.g., small sample sizes), and there are arguments in favor of and against each approach.

The static evaluation approach is simple and straightforward. An implementation of this approach follows a comparison between those who received a training voucher and those who did not over a certain period (here, 12 months). Using this approach, I would ignore the time dimension of the treatment. The standard argument against a static approach in the evaluation of ALMP for the unemployed comes from the observation that in countries such as Germany, nearly all unemployed persons would receive a treatment if their unemployment spells were sufficiently long (Frederiksson and Johansson, 2008). Accordingly, individuals who find jobs rapidly are less likely to receive training, as the treatment definition is restricted to unemployment periods. Frederiksson and Johansson (2008) argue, that ignoring the timing dimension of the treatment may lead to an underestimation of the results because such a constructed control group includes a higher share of individuals with better labor market characteristics than those in the treatment group. However, this argument is not necessarily transferable to the case of job returners. They register at local employment agencies to receive counseling and advice for returning to work (without receiving benefits). Thus, those who do not receive a voucher may be those who are still unsure of their return decision. These women do not necessarily represent a positive selection. In this case, a bias possibly induced by a static evaluation approach can occur in both directions.

Dynamic approaches allow for considerable flexibility but demand considerable huge data. Nevertheless, they capture the timing dimension of the treatment, which may be important. Applying a dynamic approach as proposed by Sianesi (2004) and Frederiksson and Johansson (2008), estimates the effects conditional on the time elapsed since return. That is, I would estimate the effect of receiving a training voucher in the current month versus not receiving a training voucher in the current month but possibly receiving it later.<sup>6</sup> Because job returners are a relatively small group (compared to the number of

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<sup>6</sup>One major concern of dynamic evaluation approaches is raised by Lechner, Miquel, and Wunsch (2011). They argue that the composition of control persons would change each month elapsed since return, a fact that hinders the interpretation of the results.

Table 2: Number of observations in the different evaluation samples

Static evaluation approach												
Time window	3 months			6 months			12 months			12 months + (pseudo) elapsed duration		
Num. treated	1468			1932			2951			<b>2710</b>		
Num. controls	5035			5035			5035			<b>4669</b>		
Num. altern. controls	4068			4068			4068			3761		
Dynamic evaluation approach												
Elapsed duration	1	2	3	4	5	6	7	8	9	10	11	12
Num. treated	530	423	343	288	262	238	187	168	163	131	118	100
Num. controls	7432	6923	6502	6145	5805	5501	5242	5015	4782	4586	4402	4241

Note: The main sample (bold numbers) is constructed using a static evaluation approach over a period of 12 months. I condition on simulated start dated for non-treated job returners (pseudo elapsed duration). The employment and earnings effects using all different evaluation approaches are presented in Figure 11- 12 in Appendix A.

regular unemployed individuals), I am faced with restrictions regarding the number of observations. I start with a static evaluation approach and compare those job returners who received a training voucher within the first 12 months of return to those who did not receive a training voucher within this period. To reduce the possibility of a bias introduced by a static approach, I randomly assign pseudo treatment start dates to each individual in the control group to partly capture the timing dimension. Thereby, I recover the conditional distribution of the time elapsed since return at the treatment start from the treatment group (similar to, e.g., Lechner and Smith, 2007). To make the treatment definitions comparable between the treatment and control samples, I only consider individuals who remain unemployed at the start of their (pseudo) treatment. I use this constructed duration to control for the fact that immediately treated job returners are not necessarily comparable to job returners who are treated later. To further check the robustness of the results, I narrow the time window in which the treatment occurs to six and three months. As a further robustness check, I implement a dynamic approach. The results from all approaches will be presented in Section 5.1. I find no noteworthy differences in the estimated effects between the static evaluation approach using different time windows or the (pseudo) elapsed duration as a control variable. Nevertheless, I find evidence for attenuation effects using the dynamic evaluation approach.

The numbers of observations for the different evaluation samples are presented in Table 2. A total of 2,710 job-returners who received a training voucher (treatment group) and 4,669 job returners who received another or no ALMP measure (control group) are included in the main sample. An alternative control group consists of those job returners who did not participate in any labor market program within this period. The share of control persons participating in other ALMP programs is low at 20%. In light of

the smaller sample sizes, I decided to include those individuals. The majority of these participate in short training measures that are assumed to have only a minor influence on the human capital accumulation of job returners. I present effect estimates using this alternative control group in Figure 11 in Appendix A. There are no differences in the estimated effects, which supports my evaluation strategy.

## 4 Empirical strategy

### 4.1 Identification and estimation

Randomized trials are the gold standard for determining causality. Because randomization rarely occurs when public policies are implemented, I am confronted with selection problems. One strategy to address selection problems is to rely on a selection-on-observables identification strategy, which is motivated by the richness of the administrative data.<sup>7</sup> The treatment of interest is a voucher awarded during the first twelve months after registration as job returner between January 2003 and December 2005. Each woman is observed for at least 76 months. The voucher award as an intention to treat is denoted as  $D_i \in \{0, 1\}$  (for individuals  $i = 1, \dots, N$ ). The outcome variable is denoted as  $Y_{it}$  (where  $t = 1, \dots, 76$ ) and indicates the number of months since the award of the voucher. To account for the dynamics of the assignment process, I match on the time elapsed between return and treatment.

Following the notation of Rubin (1974), the potential outcomes are indicated as  $Y_{it}^d$ , where  $d = 1$  under treatment and  $d = 0$  otherwise. For each job returner, only the realized outcome is observed. Thus, the potential outcome  $E[Y_{it}^1 | D_i = 1]$  is directly observed from the data.  $E[Y_{it}^0 | D_i = 1]$  is the counterfactual expected potential outcome, as  $Y_{it}^0$  is never observed for the treated sub-population. It is the expected non-treatment outcome for those who received a training voucher. Thus, I aim to identify the expected difference  $\gamma_t$  between the outcomes  $Y_{it}^1$  and  $Y_{it}^0$  for those women who received a training voucher for each month of the post-treatment period. This effect is known as average treatment effect of the treated (ATT) in the literature (e.g., Imbens 2004).

$$\gamma_t = E[Y_{it}^1 | D_i = 1] - E[Y_{it}^0 | D_i = 1]$$

To identify this parameter, I must control for a large set of pre-treatment variables  $X$  that jointly influence the treatment assignment and the outcome. The formalization of this condition is established as the conditional mean independence assumption (CIA).

<sup>7</sup>Another way to address selection into treatment would be to find a strong and convincing instrument that generates random assignment. A strong and convincing instrument is not available in this particular setting.

**Assumption 1** (*Conditional Mean Independence*). For all  $d \in \{0, 1\}$ ,

$$E[Y_{it}^d | D_i = 0, X_i = x] = E[Y_{it}^d | D_i = 1, X_i = x] \text{ for } \forall x \in \mathbb{X},$$

and  $t \in 1, \dots, 76$ , where  $\mathbb{X}$  denotes the support of  $X_i$ , and all necessary moments exist. The (pseudo) start dates  $M$  of the treatment are included in the vector of control variables ( $M \in X$ ). According to the CIA, conditional on the pre-treatment control variables  $X_i$ , individuals are randomly assigned to the treatment, and the expected potential outcomes are independent of the treatment status  $D_i$ . In the following, I will argue that the CIA is satisfied in the case of the assignment of training vouchers for female job returners.

First, I argue that the group of registered job returners is quite homogenous with respect to their motivation to return. Registration as job returner produces obligations (e.g., attend regular meetings with the caseworker, be available for job offers), but in contrast to regular unemployed workers, they are typically not eligible to receive benefits (75%).

Second, despite homogeneity with regard their motivation to return, returners might differ considerably in terms of their vocational background, pre-interruption attachment to the labor market and family situation. All these factors are also observable to the caseworker and play a role in the decision to assign a training voucher to a job returner. Rich administrative data allow me to account for these factors. I condition on a large set of personal and family background characteristics (e.g., age, health status, family status, number of children) as well as on information about the educational and vocational degrees of the women in all estimations. Because I observe their employment and welfare history up to 25 years before interruption, I carefully control for their long- and short-term employment histories (lagged employment, accumulated wages and benefits) before interruption. In the case of job returners, it may be particularly important to include information for the period shortly before the interruption. Therefore, I use information for the last occupation (sector, occupation, information about part-time employment), monthly wages for the last three years before interruption and employment status directly before interruption. The length of a woman's employment interruption might be driven by many factors that are not directly observable in the data, for example, her ability and toughness, her attitude toward employment and her situation within the family. I can account for these effects using the interruption duration as an additional control variable.

Third, the probability of receiving a training voucher depends on regional labor market conditions. Caseworkers must consider local labor demand in their assignment decisions. For this reason, I include considerable monthly regional information. I condition, among other variables, on regional dummies, industry dummies, unemployment rates, population density rates, and the share of vacant jobs in each region.

Fourth, eligibility for a training voucher receipt depends critically on registration status after return. As described in Section 2.1, only registration as unemployed makes job returners eligible to receive a training voucher. This may induce a bias if there is selection into different registration states and job returners only register as unemployed because they will receive a training voucher. Recently, Ebach and Franzke (2014) conducted a case study with caseworkers and job returners. They report that registered job-returners are often not informed about the differences between the registration states, and they observe many switches between them. I control for possible selection from the registration process by including information on registration status directly after return, number of switches within the first year of return and eligibility for unemployment benefits. I argue that benefit-eligible job returners are motivated to register as unemployed mainly by monetary reasons, not by the possibility of participating in training.

