ZEW Economic Studies 46

Thomas Walter

Germany's 2005 Welfare Reform

Evaluating Key Characteristics with a Focus on Immigrants

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To my parents

Preface

This thesis was written during my time at the Centre for European Economic Research (*Zentrum für Europäische Wirtschaftsforschung*, ZEW) in Mannheim. It is based on the research projects Evaluation of the Experimentation Clause in Chapter 6c of Book II of the German Social Code, Research Area 3: Impact and Efficiency Analysis (*Evaluation der Experimentierklausel nach §6c SGB II, Untersuchungsfeld 3: Wirkungs- und Effizienzanalyse*) and Effects of Book II of the German Social Code on Persons with a Migration Background (*Wirkungen des SGB II auf Personen mit Migrationshintergrund*). Both projects were commissioned by Germany's Federal Ministry of Labour and Social Affairs (*Bundesministerium für Arbeit und Soziales*) to evaluate the German welfare reform of 2005 and were conducted by ZEW and several project partners, including the Institute for Work, Skills and Training (*Institut Arbeit und Qualifikation*) and TNS Emnid. The completion of the thesis would not have been possible without the support of a number of people to whom I owe many thanks.

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Mannheim, July 2012 *Thomas Walter*

Table of Contents

1 Introduction 1

2 Background for the Empirical Analyses: Institutional Details and Data 7
2.1 The German Welfare System and Its 2005 Reform 7
2.2 Selected Indicators on Macroeconomic Conditions and the Welfare System in Germany from 2000 to 2009 14
2.3 Data Sources 22
2.3.1 Data Base Used in Chapters 3 and 4 26
2.3.2 Data Base Used in Chapters 5 and 6 29

3 Centralized versus Decentralized Welfare Administration . 33
3.1 Introductory Remarks 33
3.2 Estimation Sample 35
3.3 Estimation Approach 35
3.3.1 Estimation of the Average Effect of Treatment on the Treated 35
3.3.2 Plausibility of the Conditional Independence Assumption and Specification of the Propensity Score . 37
3.3.3 Common Support and Balancing Quality of the Matching Estimator 40
3.4 Empirical Results 42
3.5 A Closer Look at the Black Box of Welfare Administration . . 46
3.6 Summary 49

4 The Employment Effects of an Intensified Use of Benefit Sanctions 51
4.1 Introductory Remarks 51
4.2 Data Preparation and Estimation Sample 54
4.3 Estimation Approach 55

4.4 Empirical Analysis . . . 61
4.4.1 Descriptive Evidence . . . 61
4.4.2 Estimation Results . . . 65
4.5 Summary . . . 71

5 The Effectiveness of Temporary Extra Jobs and Short-Term Training Programs . . . 73
5.1 Introductory Remarks . . . 73
5.2 Related Literature . . . 75
5.3 Institutional Background . . . 79
5.3.1 Temporary Extra Jobs . . . 79
5.3.2 Short-Term Training Programs . . . 81
5.4 Data for Empirical Analysis . . . 83
5.4.1 Estimation Sample and Available Information . . . 83
5.4.2 Characterization of the Sampled Immigrants and Natives . . . 85
5.5 Evaluation Approach . . . 88
5.5.1 Estimation of Treatment Effects . . . 88
5.5.2 Plausibility of the Dynamic Conditional Independence Assumption and Implementation of the Matching Estimator . . . 91
5.5.3 Estimation of the Immigrant Fixed Effect . . . 94
5.6 Empirical Results . . . 97
5.6.1 Quality of the Estimates . . . 97
5.6.2 Program Effects . . . 99
5.6.3 Immigrant Fixed Effects . . . 108
5.6.4 Effects for Subgroups of Immigrants . . . 110
5.7 Summary . . . 114

6 Fiscal Cost-Benefit Analyses for Temporary Extra Jobs and Short-Term Training Programs . . . 117
6.1 Introductory Remarks . . . 117
6.2 Conceptual Framework . . . 121
6.3 Determination of Fiscal Costs and Benefits . . . 124
6.3.1 Fiscal Costs . . . 124
6.3.2 Fiscal Benefits . . . 125
6.4 Comparison of Fiscal Costs and Benefits . . . 134
6.5 Summary . . . 138

7 Conclusions . . . 141

Appendices . . . 149
Appendix to Chapter 2 . . . 151
Appendix to Chapter 3 . . . 155
Appendix to Chapter 4 . . . 177

Appendix to Chapter 5 187
Appendix to Chapter 6 219

List of Abbreviations 245

List of Figures 247

List of Tables 249

References 253

Chapter 1
Introduction

At the beginning of 2005, the German government enacted a substantial reform of the welfare system. This welfare reform was the final part of a series of four major reforms of the German labor market that came into force between 2003 and 2005 in response to a high and rising unemployment rate and the associated large expenditures on active and passive labor market policies.[1] The reform replaced the previously existing two-tier system of unemployment assistance (*Arbeitslosenhilfe*) and social assistance (*Sozialhilfe*) with a single welfare benefit called unemployment benefit II (UBII; in German: *Arbeitslosengeld II*). Along with the introduction of the new benefit, the reform redesigned the organization of welfare administration and increased job search requirements among welfare recipients, i.e. among individuals who are of working age and capable of working but who do not earn a sufficient living income and need financial support from the government in the form of welfare benefits. The ultimate goal of the reform was to increase the effectiveness and efficiency of labor market activation of welfare recipients (see Jacobi and Kluve, 2007). Thus, the German reform was similar to the welfare reforms conducted in other industrialized countries (e.g. in the US, in the UK, in Denmark, in Sweden, and in the Netherlands) during the last decades. Most of these reforms involved organizational changes and resulted in a shift from passive benefit payment toward an intensified activation of welfare recipients, with the objective of encouraging employment uptake and reducing welfare dependency (see Konle-Seidl, 2008 and 2009; and Huber et al., 2011).

In this thesis, I will evaluate two key characteristics of Germany's 2005 welfare reform that directly influence the way labor market activation of welfare

[1] These reforms have been implemented by four "Acts for modern Labor Market Services" (*Gesetze für moderne Dienstleistungen am Arbeitsmarkt*). They are now commonly known as the "Hartz reforms", named after the chairman of the commission proposing the reforms. Since the reform of the welfare system is the final of the four reforms, it is also referred to as the "Hartz IV reform". See Jacobi and Kluve (2007) for a description of all four Hartz reforms.

recipients is conducted. The first key characteristic is that the reform introduced two different organizational models for the labor market activation of welfare recipients. In the majority of the 439 German welfare districts[2], a centralized organization was established. Within these districts, the existing local employment office (LEO; in German: *Agentur für Arbeit*) of the Federal Employment Agency (FEA; in German: *Bundesagentur für Arbeit*) and the local authorities had to form a joint venture to constitute the new local welfare agency.[3] In these ventures, the LEO and local authorities work together to help welfare recipients find employment. The LEO is responsible for the allocation of Active Labor Market Programs (ALMP), for job placement, and for the administration of basic welfare payments, whereas the local authorities manage financial support for housing costs and additional needs and offer counseling for specific obstacles to employment such as single parent status. The joint venture is under the direct supervision of the FEA. It can thus be considered a centralized welfare agency. The German term for a centralized agency is *Arbeitsgemeinschaft (ARGE)*.

By contrast, 69 of the 439 districts were allowed to establish a decentralized organization of welfare administration and to constitute their own decentralized welfare agency (in German: *zugelassener kommunaler Träger*, or *zkT*). In the decentralized agencies, the local authorities alone organize all aspects of labor market activation of welfare recipients. They are responsible for the entire activation process including counseling, the allocation of ALMP, job placement, and the disbursement of benefits. The FEA is not involved in any task. The 2005 welfare reform did not, therefore, implement a homogenous administrative setting. Rather, it put into operation two very different organizational models.

The second key reform characteristic is the enforcement of the principle of "supporting and demanding" (*Fördern und Fordern*) in the activation of welfare recipients across all welfare agencies, irrespective of the organizational model. The demanding component of this principle requires the welfare recipients do everything in their power to leave the welfare system as quickly as possible by taking up employment. In particular, welfare recipients have to actively engage in the job search, they have to accept any reasonable job offer, and they have to participate in ALMP if assigned by the welfare agency. These requirements are closely monitored, and, in case of non-compliance, benefit sanctions can be imposed, which reduce the monetary payments to the welfare recipients for a certain period of time. In turn, the welfare agency

[2] The welfare districts are called *Träger der Grundsicherung für Arbeitsuchende* in German. They are geographically similar to the political districts (*Landkreise* and *kreisfreie Städte*) and to the labor market districts (*Agenturbezirke*) of the Federal Employment Agency (FEA) but not identical in every single case. Throughout this thesis, the terms "welfare districts" and "districts" will be used interchangeably to refer to the *Träger der Grundsicherung für Arbeitsuchende*.

[3] From 2005 onwards, the terms "(welfare) district" and "(welfare) agency" have functioned synonymously.

acts as a service provider to help welfare recipients find a way out of welfare dependency. This support is reflected by the counseling activities of welfare agencies (e.g. counseling on job search strategies or counseling on individual obstacles to employment) and, more importantly, by the provision of ALMP. This provision constitutes a remarkable change in German welfare policy. For the first time, all welfare recipients available to the labor market are a target group for potential participation in ALMP. A large number of programs are offered. The most widely used programs are so-called Temporary Extra Jobs (*Arbeitsgelegenheiten in der Mehraufwandsvariante*; commonly referred to as *Ein-Euro-Jobs*) and short-term training programs (*Trainingsmaßnahmen*). Temporary Extra Jobs offer temporary work opportunities in the public sector. German legislation stipulates that these jobs be of public interest and additional in the sense that they do not compete with regular employment. While Temporary Extra Jobs can last for up to six months, short-term training has a maximum duration of only twelve weeks. The content of short-term training is rather heterogeneous and can include job application training, the provision of general skills required for employment (e.g. computer courses) and the training of specific occupational skills.

Both key characteristics indicated above have aroused considerable public interest (see e.g. Eichhorst et al., 2010). This interest is also reflected in Book II of the German Social Code (*Sozialgesetzbuch Zweites Buch, SGB II*), the legal basis of the reform, which explicitly mandates the evaluation of these key characteristics and the evaluation of labor market activation of welfare recipients in general.[4] According to chapter 1 of SGB II, labor market activation has to aim at integrating welfare recipients into self-sufficient employment, i.e. employment generating a sufficient wage such that no welfare benefits are needed any longer. In this thesis, my analyses are carried out from a microeconometric perspective and with respect to the aim of activation as noted in chapter 1 of SGB II. Specifically, I investigate the effects of the key reform characteristics on the individual probability of welfare recipients' finding self-sufficient employment. Due to the relative recentness of the reform and my analyses, I focus on short-run effects of up to one year.

Moreover, this thesis has a focus on welfare recipients with a migration background. Immigrants[5] are highly over-represented in the German welfare system, but there is little data on their employment prospects. While about one in three welfare recipients has a migration background, the correspond-

[4] To fulfill the legal evaluation requirements, the Federal Ministry of Labour and Social Affairs (*Bundesministerium für Arbeit und Soziales*) commissioned several research projects. The projects Evaluation of the Experimentation Clause in Chapter 6c of Book II of the German Social Code, Research Area 3: Impact and Efficiency Analysis (*Evaluation der Experimentierklausel nach §6c SGB II, Untersuchungsfeld 3: Wirkungs- und Effizienzanalyse*) and Effects of Book II of the German Social Code on Persons with a Migration Background (*Wirkungen des SGB II auf Personen mit Migrationshintergrund*) initiated the analyses presented in this thesis.

[5] The terms "persons with a migration background" and "immigrants" will be used interchangeably in this thesis.

ing share in the population is only about 19% (see Bundesministerium für Arbeit und Soziales, 2009; and Statistisches Bundesamt, 2007). Given this over-representation of persons with a migration background on welfare, it is crucial to understand how labor market activation affects this group. This question is especially interesting in view of the ongoing public dispute in Germany about the difficult integration of immigrants into German society in general and into the labor market in particular (see e.g. the introduction to the volume edited by Knuth, 2010). A successful labor market integration could be a stepping stone on the way to a successful social integration. Consequently, the design of labor market activation and welfare policy might have a considerable impact on the integration pathway.

My analyses are based on unique data that have been specifically collected for the research questions presented in this thesis. The data comprise survey and administrative information on welfare recipients and information on welfare agencies, in particular their activation strategies and internal organization. They are described in detail in **chapter 2**. This chapter also provides a comprehensive description of the German welfare system and its 2005 reform.

Concerning the first key reform characteristic, the question arises of who should be responsible for the activation of welfare recipients. Should the activation be administered on the discretion of local authorities, or should the FEA be in charge? Rephrasing this question, one can ask whether welfare administration should be decentralized or centralized. **Chapter 3** is devoted to this question. It adopts a propensity score matching approach, in which individuals registered at decentralized welfare agencies are considered as the treatment group, and welfare recipients from centralized agencies form the control group. Treated and non-treated individuals are compared with respect to their likelihood of taking up self-sufficient employment. This comparison contributes to the literature by providing the first quantitative evidence on the effects of a decentralized versus centralized organization of welfare administration on individual employment outcomes.

Independent of the organization of welfare administration, strategical features of welfare agencies may affect success rates for integrating welfare recipients into self-sufficient employment. One such feature could be the sanction strategy of a welfare agency. Sanctions are a crucial element of the principle of demanding. One assumes that they foster compliance of welfare recipients with their duties during the activation process (see van den Berg et al., 2004). Even though the deployment of sanctions are determined by law, their use differs widely between welfare agencies. Some agencies sanction frequently, whereas others use this measure rarely. In **chapter 4** of this thesis, variation in sanction strategies and rates across welfare agencies is used to investigate whether an intensified use of benefit sanctions is an effective tool to speed up transitions from welfare to employment. For this investigation, an instrumental variable approach is applied to estimate the effect of a benefit sanction for those individuals who are not sanctioned in a welfare agency with a mod-

erate sanction regime but who will be sanctioned if the agency decides to change its policy and impose sanctions more frequently. This effect provides an estimate of the effectiveness of an intensified use of sanctions. The analysis acknowledges that welfare agencies play a crucial role for the imposition of benefit sanctions. To date, researchers have neglected the potential role played by these agencies.

Along with the principle of demanding, the second key characteristic of Germany's 2005 welfare reform is comprehensive support for welfare recipients by the welfare agencies. This supporting function of welfare agencies is mainly visible in the provision of ALMP, which are intended to increase the employment chances of individuals. Whether the intended goal is actually achieved is considered in **chapter 5**. This chapter estimates the employment effects of the most frequently used programs since 2005: Temporary Extra Jobs and four different short-term training programs. For the estimation, a dynamic propensity score matching approach is adopted that takes account of the starting point of program participation while persons are on welfare. The analysis has a particular interest in the following research questions: Are programs similarly effective for immigrants and native Germans, or do their effects differ? And, if differences in effects are observable between both groups, what are the causes of these differences? Are they due to observable differences in sociodemographic characteristics, or are they due to unobservable differences that must be attributed to immigrant status? In order to answer these questions, differences in treatment effects between native Germans and immigrants that result from the attachment of the individual to the immigrant group are identified on the basis of a matching estimator. Identifying causes of potential differences in treatment effects between natives and immigrants contributes to the existing literature on the effectiveness of ALMP in the German welfare system. Most researchers have so far ignored the question of effect heterogeneity.

Besides evaluating the effectiveness of Temporary Extra Jobs and short-term training programs, I also conduct fiscal cost-benefit analyses to investigte their efficiency from the perspective of the government that provides the programs (see **chapter 6**). Only combined information about the effectiveness and efficiency of programs allows for a meaningful discussion of policy recommendations on the use of ALMP. Depending on the effectiveness of programs, the government can obtain either positive or negative benefits. In the case of a positive impact of a program on the employment chances of an individual, the government benefits in terms of increased income tax payments and social insurance contributions. It also realizes savings on welfare payments and administrative costs. These positive fiscal benefits need to be compared with the operating costs of the program. If positive benefits outweigh costs, the program in question is considered to be fiscally efficient. The comparison of costs and benefits at different points in time allows one to measure how long it takes a program to pay off and how its efficiency over time compares with other programs. To perform the fiscal cost-benefit analy-

ses, the post-welfare wages of individuals who leave welfare for self-sufficient employment are estimated with a standard wage equation. Based on these estimates, fiscal benefits in terms of income tax and social insurance contributions are derived in accordance with the relevant tax legislation. In addition, savings on welfare payments and administrative costs are calculated, while program costs are obtained from the FEA. To the best of my knowledge, the efficiency of the most frequently used ALMP in the German welfare system has yet to be systematically evaluated.

Finally, **chapter 7** concludes this thesis by summarizing the results of the empirical analyses and discussing policy implications. In particular, the results will be considered with regard to current political proposals for improving the German welfare system.

Chapter 2

Background for the Empirical Analyses: Institutional Details and Data

This chapter lays the foundations for the empirical analyses presented in chapters 3 to 6. In section 2.1, a description of the main features of the German welfare system before and after the 2005 welfare reform is provided. Section 2.2 summarizes the macroeconomic conditions in Germany during this time period and presents selected figures on the German welfare system in particular for the post-reform period. In section 2.3, the data sources used for the empirical analyses are introduced.

2.1 The German Welfare System and Its 2005 Reform

Prior to the 2005 welfare reform, Germany was characterized by a two-tier system of welfare benefits. This system differentiated between individuals who had contributed sufficiently and individuals who had not contributed sufficiently to unemployment insurance (see Wunsch, 2005).[6] In case of sufficient contributions unemployed individuals were entitled to unemployment insurance benefits (*Arbeitslosengeld*), which were granted for no longer than 32 months, and, after exhaustion of these benefits, to unemployment assistance (*Arbeitslosenhilfe*).[7] Unemployment assistance was means-tested and proportional to former earnings. Claimants received 57% of their previous av-

[6] Contributions of an individual were sufficient if the individual had been employed and had paid social insurance contributions for at least twelve months during the final three years before becoming unemployed. In February 2006, this qualification period was reduced from three to two years.

[7] The general setting of unemployment insurance benefits was not targeted by the 2005 reform. In case of unemployment, persons with sufficient contributions to unemployment insurance can claim unemployment benefits for a limited period of time. The time limit depends on the time of contribution and age. Before January 31, 2006 the time limit varied between 6 and 32 months. The maximum duration was afterwards reduced to 18 months, but in January 2008 again increased to 24 months for persons age 58 or above. Unemployment insurance benefits are earnings related. They replace

erage net monthly earnings if they had at least one dependent child. Without dependent children, the replacement rate amounted to 53%. Unemployment assistance was funded from general taxes and was paid for a potentially unlimited period of time until retirement age (see Eichhorst et al., 2010).

Individuals who had not contributed sufficiently to unemployment insurance could only apply for social assistance (*Sozialhilfe*). This was a means-tested, monthly payment whose amount depended on the income and wealth of all household members (see Bäcker and Koch, 2004; or Clauss and Schnabel, 2008). It was not related to previous earnings and provided a basic income in order to guarantee the socio-cultural subsistence level. Social assistance did not require the claimants to be unemployed. The "working poor", who could not live upon their own earnings, were eligible, too. Moreover, persons in retirement age who had no sufficient income from their pensions or other sources could apply for social assistance. Like unemployment assistance, social assistance was financed from general taxes (see Wunsch, 2005). However, the means-test for social assistance recipients was stricter and the payments, in general, lower than for unemployment assistance recipients (see Wunsch, 2005, and Eichhorst et al., 2010). According to Ochel (2005), in 2004 the average base payment of social assistance (not including additional benefits like payments for housing costs) for a single person amounted to 296 euros in West Germany and to 283 euros in East Germany. The average amount of unemployment assistance (excluding additional benefits) was 583 euros in the western part and 516 euros in the eastern part of Germany, respectively (see Bundesagentur für Arbeit, 2005). In cases where unemployment assistance was insufficient to reach the socio-cultural subsistence level, it was topped up with social assistance.

In addition to the different eligibility criteria, there were two different authorities responsible for the administration of benefits and the labor market activation of welfare recipients. The Federal Employment Agency (FEA), represented by its local employment offices (LEOs), had been in charge of unemployment assistance and was responsible for the labor market activation of unemployment assistance recipients and their job placement. For labor market activation, it could make use of nearly all Active Labor Market Programs (ALMP), which it also directed at unemployment insurance benefit recipients.

By contrast, local authorities were financially responsible for social assistance and in charge of the activation and placement of social assistance recipients. Yet there was only a rudimentary set of integration measures available to local authorities. In particular, local authorities had no access to ALMP of the FEA. Thus, the two-tier system of welfare benefits did not provide

67% of previous average net earnings for an individual with at least one dependent child. Without children, the replacement rate amounts to 60%. Unemployment insurance benefit recipients have to register with the local employment office (LEO), which is part of the Federal Employment Agency (FEA). The LEO is responsible for the labor market activation of its clients.

equal activation opportunities for unemployment assistance and social assistance recipients. While some local authorities used their limited activation means fairly well, others did not enforce a systematic activation approach (see Eichhorst et al., 2010).

The system also resulted in undesirable incentives. For instance, local authorities had an incentive to place social assistance recipients in temporary employment measures that were subject to unemployment insurance contributions. Participation in these measures created entitlements to unemployment insurance and unemployment assistance benefits and thus shifted the claimants from the local authorities to the FEA (see Eichhorst et al., 2010). Moreover, incentives for caseworkers at LEOs to integrate unemployment assistance recipients into employment were weak. Since the LEOs had been mainly financed by employer and employee contributions to unemployment insurance, their caseworkers concentrated their efforts on short-term unemployed receiving unemployment insurance benefits and neglected unemployment assistance recipients (see Tergeist and Grubb, 2006).

To remove these unintended incentives and to unify labor market activation of all welfare recipients, a reform of the welfare system was implemented in January 2005. The reform abolished unemployment assistance and social assistance and merged both types of benefits into a single program: unemployment benefit II (UBII, in German: *Arbeitslosengeld II*).[8] Unlike unemployment assistance and like former social assistance, UBII does not depend on former earnings. UBII is means-tested, taking into account the income and wealth of all individuals living in the household[9] of the claimant.[10] To be eligible for UBII, persons have to be age between 15 and 64 and must be able to work for at least 15 hours per week. Current unemployment is not a prerequisite for UBII receipt. Individuals who work but who earn too little to have a sufficient household income are also eligible for the benefit. Since the means-test is conducted at the household level, all persons living in the household receive UBII provided that they fulfill the eligibility criteria. Those persons of a needy household who are not able to work for at least 15 hours a week and hence do not fulfill the eligibility criteria receive a so-called social allowance (*Sozialgeld*). This benefit is nearly identical to UBII but does not require claimants to actively look for a job and to participate in ALMP. Social allowance is predominantly paid to the children of UBII recipients age less than 15 (see Eichhorst et al., 2010). In this thesis, I will focus on UBII recipients.

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[8] The name UBII was created to distinguish the new welfare benefit from the unemployment insurance benefit, referred to as unemployment benefit I (UBI).

[9] For the legal definition of the term *household*, I refer to chapter 7 of Book II of the German Social Code (*Sozialgesetzbuch Zweites Buch, SGB II*).

[10] The means-test is similar to the one used for social assistance recipients, and is thus stricter than the means-test used for unemployment assistance recipients before the reform (see Bäcker and Koch, 2004; and Bruckmeier and Schnitzlein, 2007).

It is important to stress that welfare benefits (UBII, social allowance) are only granted to each individual of a household when all members of the household taken together are in need of governmental support. If one household member takes up employment and earns sufficiently, he or she must share his or her financial resources with all other household members to bring the household above the socio-cultural subsistence level. Governmental support in terms of welfare benefits is only subsidiary. In this sense, all households in the German welfare system form "communities with joint responsibility" (*Bedarfsgemeinschaften*).

At the beginning of 2005, the base payment of UBII (*Regelbedarf zur Sicherung des Lebensunterhalts*) amounted to 345 euros in West Germany and to 331 euros in East Germany. Since then the level of the base payment in East Germany has been adjusted to the level in West Germany, and the level in both parts of the country gradually raised – in order to compensate for inflation, with the base payment now totalling 364 euros. Thus, the base payment of UBII is less generous than unemployment assistance but more generous than social assistance. On top of the base payment, welfare payments also cover housing costs for rent and heating (*Bedarfe für Unterkunft und Heizung*) and social insurance contributions (*Zuschuss zu Versicherungsbeiträgen*). Further costs for additional needs (*Mehrbedarfe*) such as those that arise during pregnancy might be financed as well (see Kemmerling and Burttel, 2005).

In addition to unifying the benefits, the reform also demanded that all welfare services (benefit payments, counseling, labor market activation, etc.) be provided by a single responsible institution per welfare district, as opposed to the former system where the responsibilities were divided between the federal and the local level (see Eichhorst et al., 2010). Yet there was no political consensus on where the new welfare agencies should be established: within the system of the centralized FEA or at the local authorities. Ultimately, as a compromise it was decided to experiment with two different organizational models – the one centralized, the other decentralized – for a limited period of time and then settle for the more effective one.[11]

It was agreed to apply a centralized organization in 370 out of the 439 German welfare districts. In these districts, the LEO and the local authorities formed a joint venture to constitute the new local welfare agency. This joint venture is under the direct supervision of the FEA. The FEA is in charge of the administration of the base welfare payments (base payment of UBII, social allowance, social insurance contributions), job placement, and the application of ALMP. In particular, guidelines of the FEA on the use of ALMP and the application of technical standards, e.g. computer software, are binding for joint ventures. Due to the influence of the FEA, the joint ventures

[11] This compromise has been codified in the so-called experimentation clause of chapter 6c of Book II of the German Social Code. A description of the experimentation clause with details of implementation, context, and policy results is provided by Deutscher Bundestag (2008).

can be considered as institutions acting under central directives. I henceforth refer to them as centralized welfare agencies.[12] Local authorities in the centralized welfare agencies are tasked with administrating payments for housing costs and additional needs. Moreover, they provide counseling in specific contexts such as single parent families, home care for elderly/disabled relatives or alcohol and drug addictions (see Wunsch, 2005).[13]

Of the 439 German districts, 69 were allowed to opt for a more decentralized organization of welfare administration and to constitute their own decentralized welfare agencies.[14] In the decentralized agencies, the local authorities autonomously organize welfare administration. They are responsible for the entire activation process, including counseling, the allocation of benefit recipients to ALMP, job placement, and the disbursement of benefits. The FEA is hence not involved, and decentralized welfare agencies can decide on their own on how to activate the welfare recipients. The decentralized welfare agencies are legally and organizationally independent from central directives and guidelines.

Table 2.1 summarizes the main features of centralized and decentralized welfare agencies. In both agency types, the largest share of welfare payments is financed by the federal government; only a small fraction of overall expenditure – identical in all agencies – is taken from local budgets. Potential advantages and disadvantages of both agency types will be discussed in chapter 3.

The number of decentralized welfare districts (69) has been determined based on political considerations. It is equal to the number of deputies in the *Bundesrat*, the second chamber of the German parliament. Each federal state could have between three and six decentralized districts, depending on its number of deputies in the *Bundesrat*. Within each state, districts could apply to opt out of the centralized system. In cases of excess demand, the state government selected from the applying districts.

In several federal states, the maximum number of districts that could opt for decentralized organization was not exhausted. The vacant places could then be filled by the districts not selected from other states in the first round. Looking at the regional distribution of applications, it appears that the selection process was strongly influenced by political affiliations. In the federal states Lower Saxony (*Niedersachsen*) and Hesse (*Hessen*), where the conservative governments were strongly in favor of the decentralized system, 13 dis-

[12] A centralized welfare agency is called *Arbeitsgemeinschaft (ARGE)* in German.

[13] A variant of the centralized model emerged in instances where the LEO and local authorities could not agree on forming a joint venture. In 19 out of 370 cases, both institutions continued to work separately in the district (*Grundsicherungsstellen mit getrennter Aufgabenwahrnehmung*). But because tasks are shared in a similar way as in the case of the centralized model, this thesis does not differentiate between these two types in its empirical analyses.

[14] The German term for a decentralized welfare agency is *zugelassener kommunaler Träger (zkT)*.

Table 2.1: Main features of centralized and decentralized welfare agencies

	Centralized agencies	Decentralized agencies
Number of entities	370	69
Legal form	Part of the Federal Employment Agency (FEA), but separate legal entity	Part of local administration
Organizational affiliation	Joint venture between local employment office of the FEA and local authorities	Local authorities
Main source of financing	Federal government	Federal government
Standards of the FEA	Binding for job placement, provision of ALMP, monitoring of job search efforts	Not binding, although legal restrictions exist

Remarks: The numbers are based on the 439 German welfare districts that existed as of October 2006.

tricts were allowed to opt out, even though these states only had 6 and 5 seats in the *Bundesrat*, respectively. In contrast, hardly any districts were proposed from the federal states Mecklenburg-Western Pomerania (*Mecklenburg-Vorpommern*) and Rhineland-Palatinate (*Rheinland-Pfalz*), both of which were run at that time by social democrats. Hence, the rules for selection resulted in a concentration of decentralized agencies in certain states (see WZB et al., 2008).

Despite the different organizational settings, the enforcement of all other reform aspects was identical throughout all welfare agencies in Germany. In particular, the principle of "supporting and demanding" (*Fördern und Fordern*), was enforced uniformly across all centralized and decentralized agencies. This principle of mutual obligations requires both the welfare recipient and the welfare agency to do everything in their power to help recipients find self-sufficient employment as quickly as possible. The rights and duties of both parties in the activation process are set out in a so-called "integration contract" (*Eingliederungsvereinbarung*), an agreement between the welfare agency and the benefit recipient containing obligations with respect to job search activities and participation in ALMP, as well as detailing the services provided by the welfare agency. This contract states the number of job applications the welfare recipient is supposed to write within the next few weeks and the number of job interviews he or she should manage to attend. It also specifies the counseling offers of the welfare agency the welfare recipient can call upon and the ALMP he or she must participate in.

Welfare agencies offer a wide range of different ALMP. These include above all so-called Temporary Extra Jobs (*Arbeitsgelegenheiten in der Mehraufwandsvariante*), short-term training programs (*Trainingsmaßnahmen*), long-term training programs (*Förderung der beruflichen Weiterbildung*), wage sub-

sidies to employers (*Eingliederungszuschüsse*), and start-up grants (*Einstiegsgeld*) (see Huber et al., 2011). Temporary Extra Jobs provide work opportunities in the public sector (see Thomsen and Walter, 2010a). The work assigned within this program must be additional in the sense that it would not be accomplished otherwise by existing public or private sector firms. Temporary Extra Jobs last for up to six months with a typical work load of 20 to 30 hours per week. By contrast, short-term training programs have a usual duration of one to three weeks and do not last longer than twelve weeks (see Kurtz, 2003). They consist of three different types of measures that can be offered either separately or in combination. First, short-term training programs are used to assess the suitability of participants in terms of skills, capability, and labor market opportunities for specific occupations. Second, they aim at improving the participants' job search abilities. And, third, they provide general skills and techniques required for employment. Long-term training comprises a more substantial human capital investment and focuses on the adaption of the professional skills and qualifications of participants to recent labor market requirements, e.g. to mitigate mismatches in times of structural change (see Huber et al., 2011). The programs typically aim at improving skills in the individual's profession, providing additional qualifications, offering a first professional degree or retraining. Program durations vary from a few months to up to three years. Wage subsidies are paid to firms that employ individuals facing competitive disadvantages on the job market during the first months of employment (see Bernhard et al., 2008, or Boockmann et al., forthcoming). They are meant to generate an incentive to hire such individuals by compensating employers for initial productivity gaps. Similarly, start-up grants are bridging allowances for taking up a low-paid job or for creating a private business and becoming self-employed. They are directly paid to the benefit claimants (see Wolff and Nivorozhkin, 2008).

All activation efforts of welfare agencies aim at integrating individuals into jobs that generate a sufficient income such that no welfare benefits are needed any longer. The income must be sufficient to bring all household members above the socio-cultural subsistence level. Activation by welfare agencies thus targets the household as a whole.

The integration contract is usually set up after the first meeting of a welfare recipient with his or her caseworker. The caseworker counsels and advises the welfare recipient and decides about placement in ALMP. If the caseworker detects non-compliance of the welfare recipient during the activation process, the welfare agency is legally required to impose a sanction by benefit revocation (see chapter 31, 31a, 31b and 32 of Book II of the German Social Code). But it is at the discretion of the welfare agency to impose a sanction. Welfare recipients are informed about the possibility of sanctions in the integration contract and each time they are assigned to a program. Yet there is no formal warning process when non-compliance is detected. Although a hearing of the benefit recipient before the imposition of a sanction is legally required, there is evidence that this requirement is not implemented in practice (see

Baethge-Kinsky et al., 2007), and welfare agencies may immediately impose a sanction. Sanctions have a duration of three months and can be imposed for various reasons. For minor non-compliances, such as the failure to properly report on job search activities to the welfare agency or not showing up for an appointment with the caseworker, benefits are cut by 10%. More severe infringements (lack of job search effort, refusal to accept a suitable job offer, refusal to participate in a program) lead to a benefit reduction of 30%.[15] In case of repeated incidents of severe infringements within a year, a second (60% cut) or a third sanction (100% cut) can be imposed. For welfare recipients below 25 years of age, benefits can be reduced by 100% even for the first incident of non-compliance.

With the enforcement of the principle of "supporting and demanding", Germany followed other industrialized countries (e.g. the US, the UK, Denmark, Sweden, and the Netherlands), which reformed their welfare systems somewhat earlier and which also put emphasis on mutual obligations in the activation process of welfare recipients (see Konle-Seidl, 2008). With respect to the organization of welfare administration, Germany's system of centralized and decentralized welfare agencies is unique. All other countries have installed a unified administration of welfare, though their administrative structures vary. While local authorities are responsible for labor market activation of welfare recipients in Denmark and the Netherlands, welfare administration is part of the central government structure in the UK (see Tergeist and Grubb, 2006; and Konle-Seidl, 2009).

2.2 Selected Indicators on Macroeconomic Conditions and the Welfare System in Germany from 2000 to 2009

To put the 2005 welfare reform into perspective, this section will shed light on the macroeconomic conditions in Germany before and after the reform. In addition, it will provide selected descriptive statistics on the German welfare system with emphasis on the post-reform period. Even though the empirical analyses in chapters 3 to 6 will focus on the years 2006 to 2008, the post-reform statistics will be presented for the years from 2005 to 2009 to describe the welfare system as exhaustively as possible. As indicated, the statistics refer either to the entire welfare system or to centralized welfare agencies only. Unfortunately, reliable information for decentralized welfare agencies is not available in many cases.

[15] Irrespective of a 10% or 30% cut, the calculation base for a sanction is the base payment of unemployment benefit II. Since welfare recipients have little to no savings, sanctions are substantial.

