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ARE TRAINING PROGRAMS MORE EFFECTIVE WHEN UNEMPLOYMENT IS HIGH?

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Abstract (and nontechnical summary)

We estimate short-run, medium-run, and long-run individual labor market effects of training programs for the unemployed by following program participation on a monthly basis over a ten-year period. Since analyzing the effectiveness of training over such a long period is impossible with experimental data, we use an administrative database compiled for evaluating German training programs. Based on matching estimation adapted to address the various issues that arise in this particular context, we find a clear positive relation between the effectiveness of the programs and the unemployment rate over time.

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1 Introduction[1]

Although the body of knowledge about the effectiveness of training programs for the unemployed is rapidly growing, there is not much convincing evidence on the relationship between the effectiveness of the programs and the state of the economy. Such information is, however, important. If, for example, changes in the effectiveness of the policy or its different instruments are related to the business cycle at the time when decisions have to be made, then policymakers can react by adjusting the policy accordingly. Thus, the policymaker should be interested in knowing under which macroeconomic circumstances the programs are more or less beneficial. It is the goal of this paper to provide systematic insights on this issue.

The empirical literature on the effects of active labor market policies (ALMPs) suggests that almost all programs reduce (unsubsidized) employment and earnings in the short-run. This so-called lock-in effect is well documented in many studies and typically attributed to reduced search intensity of program participants or fewer job offers by caseworkers while participating in the program (e.g. van Ours, 2004). This lock-in effect is one of the (indirect) cost components of ALMPs. If it varies with labor market conditions, this would be an important argument for varying the composition of programs and program size over time. Our results show that this is indeed the case.

[1] The first author has further affiliations with ZEW, Mannheim, CEPR and PSI, London, IZA, Bonn and IAB, Nuremberg. Financial support from the Institut für Arbeitsmarkt- und Berufsforschung (IAB), Nuremberg, (project 6-531a) is gratefully acknowledged. The data originated from a joint effort with Annette Bergemann, Bernd Fitzenberger, Ruth Miquel, and Stefan Speckesser to make the administrative data accessible for research. The paper has been presented at workshops at the University of St. Gallen, 2006, at the IRP Summer Research conference in Madison, 2006, the 3rd Conference on evaluation of labour market programs at the ZEW, Mannheim, 2006, at the COST A23 meeting in Essen, 2007, at the IAB, Nuremberg, 2007, at the Annual Meeting of the Swiss Society of Economics and Statistics in St. Gallen, 2007, at the University of Rome Tor Vergata, 2007, Annual Meeting of the German Economic association in Munich, 2007, as well as the Annual Meeting of the Society of Labor Economists in Chicago, 2007. We thank participants, in particular John Ham, for helpful comments and suggestions. Additional background material for this paper is available in an internet appendix on our website *www.siaw.unisg.ch/lechner/lw_cycles*.

With respect to the medium-run to long-run effects, some wage subsidies and training programs increase employment and earnings (e.g., Couch, 1992; Hotz, Imbens, and Klerman, 2006, Winter-Ebmer, 2006, Jacobson, LaLonde, and Sullivan, 2005, Jespersen, Munch, and Skipper, 2008, Fitzenberger and Speckesser, 2007, Lechner, Miquel, and Wunsch, 2009). Most of this particular literature, which is more optimistic about the effectiveness of ALMPs than most of the older experimental literature, is based on large administrative data sources with long follow-up periods. Understanding the differences between short-run lock-in effects and medium-run to long-run effects that may capture more accurately the effects of the human capital added by the programs was an important step towards understanding how these programs work.[2] In fact, this difference will turn out to be crucial for the interpretation of our findings in this paper as well. However, none of these studies systematically investigates if and eventually why the effects of different types of programs change over time.[3]

The most closely related literature is Raaum, Torp, and Zhang (2002). They analyze how the effects of labor market training in Norway are related to post-training job opportunities. They exploit the fact that labor market conditions are different at different points in time after training was completed. They find a positive correlation. This is in line with the findings of the related literature on the effect of the state of the economy at labor market entry on the labor market performance of different cohorts (see in particular Raaum and Roed, 2006). However, these studies focus on labor market conditions after labor market training or secondary

2 The recent increase in evaluation studies is documented for example by the surveys of Heckman, LaLonde, and Smith (1999), Martin and Grubb (2001), Kluve and Schmidt (2002), and Kluve (2006). For examples of studies based on a selection on observables strategy, see Gerfin and Lechner (2002), van Ours (2004), or Sianesi (2004). A recent example of papers using instrumental variable types of assumptions is Frölich and Lechner (2006). The experimental literature is well documented in the survey by Heckman, LaLonde, and Smith (1999). Boone and van Ours (2004) provide and survey empirical evidence based on aggregated time series data.

3 Roed and Raaum (2003, 2006) and Richardson and Van den Berg (2006) include the relevant individual local or occupational unemployment rate as a determinant in a parametric specification of the training effect on the transition rate from unemployment to employment.

education, which are unobserved at the time when decisions about training assignment or investment in education have to be made. Thus, the results have little relevance from a policy perspective. Moreover, they do not take into account time variation in the composition of the participating cohorts, which can induce a spurious correlation that must be separated from the time variation in the effectiveness of the programs. Our study takes care of both issues.

There is also some evidence based on analyzing regional data over time. For example, Johansson (2001) uses variation in Swedish active labor market programs over municipalities. She shows that the effect of these programs is to prevent the unemployed from leaving the labor force during a downturn. She concludes that ALMPs are most effective during a downturn.[4]

An alternative to macro studies that come with the usual caveats of aggregation bias and policy endogeneity is to exploit the fact that different micro studies are conducted under different economic conditions. Meta-analyses are based on this idea. For example, Kluve (2006) combines more than 100 studies, and each study (or specification within a study) constitutes one data point. In a regression type approach he controls for different aspects of the methods and data used, features of the program, as well as the economic environment. Although the analysis of the latter is not the main thrust of his study, he finds the program effects to be somewhat larger when unemployment rates are higher. Thus, his results seem to be roughly in line with Johansson (2001). Although meta-analyses provide interesting summaries of the literature, there are problems as well. The different individual studies that are treated as the data of the meta-analyses are based on heterogeneous programs that are run in different institutional environments and economic conditions, and with different types of participants. It is obviously very challenging to control for all these background factors within

[4] This pattern of the programs leading to a redirection of the flows from unemployment to out-of-labor-force towards unemployment and then towards employment appears in the cross-sectional study by Lechner, Miquel, and Wunsch (2009), as well.

a regression framework using only a few control variables and tight functional forms dictated by the limited degrees of freedom available.

In this paper, we retain the advantage of the classical microeconometric evaluation studies, like nonparametric identification and heterogeneity of the program effects, but adjust the standard methodology to learn important lessons about the evolution of the effects over time. Since there are no experiments running for a sufficiently long period to be interesting for such an investigation, any such endeavor has to rely on observational data. Survey data, however, are typically problematic because of insufficient sample sizes, insufficient covariate and program information, short time windows to observe outcomes, misreporting, and attrition. Newly available high-quality administrative data can overcome these problems. Europe, where experiments are rare because of strong political resistance, has gained a comparative advantage in providing large and informative administrative databases that allow much richer analyses than experimental data which are usually used in the U.S.[5]

We exploit a particularly informative administrative micro data set for Germany that became available only recently. These data contain reliable information on participants (and non-participants) in different types of training programs on a monthly basis from 1986 to 1995. Information on labor market outcomes is available monthly from 1980 to 2003. Thus, the data allow us to investigate whether changes in labor market conditions influence the lock-in effects in a different way than the medium-run or long-run effects.

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[5] There are only few observational studies using U.S. data. None of the databases used are sufficiently informative in terms of covariates and the time horizon covered to study time variation of the effects of ALMP in sufficient detail (see in particular the survey by Heckman, LaLonde, and Smith, 1999, as well as Jacobson, LaLonde, and Sullivan, 2004, and Mueser, Troske, and Gorislavsky, 2007, for example). However, the study by Herbst (2008) investigates the dependence of the effects of welfare policies on the economic cycle using the US March Population Surveys over 20 years. He finds that social policies are more effective under good economic conditions. Although, that paper is similar in spirit to our analysis, it is based on less informative data and less robust, parametric estimation methods.

These data have been used recently in classical evaluation studies by Fitzenberger and Speckesser (2007), and Lechner, Miquel, and Wunsch (2009), among others. These studies argue that the data are informative enough to control for selective participation and thus allow identification of program effects by matching methods. Based on this identification strategy, we analyze the effects of training programs on short-run to long-run labor market outcomes for unemployed workers entering programs over 10 years on a monthly basis. Another advantage of using Germany for analyzing potential time variation in the effects of training is that no major changes occurred within the broad types of training programs considered in this paper, or in the institutional setup.

Our empirical strategy relies on different matching estimators. We begin by analyzing the evolution of the effects over time. Thus, in this specification the characteristics of participants and the use of different program types may vary over time. Any time pattern of the effects that we might isolate from this step may thus be due to changes in the composition of programs, of participants, and/or of economic conditions. Next, by modifying the matching estimator, we keep the characteristics of the program participants constant over time. Thus, the remaining dynamics in the effects reflect changes in program composition and economic conditions only. Then, additionally keeping the shares of the various subprograms and planned program durations constant allows us to isolate the effects of the economic environment. Finally, to improve our confidence in a causal interpretation of the strikingly clear pattern we obtain, the results are subjected to an intensive sensitivity analysis.

In line with the recent literature mentioned above, we consistently find negative lock-in effects as well as positive medium-run to long-run employment and earnings effects of the training programs in the 10-year period we consider. However, we detect considerable variation of those effects over time, even for the case of a fixed population of participants and a fixed composition of the programs. This variation is clearly related to the unemployment rate prevailing at the start of the program: The negative lock-in effects are smaller and the positive long-run effects are larger in times of higher unemployment. As argued above, this has important implications from a policy perceptive as policymakers can exploit this.

The remainder of the paper is organized as follows: Section 2 provides background information on the economic conditions, the unemployment insurance system, and the use of active labor market policies in West Germany in the relevant period. In Section 3, the data and the sample are outlined. Section 4 details the econometric identification and estimation strategy. In Section 5, we discuss in detail the effects of training over time. In the following section, we analyze how the changing characteristics of participants or the changing composition of programs over time may have influenced the effectiveness of training. Section 7 describes the results of our extensive sensitivity analyses. The last section concludes. An appendix contains further details on the data, on the definition of our sample and the outcome variables, as well as on the estimation procedure. A second appendix, available in the internet, contains detailed background material.