Fifth, recent studies assessed the plausibility of the CIA in the context of the evaluation of ALMPs in Germany (Biewen et al., 2014 and Lechner and Wunsch, 2013) and support its plausibility in cases of flexible conditioning on lagged employment, welfare histories, personal characteristics, and regional labor market information. They focus on the plausibility of the CIA with respect to participation in a certain program. In this study, the aim is to identify effects of intention to participate in training (being awarded a voucher) instead of the participation decision itself. I argue that the selection process to identify the intention to treat effect is less demanding because actually starting the program is not part of the selection (e.g., the CIA must not hold for the selection process of voucher redemption).

Sixth, to further check the robustness of my results and support a causal interpretation, I perform an impact estimation on pre-treatment (here, pre-interruption) outcomes as a specification test. If there are unmeasured constant differences between treated and control individuals, I would observe these differences in effect estimates before interruption. Another robustness check is performed by estimating treatment effects for job returners who do not redeem training vouchers. If there exist other effects aside from the effect I want to measure (e.g., threat effects, anticipation effects), they should be discernable in the group of job returners with unredeemed training vouchers. The results of all robustness checks are presented in Section 5.2.

I use radius matching on the propensity score with linear bias adjustment to estimate average treatment effects for job returners who receive training vouchers. This estimator aims to compare outcomes of observations (here, job returners) that are similar with respect to their probability of being treated conditional on all observed characteristics but differ only in terms of receiving a treatment (here, voucher award). Radius matching is found to perform well in a recent empirical Monte Carlo simulation study conducted by Huber, Lechner, and Wunsch (2013). Its good performance relative to ordinary least

square (OLS) regressions, especially in evaluation studies that rely on small samples, is highlighted. To reduce the bias that may result if the propensity score model is not correctly specified, I match on a Mahalanobis distance specified by selected control variables in addition to the propensity score (see discussion in Huber, Lechner, and Steinmayr, 2014). All variables that are included in the specification of the propensity score and that are added as control variables in the Mahalanobis distance are summarized in Table 3 in the next Section as well as in Table 5 in Appendix B.

The identification of the effect of interest requires overlap in the distributions of the propensity scores between the samples of treated and controls (see the discussion in Lechner and Strittmatter, 2014).

**Assumption 2** (*Common Support*).

$$p(x) < 1, \text{ where } p(x) = Pr(D_i = 1 | X_i = x)$$

with  $p(x)$  is the conditional treatment probability (propensity score).

I enforce Assumption 2 by excluding all observation outside of the common support from the estimations. This does not affect the results, since the share of excluded observations only amounts to 0.16%.

## 4.2 Descriptive statistics

In Table 3, I report means of the observed characteristics of selected control variables with large standardized differences before matching. Additionally, I present descriptive statistics for observed characteristics with small standardized differences in Table 5 in Appendix B. Information on individual characteristics refers to the time of the return and registration as a job returner at a local employment agency. Only the characteristics of local employment agency districts refer to the (pseudo) treatment time.

The means of control variables for treated and non-treated job returners are reported in the first two columns of Table 3. There are large differences between treated and non-treated women with regard to family status, last occupation and employment history. Those who receive a training voucher are more likely to be married and to have children younger than six years old, but they are less likely to have children younger than three years old. They were more often employed in finance and insurance occupations. Non-treated job returners were more often employed in the manufacturing, trade and service sectors. The share of job returners who have previously worked part time is higher in the control group, but those in the treatment group exhibit a preference to work part time in the future. There is a sizable difference between the duration of the employment interruption of treated and non-treated women. Training voucher recipients had an av-

Table 3: Mean values with large standardized differences

	Treatment- group	Control- group	SD between (1) and (2)	Matched Control group	SD between (1) and (3)
<b>Personal and family characteristics</b>					
Age in years	36.614	35.174	22.647	36.324	4.705
Children $\leq$ 3 years	0.242	0.396	33.443	0.252	2.258
<b>Occupational characteristics</b>					
Blue-collar worker	0.217	0.305	20.235	0.222	1.172
Finance and insurance sector	0.396	0.301	20.022	0.390	1.374
<b>Employment and welfare history</b>					
Eligible for unemployment benefits	0.132	0.317	45.541	0.138	1.954
Monthly wages last 3 years (Euros)	531	339	31.311	524	1.030
Cumulated wages last 25 years (Euros)	116,204	92,019	22.405	113,093	2.748
Cumulated benefits last 25 years (Euros)	8,236	12,254	30.378	8,167	0.577
Months employed last 1 years	3.224	2.102	27.200	3.255	0.700
Months employed last 3 years	11.177	7.789	30.010	11.135	0.346
Months unemployed last 3 years	5.080	7.135	26.464	5.026	0.754
<b>Interruption and return characteristics</b>					
Interruption duration (months)	74.172	39.688	61.544	73.365	1.193
Return 4th quarter 2004	0.067	0.131	21.594	0.062	1.929
Start interruption 1980-1989	0.117	0.036	30.940	0.116	0.562
Start interruption 1990-1999	0.307	0.142	40.338	0.299	1.636
Status prior interruption: employed	0.470	0.317	31.657	0.472	0.551
Switch before treatment: 1	0.114	0.262	38.670	0.122	2.504
Switches before treatment: 2	0.006	0.091	40.637	0.006	0.751
<b>Regional characteristics</b>					
Lower-Saxony, Saxony, Thuringia	0.099	0.202	29.120	0.102	0.976
Share of employed assurance sector	0.039	0.036	24.055	0.039	1.211
Share of employed construction sector	0.062	0.067	26.339	0.062	0.192
Share of employed education sector	0.038	0.043	28.000	0.038	0.924
Share of employed private sector	0.001	0.001	33.066	0.001	1.153
Share of employed public service sector	0.064	0.068	23.193	0.064	0.405
Share of vacant full-time vacancies	0.787	0.766	20.004	0.782	4.551
Unemployment rate	11.636	13.451	33.118	11.598	0.745
West-Germany	0.148	0.313	40.091	0.151	0.817

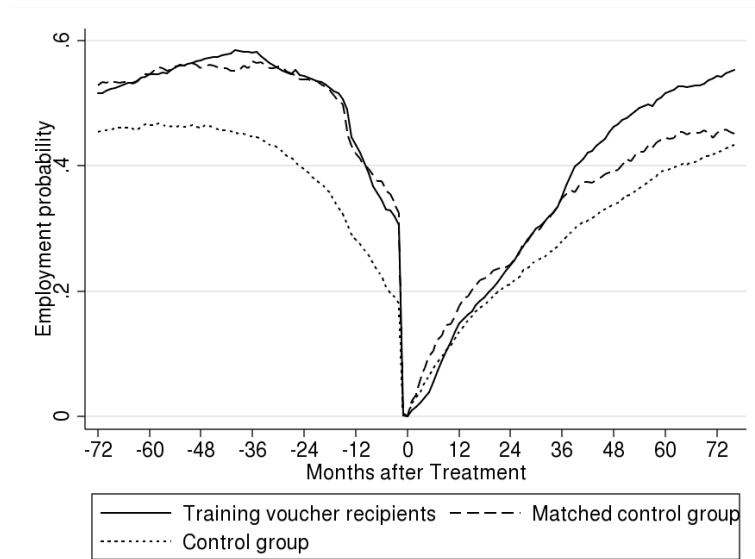
Note: See Table 5 in Appendix B for mean values of observed characteristics with small standardized differences. In columns (1) and (2), the mean values of observed characteristics for the treated and non-treated sub-samples are reported. Column (4) shows the mean values of the matched control group. Information on individual characteristics refers to the time of registration as job-returned in local employment agencies, with the exception of the monthly regional labor market characteristics which refer to the (pseudo) treatment time. Additionally, I report the standardized differences between the different samples.

erage interruption duration of 74 months (more than 6 years). The interruption among the non-treated women is considerably shorter (40 months). Regarding their employment and welfare histories, I observe a positive selection of those who received a voucher. They were employed longer, had higher wages and received fewer benefits.

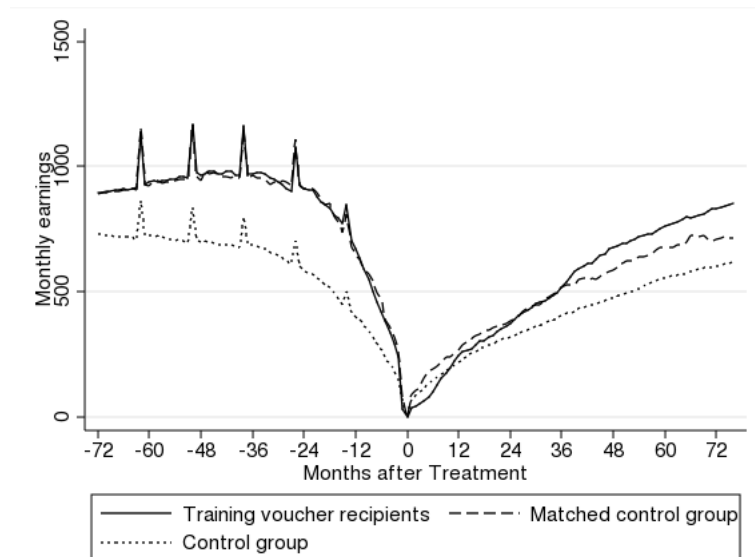
The evaluation literature stresses the importance of employment history as a control variable (e.g., Heckman et al. 1999, and Mueser et al. 2007 with regard to US programs, and Biewen et al. 2014, and Lechner and Wunsch, 2013, with regard to German training programs). Lagged employment, wages and benefit receipt history appear to be good predictors of future outcomes. This information is available in the data, and I use it in a flexible manner. In Figure 2, I present the average employment and earnings levels