As can be seen from table 2.2, the number of social assistance recipients and particularly the number of unemployment assistance recipients increased strongly at the beginning of this century. While the number of all welfare recipients had amounted to about 3.0 million in 2000, it increased by more than a third to 4.1 million in 2004. This increase was due to a combination of a rising inflow into welfare and unsatisfactory low outflow rates from welfare. As has been described in the previous subsection, it is likely that the low outflow rates partly resulted from the fact that activation of unemployment assistance and social assistance recipients was not enforced systematically and was characterized by disincentives for the responsible authorities. The rising inflow into the welfare system had two main reasons. First, welfare benefits were increasingly used as part of early retirement schemes (see Bundesagentur für Arbeit, 2005). And second, unfavorable economic conditions led to a rise in the number of persons not able to earn sufficiently to cover their living expenses. After an economic boom at the end of the last century, Germany experienced a downturn beginning in 2001 (see table 2.2). Gross Domestic Product (GDP) stagnated in 2002 and even declined in 2003. The number of persons registered unemployed increased from 3.85 million in 2001 to 4.38 million in 2004 when a mild recovery set in with a moderate GDP growth of 1.2%.

Table 2.2: Macroeconomic conditions in Germany (2000-2004)

	2000	2001	2002	2003	2004
Unemployment assistance recipients[a]	1,418	1,581	1,815	2,067	2,262
Social assistance recipients[b]	1,620	1,632	1,681	1,783	1,867
Unemployed persons[c]	3,890	3,853	4,061	4,377	4,381
Unemployment rate (in %)[d]	9.6	9.4	9.8	10.5	10.5
GDP growth (in %)[e]	3.2	1.2	0.0	-0.2	1.2

[a] Figures are in the thousands and measured on December 31st of each year. Source: Statistics of the Federal Employment Agency at http://www.pub.arbeitsagentur.de (accessed November 1, 2010).
[b] Figures are in the thousands and measured on December 31st of each year. Source: Statistisches Bundesamt (2005).
[c] Figures are in the thousands and measured as the average annual stock of persons registered unemployed. Source: Bundesagentur für Arbeit (2001, 2002, 2003, 2004, 2005).
[d] The unemployment rate relates to the entire civilian labor force. Source: Bundesagentur für Arbeit (2001, 2002, 2003, 2004, 2005).
[e] Figures on Gross Domestic Product (GDP) are price-adjusted and chain-linked. Source: Arbeitskreis Volkswirtschaftliche Gesamtrechnungen der Länder (2010).

After abolishing unemployment assistance and social assistance, about 4.5 million persons registered for the newly introduced unemployment benefit II (UBII) in January 2005 (see Bundesagentur für Arbeit, 2006a). This number was considerably larger than the sum of former unemployment assistance and social assistance recipients. The reason for this increase was the changing eligibility criteria for welfare benefits prior to and after the reform.

Even though some of the former social assistance recipients did not qualify for UBII (they were not deemed able to work) and even though some unemployment assistance recipients did not pass the stricter means-test for UBII, the reform also made persons eligible who did not claim benefits before 2005. These persons mostly included partners and household members of former unemployment assistance claimants. While the relatively generous unemployment assistance put partners above the subsistence level, the lower UBII made them welfare dependent (see Kaltenborn and Schiwarov, 2006). It took several months until all eligible individuals registered for the new benefit, so that the number of UBII claimants increased steadily in the early post-reform period, producing an average of 4.9 million UBII claimants in 2005 (see table 2.3). In 2006, the average number of claimants increased to 5.4 million. Between 2006 and 2009 the average number of persons receiving welfare benefits sank to 4.9 million.

Table 2.3: Macroeconomic conditions in Germany (2005-2009)

	2005	2006	2007	2008	2009
Unemployment benefit II recipients[a]	4,982	5,392	5,277	5,010	4,908
Unemployed persons[b]	4,861	4,487	3,777	3,268	3,423
Unemployment rate (in %)[c]	11.7	10.8	9.0	7.8	8.2
GDP growth (in %)[d]	0.8	3.2	2.5	1.3	-5.0

[a] Figures are in the thousands and measured as the average annual stock of persons receiving unemployment benefit II. Source: Bundesagentur für Arbeit (2006a, 2007a, 2008a, 2009a, 2010a).
[b] Figures are in the thousands and measured as the average annual stock of persons registered unemployed. Source: Bundesagentur für Arbeit (2006a, 2007a, 2008a, 2009a, 2010a).
[c] The unemployment rate relates the number of persons registered unemployed to the entire civilian labor force. Source: Bundesagentur für Arbeit (2006a, 2007a, 2008a, 2009a, 2010a).
[d] Figures are price-adjusted and chain-linked. Source: Arbeitskreis Volkswirtschaftliche Gesamtrechnungen der Länder (2010).

The number of persons registered unemployed also increased significantly immediately after the reform, rising from 4.38 million in 2004 to 4.86 million in 2005 (see table 2.3). This increase was mainly due to a more precise statistical recording of unemployed persons (see Bundesagentur für Arbeit, 2006a). Before 2005, not all unemployed social assistance recipients were registered at the local employment offices (LEOs). The same is true for partners and household members of former unemployment assistance recipients who had often been inactive. These people began to appear in official statistics in 2005, when welfare reform made UBII receipt conditional on being registered as a job seeker.

After the statistical increase in 2005, the number of registered unemployed decreased considerably from 2006 (4.49 million) to 2008 (3.27 million). This decrease was promoted by a favorable macroeconomic development. GDP grew by 3.2% in 2006 and by 2.5% in 2007. At the end of 2008, the banking

and financial crisis set in, causing a 5% decline in GDP in 2009. Unemployment increased only slightly due to, among other reasons, a large-scale use of short-term work (see Bundesagentur für Arbeit, 2010a).

Table 2.4 provides some more information on the German labor market and the population of welfare recipients in the post-reform period. As can be seen from the table, ca. 54 million of Germany's resident population of about 82 million are of working age, i.e. between 15 and 64. During the observation period, both of these figures declined slightly, while between 2005 and 2008 the number of persons working increased on account of favorable macroeconomic conditions. In 2005, the number of persons in the workforce amounted to 38.8 million; in 2008, it amounted to 40.3 million. Accordingly, the number of persons in employment subject to social insurance contributions increased from 26.2 million to 27.5 million. After the onset of the banking and financial crisis, both employment figures decreased slightly in 2009.

With respect to the number of persons in working age, the number of persons receiving UBII – 5 million – is quite substantial. Nearly 10% of the working age population is welfare dependent. In addition to the UBII recipients, almost 2 million people receive social allowance (mostly children less than 15; see section 2.1). Taken together, nearly 7 million people in Germany are welfare dependent. This is a share of more than 10% of the total population age less than 65. Even though this share declined to some extent beginning in 2006, it is larger than the share of UBII recipients of the working age population. This indicates that children under the age of 15 are particularly over-represented in the welfare system. An effective labor market activation is thus not only beneficial for activated adult welfare recipients; it is also crucial for their offspring, who may suffer from long-lasting welfare spells with potentially adverse effects on educational attainment and future labor market performance.[16]

In this regard, it is positive to note that the absolute number of young welfare dependent persons age 15 to 24 and their share of all UBII recipients have declined steadily from 2006. This reduction might be due to the fact that persons under the age of 25 are a special target group of welfare agencies. According to chapter 3 of Book II of the German Social Code (*Sozialgesetzbuch Zweites Buch, SGB II*), UBII recipients under the age of 25 must be placed into employment or vocational training as quickly as possible after entering the welfare system to prevent deterioration of human capital and employability. The declining share of young UBII recipients, however, comes at the expense of the elderly welfare recipients age 50 or above. While this group

[16] Long-lasting welfare spells are rather common. According to Bundesagentur für Arbeit (2010c), the average completed duration of a welfare spell amounted to 20 months for persons who left welfare in December 2008. The median value was 13 months. Thus, a majority of all individuals who left the welfare system in December 2008 were receiving welfare benefits for more than one year. About a quarter of these individuals were on welfare for three years or longer. The elapsed duration of welfare receipt measured for the stock of welfare recipients in December 2008 amounted to 29 months on average and to 34 months at the median level.

Table 2.4: Selected figures on Germany's population and welfare recipients (2005-2009)

	2005	2006	2007	2008	2009
Population[a]					
Resident population	82,438	82,315	82,218	82,002	81,802
Resident population age below 65	66,568	66,016	65,699	65,273	64,901
Resident population age 15 to 64	54,918	54,574	54,417	54,134	53,878
Working population age above 14	38,846	39,097	39,724	40,279	40,265
Number of persons employed[b]	26,178	26,354	26,855	27,458	27,380
Number of foreigners	7,289	7,256	7,255	7,186	7,131
Share of foreigners in resident population (in %)	8.8	8.8	8.8	8.8	8.7
Welfare recipients[c]					
UBII recipients	4,982	5,392	5,277	5,010	4,908
Social allowance recipients	1,774	1,955	1,964	1,897	1,817
Welfare dependent persons	6,756	7,347	7,241	6,907	6,725
UBII ratio (in %)[d]	9.1	9.9	9.7	9.3	9.1
Welfare ratio (in %)[e]	10.2	11.1	11.0	10.6	10.4
Composition of UBII recipients[f]					
Share of persons age 15 to 24 (in %)	20.7	20.8	19.7	19.1	18.6
Share of persons age 25 to 49 (in %)	59.1	58.7	58.4	57.4	57.1
Share of persons age 50 to 64 (in %)	20.3	20.5	22.0	23.5	24.3
Share of women (in %)	48.9	49.6	50.7	51.4	51.1
Share of persons unemployed (in %)	55.6	52.4	46.0	43.7	43.8
Share of foreigners (in %)	19.2	18.7	18.5	19.0	19.4

[a] All figures on Germany's population are in the thousands unless stated otherwise. Source: Bundesagentur für Arbeit (2006a, 2007a, 2008a, 2009a, 2010a), Statistisches Bundesamt (2010) and information of *Statistisches Bundesamt* at http://www.destatis.de (accessed November 1, 2010, and July 11, 2011).

[b] The figures refer to employment subject to social insurance contributions.

[c] All figures on welfare recipients are in the thousands unless stated otherwise. Source: Bundesagentur für Arbeit (2006a, 2007a, 2008a, 2009a, 2010a) and own calculations.

[d] The UBII ratio relates the number of UBII recipients to the resident population age 15 to 64.

[e] The welfare ratio relates the number of all welfare recipients (UBII and social allowance) to the resident population below 65.

[f] All figures are measured as the percentage of the respective group out of all UBII recipients. The figures on age might not add up to 100% due to rounding errors. Source: Bundesagentur für Arbeit (2006a, 2007a, 2008a, 2009a, 2010a) and own calculations.

accounted for 20.3% of all welfare recipients in 2005, they formed nearly a quarter of the welfare dependent population in 2009.

The share of women among the UBII recipients also increased during the observation period, rising from 48.9% in 2005 to 51.1% in 2009. An increasing share of welfare recipients was formed by persons who are employed but whose earnings are insufficient to cover living expenses. While in 2005 about 55% of welfare recipients were unemployed, this figure dropped to less than 44% in 2009. The reasons for this increase in people employed but dependent on welfare have yet to be uncovered (see Bundesagentur für Arbeit, 2010b).

Several explanations are possible. Labor market integration of unemployed welfare recipients might be only partially successful and concentrated on low-paying jobs so that additional welfare payments are needed even after employment uptake. The post-reform setting of the welfare system could also be exploited by employers to reduce wages so that employees are forced to claim supplementary welfare benefits to cover their living expenses.

A relatively stable share of the welfare population is formed by foreigners. From 2005 to 2009, about 19% of welfare recipients had a non-German citizenship. With this share, foreigners were highly over-represented in welfare. In the resident population, only about 8.8% of people were foreigners during those years. When counting not only foreigners but also persons who were born abroad or who have at least one parent born abroad, the Bundesministerium für Arbeit und Soziales (2009) reports that 34.8% of all welfare recipients in 2007 were immigrants. In the resident population, only 18.6% of persons had a migration background in 2005 (see Statistisches Bundesamt, 2007).[17] Despite this over-representation, immigrants do not represent a special target group of welfare agencies such as welfare recipients under the age of 25. Book II of the German Social Code lacks a legal definition of the term migration background and does not include any activation measure specifically designed and used for immigrants. Instead, immigrants are assigned to the standard measures available for all welfare recipients.

The most frequently used activation measures are Temporary Extra Jobs (*Arbeitsgelegenheiten in der Mehraufwandsvariante*) and short-term training programs (*Trainingsmaßnahmen*). Table 2.5 depicts the number of participants entering these programs from 2005 to 2009. With more than 700,000 participants per year, Temporary Extra Jobs are clearly the most common program. Short-term training programs are used somewhat less frequently but the number of participants is still large. It reached a maximum in 2008,

[17] The definition of migration background differs slightly between the Bundesministerium für Arbeit und Soziales (2009) and the Statistisches Bundesamt (2007), but these different definitions cannot account for the different shares of immigrants in the resident population and in the welfare system. According to the Statistisches Bundesamt (2007), persons with a migration background include all men and women who were born abroad and immigrated to Germany after 1949, all persons who were born in Germany but do not possess German citizenship, and all persons with German citizenship born in Germany who have at least one parent born abroad or born in Germany as a foreigner. As stated above, the Bundesministerium für Arbeit und Soziales (2009) considers those persons to have a migration background who are foreigners or who were born abroad or who have at least one parent born abroad. Thus, the definition of Bundesministerium für Arbeit und Soziales (2009) does not restrict the immigration date and does not consider persons to have a migration background who are German citizens born in Germany with at least one parent born in Germany as a foreigner. Unfortunately, neither the Statistisches Bundesamt nor the Bundesministerium für Arbeit und Soziales survey the number of immigrants in detail on a regular basis. Hence, the figures of both institutions can only be reported for 2007 and 2005, respectively, when the latest detailed surveys were conducted.

when nearly 630,000 welfare recipients participated in short-term training.[18] In addition to the number of program participants, table 2.5 indicates the number of individuals who were sanctioned due to non-compliance during the activation process. More than 600,000 welfare recipients received a benefit cut each year. This number shares a similar magnitude with the number of individuals who participate in Temporary Extra Jobs or short-term training programs.

Table 2.5: Number of participants in Temporary Extra Jobs and short-term training programs, and number of sanctions (2005-2009)

	2005	2006	2007	2008	2009
New participants in Temporary Extra Jobs[a]	604,062[b]	775,866	774,893	764,212	719,232
New participants in short-term training programs[a]	410,884[b]	446,641	548,024	627,739	256,656[c]
Newly imposed benefit sanctions[d, e]	-	-	784,385	764,912	732,648
Number of sanctioned individuals[e]	-	-	625,708	649,075	640,617

[a] Figures obtained from Bundesagentur für Arbeit (2006a, 2007a, 2008a, 2009a, 2010a).
[b] Number refers to centralized welfare agencies only.
[c] Number is not comparable with the other years. On January 1st, 2009, a legislation amendment introduced chapter 46 into Book III of the German Social Code (*Sozialgesetzbuch Drittes Buch, SGB III*) which is the legal basis of most ALMP in Germany. This chapter subsumed short-term training programs and other ALMP under a new label (*Maßnahmen zur Aktivierung und beruflichen Eingliederung*) and changed the statistical recording of program participations. The statistics of the FEA do not allow single programs to be differentiated from the new label. According to Bundesagentur für Arbeit (2010a), 550,777 welfare recipients participated in programs labeled as *Maßnahmen zur Aktivierung und beruflichen Eingliederung*. The reported number of 256,656 participants in short-term training refers to individuals who started a program in the transition period until the legislation amendment was completely enforced.
[d] Figures count all sanctions imposed on welfare recipients including those for repeated incidents of non-compliance.
[e] Source: Statistics of the Federal Employment Agency at http://statistik.arbeitsagentur.de (accessed November 1, 2010). Figures for 2005 and 2006 are not available.

Finally, table 2.6 summarizes the expenditures on welfare in Germany from 2005 to 2009. Since both UBII and social allowance are only granted if the resources of the entire household are insufficient to cover living expenses, the table also includes information on the number and size of households dependent on welfare. In total, there were about 3.7 million households on average claiming welfare benefits during the observation period. The average household size was 1.8 persons in 2005 and 1.9 persons in 2009. Households received on average monthly welfare payments of 838 euros in 2005 and 850 euros in 2009. These payments included the base payment of UBII, social allowance, housing costs, social insurance contributions and expenses for additional needs. While the amount of social insurance contributions declined

[18] No reliable figures are available for 2009 due to changes in statistical recording.

over the years, payments for housing costs increased from 274 euros in 2005 to 320 euros in 2009.

Table 2.6: Expenditures on welfare in Germany (2005-2009)

	2005	2006	2007	2008	2009
Households on welfare					
Number of households on welfare[a]	3,717	3,979	3,725	3,576	3,559
Average size of households on welfare[b]	1.8	1.8	1.9	1.9	1.9
Average monthly payment to a household on welfare[c]					
Total payment	838	854	818	822	850
Base payment of UBII	340	342	341	338	343
Social allowance	14	14	15	15	18
Housing costs	274	295	308	311	320
Social insurance contributions	206	198	150	154	163
Expenses for additional needs	4	4	4	4	5
Expenditures on welfare[d]					
Spending for the base payment of UBII, social allowance and housing costs	32.8	34.7	31.5	30.2	31.1
Spending for ALMP	3.1	3.8	4.2	4.7	5.0
Administrative costs	2.6	3.1	3.2	3.3	3.7
Total expenditures	38.5	41.6	38.9	38.2	39.8

[a] All figures are in the thousands. Source: Bundesagentur für Arbeit (2006a, 2007a, 2008a, 2009a, 2010a).

[b] The household size is measured as the number of all welfare dependent persons living together in one household.

[c] All numbers are given in euros. Source: Bundesagentur für Arbeit (2006a, 2007a, 2008a, 2009a, 2010a).

[d] The numbers on expenditures are given in billions of euros and refer to centralized welfare agencies only. Comparable data for decentralized agencies is not available for the entire period from 2005 to 2009. Source: Bundesagentur für Arbeit (2006b, 2007b, 2008b, 2009b, 2010b).

On an aggregate level, centralized welfare agencies spent 32.8 billion euros on the base payment of UBII, social allowance, and housing costs in 2005. With 3.1 billion euros in expenditures on ALMP and 2.6 billion euros in administrative costs, the total spending on welfare amounted to 38.5 billion euros in that year. Along with the number of welfare recipients, total spending increased in 2006 to 41.6 billion euros. After this peak, expenditures declined in 2007 and 2008 but began to rise again in 2009 (39.8 billion euros). While spending for the base payment of UBII and social allowance was lower in 2009 (31.1 billion euros) than in 2005 (32.8 billion euros), expenditures for ALMP and administrative costs were considerably larger. Administrative costs increased by more than 40% (from 2.6 billion euros in 2005 to 3.7 billion euros in 2009) and spending on ALMP by more than 60% (from 3.1 billion euros in 2005 to 5.0 billion euros in 2009). According to Bundesagentur für Arbeit (2009b and 2010b), the increase in spending on ALMP

was due to a more frequent assignment of programs, while the rise in administrative costs resulted mainly from increased labor costs in welfare agencies and from increasing expenditures on external services, e.g. on technical maintenance and public relation services.[19]

2.3 Data Sources

In order to evaluate thoroughly the effectiveness and efficiency of labor market activation of welfare recipients, comprehensive and high-quality data on individual basis are needed. Before 2005, no appropriate data were available. In particular, there was a lack of information on social assistance recipients. Data on these persons were not collected systematically; it was collected only in small surveys, if at all. Information on unemployment assistance recipients was richer because the Federal Employment Agency (FEA) sampled these persons in administrative records. After the 2005 welfare reform, administrative data coverage was expanded to all welfare recipients. Yet the disruptions caused by the reform created considerable problems for the quality of administrative data during several months after the introduction of the reform. Reliable information did not become available until 2006. This thesis thus relies on data from 2006 and after. But since centralized and decentralized welfare agencies do not report their data in the same way, this thesis also makes use of comprehensive surveys at individual and agency levels. The data sources are described in this subsection.

All local employment offices (LEOs) and welfare agencies in Germany must collect data on the sociodemographic characteristics of their clients (unemployment insurance benefit and welfare recipients) and the activation measures applied. These data are used for administrative purposes and are transferred monthly to the FEA for statistical reporting. In addition, employers are required to submit individual information on all employees who are subject to social insurance contributions or who have minor employment (*geringfügige Beschäftigung*) on a yearly basis. The FEA compiles the incoming information into different data sources, described as follows (see Dorner et al., 2010):

- **Employee History** (*Beschäftigten-Historik*, BeH): The Employee History contains all employment spells of an individual provided that employment

[19] Unfortunately, there is no comprehensive data available on the expenditures of decentralized welfare agencies for the period from 2005 to 2009. According to Bundesagentur für Arbeit (2008b), overall expenditures on welfare (including centralized and decentralized welfare agencies) amounted to 44.4 billion euros in 2005, to 49.1 billion euros in 2006, and to 45.3 billion euros in 2007. This information is missing for 2008 and 2009.

is subject to social insurance contributions or considered minor employment.

- **Benefit Recipient History** (*Leistungsempfänger-Historik*, LeH): The Benefit Recipient History documents all periods during which an individual received insurance based benefits from the FEA. These benefits include unemployment insurance benefits and, prior to 2005, unemployment assistance.[20]
- **Welfare Recipient History** (*Leistungs-Historik-Grundsicherung*, LHG and XLHG, respectively): The Welfare Recipient History covers all spells of UBII and social allowance receipt. The method of welfare recipient data collection changed once welfare administration was divided into centralized and decentralized welfare agencies. While the centralized agencies use the standard software system of the FEA, each decentralized agency applies its own software solution to collect data on their clients. Even though the decentralized agencies are required to report their data in a standardized way to the FEA, the content of the data differed considerably from the data of centralized agencies in the early post-reform period.[21] The FEA has since tried to harmonize the information, but the data of centralized and decentralized welfare agencies are still not identical which is why they are stored in two different data sets. The LHG contains the data of centralized agencies and the XLHG contains the data of decentralized agencies.[22]
- **Jobseeker History** (*Arbeitsuchenden-Historik*, ASU and XASU, respectively): The Jobseeker History provides information about periods of job search including job search during spells of unemployment but also job search on the job. Moreover, it includes periods of up to six weeks during which an individual was not available to the labor market due to illness. Since 2005 the information of the jobseeking history of welfare recipients has been collected in a different way by centralized and decentralized agencies. Despite efforts to harmonize the information, it still differs in a number of characteristics and is stored in two separate data sets. In the case of

[20] In addition, prior to 2005, the LeH documented all spells of maintenance allowance (*Unterhaltsgeld*) receipt. Maintenance allowance could be paid to participants in ALMP.

[21] Differences in the data did not occur with respect to the quality of the reported start and end dates of individual welfare spells but rather with respect to the structure of additional information on the welfare spells. For example, centralized welfare agencies reported in detail on the welfare payments associated with a spell separately for base payment of UBII, social allowance, housing costs, social insurance contributions and other expenses for additional needs, while decentralized agencies reported only an aggregate figure for all payments. The level of detail also varied with respect to sociodemographic variables as, for instance, family status (see ZEW et al., 2007 and 2008).

[22] The first letter in the name of the XLHG data set refers to the interface XSozial, used by decentralized welfare agencies to report their data.

centralized agencies, the information is stored in the ASU data set; with decentralized agencies, it is stored in the XASU data set.[23]

- **Program Participation History** (*Maßnahme-Teilnehmer-Historik*, MTH and XMTH, respectively): The Program Participation History lists all spells of participation in Active Labor Market Programs (ALMP). Since 2005, data for welfare recipients are collected separately by centralized (MTH data set) and decentralized agencies (XMTH data set). The data sets differ greatly (see ZEW et al., 2008). Names and labels of documented programs are not identical in MTH and XMTH. The classification of programs is much more detailed in centralized agencies. Yet the broader categories in XMTH are not an aggregation of the finer MTH categories. Some programs reported by centralized welfare agencies are not documented by decentralized welfare agencies, and some program names in XMTH do not match with programs reported in MTH. In general, program participation seems to be underreported in XMTH.

All data sets provide information on a daily basis, i.e. all spells of employment, unemployment, job search, benefit receipt and program participation are documented with their start and end date. Moreover, all data sets include a number of variables describing further characteristics associated with the respective spell (see Oberschachtsiek et al., 2009; and Dorner et al., 2010). The Employee History (BeH), for example, includes information on sex, age, nationality, family status, place of residence, professional qualification, profession, occupational status, industrial sector, and wage. The Benefit Recipient History (LeH) and the Welfare Recipient History (LHG and XLHG) provide information on type and amount of benefits received.[24] Moreover, the latter data set contains detailed information on household composition. The Jobseeker History (ASU and XASU) includes information on schooling of the job seekers and their desired form of employment. It also documents disabilities acting as potential obstacles to employment. The Program Participation History (MTH and XMTH) covers the type of the programs participated in as well as their planned and actual duration.

The FEA merges all five data sources in order to create a single data set that contains all documented labor market spells of an individual in chronological order. This single data set is referred to as **Integrated Employment**

[23] As in the case of LHG and XLHG, differences between ASU and XASU do not relate to the quality of reported start and end dates of job search spells but rather to the structure of additional information on the spells. Job search spells seemed to be underreported in XASU in the early post-reform period (see ZEW et al., 2008).

[24] As noted above, the information on benefits in LHG and XLHG differs in its level of detail. In both data sets, the reported figures are unreliable in several cases (see ZEW et al., 2007). For example, in some spells, negative welfare payments are reported, and in other spells, the documented figures are either implausibly high or implausibly low. The monetary values contained in the LeH and BeH data sets are similarly unreliable. For this reason, I do not make use of monetary values reported in the administrative data.

Biographies (*Integrierte Erwerbsbiografien*, IEB). For reasons of technical manageability, not all variables of the underlying five data sources are integrated into the IEB data set. But it is possible to combine each IEB spell with the original spell in order to add more detailed information.

The IEB data set is widely used for the evaluation of ALMP targeted at unemployment insurance benefit recipients (see Wunsch and Lechner, 2008; and Fitzenberger et al., 2010). Due to its large size, the long time period it covers, and the detailed information for various labor market states, it is often seen as more advantageous than survey data sets such as the German Socio-Economic Panel (GSOEP). With regard to welfare recipients, however, the IEB data set is not without shortcomings. As noted above, the division of welfare administration in 2005 also led to different data collection approaches by centralized and decentralized welfare agencies. The centralized agencies use the standard FEA software that is also used in the LEOs to collect data on unemployment insurance benefit recipients. The use of the standard software made it relatively easy to integrate data of welfare recipients on job search and program participation into the Jobseeker History ASU and the Program Participation History MTH, respectively. Along with the LHG, data on job search and program participation of welfare recipients registered at centralized agencies could thus be integrated quite early into the IEB data set, even though there were adjustment problems in the early post-reform period.

By contrast, decentralized welfare agencies use their own data collection systems with the result that XLHG, XASU, and particularly XMTH differ in many respects from LHG, ASU, and MTH, respectively. Thanks to the immense efforts to harmonize the data, XLHG and XASU could be integrated into the IEB in June 2009. Yet these data sets are still not identical with their counterparts from centralized agencies. The program participations documented in XMTH have yet to be integrated into the IEB. Thus, neither the IEB data set nor the underlying data sources provide a consistent and comprehensive basis of information on all welfare recipients in Germany.[25]

This data restriction has crucial implications for the empirical analyses of this thesis. For the first empirical analysis – whether centralized or decentralized welfare agencies are more successful at integrating welfare recipients into self-sufficient employment – it means that administrative records cannot be used as the only data source. Comparable data on the characteristics of welfare recipients in centralized and decentralized welfare agencies are needed as well. Since such comparable information about the characteristics of welfare

[25] It is again important to note that the reported start and end dates of spells in XLHG and XASU can be considered as reliable except for the early post-reform period. Yet the structure of additional information on the spells is not identical with information in LHG and ASU. XMTH cannot be considered as a reliable data source. Program participations of welfare recipients registered at decentralized welfare agencies are likely to be underreported, and the documented information differs widely from the information in the MTH data set (see ZEW et al., 2008).

recipients could only be assured on the basis of a survey, it was decided to build up a new data base and to use the administrative data as additional information only. At the heart of this new data base is a large-scale survey of almost 25,000 welfare recipients in centralized and decentralized welfare agencies. The survey data can be combined with administrative records at the individual level. In addition, surveys were conducted at the agency level to gather information on internal organization and strategies of welfare agencies including information on the use of benefit sanctions within welfare agencies. The agency information can be combined with the individual survey data as well. The data base thus provides information for studying the relative success of centralized and decentralized welfare agencies and the effectiveness of benefit sanctions with respect to employment uptake of welfare recipients. Subsection 2.3.1 gives a detailed description of this data base used in chapters 3 and 4. Table A.2.1 in the appendix to this chapter summarizes its main characteristics.

As for the evaluation of the effectiveness and efficiency of ALMP targeted at welfare recipients, the restriction of the administrative data implies that only program participants registered at centralized welfare agencies can be considered on the basis of these data. Welfare recipients registered at decentralized agencies must be excluded from the analysis because of missing or incomplete information in XMTH. Of course, program evaluation could also be based on survey data for centralized and decentralized agencies. But the administrative data are advantageous for the evaluation of ALMP. Due to their large size, they enable a detailed evaluation of ALMP with respect to different program types and subgroups of welfare recipients, particularly immigrants. Moreover, administrative data provide a more accurate recording of start and end dates of assigned programs than self-reported survey data. The analysis in chapter 5 is based on administrative data with the IEB data set as the main source. The fiscal cost-benefit analyses in chapter 6 build directly on the results of chapter 5 and are thus based on the same administrative data. Yet since the administrative data lack reliable information on post-welfare wages of successfully integrated individuals, chapter 6 makes also use of the GSOEP to estimate the fiscal benefits of an integration into employment after a successful participation in ALMP. Subsection 2.3.2 describes the data base used in chapters 5 and 6 in more detail. Its main characteristics are summarized in table A.2.1 in the appendix to this chapter.

2.3.1 Data Base Used in Chapters 3 and 4

To evaluate the relative success of centralized and decentralized welfare agencies and to analyze the effectiveness of benefit sanctions, a unique data base has been created that combines individual information on welfare recipients with information on the welfare agencies at which the individuals are regis-

tered.[26] The data are confined to 154 agencies, a subset of all 439 German welfare agencies. Of the sample agencies, 51 exhibit a decentralized organization. The remaining 103 agencies have a centralized organization. They were selected to obtain regional units with local labor market conditions comparable to the 51 decentralized agencies. When choosing comparable agencies, this thesis is built directly on the previous work of Arntz et al. (2006). Based on comprehensive information on the situation of regional labor markets prior to 2005 (before the reform took place), the authors determine comparable welfare districts across Germany in a microeconometric analysis combining individual and regional data. In a first step, the authors identify regional variables that are, in addition to personal characteristics, relevant to the transition of long-term unemployed individuals into employment. Then, in a second step, the authors use the reduced set of relevant regional variables and apply the distance matching suggested by Zhao (2004) to identify comparable districts in a regional matching procedure. Figure A.2.1 in the appendix to this chapter illustrates the regional location of the sampled welfare agencies.[27]

To obtain further information on the internal organization and the strategies of the sampled welfare agencies, case studies and management surveys were conducted in the agencies. These studies and surveys delivered information for the year 2006 about the type of case management, the activation concept, the placement approach, the mix of ALMP and many other organizational and strategical aspects, including the use of benefit sanctions within welfare agencies. In addition, a wide range of regional variables (e.g. unemployment ratio, GDP per employed person, rate of social assistance recipients) were collected for each welfare district on a monthly basis for the pre-reform period from 1999 to 2004.

The individual part of the data base consists of a repeated survey of welfare recipients registered at the 154 agencies. These individuals were randomly sampled from administrative records of the FEA. The sample is stratified according to the following characteristics: age (15 to 24 years, 25 to 49 years, 50 to 64 years), single parent status, and children younger than three years living in the household. This stratification is made to ensure that the number of observations is sufficiently high for these groups. Per household, only one welfare recipient was drawn. In the survey, two computer-assisted telephone interviews were conducted at the beginning of 2007 (January to April, first wave) and about one year later (November 2007 to March 2008, second wave). Depending on the size of the welfare agency, 100 to 300 telephone interviews with welfare recipients were conducted within each agency in the first wave.

[26] Parts of this data base have been made publicly available as a scientific use file at the Federal Employment Agency (FEA). See Oertel et al. (2009) for details on data content and access.

[27] The sampled welfare agencies provide a fairly representative picture of all welfare agencies in Germany, in particular with respect to internal organization, activation strategies, and the composition of welfare recipients registered at the agencies (see IAW and ZEW, 2006).

In total, 24,563 interviews were realized. About 80% (ca. 20,300) of the individuals interviewed were drawn from the stock of welfare recipients receiving welfare benefits in October 2006 (stock sample), whereas 20% (ca. 4,300) of the interviews are from an inflow sample of persons who entered the welfare system between August and December 2006.

Despite 93% of interviewees agreeing in the first wave to participate in the follow-up interview, attrition was high. Mainly due to relocation problems and refusal to participate, the second wave yielded 13,497 panel cases only. To compensate these losses, a refreshment sample of 7,086 cases was drawn from the same populations as the original samples (5,736 persons from the stock sample and 1,350 from the inflow sample). The participants of the refreshment sample had to answer retrospective questions to make up for the information collected from the panel cases in the first wave.[28]

The survey data include individual characteristics (e.g. sex, age, marital and parent status, education, health and disability status), information on the members of the household (number and age of household members and interviewees' relation to them), and details concerning the labor market status, labor market history, and labor market activation (current labor market state, former spells of employment subject to social insurance contributions, former spells of minor employment, former spells of unemployment, receipt of welfare benefits, participation in ALMP). They also contain information about basic skills (e.g. reading, writing, math and computer skills), other qualifications (e.g. driver's license), job search activities, the concessions respondents would be willing to make to obtain a new job, and information about benefit sanctions. Information about sanctions is used in chapter 4 and is described in more detail there. Finally, the data contain comprehensive information on the migration background of interviewees and their parents, allowing for a detailed definition of immigrants. In the analyses of chapters 3 and 4, immigrants are thus categorized into three groups:

1) Individuals with non-German citizenship,
2) German citizens who were born abroad and who have at least one parent also born abroad, and
3) German citizens who have at least one parent born abroad and who use a foreign language as the main language within the family circle.

In addition to the survey data, administrative data on the interviewed persons supplemented the individual information. The administrative data were provided by the FEA in terms of the IEB data set and the source files (X)LHG, (X)ASU and (X)MTH.[29] The version of the IEB used in this thesis

[28] The data contain individual sample weights that take into account both stratification and attrition.

[29] In this thesis, version V7.02 of the IEB is used (release: December 2008). This version does not include XLHG, XASU and XMTH. This is why these data sets were ordered separately. As noted above, for technical manageability, the IEB data set

covers employment spells on the basis of BeH from January 1990 to December 2007. Information on unemployment insurance benefit receipt derived from LeH has the same start date but ends somewhat later, in October 2008. ASU and MTH contain spells from January 2000 to September 2008. All other data sets (LHG, XLHG, XASU, XMTH) start in January 2005 and end in October 2008. The administrative data sets have a personal identifier, which is also used in the survey data. Survey and administrative data can thus be combined via this identifier for those persons who explicitly agreed with this combination in the interview. This allows the addition of further information on the labor market history to the rich survey data.