2 Economic conditions and institutions in West Germany

2.1 The West German economy between 1984 and 2003

During the economic slowdown following the second oil-price shock, unemployment in West Germany had risen to a quite persistent 9% in the mid-1980s.[6] Economic activity kept declining until 1988 when a slow recovery started. Directly after unification in 1990, West Germany experienced a boom with substantial East German spending diverted away from domestic products to previously unavailable West German goods. Accordingly, production and labor demand increased in West Germany. GDP grew 5.7% in 1990 and 5% in 1991. Registered unemployment declined to a rate of 6.3% in 1991 despite a significant growth of

[6] All numbers presented in this section are taken from official statistics published by the Federal Employment Agency, the Institute for Employment Research, and the Federal Statistical Office 1984-2004.

the labor force due to migration from East Germany and Eastern Europe. At the same time, the world economy was experiencing a recession. In 1992, this recession hit West Germany as well. Economic growth slowed down to 1.7%. One year later, the West German economy was deep in recession. GDP declined by 2.6% in 1993 and unemployment rose to 8%. With the recovery of the world economy in the late 1990s, the situation began to improve. GDP growth increased from 0.6% in 1996 to more than 3% in 2000. However, economic growth decelerated following the slowdown of the world economy after September 11, 2001, and registered unemployment returned to a level of more than 9% in 2003.

During the period 1984-2003, economic activity shifted especially from the primary and secondary sectors to the service sector. The structure of unemployment changed as well. The fraction of unemployed without any occupational qualification declined constantly from almost 50% in 1984 to 41% in 2003. The share of foreigners increased over time by about 4% to 17% in 2003 with a temporary dip during the post-unification boom. Long-term unemployment (one year or longer) has largely moved with total unemployment varying between 26% and 38% in the period 1984-2003.

As shown by Figure 1, expenditures on ALMPs, in particular on training, varied considerably over the years. However, they are only mildly correlated with GDP growth and unemployment (note the different scaling used for ALMP expenditures), because political considerations (e.g. upcoming elections in 1986, 1990, and 1998) and changes in the mix of ALMP instruments (1997, 2003) had strong impacts on ALMP expenditure. The fraction of it spent on training almost continuously increased from 33% in 1984 to almost 45% in 1998. It dropped slightly afterwards. In 2003, there was a large decline to 30% resulting from a regime change in the use of training from longer, more intense programs to short courses with less substantial adjustment of skills. The changes that occurred after 1995 are of limited interest to our empirical study, because we analyze programs that start between 1986 and 1995, only.

- Figure 1 about here -

2.2 Unemployment insurance in Germany 1986 to 1995

In Germany, unemployment insurance (UI) is compulsory for all employees with more than minor employment (i.e. earning more than about 315 € per month) as well as apprentices in vocational training.[7] German UI does not cover the self-employed. Persons who have contributed to the UI for at least 12 months within the three years preceding an unemployment spell are eligible for unemployment benefits (UB). The minimum UB entitlement is six months. The maximum claim increases stepwise with the total duration of the contributions in the seven years before becoming unemployed, and age, up to a maximum of 32 months at age 54 or above (requiring previous contributions of at least 64 months). Participation in government-sponsored training counts towards the contribution period for both the acquisition and the duration of UB claims. Actual payment of UB for eligible unemployed is conditional on active job search, regular appearances at the public employment service (PES), and participation in ALMP measures. Since 1994, the replacement rate is 67% of previous average net earnings from insured employment with dependent children, and 60% without. Before, replacement rates were 68% and 63%, respectively.

Until 2005, unemployed workers became eligible for unemployment assistance (UA) after exhaustion of UB. In contrast to UB, UA was means tested and potentially indefinite. However, like UB, UA was proportional to previous earnings but with lower replacement rates than UB.[8]

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[7] However, civil servants (Beamte), judges, professional soldiers, clergymen and some other groups of persons are exempted from contributions. For further details on the German UI and ALMP, see the comprehensive survey by Wunsch (2005).

[8] Before 1994, UA replacement rates were 58% (with children) and 56% (no children). Thereafter, they decreased to 57% and 53%.

Unemployed workers who were ineligible for UB and UA could receive social assistance, which was a fixed monthly payment unrelated to previous earnings, means tested and administered by local authorities.

For the following empirical analysis, it is important to note that except for the change in the UB/UA replacement rate, UI institutions were stable in the period 1986-1995.

2.3 German ALMP 1986 to 1995

ALMP has a long tradition in Germany. Among OECD countries, Germany's expenditure on ALMP is one of the highest (OECD, 2004). With increasing unemployment in the 1980s, the main objective of German ALMP shifted from keeping employment high and fostering economic growth towards reducing unemployment by increasing the employability of jobseekers. The main instruments traditionally used in German ALMP are counseling and job placement services, labor market training, subsidized employment, and support of self-employment.

Training has always been the most important program group in West Germany. It consists of heterogeneous instruments that differ in the form and intensity of the human capital investment as well as in their respective duration. Durations range from a few weeks to three years. Traditionally, German training courses have the aim of assessing, maintaining, or improving the occupational knowledge and skills of the participant, of adjusting skills to technological changes, of facilitating a career improvement, or even of awarding a first occupational qualification. So-called 'career improvement measures', for which the employed may also be eligible, had played a major role before unemployment rose in the 1980s. Since then they have become negligible as the focus shifted towards removal of the skill deficits and skill mismatch of the unemployed.

In the period under consideration, there are five types of training. Basic job-search assistance (JSA) existed only until 1992. So-called practice firms (PF) simulate - under realistic conditions - working in a specific occupation. Short training (ST) with a planned duration of up to six months, and long training (LT) with a planned duration of more than six months provide a general update or adjustment of skills. Retraining (RT) leads to an occupational qualification equivalent to a qualification obtained in the German apprenticeship system. JSA and PF have always been relatively small programs. ST and LT were by far the most important programs with LT gaining importance relative to ST. LT more than doubled its share in the period we consider. RT was relatively small as well, but became more important from the early 1990s on. However, given its long durations it is the most expensive program so its share in expenditure is substantially larger than its share among participants. Overall, the average direct costs of training varied between 2,000 and 4,000 EUR per participant in the period 1986-1995, with an average of about 3,000 EUR per participant. Thus, costs are quite large which mainly results from the relatively long durations of many German programs.

Access to training courses is largely limited to unemployed workers who are eligible for UB or UA. To underline the character of *further* job related training rather than primary occupational training, eligibility also required holding a first occupational qualification (before 1994, in addition 3 years of work experience) or at least three years of work experience (before 1994, six years). Usually, participants receive a transfer payment, which is called the maintenance allowance (MA). Since 1994, MA is of the same amount as UB. Before, MA had been somewhat higher than UB with a replacement rate of 73% with dependent children and 65% without. Moreover, the PES bears the direct cost of the program, and it may cover parts of additional expenses for childcare, transportation, and accommodation.

Note that with respect to eligibility and MA, replacement rates and training regulations have been relatively stable. Moreover, our data allow us to control for the few changes that actually occurred, especially with respect to the shifting emphasis on specific types of programs.

3 Data and sample definition

We use the same administrative data sources as Lechner, Miquel, and Wunsch (2009) which combine information from social insurance records on employment, data on benefit receipt during unemployment, and information on participation in training programs. The original data covers the period 1980-1997, but employment and unemployment records up to 2003 have been added to allow the construction of long-run outcome variables. The database is unique in several respects. In particular, it is much more informative than observational data that was previously available (e.g., see Jacobson, LaLonde, and Sullivan, 2005, for the US). It is the first micro database that allows the analysis of program participation over a sufficiently long period (10 years) on a monthly basis to capture business cycle movements. Moreover, it allows the construction of up to 24 years of individual employment histories on a monthly basis. In total, we have available between 6 and 15 years of pre-program history, as well as at least 8 years of post-program-start observations for the outcome variables. Detailed personal, regional, employer and earnings information of good quality allow us to control for all of the main factors that determine selection into programs (see the discussion in the next section) and to analyze precisely measured outcome variables of interest (in particular employment status and earnings). Table 1 provides the relevant details on the administrative data sources used.

For our analysis, we use a sample of participants in training and eligible non-participants. We focus on the prime-age part (age 20-55) of the West German labor force covered by social insurance.[9] All of the unemployed who start a program in a particular month in the period

[9] In particular, we only use persons who are observed at least once in employment subject to social insurance before a (potential) program start. We exclude persons who were last employed as home workers, apprentices,

1986-1995 (in total 120 months) are considered participants. In contrast, non-participants are all unemployed workers who do not start a program but receive UB/UA in that month.[10] To ensure that we do not use unemployed workers who completed a program shortly before the (potential) program start (and so are still in an earlier unemployment-participation-unemployment spell), we require that nobody participated in a program in the four years before the (potential) program start we consider. Moreover, to make sure that all persons we consider are eligible for participation, we require that they received UB/UA in the month before the (potential) program start.

- Table 1 about here -

Defining non-participants as those persons who do not start a program in a particular month is similar to the approach of Sianesi (2004) as the control group of 'non-participants' includes some future participants. An important difference, however, is that we do not stratify the sample by elapsed unemployment duration up to that month (but do match on this information), and that for non-participants we require that they did not participate in the 11 months following potential program start. The reason is that in contrast to Sianesi (2004) our parameter of interest is not the effect of starting a program at different points in the unemployment spell, but the effect of training relative to a well-defined and relatively stable non-treated comparison group, at different points in the business cycle. If we would choose the Sianesi (2004) approach, the comparison group would change immediately after the starting month considered depending on the fraction of non-participants who start a program in the next month(s), and a very different policy parameter would be estimated. Furthermore, the composition of such a control group would be affected directly by the business cycle as the probability of receiving a program in the next month clearly varies with the business cycle.

trainees, or part-time workers below half of the full-time equivalent, because we want to focus on the most common forms of regular employment.

[10] Note that as long as they fulfil all sample selection criteria individuals can be participants in one and non-participants in another month.

The price to pay for our approach is the possibility of some bias because we potentially condition on future outcomes (see Fredriksson and Johansson, 2003, 2008).

To obtain a sufficient number of participants we pool participants and non-participants over a six-month window in the estimation. The effect of starting a program in month t is estimated using all persons who start or do not start a program in one of the months t to $t+5$. Thus, we estimate effects for 115=120-5 different program starts in the period 1986-1995.

Since all the choices mentioned in this subsection may affect our estimation results, we perform an extensive sensitivity analysis, which is detailed in Section 7.

4. Econometrics

4.1 Effects of interest and identification

We are interested in the mean effects of participating in training in period t (θ_t) for some population of participants (P_t). Varying the latter in an interesting way will be one of the key issues in the following empirical sections. Based on the usual notation of the evaluation literature, we denote by Y_t^1 the potential outcome of participation in a program, by Y_t^0 the potential outcome of not participating in a program, and by Y_t the observed outcome. Thus, the mean of the effect of the policy for a member of the population of interest, P_t, is given by $\theta_t(P_t) = E(Y_t^1 | P_t) - E(Y_t^0 | P_t)$. P_t may or may not change over time.

Since participation and non-participation are not observable for the same individual, the issue of the identification of the effects arises. Below we argue that given the institutional setup, the newly created data are informative enough such that a selection on observed variables strategy (the conditional independence assumption, CIA) identifies the effects conditional on treatment status and covariates. In particular, we obtain expressions for the mean potential outcomes conditional on covariates that are functions of participation status, observed outcomes (Y_t), and covariates only:

$$E(Y_t^1 \mid D_t = 0, X_t = x_t) = E(Y_t \mid D_t = 1, X_t = x_t); \quad E(Y_t^0 \mid D_t = 1, X_t = x_t) = E(Y_t \mid D_t = 0, X_t = x_t) .$$

This equation must hold for all values of x_t that are of interest, i.e. those that affect both selection into the population of interest and the outcome. Given identification, under the usual assumptions a matching strategy identifies our parameters of interest, because

$$\theta_t(P_t) = \int E(Y_t \mid D_t = 1, X_t = x) f_{X_t|P_t}(x)dx - \int E(Y_t \mid D_t = 0, X_t = x) f_{X_t|P_t}(x)dx .$$

$f_{X_t|P_t}(x)$ denotes the distribution of X_t in the population P_t. In the next section, we call $f_{X_t|P_t}(x)$ the target population towards which the distributions of X_t for participants and non-participants are adjusted. An example of such a target population would be the participants in period *t*. In this case, we would estimate the classical average treatment effect on the treated (ATET).

