A Tale of Vouchers and Caseworkers in Public Training Programs*

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Abstract

This paper studies the role of training vouchers and caseworkers in publicly financed training programs. Using a rich administrative data set from the Federal Employment Agency in Germany, we apply regression and propensity score matching procedures to measure the effect of the Hartz reform in 2003, which introduced training vouchers and imposed more selective criteria on the applicants. Besides estimating the total reform effect, we disentangle the effect based on the introduction of vouchers—the voucher effect—from the effect induced by changes in the composition of program participants—the selection effect. Our results indicate that the selection effect plays only a minor role in explaining the generally positive impacts of the reform—in particular, as far as the most important program type (occupation-related or general training) is concerned.

Keywords: Active Labor Market Policy; Program Evaluation; Propensity Score Matching; Voucher

JEL: J64, J68, H43

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1 Introduction

In 2003, Germany reformed its active labor market policy (ALMP)—known as the Hartz reform. One major change of the reform is the introduction of the job-training vouchers. Like the school voucher policy, the aim of this reform is to increase the competition on the supply side of the "training" market and thereby to increase the quality of the training programs. Unlike the school educational vouchers for the primary school in which the parents make decision for their children, the participants of job-training programs are adults and they make their own decision on how to use the voucher.

Although educational vouchers are extensively studied in the economic literature,[1] there are fewer cases of vouchers for public training programs, and few studies on the job-training vouchers.[2]

However, along with the introduction of the vouchers into the ALMP, other important components of the Hartz reform include a stricter selection rule for participants, and a matching process between program types and participants by the caseworkers, based on the (caseworkers') expected re-employment probability of the participants.[3] The overall effect of the Hartz reform could thus result from the introduction of the vouchers—the voucher effect—and/or from a change in the composition of participants because of the new selection rule—the selection effect. To decompose the overall reform effect into these two effects, we apply a two-step propensity score matching procedure to a rich administrative data set.[4]

[1]For instance, Manski (1992) provides a theoretical model and some simulation results on school vouchers and social mobility. Using a general equilibrium model, Nechyba (2000) studies private-school vouchers and residential mobility. Angrist *et al.* (2002) find that private-school vouchers improve the academic outcomes of students who win voucher lotteries. Ladd (2002) reviews major studies on school vouchers.

[2]Winterhager *et al.* (2006) have studied the job placement vouchers in Germany.

[3]Lechner and Smith (2007) have studied the issue of caseworkers using Swiss data.

[4]Mueser *et al.* (2005) explore the issue of using administrative data to measure the performance of vocational training programs. They find that "propensity score matching is generally most

The remainder of this paper is organized as follows. Section 2 briefly describes the background of public training programs in Germany and of the Hartz reform. Section 3 outlines the analytical framework. We describe the administrative data set and the program types in section 4. Section 5 presents our results. Finally, section 6 concludes.

2 Background and Related Literature

One central aim of ALMP in Germany is to increase the employment prospects of the unemployed. For this purpose, the Federal Employment Agency (FEA) spends substantial amounts of money on measures such as public employment services, training programs, or employment subsidies. For instance, about 13.8 billion Euros were spent on ALMP measures in 2003. The most important part of ALMP in Germany are publicly financed training programs, accounting for more than 36 percent of this amount. However, the number of participants in these programs decreased over the last several years (see Figure 1). While 375,429 unemployed individuals entered a vocational training program in 2000, only 137,708 persons entered these measures in 2004.

A number of studies evaluates the general effectiveness of publicly financed training programs in Germany.[5] So far, the results are quite heterogeneous—depending on the method, the investigation period and the underlying data set.[6] Examples for insignificant or even negative effects are Lechner (1999, 2000), Hujer and Wellner (2000), and Hujer *et al.* (2006). Papers that find inconclusive results are Hübler (1997) or Kraus *et al.* (1999), and papers with positive findings

effective".

[5]The international literature on the evaluation of ALMP is summarized by Heckman *et al.* (1999), LaLonde (2003) and Kluve (2006), among others.

[6]For a recent review of the results see e.g. Caliendo and Steiner (2005).

are Fitzenberger and Prey (2000), Fitzenberger *et al.* (2006), and Lechner *et al.* (2005a, 2005b). The major lesson of these mixed results seems to be that positive effects mainly occur—if at all—in the long run, and that studies which find positive long-term effects are also reporting negative short-term effects.

While the focus of these studies lies on average effects, Rinne *et al.* (2007) extend the picture by investigating the effect heterogeneity of publicly financed training programs in Germany. Based on entrants into programs in 2000 and 2001, they present evidence for heterogeneous treatment effects—in particular, low-skilled individuals notably benefit from participation in the most important program type (occupation-related or general training). If this result is associated with the reform in 2003, it is—at least in part—conflicting with the strategy to increasingly select individuals with comparatively good employment prospects into publicly financed training programs.