The administrative data also allow one to construct the outcome variable of interest, namely self-sufficient employment. This variable is measured as a binary indicator on a monthly basis through December 2007. It takes on the value 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0.[30]

2.3.2 Data Base Used in Chapters 5 and 6

For the empirical analyses in chapters 5 and 6, a sample of all inflows into welfare in Germany from January 1, 2006 to December 31, 2006 is used. This allows one to construct a data base that is large enough to look at different ALMP and at different subgroups, in particular, persons with a migration background. The data on the inflows stem from administrative records of the FEA and refer to centralized welfare agencies only. To ensure that inflows in the data are not short-term recurrences of welfare episodes – due, say, to false reporting or data errors – only those persons are considered who have not been registered in welfare for three months before the sampling date.

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does not contain all variables from LHG, ASU and MTH, respectively. However, all spells in the IEB can be merged with the original information. In order to be able to supplement the information in the IEB, the data sets LHG, ASU and MTH were ordered separately in addition.

[30] Employment and welfare status are measured on the first day of each month. As has been noted above, the information in LHG and XLHG on start and end dates of welfare spells is reliable. These data sets can thus be used for the construction of reliable outcome variables. The employment status is reported by employers and, thus, not subject to the different data collection approaches by centralized and decentralized welfare agencies. Note, however, that the outcome variable only considers employment subject to social insurance contributions. It does not include spells of minor employment or self-employment. The definition of the outcome variable does not preclude employers from receiving wage subsidies for hiring. Yet since German wage subsidies were found to give rise to huge deadweight effects (see Boockmann et al., forthcoming), I neglect the distinction between subsidized and non-subsidized hiring.

All considered individuals originate from different households, i.e. only one welfare recipient was drawn per household.

To obtain information on the sampled individuals, different administrative data sets are used. The main source is the IEB data set, which provides comprehensive information on welfare recipients with regard to sociodemographic characteristics, labor market history, and participation in ALMP.[31] The detailed data allow the differentiation of short-term training programs and Temporary Extra Jobs. These will be evaluated in chapters 5 and 6.

The analyses of chapters 5 and 6 explicitly distinguish between immigrants and native Germans. In these chapters, immigrants are defined to comprise all foreigners and naturalized persons. Foreigners are persons who do not possess German citizenship. The naturalized group contains German resettlers (*Aussiedler/Spätaussiedler*) from the former Soviet Union and Eastern Europe and naturalized foreigners. Although citizenship is recorded in the IEB, identification of naturalized foreigners and German resettlers could only be carried out partially from this data set. To comprehensively identify resettlers, additional information of an extended version of the Jobseeker History (ASU) – dating back to 1990 – is considered, which contains resettler status. For the identification of naturalized foreigners, information is used from the IEB for the years 1990 to 2006 and from an extended version of the Employee History (BeH) for the years 1975 to 1989. A person with German citizenship at the time of inflow into the welfare system in 2006 who was recorded being a foreigner in any spell since 1975 is treated as a naturalized foreigner. Unfortunately, the administrative records of the FEA contain neither information about the place of birth nor about the parents of the individual. Moreover since minors (persons under 15 years of age) appear neither in the IEB nor in the BeH, it is impossible to identify immigrants who were naturalized at an early age and to distinguish between first and second generation immigrants. The remaining group of persons, who were not identified as immigrants and are labeled as native Germans, might thus include some persons with a migration background as well.

Based on this identification strategy of immigrants, an analysis sample was drawn in a 1:1 ratio of persons with a migration background and native Germans on a regional level. In a first step, about 80,000 immigrants were randomly drawn from the total inflow population into welfare in 2006. In a second step, for each immigrant randomly drawn from a welfare district one native German was drawn from the same district, resulting in an overall sample of about 160,000 welfare recipients. Due to the sampling procedure, immigrant-native German ratios are balanced across welfare agencies and should mitigate regional imbalances in the distribution of immigrants.

Like those in chapters 3 and 4, the analyses in chapters 5 and 6 focus on the binary outcome variable self-sufficient employment. Yet since programs are evaluated that start in 2006 and 2007, the IEB data set and its source

[31] Like the data base described in subsection 2.3.1, version V7.02 of the IEB is used.

files are not appropriate for the construction of this outcome measure because of their limited time horizon. Instead, another data set of the FEA is used, the so-called **Verbleibsnachweise** (VbN). The VbN data set for labor market states of individuals is more up to date than the IEB. It allows one to construct the outcome variable on a monthly basis from the sampling date through July 2008.[32]

It has to be noted, though, that due to delays in reporting by employers information available in the VbN has up to a two-year time lag. In a first step, the FEA forecasts the information included in the VbN and in a second step replaces the forecast by the actual reported information. As a result, assessing contemporary effects of ALMP is possible but based purely on forecasted employment information. As the evaluation of program effects should be based on reported rather than forecasted information, the observation period for the analyses in chapters 5 and 6 ends in July 2008. Data were extracted in February 2009. As the time span between July 2008 and the date of data extraction from the VbN for the analyses amounted to only seven months, the relation between reported and forecasted data was extensively checked. The share of forecasted data used in the analyses amounts to between 4% and 10% at most. See Fröhlich et al. (2004) for more information on the relation of forecasted and reported data in the VbN.

What is lacking in the administrative data is reliable information on wages. In particular, there is no reliable information on wages individuals earn after leaving welfare. Since this information is crucial for fiscal cost-benefit analyses of Temporary Extra Jobs and short-term training programs, chapter 6 also makes use of the German Socio-Economic Panel (GSOEP). The GSOEP is a large, nationally representative longitudinal data set that has been surveying households and individuals in Germany on a yearly basis since 1984 (see Haisken-DeNew and Frick, 2005; and Wagner et al., 2007). In this thesis, the waves from 2006 to 2008 are considered, with about 20,000 individuals in 11,000 households sampled in each year. Along with detailed sociodemographic information, the GSOEP surveys the labor market status and the monthly wage of interviewees. Based on this self-reported information, it is possible to estimate post-welfare wages of former welfare recipients. In turn, the estimated wages allow one to determine fiscal benefits of participation in ALMP and thus provide – together with information on program costs obtained from statistics of the FEA – the necessary ingredients for cost-benefit analyses. Chapter 6 will describe the conceptual framework for these analyses in more detail.

[32] The definition of the outcome variable constructed on the basis of the VbN data set is identical to the one described in subsection 2.3.1. Again, employment and welfare status are measured on the first day of each month.

Chapter 3

Centralized versus Decentralized Welfare Administration*

3.1 Introductory Remarks

In this chapter I consider the first key characteristic of Germany's 2005 welfare reform and evaluate whether a centralized or a decentralized organization of welfare administration is more effective at integrating welfare recipients into self-sufficient employment. As has been noted in the previous chapter, the majority of Germany's 439 welfare districts have adopted a centralized organization, while 69 districts have opted for decentralized welfare agencies. Theoretical arguments in favor of a decentralized organization are based on the idea that local authorities are better informed about the characteristics of the local labor market. They are assumed to have detailed knowledge about the specific regional attributes relevant for a successful activation process, and to be more effective in providing services that are tailored to local conditions. Centralized organizations are often considered to have an advantage in bundling resources, collecting information from various sources, and imposing best-practice strategies for their local offices (see Finn, 2000).

Given these favorable theoretical arguments for both organizational models, it is not surprising that in practice the degree of centralization of welfare administration varies considerably between countries. In the Netherlands, for example, local authorities form the basis of the public welfare system. By contrast, in the UK, public welfare administration is part of the central government structure (see Konle-Seidl, 2009). In other countries, welfare reform has changed the degree of centralization of welfare administration. The 1996 US welfare reform, for instance, devolved greater program authority from

* This chapter builds on joint work with Bernhard Boockmann, Stephan Thomsen, Christian Göbel, and Martin Huber. It is a revised and extended version of Boockmann et al. (2010). Extensions include subgroup analyses for individuals living on their own (singles) and individuals who live with at least one other person in the same household (non-singles). I also look separately at immigrants and native Germans.

the federal level to the states, and the Canadian reform that same year gave greater discretion to the provinces (see Blank, 2002).

Even though there is an increasing evaluation literature concerning the effectiveness of ALMP and certain elements of welfare reform (most of them from the US,[33] Germany,[34] and other European[35] countries), there is little data on the effects of the organization of welfare administration. One reason for this is that centralization or decentralization applies to countries as a whole, which makes it difficult to disentangle the effects of a particular organizational setting from other aspects of the welfare system or its reform. To date, conclusions have been derived from case studies only (see Tergeist and Grubb, 2006; and Lindsay and McQuaid, 2008).

Germany's welfare reform of 2005 allows to fill this research gap. The reform introduced two competing types of organization – a centralized and a decentralized one – in an otherwise homogenous institutional framework within one country. All components of public welfare and labor market policy – such as benefit entitlements, the tax-benefit system in general, and labor market institutions such as minimum wages and employment protection – apply equally to all of Germany's centralized and decentralized welfare agencies. Exploiting these facts, the aim of this chapter is to investigate whether a welfare recipient's chance of integration into self-sufficient employment is larger under a centralized or decentralized setting of welfare administration.

The remainder of this chapter is organized as follows. In the next section, I describe the estimation sample, while section 3.3 outlines the methodological approach. Estimation results are presented in sections 3.4 and 3.5, and conclusions are drawn in the final section of this chapter.

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[33] For a review of US welfare reforms and the related empirical literature, I refer to Blank (2002), Moffit (2002), and Grogger and Karoly (2005). Bloom and Michalopoulos (2001) synthesize the results of evaluations of 29 welfare reform initiatives in the US.

[34] Hohmeyer and Wolff (2007), Wolff and Jozwiack (2007), Bernhard et al. (2008), Wolff and Nivorozhkin (2008), Boockmann et al. (2009), Hohmeyer (2009), Kopf (2009), Aldashev et al. (2010), Thomsen and Walter (2010a), and Huber et al. (2011) have evaluated the effects of several German ALMP assigned after 2005 and obtained mixed results on employment uptake and welfare receipt depending on the particular programs and populations considered.

[35] Surveys on welfare reforms in Europe (countries other than Germany) are provided by Torfing (1999), Kildal (2001), and Halvorsen and Jensen (2004) for the Nordic countries, Finn (2000), Beaudry (2002), and Dostal (2008) for the UK, and Finn (2000), and Knijn and van Wel (2001) for the Netherlands. See also Martin and Grubb (2001) and Kluve (2010) for comprehensive overviews.

3.2 Estimation Sample

In order to investigate whether centralized or decentralized welfare agencies are more successful in integrating welfare recipients into self-sufficient employment, I use the data set described in subsection 2.3.1. I restrict the data in several ways. First, I only use information from the first wave of the survey, i.e. I do neither consider the second wave nor the refreshment sample. Second, in this chapter, my analysis is based on the stock sample. Because a large share of UBII recipients depends on welfare benefits for an extended period of time (see section 2.2), the stock sample covers those individuals for whom the organization of welfare administration matters the most. Third, since my analysis focuses on integration into employment, I restrict the sample to individuals who were unemployed at the time they entered the welfare system and at the time of sampling. Finally, I restrict the data to persons between 18 and 57 years of age. At the time of the analysis in 2006 and 2007, persons age 58 or older were no longer required to actively search for employment but could remain on welfare benefits until they reached the official retirement age of 65. Individuals age 15 to 17 years are subject to compulsory schooling and cannot be expected to take up employment. Due to these restrictions, I have 13,286 observations in my estimation sample (8,797 persons registered at centralized welfare agencies and 4,489 persons registered at decentralized agencies).

3.3 Estimation Approach

3.3.1 Estimation of the Average Effect of Treatment on the Treated

To evaluate the relative performance of centralized versus decentralized welfare administration on the individual level, I consider decentralized organization of the local welfare agency as my treatment variable and use the potential outcome approach of causality, comprehensively described by Heckman et al. (1999) and variously attributed to Splawa-Neyman (1923), Roy (1951), and Rubin (1974). Accordingly, I define two possible treatment states for individual i, with $D_i = 1$ for being registered at a decentralized welfare agency and $D_i = 0$ for being registered at a centralized welfare agency. The potential binary employment outcomes corresponding to each of the states are denoted Y_i^1 and Y_i^0. Thus, Y_i^1 is equal to 1 if individual i is registered at a decentralized agency and finds self-sufficient employment, and it is 0 if he or she fails to do so. In case the individual is registered at a centralized agency, an equivalent definition applies to Y_i^0. The observable outcome for i is given by $Y_i = Y_i^1 \cdot D_i + (1 - D_i) \cdot Y_i^0$. The individual treatment effect is defined as the

difference between the two potential outcomes. Since the individual cannot be in both states at the same time, one of the potential outcomes is unobservable, and direct estimation of the individual treatment effect is impossible. Instead, I estimate the Average Effect of Treatment on the Treated (ATT) defined as

$$ATT = E(Y^1 - Y^0|D = 1) = E(Y^1|D = 1) - E(Y^0|D = 1). \tag{3.1}$$

The term $E(Y^0|D = 1)$ on the right-hand side of equation (3.1) cannot be identified from the data without additional assumptions. Simply using the observable average outcomes of persons living in districts with centralized welfare agencies to approximate the unobservable outcomes for welfare recipients registered at decentralized agencies leads to biased estimates if individuals in the two types of agencies differ in characteristics that affect the expected outcomes, i.e. $E(Y^0|D = 1) \neq E(Y^0|D = 0)$.

To overcome this potential bias, I apply a matching estimator using a control group that consists of individuals living in centralized districts who are similar in all relevant pre-treatment characteristics X to the treatment group of individuals registered at decentralized welfare agencies. The method is based on the intuitive principle that it is possible to adjust away differences between treated and non-treated outcomes by finding appropriate matches (see Heckman et al., 1999). If this is true, the matching approach makes it possible to compare the treated and non-treated outcomes directly without imposing further structure on the estimation problem.

The method of matching is a non-parametric approach, and therefore no structural or parametric assumptions are required. Yet it puts strong requirements on the data. In order for the ATT to be identified, the so-called Conditional Independence Assumption (CIA) must be fulfilled (see Lechner, 1998). This means that, based on the set of relevant observable covariates X, the potential outcome Y^0 is independent of the organizational model: $Y^0 \amalg D|X$. Hence, all relevant variables influencing both treatment status and the outcome variable have to be observed. In addition, it has to be ensured that there are individuals from the treatment and control group for all relevant covariates. In other words, it must be guaranteed that individuals who resemble the welfare recipients' characteristics distribution in districts with decentralized organization are available in districts with centralized welfare administration (common support condition), i.e. $Pr(D = 1|X) < 1$ (see Smith and Todd, 2005a). Finally, I need to invoke the Stable Unit Treatment Value Assumption (SUTVA) (see Rubin, 1986) to render the model useful for causal analysis. The SUTVA rules out cross-effects, meaning that the outcome for any individual must not depend on whether any other individual is registered at a centralized or decentralized agency. In particular, substitution effects must be excluded. This requires that regional labor markets be sufficiently separated so that the success of one welfare agency with respect

to job placement does not come at the cost of another welfare agency. Nor should there be any general equilibrium effect. These requirements are addressed by a study at the aggregate level of all 439 welfare agencies (see IFO and IAW, 2008). From the findings of this study, there is robust evidence that the SUTVA holds.

Direct matching on X is difficult if X is of high dimension. Rosenbaum and Rubin (1983) thus suggest the use of balancing scores. One possible balancing score is the probability of being subject to treatment: the propensity score $p(X) = E(D = 1|X)$. This statistic summarizes the relevant covariates X in a single index function. All biases due to observable covariates are removed by conditioning solely on the propensity score. In this thesis, a probit model is used for the specification of the propensity score in which the dependent variable is 1 if an individual is registered at a decentralized welfare agency and 0 if an individual is not.

The literature provides a number of different matching estimators (see Heckman et al., 1999). These estimators differ with respect to the weights given to individuals in the control group that are considered to form the counterfactual outcome. In my analysis, I use a kernel density matching estimator with bootstrapped standard errors based on 250 replications. According to Abadie and Imbens (2008), bootstrapped standard errors are unbiased for kernel matching. The applied estimator weights the control observations according to their distance (in terms of the propensity score) to the treated individuals by means of an Epanechnikov kernel function and a bandwidth of 0.06.[36] It has to be taken into account that individuals residing in the same district may be affected by common shocks. This could bias the standard errors and invalidate the statistical inference (see Moulton, 1986 and 1990). I account for this problem by estimating clustered standard errors at the agency level using the non-overlapping block bootstrap, i.e. by re-sampling persons on the agency level but not on the individual level.

3.3.2 Plausibility of the Conditional Independence Assumption and Specification of the Propensity Score

To identify the causal effect of decentralized welfare administration on individual employment prospects, I have to rule out selective participation in treatment. The most common types of selectivity encountered in the evaluation of labor market policies are self-selection and selection by a caseworker into the treatment. In the case of my analysis, these types of selectivity are very unlikely. From the point of view of a welfare recipient or the caseworker,

[36] I use the matching algorithm provided by Leuven and Sianesi (2003).

the 2005 reform of welfare administration and organization is an exogenous event that cannot be easily influenced or avoided. The only way to select into treatment or out of treatment would be to move to another district. But welfare recipients usually cannot afford to relocate and are not encouraged by welfare agencies to move if they remain on welfare.

A more serious problem might be that my estimation sample was not drawn in January 2005 (when the reform was introduced) but in October 2006, i.e. more than one and a half year after the implementation of the reform. As has been noted in section 2.3, the reason for this delay is that the disruptions caused by the reform created considerable problems for the quality of administrative data during several months after the introduction of the reform. Reliable data are only available from 2006 and after. At this point in time, however, the composition of welfare recipients in the districts could itself be an outcome of centralized or decentralized organization. Thus, for example, if the centralized model was faster in integrating welfare recipients with good employment prospects in the early periods after the reform, the stock of welfare recipients in 2006 might contain fewer welfare recipients with favorable characteristics than in decentralized districts. This example is purely hypothetical. In fact, the extensive implementation studies that were conducted as part of the evaluation of Germany's 2005 welfare reform do not suggest that either of the two organizational models had an advantage in integrating easy-to-place individuals in 2005 or 2006 (see WZB et al., 2008). Nevertheless, to avoid composition bias, it is necessary to control for individual characteristics.

Another similar kind of potential selection concerns the inflow into welfare receipt. As mentioned in section 2.1, UBII recipients have to be able to work for at least 15 hours per week. In determining whether claimants to UBII meet this requirement, welfare agencies possess a considerable degree of leeway. If ability criteria differ systematically between centralized and decentralized welfare agencies, this may result in a different composition of welfare recipients with regard to characteristics such as illness or disability.

In order to solve all these potential problems, I can include a comprehensive characterization of the individual situation in the propensity score. I have access to a wide range of sociodemographic characteristics usually not available, such as migration background, household size and members, health impairments, basic mathematics, literacy and computer skills, self-assessed working capacity (measured in hours per day), and obstacles to employment such as the provision of care for relatives or disability status. In addition, I have detailed information on the labor market history of each individual between 2001 and 2004 as well as information on the more recent labor market history.[37]

[37] The information for the years 2001 to 2004 is based on the administrative part of the data set. The administrative data provide information on episodes of employment, unemployment, job seeking while employed, participation in ALMP, and episodes out of the labor force. The episodes were decomposed into spells of 14-days length (half-

Direct measures of individual motivation and attitudes are not included in the data. It is likely, however, that these characteristics are relatively persistent over time such in that they impacted labor market success before the treatment as well. For this reason, it is crucial to condition on individual labor market histories in detail. This is also emphasized by Card and Sullivan (1988) and Heckman et al. (1998). Making use of my unusually rich data set, I am confident that I capture all relevant factors that affect both participation in treatment and my outcome variable.

As far as potential regional selection of the adoption of a centralized or decentralized organization is concerned, it is important to recall that I consider only comparable centralized and decentralized districts as determined by Arntz et al. (2006) and described in subsection 2.3.1. Table A.3.1 in the appendix to this chapter shows numerous regional variables that have been used by Arntz et al. (2006) for the determination of comparable districts and compares their means between centralized and decentralized districts. The table also displays the p-value for an equality-of-means test for each variable. As can be seen, equality of means cannot be rejected for the vast majority of variables. The only exceptions are those variables that depend on the degree of urbanization of a district such as the share of commuters and the share of foreigners. Here, the mean in centralized districts is slightly larger than it is in decentralized districts. To take account of these slight differences, I include regional covariates, in particular a dummy variable for urban districts (as opposed to rural districts), in my propensity score specification.

Since I have a large number of potential control variables at my disposal for the specification of the propensity score, and because including irrelevant covariates may introduce noise into the calculation of the propensity score, I choose different probit specifications in order to check the robustness of the estimated treatment effects. The first specification contains the most important individual characteristics – age, schooling, migration background, household size, number of children, obstacles to employment, and several indicators for labor market history – as well as information on the duration of the current welfare spell[38] and limited regional information including the dummy vari-

months), resulting in up to 96 labor market spells per individual. These spells were aggregated into a number of different variables summarizing the labor market history of individuals in a way to make it feasible for estimation. Variables include, for example, the number of employment spells in the years from 2001 to 2004 and the number of half-months spent in unemployment in a specific year in this period. The information on the more recent labor market history is taken from the survey of welfare recipients.

[38] The duration of the welfare spell is measured as the number of months individuals receive unemployment benefit II before the sampling date. Due to the time span between sampling and interview date, not all individuals report a start date of welfare receipt before the sampling date. Some left and re-entered the welfare system during fall and winter 2006/2007 and thus report a start date after the sampling date. For these individuals, the duration variable is set to 0. An additional dummy variable takes these late start dates into account.

able for urban districts. Based on the results of balancing tests, I prefer this parsimonious specification. In the second specification, I add further regional information (e.g. GDP per employed person, population density, labor market conditions). The third specification contains additional individual covariates (e.g. professional qualification, health status, self-assessed working capacity). Descriptive statistics for all variables included in the different propensity score specifications are provided in table A.3.2 of the appendix to this chapter.

I carry out all estimations separately for men and women, as many evaluation studies have found that the effectiveness of labor market activation differs between genders (see Bergemann and van den Berg, 2008, for a survey on recent European evidence). I look at different subgroups as well. First, I split the sample according to household size and differentiate between individuals living on their own (singles) and individuals who live with at least one other person in the same household (non-singles). Unlike ALMP, which are targeted directly at individuals, the organization of welfare administration and, thus, the entire activation efforts of centralized and decentralized welfare agencies are a treatment directed to the household as a whole (see also chapter 2). Only for single households, this is the unit which may also be integrated into employment. I thus consider single and multi-person households separately. Second, I distinguish between immigrants and native Germans. The estimation results of the preferred propensity score specifications for men and women and all considered subgroups are illustrated in table A.3.3 in the appendix to this chapter.

3.3.3 Common Support and Balancing Quality of the Matching Estimator

The Average Effect of Treatment on the Treated (ATT) is only identified for the region of common support. To ensure common support in each estimation sample, I eliminate all treated individuals whose propensity score is smaller than the smallest or larger than the largest propensity score in the respective group of non-treated individuals. The number of lost observations due to the common support condition is very small. For example, in the sample of men I lose 4 out of 2,066 treated observations in the preferred specification of the propensity score and only 2 observations in the sensitivity analyses. None of the 2,423 treated observations is lost in the preferred specification for women. In the specification with additional regional variables, 35 treated women must be excluded from the analysis, and, in the specification with additional regional and individual covariates, 22 observations are lost.

To assess the quality of matching, I apply four balancing tests. First, I compare the means of the variables included in the propensity score between treatment and control group and test for differences in means by applying

equality-of-means tests. After successful matching, there should be no remaining differences in the distribution of the covariates. Second, I follow Rosenbaum and Rubin (1985), who suggest the use of the so-called standardized difference in %. Differences in means of single covariates between the treatment and control group are compared before and after matching, standardized by the mean standard deviation across groups before matching. A third test relates to the explanatory power of the propensity score model after matching. Re-running the same probit regression on the matched sample should result in an explained treatment variation of almost zero, as measured by the McFadden-R^2 (see Sianesi, 2004). Fourth, I apply the quality indicator proposed by Smith and Todd (2005b):

$$\begin{aligned} X_k = \beta_0 + \beta_1\hat{p}(X) + \beta_2\hat{p}(X)^2 + \beta_3\hat{p}(X)^3 + \beta_4\hat{p}(X)^4 + \beta_5 D \\ +\beta_6 D\hat{p}(X) + \beta_7 D\hat{p}(X)^2 + \beta_8 D\hat{p}(X)^3 + \beta_9 D\hat{p}(X)^4 + \eta. \end{aligned} \quad (3.2)$$

Each variable X_k included in the propensity score is regressed on a higher-order polynomial of the estimated propensity score $\hat{p}(X)$, the treatment indicator D, and the interaction between both. In the ideal case, coefficients β_5 to β_9 should be zero for all X_k, indicating that there is no further observable selection into treatment conditional on the propensity score.

As can be seen from the results of the balancing tests depicted in table 3.1 and table A.3.2 in the appendix to this chapter, matching quality is very satisfactory.[39] The latter table shows that the equality of means of the variables included in the propensity score specification between treatment and control group cannot be rejected in the vast majority of cases. According to table 3.1, the mean standardized difference in % is strongly reduced after matching. The McFadden-R^2 estimates of the third test are almost zero after matching. Thus, as intended, re-running the propensity score specification on the matched sample does not result in any explanatory power for the included covariates. Almost all variables included in the propensity score model pass the test suggested by Smith and Todd (2005b).

[39] The corresponding results for the subsamples of singles, non-singles, immigrants, and native Germans are presented in table A.3.4 in the appendix to this chapter.

Table 3.1: Indicators for matching quality

	Men	Women
Indicators before matching		
McFadden-R^2	0.012	0.009
LR-Test	92.730	77.630
p-value	0.000	0.000
Mean standardized difference in %	4.686	4.977
Indicators after matching		
McFadden-R^2	0.000	0.001
LR-Test	2.740	9.050
p-value	1.000	0.999
Mean standardized difference in %	0.829	1.440
Smith and Todd (2005b) balancing test		
p-values > 0.05	25	19
p-values > 0.01	25	24

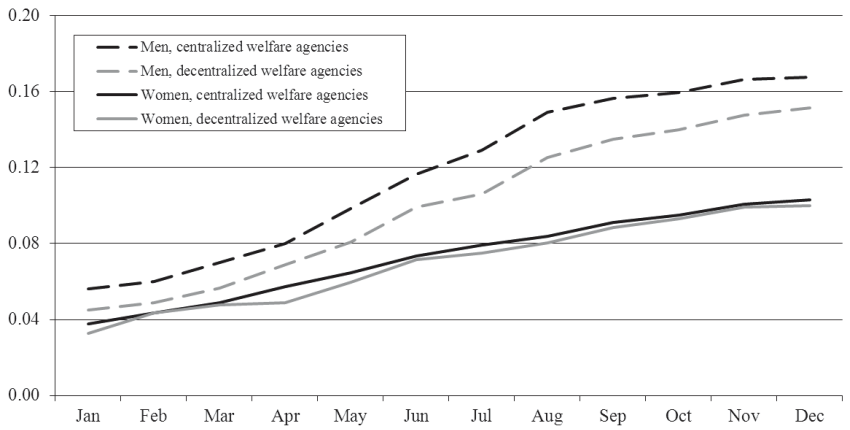
Remarks: All indicators are based on the preferred propensity score specification for men and women, respectively. The McFadden-R^2 derives from a probit estimation of the propensity score on all covariates considered. The LR-statistic and the corresponding p-value derive from a likelihood-ratio test of the joint insignificance of all covariates. The mean standardized difference in % has been calculated as an unweighted average of all covariates. The Smith and Todd (2005b) balancing test displays the number of covariates passing the test at the indicated significance level. There are 26 covariates included in the preferred propensity score specifications for men and women.

3.4 Empirical Results

Before presenting the estimation results, I briefly describe the means of my outcome variable self-sufficient employment and compare them across individuals who are registered at centralized and decentralized welfare agencies (see figure 3.1). For men, employment rates in centralized districts are larger than they are in districts with decentralized organization. By December 2007, I observe a difference of about 1.5 percentage points between centralized and decentralized welfare agencies (16.8% for centralized and 15.2% for decentralized welfare agencies). There is no difference between the two organizational models for women. In both types of agencies, employment rates are substantially lower for women than for men.

My econometric analysis is consistent with these descriptive findings. As discussed in section 3.3, I use three different specifications for the propensity score. The estimated treatment effects of decentralized welfare administration on integration into self-sufficient employment are presented in figures 3.2 and 3.3 for men and women, respectively. Rather than showing treatment effects at a single observation date, I display their evolution over the course of 2007, the year after sampling. The estimated effects are also depicted in table A.3.5 in the appendix to this chapter.

Fig. 3.1: Means of the outcome variable self-sufficient employment

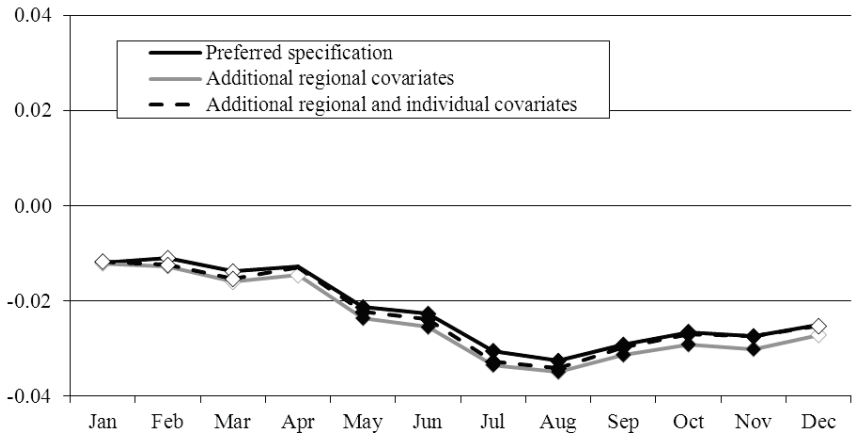


Remarks: For each month, the outcome variable self-sufficient employment is defined to be 1 if an individual is employed and does not receive welfare benefits anymore. Otherwise, the variable is 0. The displayed means refer to the year 2007. The sampling date is October 2006.

For men, I observe a negative treatment effect, i.e. decentralized welfare agencies are less successful than centralized agencies in placing male welfare recipients in jobs that provide a sufficient living income. The absolute effect rises from 1 to over 3 percentage points from January to August 2007, and declines moderately thereafter. These magnitudes are slightly larger than the descriptive evidence presented in figure 3.1. The effects for May to November are significant at the 5%-level, with t-statistics ranging from 1.96 to 2.91. With the exception of April, the effects for the other months are significant at the 10%-level. The inclusion of further covariates leaves the estimated effects virtually unaffected (see figure 3.2 and table A.3.5). Given the relatively small fraction of people taking up self-sufficient employment (see figure 3.1), the effects for men are substantial. The largest estimated effect of nearly 3.5 percentage points, estimated for August 2007, implies that decentralized agencies have an integration quota 24% lower than centralized agencies. I also find negative treatment effects for women, but these are smaller in magnitude than for men and which are not statistically significant, besides (see figure 3.3 and table A.3.5). Again, the results are insensitive to the specification of the propensity score.

Gender differences are also present when I split the sample into single and non-single households (see table A.3.6 in the appendix to this chapter, which provides estimated treatment effects based on the preferred propensity score specifications). As can be seen from the table, for single men I estimate a substantially negative employment effect of decentralized welfare agencies.

Fig. 3.2: Estimated treatment effects of decentralized welfare administration on self-sufficient employment for men

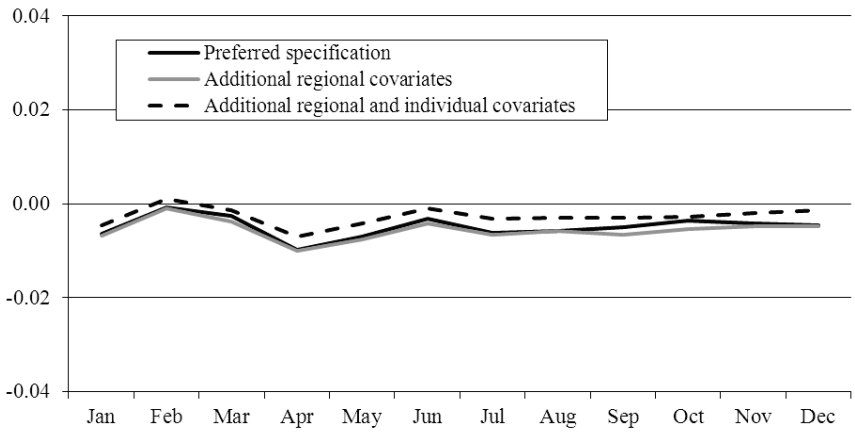


Remarks: Displayed are Average Effects of Treatment on the Treated (ATT). The effects refer to the year 2007. The sampling date is October 2006. ♦ indicates significance at the 5%-level, ◊ significance at the 10%-level.

The effect amounts to 4.5 percentage points in absolute terms. For single women, I observe a negative treatment effect, but this effect is only slightly significant at the beginning of my observation period. Thereafter, it is insignificant and of smaller magnitude than the effect found for single men. In case of non-single men, I estimate a negative treatment effect of decentralized welfare agencies, with an absolute value of up to 2.9 percentage points. This effect is of smaller magnitude than the effect found for single men, but it is larger than the effect for non-single women. For the latter subgroup, I have been unable to establish a significant treatment effect.

When I look separately at persons with and without a migration background, immigrants are defined to comprise individuals with non-German citizenship, German citizens who were born abroad and who have at least one parent also born abroad, and German citizens who have at least one parent born abroad and who use a foreign language as their main language within their family circle (see subsection 2.3.1). Table A.3.7 in the appendix to this chapter depicts my estimation results based on the preferred propensity score specifications. Neither for female natives nor for female immigrants have I been able to detect any difference between the effects of centralized and decentralized welfare agencies on the outcome variable self-sufficient employment. Decentralized welfare agencies tend to lower the employment chances of women with and without a migration background, but the effects are not statistically significant. By contrast, I observe many more adverse effects

Fig. 3.3: Estimated treatment effects of decentralized welfare administration on self-sufficient employment for women



Remarks: Displayed are Average Effects of Treatment on the Treated (ATT). The effects refer to the year 2007. The sampling date is October 2006. ♦ indicates significance at the 5%-level, ◊ significance at the 10%-level.

from decentralized agencies in case of male natives and male immigrants. For both male subgroups, the effects amount to 3.5 percentage points in absolute terms. They are somewhat more persistent for male natives than for men with a migration background. Nevertheless, they confirm my overall result: decentralized welfare agencies are less effective in the labor market activation of male welfare recipients than their centralized counterparts. There is no significant difference between the two types of welfare agencies with respect to the employment chances of women.

In discussing the potential reasons for the gender differences in my results, I refer to the study of IAQ et al. (2009) that explicitly investigates the labor market activation of male and female welfare recipients from the perspective of gender mainstreaming. This study combines analyses of survey and administrative data with case studies within welfare agencies. It shows that women are less intensively activated than men, irrespective of the agency type. In particular, women are less frequently assigned to ALMP than men. While about half of all welfare recipients are female (see table 2.4 in chapter 2), the share of women among participants in ALMP is substantially lower than 50%. A less frequent use of ALMP for women has also been documented by Thomsen and Walter (2010b) and Boockmann et al. (2011), who estimate multivariate probit models to determine the participation probabilities of welfare recipients in various ALMP. For most of the programs considered, participation probabilities of women are significantly lower compared with

men even when controlling for other sociodemographic, regional, and agency characteristics. A relatively low participation rate of women in ALMP will become evident from the analysis in chapter 5 of this thesis.