For the identification strategy to be plausible, it is also important that in addition to the CIA the so-called stable unit value treatment assumption holds (SUTVA, Rubin, 1979). This assumption rules out so-called feedback or general equilibrium effects.[11] Such effects are particularly likely either when the program under consideration is sufficiently large to alter labor demand and supply relations, or when the intervention is likely to exhibit substantial displacement effects, as is argued for many public employment and wage subsidy schemes.[12] Here, we are concerned with training programs only. Although they are a very costly part of

[11] For the relevant literature discussing and 'solving' general equilibrium problems for the evaluation of public programs, see Calmfors (1994), Heckman, Lochner, and Taber (1998), Coady and Harris (2004), Lise, Seitz, and Smith (2009), and the references given in those papers. Since we measure outcomes at different points in time, SUTVA has to hold for the full time-path of the potential outcomes.

[12] However, the literature suggests that the displacement effects of such programs are rather small; see e.g. Davidson and Woodbury (1993), Blundell et al. (2004).

the German active labor market policy, in West Germany they appear not to be large enough to seriously alter demand and supply relations in relevant skill segments of the labor market.

4.2 Is the matching assumption plausible with our data?

In Germany, selection into programs is determined by three main factors: eligibility, selection by caseworkers and self-selection by potential participants. Eligibility is ensured by the construction of our sample (see Section 3). Caseworkers select participants based on individual employment prospects and corresponding skill deficits, chances for successful completion of a program and conditions in the local labor market. Variables capturing information about employment prospects and chances for successful completion of a program comprise age, educational attainment, marital status, presence of children and past performance in the labor market including information about firm size, earnings, position in job, specific occupation, and industry. Moreover, our data contain detailed regional information that allows us to control for local labor market conditions. In Internet Appendix IF we present various statistics which show that regional variation in economic and labor market conditions is quite large in West Germany. Moreover, there is a statistically significant correlation between the industry structure of a region and the regional unemployment rate (Table IF.1). Thus, it is important to control for these factors.

When constructing regional control variables we have to make sure that we do not (implicitly) condition on the development of the national labor market over time, because this would take out some of the effect we want to measure. We avoid dependence of the regional control variables on the national trend of interest by using deviations from the national mean at the time of measurement instead of the actual values of the respective regional indicators. The implicit assumption underlying this approach is that the deviations are unaffected by the overall time trend. Table IF.2 in Internet Appendix IF shows for the regional unemployment rates that the dispersion of the deviations increases somewhat with the national unemployment rate. Therefore, to further reduce the impact of the overall time trend we also include region dummies which are time independent by construction.

From the point of view of the unemployed, his decision whether or not to participate in a program is guided by considerations very similar to those of the caseworker. In addition, legislation provides rather strong incentives for individuals to participate. On the one hand, persons who refuse to participate, risk suspension of their benefits. On the other hand, periods during which individuals receive transfer payments while participating in a training program count towards acquisition of unemployment benefit claims. Therefore, we constructed variables from the (un-)employment histories that indicate the UB claim at the beginning and at the end of a spell.

Although this is much more information than usually available in studies that rely on the CIA (e.g. Heckman and Smith, 1999, Brodaty, Crépon, and Fougère, 2001, Larsson, 2003, Dorsett, 2005), there are some potentially important factors missing. In contrast to Gerfin and Lechner (2002) or Sianesi (2004), there is no information about the caseworker's direct assessment of the characteristics and prospects of the unemployed, for example with respect to motivation and ability. Moreover, we do not observe crime and health histories. For these variables, we rely on their indirect effects, i.e. on their effects on the employment and earnings history that materialized in the past. This is a reasonable approach given that our data allow us to reconstruct between 6 and 15 years of individual pre-program employment histories on a monthly basis including information on the stability and quality of employment.

To conclude, our data allow us to control - either directly or indirectly - for all of the main factors that determine selection into Germany's active labor market programs. In fact, since in most cases considered below we interpret only the changes in the effects over time, any violation of the conditional independence assumption that leads to a bias that does not change over time would not change the main conclusions in this paper. [13]

4.3 Estimation

Having established identification of the effects, the question of the appropriate estimator arises. All possible parametric, semi- and nonparametric estimators are (implicitly or explicitly) built on the principle that for every comparison of two programs and for every participant in one of those programs we need a comparison observation from the other program with the same characteristics regarding all factors that jointly influence selection and outcomes.[14] Here, we use propensity score matching estimators to produce such comparisons. An advantage of these estimators is that they are semi-parametric and that they allow arbitrary individual effect heterogeneity (see Heckman, LaLonde, and Smith, 1999; Imbens, 2004, provides an excellent survey of the recent advances in this field).

We use a matching procedure that incorporates the improvements suggested by Lechner, Miquel, and Wunsch (2009). These improvements aim at two issues: (i) To allow for higher precision when many 'good' comparison observations are available, they incorporate the idea of caliper or radius matching (e.g. Dehejia and Wahba, 2002) into the standard (nearest-neighbor) algorithm. (ii) Furthermore, matching quality is increased by exploiting the fact that appropriately weighted regressions that use the sampling weights from matching have the so-called double robustness property. This property implies that the estimator remains consistent if either the matching step is based on a correctly specified selection model, or the regression model is correctly specified (see in particular Scharfstein, Rotnitzky and Robins, 1999, Robins, 2000, Bang and Robins (2005) as well as Rubin, 1979, and Joffe, Ten, Have, Feldman, and Kimmel, 2004). Moreover, this procedure should reduce the small sample bias as well as

[13] See also Lechner, Miquel, and Wunsch (2009) and Fitzenberger and Speckesser (2007), who justify a selection-on-observables strategy based on the same data as well.

[14] Of course, parametric models may construct such a group artificially outside the support of the data.

the asymptotic bias of matching estimators (see Abadie and Imbens, 2006, 2008) and thus increase the robustness of the estimator. The actual matching protocol is shown in Table B.1 in the appendix, which also contains the technical information about the estimator.

We use the fixed-weight standard error estimator proposed by Lechner, Miquel, and Wunsch (2009). It is the same as the one suggested by Lechner (2001) and applied in Gerfin and Lechner (2002) except that heteroscedasticity is allowed for. See the Internet Appendix IE for the motivation and all the details for this variance estimator, which shows some resemblance to the estimator suggested by Abadie and Imbens (2009).

5 The program effects over time

According to German legislation, the most important objectives of ALMPs are to increase reemployment chances and to reduce the probability to remain unemployed. Therefore, we use outcome variables related to the employment status, in particular registered unemployment, and employment subject to social insurance.[15] We also consider gross earnings as a crude measure for individual productivity. All effects are measured from the month of the (potential) program start. Focusing on the beginning instead of the end of the programs accounts for the potential endogeneity of actual program durations and allows the detection of potential negative lock-in effects of the programs. We consider a program most successful if everybody would leave for employment immediately after starting participation. Whenever a person participates in a program, he is considered as registered unemployed (and not employed).

We estimate the effects for different times after the start of the program for a better understanding of their dynamics. We expect the programs to begin with a negative lock-in effect before the effect reaches its long-run level. The lock-in effect is approximated by the effect after 6 months. The long-run effect is approximated by the effect after 8 years. We also esti-

[15] 'Registered unemployment' is defined as receipt of UB or UA or participation in training.

mate the effects 3 and 6 years after program start, but the effects appear not to change too much after 3 years.[16]

We also consider the cumulated outcomes of training from its beginning to the respective point of measurement, i.e. we add the monthly outcomes up to this point. This provides something like a net effect that trades off negative lock-in effects against potential positive long-run effects, which gives a summary of the overall effect. Due to a lack of reliable cost information, an exact cost-benefit analysis is not possible with the available data.

Figure 2 shows the short-run and long-run effects of training for each starting month in the period January 1986 to July 1995. We find that after 6 months, programs increase the unemployment probability by about 25 percentage-points for participants, and, correspondingly, reduce the employment probability by about 15 percentage-points. In the long run, employment is increased by about 10 percentage-points, but any effect on unemployment is hard to spot (if there is any, then unemployment is increased). Thus, the program effect operates by increasing employment at the expense of the share of individuals leaving the labor force rather than reducing registered unemployment.[17] Considering the effects on earnings (non-employment is counted as zero), we find similar effects with an average long-run monthly earnings gain of about 100 EUR. Although all effects show considerable variation over time, it is hard to spot any relation with the unemployment rate, which is shown in Figure 2 as well (for clarity, it is presented net of its mean over the 115 months presented in the table).

- Figure 2 about here -

[16] Choosing one particular month only may be a noisy measurement of these effects. Therefore, we calculate the short-run effects as the mean of months 5-7, the medium-run effects as the mean of months 33-39, the long-run effects after six years as the mean of months 61-72 and the long-run effects after eight years as the mean of months 85-96.

[17] These findings are largely consistent with the studies analyzing the effects of post-1992 training programs with these data (i.e. Fitzenberger and Speckesser, 2007, Fitzenberger, Osikominu and Völter, 2006, and Lechner, Miquel, Wunsch, 2005; note the different definitions of participation and non-participation in these studies).

Figure 3 shows the estimates for the mean of the potential outcome variable employment that underlies the corresponding effect estimates in Figure 2. The short-run outcomes show a clear seasonal effect (at least for the first 8 years), whereas, not surprisingly, such a relationship does not appear for the long-run effects.

- Figure 3 about here -

Finally, Figure 4 shows the cumulated effects in months of (un-)employment over time. They imply that the total negative effect in the first 6 months after program start corresponds to a reduction of about 1.5 months of employment as well as an additional month of unemployment. In the long run, there appears to be a gain of about 4 to 6 months of employment and an additional 4 to 6 months of unemployment (!), which again suggests that the programs reduce the share of people leaving the labor force drastically.[18] Comparing the cumulated effects with a particular point-in-time estimate after treatment, we find very similar shapes of the effects over time, although obviously the magnitudes and sampling uncertainty differs.

- Figure 4 about here -

The next step is to condense the dynamic information about the effects and examine their correlation with indicators for the state of the economy more thoroughly. In Table 2 we display the correlation of the effects presented, including earnings, as well as the effects measured 3 and 6 years after program start, with the quarterly GDP growth rate, the monthly national unemployment rate, and the monthly number of participants in training programs. These correlations are obtained from a regression of the respective estimated impact on a constant term and the macroeconomic variable of interest.[19] The internet appendix presents

[18] This seems to be mainly an eligibility effect (program participation extends UB eligibility) rather than an effect for groups with lower labor market participation rates (we find, for example, no significant gender differences).