Schneider and Uhlendorff (2006) compare the effectiveness of publicly financed training programs in Germany before and after the reform in 2003. Both studies conclude that the effectiveness increases between the pre- and post-reform period. However, the question which elements of the reform were responsible for the increased effectiveness—and to what particular extent—remains unanswered. In our point of view, two channels, vouchers and selection by caseworkers, are particular important and interesting in this regard.

Selection effect. The target group of publicly financed training in Germany has shifted after the reform. Individuals with comparatively good employment prospects are increasingly considered as participants in the post-reform period: persons entering publicly financed training programs are supposed to meet the criterion of a reasonable individual-specific expected re-employment probability (subjectively assessed by the caseworker). While this procedure intends to improve effectiveness,

it is not clear whether this is actually achieved. Without taking the counterfactual outcome if the individual under consideration would have not participated into account (as the expected re-employment probability does), essentially nothing can be said about effectiveness. Moreover, individuals with comparatively bad employment prospects are systematically excluded from participation. This may reflect the view of a positive correlation between the effectiveness of publicly financed training programs and participants' employment prospects. Besides equity considerations, one could argue that individuals with comparatively bad labor market prospects should represent the particular target group as this group exhibits the largest potential for improvements.

Voucher effect. The voucher effect represents the residual reform effect that is based on features of the reform not related to the selection process of program participants. The introduction of compulsory vouchers is the most important component in this context. The innovative instrument is intended to initiate a considerable enhancement of the quality of publicly financed training programs by means of an increased competition among the providers on the supply side of the market. After the reform, persons identified as belonging to the target group by the caseworker receive a voucher and select the most appropriate provider by themselves. The voucher only contains the contents of the measure, its maximum duration, and its maximum costs.

There are few studies on the above two effects. While Lechner and Smith (2003) find the value added by Swiss caseworkers is insignificant, Winterhager *et al.* (2006) show a positive effect from vouchers for job placement vouchers in Germany.

3 Analytical Framework

The aim of this paper is to estimate the effect of vouchers for publicly financed training programs, and to test the hypothesis that their introduction improves the quality (i.e. the effectiveness) of the programs under consideration. However, it is difficult to isolate the impact of vouchers since along with the introduction of the voucher system, there are other major changes going on as described above (especially a stricter selection rule for participants). The reform effect could thus result from a change of program quality and from a change of the composition of participants.[7]

A simple model to capture these two aspects and to isolate the voucher effect is the following:

$$Y_i = \alpha + X_i\beta + \delta D_i + \gamma R_i + \eta(D_i \times R_i) + \epsilon_i \tag{1}$$

where D_i is a dummy variable that takes the value one if individual i participates in the program and zero otherwise. R_i indicates the pre- and post-reform period similarly. Assuming that (i) there is no selection on unobservables, (ii) the treatment effect is homogeneous and (iii) the outcome and covariates have a linear relationship, the coefficient η in equation (1) represents the impact of the voucher. In this case, controlling for X_i is equivalent to controlling for the compositions of training participants before and after the reform.[8]

[7]Changes in the general economic situation may be another component of the reform effect. However, in what follows we control for this issue as participants and matched non-participants are subject to the same cyclical environment.

[8]We run this regression on the raw sample of participants and non-participants separately for each program type twice: (i) *without* controlling for individual characteristics X_i; and (ii) controlling for individual characteristics X_i. The results of these regression are discussed in Section 5.1.

However, in order to avoid parametric assumptions and to accommodate heterogeneity of treatment effects, we also apply propensity score matching procedures to isolate the impact of vouchers. Using the potential outcome framework as in Rubin (1974), we assume that each individual has two potential outcomes for the program: Y_{1i} is the outcome if individual i participates, and Y_{0i} if not. Let D_i be again an indicator for participation, we can define different treatment effects in a similar way as Heckman and Vytlacil (1999, 2005):

$$TE_i = Y_{1i} - Y_{0i} \qquad \text{(Treatment effect for individual } i\text{)}$$

$$ATE = E[TE_i] \qquad \text{(Average treatment effect for the population)}$$

$$ATT = E[TE_i|D_i = 1] \qquad \text{(Average treatment effect on the treated)}$$

and the average treatment effects on the treated before and after the reform are

$$ATT_b = E[TE_i|D_i = 1, R_i = 0] \qquad \text{(ATT pre-reform period)}$$

$$ATT_a = E[TE_i|D_i = 1, R_i = 1] \qquad \text{(ATT post-reform period)}$$

Under the matching assumptions of Rosenbaum and Rubin (1983)

$$(Y_{0i}, Y_{1i}) \perp D_i|X_i \qquad \text{(Conditional independence assumption)}$$

$$0 < prob(D_i = 1|X_i) < 1 \qquad \text{(Common support assumption)}$$

ATT_b (or ATT_a) can be estimated from pre-reform data (or post-reform data) by propensity score matching methods and it corresponds to δ (or $\delta+\eta$) in equation (1).[9]

[9]Heckman *et al.* (1997, 1998) present a weaker version of the conditional independence assumption: $E[Y_{0i}|D_i = 0, X_i] = E[Y_{0i}|D_i = 1, X_i]$.