Some women, in particular mothers of small children under the age of three years, are not activated at all. Chapter 10 of Book II of the German Social Code (*Sozialgesetzbuch Zweites Buch, SGB II*) allows parents of small children to withdraw from the active job search if participation in ALMP or employment uptake would put the well-being of the children at risk. The findings of IAQ et al. (2009) reveal that mothers in particular make use of this option. In some cases, they are even encouraged by welfare agencies to do so. By contrast, fathers face more suspicion when considering a withdrawal from the active job search since this is often interpreted by caseworkers as a lack of motivation to overcome welfare dependency.

Moreover, the case studies of IAQ et al. (2009) indicate that activation efforts of welfare agencies differ between genders for efficiency reasons. Due to limited time resources of the caseworkers and the overall goal of realizing as many transitions to employment as possible, activation is mainly targeted at those individuals who are considered to be most easily placeable and most likely to take up a job that generates a wage sufficient for leaving the welfare system. In most cases, welfare agencies assume that men are more easy to place than women.

If women are less activated than men or not activated at all, I cannot expect significant differences in the success of centralized and decentralized welfare agencies at integrating female welfare recipients into self-sufficient employment. Differences can only be present for individuals who are subject to activation like men are. Thus, the findings of IAQ et al. (2009) and the other cited studies might explain why I observe gender differences in my results.

3.5 A Closer Look at the Black Box of Welfare Administration

The significant treatment effect for men raises the question of why centralized organization performs better in placing welfare recipients. Is the relative success of centralized agencies due to their use of more successful approaches of internal organization that could also be adopted by decentralized agencies? All centralized welfare agencies are subject to central FEA guidelines, central controlling, and certain directives regarding the use of activation measures. Nevertheless, welfare agencies have leeway in the way they internally organize their services for welfare recipients. The implementation of organizational approaches is not specific to either administrative model, and I observe variation within both agency types. In the following, I analyze the

effect of the adopted approaches and check if they are able to explain the positive effect of centralized organization.

According to the studies conducted to evaluate the implementation of Germany's 2005 welfare reform, the following approaches are the most important elements in the internal organization of welfare agencies (see IAW and ZEW, 2008; and WZB et al., 2008):

1) **Generalized case management** for all clients as opposed to case management by specialized staff for clients with multiple obstacles to employment
2) **Integration of activation and placement** as opposed to the separation of these functions
3) Use of **customer segmentation** procedures
4) Establishment of an **employer service**, i.e. specialized staff maintaining contact to employers
5) **Subcontracting of placement services** to private providers

Table 3.2 provides a more detailed description of these organizational approaches and outlines some arguments as to why they could affect the integration success of welfare recipients. Customer segmentation and particularly generalized case management tend to be used much more frequently by decentralized agencies. Integration of activation and placement is slightly more common among centralized agencies, while the other two approaches are not related to agency type.

Table 3.2: Definition of organizational approaches

Definition	Possible impact on integration	Frequency in sample
	Generalized case management	
Caseworkers activate all types of clients. There is no assignment of welfare recipients with multiple obstacles to employment to different specialized caseworkers.	Generalized case management facilitates individual activation, as clients have fewer contact persons. Yet there is better placement under specialized case management if clients with specific problems require specialized expertise.	69% of decentralized agencies 25% of centralized agencies

Continued on next page

Table 3.2: Definition of organizational approaches (continued)

Definition	Possible impact on integration	Frequency in sample
Integration of activation and placement		
Clients are activated and placed into employment by the same staff members. There is no assignment of specialized staff to the two tasks.	Integration reduces the number of contact persons for each welfare recipient and facilitates a holistic approach to support employment uptake. By contrast, separation leads to gains from specialization but may create coordination problems at the interface of both tasks.	51% of decentralized agencies 60% of centralized agencies
Customer segmentation		
Classification of clients into different groups receiving different treatment during activation. In most cases, classification according to employability of clients.	Segmentation may increase employment rates among groups that are activated more intensely but reduces integration into employment in other groups.	84% of decentralized agencies 67% of centralized agencies
Employer service		
A team of agency staff members maintains a network with employers and serves as contact for them.	Networking may result in better placement. But internal coordination problems between the employer service and caseworkers may arise.	86% of decentralized agencies 83% of centralized agencies
Subcontracting of placement services		
The welfare agency uses private employment services to place some of their clients into employment.	Specialization gains may occur. However, private agencies may work more or less effectively compared to the public employment service. Requires proper assignment of welfare recipients to service providers.	41% of decentralized agencies 40% of centralized agencies

Remarks: For one centralized welfare agency, information on the organizational approaches is missing. Hence, the frequencies denoted in the rightmost column of the table are based on 51 decentralized welfare agencies and 102 centralized agencies.

To check whether the effect of decentralized agencies can be attributed to one of these approaches, I required a multivariate framework. For this purpose, I use binary probit models. The probit estimations contain all covariates used in the preferred specification of the propensity score. In addition, dummy variables for decentralized welfare agencies and for the organizational

approaches are included. I then test whether a significant effect of decentralized agencies on self-sufficient employment remains despite controlling for the approaches of internal organization. Tables A.3.8 and A.3.9 in the appendix to this chapter display my estimation results, which account for potential clustering of error terms at agency level (see e.g. Moulton, 1986 and 1990). The entries in the tables are marginal effects of the dummy variables on the outcome variable; their magnitudes and treatment effects from matching are thus comparable. Since results do not differ much between the (sub)groups considered, I rely on the overall samples of men and women.

As with the matching results, I find a negative effect of decentralization for men. But the effect is slightly smaller with a magnitude of up to 2.5 percentage points. For women, the effect of decentralized agencies is insignificant. The organizational approaches themselves are mostly insignificant, the effect of an employer service in the subsample of women being the only exception. As can be seen from table A.3.9, this effect is negative and significant at the 10%-level in the last four months of the observation period. For men, the marginal effect of an employer service tends to be negative, too, although it is not statistically significant. These results could be interpreted as an indication that the presence of a specific organizational unit of the agency specializing in employer contacts is a disadvantage. It might be that the installation of an employer service shifts away valuable resources from the activation process of welfare recipients, which might have negative consequences for employment uptake. Unfortunately, there is no direct evidence to validate these results.

Despite the slight decline in magnitude, the significant negative effect of decentralized welfare agencies on self-sufficient employment for men is largely robust to the inclusion of further organizational approaches. I thus conclude that the individual employment effects of (de)centralized organization of welfare agencies are not due to the adoption of particular forms of internal organization. A more likely explanation of the effect differences relates to the theoretical argumentation (see section 3.1). The advantages of centralized organization in bundling resources, collecting information from various sources, and imposing best-practice strategies for the local offices tend to outperform the favorable properties of decentralized organization.

3.6 Summary

The German welfare reform of 2005 introduced two different organizational models for the labor market activation of welfare recipients in an otherwise homogenous institutional setting: centralized and decentralized welfare agencies. In order to evaluate their relative performance, I estimate their effect on the integration of welfare recipients into self-sufficient employment. My analysis takes regional differences as well as individual selection into account. Estimation is based on exceptionally rich data from various sources. I combine

a detailed survey of welfare recipients with administrative records from the FEA. In addition, I use a large set of variables that describe the local labor market. Finally, I consider unique information on the internal organization of welfare agencies.

My analysis provides the first quantitative evidence about the effects of a decentralized versus centralized organization of welfare administration for individual employment outcomes. I find that decentralized welfare agencies have a negative effect on male welfare recipients with respect to integration into self-sufficient employment. Given the low transition rate from welfare receipt into self-sufficient employment in general, the magnitudes of the effects for men are substantial. The integration quota of decentralized welfare agencies is up to 24% lower than the quota of centralized agencies. I also find negative treatment effects for women, but these are smaller in magnitude than for men and are statistically insignificant.

Gender differences are also found within all subgroups considered in this chapter (singles, non-singles, immigrants, native Germans). They might result from a different activation intensity between men and women. Evidence suggests that, irrespective of agency type, the activation intensity of women is far lower compared with men. For example, women are less frequently assigned to ALMP. In some cases, in particular when they have small children, they are not activated at all. If welfare agencies concentrate their activation efforts predominantly on men rather than on women, it is harder to uncover significant differences in the relative performance of centralized and decentralized agencies for the latter subgroup.

I have further explored potential reasons through which my results may have emerged. Because welfare agencies have significant discretionary power with respect to internal organization, I have checked whether the organization of tasks at individual welfare agencies is responsible for the result of decentralization. Although the effects are slightly weakened by the inclusion of the most relevant organizational approaches, the overall result is not affected. I conclude that the negative effect of decentralization is not due to different choices regarding the internal organization of tasks between centralized and decentralized welfare agencies. The effect differences are related to the very nature of centralized organization. Examples are the application of central best practice guidelines of the FEA concerning the use of ALMP and other instruments of activation, as well as the centralized controlling system.

Chapter 4

The Employment Effects of an Intensified Use of Benefit Sanctions*

4.1 Introductory Remarks

Regardless of centralization or decentralization and of internal organization, the strategical elements used by welfare agencies might make a difference for the success of labor market activation for welfare recipients. One such element might be the sanction strategy of welfare agencies, i.e. the way welfare agencies deal with benefit revocations in case of non-compliance by welfare recipients. To analyze the impact of the sanction strategy, this chapter takes a closer look at the use of benefit sanctions by welfare agencies and evaluates whether an intensified use of sanctions is an effective tool to increase employment uptake among affected individuals. As noted above, benefit sanctions play a major role in the second key characteristic of Germany's 2005 welfare reform, namely the enforcement of the principle of "supporting and demanding" (*Fördern und Fordern*) in the activation of welfare recipients.

Benefit sanctions are increasingly being used as part of the unemployment insurance and public welfare systems in many countries.[40] Benefit payments are made conditional on requirements such as actively searching for a job or participating in ALMP. Sanctions (or the threat to impose them) are intended to provide incentives for the benefit recipients to comply with these requirements. In Germany, these elements have become much more commonplace

* This chapter builds on joint work with Bernhard Boockmann and Stephan Thomsen. It is a revised and extended version of Boockmann et al. (2009). Extensions include analyses for several subgroups of welfare recipients (individuals registered at centralized and decentralized welfare agencies, men and women, immigrants and native Germans). But while Boockmann et al. (2009) estimate the effects of sanctions on the drop-out from welfare (irrespective of employment uptake) and on the transition to employment (irrespective of drop-out from welfare), I consider self-sufficient employment.

[40] Grubb (2000) provides an overview of a number of different unemployment insurance systems of selected Organisation for Economic Co-operation and Development (OECD) countries with a particular focus on benefit sanctions.

after the 2005 reform of the welfare system. The obligation to actively search for employment and to participate in ALMP marks an important change in German welfare policy. For the first time, equal activation opportunities are provided for all welfare recipients. Benefit sanctions are now frequently imposed in the form of a partial or a complete revocation of benefits for a given period of time (see sections 2.1 and 2.2).[41]

The empirical literature on the effects of sanctions is still limited. Fredriksson and Holmlund (2006) provide a survey of theoretical and empirical research. A number of recent empirical studies have analyzed the ex post effects of sanctions imposed on unemployment insurance benefit recipients, e.g. Abbring et al. (2005) for the Netherlands, Lalive et al. (2005) and Arni et al. (2009) for Switzerland, Svarer (forthcoming) for Denmark, or Müller and Steiner (2008) and Hofmann (2008) for Germany.[42] Some empirical evidence also exists for welfare recipients, e.g. van den Berg et al. (2004) for the Netherlands, Blank and Haskins (2001) and Grogger and Karoly (2005) for the US, and Schneider (2008) for Germany. Most of these studies establish positive effects on labor market outcomes. For example, van den Berg et al. (2004) find that the transition rate from welfare to work more than doubles after a benefit sanction has been imposed. Similarly, Müller and Steiner (2008) report positive effects of benefit sanctions on employment uptake for unemployed individuals in Germany. By contrast, Schneider (2008) analyzes the effects of benefit sanctions on the reservation wage for unemployment benefit II (UBII) recipients and finds no significant impact.

To date, the extant empirical literature has not produced clear policy recommendations. A major drawback is that these studies either rely on limited individual information or are restricted to small geographic areas. For example, Abbring et al. (2005) use Dutch register data containing only a small number of individual characteristics. Therefore, they model unobserved heterogeneity explicitly by using a multivariate mixed proportional hazard model to avoid potential selection bias in the estimates. The results of Arni

[41] Benefit sanctions have also been used for recipients of unemployment assistance or social assistance before 2005, albeit far less intensely than unemployment benefit II (UBII) sanctions. See Wilke (2004) for descriptive evidence on the use of sanctions for recipients of unemployment assistance.

[42] Ex post effects have two different forms: first, and most important, ex post effects comprise the effects of actually imposed sanctions. Second, they can also appear as warning effects in sanction systems where non-complying individuals are informed before the actual imposition of a sanction that an investigation has been initiated and that they might receive a benefit cut (as for example in Switzerland; see Lalive et al., 2005). Sanctions can also have ex ante effects. The threat of receiving a sanction might induce all benefit recipients to search more intensively for a job than in a system without sanctions (see Lalive et al., 2005; and Arni et al., 2009). But in a legally defined homogenous sanction system such as the German case, the identification of ex ante effects is difficult since the counterfactual setting without (or with fewer) sanctions is not observable and might not even be a realistic reference point for the individual benefit recipient, who is at most aware of the sanction system he or she is actually exposed to.

et al. (2009) are based on data for only seven out of 26 Swiss cantons and the results of Lalive et al. (2005) are based on as few as three cantons. Van den Berg et al. (2004) focus solely on welfare recipients in the Dutch city of Rotterdam. Generalizing these findings to unemployment insurance benefits and welfare recipients in other regions or countries is not straightforward and requires very strong assumptions.

Another limitation is the degree of sophistication in statistical analysis and the treatment effect chosen as the parameter of interest. For instance, while recent empirical studies for Germany are mostly based on comprehensive administrative data, the results are either purely descriptive (for example, Wilke, 2004) or affected by potential selection biases. Schneider (2008), Hofmann (2008), and Müller and Steiner (2008) use propensity score matching estimators to estimate Average Effects of Treatment on the Treated (ATT) but do not take into account selection on unobservables. Estimation of the ATT is reasonable if the group intended to be sanctioned by the policy maker is actually exposed to benefit sanctions. But identifying the intended group from observational data may be incomplete since sanctions are not imposed mechanically for certain infringements but instead depend on multiple factors. Moreover, although the probability of infringement may be systematically related to certain characteristics of the individual, addressing selectivity solely with respect to observed characteristics is a questionable approach.

In practice, the likelihood of being sanctioned will not only depend on the observable and unobservable characteristics of the individual but also on the characteristics of the welfare agency at which an individual is registered and which is in charge of the actual imposition of sanctions. Benefit sanctions are not imposed uniformly when the individual does not comply with his or her duties during the activation process; rather, there is substantial discretion at the agency level. As my data show, some agencies are reluctant to use sanctions, while others apply sanctions more frequently sometimes even in cases of minor infringements. The role of the policy regime at local welfare agencies has been completely neglected in the literature.

This chapter aims to contribute to the literature in the following respects. Based on the data set described in subsection 2.3.1, I estimate the effects of benefit sanctions for UBII recipients on the transition to self-sufficient employment. The data provide information on the sanction strategies that are used in the 154 sampled welfare agencies. Based on the different strategies of the welfare agencies, as well as on the frequency with which sanctions are actually imposed, I instrument the selection process at the individual level and estimate Local Average Treatment Effects (LATEs; see Imbens and Angrist, 1994). Specifically, I estimate the effect of a sanction on those individuals who are not sanctioned in a welfare agency with a moderate sanction policy but who would be sanctioned if the agency changed its policy and imposed sanctions more frequently. The estimated LATEs can thus be interpreted as a measure of the effectiveness of an intensified use of benefit sanctions.

The remainder of this chapter is organized as follows. In the next section, I describe my data preparation and the estimation sample for the empirical analysis. Section 4.3 discusses the identification strategy. In section 4.4, I present the estimation results. The final section summarizes my findings.

4.2 Data Preparation and Estimation Sample

To evaluate the impact of benefit sanctions, I use the data set described in subsection 2.3.1 and consider all persons interviewed in the first wave of the survey of welfare recipients. In the survey, individuals were asked whether they have ever been sanctioned by their welfare agency and, if so, in which month the last sanction was imposed. I define those individuals as treated who report to have been sanctioned for the first time and only once between October 2006 and April 2007. All other persons form the control group. Since I focus on the effect of the first sanction imposed, persons with more than one sanction are deleted from the estimation sample, as earlier sanctions may have been imposed before the sampling date and the sampling procedure conditions on the outcome (failure) of sanctions imposed before this date. Individuals who explicitly state that they were sanctioned before the sampling date have been dropped from the data for the same reason. Since I do not have precise information on the amount of the benefit cuts, I am unable to distinguish between different levels of sanctions and thus only use the information as a binary indicator. In addition, I impose two further restrictions on the estimation sample. Since self-sufficient employment is my outcome variable of interest, I only look at individuals age 18 to 57 who were unemployed at the beginning of their respective UBII spell and at the sanction date. Due to these restrictions, my sample size reduces to 15,361 observations.[43]

Regarding the outcome variable, I follow each sanctioned individual for six months after the imposition of the sanction. For instance, for a person sanctioned in January 2007, I examine the labor market states from February 2007 to July 2007, and for a person sanctioned in April 2007, I use information on the period from May 2007 to October 2007. In order to compare the sanctioned individuals with the control group, I construct a hypothetical sanction date for the non-sanctioned individuals by drawing randomly from a uniform distribution of sanction dates from the period of October 2006 to April 2007, i.e. the probability of receiving a (hypothetical) sanction in each

[43] In detail, I drop 845 individuals age 15 to 17, 2,116 persons age 58 to 64, and 3,771 welfare recipients who are employed. Moreover, I exclude 811 persons with more than one sanction, 989 individuals with a sanction before the sampling date, 181 welfare recipients with a sanction in a former UBII spell, and 489 observations with missing information.

of these months is assumed to be 1/7 for each individual.[44] Given the (hypothetical) sanction dates, the non-sanctioned individuals are also followed for six months.[45]

4.3 Estimation Approach

To estimate the effect of a sanction on self-sufficient employment, I consider the linear probability model

$$Y_i = X_i\beta + S_i\theta + u_i, \tag{4.1}$$

where Y is the binary outcome variable of interest ($Y_i = 1$ in case of self-sufficient employment of individual i and $Y_i = 0$ otherwise), X is a vector of covariates (at individual and agency level), and S is a dummy variable indicating whether individual i has been sanctioned ($S_i = 1$) or not ($S_i = 0$). Under the additional assumption that the error term u has zero mean conditional on all covariates X and S, one could apply simple Ordinary Least Squares (OLS) estimation to identify the effects of the right-hand side variables on Y. But I cannot rule out that S is endogenous in my analysis, i.e. it may be that $Cov(S, u) \neq 0$. In this case, an OLS estimate of the sanction effect on Y would be biased. Endogeneity of S could be due to various reasons. For example, there could be a negative selection process driven by motivation or other unobservable factors such that individuals with unfavorable characteristics are sanctioned with higher probability. In such a case, the OLS estimate of the effect of S on Y would be underestimated. Yet there could also be a positive selection process initiated by the welfare agencies if those with unfavorable characteristics are not activated and, thus, not sanctioned. A positive selection of this kind would result in an overestimated

[44] As a robustness check, I alternatively construct the hypothetical sanction dates for the non-sanctioned individuals by imaging the observed distribution of sanction dates among the sanctioned individuals. In my sample, 11.54% of all sanctions are imposed in October, 8.25% in November, 21.14% in December, 26.84% in January, 19.19% in February, 10.94% in March, and 2.10% in April. When randomly assigning hypothetical sanction dates based on this distribution to non-sanctioned individuals, my estimation results are of the same quality and magnitude as in the case of a uniform distribution of hypothetical sanction dates.

[45] In some cases, welfare recipients appeal against the imposition of a sanction. Social courts then have to decide on the legitimacy of the sanction. The average duration of litigations in these instances was 13.7 months in 2007 (see Statistisches Bundesamt, 2009). This period is considerably longer than my observation period of six months. Therefore, I do not expect that a potential appeal of a sanctioned survey participant against the benefit cut affects his or her behavior in my observation period. Since the sanction is in force despite the appeal, there is the same incentive to increase job search effort as in the case without an appeal.

effect of S on Y. Hence, the bias of a simple OLS estimation is undetermined ex ante.

To overcome the potential endogeneity of S, I use an instrumental variable approach estimating the sanction effect on the outcome variable Y. Specifically, I consider a binary instrument Z such that the first stage equation of a two-stage least squares estimator can be written as

$$S_i = Z_i\delta + X_i\alpha + \epsilon_i, \tag{4.2}$$

where X is the same vector of covariates as in equation (4.1) and ϵ is an error term with zero conditional mean. I allow this error term and the error term u of equation (4.1) to be correlated across observations from the same welfare agency. Under this specification, the two-stage least squares estimator provides an estimate of θ in equation (4.1) that can be interpreted as a LATE (see Imbens and Angrist, 1994): it measures the impact of a benefit cut on those individuals who receive a sanction when Z changes its value from 0 to 1.[46]

For instrument Z, I use two different variables: the sanction strategy of welfare agencies as reported by the agency managers (denoted as Z_1) and, alternatively, the observed sanction rate within the agencies (denoted as Z_2).

Welfare agencies play a crucial role in the use of sanctions. Though the imposition of sanctions is governed by legal regulations in practice there is substantial discretion at the agency level. Whether a sanction is actually imposed depends not only on the detected infringement but also on the general sanction policy of the welfare agency. Some agencies have a high affinity for sanctions, while other agencies are more reserved. In the surveys and case studies at agency level described in subsection 2.3.1, agency managers were asked about the use and importance of sanctions within their agencies. The answers of managers were classified as depicted in table 4.1.

The distribution of answers makes clear that sanctions are valued differently across welfare agencies. While some agencies use them only to comply with legal requirements, other agencies either apply sanctions more frequently, even when minor non-compliances of clients are detected, or less frequently, letting some infringements go unpunished. The agency thus has an impact on the individual probability of being sanctioned.[47] Option a) of

[46] Alternatively, instead of a two-stage least squares estimator, I could, for example, also apply a limited information maximum likelihood estimator. In the case of only one instrumental variable for one potentially endogenous variable, however, both estimators yield identical results. Moreover, the two-stage least squares estimator and the limited information maximum likelihood estimator have the same asymptotic distribution independent of the number of instruments and potential endogenous variables (see Anderson, 2005).

[47] An important instrument for agency managers in implementing a strategy are local guidelines (*verhaltenslenkende Weisungen*) for caseworkers. These guidelines are issued to control caseworker behavior in general. In some agencies they may be used to induce caseworkers to impose sanctions frequently. Other welfare agencies

Table 4.1: The use of benefit sanctions in welfare agencies

a)	Sanctions are part of the activation strategy and applied frequently	(32)
b)	Sanctions are supported but are not part of the activation strategy	(41)
c)	Sanctions are used to comply with legal requirements	(41)
d)	Sanctions are regarded sceptically but are used nevertheless	(21)
e)	Sanctions have no special role (neither positively nor negatively)	(12)
f)	Sanctions are rarely used	(5)
g)	Sanctions are used for general monitoring purposes but are not regarded as useful in single cases	(2)

Remarks: The rightmost column denotes the number of agencies falling in the respective category.

table 4.1 differentiates between agencies that strategically impose sanctions with high incidence and agencies that do not. I use this differentiation to specify my first instrument Z_1, which has a value of 1 if an individual is registered at an agency that frequently imposes sanctions as part of its activation strategy and a value of 0 otherwise, i.e. if the welfare agency is classified into one of the options b) to g) in table 4.1. Using Z_1 as an instrument, the estimate of θ depicts the LATE of a sanction on those individuals who are sanctioned in an agency that frequently imposes sanctions but who would not be sanctioned in another (more reserved) agency. In other words, it measures the impact of a sanction on those individuals who would be sanctioned if the welfare agency decided to change its sanction strategy from a more reserved one to a strict one. Thus, this LATE can be interpreted as an estimate of an intensified use of sanctions.

For my alternative instrument Z_2, I make use of the observed sanction rates within the agencies. The sanction rate of an agency is defined to be the ratio of observed individuals who received a benefit cut between October 2006 and April 2007 to all sampled welfare recipients registered at the respective agency.[48] To obtain a binary instrument, I differentiate between agencies with a sanction rate above the median ($Z_2 = 1$) and agencies with a sanction rate below the median ($Z_2 = 0$) across all 154 sampled welfare agencies. Under this specification, I estimate the LATE of a sanction on those individuals who would be sanctioned if the agency decided to increase the sanction rate from below to above the median level. Again, this effect can be interpreted as an estimate of a more intensive use of sanctions.

No matter which instrument Z is used, identification of a LATE depends on 3 conditions:

might follow a milder sanction policy and refrain from encouraging or even discourage caseworkers from imposing sanctions. This means that sanction strategy influences caseworkers' actions, creating variation in the use of sanctions across welfare agencies.

[48] Both instruments Z_1 and Z_2 are highly correlated since the strategy of imposing sanctions frequently leads to a relatively high sanction rate within an agency, as will be shown in more detail in section 4.4.

1) Z is a valid instrument, i.e. $Cov(Z, u) = 0$ and Z is partially correlated with S once it is controlled for X.[49]
2) The probability of being sanctioned changes with the value of Z.
3) Monotonicity: individuals who are sanctioned in agencies of the type $Z = 0$ must also be sanctioned in agencies of the type $Z = 1$. And those individuals who are not sanctioned under $Z = 1$ must not be sanctioned under $Z = 0$.

In the following, I verify these conditions referring to instrument Z_1, the sanction strategy of welfare agencies. A similar reasoning also applies for instrument Z_2. The monotonicity assumption is not testable, but it is very likely to hold in my analysis. If a sanction is imposed on an individual that is registered at an agency that rarely uses sanctions, the non-compliance must be severe and should also be detected and punished in an agency that sanctions frequently and cuts benefits even for minor infringements. Condition 2) is also satisfied. As will be shown in more detail in section 4.4, the sanction rate is indeed larger in agencies that frequently impose sanctions as part of their activation strategy. The probability of an individual getting a sanction, therefore, increases when Z_1 changes its value from 0 to 1. The partial correlation between Z_1 and S as required in condition 1) also exists and will be discussed in more detail in the next section.

The final condition that has to be shown to hold is $Cov(Z_1, u) = 0$. This condition requires that Z_1 has no direct causal effect on Y. A direct effect would exist if individuals changed their behavior due to the actual realization of Z_1. This, however, is highly improbable. The welfare agency's sanction strategy is not communicated to its clients, and individuals do not know whether or not sanctions are used frequently in their agency. Hence, they cannot draw comparisons with other agencies, whose strategies are even more difficult to determine. This leads one to expect the absence of behavioral responses to the sanction strategy.

A possible counterexample is provided by Lalive et al. (2005) and Arni et al. (2009). For the Swiss case, these authors argue that monitoring intensity and warnings may have a positive effect on the exit rate from unemployment, independent of the actual sanctions being imposed. If monitoring intensity and warnings are associated with higher sanction intensity, it will be difficult to identify the effect of actual sanctions without information on monitoring and warnings. It may appear that a positive association is not unlikely, as a first infringement may result in a warning and only the second or later infringement in an actual sanction if the agency pursues a tough sanction strategy.

[49] In my specification, I control for sex, age, schooling, migration background, household size, number of children, obstacles to employment, labor market status before welfare receipt, previous unemployment spells, regional information, and the duration of the current welfare spell, the last of which is measured as in chapter 3.

However, the application of sanctions in Germany differs from the Swiss case. As has been described in section 2.1, there is no formal warning process when a non-compliance is detected. The degree of caseworker discretion is higher, and warnings may or may not be given only informally. The qualitative study by Baethge-Kinsky et al. (2007) suggests that informal warnings are used regularly, but remain an abstract threat and are followed by the imposition of a sanction in few cases. As a consequence, it appears that welfare recipients can learn little about their agency's sanction policy from informal warnings given during the monitoring process.

If welfare recipients were aware of the sanction strategies of the agencies, the only way to avoid a tough sanction regime for a person registered at an agency that frequently imposes sanctions would be to move to another region, as individuals are forced to register with the agency at the place of residence. Yet financial constraints are likely to prevent welfare recipients from moving and welfare agencies are reluctant to support a move because they would have to pay most of the costs.

Sociodemographic composition of the welfare recipients or the labor market situation is also unlikely to drive an agency to adopt a particular sanction strategy. As can be seen from figure A.4.1 in the appendix to this chapter, agencies that sanction frequently and agencies that do not appear to be distributed more or less randomly across the sampled regions. In many cases, agencies that sanction frequently and non-frequently directly border each other. In the city of Berlin, five welfare agencies are sampled. Two of them use sanctions frequently whereas three do not. Since Berlin can be regarded as a single labor market, which is identical for all five agencies within the city, and since the composition of welfare recipients in these five agencies is quite similar, it is unlikely that the sanction strategy of an agency depends on the labor market state or on the sociodemographic characteristics of the welfare recipients. A similar picture arises with respect to the observed sanction rates within the agencies (see figure A.4.2).

To support the argument that sanction attitudes or aggregate sanction rates are independent of local economic performance and activation process set-up in general, I have regressed both instruments Z_1 and Z_2 on (a) a characterization describing the organizational set-up of the activation process, (b) a number of regional variables intended to approximate the economic performance of the local labor market, and (c) both. In the regression analysis, the activation process is classified into four categories (1a, 1b, 2a and 2b), where the main categories "1" and "2" distinguish between specialized and generalized case management. In the first case, caseworkers specialize in activating certain types of welfare recipients, while in the second case, caseworkers activate all welfare recipients without distinction (see also section 3.5). The subcategories "a" and "b" denote whether activation and placement are integrated or not. Subcategory "a" denotes that the functions are not integrated, which is to say, they are implemented by different staff members in the agency. In subcategory "b", welfare recipients are activated and placed

into employment by the same staff members (see also section 3.5). The regional variables include dummy variables for urban districts, East Germany, the unemployment ratio, GDP per employed person, and the welfare ratio.[50] The corresponding regression results (see table A.4.1 in the appendix to this chapter) can be interpreted as correlations between the instruments and local labor market conditions or organizational aspects that affect the transition to employment. But since none of the estimated coefficients is statistically significant, this can be interpreted to support the required orthogonality of the instruments from factors relevant for job finding, i.e. neither the sanction strategy nor actual sanction rates at the local level systematically depend on macroeconomic conditions and the organizational set-up of the activation process.

I have regressed the instruments Z_1 and Z_2 on two variables reflecting the caseload of welfare agencies (see table A.4.2 in the appendix to this chapter). The first variable relates the number of welfare recipients registered at an agency to the full-time equivalent number of overall staff in that agency. The second variable measures the ratio of welfare recipients to the full-time equivalent number of agency staff responsible for labor market activation. Neither of the two variables indicate that a high or low caseload causes the welfare agency to adopt a certain sanction approach.

Finally, I have checked the relationship between my instruments and other strategical features of welfare agencies. The surveys conducted at the agency level asked agency managers how important other activation elements are (in addition to sanctions) for the strategy of their welfare agency. These activation elements include job creation schemes, start-up subsidies, wage subsidies for employees and employers, the promotion of professional qualifications, training of key skills for the activation process, internships, the support for disadvantaged young or disabled welfare recipients, the supply of counseling services, the supply of child care facilities, and the supply of other social services. For each element, managers could answer on a scale from 1 (not important) to 5 (very important). I use this information to define a variable that distinguishes between agencies for which an element has a high strategical value and agencies for which this is not the case. The distinction between the agencies is done at the median level of managers' answers. When regressing Z_1 and Z_2 on these variables, I find no indication that the sanction approach of a welfare agency is determined by or related to other strategical considerations (see tables A.4.3 and A.4.4 in the appendix to this chapter).

Given this (descriptive) evidence, I conclude that both Z_1 and Z_2 satisfy all requirements to be valid instruments for the identification of the LATEs

[50] The three macroeconomic variables (unemployment ratio, GDP per employed person, welfare ratio) are binary dummy variables. They were measured in December 2003 and indicate a value higher than the 0.75 quantile of the distribution of the respective variable across all German welfare agencies. The welfare ratio does not include unemployment assistance recipients, referring solely to social assistance recipients. It relates the number of social assistance recipients to the resident population.

I want to estimate using equations (4.1) and (4.2). My empirical analysis is presented in the next section.

4.4 Empirical Analysis

4.4.1 Descriptive Evidence

As can be seen from table 4.2, the agency strategy to apply sanctions frequently is indeed highly correlated with the imposition of sanctions as reported by the individuals in the survey. While in the full sample 4.34% of the interviewees received a sanction between October 2006 and April 2007, the sanction rate is significantly larger for agencies that frequently impose sanctions as part of their activation strategy. Within these agencies, the sanction rate totals 5.75%, compared with 4.01% in the other agencies. This is a remarkable difference considering that the variables are measured at different levels and that the sanction strategy varies solely at the agency level. The difference is even larger when I look at the observed sanction rates. In agencies with a sanction rate above the median, 6.54% of welfare recipients received a sanction, while for the other agencies, I observe a sanction rate of 1.89%. Given these differences, both Z_1 and Z_2 fulfill the second requirement mentioned in section 4.3 for identifying a LATE: the probability of being sanctioned changes with the value of the instrument.

Table 4.2: Sanction rates and number of observations

	Full sample				
	4.34%				
	$Z_1 = 0$	$Z_1 = 1$		$Z_2 = 0$	$Z_2 = 1$
	4.01%	5.75%		1.89%	6.54%
Sanctioned	500	167		137	530
Non-sanctioned	11,957	2,737		7,121	7,573
Total observations			15,361		

Remarks: Displayed are sanction rates in the upper part of the table and absolute numbers of sanctioned and non-sanctioned individuals in the lower part. Z_1 refers to the sanction strategy of welfare agencies. $Z_1 = 0$ denotes less strict agencies, and $Z_1 = 1$ denotes frequently sanctioning agencies. Z_2 refers to the observed sanction rates within welfare agencies. $Z_2 = 0$ denotes agencies with a sanction rate below the median across all 154 sampled agencies, and $Z_2 = 1$ denotes agencies with a sanction rate above the median.

Table 4.3 presents descriptive statistics. I compare means of the variables used as covariates in the econometric analysis for sanctioned and for non-sanctioned individuals and for those individuals registered at agencies that frequently impose sanctions and for indivduals registered at all other agencies.

In addition, p-values of equality-of-means tests are displayed to allow for a meaningful discussion of the differences.

The left-hand side of table 4.3 shows that male, younger, and less qualified individuals are most likely to have a benefit sanction imposed on them. Moreover, singles, and, to a lesser degree, individuals without children are subject to sanctions relatively frequently. Disabled persons and those with care obligations are less likely to receive a sanction. There are also regional differences in sanction probabilities. In East Germany, there are fewer sanctions than in West Germany. In general, the better the labor market conditions, the more sanctions are imposed, as is reflected by the differences with respect to the unemployment ratio and the GDP. Still, no significant differences in sanction probabilities exist with respect to the share of welfare recipients in a region.