[19] The significance levels of the correlations are obtained from a bivariate regression of the effects on a constant and the respective macroeconomic indicator using the Newey-West procedure to correct for the correlation of

correlations of the unemployment rate with the estimated means of the potential outcomes as well. Since we are interested in indicators that can be used by policymakers when decisions are to be made, we focus on their values at program start (rather than, for example, at outcome measurement).[20]

The results suggest that the programs are more effective when unemployment is higher at the time when the program starts. This positive dependence of the program effect on the unemployment rate is somewhat larger for the long-run effects than for the lock-in effects. If these correlations have a causal interpretation, their magnitudes imply, for example, that on average the employment effect of the programs increases by about 0.7-1.8 percentage-points when the national unemployment rate is increased by 1 percentage-point (depending on the point in time after program start when the outcome is measured). The quarterly GDP figures appear to be too rough to detect any correlation. Similarly, no systematic correlation can be detected with indicators of program size, such as the number of participants.

- Table 2 about here -

In the remainder of the paper, we will try to gain more insights on why there is such a positive correlation between the effectiveness of the programs and labor market conditions as characterized by the monthly national unemployment rate at program start.

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the program effects over time. The significance level presented relates to a two-sided t-test of the null hypothesis that the coefficient on the unemployment rate is zero.

[20] In the original discussion paper version of this paper, we also look at the correlation with the national monthly unemployment rate at the time after program start when the different outcomes are measured. In line with Raaum, Torp, and Zhang (2002) we find a strong and significant negative correlation with the employment and earnings effects of training.

6 The changing composition of program participants and programs

6.1 Participants

The key question raised by the relationship between the effects and the state of the labor market documented in the preceding section is whether these correlations reflect the fact that the same programs have different effects (different production functions) depending on the state of the economy or whether the correlations are spurious. A spurious correlation could be induced by some other background factor moving the effects in a similar direction as the unemployment rate. Therefore, it is important to 'eliminate' other potentially important factors that change over time, and affect program effectiveness.

The first such potential factor relates to the dependence of the pool of potential participants (from which the actual participants are selected) on the state of the economy. In a recession, there might be excess supply of unemployed who would benefit from the programs. When the economy recovers fewer of them would be available, but program places still have to be filled (for example because there is a rigidity in the adjustment of the supply of courses due to long-run contracts between the PES and suppliers). Figures IA.1 and IA.2 in the internet appendix show the changes of the composition of participants and non-participants over time for some selected characteristics. We see that both groups change, and that they change in a similar fashion. In particular, the share of women, the employment histories and the education levels fluctuate, whereas the share of foreigners increases more or less continuously.[21]

The impression that the change in the characteristics of participants over time merely reflects changes in the supply of unemployed is reinforced as well by looking at the monthly probit models for program participation that do not show any large difference in the

[21] See, e.g., Black, Smith, Plesca, and Plourde (2003) for US evidence on increasing variation in the characteristics of unemployment insurance claimants as the unemployment rate increases.

conditional selection model over time. Almost two thirds of the coefficients have the same sign in all 115 periods when significant (see also Table IB.1 in the internet appendix).

A key question that remains is whether these changes in the composition of program participants are also correlated with the situation in the labor market. Table 3 shows that this is indeed the case. Keeping in mind that current unemployment rates are likely to be negatively correlated with average unemployment rates in the last six years, the negative correlation between past unemployment and the positive correlation with past employment is expected.[22] However, the relative participation of women, foreigners, and unemployed workers with lower education is also lower during times of higher unemployment.

- Table 3 about here -

To the extent that there is effect heterogeneity, a fact that is documented in numerous evaluation studies (for West Germany, e.g., Lechner, Miquel, and Wunsch, 2009), such systematic relationships between the state of the labor market and the characteristics of participants might influence the correlation with the effects as well. Therefore, we re-estimate the effects of the training programs for a fixed population of participants. Month by month, we match participants as well as non-participants with respect to that target distribution to obtain estimates of the potential outcomes of both participation and non-participation for a population that resembles the chosen target. The average treatment effect on the target population is then obtained by subtracting these estimated potential outcomes. Since the target distribution is the same for all periods, characteristics of the participants are held constant in the estimation of the effects of training.[23]

[22] The higher unemployment was in the past, the more likely it is that cumulated individual unemployment is high and, at the same time, that the current unemployment rate is low.

[23] Note again that in the matching we carefully avoid dependency of the matching variables on time or a function of it, which would pick up some of the effect of time-varying labor market conditions we are interested in.

- Figure 5 about here -

The target population is defined as having the average characteristics of the intersection of all common supports between the overall population of participants in the period 1986-1995 and the period-participants over time (based on the estimated propensity scores). That is, more technically speaking, we define a target population of participants with comparable participants and non-participants in all months.[24] It is important to note that the strict support criterion is necessary here to ensure that the population for which we estimate the effects really does not vary over time. Moreover, in our setting this population does not have to fulfill representativeness because it is only a device to keep participant characteristics constant over time. It can be chosen arbitrarily (as long as it is of some interest from a policy point of view). Also note that, when estimating the potential participation outcome for a given period by matching the participants of that period to the reference population, no participants of that period are dropped when implementing the common support because they are the comparison group in this case (they only might get zero weight). We examine the sensitivity of our results to allowing for less strict support criteria in Section 7.4.

- Table 4 about here -

Albeit somewhat larger, the results in Figure 5 are similar to those for the specification that allows the characteristics of the participants to vary over time. This is particularly so when we take into account that sampling uncertainty is somewhat larger due to a reduced sample size resulting from the far more restrictive common support requirement. Checking the correlation of the effects that results from this specification with different indicators of the macroeconomic situation, it turns out that, if anything changes, the correlations increase. Table 4 shows the exact values of those correlations for the unemployment rates and selected outcome variables. For better comparability, in the second column of Table 4 we show the results of the

[24] We exclude observations with a propensity score higher than the maximum in the comparison groups. Out of 9418 participants in the reference population, 2101 (22%) fulfill this criterion.

specification with varying participant characteristics when the same strict common support over time is used for the participants as for the reference population. We obtain similar results.

6.2 Programs

Figure IA.3 in the internet appendix shows that other import factors that change over time are the composition of the training policy and average planned durations of the training courses.[25]

Long training increases over time, whereas the job search assistance programs were terminated after 1992. The shares of the other program groups fluctuate in an unsystematic matter. Similarly, the planned program duration of all participants fluctuates considerably. It reaches its peak of more than 12 months for programs beginning in the second part of 1993, when the rather long retraining courses were used extensively. The lowest level of about 6 months appears in 1986, when short training was most important.

As before, the key question is whether these changes are related to labor market conditions as well. Table 5 shows the correlation of those variables with the unemployment rate at program start. These results suggest that this correlation exists, at least for short and long training (positive) and job search assistance (negative). This finding holds for all participants as well as those used in the previous section.

- Table 5 about here -

In the next step, the effects of changing program shares and planned durations over time is eliminated by keeping the characteristics of participants (as in the previous section) as well as the program shares and planned program durations constant over time. This is implemented

[25] This figure is based on the participants used in Section 5. The plot for the target population defined in Section 6.1 is very similar and therefore relegated to the internet appendix. That appendix also shows a plot of the program type specific planned durations.

by adding the type of program as well as its planned duration to the set of control and matching variables when we match the program participants of the particular period to the target population of program participants. Obviously, nothing changes for non-participants. Figure 6 shows the results. They are based on a population of participants with an average duration of programs of 9.4 months (standard deviation is 7.5 months). 46% of those participants take part in short training, 34% in long training, and 20% in retraining. Participants receiving job search assistance are omitted because the program is terminated after 1992. Since participants in job search assistance are no longer part of the reference population to which participants and non-participants are matched, they are removed from the sample and estimates of the effects for this program do not appear in this figure.

- Figure 6 about here -

- Table 6 about here -

Although the effects seem to be somewhat larger than in the previous specifications, these changes are within a range that could be attributed to sampling error. Analyzing the correlations of the effects with the unemployment rate (Table 6), we find that, at least for employment, the correlations increase further compared to the previous two specifications. Overall, we conclude that keeping the characteristics of the participants and the training policy constant reaffirms the findings of the previous section that programs are more effective when unemployment is high.

7 Sensitivity analyses

7.1 Seasonal patterns

Looking at the effect estimates in the various specifications presented before may suggest that the correlation with the unemployment rate is merely a reflection of some seasonal variation, instead of a more long-term macroeconomic trend. To understand whether this may be a valid interpretation, we analyze the seasonal pattern of the effects directly.

- Table 7 about here -

We control for seasonal differences in the effects by including, first, four dummies for the quarter in the regressions (which we run to obtain autocorrelation-robust standard errors for the correlations presented in the table), and second, we add interactions of these with a post-unification dummy.[26] The results are displayed in Table 7. This table shows that the correlations remain almost unchanged compared with the baseline estimates presented in Table 6. Therefore, seasonal correlation does not explain the detected correlation patterns between the effects and labor market conditions.

7.2 Regional variation

If it is true that the effectiveness of the training programs increases with unemployment, then one should expect that programs are more effective in regions with higher unemployment than in regions with lower unemployment. As a first way to investigate this, we repeat the analysis for high and low unemployment regions. Since splitting the sample increases the noise in our estimates considerably, we choose an overlapping split (60% of all unemployed facing the lowest regional unemployment rates in a specific period versus 60% of those unemployed facing the highest ones).[27]

The corresponding results are shown by Figures ID.8 and ID.9 in the internet appendix. Again, it is difficult to spot the difference between the two plots. In other words, our test has

[26] In the baseline setup, we only include a constant and the unemployment rate. Here, to capture seasonality effects, we replace the constant by four dummies for the quarters February-April, May-July, August-October, and November-January. We do not use dummies for the months, because this would inflate the number of regressors too much, given that we only have 115 observations.

[27] We used a classification in terms of the deviation of the local unemployment rate from the national unemployment rate in the period under consideration (at program start) to rule out conditioning on the business cycle.

not much power, because splitting the sample led to increased variability of the estimates. However, when we consider the mean effects over time we find that the long-term effects are indeed lower in regions with lower unemployment, albeit only by about 1%-point for employment. Moreover, columns 2 and 3 in Table 8 show that within the two types of regions, the effects are again positively correlated with the development of unemployment over time.

- Table 8 about here -

Just comparing the effects in regions with low and high unemployment is not very powerful, because many aspects of the labor market that might influence program effects, like the industry structure and the characteristics of the participants differ across local labor market. To overcome this drawback we mimic the strategy applied to examine the correlation between the effects and labor market conditions over time by estimating regional program effects and relating them to the average regional unemployment rate. In particular, we estimate the effects of training separately for 36 regions (without holding participant characteristics constant)[28] as well as the correlation of these effects with the corresponding average (1986-1995) regional unemployment rate. The results are displayed in the last column of Table 8. In most cases, in particular for the outcome unemployment, the correlations have the expected sign that indicates a positive relation between regional unemployment and the program effects. However, the link is much weaker than for labor market conditions over time. This is possibly due to the reduced precision (only 36 observations) as well as to the potential mobility of jobseekers across regions,[29] which is of course not possible across time.