However, the difference between ATT_b and ATT_a does *not* equal the effect of the introduction of vouchers, since the participants before and after reform may have different characteristics. As mentioned above, compared to the pre-reform period, the post-reform programs are more selective (possibly leading to a selection effect, SE) and vouchers are introduced (which may cause a voucher effect, VE), so:

$$ATT_a = ATT_b + VE + SE \tag{2}$$

Hence, the total reform effect (RE) can be written as:

$$\begin{aligned} RE &= ATT_a - ATT_b \\ &= VE + SE \end{aligned} \tag{3}$$

To be able to isolate the voucher effect, we apply a two-step propensity score matching procedure. In the first step, pre-reform participants are matched with post-reform participants. As a result, the obtained pairs of participants only differ with respect to the timing of participation. Importantly, observable characteristics do not differ anymore. In the second step, the matched pre-reform participants in 2002 are matched with non-participants of the same year. The corresponding treatment effect is the effect *only* for those participants under the pre-reform regime who are comparable to participants after the reform (we refer to this effect as ATT_{bQ}), so this step controls for the selection effect.

With this treatment effect, we can calculate the difference in differences of the treatment effects to estimate the voucher effect (VE):

$$VE = ATT_a - ATT_{bQ} \tag{4}$$

Finally, the comparison of the voucher effect with the reform effect gives us an estimate of the selection effect (SE):

$$\begin{aligned} SE &= RE - VE \\ &= (ATT_a - ATT_b) - (ATT_a - ATT_{bQ}) \\ &= ATT_{bQ} - ATT_b \end{aligned} \tag{5}$$

4 Data

In this paper, we use a particularly rich administrative data set, the Integrated Employment Biographies (IEB) of the FEA.[10] It contains detailed daily information on employment subject to social security contribution including occupational and sectoral information, receipt of transfer payments during periods of unemployment, job search, and participation in different programs of ALMP. Furthermore, the IEB comprises a large variety of variables like age, marital status, number of dependent children, disability, nationality and education.

The IEB contains information from four different administrative data sources: the employees' history (BeH), the benefit recipients' history (LeH), the job seekers' data base (ASU/BewA), and the program participants' master data set (MTH). The BeH comprises remuneration notifications of employers about employment subject to social security contributions. This information is included in the IEB from 1990 onwards. The LeH contains information about phases of benefit receipt starting in 1990. The LeH benefits mainly include unemployment benefits and unemployment assistance. The ASU/BewA contains data on individuals searching for a job. For 1997 and subsequent years, additional information about the labor market status of a given individual is provided by this administrative data source. The MTH contains

[10]The IEB is in general not publicly available. Only a 2.2% random sample (the Integrated Employment Biographies Sample, IEBS) can be obtained for research purposes. See e.g. Hummel *et al.* (2005) for details on the IEBS.

basic information about participation in active labor market programs—including publicly financed training programs—as well as about individual characteristics. Entries into programs of ALMP are identified from January 2000 onwards.

Since publicly financed training programs in Germany are quite heterogenous (especially with respect to content and duration), we concentrate on 4 particular types in what follows:

- Type 1: occupation-related or general training,
- Type 2: practice training in key qualifications,
- Type 3: practice firms, and
- Type 4: group training with occupation-related certificate.

Participants in type 1 learn specific skills required for a certain vocation (e.g. computer-aided design for a technician/tracer) or receive qualifications that are of general vocational use (e.g. MS Office, computer skills). Numerically, this type constitutes the most important type among all publicly financed training programs. In 2000, roughly 60 percent of all participants in training programs were assigned to this type (see Figure 2). After the reform in 2003, this share increased to more than 70 percent. Figure 3 shows that this measure is short-term oriented with a median duration of about 6–8 months. Moreover, the program duration decreased in the post-reform period. While about 70 percent of the participants finishes the scheme in less than 12 months in the pre-reform period, about 90 percent does so after the reform.

Type 2 is a program that is related to key qualifications, but very practically oriented. It contains only very few theoretical elements since it works according to the principle 'learning by doing'. Its median duration amounts to about 6 months

(see Figure 3). Participants in the post-reform period face a shorter program duration than in the pre-reform period.