Table 4.3: Descriptive statistics of the estimation sample

	S = 0	S = 1	p	$Z_1 = 0$	$Z_1 = 1$	p	$Z_2 = 0$	$Z_2 = 1$	p
Gender									
Male	0.459	0.565	0.000	0.464	0.464	0.973	0.465	0.463	0.821
Age									
18-24	0.203	0.381	0.000	0.210	0.217	0.422	0.209	0.213	0.469
25-34	0.207	0.258	0.002	0.209	0.208	0.863	0.205	0.213	0.264
35-44	0.211	0.174	0.023	0.209	0.207	0.798	0.210	0.208	0.726
45-57	0.379	0.187	0.000	0.371	0.368	0.751	0.376	0.366	0.208
Schooling									
Secondary general school	0.443	0.505	0.001	0.441	0.466	0.013	0.412	0.475	0.000
Intermediate secondary school	0.338	0.262	0.000	0.338	0.319	0.045	0.376	0.297	0.000
University entrance diploma	0.160	0.123	0.010	0.160	0.154	0.467	0.156	0.161	0.332
Other or missing	0.059	0.109	0.000	0.061	0.061	0.939	0.056	0.066	0.006
Migration background									
Immigrant	0.247	0.258	0.539	0.252	0.231	0.018	0.223	0.270	0.000
Household size									
1 person	0.345	0.402	0.003	0.341	0.376	0.000	0.323	0.370	0.000
2 persons	0.278	0.229	0.006	0.280	0.257	0.011	0.295	0.258	0.000
3 or more persons	0.377	0.369	0.666	0.379	0.367	0.233	0.383	0.371	0.155
Number of children									
No children	0.611	0.651	0.040	0.609	0.629	0.040	0.610	0.615	0.548
1 child	0.214	0.201	0.425	0.216	0.203	0.140	0.223	0.205	0.006
2 or more children	0.175	0.148	0.075	0.176	0.167	0.293	0.167	0.180	0.030
Obstacles to employment									
Disabled person	0.105	0.054	0.000	0.103	0.101	0.766	0.098	0.106	0.119
Care obligation	0.033	0.019	0.055	0.033	0.030	0.488	0.035	0.030	0.065
Status before receipt of welfare benefits									
(Minor) employment	0.322	0.396	0.000	0.326	0.325	0.943	0.319	0.331	0.109
Number of previous unemployment spells									
0 or 1	0.389	0.370	0.340	0.387	0.393	0.541	0.391	0.385	0.418
2 or 3	0.370	0.379	0.624	0.373	0.358	0.130	0.364	0.376	0.150
4 or more	0.183	0.196	0.384	0.183	0.187	0.606	0.188	0.180	0.209
Missing	0.058	0.054	0.644	0.057	0.062	0.317	0.056	0.059	0.428
Regional information									
Urban district	0.300	0.337	0.040	0.289	0.355	0.000	0.283	0.318	0.000
East Germany	0.253	0.153	0.000	0.244	0.267	0.009	0.369	0.140	0.000
Unemployment ratio (high)	0.255	0.160	0.000	0.247	0.263	0.074	0.366	0.147	0.000

Continued on next page

Table 4.3: Descriptive statistics of the estimation sample (continued)

	S = 0	S = 1	p	$Z_1 = 0$	$Z_1 = 1$	p	$Z_2 = 0$	$Z_2 = 1$	p
GDP per employed person (high)	0.286	0.361	0.000	0.291	0.280	0.216	0.209	0.360	0.000
Welfare ratio (high)	0.284	0.289	0.779	0.276	0.323	0.000	0.281	0.287	0.404
Current welfare spell									
Months in welfare before 10/2006	12.437	10.927	0.000	12.356	12.440	0.660	12.806	11.982	0.000
Start after 10/2006 or missing	0.183	0.178	0.774	0.186	0.170	0.047	0.183	0.182	0.912
Observations	14,694	667	15,361	12,457	2,904	15,361	7,258	8,103	15,361

Remarks: S = 0 denotes non-sanctioned individuals, and S = 1 denotes sanctioned individuals. Z_1 refers to the sanction strategy of welfare agencies. $Z_1 = 0$ denotes less strict agencies, and $Z_1 = 1$ denotes frequently sanctioning agencies. Z_2 refers to the observed sanction rates within welfare agencies. $Z_2 = 0$ denotes welfare agencies with a sanction rate below the median across all 154 sampled agencies, and $Z_2 = 1$ denotes agencies with a sanction rate above the median. The p-values derive from equality-of-means tests of the displayed variables for S = 0 and S = 1, for $Z_1 = 0$ and $Z_1 = 1$, and for $Z_2 = 0$ and $Z_2 = 1$, respectively.

The middle part of table 4.3 clearly shows that agencies with different sanction strategies are quite similar with respect to the composition of their clients. Only small differences are apparent in schooling achievement, household composition, and migration background. Slightly more singles, persons without children, and persons with the lowest school leaving certificate (secondary general school) are registered at agencies that frequently impose sanctions than at other agencies. Immigrants are somewhat under-represented. More important differences exist with respect to regional characteristics. In rural areas, agencies seem to impose fewer sanctions than in urban regions. For East Germans, the probability of being registered at an agency with a tough sanction policy is somewhat higher than for West Germans. No clear picture arises with respect to the macroeconomic variables. While agencies that apply sanctions frequently are over-represented in regions with a high welfare ratio, almost no difference exists with respect to the unemployment ratio. Nor is there a statistically significant difference with respect to GDP. Hence, the labor market state does not seem to determine the sanction strategy of an agency, as is required for a strategy in order to be a valid instrument (see also the evidence presented in section 4.3). Nevertheless, the descriptive analysis makes it clear that one should control for regional variables when estimating the effect of benefit cuts. This is particularly important when using instrument Z_2. As can be seen from the right-hand side of table 4.3, differences between agencies with $Z_2 = 0$ and $Z_2 = 1$ are somewhat larger than between agencies with $Z_1 = 0$ and $Z_1 = 1$.

Table 4.4 displays the means of my outcome variable self-sufficient employment for sanctioned and non-sanctioned individuals registered either at agencies that frequently impose sanctions or at agencies that do not. The means of the variables are measured for the first six months after the (hypothetical) sanction date.

Table 4.4: Description of the outcome variable self-sufficient employment

	Z_1				Z_2			
	$Z_1 = 0$		$Z_1 = 1$		$Z_2 = 0$		$Z_2 = 1$	
	S = 0	S = 1	S = 0	S = 1	S = 0	S = 1	S = 0	S = 1
Month								
1	0.0591	0.0260	0.0617	0.0240	0.0524	0.0292	0.0664	0.0245
2	0.0648	0.0460	0.0745	0.0479	0.0588	0.0511	0.0739	0.0453
3	0.0736	0.0560	0.0811	0.0898	0.0653	0.0511	0.0841	0.0679
4	0.0840	0.0640	0.0891	0.1257	0.0729	0.0657	0.0964	0.0830
5	0.0965	0.0740	0.1023	0.1677	0.0829	0.0876	0.1114	0.1000
6	0.1059	0.0880	0.1155	0.2036	0.0932	0.0949	0.1212	0.1226

Remarks: The outcome variable is defined to be 1 if an individual is employed and does not receive welfare benefits anymore. Otherwise, the variable is 0. The variable is displayed for the first six months after the (hypothetical) sanction date. S = 0 denotes non-sanctioned individuals, and S = 1 denotes sanctioned individuals. Z_1 refers to the sanction strategy of welfare agencies. $Z_1 = 0$ denotes less strict agencies, and $Z_1 = 1$ denotes frequently sanctioning agencies. Z_2 refers to the observed sanction rates within welfare agencies. $Z_2 = 0$ denotes welfare agencies with a sanction rate below the median across all 154 sampled agencies, and $Z_2 = 1$ denotes agencies with a sanction rate above the median.

As can be seen from table 4.4, the share of persons taking up self-sufficient employment and thereby leaving the welfare system increases over time in all subgroups. For example, 5.91% of the non-sanctioned persons who are registered at agencies that do not follow a tough sanction strategy ($Z_1 = 0$) have left the welfare system for employment one month after the (hypothetical) sanction date. This share increases steadily and amounts to 10.59% six months after the (hypothetical) sanction. For the sanctioned group in the same agencies, I observe an increase in the outflow rate from 2.60% to 8.80%. When looking at agencies that frequently impose sanctions ($Z_1 = 1$), I detect an even more pronounced increase in the employment probability of sanctioned individuals. Here, outflow rates start at 2.40% in the first month and rise to more than 20% in the final month of my observation period. For the alternative instrument Z_2 and individuals who are sanctioned in agencies with a sanction rate above the median ($Z_2 = 1$), I observe a more modest increase in employment rates from 2.45% to 12.26%. Nevertheless, as in the case of Z_1, this subgroup has the largest employment share at the end of my observation period.

Irrespective of the instrument, employment rates for sanctioned individuals start at a lower level than the corresponding figures of the non-sanctioned. This might reflect the fact that the sanctioned persons are a selective group that faces disadvantages with respect to labor market participation. As shown in table 4.3, for example, less qualified persons are over-represented among the sanctioned group. But at the end of my observation period, sanctioned persons do better than non-sanctioned individuals. The only exception is employment uptake in agencies that do not follow a tough sanction strategy ($Z_1 = 0$). Here, employment rates after six months are about 1.8 percentage points lower for sanctioned individuals than for non-sanctioned welfare recipients. Given the large increase in employment rates among sanctioned

individuals, however, this group is likely to catch up and to overtake the non-sanctioned group shortly after my observation period ends. The numbers displayed in table 4.4 thus provide descriptive evidence for the effectiveness of sanctions.

4.4.2 Estimation Results

My econometric analysis confirms the descriptive evidence.[51] Before describing the results in detail, I briefly look at the first stage regression results. The first stage regression is identical for each of my six outcome months after the (hypothetical) sanction date and is presented for instrument Z_1 in the left part of table 4.5. As the table reveals, the instrument has a significantly positive effect on the sanction probability. Individuals registered at agencies that frequently impose sanctions have a 2.86 percentage points higher chance of receiving a sanction than individuals registered at other agencies. Given the low average sanction rate on average, this is a large effect. The effect is also large in absolute terms when compared with the coefficients of the other covariates considered.[52]

[51] For the estimation of the Local Average Treatment Effects (LATEs), I make use of the sample weights contained in the data. The estimated standard errors take into account the clustering of observations at agency level (see Moulton, 1986 and 1990).

[52] With respect to the other covariates included in the model, I observe that men are more likely to receive benefit cuts than women. Younger individuals (age 18 to 24) face a higher risk of being sanctioned than older people. In addition, the less educated are relatively prone to sanctions. Individuals with the lowest educational achievement (secondary general school) have a sanction probability 1.3 to 2.1 percentage points higher than persons with an intermediate secondary school degree or a university entrance diploma, respectively. Household size and the number of children do not influence sanction probability. Disabled persons and persons with care obligations are less likely to receive a benefit cut, but the effect is statistically insignificant. No differences are found between immigrants and native Germans. Those individuals who have been employed before welfare receipt are more likely to receive a sanction. Previous spells of unemployment do not matter, nor do the regional variables included in the model have any effect on sanction probability. Labor market or macroeconomic conditions in the regions, therefore, do not influence or determine whether a person is sanctioned.

Table 4.5: First stage estimation results based on instruments Z_1 and Z_2

	Z_1	Z_2
Z	0.0286***	0.0478***
	(0.0087)	(0.0043)
Gender (reference: female)		
Male	0.0177***	0.0184***
	(0.0051)	(0.0051)
Age (reference: 25 to 34 years)		
18 to 24 years	0.0184*	0.0180*
	(0.0101))	(0.0101)
35 to 44 years	−0.0171**	−0.0172**
	(0.0084)	(0.0085)
45 to 57 years	−0.0337***	−0.0345***
	(0.0097)	(0.0097)
Schooling (reference: secondary general school)		
Intermediate secondary school	−0.0131**	−0.0116**
	(0.0053)	(0.0053)
University entrance diploma	−0.0214***	−0.0203**
	(0.0082)	(0.0083)
Other or missing	0.0097	0.0091
	(0.0110)	(0.0109)
Migration background (reference: native Germans)		
Immigrant	0.0049	0.0044
	(0.0074)	(0.0072)
Household size (reference: 2 persons)		
1 person	0.0090	0.0077
	(0.0067)	(0.0068)
3 or more persons	-0.0046	-0.0046
	(0.0063)	(0.0062)
Number of children (reference: 1 child)		
No children	-0.0010	-0.0018
	(0.0069)	(0.0069)
2 or more children	-0.0050	-0.0070
	(0.0072)	(0.0071)
Obstacles to employment		
Disabled	-0.0078	-0.0085
	(0.0133)	(0.0135)
Care obligation	-0.0106	-0.0063
	(0.0099)	(0.0098)
Status before receipt of welfare benefits		
(Minor) employment	0.0141**	0.0136**
	(0.0058)	(0.0058)
Number of previous unemployment spells (reference: 2 or 3)		
0 or 1	-0.0014	-0.0007
	(0.0047)	(0.0046)

Continued on next page

Table 4.5: First stage estimation results based on instruments Z_1 and Z_2 (continued)

	Z_1	Z_2
4 or more	0.0140	0.0139
	(0.0095)	(0.0095)
Missing	0.0043	0.0046
	(0.0104)	(0.0103)
Regional information		
Urban district	0.0088	0.0110
	(0.0085)	(0.0086)
East Germany	-0.0179	-0.0043
	(0.0142)	(0.0114)
Unemployment ratio (high)	-0.0001	-0.0023
	(0.0139)	(0.0105)
GDP per employed person (high)	0.0025	-0.0036
	(0.0077)	(0.0064)
Welfare ratio (high)	-0.0052	-0.0041
	(0.0074)	(0.0077)
Current welfare spell		
Months in welfare before 10/2006	-0.0007	-0.0006
	(0.0005)	(0.0005)
Start after 10/2006 or missing	-0.0229^{**}	-0.0205^{**}
	(0.0103)	(0.0104)
Constant	0.0594^{***}	0.0370^{***}
	(0.0103)	(0.0106)
Observations	15,361	15,361
F statistic	10.64	122.09

Remarks: The dependent variable in each estimation is defined to be 1 if an individual was sanctioned between October 2006 and April 2007. Otherwise, the variable is 0. The F statistic derives from a test of significance of the instrumental variable Z in the respective estimation. Z_1 refers to the sanction strategy of welfare agencies. $Z_1 = 0$ denotes less strict agencies, and $Z_1 = 1$ denotes frequently sanctioning agencies. Z_2 refers to the observed sanction rates within welfare agencies. $Z_2 = 0$ denotes welfare agencies with a sanction rate below the median across all 154 sampled agencies, and $Z_2 = 1$ denotes agencies with a sanction rate above the median. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. The standard errors take into account clustering at the agency level.

The F statistic for the significance of the instrument Z_1 in the first stage regression is 10.64. It is thus close to the threshold value of 10 suggested by Staiger and Stock (1997) as an indication of a potential weak instrument problem.[53] To determine whether a weak instrument problem is in fact present, I use the second instrument Z_2, which differentiates between welfare agencies with a sanction rate above and below the median level across all 154 sampled agencies. For Z_2, I estimate an even larger impact on the

[53] I rely on this rule of thumb because I cannot apply the tests proposed by Stock and Yogo (2005): I use only one instrument and do not assume homoscedastic errors; instead, I allow for clustering at the agency level.

individual sanction probability in the first stage regression. As can be seen from the right-hand side of table 4.5, the coefficient of the instrument Z_2 is highly significant and amounts to 4.78 percentage points. The F statistic is 122.09 – considerably larger than the threshold value. Besides giving further insight into the effectiveness of sanctions, Z_2 thus allows us to assess how precisely the sanction effect is estimated using instrument Z_1.

Based on Z_1, the upper part of table 4.6 provides the estimation results for the Local Average Treatment Effect (LATE) of a sanction on self-sufficient employment in the first six months after the (hypothetical) benefit cut.[54] The positive coefficients of the sanction variable reveal that the outflow from welfare to self-sufficient employment increases due to an imposed sanction even after controlling for a number of individual and regional characteristics. During the first three months, the effect is modest and not statistically significant. In the fourth month, I observe a large increase in the outflow rate. The effect becomes statistically significant in the fifth month and amounts to 0.6766 at the end of the observation period. This figure indicates that a benefit cut induced by a change of the sanction regime increases the probability that affected individuals will leave the welfare system within six months after the benefit cut by nearly 68 percentage points. It thus constitutes a considerable sanction effect.

Table 4.6: Estimated sanction effects

Month	1	2	3	4	5	6
Sanction	0.0092	0.1347	0.1689	0.4475	0.5604*	0.6766***
(based on Z_1)	(0.2015)	(0.2383)	(0.2436)	(0.2795)	(0.2927)	(0.2607)
Sanction	0.2802**	0.2402**	0.3594***	0.4496***	0.4950***	0.5802***
(based on Z_2)	(0.1124)	(0.1216)	(0.1249)	(0.1353)	(0.1437)	(0.1396)
Observations			15,361			

Remarks: The upper part of the table refers to the estimation in which Z_1 (sanction strategy of welfare agencies) is used as instrument; the lower part of the table refers to the estimation in which Z_2 (observed sanction rates within welfare agencies) is used as instrument. The dependent variable in each estimation and for each month is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0. Displayed are the estimated sanction effects and standard errors in brackets. The results refer to the first six months after the (hypothetical) sanction date. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. The standard errors take into account clustering at the agency level. Detailed estimation results for Z_1 and Z_2 including all covariates are displayed in tables A.4.5 and A.4.6 in the appendix to this chapter.

[54] Table A.4.5 in the appendix to this chapter depicts the detailed estimation results including all considered covariates.

The precision of the estimated effects, however, suffers from large standard errors. I thus use instrument Z_2 to check for robustness. As can be seen from the lower part of table 4.6, the results based on this instrument reveal a similar effectiveness of sanctions as obtained by using instrument Z_1, but results are more precisely estimated as reflected by the lower standard errors.[55] Again, I observe an increasing sanction effect during the observation period. In the first three months, the estimated effects are larger than in the case of Z_1 and already statistically significant. At the end of the observation period, the effects are somewhat lower compared with Z_1 but highly significant. Six months after the (hypothetical) benefit cut, the effect amounts to 0.5802. This figure signifies that the probability of taking up self-sufficient employment for a welfare recipient who is at risk of being sanctioned increases by about 58 percentage points when the welfare agency decides to increase the sanction rate from below to above the median level, thereby imposing a sanction on the individual. The effect is in the same range as the effect measured by instrument Z_1. Given the similarity between the two estimates, I conclude that my estimation approach does not suffer from a weak instrument problem. The sanction effect is of considerable size.

The smaller size of the coefficient for the treatment variable after six months in the estimation approach using Z_2 rather than Z_1 as the instrumental variable is not necessarily due to the reduced variance of the estimates. To stress again, my estimate is for the Local Average Treatment Effect. This effect is identified only for the so-called compliers, who receive a sanction when the instrument changes its value from 0 to 1. As I use two different instruments, I look at two different groups of compliers. One group is affected by a change in the sanction strategy of the welfare agency, whereas the other is affected by a change in the observed sanction rate. Since both groups are not identical, it cannot be expected that the estimated Local Average Treatment Effects are exactly the same.

The estimated figures for both instruments fit with findings in the existing literature. For example, van den Berg et al. (2004) find that the probability of an average 25-year-old Dutch welfare recipient in the city of Rotterdam of leaving the welfare system within two years after inflow increases from 66% to 91% if a sanction is imposed after six months of being on welfare. For a 50-year-old, the corresponding probability increases from 29% to 54%. Keeping in mind that welfare recipients have, if at all, only limited savings to rely on during a period of reduced benefit payments, the substantial effects of sanctions are in line with expectations.

The influence of the other covariates is as expected but far more modest quantitatively when compared with the sanction effect (see tables A.4.5 and A.4.6 in the appendix to this chapter). Men are more likely to find a job than women. Individuals age 25 to 34 face better employment prospects than all other age groups. Employment chances increase with the level of educational

[55] Detailed results including all considered covariates are displayed in table A.4.6 in the appendix to this chapter.

attainment. Immigrants, disabled people, and persons with care obligations have a lower chance of finding a job. Individuals with children are also more at risk of remaining in the welfare system. Persons who have been employed prior to welfare receipt tend to have better chances of finding a job than formerly non-employed persons, but the effect is only statistically significant at the beginning of the observation period. The number of previous unemployment spells does not matter much for employment chances. With respect to the regional and macroeconomic variables included in the model, I find that persons living in regions with high GDP have relatively good chances of finding a job, whereas employment probabilities are lower in East Germany and in regions with a high welfare ratio (as measured at the end of 2003).

I note again that the estimated sanction effects have to be interpreted as LATEs. They measure the effect of a sanction on those individuals who are not sanctioned in an agency with a less strict sanction regime but who will be sanctioned if the agency decides to change its policy and impose sanctions more frequently. Thus, these LATEs can be interpreted as an estimate of the effect of an intensified use of sanctions. My results show that tightening the sanction policy is quite effective in reducing welfare dependency and increasing employment uptake.

To check for the sensitivity of my results and to investigate potential effect heterogeneity of sanctions, I look at different subgroups of welfare recipients (see table A.4.7 in the appendix to this chapter). First, I distinguish between individuals registered at centralized and decentralized welfare agencies. For both agency types, I find a similar result as in the overall sample.[56] Sanctions exhibit strong positive employment effects. The effect is larger in decentralized than in centralized agencies. Six months after the (hypothetical) sanction date, the effect amounts to 0.6903 in decentralized welfare agencies and to 0.4477 in centralized agencies. Even more pronounced differences are found with respect to gender. Women react more strongly to sanctions than men. For female welfare recipients, I estimate a sanction effect of 0.8216 at the end of the observation period, whereas the same effect amounts to 0.4309 for men. Differences at the end of the observation period are also found for native Germans and immigrants.[57] The latter subgroup (0.7281) shows a more pronounced sanction effect than the former (0.5143). But there is no clear picture on the relative effect size between both groups during the earlier months of the observation period. In four of the five earlier months, effects are larger for native Germans than for immigrants.

It has to be kept in mind that the number of sanctioned individuals in the overall sample is small and even smaller in the subgroups. All subgroup

[56] For the sensitivity analyses, I solely use instrument Z_2. Instrument Z_1 does not allow for a sufficient number of observations of sanctioned individuals registered at decentralized welfare agencies that frequently impose sanctions as part of their activation strategy.

[57] I apply the definition of immigrants outlined in subsection 2.3.1 and used in chapter 3.

analyses thus have to be interpreted with caution. The sensitivity checks nevertheless indicate a considerable sanction effect.

4.5 Summary

Recent studies investigating the effect of sanctions imposed on unemployment insurance benefits and welfare recipients show that benefit cuts substantially reduce unemployment and increase employment uptake among the sanctioned individuals. Their empirical basis is limited, however, and it is hard to generalize the findings. First, the existing studies are restricted to small geographic areas and may thus be based on a specific subgroup of benefit recipients. Second, most studies rely on limited individual information. In addition, virtually all studies neglect the fact that the imposition of sanctions depends not only on characteristics of the individual but also on the policy of the welfare agency at which the individual is registered and which is in charge of the actual imposition of sanctions.

My data for German welfare agencies show that benefit sanctions are not imposed uniformly when an individual does not comply with his or her duties during the activation process. Rather, there is substantial discretion at the agency level determining whether a sanction is applied. While some agencies frequently impose sanctions, the policy of others is less tough. I use these differences in sanction strategies and rates across the 154 sampled welfare agencies as instrumental variables to estimate the effect of a benefit cut on individual employment probability. Specifically, I estimate the effect of a sanction on those individuals who are not sanctioned by an agency with a reserved sanction policy but who would be sanctioned if the agency decided to impose sanctions more frequently. This LATE can be interpreted as an estimate of the effectiveness of an intensified use of sanctions.

My results show that an intensified use of sanctions is quite effective in increasing employment rates. A sanction increases the probability of the affected compliers to leave the welfare system for self-sufficient employment within six months after the benefit cut by 58 to 68 percentage points. There is some effect heterogeneity across the subgroups (individuals registered at centralized and decentralized welfare agencies, men and women, native Germans and immigrants), but all sensitivity checks confirm a substantial sanction effect. I thus conclude that a more intensive use of benefit cuts by welfare agencies within the legal framework and within the principle of "supporting and demanding" (*Fördern und Fordern*) will contribute to making the labor market activation of welfare recipients more effective and will substantially increase the outflow rate from welfare to employment.

Chapter 5

The Effectiveness of Temporary Extra Jobs and Short-Term Training Programs*

5.1 Introductory Remarks

Besides a more intensive monitoring of job search efforts with the potential imposition of benefit sanctions, the principle of "supporting and demanding" (*Fördern und Fordern*) also requires that welfare agencies do everything in their power to increase the employability of welfare recipients and, thereby, to increase their employment chances. To this end, the 2005 welfare reform introduced a comprehensive set of Active Labor Market Programs (ALMP) whose primary aim is to integrate welfare recipients into the labor market and to reduce welfare dependency rates (see e.g. Huber et al., 2011). For the first time in German welfare policy, this set provides a consistent activation framework for all welfare recipients (see chapter 2). In this chapter, I evaluate the effectiveness of the most important ALMP used since 2005: so-called Temporary Extra Jobs (*Arbeitsgelegenheiten in der Mehraufwandsvariante*) and four different short-term training programs (*Trainingsmaßnahmen*).

The analysis has a particular focus on persons with a migration background. Given the high rate of immigrants in welfare, this group can be perceived to have a higher need for participation in ALMP. Nevertheless, little is known about the impacts of programs aiming at the labor market integration of immigrant welfare recipients. A reason for this may be the absence of ALMP specifically designed for the needs of immigrants.[58] Instead,

* This chapter builds on joint work with Alisher Aldashev and Stephan Thomsen. It is a revised and extended version of Aldashev et al. (2010) and Thomsen and Walter (2010a). Extensions include subgroup analyses for individuals with a Turkish, an Eastern European, or a Southern European migration background. The work of Thomsen and Walter (2010a) has been used here with kind permission of Wiley-Blackwell.

[58] The lack of information is not particular to Germany. To the best of my knowledge, there exist only three studies that analyze ALMP with a focus on immigrants. Clausen et al. (2009) evaluate the effects of ALMP on the hazard rate of regular employment for newly arrived immigrants in Denmark. The programs they consider are part of

immigrants participate in intervention programs that have been designed for all welfare recipients.

One major program widely used for native and immigrant welfare recipients consists of Temporary Extra Jobs, which offer temporary job opportunities in the public sector. The jobs comprise a variety of activities, frequently in community services or in public infrastructure. To avoid distortions of competition, jobs must be additional in nature and must not compete with regular employment. Since their introduction, more than 700,000 welfare recipients have been newly employed in Temporary Extra Jobs each year (see table 2.5 in chapter 2), making the program the most frequently used ALMP in the German welfare system. The main purpose of the program is to maintain and improve the employability of the participants and to be a means for (later) integration into regular jobs.

Short-term training programs are also widely used for the activation of native and immigrant welfare recipients. Short-term training lasts for a maximum of 12 weeks and contains aptitude tests, courses improving job search skills, and the provision of necessary skills and abilities required for employment. Easily adaptable to individual needs, these training programs combine elements of job search assistance and short vocational qualification. Their objective is to improve individual employability by increasing search efficiency and productivity.

I evaluate the effects of Temporary Extra Jobs and four different short-term training programs for their ability to help native and immigrant welfare recipients find self-sufficient employment. I am particularly interested in the following questions: Are programs similarly effective for both immigrants and natives, or do their effects differ? And, if differences in effects are observable between natives and immigrants, are they caused by unobserved differences between them? The purpose of the second question is to determine the potential differences in program effectiveness that are not caused by observable differences in group composition. I refer to differences in program effectiveness caused by unobservable differences between immigrants and natives as "immigrant fixed effects".

To answer my questions, I use the data described in section 2.3.2, which provides detailed information about sociodemographic characteristics, labor market history, program participation, and the outcome variable of interest for native and immigrant welfare recipients. An important feature of these data is that they allow the identification of immigrants beyond the concept of citizenship. I analyze the program effects of participation in Temporary Extra

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the integration policies specifically designed to facilitate the labor market integration of newly arrived immigrants (introduced in 1999). In addition, Cohen-Goldner and Eckstein (2010) evaluate a government-provided training program in Israel for highly-skilled female immigrants from the former Soviet Union. A third study, by Hämäläinen and Sarvimäki (2010), evaluates the effects of integration plans for immigrants in Finland. The aim of these plans is to promote social and labor market integration by offering measures that help provide information and skills needed in Finnish society.

Jobs or short-term training during the first year of the welfare spell. The estimation of the treatment effects is carried out by propensity score matching estimators in a dynamic setting in which treatment effects vary conditionally on the preceding duration in welfare. As has been noted by Sianesi (2004), it is important to consider the timing of treatment when evaluating ALMP. To answer the second question, I estimate differences in treatment effects between natives and immigrants that result from the attachment of the individual to the immigrant group. The estimation is carried out with a matching estimator and considers only treated individuals for whom group attachment is the treatment in question.

Whether these programs reach their purposes for immigrants and natives is not clear ex ante, and there are a number of factors that can affect outcome. Clearly, successful integration depends on labor demand. If participant productivity is too low and programs are unable to increase productivity to meet the required standards, there will not be any effect. Similarly, if potential employers practice discrimination then even if productivity is improved by participation the probability of placement could be different between immigrants and natives. In the absence of demand-side effects, differences in placement may result from a different value of the programs for immigrants and natives. If programs are geared to the needs of the average native welfare recipient, they may not work (well) for immigrants, and policy makers should think about restructuring programs to address the specific needs of immigrants. Conversely, if programs are particularly effective for immigrants even though not initially designed for them, policy makers should intensify their use. Determining the source of differences in program effectiveness between the groups is important. If, for example, differences in program effectiveness are caused by differences in the sociodemographic composition of native and immigrant welfare recipients, a general potential for welfare agencies to improve the targeting of programs to participants is implied. If differences are due to a migration background, then this points to other reasons, e.g. discrimination, and the use of programs for either immigrants or natives should be reconsidered.

The remainder of this chapter is organized as follows. In the next section, I summarize the related literature. Section 5.3 provides more information on Temporary Extra Jobs and short-term training programs. In section 5.4, I describe my estimation sample and the available information for the empirical analysis. My evaluation approach is discussed in section 5.5. In section 5.6, I present the estimation results. The final section concludes my findings.

5.2 Related Literature

Unlike the US, where welfare research has played a more prominent role and ALMP for welfare recipients have been adopted in various states, particu-

larly during the 1990s, in European countries ALMP in the welfare system have become important more recently only as a result of substantial reforms. Accordingly, the empirical literature evaluating social intervention programs in Europe focuses on ALMP for unemployment insurance benefit recipients rather than for welfare recipients. Reviews of the numerous available studies are provided by Martin and Grubb (2001), Kluve and Schmidt (2002), and Kluve (2010).

Employment programs similar to Temporary Extra Jobs, e.g. job creation schemes (*Arbeitsbeschaffungsmaßnahmen*), exist in a number of countries and have been used for persons covered by unemployment insurance in Germany as well. Comprehensive evaluations of those programs in Germany report disappointing results with respect to the employment chances of participating individuals (see e.g. Hübler, 1997; Thomsen, 2007; and Hujer and Thomsen, 2010). These findings are in line with the international experiences (see e.g. Puhani, 1999, for Poland; Gerfin and Lechner, 2002, for Switzerland; and Fredriksson and Johansson, 2003, for Sweden). In a meta-analysis, Kluve (2010) considers the effects of different ALMP from 19 countries. Within the category "direct employment programs in the public sector", he regards activities aimed at direct job creation or public work provision and other activities that produce public goods and services. He identifies 24 studies evaluating this kind of program. Only five of the studies considered find those programs to be effective. Twelve studies report negative effects for individuals, and another seven report no effects (insignificant estimates). Thus, the international picture on direct job creation is not very promising with regard to reducing unemployment and increasing employment.[59]

Directly transferring these findings to the case of welfare recipients participating in Temporary Extra Jobs and, more specifically, to the large group of immigrant welfare recipients is not possible: welfare recipients usually differ from persons covered by unemployment insurance with respect to employment chances and other characteristics relevant for the labor market. The Temporary Extra Jobs program thus requires its own form of evaluation.

To the best of my knowledge, Temporary Extra Jobs have been analyzed in three former studies by Hohmeyer and Wolff (2007), Hohmeyer (2009), and Huber et al. (2011). These studies are not without drawbacks, however. Hohmeyer and Wolff (2007) use a January 2005 stock sample of welfare recipients who participated in Temporary Extra Jobs from February to April 2005 and who were followed for 20 months after having started. They find insignificant effects on employment uptake for men and slightly positive effects for women. They also distinguish between immigrants and native Germans and

[59] In this context, it should be mentioned that not only the features of the programs and the addressed target groups of unemployment insurance benefit or welfare recipients differ slightly across countries, but so do the empirical methods employed to obtain the results. This has to be considered in the interpretation of Kluve's (2010) findings. Nevertheless, there tends to be a robust negative tendency in the results of public-sector direct employment programs (see Card et al., 2010).

find a positive employment effect for female immigrants in West Germany at the end of their observation period.[60] Yet, several shortcomings may cast doubt on the reliability of the estimates. First, the substantial German welfare reform and Temporary Extra Jobs were not introduced until 2005, and implementation took almost the whole year due to a number of problems. In particular, data collection and data transmission problems in the welfare agencies occurred during that time, which may have affected the results (see section 2.3). Second, the evaluation design does not take into account the exact timing of treatment during the welfare spell. The underlying stock sample consists of individuals who entered the welfare system quite recently in January 2005 and of individuals who had already been registered in the former systems of unemployment assistance and social assistance for months or years. The considered treatment is thus very heterogenous. While for some individuals the treatment sets in immediately at the beginning of their welfare spell, others have spent a substantial amount of time on welfare benefits before receiving treatment. The estimated treatment effect is, thus, an undefined mixture of the effects of early and late program participations during the welfare spell. The third and final point is that the choice of the outcome variable is not ideal. Hohmeyer and Wolff (2007) look at unconditional employment, i.e. employment uptake irrespective of welfare status, but it remains unclear what can be learned from this variable. A more appropriate outcome is the combined measure indicating drop-out from welfare receipt conditional on employment uptake – in other words, self-sufficient employment. The success of the program can better be assessed with this combined measure as its objective is to reduce welfare dependency by bringing welfare recipients back into employment.

Huber et al. (2011) apply this combined measure. They use the data set described in subsection 2.3.1 and evaluate the effects of a set of different ALMP including Temporary Extra Jobs. Based on the stock sample of welfare recipients from October 2006 with an observation period through the end of 2007, they find no significant treatment effects for participation in Temporary Extra Jobs on leaving welfare by taking up employment. They also attempt to consider immigrants separately, but the sub-sample size becomes very small, and no significant effects are obtained. Based on the comparably informative data for the later time period, the estimated effects are not burdened with implementation or data collection problems. As in the case of Hohmeyer and Wolff (2007), however, the evaluation design does not take into account the exact timing of treatment during the welfare spell.