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[28] Since our target population used to hold participant characteristics constant includes individuals from different regions, we cannot estimate the effects for this population when we are interested in regional effects.

[29] However, note that inter-regional mobility in West Germany is relatively low: 3.7% of persons employed in April 1996 worked more than 50 km away from their home and 4.2% commuted across federal state borders. The numbers for 2000 and 2004 are very similar. See Statistisches Bundesamt (2005).

7.3 *Stability of the correlation between the effects and unemployment over time*

There may be a concern that the relation between unemployment and program effects is different before and after German reunification. Therefore, we repeat our correlation analysis for the first and second half of the ten-year period to see whether the correlations between the effects of the programs and the labor market conditions remain constant before and after German reunification.

- Table 9 about here -

When splitting the sample in October 1990, the findings shown in Table 9 confirm again that short and long-term employment and earnings impacts are positively related to the unemployment rate. However, one of the measures for medium-run outcomes (3 years after program start) is large and significant in the first period, whereas the correlation for the other long-run outcome (8 years after program start) is large and significant in the second period. We conjecture that this results from the additional sampling uncertainty coming from reducing the sample by half. When estimating the pre- and post-unification correlation by including an interaction term of the unemployment rate with a corresponding period-dummy in the regression, the results are very similar to the baseline case, independent of whether seasonal dummies (quarters as in Section 7.1) are included or not (see the last four columns of Table 9).

7.4 *Further sensitivity checks*

This section summarizes further checks to improve the credibility of our key result that the effects of the training programs are positively correlated with the unemployment rate over time. For the sake of brevity, all the details are relegated to the internet appendix (Section ID).

Before discussing the different checks, the reader should be aware of the limitations of the data. Given that we are interested in the dynamic evolution of the effects in relation to the starting dates of the program, the sample sizes for participants quickly become too small to have enough precision to detect individual heterogeneity. For example, it would be interesting to investigate the correlations of the effects of the different types of programs with the labor market conditions. Clearly, there are not enough observations for practice firms, retraining and job search assistance, but even the estimates for the larger groups of short and long training courses are too noisy to allow any firm conclusions. In a similar vein, it is not possible to investigate the issues of participant subgroup heterogeneity much further.[30]

A crucial issue that comes up in our implementation is how observations are aggregated over time. For each month, the results above are based on the participants and non-participants in that month and the next five months. For a sensitivity check that window is, first, reduced to four months and, second, increased to nine months. The results are detailed in the internet appendix. Qualitatively, the results do not change, but, again, in the first case sample size becomes an issue. Thus, in the first case the precision of the estimated coefficients is reduced whereas in the second case precision increases. With respect to the correlation of the effects with the unemployment rate over time, we find somewhat smaller (larger) correlations when the pooling window is reduced (increased) but the overall conclusions do not change.

Another important issue is how a non-participant is defined. The papers by Fredriksson and Johansson (2003, 2008) and Sianesi (2004) provide an extensive discussion of the problems that can arise for different definitions of non-participation. In the results presented above, nonparticipants are required not to participate for the 12 months following and including their potential program start. We checked the sensitivity of our results by requiring six (24) months instead which reduces (increases) potential bias but makes non-participants in

[30] We estimated the specification with constant participants and programs separately for men and women. The correlations are somewhat smaller for women than for men. However, because of small sample sizes the estimates are too noisy to draw any conclusions.

a particular month more (less) similar to participants. In both cases, we find very similar results to the ones presented above.

A related issue is that future participation rates of non-participants might be related to the business cycle. We find that future participation rates for both participants and non-participants are decreasing over the ten-year period we consider and that they are uncorrelated with the unemployment rate. Thus, the correlation of the program effects with the unemployment rate we find is not due to differential future program participation of non-participants over the business cycle.

Furthermore, the fact that we require all persons not to have participated in a program in the 48 months before (potential) program start might affect our results. For the most important specification with stable population and program characteristics, this choice does not matter at all since the common support of the reference population we choose only includes persons who have never participated in a program before.

The construction of the target population that kept all characteristics constant required a substantial reduction in the target population in order to ensure that the same type of participants is observable in all periods. To examine the sensitivity of our results with respect to that reduction, we use less strict criteria for the common support. In particular, we use definitions of the 'common' support that allow for specific non-overlap by extending the bounds of the support by 5%, 10%, and 20% of the standard deviation of the propensity score. This induces possibly some bias as for some members of the target population there are no similar treated and controls, which in turns implies that participant characteristics do vary to some extent over time even if we match all groups to the target population. However, the results remain very similar.

The estimation of and inference for the correlations may be questioned. Using regression-based inference based on the Newey-West t-values should take care of any autocorrelation and heteroscedasticity that is inherent in the effects (e.g. by construction of the moving 6-months defining participation). In addition, the dependent variable in that regression is estimated and thus mismeasured. Since it is consistently estimated, this type of measurement error in the dependent variable should not induce bias. Nevertheless, as a sensitivity check we estimated a weighted regression in which the weights are proportional to the precision of the effects. Again, the results confirm our findings.

It remains to check the sensitivity of the results with respect to some operational characteristics of the chosen matching estimator, like the bias correction procedure or the choice of the caliper width. Such checks have been extensively performed and documented by Lechner, Miquel, and Wunsch (2009) who use an identical estimator, but apply it only at one point in time. The reader is referred to their results that indicate a low sensitivity of the estimator with respect to modest changes in these parameters.

8 Conclusions

We analyze the effects of training programs for the unemployed over a ten-year period based on newly available very informative German administrative data. We generally find negative lock-in effects as well as positive medium to long-run employment and earnings effects of the training programs. Moreover, the cumulated effects that add up periods of employment and earnings over time, thus trading off negative short and positive long-run effects, suggest that there is a net gain in employment and earnings 8 years after program start.

We also detect considerable variation in the effects over time. This variation remains even when we artificially (econometrically) keep the characteristics of participants and the composition of the programs (both of which show considerable variation) constant over time. We find that this variation is related to the level of unemployment at the start of the program, in the sense that the negative lock-in effects are larger in times of low unemployment and the positive long-run effects are larger in times of high unemployment.

At least for the first part of this finding the explanation appears to be obvious. The negative lock-in effects occur because, while in the program, the unemployed show reduced job search effort and receive fewer job offers from the caseworkers who want to recoup their investment in the participants' human capital. However, participants have an incentive to stay in the program because the net gain of the program is likely to be positive and because of the possibility to extend their unemployment benefit claim by staying in the program. Therefore, unemployed not 'locked-in' in a program find jobs faster. When unemployment is high, it takes longer to find a job. Hence, the cost of reduced job search because of attending a program is lower. Since this affects the current participants in the program, we expect the lock-in effect to worsen when the labor market situation improves.

For the long-run effects, it is not so obvious why this correlation between effects and labor market conditions exists. One possible explanation is that the negative lock-in effects reduce future job finding probabilities because of worsened employment histories, although this effect is dominated by the positive effects of the additional human capital received in the programs. Thus, even if the human capital effect is more or less unrelated to labor market conditions, the correlation of the lock-in effect is sufficient to induce the same correlation in the medium and long-run effects as found for the lock-in effects. An additional explanation is, however, that in times of high unemployment non-participants are not only less likely to find a job match but also more likely to find a worse match than in times of low unemployment, and that this has a persistent effect on labor market outcomes, e.g. because of shorter job durations and recurring unemployment. In contrast, program participants avoid unfavorable job matches because by the time they have completed their program, labor market conditions have improved relative to the ones faced earlier by non-participants.

In conclusion, our results suggest that when the economy picks up and unemployment falls, one may want to reduce the volume of training programs by more than the proportional reduction in unemployment would suggest. On the other hand, when labor market conditions worsen, then the share of unemployed in the programs might be increased.

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Appendix A: Further details on the data

A.1 Sample sizes of participants

Sample sizes decrease when keeping first the characteristics of participants and then the composition of programs constant over time due to enforcing common support with our reference population (see A.5 for details). The minimum is about 200 participants pooled over 6 months. The average is about 400 participants. While these numbers are sufficient for many interesting analyses, it is clear that insights from further disaggregation of the data are limited.

- Figure A.1 about here -

A.2 Characteristics of the target population

The following table provides descriptive statistics for selected characteristics of the target population we use to keep the composition of participants and programs constant over time. As target population, we chose the pool of all training participants in our sample in the period 1986-1995, reduced to the intersection of the common support in all starting months.

- Table A.1 about here -

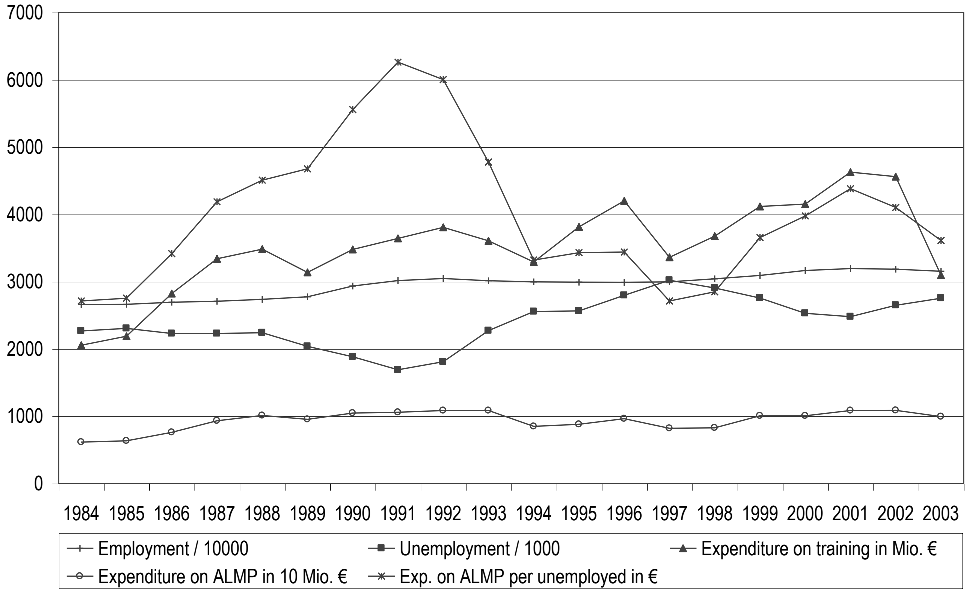
Appendix B: Technical details of the matching estimator used

- Table B.1 about here -

The parameter used to define the radius for the distance-weighted radius matching (*R*) is set to 90%. This value refers to the distance of the worst match in a one-to-one matching based on the propensity score. Different values for *R* are checked in the sensitivity analysis in Lechner, Miquel, and Wunsch (2009). The results were robust as long as *R* did not become 'too large'.