Type 3 simulates employment in practice firms (or practice factories). An example constitutes technical training in the wood industry: participants learn woodworking skills at workbench and machines while being supervised by instructors. Its duration is similar to that of type 2 with the same median duration of about 6 months (see Figure 3). Again, participants in the post-reform period face a shorter program duration than in the pre-reform period.

Type 4 is a group training measure aiming to provide an occupation-related certificate. More specifically, a group of participants attends the same retraining measure at an educational institution. The measure also includes periods of practical training in certified companies/organizations. The aim is to provide participants a vocational degree by passing an examination at the respective academic institution. The median duration of this type is almost 24 months in the pre-reform period (see Figure 3). However, the duration markedly decreased after the reform. In the post-reform period, about 70 percent finish the scheme in 12 months or less.

Our sample of participants consists of 275 unemployed persons per quarter and program for the year 2003, i.e. we observe around 1,100 participants for each program type after the introduction of the training vouchers. In order to apply the matching approach as described in section 2, 20 participants from the period before the reform were drawn per participant in 2003, i.e. we draw a sample of 22,000 participants in 2002 per program type.

Beyond the matching of participants after with participants before the reform, we need to match participants with non-participants. Therefore, we draw 80 non-participants per participants. These individuals had to be unemployed for the same duration as the corresponding participants. Moreover, they are required to not

having participated in the respective type of training before and in the quarter of the participant's program entry, but we do not condition on future non-participation.

5 Results

In this section, we report the results based on the two approaches outlined above: (i) regression analysis; and (ii) two-step matching procedure.

The success of program participation is evaluated by looking at the probability of being regularly employed at a given point in time. In this context, regular employment only refers to jobs in the primary labor market. For instance, participation in job creation schemes and short-time employment (alone) are not included in this outcome measure. It is measured over a maximum period of 24 months starting at—and relative to—the program entry. We thus follow the prevailing approach in the recent evaluation literature.[11]

5.1 Regression Analysis

We run the regression according to equation (1) on the raw sample of participants and non-participants separately for each program type twice: (i) *without* controlling for individual characteristics X_i; and (ii) controlling for individual characteristics X_i. We present estimates on the probability of being employed 6, 12, 18, and 24 months after the program entry, respectively.

The results of these regression are in Table 1. As mentioned above, our primary interest lies on the estimate for the coefficient η, which represents the impact of the voucher. For all program types being analyzed, we consistently find positive

[11] A different approach concentrates on treatment effects only after the end of the program. For advantages and disadvantages of both approaches see e.g. Caliendo and Kopeinig (2006).

voucher effects on the outcome variable 6 and 12 months after the program entry, respectively. Moreover, these results are quite robust to controlling for individual characteristics X_i. However, the voucher effects turn out to be in general negative 18 and 24 months after the program entry, respectively. Only for program type 4, the voucher effect remains positive.

5.2 Two-Step Matching Procedure

By applying the described two-step matching procedure, we are able to decompose the reform effect into two separate components: a voucher effect and a selection effect. For this purpose, the treatment effects for the respective matched samples are calculated as the difference in mean outcomes between the matched groups. Below, we present estimates of differences in employment probabilities for a period of 24 month after the program entry, calculated every fortnight.

Firstly, we display estimates of the reform effect (RE) in Figure 4. The underlying ATT_a and ATT_b are based on differences in mean outcomes between program entrants in 2002 for the pre-reform period (in 2003 for the post-reform period) and matched non-participants *without* controlling for potential changes in the composition of participants between the two periods. The reform effect is then simply the difference between the two depicted lines.

We observe that participants in all analyzed program types face a substantial lock-in effect.[12] In the first months after entering the program, the employment probabilities of participants are considerably lower than those of matched non-participants. The duration and the extent of these lock-in effects vary by program type. While the rather short-term oriented program types 1–3 exhibit relatively moderate lock-in effects, the longer-term program type 4 leads to a considerable

[12]While participating—or being 'locked-in' in the program—individuals probably reduce their search activities for new jobs (van Ours, 2004).

reduction in employment probabilities for the whole observation period. In contrast, the treatment effects for program types 1–3 become generally positive about 12 months after the program entry.

The comparison of the estimates between the pre- and the post-reform period in Figure 4 reveals—if anything—positive reform effects for the program types being analyzed. However, the differences between ATT_a and ATT_b are very small for types 2 and 3. On the other hand, we observe significantly positive reform effects for types 1 and 4—at least for parts of the observation period. In particular, the lock-in effect of program type 4 seems to be considerably reduced after the reform.

In Figure 5 we present estimates of the voucher effect (VE). The underlying ATT_a and ATT_{bQ} are based on differences in mean outcomes between *matched* program entrants in 2002 for the pre-reform period (program entrants in 2003 for the post-reform period) and matched non-participants, respectively. By doing so, we take potential changes in the composition of participants between the two periods into account. The voucher effect is the difference between the two depicted lines.