As far as short-term training is concerned, similar programs are also used in many other countries and commonly subsumed under the term "job search

[60] Hohmeyer (2009) uses the same data as Hohmeyer and Wolff (2007) but extends the observation period to 28 months and distinguishes between different types of Temporary Extra Jobs according to overall program duration and working hours per week. She finds similar results as Hohmeyer and Wolff (2007) but does not look at immigrants separately.

assistance programs". Activities comprise a more intensified counseling of the benefit recipients, a close monitoring of search efforts by the institution administrating the benefits, and usually so-called individual action plans, including the provision of short courses to improve skills required for a successful job application process and training of specific skills and techniques to enhance the employment chances of the individuals.[61] Leaving country-specific differences in labor market institutions, program characteristics and enrolment rules aside, the overall picture of the employment effects of these programs is quite promising. In the majority of cases studied, programs tend to increase the employment chances of participating individuals. Since interventions are usually short in duration or, if arranged over a longer interval of the benefit spell, do not require full-time participation of the benefit recipients, the risk of potential locking-in effects is small. If locking-in effects are present, persons reduce their search efforts substantially while in the program, resulting in a lower transition rate to employment.

Despite the comprehensive literature analyzing the impacts of this kind of programs, persons with a migration background have been considered as a subgroup for Germany only by Wolff and Jozwiack (2007) and Huber et al. (2011).[62] Using the same data set used by Hohmeyer and Wolff (2007) for the evaluation of Temporary Extra Jobs and a coarser definition of immigrants than in this thesis, Wolff and Jozwiack (2007) estimate the effect of participation in short-term training on the probability of taking up unconditional employment. Their results indicate that participation increases the employment chances of male immigrants in West Germany but tends to decrease them for male immigrants in East Germany. For female immigrants in East and West Germany, the estimated effects are negative during the first six months after program start but tend to be positive at the end of the observation period (20 months after program start). Welfare recipients without a migration background benefit from short-term training. For all considered subgroups of native Germans (men and women in East and West Germany), Wolff and Jozwiack (2007) estimate positive employment effects.

Besides analyzing Temporary Extra Jobs, Huber et al. (2011) also evaluate short-term training programs. They consider self-sufficient employment as the outcome variable of interest. For this outcome variable, the authors report positive effects of short-term training for persons without a migration background and insignificant effects for immigrants. Although the studies of

[61] Besides others, a similar program covering these features in France has been studied by Crépon et al. (2005), in England by Blundell et al. (2004), and in Denmark by Graversen and van Ours (2008). Thomsen (2009) provides a detailed synopsis on the effects of these programs from empirical studies in nine European countries. Experiences in the US are summarized in the articles by Meyer (1995) and Ashenfelter et al. (2005).

[62] Short-term training programs for persons covered by unemployment insurance in Germany have been evaluated, for example, by Hujer et al. (2006), Biewen et al. (2007), Wunsch and Lechner (2008), and Osikominu (2009). In addition, programs for welfare recipients have also been studied by Kopf (2009).

Wolff and Jozwiack (2007) and Huber et al. (2011) provide important results about the effects of short-term training, they suffer from the same potential problems as the cited studies evaluating Temporary Extra Jobs.

To avoid these potential problems, I employ an inflow sample of welfare recipients and take into account the exact timing of treatment. Moreover, I look separately at Temporary Extra Jobs and at four different short-term training programs. I do not pool different types of training into one treatment category as in Wolff and Jozwiack (2007) and Huber et al. (2011). The size of my sample allows for a meaningful comparison of estimated treatment effects for immigrants and natives. A further contribution in this chapter is the attempt to identify causes of potential differences in treatment effects between groups. Neither of the available studies tries to explain effect heterogeneity.

5.3 Institutional Background

5.3.1 Temporary Extra Jobs

Temporary Extra Jobs were introduced into German ALMP as part of the 2005 reform of the German welfare system. They provide temporary work opportunities for particularly hard-to-place welfare recipients in order to maintain or enhance the employability of the participants and to improve the job chances for regular employment. For this reason, Temporary Extra Jobs were designed to be used as a "last resort" of activation; they are by no means intended for the majority of needy people. Occupations in Temporary Extra Jobs have to be additional in nature, of value for society, and must not compete with regular jobs in the market. Activities are additional in nature if they are not undertaken now or in the near future by existing public or private sector firms. Occupations are of value for the society if the outcome is for the collective good. The last condition should rule out any deadweight losses and substitution effects (see Calmfors, 1994 and 1995, for a detailed discussion of these effects for ALMP) that could result from the activities. Temporary Extra Jobs comprise numerous types of jobs, but are frequently used for community services and jobs in public infrastructure.

With regard to the eligibility conditions, Temporary Extra Jobs resemble the formerly widely used *Arbeitsbeschaffungsmaßnahmen* or job creation schemes. Moreover, as with job creation schemes that were intended only for long-term unemployed persons, participation in Temporary Extra Jobs is restricted to welfare recipients. Persons covered by unemployment insurance cannot be placed. Yet given the disappointing impacts of job creation schemes on individual employment chances (see Hübler, 1997; Thomsen, 2007; and Hujer and Thomsen, 2010), several features of Temporary Extra Jobs have been designed to avoid the unintended outcomes of the past. First of all, programs

are clearly shorter; they last for six months at most. Second, in job creation schemes participants were paid market wages or high lump sum payments; Temporary Extra Jobs provide little remuneration. All those who take part in the program continue to receive welfare benefits. For their work, they also receive an hourly wage of between 1 and 2 euros by the organization providing the Temporary Extra Job.[63] Moreover, placement in a Temporary Extra Job does not constitute a regular employment relationship, i.e. participants remain welfare recipients and do not possess the rights and duties of regular employees. But welfare recipients are not recorded as unemployed while participating in Temporary Extra Jobs. An extensive use of these programs could therefore be used to reduce the official unemployment rate. Finally, to avoid locking-in effects that have been prevalent for employment programs in the past, jobs are usually part-time and amount to about 30 hours per week. This should enable persons to continue looking for regular employment. Nevertheless, full-time engagements are possible as well.

Despite the adjusted design of Temporary Extra Jobs and the lessons learned from job creation schemes, it is unclear whether the new program is more effective. Temporary Extra Jobs are intended to avoid or at least to reduce the loss of human capital associated with being unemployed and to provide participants with skills necessary for finding regular employment. However, the transferability of skills is questionable since occupations in Temporary Extra Jobs are additional in nature and regular employers might require other skills. Similarly, participation in Temporary Extra Jobs could act as a signal of welfare recipients' willingness to work, yet it could lead to stigmatization. If regular employers see Temporary Extra Jobs as a program targeted at the most disadvantaged persons, then participation may be seen as an adverse selection of welfare recipients with low productivity. Program effects are thus unclear ex ante, and a thorough investigation is needed.

Given the over-representation of immigrants in the German welfare system and the aim of Temporary Extra Jobs to provide a "last resort" of activation for hard-to-place welfare recipients, I expect a high share of immigrant participants in the programs. But this expectation is not supported by the empirical numbers. On the contrary: despite being a disadvantaged group, immigrants are placed in Temporary Extra Jobs less often than natives. Within the scope of all placements in ALMP during the first quarter of welfare receipt, the Bundesministerium für Arbeit und Soziales (2009) reports for an inflow sample of individuals entering the welfare system in 2006 that 31% of all native German participants are placed in Temporary Extra Jobs, while the corresponding share for immigrants amounts to only 22%.[64] In addition, immigrants are less often placed in ALMP overall. Hence, the use of Temporary Extra Jobs is less pronounced for immigrants compared with natives; nevertheless, Temporary Extra Jobs are used extensively for both groups.

[63] For this reason, Temporary Extra Jobs are also called *Ein-Euro-Jobs* in Germany.

[64] Numbers calculated in table 8.1, p. 165, of Bundesministerium für Arbeit und Soziales (2009).

Thomsen and Walter (2010b) analyze the pattern of participation in numerous ALMP for immigrant welfare recipients. According to their findings, immigrant welfare recipients are frequently placed in aptitude tests, a variant of short-term training (see subsection 5.3.2 for more details). Placement in Temporary Extra Jobs ranks second, whereas the other programs considered, e.g. long-term training programs and placement activities of private providers, are less frequently used independently of a migration background. Thomsen and Walter (2010b) also distinguish different groups of immigrants: foreigners, naturalized foreigners, and German resettlers (*Aussiedler/Spätaussiedler*). These groups differ with respect to some sociodemographic characteristics but also with respect to placement rates in Temporary Extra Jobs. Controlling for a set of observable characteristics in the estimation, foreigners as well as naturalized foreigners are less likely to be placed in the program compared with native Germans, whereas German resettlers participate as frequently as natives. In other words: the placement rates seem to indicate actual demand for activation more than any particular selection.

5.3.2 Short-Term Training Programs

Initially, short-term training programs were introduced for unemployment insurance benefit and unemployment assistance recipients in 1997 (see Kurtz, 2003). They are legally defined in Book III of the German Social Code (*Sozialgesetzbuch Drittes Buch, SGB III*). In 2005, the rules were adopted for unemployment benefit II (UBII) recipients. The primary purpose of short-term training programs is to improve the employment prospects of participating individuals. For this reason, programs consist of three different types of measures (modules) that can be offered separately or in combination and allow a flexible implementation in line with the specific needs of the welfare recipient.

The first type of course is the "aptitude test" (*Eignungsfeststellung*), which lasts for up to four weeks. Aptitude tests are used to assess the suitability of participants in terms of skills, capability, and labor market opportunities for certain occupations. During the assessment process, specific occupational skills can be provided, which shall help improve employment chances in the respective occupations. At the same time, the caseworker gains better knowledge about the skills and labor market prospects of the welfare recipient. This should increase the effectiveness and efficiency of placement efforts.

The second type of short-term training programs aims at improving the applicant's job search abilities (*Überprüfung der Verfügbarkeit/Bewerbertraining*). Its activities are designed to support the individual's efforts in finding work, especially through job-application training and job search advising. In many cases, the measure is also used by the welfare agency to assess the participants' willingness and ability to work (work-test). Measures of the

second type are promoted for up to two weeks. I refer to them below under the term "job search training".

The third type provides practical training (for up to eight weeks) in the skills and techniques required for placement in employment or vocational training (*Vermittlung notwendiger Kenntnisse und Fertigkeiten*). The courses cover specific working techniques such as business administration or computer courses. I will refer to this form of training as "skill provision".

Finally, the fourth type uses a combination of modules, e.g. an aptitude test together with a computer course, for a maximum of twelve weeks. This type of training will be referred to as "combined training programs". Combined training programs consist of two or three of the training modules. The modules can be arranged flexibly to meet the individual needs of welfare recipients.

All short-term training programs are meant to qualify participating welfare recipients for employment; their aim is not to create a threat effect, which would induce assigned individuals to quit welfare to avoid participation. While in the program, participants continue to receive welfare benefits. Additional financial support during the training is provided by the welfare agency and covers course costs, travel grants as well as child care. Short-term training programs can be provided on the job within firms and off the job. If provided off the job, activities are conducted by specialized service providers (*Bildungsträger*). Evaluation of the treatment effects of on-the-job courses may be complicated due to potential windfall gains of the supporting employers. For this reason, I concentrate on off-the-job courses only. In the literature, on-the-job training has been found to be more effective than off-the-job training (see Hübler, 1997 and 1998; and Kopf, 2009).

The institutional set up of short-term training programs suggests two channels through which programs affect employment chances and the probability of leaving welfare. The modules that support the self-contained job search of the participants or the placement efforts of the welfare agency can be expected to improve participants' search behavior by increasing the intensity as well as the efficiency of the search efforts. More efficient job search will lead to an increase in the job offer arrival rate, which increases the probability of leaving welfare. Yet it will also make job seekers more selective with respect to potential job offers and induce a negative indirect effect on the transition. The overall effect is then the sum of the positive direct and the negative indirect effect (see van den Berg, 1994). Participation in short-term training can also improve the job-relevant skills and increase the job opportunities of the participants. Increasing the skills is equivalent to increasing productivity, which enables participants to apply for jobs associated with higher wages on average. In terms of job search theory, this equals a shift of the wage offer distribution to the right. According to Mortensen (1986), an increase in the mean of the wage offer distribution increases the reservation wage by an amount less than the increase in the mean, thus increasing the probability of leaving welfare.

The theoretical perspective implies positive effects for short-term training programs on the probability of leaving welfare and the probability of taking up employment. But for these positive effects to hold in reality, there must be potential employers willing to engage the participants. Despite the relative decrease in the reservation wage due to the increased search effort or the higher productivity obtained in practical training, participants may still have a productivity too low to be remunerated by the market. In this case, participation has no positive effect.

5.4 Data for Empirical Analysis

5.4.1 Estimation Sample and Available Information

For the empirical analysis, I use the data set described in subsection 2.3.2. As noted there, the analysis sample was drawn in a 1:1 ratio of immigrants and native Germans at a regional level. Immigrants are not distributed equally across welfare districts in Germany. For example, in East Germany, the number of immigrants is far lower than in the West. Even in West Germany, strong regional disparities can be observed, with a higher concentration of immigrants in cities and urban regions with industrial production. In contrast, rural areas usually exhibit smaller numbers of immigrants. To take account of the regional distribution of immigrants in the estimation of treatment effects, the sampling of native Germans was set up in accordance with the sampling of immigrants. Thus native Germans reflect the regionally distributed sample of immigrant welfare recipients but do not reflect a random sample of native German welfare recipients.

I restrict the estimation sample to welfare recipients age 18 to 57 and unemployed at the date of sampling and (potential) program start. Although unemployment is not a prerequisite for receiving welfare benefits, it is required for participation in full-time short-term training programs and Temporary Extra Jobs. The age restriction is imposed for the same reason as in the previous two chapters. Neither welfare recipients younger than 18 years nor individuals age 58 and above can be expected to take up employment. My final sample for the analysis contains 82,774 observations of which slightly more than half are natives (43,344) and the rest are immigrants (39,430).[65] Using information contained in the Integrated Employment Biographies (IEB), I identify for each person the first assigned program during the welfare spell and evaluate participation against non-participation in any other program at the time the program starts.

[65] In the sample of natives, there are 25,953 men and 17,391 women. The sample of immigrants comprises 24,862 men and 14,568 women.

The spectrum of the available information in the data comprises sociodemographic characteristics, details of household composition, aspects of qualification and experience, a comprehensive labor market history, and regional information. The estimation of program effects is based on stratified samples by gender and immigrant status. In addition, I consider the age of the individual in four classes (18 to 24, 25 to 34, 35 to 49, and above 50) and whether the person is married or lives in cohabitation. Details of the household composition cover the number of UBII recipients, the number of UBII recipients under age 18, the number of persons age 15 to 64 who are not able to work and claim social allowance, and the number of children under age 15 living in the household and claiming social allowance as well. Moreover, I consider the total size of the household which can include further persons, e.g. individuals age 65 or above. There is also a variable indicating whether the welfare recipient in study is the head of the household. The (formal) qualification is given by the degree of schooling (no school leaving certificate, secondary general school, intermediate general school, vocational diploma (*Fachabitur*), university entrance diploma) and the professional qualification the person possesses (no professional qualification, vocational school (*Fachschule*), off-the-job training (*außerbetriebliche Ausbildung*), apprenticeship (*betriebliche Ausbildung*), degree from college of higher education (*Fachhochschulabschluss*), and university degree). Regional information is regarded by dummy variables for the sixteen federal states in Germany and a dummy variable for East Germany.

The labor market history of the individual is considered for up to 72 months (6 years) before (potential) participation in Temporary Extra Jobs or short-term training programs. The characterization takes account of episodes of employment, unemployment, job seeking while employed, participation in ALMP, and episodes out of the labor force. The available information of the labor market history is divided into spells of 14-days length (half-months), resulting in up to 144 labor market spells per individual. To make this amount of information feasible for estimation, I consider a number of spell transformations. The first transformation aggregates the data into 6-, 12- and 24-months intervals by summing up the number of half-months in the respective states during these intervals.[66] Since spells are defined in 14-days episodes, a possible limitation of this transformation is that two spells are weighted equally independently of whether they occurred continuously or were interrupted. The second transformation counts the number of non-interrupted spells within the 6-, 12- and 24-months intervals. In a third transformation, the average durations in the five labor market states during the

[66] For example, the first transformation gives information that an individual was employed for 4 half-months and unemployed for 8 half-months during the final 6 months before (potential) participation in a program. In this six-month interval, there were neither half-months of job seeking while employed nor half-months of participation in ALMP nor half-months out of the labor force. The first transformation results in a still considerable but manageable number of variables. This means that $12 \times 5 = 60$ variables are to be considered (12 six-months intervals and 5 distinct labor market states).

final 6 and the final 24 months prior to (potential) program participation are regarded (measured in number of half-months). A fourth transformation normalizes the relative time spent in different states over the 6-, 12- and 24-months intervals accounting for the period of time individuals are observed in the data. In addition to the detailed labor market history, a dummy variable is available indicating that the person was not employed during the years 2001 to 2005. The outcome variable *self-sufficient employment* is observed on a monthly basis from the sampling date until July 2008.

5.4.2 Characterization of the Sampled Immigrants and Natives

Despite being intended as a "last resort" of activation, there has been evidence from the early post-reform period in 2005 that the target groups of Temporary Extra Jobs are reached partially when caseworkers assign the program (see Hohmeyer and Kopf, 2009). Based on my data covering the years 2006 and 2007, I observe a somewhat more precise targeting than has been described in the literature so far. 1,840 of the 39,430 sampled immigrants (1,217 men and 623 women) were assigned to a Temporary Extra Job during the first year of their welfare spell. Among the 43,344 sampled natives, the corresponding number amounts to 3,532 treated individuals (2,377 men and 1,155 women). Tables A.5.1 to A.5.4 in the appendix to this chapter provide descriptive statistics of selected characteristics of my estimation sample for Temporary Extra Jobs. The means of the variables depicted refer to participants (treated) and non-participants (controls) in the program before matching. The p-values correspond to equality-of-means tests of the displayed variables between the compared groups. I find that persons under 25 are most likely to be assigned to a Temporary Extra Job. Moreover, the participation probability decreases with educational attainment and depends on labor market history. Those individuals who experienced a relatively long period of unemployment before (potential) program participation are more likely to participate in a Temporary Extra Job. The same is true for persons who spent a considerable part of the two years preceding (potential) participation out of labor force. By contrast, welfare recipients with a relatively high employment share are less likely to be treated. Despite this tendency towards a more precise targeting, however, there is sufficient overlap in the characteristics of treated and non-treated welfare recipients, which can be exploited for the identification of program effects.

It is also useful to investigate potential differences in characteristics between treated natives and treated immigrants. Observable differences may indicate particular selection patterns. Tables A.5.5 and A.5.6 in the appendix to this chapter compare treated natives and treated immigrants in Temporary

Extra Jobs. Again, figures are separated by gender. The p-values correspond to equality-of-means tests of the displayed variables between the compared groups. Even though native and immigrant participants in Temporary Extra Jobs have similar labor market histories, they are distinct with respect to some sociodemographic characteristics. Considerable differences exist in terms of household composition. Immigrants are less frequently single, so that household size for immigrants is larger than for natives on average. Moreover, variation in educational achievement is larger for immigrants than for natives. I observe a relatively large share of immigrants without school leaving certificate but also a noticeable share of persons with a university entrance diploma, especially among women. In addition, differences between the two groups are apparent at the lower end of the age distribution. While the share of treated immigrants between 18 and 24 is lower than for natives, the opposite is true for individuals age 25 to 34. The differences are more pronounced for men than for women. Despite these differences, however, there is sufficient overlap in the distribution of covariates of natives and immigrants, making both groups comparable with respect to Temporary Extra Job effectiveness.

A similar conclusion holds for native and immigrant participants in short-term training programs. Tables A.5.7 to A.5.14 in the appendix to this chapter provide a comparison of characteristics.[67] 4,628 of the 43,344 sampled natives (2,851 men and 1,777 women) were assigned to one of the four considered short-term training programs during the first year of their welfare spell. Among the 39,430 sampled immigrants, the ratio of participants is lower, with only 3,871 individuals (2,599 men and 1,272 women) treated. Despite this difference in the participation ratios, the mix of assigned training programs is similar in both groups. Aptitude tests are most frequently assigned as first training program both for natives and immigrants. Nearly one third of all assigned short-term training programs are aptitude tests. Skill provision and combined training programs have a share of about 25% each. Job search training is used with the lowest frequency, resulting in a share of somewhat less than 20%.

Training program selection is mainly driven by labor market history. While sociodemographic characteristics are similar among participants in the four programs, I observe substantial differences with respect to the time spent in employment, in unemployment, and out of the labor force prior to program start. These differences can be noticed for both natives and immigrants.

Combined training programs are mostly assigned to those persons who were out of labor force for a lengthy period during the final two years before treatment. These individuals face multiple disadvantages when trying to

[67] The tables do not provide a comparison of the overall sample of treated individuals in short-term training with non-participants (controls). The group of controls for participants in short-term training is the same as for participants in Temporary Extra Jobs. Thus, for the characteristics of the control group, I again refer to tables A.5.1 to A.5.4 in the appendix to this chapter.

resume employment. The combined training programs attempt to comprehensively tackle these disadvantages. Aptitude tests and skill provision are mainly targeted at those persons with a high incidence of unemployment. These programs are used to learn about the suitability of participants for different occupations and to refresh general human capital, which may have depreciated during unemployment. By contrast, job search training is focused on individuals with good employment records who only recently became unemployed. These persons still have a valuable human capital stock but may need support for writing job applications and attending job interviews.

Regarding the origin of the immigrants, about 75% of this group are foreigners, 20% are naturalized persons, and 5% are German resettlers. About 30% of all immigrants are Turkish in origin. The second largest group, with a share of about 20%, consists of persons from Eastern European countries and the former Soviet Union. Other large groups, with a share of more than 10%, comprise persons from the former Yugoslavia, individuals from other Southern European countries (Spain, Portugal, Italy, Greece), and people from the Middle East and Northern Africa.

It is important to note that the immigrants I am looking at are not newly arrived immigrants. The vast majority of individuals have already been living in Germany for a substantial period. Most resettlers entered the country at the beginning of the 1990s after the collapse of the former Soviet Union. A similar picture arises for naturalized persons. Even though half of them received their German passport after 2000, the naturalization process requires them to have been living in Germany for at least 8 years. For naturalization before 2000, candidates must have spent 15 years in Germany. For the foreigners, only those with permanent residency in Germany are eligible to receive welfare benefits. Seasonal workers or persons with a temporary visa are not eligible; asylum seekers are not eligible either. Unfortunately, the data do not include the date of immigration. Nevertheless, the descriptive statistics in the appendix to this chapter – together with the fact that foreigners are the largest group of immigrants in my sample – reveal that the labor market history of immigrants is very similar to the labor market history of natives. Hence, few differences exist between immigrants and natives with respect to labor market experience, and I expect the same with respect to knowledge about German society in general.

I find no differences between immigrants and native Germans when analyzing the average realized program durations for the persons in my sample. Although Temporary Extra Jobs may last up to six months, the mean duration across all subgroups is 88.06 days, or about 3 months. The standard deviations in all subgroups are around 90 days, indicating a wide dispersion of program durations independent of migration background or gender. Aptitude tests have an average duration of 14.98 days, job search training of 9.96 days, skill provision of 23.66 days, and combined training programs of 22.73 days. Irrespective of a migration background, women are under-represented in all programs (see also chapter 3).

5.5 Evaluation Approach

5.5.1 Estimation of Treatment Effects

The evaluation of the treatment effects of participation in Temporary Extra Jobs and short-term training programs must consider the set-up of the comprehensive system of ALMP in the German welfare system. This system is characterized by a wide array of programs that take place continuously over time and that are open to welfare recipients who meet certain eligibility criteria at different points of time during the welfare spell. Recent empirical literature highlights the need to consider the timing of treatment in the unemployment or welfare spell when evaluating treatment effects. See, for instance, Abbring and van den Berg (2003), Sianesi (2004), Thomsen (2007), Fredriksson and Johansson (2008), or Hujer and Thomsen (2010). While earlier literature on the evaluation of ALMP usually deals only with binary information, i.e. whether an individual has been subject to treatment or not, this more recent literature points out the importance of treatment timing: it conveys useful information for the identification of the treatment effect and has implications for the definition of the control group. Specifically, the starting point of the program may be an important determinant for participant selection as well as for the type of program the individual is assigned to.

As in chapter 3, the basis of the empirical analysis is provided by the potential outcome approach of causality (see Splawa-Neyman, 1923, Roy, 1951, and Rubin, 1974). Following the conventional notation, let Y^1 and Y^0 denote the two potential outcomes, where Y^1 is the outcome when the individual participates in the program, and Y^0 is the outcome when the individual does not participate. Since the individual cannot be in both states at the same time, one of the potential outcomes is unobservable and direct calculation of the treatment effect is impossible.[68]

To identify the treatment effect, I must provide an estimate of the unobserved state. Since I am interested in the effect of participation in a Temporary Extra Job or short-term training program, the Average Effect of Treatment on the Treated (ATT) – defined as the difference of the expected outcomes with and without treatment for participants – provides a useful parameter for evaluating program impact. It focuses on actual participants and determines the realized net gain for the group (see Heckman et al., 1999). The counterfactual outcome necessary for estimating this parameter is the hypothetical outcome of the participants had they not participated in the

[68] Imbens (2000) and Lechner (2001) generalize the framework for situations where a whole range of programs is available. Although I analyze five programs, the focus of the analysis is on the effect of participation in a program compared with non-participation in that or any other program, not on the relative effect of comparing one program with another program. I thus forgo distinguishing between different programs in my description.

program. To take account the characteristics of program participation in the German welfare system, I focus on the ATT at a given point during welfare receipt. Conditioning on the elapsed welfare duration makes sense in the German context for the reason first raised by Sianesi (2004). She argues that, in a comprehensive ALMP system, a person will join a program provided he or she remains on welfare long enough. Accordingly, the reason why an individual's program participation goes unobserved is that the person has already left the welfare system, or that the time horizon of the analysis is too short. Although participation in a program is not mandatory after a specified duration as in some welfare systems, such as Sweden's, UBII recipients in Germany are more likely to participate in some program the longer they remain on welfare.

In line with that, participation and non-participation have to be defined dynamically, i.e. with respect to the point in time in which the comparison is made. According to Sianesi (2004), persons who have neither entered a program nor left welfare up to a specific point in time are defined as non-participants or "waiters" (in the sense that they are waiting to be assigned to a program). Non-participation can thus be interpreted as the default state for each individual, with everybody being a non-participant until entering a program or leaving welfare to take a job. Individuals who are defined as non-participants at the moment I start my comparison may enter a program at a later point in time.[69]

The evaluation approach in the dynamic setting can be formalized as follows. Let $U = \{0, \ldots, U_{\max}\}$ define the discrete elapsed welfare duration of the individual since registration at the local welfare agency. Let u denote the point in time during the welfare spell in which the program of interest starts and D_u the treatment indicator with the discrete time index. $D_u = 1$ if the individual starts a program at time u of the welfare spell, and $D_u = 0$ if the individual remains on welfare at u. Program effects are estimated for time t, i.e. the time elapsed since the program started. The hypothetical outcomes for time t given a treatment at time u are then defined as $Y^1_{t,u}$ for individuals who received the treatment at u and $Y^0_{t,u}$ for individuals who did not receive the treatment by time u. The parameter of interest for each u is the average effect in t for individuals starting a program in period u of their welfare spell compared with not joining at u:

[69] This definition of non-participants has to be kept in mind when interpreting the estimation results. Treated individuals are compared with individuals who may participate in a program later. The comparison group is not restricted to individuals who never join a program. This would require to condition the analysis on future outcomes. As has been illustrated for example by Stephan (2008), the estimated effects of a program might be sensitive to the exact definition of non-participants and, thus, to the specific treatment effect of interest.

$$\begin{aligned}\Delta_{t,u}^{ATT} &= E(Y_{t,u}^1 - Y_{t,u}^0 | D_u = 1, D_1 = 0, \cdots, D_{u-1} = 0) \\ &= E(Y_{t,u}^1 | D_u = 1, D_1 = 0, \cdots, D_{u-1} = 0) \\ &\quad - E(Y_{t,u}^0 | D_u = 1, D_1 = 0, \cdots, D_{u-1} = 0). \end{aligned} \tag{5.1}$$

While the first term of this equation is identified in the data by the observed outcome of the participants, the second term has to be estimated. Simply using the observable non-participants' outcomes to approximate unobservable participant outcomes without treatment may lead to biased estimates due to selection (see also chapter 3).

To solve the selection problem, I apply a propensity score matching estimator. As has been noted in chapter 3, the basic idea of the matching approach is to identify from a large group of non-participants those individuals who are similar to the participants in all relevant pre-treatment characteristics X. Matching can become hazardous when X has a high dimension. To deal with this dimensionality problem, Rosenbaum and Rubin (1983) suggest the use of the propensity score $p(X) = E(D = 1|X)$, i.e. the probability of participation in a program, to summarize the information from the relevant covariates X into a single index function. For the ATT to be identified with matching, however, the so-called Conditional Independence Assumption (CIA, $Y^0 \amalg D|X$ in the static binary case; see Lechner, 1998) has to be imposed. It states that, conditional on the set of relevant (observable) covariates X, the non-participation outcome Y^0 is independent of the participation decision.

For the dynamic case, I have to invoke an adjusted version, the Dynamic Conditional Independence Assumption (DCIA):

$$Y_{t,u}^0 \amalg D_u | p(X_u), D_1 = 0, \cdots, D_{u-1} = 0. \tag{5.2}$$

The hypothetical outcome at time t after not participating up to time u has to be independent of program participation at time u, conditional on the propensity score $p(X_u)$ measured at time u. The DCIA ensures that treated and non-treated individuals are comparable in their non-treatment outcomes at time t conditional on $p(X_u)$, conditional on claiming welfare benefits up to time $u - 1$, and conditional on not receiving treatment before u. In addition, the availability of non-participating analogues for the participants must be guaranteed (common support condition), i.e. $Pr(D_u = 1|X_u) < 1$ (see Smith and Todd, 2005a), and the Stable Unit Treatment Value Assumption (SUTVA) must hold (see chapter 3).

5.5.2 Plausibility of the Dynamic Conditional Independence Assumption and Implementation of the Matching Estimator

For the DCIA to hold, it is necessary to observe all covariates that, conditional on having reached a given welfare duration u, influence both the participation decision at that time (D_u) and the outcome variable where such a decision is postponed further ($Y^0_{t,u}$). In line with that, I condition on previous welfare experience by stratifying the welfare duration in quarters. Stratification by quarter is useful for the consideration of differences in the effectiveness of programs due to the timing of treatments. For the propensity scores, I estimate separate probit models for each group (native Germans and immigrants), each treatment, each gender, and for each of the first four quarters of the welfare spell. Each probit estimates the probability of starting a program in quarter u, conditional on having reached the welfare duration of $u \in \{1, \ldots, 4\}$ quarters, conditional on not having received treatment before u in the welfare spell, and conditional on X. Hence, I analyze the effects of Temporary Extra Jobs and short-term training programs for groups of individuals that join within the first year of the welfare spell. The outcomes are measured monthly from the first month of the quarter following (potential) participation through July 2008 due to the time horizon of the analysis.[70] The treatment effects are estimated using kernel density matching on the estimated propensity score. I use an Epanechnikov kernel function and a bandwidth of 0.06. Standard errors are calculated by bootstrapping with 250 replications.[71]

Program assignment arises from a profiling process in which caseworkers screen the welfare recipient's skills, deficiencies, and labor market prospects. Based on the results of the interview, the welfare recipient is classified according to his or her employment chances. This classification influences the types of programs the welfare recipient might potentially be assigned to. As pointed out by Jacobi and Kluve (2007) and Huber et al. (2011), Temporary Extra Jobs are targeted at welfare recipients whose employment chances are severely limited. By contrast, short-term training is targeted at those individuals with stronger employment prospects. Jacobi and Kluve (2007) observe that welfare recipients are admitted to short-term training only if they are expected to have a 70% probability of finding a job after completing the

[70] For programs assigned in the first quarter of the welfare spell, I use an observation period of at least 16 months for each observation. The last entry into the welfare system in my sample is December 31st, 2006. This means that a program in the first quarter could be assigned through March 31st, 2007. In this case, the observation period for the outcomes is April 2007 through July 2008. For programs assigned in the second quarter, I have an observation period of 13 months. In the third quarter, the observation period lasts for 10 months and in the fourth quarter for 7 months.

[71] I use the matching algorithm provided by Leuven and Sianesi (2003).

program. This sort of cream skimming might explain why the ratio of participants in short-term training is lower among immigrants than among natives, as described in subsection 5.4.2. Nevertheless, it ensures that I only consider comparable individuals when analyzing differences in treatment effects between treated immigrants and treated natives.

Given the comprehensive information outlined in subsection 5.4.1, my data are well suited to capture the factors that determine program participation and individual employment prospects. In particular, I consider not only sociodemographic characteristics like age, gender, marital status, household composition, schooling, and professional qualification, but also regional information and the frequency, duration, and quality of employment, unemployment, participation in ALMP, and of other labor market states for up to six years before a (potential) program start. I use this information to construct detailed variables (see subsection 5.4.1) about the individual labor market history for different intervals prior to (potential) treatment. What is lacking in my data are direct measures of individual motivation, attitudes, and aptitude. It is likely, however, that these characteristics remain persistent over time, which is why we can assume that they have impacted labor market success before the (potential) program start as well. For this reason, it is crucial to condition on individual labor market histories in detail (see Card and Sullivan, 1988; and Heckman et al., 1998).

Information about immigrant language skills is also absent from the data. However, these can be assumed to be sufficient for a successful participation in the labor market. When immigrants enter Germany, the government provides so-called integration courses (*Integrationskurse*). Integration courses consist of language training and orientation. The aim of the language training is to increase language skills of immigrants such that they can communicate without any difficulty in everyday situations and when searching for and taking up employment (see e.g. Rother, 2008). The orientation part of the course puts an emphasis on German culture, the legal system, and ethical values. Participation in the integration course is mandatory for newly arriving immigrants and those immigrants who have been living in Germany but who are not well integrated. The integration course takes precedence over any other ALMP. Language skills should thus be sufficient for taking part in Temporary Extra Jobs, short-term training programs, and regular employment. Because immigrants are likely to be a heterogenous group with respect to language proficiency, conditioning on language skills may increase the precision of the estimates. It should be noted, though, that past employment is predicated on language skills. Detailed consideration of the labor market history in the empirical analysis can thus be assumed to provide a sufficient proxy for potential differences in language proficiency: differences in ability will also have affected labor market success before the (potential) program start. In making use of the unusually rich data set and its large number of covariates, I am confident that I capture all relevant factors that affect both participant selection for the programs in question and my outcome variable of interest.

With regard to the specification of the propensity score models, I distinguish the available information into 22 variable blocks. The specifications of the final models used in the estimations are obtained by estimating probit regressions starting with the full set of variables and gradually dropping jointly insignificant variable blocks (indicated by Wald tests) for parsimonious specification. For this reason, the propensity score specifications vary across the probit models estimated for the different samples of interest.