Figures

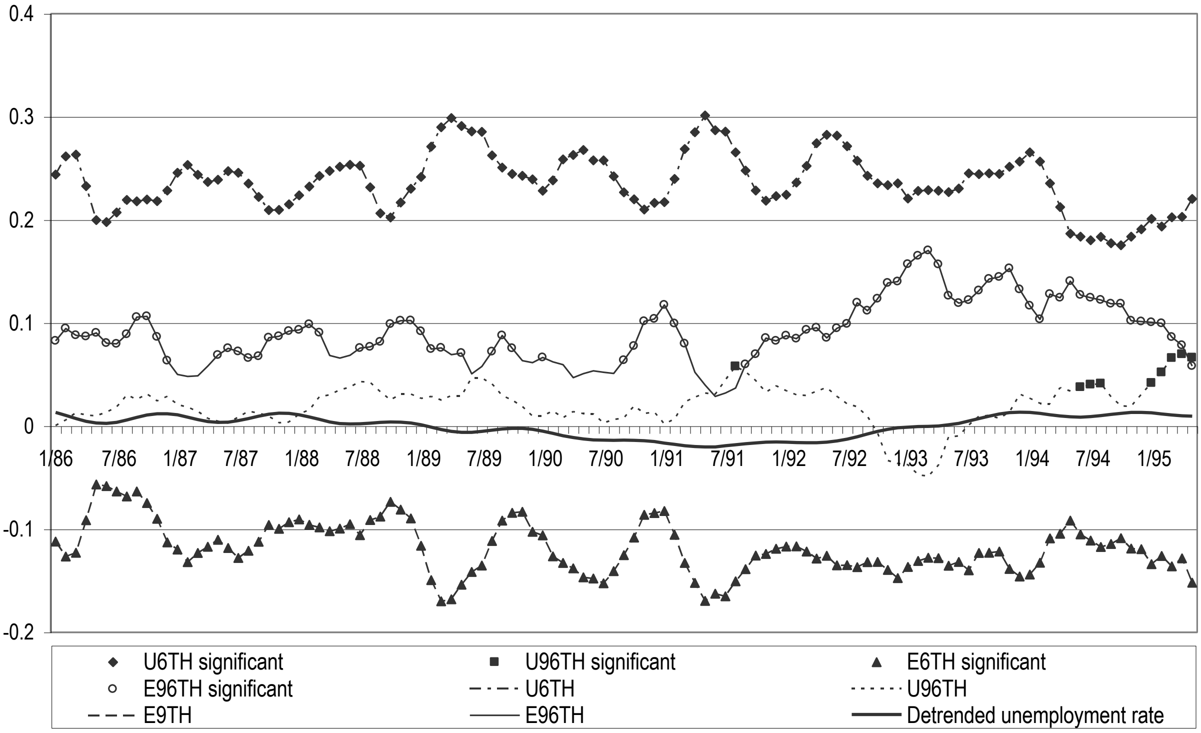
Figure 1: Selected indicators for business cycle movements in West Germany



Sources: Official statistics published by the Federal Employment Agency, the Institute for Employment Research and the Federal Statistical Office.

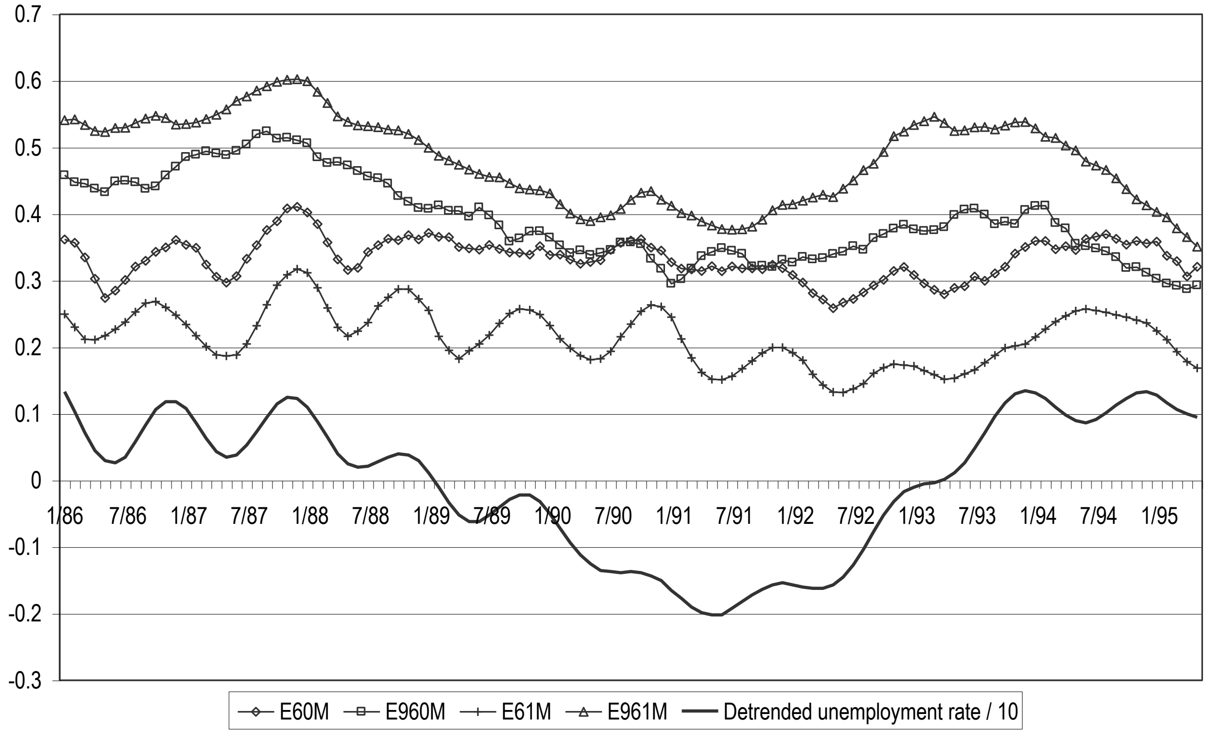
Note: The employment rate is calculated as employment plus unemployment as a percentage of the labor force (potential employment). Expenditure on active labor market policies (ALMP) and training are at 1995 prices.

Figure 2: Effect of training on the employment and unemployment probabilities of participants



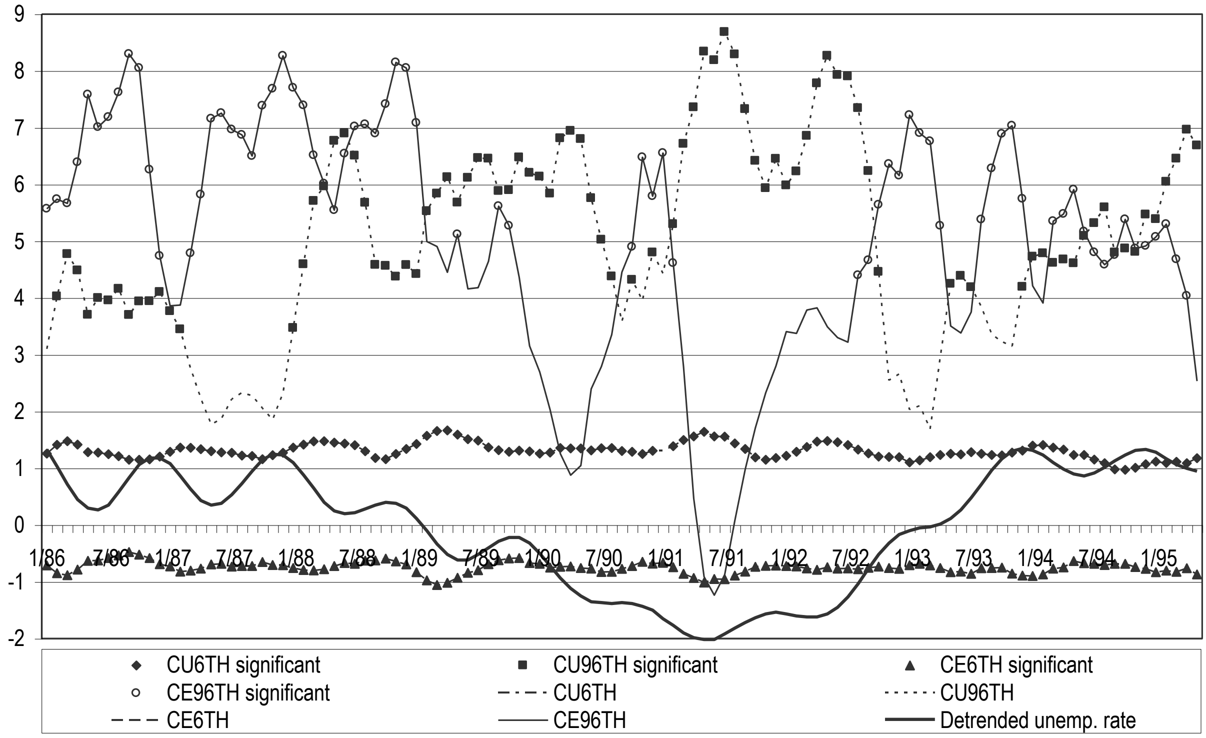
Note: The outcome variables are named as follows: U: unemployment, E: employment, 6 or 96: month after program start, TH: theta (average treatment effect on the treated). For each outcome variable, symbols appear if the effect is significant at the 5% level in a particular month. The unemployment rate is presented net of its mean 1986-1995. All effects are smoothed using three-month moving averages. 300-730 observations per period (pooled).

Figure 3: Mean employment rates given participation and non-participation for participants



Note: The outcome variables are named as follows: E: employment, 6 or 96: month after program start, 0: non-participation, 1: participation, M: mean level. 300-730 observations per period (pooled). See also note below Figure 2.

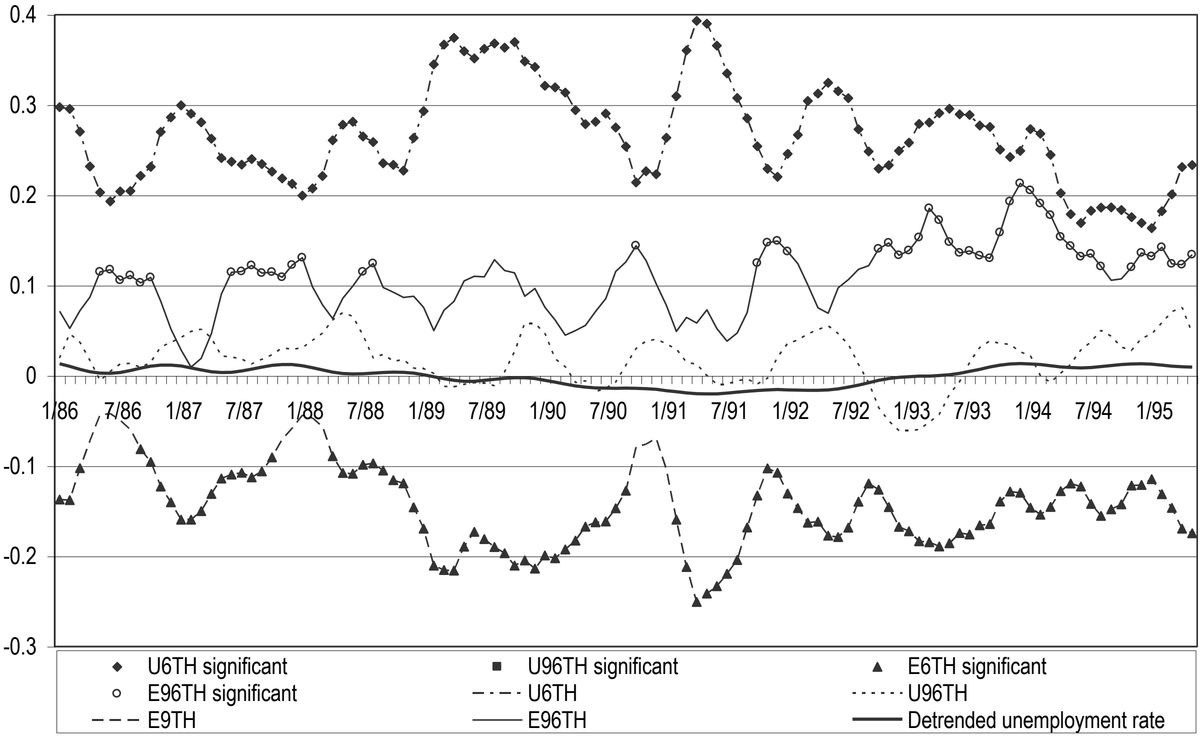
Figure 4: Cumulated effects of training on the employment and unemployment probabilities of participants (in months)



Note: The outcome variables are named as follows: CU: cumulated unemployment, CE: cumulated employment, 6 or 96: month after program start, TH: theta (average treatment effect on the treated). 300-730 observations per period (pooled). See also note below Figure 2.