Figure 2: Outcome levels of comparison groups for the pre-interruption and post-treatment period



(a) Employment probability



(b) Monthly earnings (in Euros)

Note: The levels of employment and earnings are presented for each of the 72 months before the interruption and the 76 months following the treatment (148 months). The spikes in the levels of monthly earnings before interruption appear every twelve months and are caused by end of the year bonus payments. They are obvious in the data because a high share of women interrupted their labor force participation at the end of December. Nevertheless, the return dates are equally distributed over the year.

for the treated group, control group and matched control group separately over the pre-interruption and post-treatment time horizon. I find a large difference in the employment and earnings level of treated and control persons before interruption. Therefore, it is important that I carefully condition on all relevant variables that induce selection bias. Once I flexibly condition on all relevant observed information in the data, especially for

short- and long-term employment and welfare history, the differences between the two groups disappear. I interpret this as an indicator of high match quality. By interpreting the levels of employment and earnings before interruption, I find employment rates of 50-60% for treated job returners. These levels appear reasonable because I do not restrict the sample to those who are employed before interruption. The definition of job returners also includes those who have interrupted unemployment or apprenticeship. The employment and earnings rates of the control group are lower, which confirms the positive selection of treated persons with respect to employment and welfare histories. This positive selection corresponds to a selection rules that was implemented as part of the Hartz-Reform in January 2003. The so-called 70% rule requires to award vouchers only to those unemployed individuals who are expected to have at least a 70% chance of entering employment soon after the program.

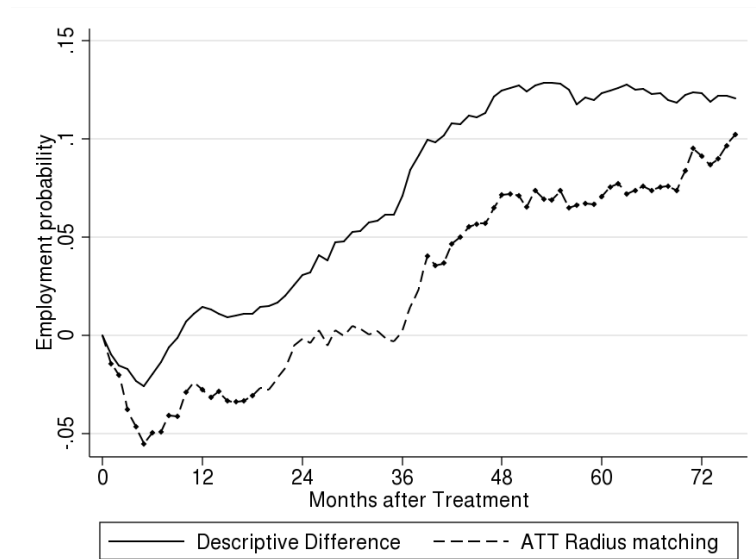
## 5 Empirical results

### 5.1 Employment and earnings effects

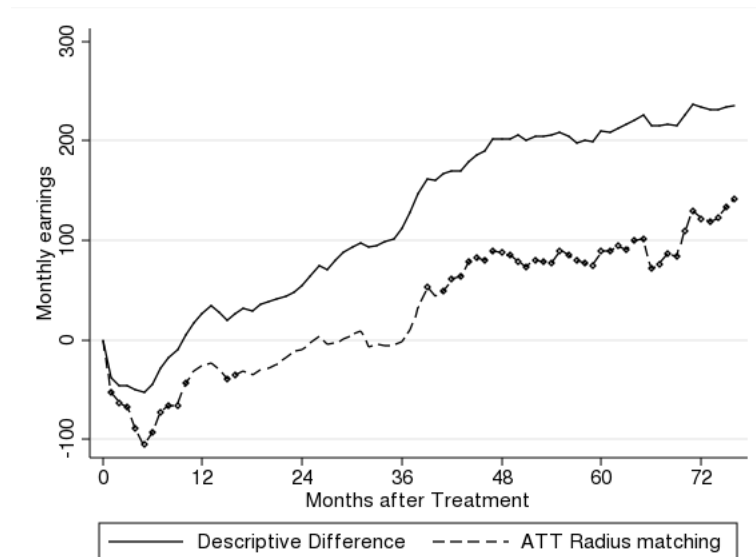
Awarding a training voucher has the objective of re-integrating job returners into the labor market and increasing their human capital accumulation. Thus, the outcomes of main interest here are the probability of being employed and monthly earnings. I estimate the effects of interest for each month after the training voucher award (with simulated pseudo start dates for those who did not receive a training voucher). I count a job returner as employed if I have data for non-subsidized and non-marginal employment for at least 31 days. Earnings are calculated as real gross earnings per month. I construct the outcome measures over a period of 6 years and 4 months for each woman in the sample.

The effect estimates for the main sample are presented in Figure 3 for each month separately. The lines indicate the point estimates for each month. Diamonds indicate significant effects at the 5% level. I compare women who receive a training voucher to those who do not within a twelve-months period after return and condition on the (pseudo) start dates of the treatment. The black solid line indicates the unconditional difference between the treatment and control groups. The dashed line displays the ATT obtained by radius matching with linear bias adjustment for each month after voucher receipt. The effect estimates for both outcome measures show negative effects over the short term, whereas the long-term effects are positive. The negative effects over the short term are called lock-in effects. Such effects are often found in this type of evaluation study, where an observation period starts with the treatment and not when it ends. Lock-in effects simply occur because training participants reduce their search activity during the course. This reduces both the probability of being employed and monthly earnings over the short-

Figure 3: ATT on various labor market outcomes (full sample)



(a) Employment probability



(b) Monthly earnings (in Euros)

Note: Effects are estimated for each of the 76 months following the treatment. Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

term. After two years (the average duration of training courses) the point estimates become positive, and I find positive and statistically significant effects after three years. I find an employment effect of nearly ten percentage points six years after treatment. In other words, job returners who received a training voucher have a ten percentage points higher probability of being employed than had they not received a training voucher. The long-term effects on monthly earnings amount to 140 Euros (approx. 160 US-Dollar) per month. These effects are quite large compared to the effect estimates found for vouchers

Table 4: Cumulative employment and earnings

Months after treatment	Cumulated employment		Cumulated earnings	
	Coef.	Std. Dev.	Coef.	Std. Dev.
12	-.438	.097	-818.22	135.73
24	-.736	.237	-1134.41	314.93
36	-.743	.381	-1147.79	519.99
48	-.198	.525	-416.81	735.12
60	.628	.676	553.83	975.15
72	1.571	.830	1710.51	1233.91
76	1.947	.881	2227.59	1319.38

Note: Accumulated effects are presented for each of the 76 months following the treatment. Significance levels are bootstrapped with 999 replications.

awarded to regular unemployed persons during the same period (comp. Doerr et al., 2013).

The ten percentage points higher employment probability after more than six years may emerge from the high indirect cost due to foregone employment and earnings over the short run during pronounced lock-in periods. To assess the success of vocational training from a net benefit perspective, I accumulate the employment and earnings effects over time to obtain the net benefits for job returners after a period of more than six years. The results are reported in Table 4. Compared to the first month after treatment, training voucher recipients are employed two months longer than comparable non-recipients after a period of 76 months. The net benefit of receiving a training voucher seems quite small and is not statistically different from zero. The same is true for accumulated earnings. Compared to the first month, voucher recipients gain 2,227 Euros (2,500 US-Dollar) over a horizon of 76 months.

The results obtained by different evaluation approaches are presented in Figure 12 in Appendix A. There are only small differences between the effect estimates obtained from static approaches using different time windows and the (pseudo) elapsed duration as an additional control variable. Nevertheless, I find different effect estimates when applying a dynamic evaluation framework. The results from that approach are indicated by the solid gray line. Frederiksson and Johansson (2008) argue that effects might be underestimated due to a positively selected control group (see discussion in Section 3.3). In this case, the effects from a dynamic approach would always be greater than those obtained from a static approach. I have argued that this is not necessarily transferable to job returners. Job returners who are not treated within a window of 12 months may still be unsure about their return decision. In this case, the effect estimates obtained by a static approach would be overestimated. The results do not support any of those arguments. Instead, I find evidence of an attenuation effect if I apply a dynamic evaluation approach. The attenuation effect can be explained by the fact that a fraction of control persons each

month  $m$  will likely receive a training voucher in a following month  $m + t$ . The short-term effect estimates (lock-in effects) are closer to zero because we compare voucher recipients during their potential subsequent training participation with control persons who partly participate in training courses shortly thereafter. The lower effect estimates over the long term can be rationalized by the same argument. Over the long run, voucher recipients are compared with control persons that had partially participated in training courses and thus attenuate the estimated effects.

## 5.2 Robustness checks

I perform several checks to support the robustness of the results and provide a causal interpretation. First, I implement a pre-interruption outcome evaluation as a specification test. If there were time-constant unobserved characteristics that lead to selection bias that is not controlled for by included variables, these should be clear in an impact evaluation prior to interruption. Therefore, I use the same specification as in the post-treatment period and estimate the treatment effects for the 6 years before the interruption. The results of this test for employment and monthly earnings are presented in Figure 4.