For each sample, the estimated propensity score is to guarantee that the included variables are balanced between treatment and comparison group. To check the balancing property of the estimated propensity score, I apply the procedure suggested by Smith and Todd (2005b), which I also used in chapter 3:

$$\begin{aligned} X_{k_u} = \beta_0 + \beta_1 \hat{p}(X_u) + \beta_2 \hat{p}(X_u)^2 + \beta_3 \hat{p}(X_u)^3 + \beta_4 \hat{p}(X_u)^4 + \beta_5 D \\ + \beta_6 D\hat{p}(X_u) + \beta_7 D\hat{p}(X_u)^2 + \beta_8 D\hat{p}(X_u)^3 + \beta_9 D\hat{p}(X_u)^4 + \eta. \end{aligned} \tag{5.3}$$

Equation (5.3) is estimated for each variable X_{k_u} included in the propensity score specification of participation in a particular program in quarter u. Afterward, the joint null hypothesis of β_5 to β_9 being zero is tested. The test indicates whether the treatment indicator has any influence on the covariates conditional on a quartic polynomial of the propensity score. If balancing is ideal, it should have no influence.[72]

Obviously, caseworkers and welfare agencies play a crucial role for the allocation of welfare recipients to programs. Turning down an offered program can be sanctioned by benefit revocation, which is why caseworkers can be assumed to have the final word in participation decision (see section 2.1 and chapter 4). If caseworkers acted on unobservable information correlated with the individuals' potential labor market outcomes, the DCIA would be violated. But it is unlikely that caseworkers referred to unobservable information outside the large set of variables recorded. The data used in this analysis were collected by the caseworkers during the interviews with welfare recipients. Moreover, to bias the estimates further, unobserved information would have to influence both the participation decision and the outcomes. Given the large number of covariates I control for in the estimates, I assume that caseworkers act idiosyncratically given the observable characteristics of the individuals.

We must keep in mind that the comparison group does not necessarily reflect a no-program state; it may also reflect postponed participation. If I choose individuals who never participate in a program for the comparison group, this may invalidate the DCIA, as I must condition on future outcomes.

[72] In addition to the Smith and Todd (2005b) balancing test, I have also looked at the other quality indicators discussed in chapter 3. Because all indicators yield similar results, the particular choice of a quartic polynomial in the Smith and Todd (2005b) balancing test does not seem to give a misleading picture of the matching quality. The results of the balancing test will be presented in subsection 5.6.1.

For unbiased estimation, I have to rule out anticipatory effects; otherwise people will behave differently depending on future treatments. If, for example, non-participants knew in advance that they would be treated and when, then matching could not solve the selection problem and I would overestimate the treatment effect because non-participants have no reason to leave welfare immediately for employment. By contrast, if people dread the prospect of being treated and if they know when they will be treated, they will tend to leave welfare to search for employment. As a result, the program effect goes underestimated, as non-participants differ significantly from the participants, even after matching. But this is only the case if people know that they will be treated and when. Abbring and van den Berg (2003) point out that the exclusion of anticipatory effects does not rule out that individuals know and act on the determinants of treatment assignment. In other words, individuals can adjust their behavior to the determinants of the treatment process but not to the actual implementation of the treatment. This is not a problem for the analysis as long as treated and non-treated individuals anticipate the chances of these events conditional on the propensity score and the elapsed welfare duration for a given quarter in the same way. So while people may be aware of the determinants for the assignment process, it is unlikely that they know when the future treatments will take place. For that reason, I assume my estimates are not affected by anticipatory effects.

5.5.3 Estimation of the Immigrant Fixed Effect

Effect heterogeneity between immigrants and natives for a particular program can provide important insights. In identical programs differences might arise from group composition, i.e. the distribution of characteristics determining program participation and labor market success may differ from one group to another. If one conditions on all these variables, effect differences should be eliminated. Nevertheless, residual differences may continue to exist between groups solely due to individual group attachment, and when they do programs will work differently for natives and immigrants even when both groups share the same characteristics.

I call this residual part the "immigrant fixed effect". Identifying this part of the effect difference, which cannot be explained by differences in sociodemographic composition, is important for targeting programs to welfare recipients so as to best improve employability. The heterogenous value of an identical treatment may result from a set of confounding factors associated with the attachment of the individual to his or her group. To estimate the immigrant fixed effect, I apply the following estimation procedure based on a matching estimator. To abbreviate notation, I suppress the indicators of the dynamic setting.

The starting point of the analysis is the observable raw differential Δ_{Dif}^{ATT} in treatment effects between immigrants and natives for a particular program. This differential can be obtained by subtracting the estimated treatment effect for native Germans from the estimated treatment effect for immigrants:

$$\Delta_{Dif}^{ATT} = \Delta_{M=1}^{ATT} - \Delta_{M=0}^{ATT}, \tag{5.4}$$

where M is a binary index indicating immigrants if $M = 1$ and native Germans if $M = 0$. The corresponding estimates for $\Delta_{M=1}^{ATT}$ for the immigrants and $\Delta_{M=0}^{ATT}$ for the native Germans are obtained by applying the propensity score matching estimator as described in subsection 5.5.1.

The estimated effects are given by

$$\Delta_{M=1}^{ATT} = E\left(Y_{M=1}^{1} - Y_{M=1}^{0} | p(X_{M=1}), D = 1, M = 1\right), \tag{5.5}$$

i.e. the estimated ATT for persons with a migration background conditional on the estimated propensity score of the immigrant group, $p(X_{M=1})$, and by

$$\Delta_{M=0}^{ATT} = E\left(Y_{M=0}^{1} - Y_{M=0}^{0} | p(X_{M=0}), D = 1, M = 0\right), \tag{5.6}$$

the analogue estimator of the ATT for native Germans conditional on the propensity score of this group, $p(X_{M=0})$. $X_{M=0}$ denotes the characteristics of native Germans and $X_{M=1}$ the characteristics of immigrants. For the sake of illustration, I have also added the group attachment indicator to each group's potential outcome.

To estimate the immigrant fixed effect, I apply the potential outcome approach once again. Now the treatment of interest is the group attachment of the treated individual, where $M = 1$ denotes immigrants and $M = 0$ natives, and I am interested in the difference between two potential treatment effects. The first is the expected treatment effect for immigrants as estimated in equation (5.5), $E(Y_{M=1}^{1} - Y_{M=1}^{0} | X_{M=1}, D = 1, M = 1)$. The counterfactual state refers to the hypothetical treatment effect of immigrants if they had the same expected labor market success as natives with identical characteristics, i.e. $E(Y_{M=0}^{1} - Y_{M=0}^{0} | X_{M=1}, D = 1, M = 1)$. The immigrant fixed effect (IFE) is therefore given by:

$$\begin{aligned} IFE = {} & E(Y_{M=1}^{1} - Y_{M=1}^{0} | X_{M=1}, D = 1, M = 1) \\ & - E(Y_{M=0}^{1} - Y_{M=0}^{0} | X_{M=1}, D = 1, M = 1) \end{aligned} \tag{5.7}$$

Unfortunately, the latter hypothetical treatment effect (second term on the right-hand side) cannot be estimated directly from the data since the labor market outcomes of natives $(Y_{M=0}^{1}, Y_{M=0}^{0})$ are not observed for the immigrants $(M = 1)$ and vice versa. Conditioning on the estimated propensity score of immigrants $p(X_{M=1})$ within both groups is not possible either. The

notation takes this impossibility into account. Since I consider treatment effects as hypothetical outcomes in the identification of the immigrant fixed effect, I look only at participants as the population of interest, indicated by $D = 1$.

For the estimation of equation (5.7), I have to provide a proxy for the unobserved hypothetical treatment effect for the immigrant participants. The estimated treatment effect of native participants who are matched on all relevant characteristics to immigrant participants in the same program can serve as an approximation. As for the estimation of the treatment effects, matching on all immigrant characteristics $X_{M=1}$ is infeasible, so I use a balancing score to condense the information of the distribution of $X_{M=1}$ into a single index function, $b(X_{M=1})$. The estimation of this balancing score is arranged by estimating a probit model for membership in the immigrant group, $M = 1$, based on the comprehensive set of available covariates used for estimating the treatment effects. I have to assume mean conditional independence of the expected treatment effect of natives from immigrant status conditional on the estimated balancing score, i.e.

$$E(Y^1_{M=0} - Y^0_{M=0}) \amalg M | b(X_{M=1}), D = 1. \tag{5.8}$$

If assumption (5.8) holds, I can replace the second term on the right-hand side of equation (5.7) with the estimated treatment effect of natives possessing the observable characteristics of the immigrants, i.e.

$$E(Y^1_{M=0} - Y^0_{M=0} | b(X_{M=1}), D = 1, M = 1) = E(Y^1_{M=0} - Y^0_{M=0} | b(X_{M=1}), D = 1, M = 0). \tag{5.9}$$

To implement the estimator, I proceed as follows. First, I estimate the ATTs for participation in Temporary Extra Jobs and short-term training programs separately for natives and immigrants and for men and women, as described in subsection 5.5.1. Second, I keep only the treated individuals in each estimation sample. Third, I merge the samples of treated natives and treated immigrants separately for both genders and separately for the five programs of interest. Fourth, in the merged samples I consider the migration background as the new treatment variable and the individual employment effects of Temporary Extra Jobs and short-term training programs (derived from the first step) as the new outcome variables. Fifth, I estimate the balancing scores for the probability of membership in the immigrant group, incorporating all available information from the data. For the specification of the balancing scores, I apply the same selection procedure of the relevant variables as in the first step using Wald tests. The test proposed by Smith and Todd (2005b) is used to check the balancing properties of the estimated scores. Treated natives are matched to treated immigrants based on a kernel density matching. The matched partners are weighted according to the dis-

tance in balancing scores. Applying the algorithm provided by Leuven and Sianesi (2003), I use an Epanechnikov kernel function and a bandwidth of 0.06. Standard errors are calculated by bootstrapping with 250 replications. Common support must be guaranteed and the Stable Unit Treatment Value Assumption (SUTVA) must hold. The final results provide the average differences in program effects due to the immigrant fixed effect when all observable characteristics are constant.

5.6 Empirical Results

5.6.1 Quality of the Estimates

For the estimation of program effects, I stratify my data by migration background, gender, and quarter of program start during the first year of the welfare spell. In the case of Temporary Extra Jobs, I am able to estimate treatment effects for all 16 different strata. Since short-term training tends to be assigned quite early during a welfare spell, the number of treated observations declines over time. This is particularly true for job search training, which – due to its content – is mainly used in the first quarter of a welfare spell. As the numbers of treated individuals in this and other forms of training become too small in later quarters to estimate valid treatment effects, I concentrate my analysis on those quarters where the majority of participants enroll in the programs. I estimate treatment effects of short-term training for 51 strata. Consequently, I look at 67 estimation samples in total.[73]

As noted above, ATT is identified only for the region of common support. To ensure common support in each estimation sample, I delete treated individuals whose propensity score is smaller than the smallest or larger than the largest propensity score in the respective group of non-treated individuals. As can be seen from table A.5.15 in the appendix to this chapter, all treated individuals are on support in the vast majority of the 67 estimation samples. Only in 14 samples are treated observations lost due to the common support condition and all losses are negligible. In twelve out of the 14 samples, only one treated person is deleted from the respective matching analysis; in the remaining samples only two must be ignored.

Furthermore, to obtain valid treatment effects, it is crucial that the covariates included in the propensity score estimation are balanced between treatment and control group after matching. As the main balancing test, I apply the procedure suggested by Smith and Todd (2005b). Results of this

[73] In addition to the analysis of the 67 estimation samples, I look at subgroups of immigrants (individuals with a Turkish, an Eastern European, or a Southern European migration background) treated during the first quarter of welfare receipt in subsection 5.6.4. This adds another 30 samples to my evaluation of program effects.

test are summarized in table A.5.16 in the appendix to this chapter. As can be seen from the table, balancing quality is high and sufficient to obtain valid treatment effects.

For Temporary Extra Jobs, the test was passed in 865 of 916 cases (94.4%) at the 1% significance level. The matching quality is similar for men (447 of 474 tests, or 94.3%) and women (418 of 442 tests, or 94.5%) as well as for natives (430 of 458 tests, or 93.9%), and immigrants (435 of 458 tests, or 95.0%). Even at the 5% significance level, 816 out of the 916 tests were passed, with 777 at the 10%-level. For the short-term training programs, the test was passed in 2,355 of 2,481 cases (94.9%) at the 1% significance level. Again, the matching quality is similar for men (1,387 of 1,460 tests, or 95.0%) and women (968 of 1,021 tests, or 94.8%) as well as for natives (1,151 of 1,217 tests, or 94.6%), and immigrants (1,204 of 1,264 tests, or 95.3%). Even at the 5% significance level, 2,270 of the total 2,481 tests were passed, with 2,184 at the 10%-level.[74]

The exact specifications of all estimated 67 propensity scores cannot be presented here, but table A.5.17 in the appendix to this chapter displays two selected specifications for illustrative purposes (skill provision for male natives and Temporary Extra Jobs for female immigrants assigned during the first quarter of the welfare spell). The specifications that are displayed and those that are not reveal that age, schooling, professional qualification, household composition, region, and labor market history within the final two years before (potential) program participation are relevant factors that must be accounted for when estimating the effects of Temporary Extra Jobs and short-term training programs.

Figures A.5.1 to A.5.5 in the appendix to this chapter visualize the density distributions of the estimated propensity scores for the treated individuals in each quarter. The left-hand side of the graphs show the distributions for treated natives. The corresponding distributions for the treated immigrants are on the right-hand side. Irrespective of immigrant status and gender, the distributions show a similar pattern over time. For short-term training programs, the distributions in later quarters become more and more skewed to the left, and the value of the propensity score at which the density distribution reaches its maximum becomes smaller. This indicates that short-term training is mainly used right at the beginning of a welfare spell and that the probability of participation in this form of training decreases over time. Nevertheless, there is complete overlap in the distributions across all quarters for each estimation sample considered.

Though complete overlap exists between treated immigrants and treated natives, there are slight differences in the distributions for immigrants and natives with respect to skewness and mode. It is thus crucial to control for potential differences between both groups in detail when estimating the immigrant fixed effects. Given the rich data base, I am in a position to do so.

[74] The other quality indicators discussed in chapter 3 yield similar results and confirm a high matching quality.

Taking this point into account, I use a large number of covariates for the specification of the balancing score models for the analysis of immigrant fixed effects. With the short-term training programs, the number of covariates is on average even larger than the number I use for the separate estimation of treatment effects of participation in training for immigrants and natives. Nevertheless, it turns out again that age, schooling, professional qualification, household composition, region, and labor market history within the final two years before program participation should be accounted for in these models.[75] As can be seen from table A.5.18 in the appendix to this chapter, covariates are balanced very well in the analysis of immigrant fixed effects, and matching quality is of the same high degree as in the analysis of program effects. It must be noted, though, that the share of persons who are not on common support and are excluded from the analysis of immigrant fixed effects is larger than that in the analysis of programs' treatment effects. It varies between 1.87% (Temporary Extra Jobs) and 12.96% (job search training) for men and between 4.90% (job search training) and 20.30% (skill provision) for women (see table A.5.19 in the appendix to this chapter).

5.6.2 Program Effects

Before presenting my estimation results, I briefly describe the means of the outcome variable *self-sufficient employment* for the subgroups of male and female native Germans and male and female immigrants (see table A.5.20 in the appendix to this chapter). The means of the variable are displayed for the first 18 months after the inflow into welfare. As can be seen from the table, the share of persons taking up self-sufficient employment and leaving the welfare system increases over time in all subgroups. The increase is largest in the subgroup of native German men. Here, the employment rate amounts to 27.6% one and a half years after inflow into welfare. For male immigrants, the employment rate at this point in time is considerably lower: 23.1%. This is one percentage point larger than the employment rate of women without a migration background (22.1%). By far the lowest employment rate is observed for female immigrants. In this subgroup, only 13.8% left the welfare system for employment 18 months after inflow.

[75] Table A.5.17 in the appendix to this chapter illustrates the balancing score specifications for the estimation of immigrant fixed effects in case of treated men in skill provision and treated women in Temporary Extra Jobs, respectively.

5.6.2.1 Effects of Temporary Extra Jobs

The employment effects of Temporary Extra Jobs are displayed in table 5.1. They are estimated separately for each quarter, for natives and immigrants, and for men and women. I report treatment effects only up to one year after program start, even though I have a longer observation period for Temporary Extra Jobs starting during the first quarter of welfare receipt. Estimates more than one year after program start do not differ significantly from the results obtained after 12 months. In particular, I do not observe any significant positive treatment effect in any estimation sample after locking-in effects fade away. As can be seen from the table, Temporary Extra Jobs assigned during the first quarter of a welfare spell have a negative impact on the probability that immigrant males will take up employment providing a sufficient income above the subsistence level. Surprisingly, I observe only a somewhat negative treatment effect of -1 percentage point in the first six months after program start – beneath the threshold of statistical significance.[76] This effect can be interpreted as the locking-in effect, bearing in mind the average program duration of 90 days and the large standard deviation (see subsection 5.4.2). The absolute size of the negative treatment effect increases over time, however. One year after program start, the probability that participants will take up self-sufficient employment is 2.7 percentage points lower than in a situation without treatment. Thus, Temporary Extra Jobs reduce rather than increase the employment chances of male immigrants. For men without a migration background who participate in the program during the first quarter of their welfare spell, the negative treatment effects are even stronger. Here, I observe treatment effects ranging between -2.5 and -4.1 percentage points. The effect is strongest at the beginning of the observation period, indicative of a substantial locking-in effect, but even one year after starting the program the probability that participants will take up employment is 3.1 percentage points lower than without participation. The adverse effect of Temporary Extra Jobs one year after program start is, therefore, somewhat larger for native than for immigrant men (-3.1 versus -2.7 percentage points).

A similar pattern is observed for Temporary Extra Jobs starting in the second quarter of welfare receipt. Here, the estimates show negative treatment effects for participating male immigrants ranging between -1.9 and -4.4 percentage points. These effects are stronger than in the first quarter but less

[76] As noted above, the outcome variable is measured monthly from the first month of the following quarter from (potential) participation on. For individuals starting a Temporary Extra Job during the first quarter of their welfare spell, the first 6 months after program start are identical to months 4 to 9 after inflow into welfare. Thus, to assess the relative size of the program effects, the estimates must be related to lines 4 to 9 in table A.5.20 in the appendix to this chapter. Accordingly, one year after program start translates into 15 months after inflow into welfare. For Temporary Extra Jobs starting in the second quarter of welfare receipt, the first outcome measurement refers to month 7 after entering the welfare system. An analogous consideration applies for treatment starts in the third and fourth quarter of welfare receipt.

Table 5.1: Effects of Temporary Extra Jobs

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Quarter 1	Treated: 889 natives and 428 immigrants				Treated: 347 natives and 193 immigrants			
Natives	-0.0410^{***}	-0.0254^{**}	-0.0263^{**}	-0.0312^{**}	-0.0105	-0.0261	-0.0249	-0.0200
	(0.0089)	(0.0107)	(0.0116)	(0.0124)	(0.0162)	(0.0184)	(0.0194)	(0.0215)
Immigrants	-0.0080	-0.0129	-0.0242	-0.0272	-0.0390^{***}	-0.0119	0.0152	-0.0090
	(0.0134)	(0.0150)	(0.0156)	(0.0168)	(0.0127)	(0.0202)	(0.0258)	(0.0253)
Quarter 2	Treated: 671 natives and 334 immigrants				Treated: 347 natives and 167 immigrants			
Natives	-0.0565^{***}	-0.0565^{***}	-0.0422^{***}	-0.0438^{***}	-0.0310^{**}	-0.0295^{*}	-0.0325^{**}	-0.0285^{*}
	(0.0090)	(0.0111)	(0.0127)	(0.0150)	(0.0130)	(0.0156)	(0.0161)	(0.0171)
Immigrants	-0.0438^{***}	-0.0185	-0.0408^{**}	-0.0243	-0.0277^{**}	-0.0392^{**}	-0.0407^{**}	-0.0277
	(0.0141)	(0.0182)	(0.0192)	(0.0209)	(0.0138)	(0.0157)	(0.0190)	(0.0215)
Quarter 3	Treated: 474 natives and 252 immigrants				Treated: 239 natives and 126 immigrants			
Natives	-0.0263^{**}	-0.0172	-0.0306^{**}	-	-0.0245	-0.0145	-0.0152	-
	(0.0115)	(0.0149)	(0.0156)	-	(0.0150)	(0.0183)	(0.0196)	-
Immigrants	-0.0176	0.0086	0.0024	-	-0.0249	-0.0370^{**}	0.0097	-
	(0.0163)	(0.0218)	(0.0232)	-	(0.0154)	(0.0171)	(0.0246)	-
Quarter 4	Treated: 339 natives and 201 immigrants				Treated: 171 natives and 108 immigrants			
Natives	-0.0249^{*}	-0.0138	-	-	-0.0286^{*}	-0.0277	-	-
	(0.0130)	(0.0174)	-	-	(0.0149)	(0.0201)	-	-
Immigrants	0.0260	0.0349	-	-	-0.0055	0.0053	-	-
	(0.0216)	(0.0246)	-	-	(0.0223)	(0.0280)	-	-

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. Standard errors were obtained through bootstrapping based on 250 replications. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Fields marked by a - indicate that no outcome variable could be observed for the respective month.

adverse compared with the effects for natives starting a Temporary Extra Job at the same time. For this group, I estimate locking-in effects of -5.7 percentage points during the first six months after program start. Thereafter, the absolute size of the treatment effect declines just slightly. It still amounts to -4.4 percentage points twelve months after program start.

For Temporary Extra Jobs starting in the third or fourth quarter of welfare receipt, the treatment effects range between -1.4 and -3.1 percentage points for native males. The program thus clearly fails to achieve its objectives when starting during these quarters as well. For immigrant males who take up a Temporary Extra Job during the third and fourth quarter of their welfare spell, I do not estimate any significant treatment effect. Unlike the effects for programs starting during the first two quarters and unlike the picture observed for natives, effects tend to become positive at the end of the observation period. Hence, even though the program does not achieve its objectives, it at least does not reduce employment chances.

As in the case of native men, the estimated treatment effects of Temporary Extra Jobs on the employment chances of female natives are negative irrespective of the timing of the assignment to the program. For women without a migration background who participate in the program during the first quarter of their welfare spell, I cannot establish significant estimates of the treatment effects, but the point estimates range between -1.1 and -2.5 percentage points. In the second quarter, the negative treatment effects are more pronounced and amount to about -3 percentage points over the entire observation period. Like men, programs starting in the second quarter of the welfare spell exhibit the most adverse treatment effects for women too. For Temporary Extra Jobs assigned during the third and fourth quarter of the welfare spell,

I observe significant locking-in effects only up to three months after program start, which is equal to to the average duration of participation.

A similar result is found for female immigrants participating in Temporary Extra Jobs during the first quarter after entering the welfare system. Here, the negative locking-in effect is significant only in the first three months after program start and amounts to -3.9 percentage points. Thereafter, the estimate is statistically insignificant. Female immigrants treated in the second quarter face more adverse treatment effects. Participating in a Temporary Extra Job at this time reduces the probability of taking up employment by 3 to 4 percentage points over the entire observation period. In the third quarter, statistically significant locking-in effects last beyond the average duration of participation until the sixth month after program start. During the following months, the sign of the estimate turns positive, but the effect is statistically insignificant. For Temporary Extra Jobs assigned in the fourth quarter, no significant impact can be found.

My results differ to some extent from previous empirical evidence but deliver new insights into the effectiveness of Temporary Extra Jobs. Unlike Hohmeyer and Wolff (2007) and Hohmeyer (2009), I do not find positive employment effects. This could be due to the use of a different outcome measure. Hohmeyer and Wolff (2007) and Hohmeyer (2009) look at employment uptake irrespective of welfare status, whereas I focus on employment uptake conditional on drop-out from welfare receipt. The unconditional employment measure does not distinguish between employment that allows one to live independent of welfare benefits and employment that keeps workers in the welfare system because wages are too low. Both studies consider all Temporary Extra Jobs that lead to some sort of employment to be successful even if wages are low and the individual remains dependent on welfare. Yet I do not think that this is a meaningful outcome variable to evaluate the impact of Temporary Extra Jobs. From an economic point of view, a more appropriate outcome measure is employment uptake that generates a sufficient income such that no additional welfare payments are needed. Huber et al. (2011) use this outcome variable and find Temporary Extra Jobs to be ineffective at increasing employment chances conditional on drop-out from welfare receipt. Their results are based on a stock sample, however, which raises the concern that individuals with long-lasting welfare spells are over-represented. To prevent this, I use an inflow sample in my analysis and estimate negative treatment effects when Temporary Extra Jobs are assigned during the first two quarters of the welfare spell. When looking at programs that start in the third or fourth quarter of welfare receipt, I find mainly insignificant effects. The samples for the third and fourth quarter come close to a stock sample because they consist of individuals who experience relatively long welfare spells. Considering this similarity with a stock sample, my results are in line with Huber et al. (2011). Nevertheless, the results provided here are more robust given the relatively large number of immigrants observed in the data. My results further add to the literature by acknowledging that Tem-

porary Extra Jobs assigned during the first six months of a welfare spell are counterproductive and reduce employment chances of treated individuals.

5.6.2.2 Effects of Short-Term Training Programs

The estimated treatment effects for short-term training programs are displayed in tables 5.2 to 5.5. Table 5.2 shows that aptitude tests have a positive impact on the probability that native and immigrant males will take up employment providing a sufficient income above the subsistence level. This positive impact is independent of the exact timing of the training. Yet the absolute size and the significance of the effects differ across quarters and also between the two considered groups. For aptitude tests starting in the first quarter of welfare receipt, I observe larger treatment effects for natives than for immigrants. One year after starting the program, the effect for male natives amounts to 9.4 percentage points, whereas the corresponding value for men with a migration background is 6.5 percentage points. This means that the probability of native participants in aptitude tests taking up employment with a sufficient income is more than 9 percentage points larger than in a situation without training. Even though the estimated effect for immigrants is about 3 percentage points lower, it is still of considerable size.[77] In contrast to the first quarter, I observe for aptitude tests starting in the second quarter after the inflow into welfare that treatment effects are larger for immigrants than for natives. During the entire observation period, the estimated effect for immigrants amounts to slightly more than 10 percentage points, whereas the corresponding estimate for natives ranges between 7.6 and 9.7 percentage points. In the third quarter, the effectiveness of aptitude tests further increases for immigrants. Nine months after starting the program, the probability of treated immigrants finding employment is 15.7 percentage points larger than without the training. Treatment effects for natives, however, are lower than in the first two quarters. At the end of the observation period, the estimated treatment effect totals 6.4 percentage points and is only slightly significant. In the fourth quarter, the picture is reversed again. I find large treatment effects with a magnitude of more than 15 percentage points for men without a migration background, while the effect for immigrants is lower and amounts to about 10 percentage points.

For women, I also find positive effects of aptitude tests but observe a more uniform pattern of the estimated effects. Irrespective of the quarter of program start, native females benefit more from aptitude tests than women with a migration background. In the first quarter, the probability of taking

[77] The estimated treatment effects are substantial when compared with the overall outflow rate from welfare one year after (potential) program start (see table A.5.20 in the appendix to this chapter). On average, 25% of male natives have left the welfare system at this point in time (15 months after inflow). For male immigrants, I observe an outflow rate of about 20.3%.

Table 5.2: Effects of aptitude tests

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Quarter 1	Treated: 507 natives and 448 immigrants				Treated: 297 natives and 180 immigrants			
Natives	0.0668***	0.0867***	0.0834***	0.0939***	0.0490**	0.0690***	0.0673***	0.0948***
	(0.0178)	(0.0196)	(0.0206)	(0.0204)	(0.0202)	(0.0226)	(0.0246)	(0.0255)
Immigrants	0.0464***	0.0441**	0.0703***	0.0653***	0.0425*	0.0727***	0.0608**	0.0524*
	(0.0175)	(0.0181)	(0.0198)	(0.0214)	(0.0233)	(0.0270)	(0.0278)	(0.0291)
Quarter 2	Treated: 260 natives and 213 immigrants				Treated: 99 natives and 85 immigrants			
Natives	0.0965***	0.0848***	0.0760***	0.0947***	0.0675*	0.1205***	0.1170***	0.1159***
	(0.0248)	(0.0261)	(0.0268)	(0.0274)	(0.0400)	(0.0455)	(0.0448)	(0.0443)
Immigrants	0.1007***	0.1032***	0.1129***	0.1020***	0.0227	0.0194	-0.0111	0.0531
	(0.0267)	(0.0276)	(0.0306)	(0.0309)	(0.0328)	(0.0339)	(0.0315)	(0.0437)
Quarter 3	Treated: 138 natives and 143 immigrants				Treated: 62 natives and 76 immigrants			
Natives	0.0815**	0.0889**	0.0640*	-	0.1126**	0.0890*	0.0992*	-
	(0.0335)	(0.0355)	(0.0344)	-	(0.0500)	(0.0497)	(0.0554)	-
Immigrants	0.1033***	0.1279***	0.1566***	-	0.0601	0.0665	0.0767*	-
	(0.0326)	(0.0317)	(0.0362)	-	(0.0369)	(0.0415)	(0.0428)	-
Quarter 4	Treated: 91 natives and 98 immigrants							
Natives	0.1532***	0.1857***	-	-	/	/	-	-
	(0.0473)	(0.0501)	-	-	/	/	-	-
Immigrants	0.1085***	0.0943**	-	-	/	/	-	-
	(0.0367)	(0.0378)	-	-	/	/	-	-

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. Standard errors were obtained through bootstrapping based on 250 replications. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Fields marked by a - indicate that no outcome variable could be observed for the respective month. Fields marked by a / indicate that the number of treated individuals in the respective cell was too low to estimate valid treatment effects.

up a job providing a sufficient income within one year after the training increases for a female native participant by about 9.5 percentage points. By contrast, female immigrants participating in aptitude tests face only a slightly significant increase: 5.2 percentage points. In the second quarter, all estimated employment effects are insignificant for immigrant females. For native women, employment effects are highly significant and amount to more than 10 percentage points. Similar employment effects for native females are found in the third quarter. In this quarter, employment effects are positive for female immigrants but somewhat lower in magnitude compared with natives and only slightly significant.

In contrast to aptitude tests, job search training is mostly ineffective (see table 5.3). For native males, I estimate insignificant employment effects in all considered quarters. In case of male immigrants, the estimated effects are insignificant in the second and third quarter. Only in the first quarter do I observe significantly positive effects nine months after starting the program: about 6.4 percentage points. Yet this value represents a maximum only; it does not describe a long-lasting effect.

For female immigrants participating in job search training during the first quarter of their welfare spell, I observe negative employment effects at the beginning of the observation period. The probability of taking up a job and leaving the welfare system is reduced by up to 5.6 percentage points in the first six months after program start. Thereafter, the sign of effects turns positive, but the estimates are not statistically significant. By contrast, the corresponding figures for native women are positive and statistically significant over the entire observation period. In the first nine months after training, the treatment effect is around 6 percentage points; one year after program

Table 5.3: Effects of job search training

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Quarter 1	Treated: 303 natives and 270 immigrants				Treated: 189 natives and 102 immigrants			
Natives	-0.0123	0.0307	0.0224	0.0329	0.0621**	0.0585**	0.0556*	0.0910***
	(0.0188)	(0.0248)	(0.0241)	(0.0265)	(0.0269)	(0.0296)	(0.0305)	(0.0317)
Immigrants	-0.0014	0.0193	0.0637***	0.0414*	−0.0382**	−0.0564***	0.0131	0.0362
	(0.0184)	(0.0226)	(0.0237)	(0.0234)	(0.0181)	(0.0197)	(0.0322)	(0.0376)
Quarter 2	Treated: 116 natives and 110 immigrants				Treated: 69 natives			
Natives	-0.0073	0.0191	-0.0045	0.0026	0.0703	0.1005**	0.1205**	0.0848
	(0.0301)	(0.0337)	(0.0350)	(0.0377)	(0.0458)	(0.0488)	(0.0522)	(0.0523)
Immigrants	0.0034	-0.0323	0.0126	0.0193	/	/	/	/
	(0.0283)	(0.0285)	(0.0380)	(0.0414)	/	/	/	/
Quarter 3	Treated: 48 natives							
Natives	-0.0399	-0.0120	0.0191	-	/	/	/	-
	(0.0386)	(0.0474)	(0.0542)	-	/	/	/	-
Immigrants	/	/	/	-	/	/	/	-
	/	/	/	-	/	/	/	-
Quarter 4								
Natives	/	/	-	-	/	/	-	-
	/	/	-	-	/	/	-	-
Immigrants	/	/	-	-	/	/	-	-
	/	/	-	-	/	/	-	-

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. Standard errors were obtained through bootstrapping based on 250 replications. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Fields marked by a - indicate that no outcome variable could be observed for the respective month. Fields marked by a / indicate that the number of treated individuals in the respective cell was too low to estimate valid treatment effects.

start it rises to 9 percentage points. In the second quarter, employment effects for native women are highly positive, reaching a maximum of 12 percentage points nine months after program start and slightly declining afterward.

According to the findings, skill provision is more effective than job search training (see table 5.4). For native men who start training in the first quarter of the welfare spell, I observe positive employment effects. The training increases the participants' probability of taking up a job and leaving the welfare system by more than 10 percentage points in the second half of the observation period. For male immigrants, I also observe positive employment effects. Compared with native men, however, effects are smaller in magnitude and only slightly significant. One year after program start, the probability of finding a job increases by 6 percentage points. In the remaining quarters, employment effects are mostly insignificant for natives and immigrants. Only for immigrants participating in the third quarter do I observe increasingly positive effects during the observation period.

For the women, I find that skill provision in the first quarter is more effective among participants with a migration background than for natives. While for native women employment effects amount to nearly 6 percentage points one year after program start, for immigrants I estimate considerably larger effects, around 12.6 percentage points. By contrast, I find that employment effects in the second quarter are insignificant for female immigrants, and significantly positive for female natives. For the latter group, the probability of taking up a job increases by 12.5 percentage points six months after program start and then remains at a level of about 10 percentage points until the end of the observation period. In the third quarter, all estimated effects are posi-

Table 5.4: Effects of skill provision

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Quarter 1	Treated: 288 natives and 244 immigrants				Treated: 218 natives and 133 immigrants			
Natives	0.0430*	0.0764***	0.1192***	0.1026***	0.0318	0.0556**	0.0639**	0.0591**
	(0.0235)	(0.0280)	(0.0282)	(0.0285)	(0.0217)	(0.0258)	(0.0277)	(0.0292)
Immigrants	0.0218	0.0497*	0.0522*	0.0617**	0.0741***	0.0956***	0.1153***	0.1263***
	(0.0228)	(0.0263)	(0.0280)	(0.0295)	(0.0281)	(0.0304)	(0.0341)	(0.0355)
Quarter 2	Treated: 160 natives and 146 immigrants				Treated: 152 natives and 80 immigrants			
Natives	0.0157	0.0380	0.0308	-0.0029	0.0609*	0.1253***	0.1148***	0.0934***
	(0.0277)	(0.0329)	(0.0331)	(0.0355)	(0.0317)	(0.0385)	(0.0376)	(0.0358)
Immigrants	-0.0096	0.0332	-0.0084	0.0032	-0.0052	0.0039	-0.0026	-0.0068
	(0.0230)	(0.0288)	(0.0288)	(0.0326)	(0.0245)	(0.0288)	(0.0309)	(0.0325)
Quarter 3	Treated: 94 natives and 82 immigrants				Treated: 72 natives and 52 immigrants			
Natives	0.0124	-0.0115	-0.0016	-	0.0163	0.0201	0.0253	-
	(0.0348)	(0.0357)	(0.0391)	-	(0.0377)	(0.0411)	(0.0463)	-
Immigrants	0.0213	0.0527	0.0998**	-	0.0489	0.0086	0.0503	-
	(0.0362)	(0.0435)	(0.0455)	-	(0.0418)	(0.0393)	(0.0458)	-
Quarter 4	Treated: 69 natives and 67 immigrants				Treated: 48 natives and 41 immigrants			
Natives	0.0722	0.0688	-	-	-0.0214	−0.0486*	-	-
	(0.0454)	(0.0478)	-	-	(0.0264)	(0.0265)	-	-
Immigrants	0.1028**	0.0563	-	-	-0.0231	−0.0400*	-	-
	(0.0450)	(0.0437)	-	-	(0.0223)	(0.0225)	-	-

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. Standard errors were obtained through bootstrapping based on 250 replications. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Fields marked by a - indicate that no outcome variable could be observed for the respective month.

tive but insignificant; in the fourth quarter, treatment effects are increasingly negative over the entire observation period.