Figure 5: Effect of training on the employment and unemployment probabilities of participants

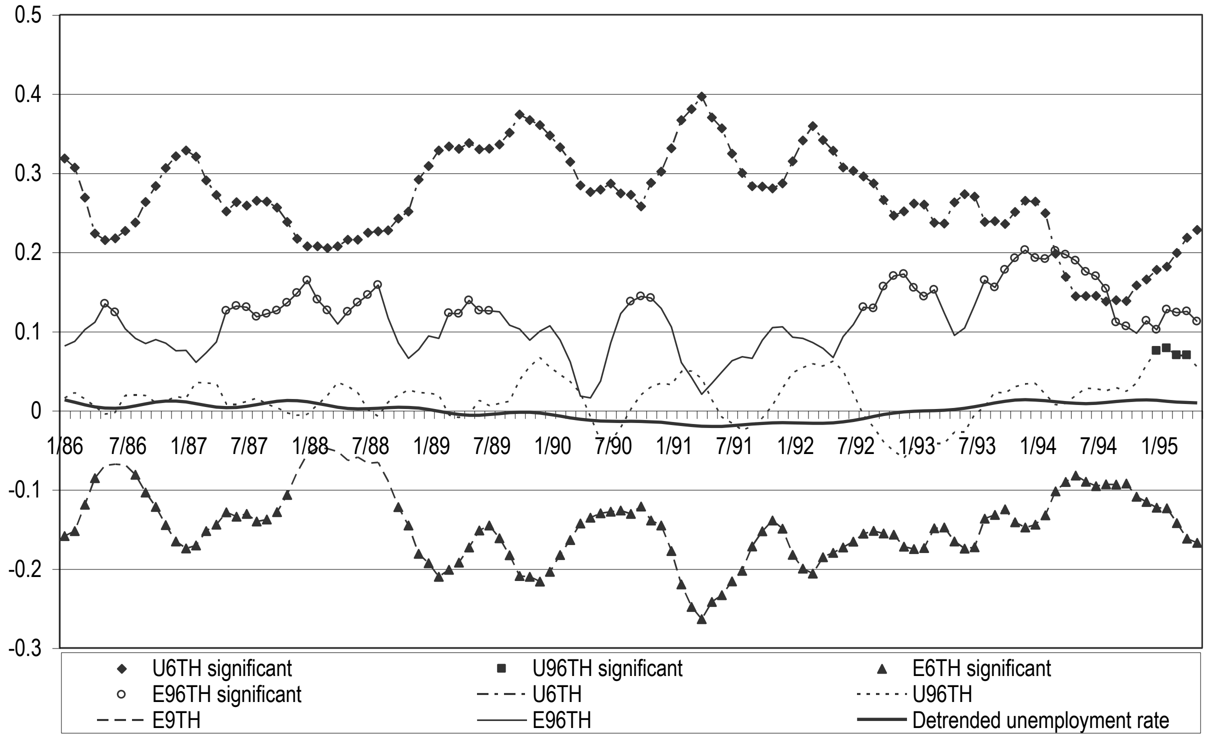
(stable characteristics of participants)



Note: The outcome variables are named as follows: U: unemployment, E: employment, 6 or 96: month after program start, TH: theta (average treatment effect on the treated). 220-590 observations per period (pooled). See also note below Figure 2.

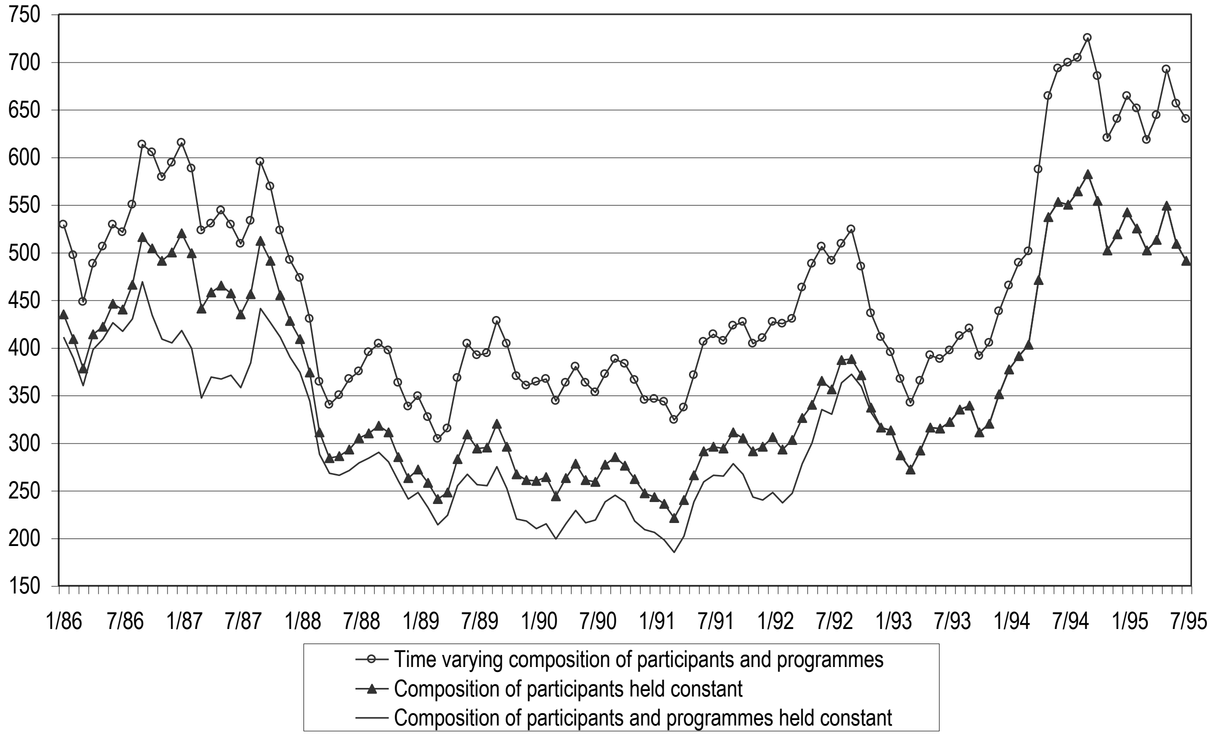
Figure 6: Effect of training on the employment and unemployment probabilities of participants

(stable characteristics of participants, program types, and program durations)



Note: The effects are named as follows: U: unemployment, E: employment, 6 or 96: month after program start, TH: theta (average treatment effect on the treated). 200-590 observations per period (pooled). See also note below Figure 2.

Figure A.1: Number of participants over time



Note: From January 1993 on the number of participants in the specification where the composition of both participants and programs is held constant is the same as in the specification where only the composition of participants is held constant (because the latter de-selects participants in job search assistance that ceases to exist after 1992).

Tables

Table 1: Combined administrative data sources used

	IAB Employment Subsample (ES)	Benefit Payment Register (BPR)	Training Participant Data (TPD)
Source	Employer supplied mandatory social insurance entries.	Benefit payment register of the PES.	Questionnaires filled in by PES staff for statistical purposes.
Period	1980-2003	1980-2003	1980-1997
Population	1% random sample of all persons covered by social insurance for at least one day 1975-1997. Self-employed, civil servants and university students are not included.	Recipients of benefit payments from the PES.	Participants in further training, retraining, short training, German language courses and temporary wage subsidies 1980-1997.
Available information	Start and end date of employment, gender, age, nationality, education, profession, occupational status, industry, firm size, earnings, regional information.	Start and end date of benefit receipt, marital status, number of children, type and amount of benefits received, regional information.	Start and end date, type, planned and actual duration of the program, type of income support paid during participation, regional information.

Note: The merged data is based on monthly information. For detailed information on the merging and recoding procedures, see Bender et al. (2005). The construction of this database is a result of a three-year joint project of research groups at the Universities of Mannheim (Bergemann, Fitzenberger, Speckesser) and St. Gallen (Lechner, Miquel, Wunsch) as well as the Institute for Employment Research of the FEA (Bender). A detailed description of the ES is provided by Bender, Hilzedegen, Rohwer, and Rudolph (1996), and Bender, Haas, and Klose (2000). For the TPD see Miquel, Wunsch, and Lechner (2002).

Table 2: Correlation of the program effects with indicators for the macroeconomic situation in %

Outcome		Unemployment rate at program start	Quarterly GDP growth rate	# of participants in training
Unemployment	6 months after prog. start	-43**	3	19
	3 years after prog. start	-36*	8	10
	6 years after prog. start	-27*	15	21
	8 years after prog. start	-1	17	17
Employment	6 months after prog. start	25*	8	-1
	3 years after prog. start	45**	2	-3
	6 years after prog. start	43**	-3	-33**
	8 years after prog. start	31**	-12	-50**
Monthly earnings	6 months after prog. st.	20	7	7
	3 years after prog. start	48**	5	-2
	6 years after prog. start	53**	7	-29*
	8 years after prog. start	47**	1	-40**
Cumulated unemployment	6 months after p.s.	-43**	8	15
	3 years after prog. start	-65**	20*	24
	6 years after prog. start	-57**	16	20
	8 years after prog. start	-50**	17	22
Cumulated employment	6 months after p.s.	20	6	9
	3 years after prog. start	47**	-6	2
	6 years after prog. start	50**	-2	-10
	8 years after prog. start	52**	-4	-22
Cumulated earnings	6 months after p.s.	13	8	17*
	3 years after prog. start	46**	-2	5
	6 years after prog. start	51**	4	-5
	8 years after prog. start	56**	4	-15

Note: 115 observations (months) in each case except that the quarterly GDP growth rate only varies every three months. Inference for the correlations is based on Newey-West autocorrelation-robust t-values obtained from a regression of the particular effect estimates on a constant and the respective economic indicator: ** significant at the 1% level, * significant at the 5% level (see also footnote 18).