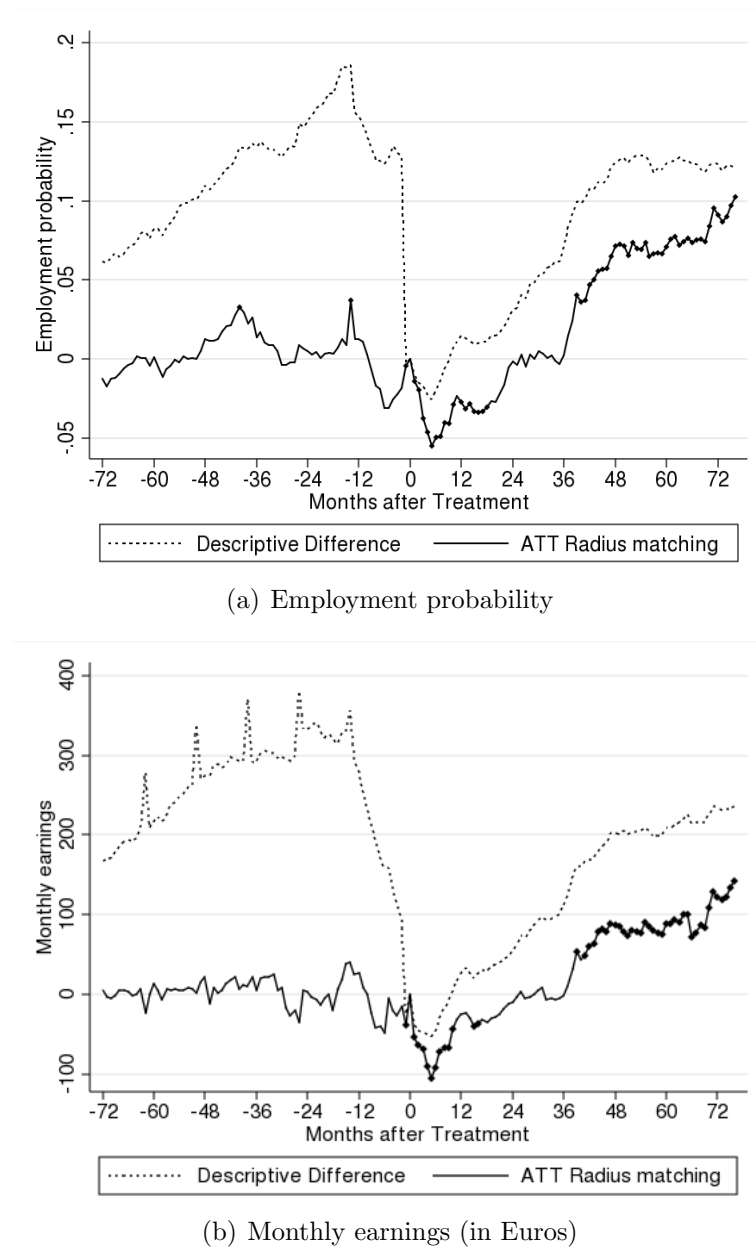
Figure 2 illustrates that the outcomes of the treated and matched control groups become similar when I condition on the observed characteristics available in the data. This result is confirmed when I estimate the effects for the 72 months before the interruption. The effect estimates are nearly never significantly different from zero. I interpret this as an indication of a causal interpretation of the main results.

In a second test, I use the unredeemed training vouchers as a placebo test. The idea behind this test is similar to the test presented above, but I now focus on the post-treatment period instead of the pre-interruption period. This allows me to rule out selection effects that may occur shortly before treatment (e.g., anticipation effects). If such effects exist they should also be visible in the effect estimates for the sub-group of job returners who did not redeem their training vouchers. The corresponding results can be found in Figure 5. Because the number of observations markedly reduces (90% of all job-returners redeem their training vouchers), I present additional effect estimates obtained by ordinary least square regressions. Both estimators imply that the treatment effects for unredeemed vouchers are nearly never significantly different from zero. Again, I interpret this result as a confirmation of the robustness of the main results.

## 5.3 Job quality

In this section, I investigate whether the receipt of a training voucher and potential subsequent participation in a training course affect the quality of employment obtained by job returners. I use the probabilities of being marginally employed, being employed full

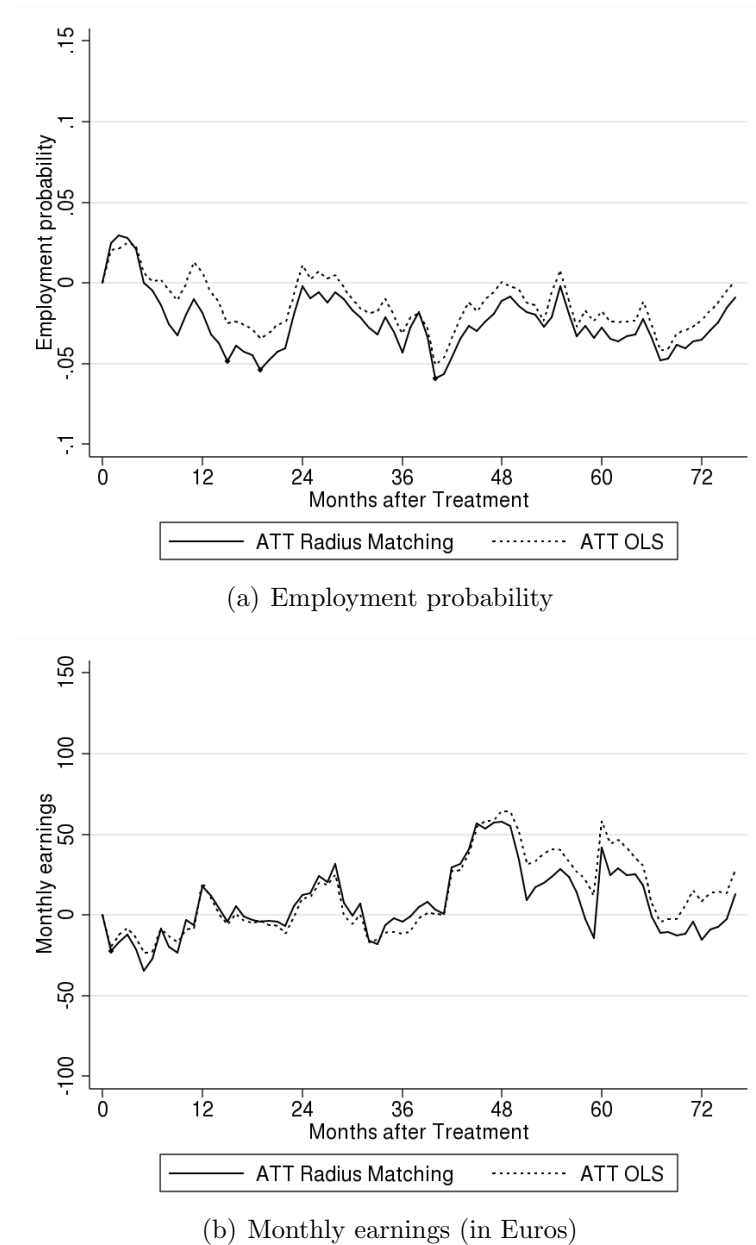
Figure 4: ATT on employment and earnings pre-interruption and post-treatment period



Note: Effects are estimated for each of the 72 months before the interruption and the 76 following the treatment (148 months). Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

time and earning at least 90% of previous earnings as indicators of job quality. Marginal employment is also a good measure of employment stability, because marginal employment tends to involve short-term contracts and no or reduced contributions to social systems. I find that the award of training voucher significantly reduces the probability of being marginally employed. The estimated effects for each month after treatment as well as the accumulated effects are presented in Figure 6. After a strong negative lock-in effect,

Figure 5: ATT for unredeemed training vouchers

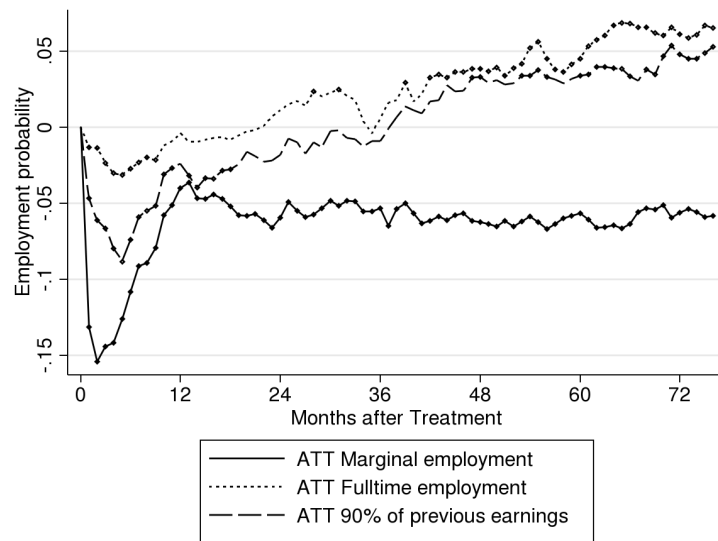


Note: Effects are estimated for each of the 76 months following the treatment. Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

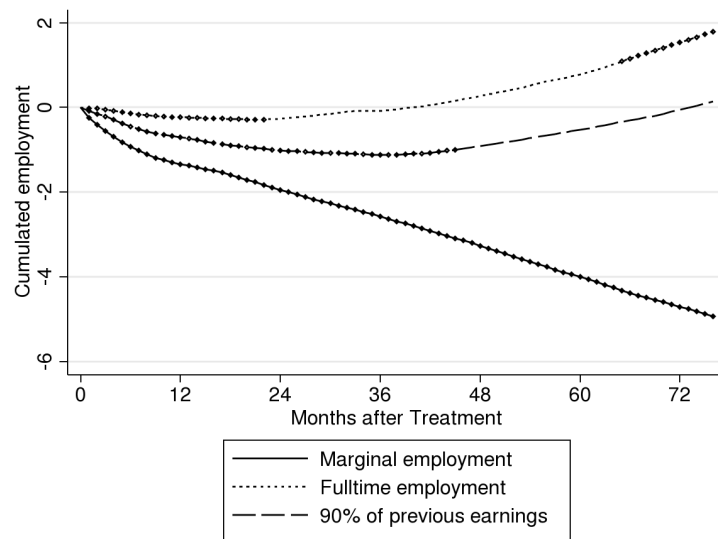
the probability of marginal employment is permanently reduced by 5 percentage points for job returners who receive a training voucher. The accumulated effects show that they were marginally employed for five fewer months than would they have been without a training voucher.