In short, Figure 5 looks quite similar to Figure 4. This is not surprising for the post-reform period, since we again depict ATT_a for this period. However, the differences between ATT_b and ATT_{bQ} seem to be rather small. Only as far as type 4 is concerned, we observe noticeable differences: the treatment effect in the pre-reform period is substantially more negative if changes in the composition of participants between the two periods are controlled for.

Figure 6 reveals more insights about the extent and magnitude of voucher and selection effects. It presents the decomposition of the reform effect, and thus summarizes the previous graphs. While the reform effect (RE) is depicted in black, the voucher effect (VE) is depicted in gray. The selection effect (SE) is not explicitly identified, but this residual effect is the difference between the two depicted lines.

The decomposition shows that with respect to types 1 and 3, the reform effect seems to be almost exclusively based on the voucher effect. On the other hand, the selection effect seems to be an important driving force behind the reform effect as far as types 2 and 4 are concerned. In both cases, the selection effect is positive—leading thus to more positive reform effects for types 2 and 4 than otherwise. This particularly applies to type 4, for which the selection effect roughly accounts for half of the reform effect.

The results of two-step matching procedures thus indicate that the selection effect only plays a minor role in explaining the generally positive impacts of the reform in 2003—in particular, as far as the most important program type 1 is concerned. For this program type, the voucher effect seems to raise program quality, leading to an increased effectiveness. Our finding is consistent with Lechner and Smith (2007). An exception to this general statement of negligible selection effects constitutes the longer-term program type 4, for which the selection effect indeed seems to be an important factor behind the increased effectiveness in the post-reform period. On the other hand, also the voucher effect accounts for about 50 percent of the increased effectiveness we observe for type 4.

6 Conclusion

This paper analyzes the impacts of the labor market reform in 2003 on the effectiveness of publicly financed training in Germany. Using a rich administrative data set from the FEA, we apply regression and propensity score matching procedures to address the question whether the introduction of vouchers raised the quality of the programs by means of an increased competition. Therefore, we decompose the total reform effect into two separate components. By distinguishing between a voucher effect and a selection effect, we disentangle the effects that are based on the in-

troduction of vouchers from those that are based on changes in the composition of program participants.

For all program types being analyzed, regression analysis reveals positive voucher effects on the outcome variable 6 and 12 months after the program entry, respectively. However, the voucher effects turn out to be in general negative 18 and 24 months after the program entry, respectively. Only for program type 4, the voucher effect remains positive.

The results based on two-step matching procedures indicate that the selection effect plays only a minor role in explaining the generally positive impacts of the Hartz reform—in particular, as far as the most important program type 1 is concerned. For this program type, the voucher effect seems to raise program quality, leading to an increased effectiveness. An exception to this general statement of negligible selection effects constitutes the longer-term program type 4, for which the selection effect indeed seems to be an important factor behind the increased effectiveness in the post-reform period. On the other hand, the voucher effect still accounts for about half of the increased effectiveness.

In sum, we present evidence for weak selection effects. Only the effectiveness of the longer-term program type 4 seems to be positively affected by the strategy of the reform to increasingly select individuals with comparatively good employment prospects into publicly financed training programs.