Table 3: Correlation of the characteristics of participants with the unemployment rate in %

Characteristics of program participants	Unemployment rate at program start
Woman	-52**
Foreigner	-24*
No occupational qualification	-67**
University/college degree	7
Duration of last unemployment spell	-51**
Fraction of months employed in the last 6 years	82**
Fraction of months unemployed in the last 6 years	-46**

Note: Correlation of monthly mean of respective variable (six-month moving average) with the unemployment rate. 115 observations. Inference for the correlations is based on Newey-West autocorrelation-robust t-values obtained from a regression of the respective characteristics on a constant and the unemployment rate at program start: ** significant at the 1% level, * significant at the 5% level (see also footnote 18).

Table 4: Correlation of the program effects with the unemployment rate at program start in %

(stable characteristics of participants)

	Outcome	Participants constant	Previous specification: participants change	
			Strict joint support+	Original sample
Unemployment	6 months after program start	-49**	-14	-43**
	3 years after program start	-48**	-39**	-36*
	8 years after program start	19	12	-1
Employment	6 months after program start	36**	33**	25*
	3 years after program start	45**	54**	45**
	8 years after program start	31*	29**	31**
Monthly earnings	6 months after program start	40**	27*	20
	3 years after program start	44**	45**	48**
	8 years after program start	53**	37**	47**

Note: 115 observations. Inference for the correlations is based on Newey-West autocorrelation-robust t-values obtained from a regression of the particular effect estimates on a constant and the respective economic indicator: ** significant at the 1% level, * significant at the 5% level (see also footnote 18). +To make the results more comparable, participants are reduced to the same joint support over time as in the reference population. Note that the number of treated observations is rather small in some months.

Table 5: Correlation of the characteristics of the training policy with the unemployment rate at program start in %

Characteristics of programs	Participants	Stable participants
Fraction of participants in practice firms	5	-4
Fraction of participants in short training	28**	26**
Fraction of participants in long training	34**	25**
Fraction of participants in retraining	-1	6
Fraction of participants in job search assistance	-42**	-27*
Planned program duration	6	1
Planned duration of practice firms	-34**	20
Planned duration of short training	-20*	27**
Planned duration of long training	-13	6
Planned duration of retraining	-21*	-54**
Planned duration of job search assistance	34**	46**

Note: Correlation of monthly mean of respective variable (six-month moving average) with the unemployment rate. *Participants* are those participants used in Section 5, whereas *stable participants* are those used in Section 6.1.
115 observations. Inference for the correlations is based on Newey-West autocorrelation-robust t-values obtained from a regression of the respective characteristics on a constant and the unemployment rate at program start: ** significant at the 1% level, * significant at the 5% level (see also footnote 18).

Table 6: Correlation of the program effects with the unemployment rate in %

(stable characteristics of participants, program types, and program durations)

Outcome		Participants and programs constant	Previous specifications: Participants constant	Previous specifications: Participants change
Unemployment	6 months after prog. start	-58**	-49**	-43**
	3 years after prog. start	-39**	-48**	-36*
	8 years after prog. start	3	19	-1
Employment	6 months after prog. start	46**	36**	25*
	3 years after prog. start	31*	45**	45**
	8 years after prog. start	40**	31*	31**
Monthly earnings	6 months after prog. start	52**	40**	20
	3 years after prog. start	37*	44**	48**
	8 years after prog. start	55**	53**	47**

Note: 115 observations. Inference for the correlations is based on Newey-West autocorrelation-robust t-values obtained from a regression of the respective effect estimates on a constant and the unemployment rate at program start: ** significant at the 1% level, * significant at the 5% level (see also footnote 18).

Table 7: Correlation of the program effects with the unemployment rate at program start in %

(participants and program composition do not change over time)

Outcome		Monthly unemployment rate 1986 - 1995: Quarter dummies only	Monthly unemployment rate 1986 - 1995: Interacted with post-unification dummy
Unemployment	6 months after prog. start	-58**	-66**
	3 years after prog. start	-31*	-28*
	8 years after prog. start	6	5
Employment	6 months after prog. start	49**	50**
	3 years after prog. start	25	19
	8 years after prog. start	38**	47**
Monthly earnings	6 months after prog. start	53**	54**
	3 years after prog. start	29*	21*
	8 years after prog. start	53**	58**

Note: 115 observations for each cell. Inference for the correlations is based on Newey-West autocorrelation-robust t-values: ** significant at the 1% level, * significant at the 5% level.

Table 8: Correlation of the program effects with the unemployment rate %

(stable characteristics of participants, program types, and planned durations)

	Outcome	Correlation of the effects with national unemployment rate over time: Regional unemployment low [a]	Regional unemployment high [a]	Correlation of regional effects with average regional unemployment rate [b]
Unemployment	6 months after prog. start	-56**	-37**	-21**
	3 years after prog. start	-47**	-37**	-18**
	8 years after prog. start	-13	-14	-14**
Employment	6 months after prog. start	38**	15	9
	3 years after prog. start	40*	36*	7
	8 years after prog. start	49**	46**	-8
Monthly earnings	6 months after prog. start	48**	27	11*
	3 years after prog. start	44**	39**	9
	8 years after prog. start	59**	60**	-4

Note: Low / high unemployment regions are those where 60% of the unemployed facing the lowest / highest regional unemployment rate in a specific period live. [a] 115 observations for each cell. [b] 36 observations for each cell. Inference for the correlations is based on Newey-West autocorrelation-robust t-values: ** significant at the 1% level, * significant at the 5% level.

Table 9: Correlation of the program effects with the unemployment rate in %

(stable characteristics of participants, program types, and durations)

	Outcome	Separate estimation: pre 10/1990 [a]	Separate estimation: post 10/1990 [b]	Interaction terms [c]: pre 10/1990	Interaction terms [c]: post 10/1990	With seasonal dummies [c]: pre 10/1990	With seasonal dummies [c]: post 10/1990
Unemployment	6 months after prog. start	-38*	-75**	-57**	-64**	-58**	-66**
	3 years after prog. start	-70**	-20	-31*	-25*	-31*	-24*
	8 years after prog. start	2	6	6	7	6	8
Employment	6 months after prog. start	33*	55**	48**	45**	49**	46**
	3 years after prog. start	62**	4	26*	13	26*	12
	8 years after prog. start	23	56**	39**	46**	38**	46**
Monthly earnings	6 months after prog. start	41**	62**	52**	49**	53**	50**
	3 years after prog. start	55**	19	29**	14	29*	15
	8 years after prog. start	29	68**	51**	54**	53**	56**

Note: [a] 57 observations for each cell. [b] 58 observations for each cell. [c] 115 observations for each cell. Inference for the correlations is based on Newey-West autocorrelation-robust t-values: ** significant at the 1% level, * significant at the 5% level.

Table A.1: Descriptive statistics for the reference population

	All participants		Common support	
	Mean	Std. deviation	Mean	Std. deviation
Age in years	34	9.1	33	8.6
Woman	39	0.49	39	0.49
Married	39	0.49	34	0.48
At least one child	33	0.47	26	0.44
Foreigner	9	0.28	3	0.16
No occupational qualification	21	0.40	18	0.38
Completed apprenticeship training	74	0.44	77	0.42
University/college degree	6	0.23	6	0.23
Blue-collar worker	39	0.49	38	0.48
High-skilled	20	0.40	15	0.36
Duration of last unemployment spell in months	11	0.14	7	0.06
Duration of last employment in months	36	0.41	33	0.30
Percent of time employed in last 6 years	54	0.29	60	0.28
Percent of time unemployed in last 6 years	26	0.24	19	0.19
Gross monthly earnings of last employment				
≤ 1000 €	57	0.50	60	0.49
1000-1500 €	29	0.45	32	0.46
1500-2000 €	6	0.23	6	0.23
> 2000 €	2	0.15	3	0.17
Practice firms	14	0.34	13	0.33
Short training	36	0.48	36	0.48
Long training	25	0.43	25	0.43
Retraining	17	0.37	18	0.39
Job search assistance	9	0.28	7	0.26
Planned program duration in months	8.5	7.3	8.9	7.5
Number of observations	9418		2101	

Note: All variables are measured at or relative to program start. If not stated otherwise the means are percentages. Only the common support is used in the estimation.

Table B.1: A matching protocol for the estimation of a counterfactual outcome and the effects

Step 1	Specify a reference distribution defined by X.
Step 2	Pool the observations forming the reference distribution and the participants in the respective period. Code an indicator variable *W*, which is 1 if the observation belongs to the reference distribution. All indices, 0 or 1, used below relate to the actual or potential values of *W*.
Step 3	Specify and estimate a binary probit for $p(x) := P(W = 1 \mid X = x)$
Step 4	Restrict sample to common support: Delete all observations with probabilities larger than the smallest maximum and smaller than the largest minimum of all subsamples defined by *W*.
Step 4	*Estimate the respective (counterfactual) expectations of the outcome variables.* **Standard propensity score matching step (multiple treatments)** a-1) Choose one observation in the subsample defined by *W*=1 and delete it from that pool. b-1) Find an observation in the subsample defined by *W*=0 that is as close as possible to the one chosen in step a-1) in terms of $p(x), \tilde{x}$. 'Closeness' is based on the Mahalanobis distance. Do not remove that observation, so that it can be used again. c-1) Repeat a-1) and b-1) until no observation with *W*=1 is left. **Exploit thick support of X to increase efficiency (radius matching step)** d-1) Compute the maximum distance (*d*) obtained for any comparison between a member of the reference distribution and matched comparison observations. a-2) Repeat a-1). b-2) Repeat b-1). If possible, find other observations in the subsample of *W*=0 that are at least as close as *R* * *d* to the one chosen in step a-2) (to gain efficiency). Do not remove these observations, so that they can be used again. Compute weights for all chosen comparisons observations that are proportional to their distance (triangular kernel). Normalise the weights such that they add to one. c-2) Repeat a-2) and b-2) until no participant in *W*=1 is left. d-2) For any potential comparison observation, add the weights obtained in a-2) and b-2). **Exploit double robustness properties to adjust small mismatches by regression** e) Using the weights $w(x_i)$ obtained in d-2), run a weighted linear regression of the outcome variable on the variables used to define the distance (and an intercept). f-1) Predict the potential outcome $y^0(x_i)$ of every observation using the coefficients of this regression: $\hat{y}^0(x_i)$. f-2) Estimate the bias of the matching estimator for $E(Y^0 \mid W = 1)$ as: $\sum_{i=1}^{N} \frac{\underline{1}(W=1)\hat{y}^0(x_1)}{N^1} - \frac{\underline{1}(W=0)w_i\hat{y}^0(x_1)}{N^0}$. g) Using the weights obtained by weighted matching in d-2), compute a weighted mean of the outcome variables in *W=0*. Subtract the bias from this estimate to get $\widehat{E(Y^0 \mid W = 1)}$.
Step 5	Repeat Steps 2 to 4 with the non-participants playing the role of participants before. This gives the desired estimate of the counterfactual non-participation outcome.
Step 6	The difference of the potential outcomes gives the desired estimate of the effect with respect to the reference distribution specified in Step 1.

Note: $\tilde{x}$ includes gender, elapsed unemployment duration until program start, and whether a person is employed in month 12 or month 24 before program start. In some specifications, we also match on education. In the specification where program composition is held constant, we also match on the type of program and planned program duration. $\tilde{x}$ is included to ensure a high match quality with respect to these critical variables.