Conversely, the probability of being employed full time significantly increases for voucher recipients. The point estimate after 76 months amounts to a 6.5 percentage points higher probability of full-time employment. The effect seems stable at the fifth

Figure 6: ATT on job quality measures (full sample)



(a) Probability for job quality outcomes



(b) Cumulated effects for job quality outcomes

Note: Effects are estimated for each of the 76 months following the treatment. Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

year (60 months) after treatment. If I again examine accumulated effects, I find that job returners with training vouchers are employed full time nearly two months longer than comparable job returners without training vouchers. The amount of monthly earnings relative to the earnings before the interruption appears to be a good measure of job quality. I construct this measure as a binary outcome that equals one if the monthly earnings are at least 90% of the average earnings of the three years before interruption. The results indicate again that the award of a training voucher clearly increases the quality of em-



ployment obtained by job returners. The probability of earning at least 90% of previous earnings is significantly increased by 5 percentage points.

I clearly find empirical evidence of increasing employment quality due to training vouchers awarded to job returners. To support a causal interpretation of this result, I present the results of a pre-treatment outcome evaluation of job quality outcomes as a specification test in Figure 15 the Appendix A.

## 5.4 Heterogeneity

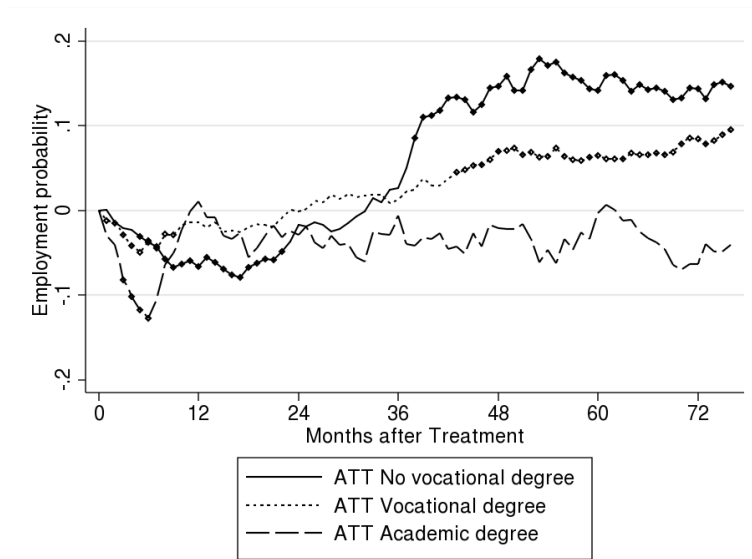
In this type of study, it is interesting to explore effect heterogeneity in more depth. There might be considerable differences in the effectiveness of training vouchers among subgroups of job returners with varying characteristics. In this study, I focus on differences among different qualified job returners and effect heterogeneity by course types. Qualifications are measured by vocational degrees at return. I distinguish between job returners with and without vocational degrees and identify high-skilled job returners with academic degrees. I stratify the main sample according to these dimensions and perform the estimations in each stratum separately. The heterogeneous effects by vocational qualification are presented in Figure 7.

The highest effect is found for the lowest-skilled job returners. I find negative short-term effects, but after 36 months, the point estimates jump to 12 percentage points for employment and to over 180 Euros (200 US-Dollar) for monthly earnings. The effect is highly stable until the end of the observation period. The lock-in effects for medium-skilled job returners with a vocational degree are shorter and less pronounced. After 3.5 years, the effects become significantly positive and amounts to a 7-9 percentage points higher employment probability. Earnings increase by nearly 130 Euros (145 US-Dollar). The accumulated effects show significant employment gains for both groups. The accumulated effects for job returners without vocational degrees are higher than for those with medium-level skills, although they suffer from stronger losses over the short run.

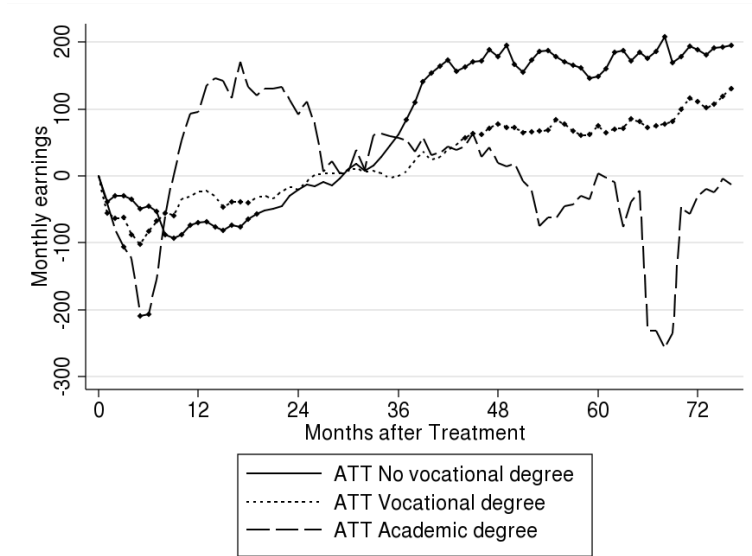
In contrast, the results for the highest-skilled job returners with academic degrees paint a more negative picture. After a lock-in period with negative effects, I find no effects of a voucher award over the entire observation period. Although the effect estimates for monthly earnings are imprecisely estimated for highly-skilled job returners, they indicate a zero effect. Thus, awarding training vouchers to high-skilled job returners with academic degrees is ineffective. I do not find positive effects on employment probability or on monthly earnings. Accumulated effects for employment instead indicate employment losses, but these effects are not significant. The results for the specification test for pre-interruption outcomes are provided in Appendix A (comp. Figure 14).

Additionally, I am interested in the effectiveness of the different course types condi-

Figure 7: ATT on employment and earnings by vocational degree



(a) Employment probability



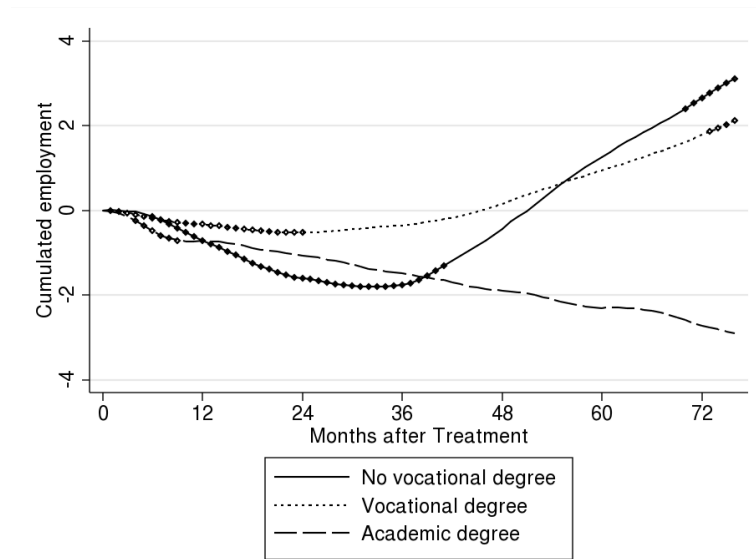
(b) Monthly earnings (in Euros)

Note: Effects are estimated for each of the 76 months following the treatment. Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

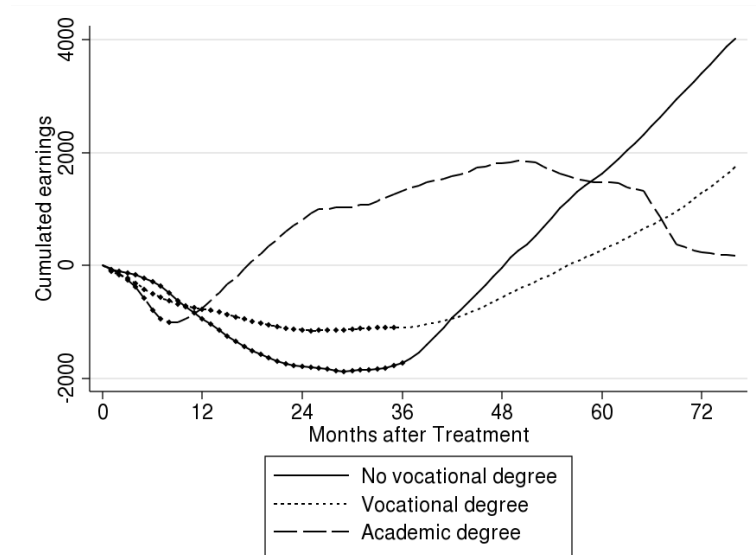
tional on voucher redemption. In the sample, 90% of all job returners redeem their training vouchers. The results for effect heterogeneity by course type cannot be interpreted in a causal way, because redemption of the voucher may also include a particular selection. The results are nevertheless suggestive, because they indicate the channel through which the causal effect operates.