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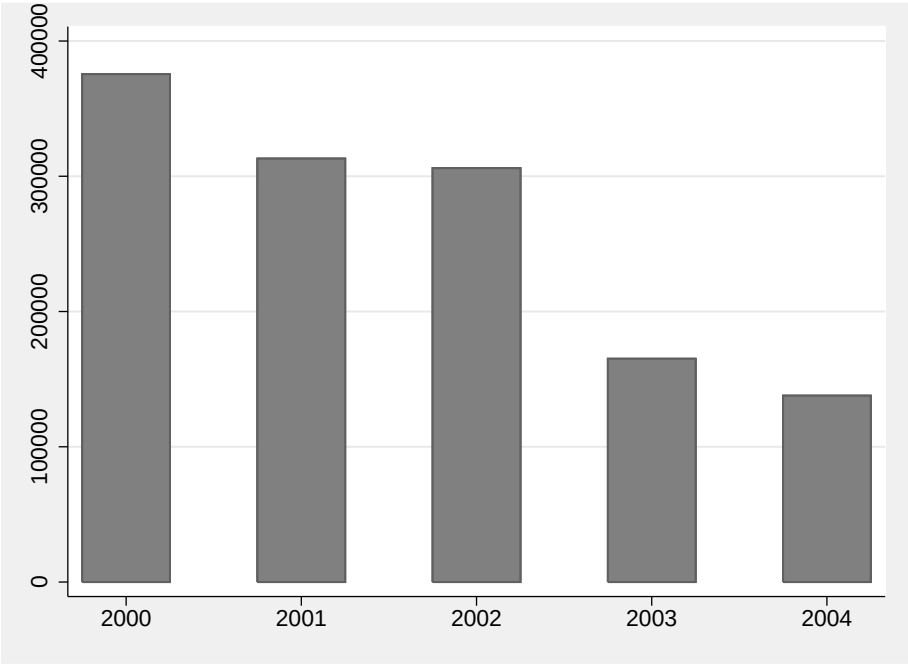
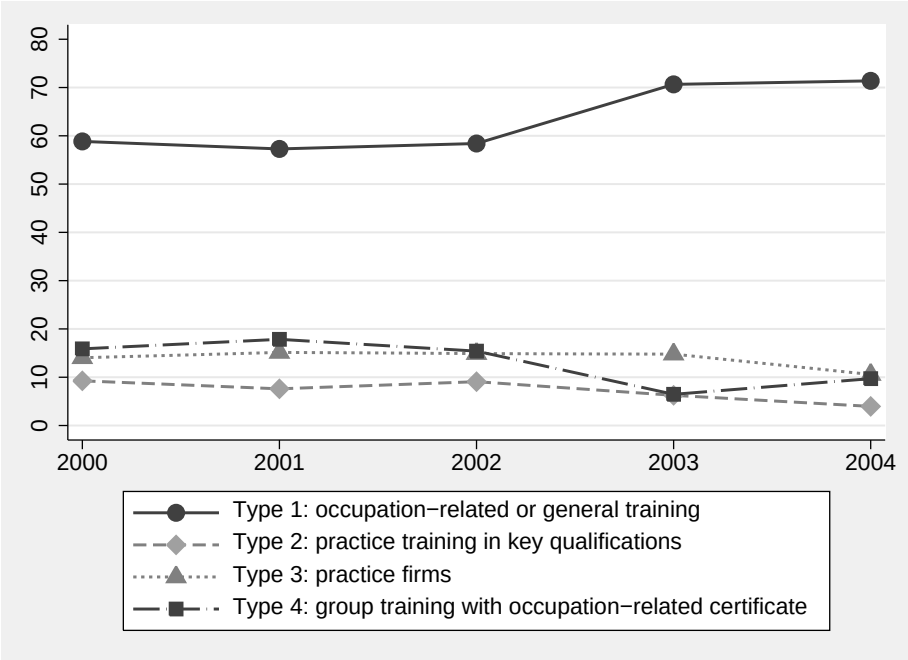
Figure 1: Annual Number of Entrants in Publicly Financed Training Programs (2000–2004).



Source: Federal Employment Agency (FEA).

Note: Annual number of entrants in publicly financed training programs.

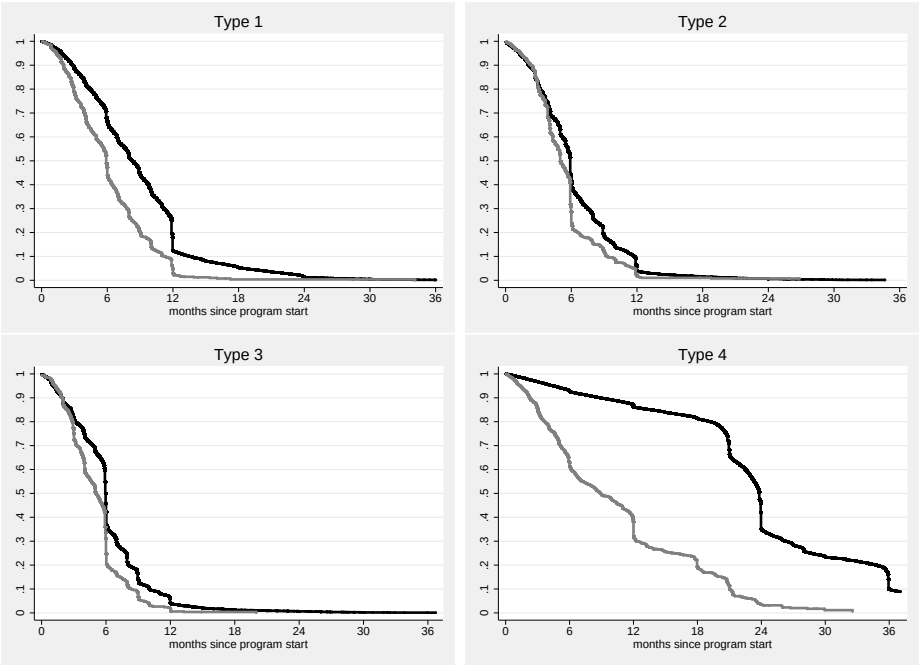
Figure 2: Share in Annual Number of Entrants by Program Type (2000–2004).



Source: Federal Employment Agency (FEA).

Note: Share (in percent) in annual number of entrants in publicly financed training programs.

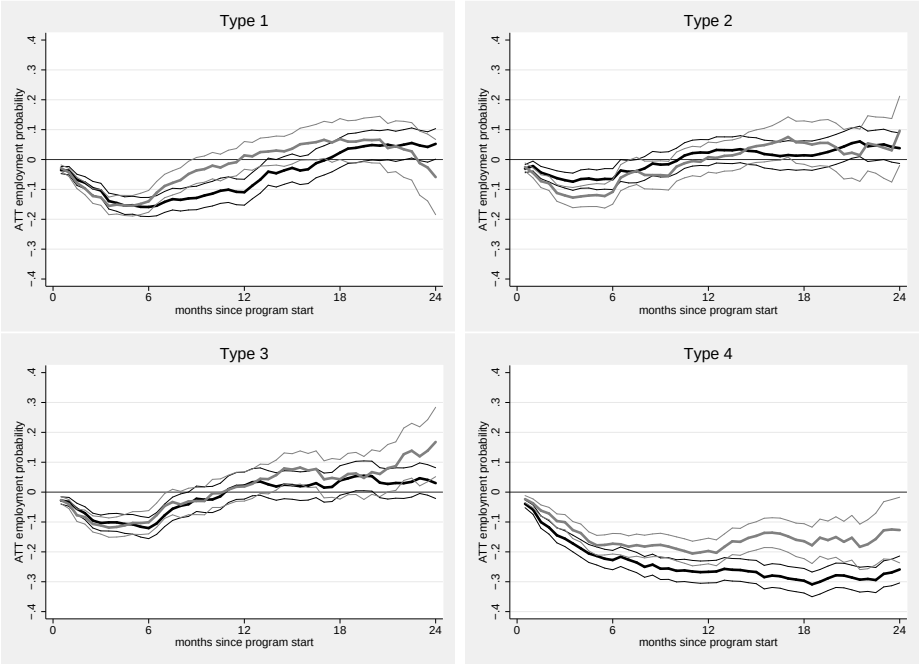
Figure 3: Actual Program Duration by Program Type.



Source: IEB, own calculations.

Note: Kaplan–Meier Estimates. Pre-reform period in black, post-reform period in gray.

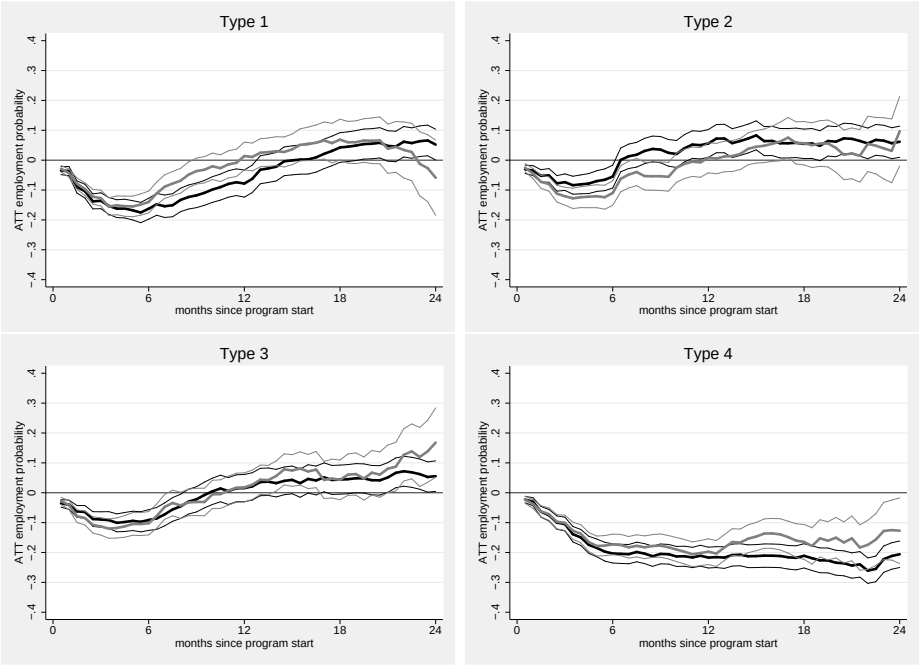
Figure 4: Reform Effect (RE).



Source: IEB, own calculations.

Note: Pre-reform period in black, post-reform period in gray. Thick lines refer to point estimates, thin lines indicate 95 percent confidence intervals.