The effect estimates clearly show that the success of the different program types increases with the human capital that is provided in the courses (comp. Figure 9). Programs

Figure 8: Cumulative employment and earnings by vocational degree.



(a) Accumulated employment

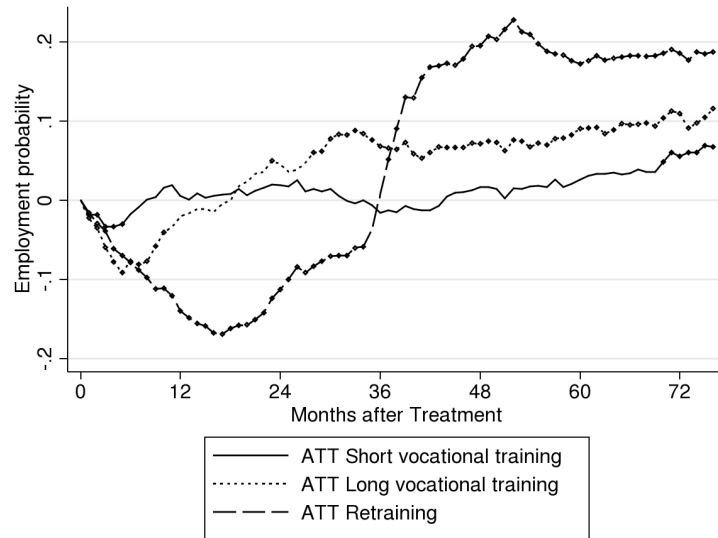


(b) Accumulated earnings (in Euros)

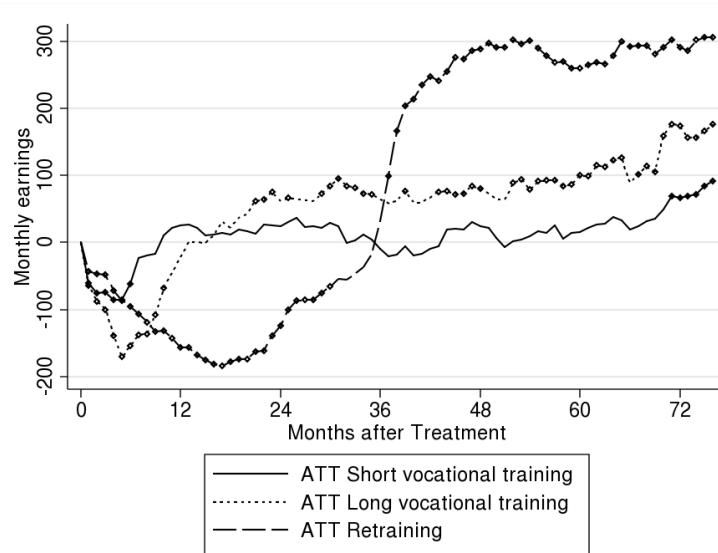
Note: Accumulated effects are presented for each of the 76 months following the treatment. Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

with rather short durations, five months on average, raise the employment probability over the long term by 6-7% and earnings by 90 Euros per month (100 US-Dollar). Long training programs provide occupation-specific knowledge and have an average duration of 10 months. The employment probability for participants in this course type increases by nearly 10 percentage points 30 months after treatment. These effects are highly stable over the period considered here. Earnings increase by approximately 150 Euros (170 US-Dollar) six years after the training voucher is awarded. Retraining courses have the

Figure 9: ATT on employment and earnings by course type



(a) Employment probability

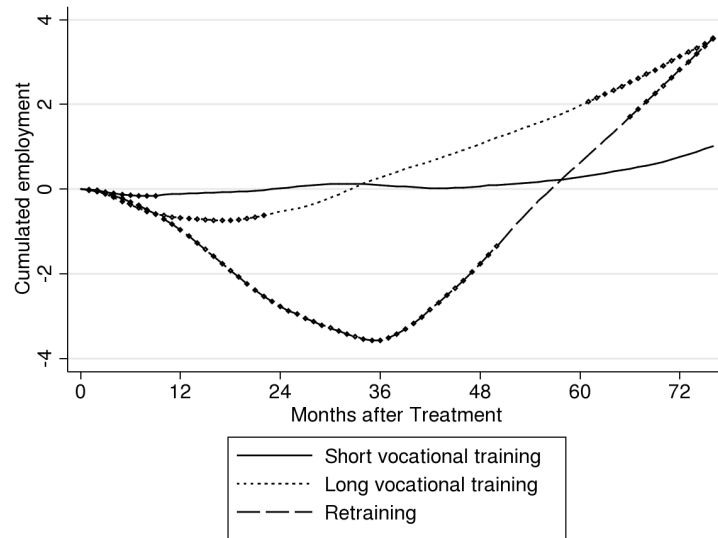


(b) Monthly earnings (in Euros)

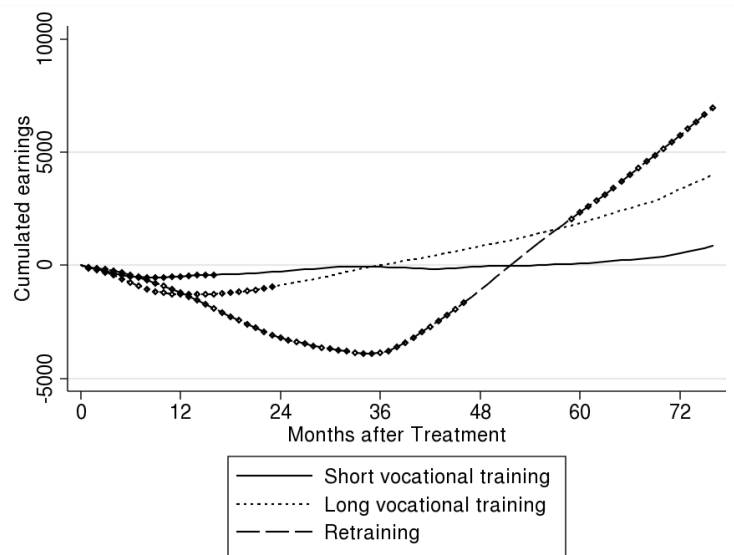
Note: Effects are estimated for each of the 76 months following the treatment. Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

longest durations (up to three years). Participating in such programs opens the possibility to obtain a vocational degree. Therefore, it is not surprising that I find long and deep lock-in effects of three years for this course type. The employment probability is reduced by 16 percentage points and earnings are decreased by 185 Euros (210 US-Dollar) over the short term. However, after 36 months, the employment gain jumps rapidly to 20 percentage points and earnings gains are relatively large at over 300 Euros (340 US-Dollar) per month.

Figure 10: Cumulative employment and earnings by course type



(a) Accumulated employment



(b) Accumulated earnings (in Euros)

Note: Accumulated effects are presented for each of the 76 months following the treatment. Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

In the case of retraining courses for which I observe pronounced negative lock-in effects, it is important to consider accumulated effects. I present the results for cumulative employment and earnings in Figure 10. I do not find any effect on cumulative employment or earnings for job returners who participated in short programs. In terms of cumulative employment, long programs and retraining programs perform similarly. Job returners who participate in these course types are employed four months longer than comparable job returners without training vouchers. In terms of cumulative earnings, I do not find

significant earnings gains for short and long courses. I find a significant earnings gain of 7,000 Euros (8,000 US-Dollar) for retraining programs. Thus over the entire observational window, long programs and retraining courses perform similarly with regard to the probability of employment, but participants in retraining courses gain significantly higher monetary returns from their investment in human capital.

## 6 Conclusion

This is the first paper to investigate the impact of publicly funded vocational training for female job returners on subsequent working careers. In particular, I analyze whether vocational training provided through a voucher has positive effects on employment probability, monthly earnings, and job quality over the long run. Social and economic arguments demonstrate the importance of the successful re-integration of women with interrupted employment histories. This study focuses on the case of Germany where job returners are defined as particular eligible individuals and have the opportunity to participate in ALMP programs if they register at local employment agencies.

Using rich administrative data on a 100% sample of job returners who are awarded a training voucher in the years 2003-2005 and a randomly generated control group, I identify the treatment effect of a training voucher award for female job returners. The results suggest that the award of a training voucher translates into a higher employment probability (10 percentage points) and higher monthly earnings (140 Euros/160 US-Dollars). In addition to these standard outcome measures, I am interested in the effects on job quality and employment stability. The award of a training voucher increases the quality of employment of job returners. They face a higher probability of receiving at least 90% of their previous earnings and of obtaining full-time employment. In contrast, the probability of acquiring unstable marginal employment decreases permanently. I perform many robustness checks that support a causal interpretation of the results.

The investigation of effect heterogeneity reveals some interesting insights regarding vocational degrees and the effectiveness of different course types. The results strongly imply that the effects of training vouchers for job returners work through the human capital channel. I find no effect for unredeemed training vouchers. Conditional on redemption of the voucher, the effectiveness of training increases with the human capital intensity provided by the respective course. This result is also in line with the finding that the award of a training voucher is ineffective for the most skilled job returners. In contrast, the effect are the highest for job returners without a vocational degree. This study shows that awarding a training voucher to low- and medium-skilled job returners contributes to the successful re-integration of these women. They benefit from higher employment probability, higher job quality and earning gains if they participate in courses that provide

a substantial amount of human capital.