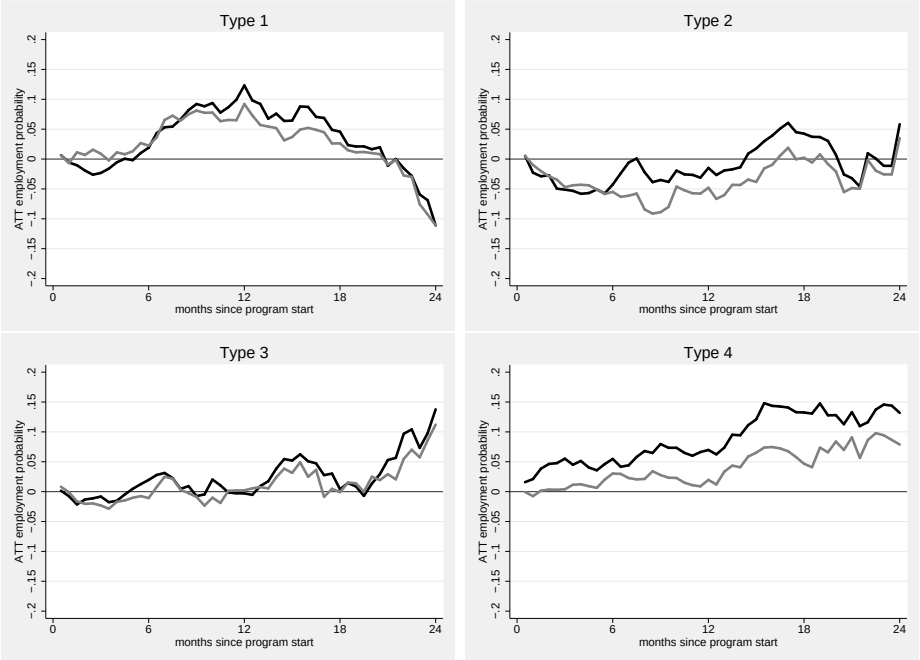
Figure 5: Voucher Effect (VE).



Source: IEB, own calculations.

Note: Pre-reform period in black, post-reform period in gray. Thick lines refer to point estimates, thin lines indicate 95 percent confidence intervals.

Figure 6: Decomposition (Reform Effect vs Voucher Effect).



Source: IEB, own calculations.

Note: Total reform effect (RE) in black, voucher effect (VE) in gray.

Table 1: Regression Analyses.

	Employment after 6 Months		Employment after 12 Months		Employment after 18 Months		Employment after 24 Months	
Type 1								
Participant	−.0944792***	−.1330146***	.0370334***	−.0173011	.1256139***	.0792834***	.1412618***	.1001083***
After	.0074511***	.0044053***	.0149896***	.0028990*	−.0628621***	−.0550741***	−.1549205***	−.1558419***
Participant × After	.0829654***	.0719106***	.0880213***	.0775458***	−.1096654***	−.1176722***	−.1813444***	−.1794058***
Vector X_i included	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.0002	0.1274	0.0005	0.1133	0.0065	0.1471	0.0510	0.1377
Type 2								
Participant	−.0274135**	−.0240299**	.0642153***	.0601559***	.0519593***	.0588800***	.0791948***	.0783010***
After	.0074511***	.0044202***	.0149896***	.0029059*	−.0628621***	−.0549131***	−.1549205***	−.1557870***
Participant × After	.0395961**	−.0108703	.0449538***	.0016311	−.0698128***	−.1173315***	−.1167093***	−.1484781***
Vector X_i included	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.0001	0.1274	0.0005	0.1134	0.0064	0.1469	0.0508	0.1375
Type 3								
Participant	−.0568398***	−.0887995***	.1034350***	.0599371***	.1288273***	.0912636***	.1253248***	.0939948***
After	.0074511***	.0043992***	.0149896***	.0029542*	−.0628621***	−.0550428***	−.1549205***	−.1557535***
Participant × After	.0531165***	.0348798**	.0331221*	.0138591	−.1001217***	−.1167416***	−.1612418***	−.1690856***
Vector X_i included	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.0001	0.1274	0.0006	0.1134	0.0065	0.1471	0.0509	0.1376
Type 4								
Participant	−.1424692***	−.1904686***	−.1417446***	−.2084322***	−.1449893***	−.1945977***	−.0636185***	−.1047193***
After	.0074511***	.0044053***	.0149896***	.0029089*	−.0628621***	−.0549982***	−.1549205***	−.1557639***
Participant × After	.0327283**	.0557146***	.0596982***	.0852465***	.0594455***	.0810859***	.0186380	.0333704**
Vector X_i included	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.0004	0.1276	0.0005	0.1133	0.0065	0.1469	0.0506	0.1373

Source: IEB, own calculations.
Note: *** significant at 1%, ** significant at 5%; * significant at 10%. Vector X_i contains similar variables as the matching specifications.