## References

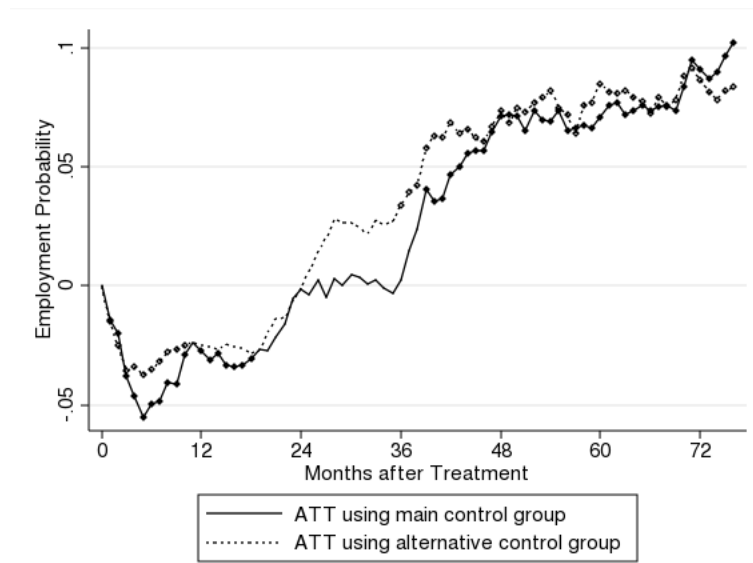
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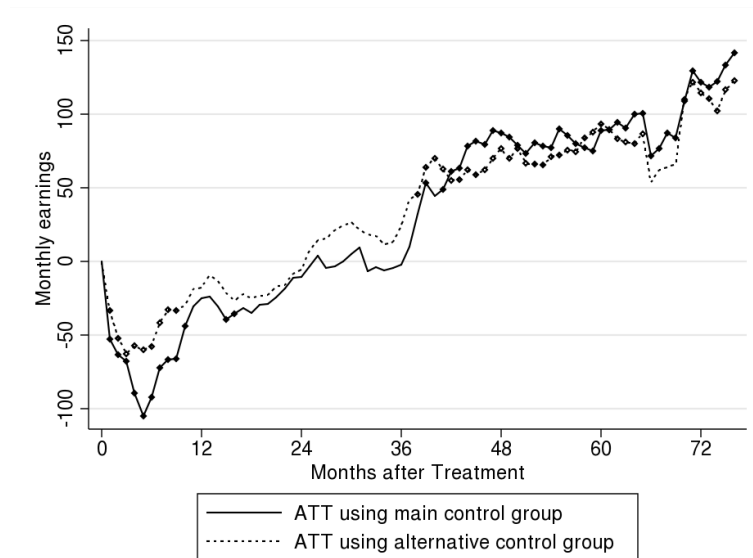
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# A Additional Graphs

Figure 11: ATT using the alternative control group



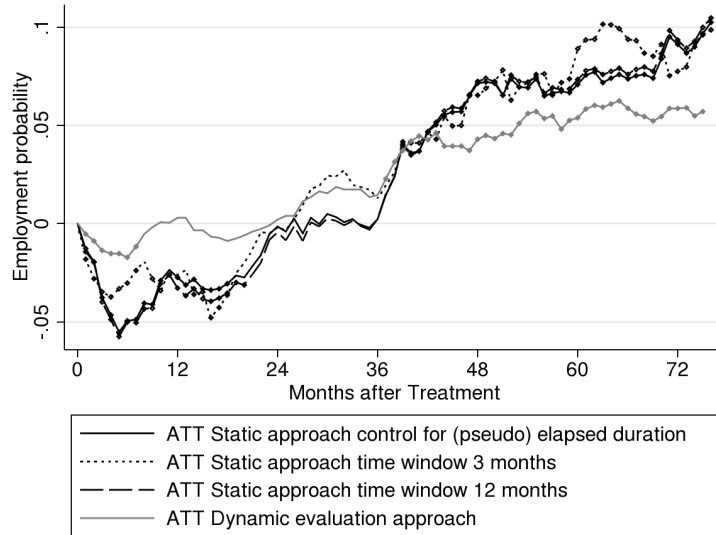
(a) Employment probability



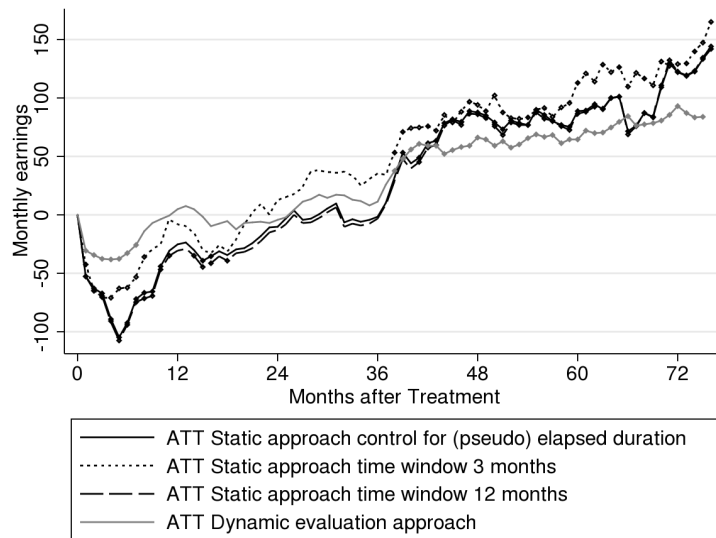
(b) Monthly earnings (in Euros)

Note: Effects are estimated for each of the 76 months following the treatment. Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

Figure 12: ATT using different evaluation approaches



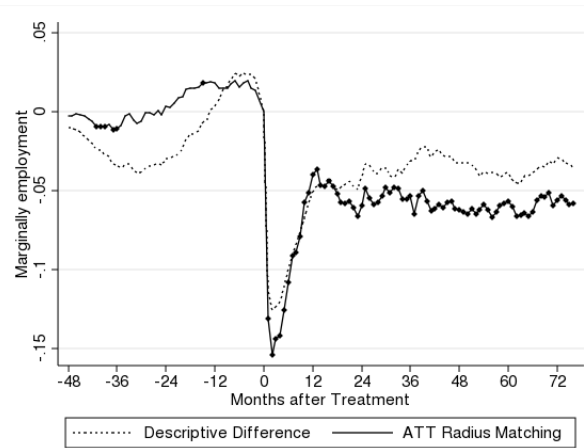
(a) Employment probability



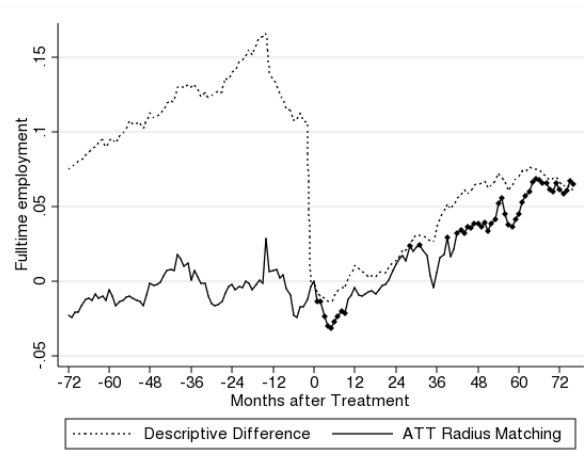
(b) Monthly earnings (in Euros)

Note: Effects are estimated for each of the 76 months following the treatment. Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

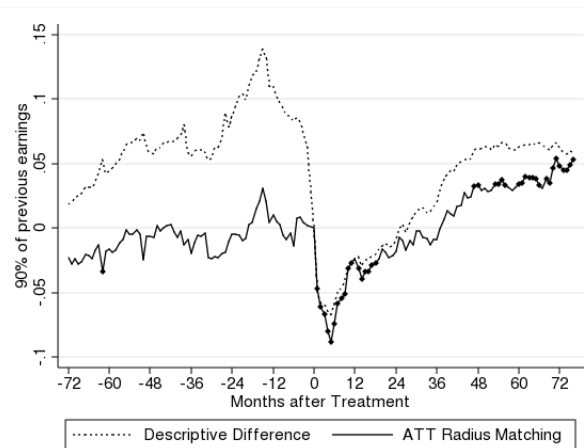
Figure 13: ATT on job quality pre-return and post-treatment period



(a) Probability to be marginally employed



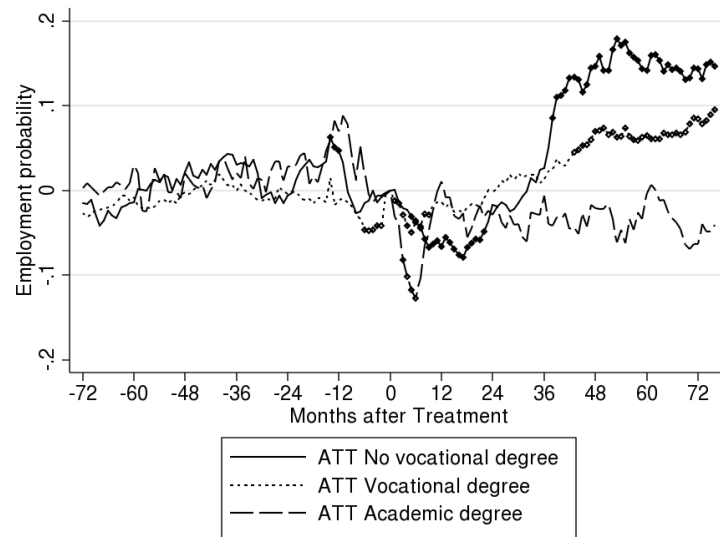
(b) Probability to be fulltime employed



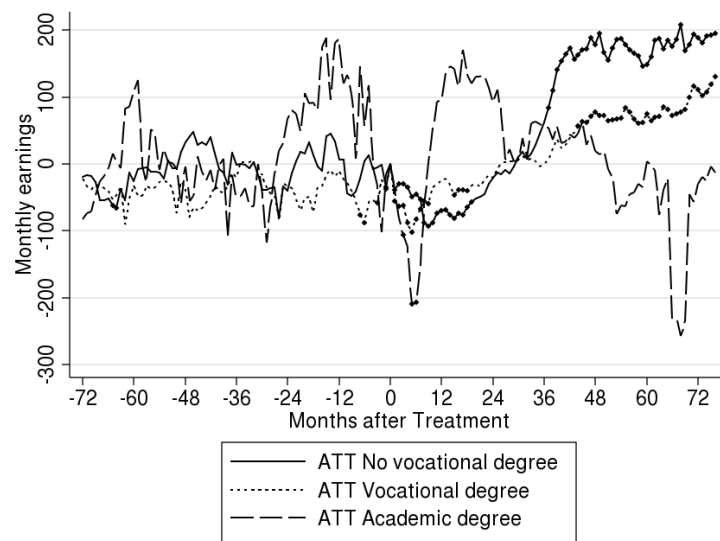
(c) Probability to earn 90% of previous earnings

Note: Effects are estimated for each of the 72 months before the interruption and the 76 following the treatment (148 months). Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

Figure 14: ATT by vocational degrees pre-return and post-treatment period



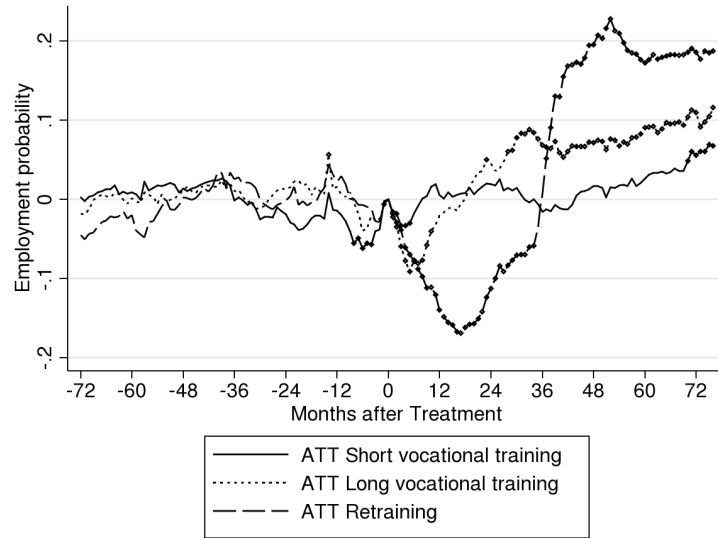
(a) Employment probability



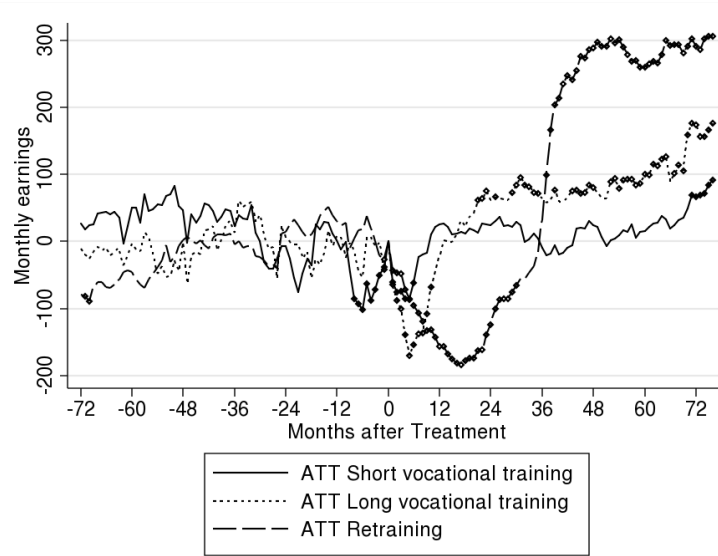
(b) Monthly earnings (in Euros)

Note: Effects are estimated for each of the 72 months before the interruption and the 76 following the treatment (148 months). Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

Figure 15: ATT by course type pre-return and post-treatment period



(a) Employment probability



(b) Monthly earnings (in Euros)

Note: Effects are estimated for each of the 72 months before the interruption and the 76 following the treatment (148 months). Diamonds report significant point estimates at the 5%-level. Significance levels are bootstrapped with 999 replications. Lines without diamonds indicate that point estimates are not significantly different from zero.

## B Descriptive statistics

Table 5: Mean values with small standardized differences

	Treatment- group	Control- group	SD between (1) and (2)	Matched Control group	SD between (1) and (3)
<b>Personal and family characteristics</b>					
Married	0.628	0.536	18.757	0.623	1.173
Single Parent	0.248	0.277	6.634	0.248	0.008
Agegroup 25-29 years	0.154	0.228	19.069	0.158	1.113
Agegroup 30-34 years	0.216	0.258	9.899	0.221	1.250
Agegroup 35-39 years	0.284	0.252	7.156	0.285	0.312
Agegroup 45-49 years	0.085	0.069	6.140	0.081	1.573
Child younger 6 years	0.253	0.172	19.818	0.258	1.025
Health problems	0.053	0.078	10.201	0.053	0.085
No german citizenship	0.036	0.040	2.031	0.036	0.278
<b>Education and occupation</b>					
No schooling degree	0.017	0.040	14.270	0.017	0.008
Schooling degree (without Abitur)	0.743	0.792	11.671	0.739	0.783
University entry degree	0.006	0.003	4.365	0.006	0.204
No vocational degree	0.179	0.234	13.644	0.182	1.012
Academic degree	0.083	0.057	10.407	0.084	0.503
Part-time	0.358	0.344	2.971	0.362	0.925
Part-time desired	0.534	0.443	18.403	0.532	0.467
Communication, and Information	0.163	0.199	9.426	0.167	1.184
Energy, Waters, and Miners	0.077	0.097	7.056	0.073	1.315
Trade and Retail	0.123	0.150	8.058	0.124	0.473
Service sector	0.137	0.127	3.159	0.135	0.677
<b>Employment and welfare history</b>					
Months employed last 25 years	58.852	49.886	18.192	57.458	2.769
Months unemployed last 1 years	3.093	3.588	11.058	3.003	2.013
<b>Interruption and return characteristics</b>					
Elapsed duration	3.778	3.693	2.643	3.951	5.300
Status prior interruption: apprenticeship	0.016	0.008	7.562	0.016	0.253
Start interruption 1970-1979	0.001	0.001	0.799	0.001	0.090
Registered unemployed	0.128	0.136	2.476	0.129	0.331
Return 1st quarter 2003	0.124	0.144	5.857	0.139	4.305
Return 2nd quarter 2003	0.139	0.115	7.201	0.151	3.508
Return 3rd quarter 2003	0.130	0.116	4.447	0.137	2.098
Return 4th quarter 2003	0.131	0.092	12.635	0.127	1.180
Return 1st quarter 2004	0.194	0.154	10.497	0.185	2.193
Return 3rd quarter 2004	0.103	0.133	9.573	0.090	4.182

< table continues on next page >

Table 5: &lt; continued &gt;

	Treatment- group	Control- group	SD between (1) and (2)	Matched Control group	SD between (1) and (3)
<b>State of Residence</b>					
Baden-Württemberg	0.089	0.087	0.415	0.088	0.334
Bavaria	0.208	0.139	18.477	0.207	0.362
Berlin, Brandenburg	0.067	0.091	9.072	0.066	0.183
Hesse	0.072	0.056	6.659	0.070	0.663
Lower Saxony, Bremen	0.108	0.104	1.237	0.111	0.775
Northrhine-Westphalia	0.221	0.153	17.563	0.217	1.090
Rhineland Palatinate, Saarland	0.058	0.044	6.644	0.057	0.461
<b>Regional characteristics</b>					
Share of employed communic. sector	0.055	0.057	14.184	0.055	1.092
Share of employed energy sector	0.009	0.010	7.334	0.009	1.090
Share of employed health sector	0.118	0.117	2.146	0.117	2.793
Share of employed mining sector	0.005	0.005	0.894	0.004	1.218
Share of employed real estate sector	0.119	0.115	10.357	0.118	2.333
Share of employed service sector	0.046	0.047	4.409	0.046	2.104
Share of employed tourism sector	0.029	0.029	2.284	0.029	0.873
Share of employed trade sector	0.151	0.149	12.691	0.150	3.479
Share of male unemployed	0.562	0.553	19.720	0.559	5.715
Population density	627	513	8.921	619	0.623

Note: In columns (1) and (2), the mean values of observed characteristics for the treated and non-treated subsamples are reported. Column (4) shows the mean values of the matched control group. Information on individual characteristics refers to the time of registration as job-returner in local employment agencies, with the exception of the monthly regional labor market characteristics which refer to the (pseudo) treatment time. Additionally, I report the standardized differences between the different samples.