The combination of two or three modules in one program is not particularly effective, though some training modules show a positive impact on the probability of taking up employment (see table 5.5). For native men, I find no significant effect in any quarter. A similar picture arises for men with a migration background for the first and second quarter. In the third quarter, however, I find increasingly positive employment effects for this group though the degree of statistical significance is low. Nine months after program start, the estimated effect amounts to 7.4 percentage points, but the effect is significant only at the 10%-level. A similar development of the estimated treatment effects is found for immigrant females participating in combined training programs in the first quarter of the welfare spell. Here, the effect amounts to 6.6 percentage points at the end of the observation period. In the second quarter, the corresponding estimate is slightly larger – 7.9 percentage points – but again the effect is significant only at the 10%-level. In the third quarter, I do not detect any significant effect. For women without a migration background, I estimate insignificant employment effects in all quarters. The general ineffectiveness of combined training programs might be due to the characteristics of the targeted group. As noted above, combined training programs are generally assigned to those persons who have been out of labor force for a substantial fraction of the two years before treatment. These persons are likely to face multiple obstacles to employment uptake and which might not be remediable by combined training programs. Another explanation might be that some combinations of training modules are effective and others are not, so that the overall effect of combined training programs is

insignificant. Unfortunately, the data do not allow us to distinguish between different types of combined training programs.

Table 5.5: Effects of combined training programs

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Quarter 1	Treated: 410 natives and 360 immigrants				Treated: 188 natives and 135 immigrants			
Natives	0.0045	0.0306	0.0293	0.0289	0.0116	0.0157	0.0167	0.0066
	(0.0161)	(0.0201)	(0.0211)	(0.0209)	(0.0234)	(0.0268)	(0.0269)	(0.0291)
Immigrants	0.0069	0.0316	0.0208	0.0272	-0.0027	0.0142	0.0533	0.0656*
	(0.0184)	(0.0215)	(0.0207)	(0.0225)	(0.0212)	(0.0278)	(0.0331)	(0.0351)
Quarter 2	Treated: 180 natives and 157 immigrants				Treated: 113 natives and 95 immigrants			
Natives	0.0207	0.0089	0.0374	0.0379	0.0470	0.0459	0.0399	0.0452
	(0.0278)	(0.0283)	(0.0325)	(0.0356)	(0.0357)	(0.0365)	(0.0377)	(0.0379)
Immigrants	0.0082	0.0271	0.0054	0.0519	0.0586*	0.0512	0.0742*	0.0786**
	(0.0267)	(0.0288)	(0.0298)	(0.0331)	(0.0339)	(0.0346)	(0.0387)	(0.0388)
Quarter 3	Treated: 90 natives and 96 immigrants				Treated: 66 natives and 56 immigrants			
Natives	-0.0250	-0.0262	-0.0355	-	0.0107	0.0176	0.0256	-
	(0.0279)	(0.0347)	(0.0363)	-	(0.0350)	(0.0424)	(0.0467)	-
Immigrants	0.0154	0.0588	0.0744*	-	0.0247	0.0228	0.0600	-
	(0.0310)	(0.0397)	(0.0438)	-	(0.0372)	(0.0414)	(0.0388)	-
Quarter 4	Treated: 63 natives							
Natives	-0.0407	-0.0108	-	-	/	/	-	-
	(0.0311)	(0.0454)	-	-	/	/	-	-
Immigrants	/	/	-	-	/	/	-	-
	/	/	-	-	/	/	-	-

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. Standard errors were obtained through bootstrapping based on 250 replications. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Fields marked by a - indicate that no outcome variable could be observed for the respective month. Fields marked by a / indicate that the number of treated individuals in the respective cell was too low to estimate valid treatment effects.

My results are in line with previous empirical evidence. Huber et al. (2011) do not distinguish between different training modules but find on average positive employment effects of short-term training. These positive effects might be driven by aptitude tests and skill provision. Kopf (2009) uses a categorization of training programs in her study similar to the one used in this thesis. Like me, she estimates positive employment effects of aptitude tests and skill provision, and finds job search training and combined training programs to be mostly ineffective. Yet my estimated treatment effects for aptitude tests and skill provision are somewhat larger than those she reports. This might be due to the different time horizon of the analysis or the different sampling of the data. While I use an inflow sample and look at programs starting in 2006 and 2007, the study by Kopf (2009) is based on a stock sample and considers programs assigned from February to April 2005. Wolff and Jozwiack (2007) use the same data as Kopf (2009) but do not distinguish between different training modules. They find that short-term training programs are effective among immigrant men in West Germany, while they tend to decrease employment chances of immigrant men in East Germany and of women with a migration background regardless of location in the short run. My results indicate that the adverse effects for female immigrants might be caused by job search training, but I also find that aptitude tests and skill provision exhibit positive employment effects in this subgroup.

5.6.3 Immigrant Fixed Effects

The previous subsection showed that the treatment effects of Temporary Extra Jobs and short-term training programs differ to some extent between native and immigrant participants. This raises the question: What causes those differences? Are they due to differences in the observable characteristics of the two groups, or are they due to unobservable differences subsumed in the immigrant fixed effect? To find some answers I estimate the immigrant fixed effect as described in subsection 5.5.3. Table 5.6 displays the results.[78]

Table 5.6: Differences in treatment effects between native Germans and immigrants

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Temporary Extra Jobs								
Δ^{ATT}_{Dif}	0.033	0.013	0.002	0.004	-0.029	0.014	0.040	0.011
p-value	0.033	0.514	0.920	0.852	0.196	0.598	0.192	0.737
IFE	0.036	0.021	0.010	0.011	-0.011	0.040	0.069	0.060
p-value	0.076	0.374	0.672	0.666	0.648	0.241	0.079	0.100
Aptitude tests								
Δ^{ATT}_{Dif}	-0.020	-0.043	-0.013	-0.029	-0.007	-0.004	-0.007	-0.042
p-value	0.413	0.119	0.649	0.339	0.839	0.921	0.864	0.295
IFE	0.013	0.017	0.064	0.030	0.032	0.060	0.042	0.011
p-value	0.684	0.620	0.065	0.453	0.488	0.241	0.456	0.847
Job search training								
Δ^{ATT}_{Dif}	0.011	-0.011	0.041	0.009	-0.100	-0.115	-0.043	-0.055
p-value	0.685	0.729	0.246	0.818	0.010	0.007	0.379	0.230
IFE	0.004	-0.004	0.024	-0.032	-0.149	-0.143	-0.000	0.007
p-value	0.930	0.949	0.721	0.648	0.082	0.129	0.999	0.950
Skill provision								
Δ^{ATT}_{Dif}	-0.021	-0.027	-0.067	-0.041	0.042	0.040	0.051	0.067
p-value	0.503	0.468	0.087	0.310	0.256	0.354	0.263	0.159
IFE	0.011	0.023	-0.078	-0.059	0.096	0.115	0.130	0.140
p-value	0.816	0.634	0.144	0.324	0.105	0.091	0.099	0.092
Combined training programs								
Δ^{ATT}_{Dif}	0.002	0.001	-0.009	-0.002	-0.014	-0.002	0.037	0.059
p-value	0.921	0.971	0.775	0.958	0.671	0.972	0.407	0.201
IFE	0.001	-0.028	-0.019	-0.039	0.039	0.018	0.081	0.043
p-value	0.970	0.556	0.696	0.454	0.615	0.835	0.396	0.697

Remarks: Δ^{ATT}_{Dif} denotes the raw differential in ATTs between immigrants and native Germans for each program and month after program start. The p-value in the second row of each block displays the statistical significance for the difference in ATTs between immigrants and natives. IFE denotes the estimated difference in ATTs for immigrants and native Germans due solely to unobservable differences between the groups, i.e. the immigrant fixed effect. The p-values in the final line of each block denote the significance of these immigrant fixed effects. Standard errors of the immigrant fixed effects were obtained through bootstrapping based on 250 replications.

The first row of each block in the table depicts the raw differential in the ATTs between immigrants and native Germans for each program. This raw differential is calculated from the results presented in the previous subsection. The p-value in the second row denotes the statistical significance for the difference in ATTs between immigrants and natives. The third row of each block in the table is denoted by IFE and shows the estimated part of the raw differential due to the immigrant fixed effect. In other words, IFE

[78] I estimate the immigrant fixed effects only for programs starting in the first quarter of welfare receipt. In later quarters, the number of program participants becomes too small, especially for short-term training programs.

indicates how much the treatment effect of a program is changed due to migration background alone. The p-value in the fourth row denotes statistical significance of the immigrant fixed effect.

The entry 0.033 in the top left block of table 5.6 states that, three months after program start, the ATT for male immigrants participating in Temporary Extra Jobs during the first quarter of their welfare spell is 3.3 percentage points larger than for treated natives. This raw differential is statistically significant at the 5%-level and cannot be explained by observable differences in the characteristics of natives and immigrants. Rather, it must be attributed to the immigrant fixed effect. If all other characteristics were kept constant, immigrants would have a treatment effect 3.6 percentage points larger than that of natives (see the third row of the top left block of table 5.6). Hence, immigrants benefit more from Temporary Extra Jobs than identical men without a migration background. Yet this immigrant fixed effect decreases over time and loses its statistical significance six months after the program is assigned. The opposite picture can be observed for women participating in Temporary Extra Jobs during the first quarter of the welfare spell. Here, the immigrant fixed effect increases over time and reaches a maximum of nearly 7 percentage points nine months after program start. As in the case of men, female immigrants benefit more from Temporary Extra Jobs than natives, all other things being equal. It must be kept in mind, however, that I estimate negative treatment effects for all subgroups. Temporary Extra Jobs decrease rather than increase employment chances of the treated individuals, as if the skills acquired during participation are not transferable to regular employment. This might be due to the fact that Temporary Extra Jobs have to be additional to market activities. Another reason for the observed negative treatment effects could be stigmatization. Regular employers might consider participants in Temporary Extra Jobs as having low levels of productivity.

Concerning short-term training, the estimation results for the immigrant fixed effect are mostly mixed. For aptitude tests, I estimate that male immigrant participants face a treatment effect three months after program start 1.3 percentage points larger on average than native participants with identical sociodemographic characteristics (see table 5.6). But, as can be seen from the p-value, this immigrant fixed effect is not statistically significant at conventional levels. Six months after program start, I observe a similar picture. The immigrant fixed effect is positive but insignificant. During the following months the immigrant fixed effect increases and reaches a maximum of 6.4 percentage points nine months after program start. In this month, the effect is statistically significant at the 10%-level. At the end of the observation period, the immigrant fixed effect decreases and amounts to about 3 percentage points one year after assignment. Even though the effect lacks statistical significance, it is of considerable size. For women participating in aptitude tests, I arrive at a similar conclusion. When all covariates remain constant, the positive immigrant fixed effect indicates that immigrants tend

to benefit more from aptitude tests than natives, though the improvement is statistically insignificant.

In my examination of job search training targeted at male welfare recipients, I do not discern statistically significant immigrant fixed effects. I observe that in the short run female immigrant participants clearly benefit less than female natives from this form of training. The large gap in treatment effects is caused by the immigrant fixed effect. Keeping everything else constant, immigrant females participating in job search training have a treatment effect nearly 15 percentage points lower than native women during the six months after program start. Even though the negative immigrant fixed effect fades away nine months after program start, job search training seems not to meet the needs of female immigrants. It might be that women with a migration background prefer an informal job search process and that they rely on networks within their community when looking for employment. If so, learning how to write job applications might be a waste of time. The program might not work for female immigrants because job search training is geared toward the needs of the average native welfare recipient.

By contrast, female immigrants clearly benefit from skill provision. For this form of training, the immigrant fixed effect increases over time and amounts to 14 percentage points one year after program start. Filtering out observable differences between immigrants and natives, the former have a treatment effect 14 percentage points larger on average than the latter. Welfare agencies should thus consider more frequent use of this program for female immigrants. For men, I do not find statistically significant differences between the groups. Here, the immigrant fixed effect is negative at the end of the observation period, indicating that immigrants tend to benefit less from participation than identical natives do.

For the combined training programs, I find no statistically significant differences. This reflects the finding that combined programs are mostly ineffective for immigrants and native Germans and for men and women.

5.6.4 Effects for Subgroups of Immigrants

In this subsection, I evaluate Temporary Extra Jobs and short-term training programs for subgroups of immigrants. I look separately at individuals with a Turkish, an Eastern European, or a Southern European migration background.[79] These subgroups comprise the vast majority of individuals with a migration background in the German welfare system. Again, I carry out my estimations separately for men and women. I focus on programs starting

[79] Individuals with an Eastern European migration background are defined to include persons from Poland, the Czech Republic, Slovakia, Hungary, Bulgaria, Romania, Albania, and the former Soviet Union. Southern European immigrants comprise individuals from Spain, Portugal, Italy, Greece, and the former Yugoslavia.

in the first quarter of welfare receipt, as the number of treated individuals becomes too small in later quarters. Note, however, that estimation samples are small already in the first quarter so that the estimated effects have to be interpreted with caution.[80]

My results for Temporary Extra Jobs are displayed in table A.5.22 in the appendix to this chapter. For Turkish and Eastern European men participating in Temporary Extra Jobs, I observe negative treatment effects, which increase in absolute terms over time. At the end of the observation period, employment chances are about 5 percentage points lower than for non-treated individuals with a Turkish or an Eastern European migration background. The estimated effects are, however, not statistically significant. The same is true for treated men from Southern Europe. In this subgroup, treatment effects tend to be positive at the beginning of the observation period and negative at the end.

For women participating in Temporary Extra Jobs, I observe significant locking-in effects three months after program start in all subgroups. These effects vary between -3.6 percentage points for Turkish women and -6.9 percentage points for female immigrants from Southern Europe. Thereafter, effects increase in all subgroups. For Eastern European women, effects become positive and amount to 7.3 percentage points one year after program start. The effect is, however, not statistically significant. For Turkish and Southern European women, effects one year after program start are insignificant, too, but tend to remain negative.

When comparing treatment effects of the subgroups of immigrants with treatment effects of native Germans participating in Temporary Extra Jobs, I observe a large raw differential between native and Southern European men (see table A.5.23 in the appendix to this chapter). Effects are about 5 percentage points larger for Southern European participants in the first six months after program start. The immigrant fixed effect in this period is even larger and amounts to about 7 percentage points. When everything else remains constant, Southern European men thus tend to benefit more from Temporary Extra Jobs than native men. The immigrant fixed effect is, however, not statistically significant. Likewise, for Turkish and Eastern European men, I find no significant differences in treatment effects between immigrants and identical natives.

In case of immigrant women, I observe an increasing immigrant fixed effect over time in all considered subgroups. One year after program start, the immigrant fixed effect is of considerable size. It varies between 9.4 percentage points for Southern European women and 12.8 percentage points for Turkish women. Despite the large size, however, I cannot establish statistical significance.

Turning to the effectiveness of short-term training programs and looking first at aptitude tests, I estimate positive employment effects in all considered

[80] The numbers of observations in the subsamples are displayed in table A.5.21 in the appendix to this chapter.

subgroups of immigrant men (see table A.5.24 in the appendix to this chapter). The largest impact of aptitude tests is found for Southern European men. In this subgroup, effects are significant during the entire observation period and amount to more than 10 percentage points one year after program start. By contrast, I find no significant effect for Southern European women. Here, the treatment effect one year after program start is virtually zero. Treatment effects are larger for Turkish and Eastern European women. In the latter subgroup, effects amount up to 7.8 percentage points at the end of the observation period, but this remains statistically insignificant.

I observe gender differences within subgroups of immigrants for the effectiveness of job search training, too (see table A.5.24). In the case of men, Eastern European immigrants tend to benefit most from the program with a treatment effect of nearly 10 percentage points one year after program start. By contrast, the overall negative effect of job search training for immigrant women derives mainly from the subgroup of Eastern European women. In this subgroup, treatment effects amount to -6.7 percentage points three months after program start and to -9.2 percentage points six months after start. Effects are highly significant during the first half of the observation period. For Turkish and Eastern European women, treatment effects are negative in the first six months but not statistically significant. In the second half of the observation period, the effects are insignificant in all subgroups but tend to be positive, in particular for Southern European women.

The overall positive effect of skill provision on employment chances of immigrant men originates mainly from the subgroup of Turkish and Eastern European men (see table A.5.24). For Turkish men, the treatment effect one year after program start amounts to 6.7 percentage points and to 9.8 percentage points for Eastern European men. In the case of female immigrants, the estimated treatment effects for these subgroups are even more pronounced. One year after program start, Turkish women who participate in skill provision have an employment probability 9 percentage points larger than without the training. For Eastern European women, the estimated treatment effect at the end of the observation period amounts to 15.7 percentage points. Southern European women benefit from skill provision in a similar way with a treatment effect of 14 percentage points one year after program start, but the effect is not statistically significant.

Although I find no significant impact of combined training programs in the overall sample of immigrants, I identify two subgroups for which treatment increases employment chances. Both Eastern European men and Eastern European women tend to benefit from combined programs (see table A.5.24). For Eastern European men, I estimate treatment effects of up to 17.5 percentage points – a statistically significant figure. The estimated treatment effect one year after program start amounts to 11.7 percentage points. A similar effect – 10.8 percentage points – is found for Eastern European women at the end of the observation period. This effect, however, is statistically insignifi-

cant. For the other subgroups of immigrants, the estimated effects are smaller or even negative but not significant.

To present the estimation results for immigrant fixed effects in case of short-term training targeted at Turkish, Eastern European, and Southern European immigrants, I look at the different subgroups in turn. Table A.5.25 in the appendix to this chapter displays the differences in treatment effects of short-term training between Turkish immigrants and native Germans. As the table shows, immigrant fixed effects tend to be negative but small in absolute size for both genders for aptitude tests. In case of job search training, I find substantially negative immigrant fixed effects. For Turkish women, immigrant fixed effects amount to -25 percentage points in the first six months after program start. This figure is very statistically significant. For Turkish men in job search training, I find negative, but insignificant, immigrant fixed effects in the second half of the observation period. For skill provision, I estimate large positive immigrant fixed effects for Turkish women. They amount to more than 10 percentage points in the first nine months of the observation period but are not statistically significant. For Turkish men, immigrant fixed effects are positive in the first six months after program start and negative thereafter. Compared with those experienced by women, these effects are small and insignificant. In the case of combined training programs, immigrant fixed effects tend to be negative for men but positive for women. Even though immigrant fixed effects amount to about 10 percentage points for women in the first six months of the observation period, I cannot establish statistical significance.

Results for effect differences between Eastern European immigrants and native Germans participating in short-term training are summarized in table A.5.26 in the appendix to this chapter. In case of aptitude tests, I find almost no difference between treated immigrants and identical native Germans. For job search training and women, I estimate considerable negative immigrant fixed effects. They amount to -27 percentage points in the first six months after program start – comparable to the immigrant fixed effects found for Turkish women in the same program. For Eastern European men in job search training, immigrant fixed effects are negative during the first half of the observation period, too, but more modest and statistically insignificant. In case of skill provision, I do not find a clear pattern of immigrant fixed effects. For both genders, immigrant fixed effects tend to be negative in the middle of the observation period but positive at the beginning and at the end. The immigrant fixed effects are not statistically significant, even though they amount to more than 10 percentage points in absolute terms for men in the middle of the observation period. For combined training programs, I have estimated positive treatment effects during the entire observation period for men and during the second half for women. During these periods, I also estimate positive immigrant fixed effects. They are, however, statistically insignificant.

Finally, table A.5.27 displays differences in treatment effects of short-term training between native Germans and Southern European immigrants. In the case of aptitude tests, immigrant fixed effects for men are positive and amount up to 16 percentage points nine months after program start. For women, by contrast, immigrant fixed effects tend to be negative but statistically insignificant. For job search training, I can estimate immigrant fixed effects only for men as the number of observations is too small for women. Immigrant fixed effects for men are positive during the first nine months after program start, are negative thereafter, and remain statistically insignificant throughout. In the case of skill provision, immigrant fixed effects tend to be negative for men and positive for women. Yet, for both genders immigrant fixed effects are insignificant. For combined training programs, I observe similar trends. Immigrant fixed effects tend to be negative for men and positive for women. The negative immigrant fixed effect for men is of considerable size and amounts to 14 percentage points in absolute terms one year after program start. The effect, however, is not significant. Likewise, I find no significant immigrant fixed effects for women.

5.7 Summary

The 2005 welfare reform was the first to establish a consistent activation framework for welfare recipients in Germany. To support the integration of welfare recipients into employment, the reform put strong emphasis on the use of ALMP by welfare agencies. Surprisingly, despite a high share of immigrants in welfare, no specific programs are offered to individuals with a migration background. Instead, immigrant welfare recipients are placed in the same programs as natives. Major programs are Temporary Extra Jobs and short-term training. Temporary Extra Jobs provide temporary work opportunities in the public sector and are intended to maintain and improve the employability of the participants for (later) integration into regular employment. Short-term training programs contain aptitude tests, job search training, and courses providing skills required for employment. The three modules can be offered separately or in combination according to the needs of welfare recipients.

Using comprehensive administrative data providing rich and unique information on immigrant and native welfare recipients, I evaluated the effects of Temporary Extra Jobs and short-term training programs on self-sufficient employment. My first step was to identify raw differentials in the program effects between both groups. To take into account the timing of treatment during the welfare spell, I applied a propensity score matching estimator in the dynamic setting. I then have estimated the part of the effect difference between immigrants and native Germans that results solely from group attachment (immigrant fixed effect). It determines differences in employment

chances of program participants from both groups that cannot be explained by differences in composition.

The estimation results for the program effects and the immigrant fixed effects exhibit substantial effect heterogeneity. Temporary Extra Jobs reduce the probability that participants will take up a regular job providing a sufficient income above the subsistence level. Treatment effects are especially adverse if a Temporary Extra Job is started during the second quarter of a welfare spell. Even though program effects for immigrants are not as unfavorable as for natives in most cases, Temporary Extra Jobs are not an effective activation measure for this group. The estimation of the immigrant fixed effects shows that immigrants tend to benefit more from Temporary Extra Jobs than natives with otherwise identical characteristics. But using this result to derive the conclusion that Temporary Extra Jobs should be used more frequently for immigrants is misleading. The negative treatment effects Temporary Extra Jobs exhibit for both native Germans and immigrants indicate that the program fails to achieve its objectives.

Participation in aptitude tests results in positive employment effects for both immigrants and natives independently of gender. This result is in line with theoretical expectations as aptitude tests are intended to increase specific occupational skills and to give caseworkers better knowledge about the abilities and labor market prospects of their clients. Both elements have positive effects on employment and placement chances.

Job search training, by contrast, is ineffective for men. Native women benefit from this form of training, while immigrant females face negative treatment effects. The differences in treatment effects between native and immigrant females are especially pronounced in the first six months after program start. There is also a considerable negative immigrant fixed effect of roughly 15 percentage points difference in employment chances between women with and without a migration background. This large immigrant fixed effect originates mainly in the subsamples of Turkish and Eastern European women. For these subgroups, I observe immigrant fixed effects of about 25 percentage points. Even though the immigrant fixed effects fade away nine months after program start, the results indicate that job search training does not meet the needs of female immigrants.

By contrast, skill provision is of clear benefit for female immigrants. This program exhibits positive effects in general when assigned early during the welfare spell. The immigrant fixed effect in the entire sample of women increases over time and amounts to 14 percentage points one year after the program begins.

Combined training programs are mostly ineffective. I do not find a significant impact on employment chances on average. Only Eastern European immigrants tend to benefit from such programs.

Chapter 6

Fiscal Cost-Benefit Analyses for Temporary Extra Jobs and Short-Term Training Programs

6.1 Introductory Remarks

In this chapter, I continue the evaluation of Temporary Extra Jobs and short-term training programs begun in chapter 5 by examining their efficiency. Programs can be recommended for use only if they are shown to be efficient as well as effective. In this chapter, I define efficiency as a state in which program benefits outweigh program costs. To determine whether the programs evaluated in chapter 5 are efficient from a government perspective, I conduct a series of cost-benefit analyses. The comparison of costs and benefits can, for instance, help quantify return on investment. By calculating the ratio of benefits to costs, one can determine the financial gain of a program for every euro invested. This benefit-cost ratio can then be used to compare efficiency between programs.

I focus on fiscal costs and benefits, as the government is responsible for the provision of ALMP and ultimately decides whether to preserve or cancel them. Programs that foster employment uptake benefit the government by increasing income tax revenues and social insurance contributions. They also bring about reductions in welfare payments and in expenditures for welfare administration. If the benefits outweigh operating costs (e.g. teaching materials, teacher reimbursement), the program use will be efficient for the government and will result in a fiscal gain. By contrast, if the costs are higher than the benefits, the program will not pay off, and the government will incur a fiscal loss.

Even though cost-benefit analyses are indispensable for drafting meaningful policy recommendations, they are conducted less frequently than impact analyses (see Kluve, 2010; and Card et al., 2010), though they are more common for US programs than for European ones. See Heckman et al. (1999), Greenberg and Cebulla (2008) and Greenberg et al. (2010) for summarized evidence on the efficiency of numerous ALMP in the US. Recent European evidence concentrates on the Nordic countries. Raaum et al. (2002) perform

a cost-benefit analysis for a labor market training program in Norway. Jespersen et al. (2008) look at different ALMP in Denmark.

For Germany, evidence on the costs and benefits of programs is scarce. Wunsch and Lechner (2008) evaluate different ALMP targeted at recipients of unemployment insurance benefits and unemployment assistance between 2000 and 2002. They find that these programs fail to improve employment chances and instead increase the probability of unemployment. Based on the estimated adverse program effects, they calculate indirect program costs in terms of additional unemployment benefit payments and expenditures on wage subsidies.

The study of Wunsch and Lechner (2008) is a manifestation of increasing political interest in cost-benefit analyses for German ALMP. Between 2003 and 2005, rigorous evaluations of ALMP became more important and in some cases legally mandatory, as German labor market reforms were passed. Consequently, the German parliament commissioned fiscal cost-benefit analyses for several activation measures targeted at unemployment insurance benefit recipients. These measures include job creation schemes (*Arbeitsbeschaffungsmaßnahmen*), wage subsidies to employers (*Eingliederungszuschüsse*), long-term training programs (*Förderung der beruflichen Weiterbildung*), and programs that promote self-employment (*Überbrückungsgeld*, *Existenzgründungszuschuss*). The analyses were conducted by different research institutes and cover the time period from 1999 to 2006.

SÖSTRA et al. (2006) investigate the costs and benefits of job creation schemes. Using descriptive statistics on government expenditures, the authors report that nearly 90% of the operating costs of these schemes are recovered by reductions in unemployment insurance benefits and by increases in tax revenue. Because they do not consider the potential benefits associated with a later integration of treated individuals into regular employment, it remains unclear whether the schemes are efficient in the long run. But given the disappointing results of more comprehensive evaluations on the employment effects of job creation schemes (see Hujer and Thomsen, 2010), efficiency of the program is highly unlikely.

ZEW et al. (2006) investigate the fiscal costs and benefits of wage subsidies for employers who hire older unemployed individuals. The authors determine the magnitude of the treatment effect that would be necessary to make the subsidies cost-neutral. Cost-neutrality is achieved if the considered fiscal benefits of employment integration (income tax, social insurance contributions, reductions in unemployment insurance benefits, reductions in other activation expenditures, and reductions in administrative expenditures) outweigh the costs of the subsidy. They find that the effect for cost-neutrality is larger than the actual treatment effect of the subsidy estimated using a Cox proportional hazard model, which indicates a fiscal loss on the part of the government, as the benefits are too small to offset the costs. Because the data base used provides only a rough measure of fiscal benefits, however, these findings may not be entirely accurate. Specifically, figures on potential

increases in tax revenue and social insurance contributions are obtained from statistics for average workers, and the potential reductions in unemployment insurance benefits and in other activation and administrative expenditures are calculated on the basis of statistics for the average unemployed. It would be more accurate to measure the fiscal benefits for the actual population of interest (older unemployed who take up a subsidized job), as fiscal benefits for this group can differ substantially from the average.

IZA et al. (2006) evaluate different long-term training programs with a duration of up to three years. The methodological approach of the authors is similar to the one used in this thesis. Program effects are estimated with propensity score matching for an observation period of up to 48 months after program start. For the cost-benefit analyses, the estimated program effects for each month of the observation period are accumulated over time and given a monetary value in terms of fiscal benefits (income tax, social insurance contributions, reductions in unemployment insurance benefits, reductions in other activation expenditures, and reductions in administrative expenditures). For programs starting between 2000 and 2002, it takes between one and three years before benefits outweigh costs. The duration of the pay-off period increases with program duration. In 2003, long-term training programs were reformed, and their duration was shortened on average. As a consequence, pay-off periods became shorter as well. Yet these results must be interpreted with caution as the accumulated benefits are not discounted and thus might be overestimated. Benefits materializing at the end of the observation period of four years are valued in the same way as benefits accruing right after program start. Since the government is likely to value immediate benefits higher than future ones, the assumption of a zero discount rate is not realistic.

The same drawback applies to the study of IAB et al. (2006) evaluating two programs promoting self-employment (*Überbrückungsgeld* and *Existenzgründungszuschuss*). Program effects are estimated with propensity score matching for up to 28 months after program start. The program effects are then accumulated, valued with the associated reductions in unemployment insurance benefits, and compared with program costs. The study's finding are mixed. For one program (*Überbrückungsgeld*) fiscal benefits outweigh costs; for another (*Existenzgründungszuschuss*) they do not. But again, because the accumulated benefits are not discounted, the results must be seen as unreliable. Another shortcoming of the study is that it fails to consider increases in tax revenues from self-employment and other potential benefits from start-up firms such as the creation of additional jobs.

Despite the increasing interest and the increased number of analyses,[81] the German Federal Ministry of Labour and Social Affairs (*Bundesministerium für Arbeit und Soziales*) reports that relatively little is known about costs and benefits of ALMP in Germany (see Bundesministerium für Arbeit und Soziales, 2011). Worse, existing studies rely on disparate assumptions and frameworks and thus fail to analyze costs and benefits systematically for a well-defined period. Furthermore, these studies focus heavily on programs for unemployment insurance benefit recipients. To the best of my knowledge, none analyze the efficiency of ALMP targeted at welfare recipients.[82]

In order to gain more insight into the efficiency of ALMP in the German welfare system and to provide comparable evidence across programs, I conduct fiscal cost-benefit analyses for Temporary Extra Jobs and four different short-term training programs (aptitude tests, job search training, skill provision, combined training programs) systematically. For each program, I measure costs and benefits using the same approach. I accumulate and discount benefits over time and contrast them with program costs monthly for a time period of up to one year after program start. This allows me to analyze the dynamic evolution of program efficiency and to compare it between programs. As in the previous chapter, I carry out the analyses separately for men and women and for native Germans and immigrants, producing 20 different sets of results.[83]

The remainder of this chapter is organized as follows. The next section explains the conceptual framework of my analyses and describes which costs and benefits I consider. In section 6.3, I outline the method I use for determining costs and benefits. Section 6.4 presents the results of my analyses, and section 6.5 concludes my findings.

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[81] In addition to the discussed studies, Pfeiffer and Winterhager (2006a and 2006b) perform cost-benefit analyses for two activation measures that are intended to foster competition between public and private employment services so as to integrate recipients of unemployment insurance benefits back into the workforce: job placement vouchers (*Vermittlungsgutscheine*) and the subcontracting of placement services to private providers (*Beauftragung Dritter mit der Vermittlung*). Stephan (2010) provides a cost-benefit analysis for wage subsidies (*Eingliederungszuschüsse*) paid to employers who hire hard-to-place unemployed individuals.

[82] ZEW et al. (2008) investigate the fiscal costs and benefits of several organizational and strategical features of welfare agencies but do not look at ALMP.

[83] My analyses in this chapter are restricted to Temporary Extra Jobs and short-term training. I do not consider the efficiency of centralized and decentralized welfare agencies because I do not have information on the costs of the administrative models. Nor do I look at costs and benefits of sanctions, as I lack data on the administrative costs of sanctions. Finally, because I estimated Local Average Treatment Effects in chapter 4, I have no precise information about the characteristics of so-called compliers for whom the treatment effects are estimated.

6.2 Conceptual Framework

According to Greenberg et al. (2010), a cost-benefit analysis can be conducted from at least three different perspectives: the government's, the participant's, and the society's. From the governmental perspective, a program that integrates a participant into employment leads to benefits in terms of an increase in income tax payments and in social insurance contributions, and reductions in welfare benefits and administrative costs. These benefits are then weighed against the operating costs of the program.

From the perspective of a program participant, employment is beneficial because of increased earnings possibilities for consumption. Being a part of the workforce may also bring with it non-monetary benefits, such as new social contacts or a more structured daily routine. As the literature on well-being shows, employed persons tend to be happier than unemployed persons (see Clark and Oswald, 1994; and Winkelmann and Winkelmann, 1998). At the same time integration into employment is also costly: taxes and social insurance contributions have to be paid and welfare benefits are lost. Participation in a program could also be costly due to forgone earnings while in the program and lost leisure time.

Society's perspective involves the costs and benefits of both the government and the participant; but it also includes others as well. On the cost side, society has to take into account displacement effects, in which program participants take up jobs that non-participants would have gotten had the program not existed. Another potential cost to society is the deadweight loss caused by raising taxes to finance such programs (see Browning, 1987). On the benefit side, employment uptake of a participant could generate economic activity and additional job openings. Participation in a program might also produce forms of social value.[84]

Conducting a cost-benefit analysis for the social perspective usually requires numerous assumptions, as variables such as displacement effects, deadweight loss due to taxation, or the social value of program output are difficult to measure. Likewise, many assumptions must be made when calculating costs and benefits from the participant's point of view, e.g. assumptions about the value of lost leisure time or about the value of new social contacts when integrated into employment. A cost-benefit analysis for government requires fewer and less tenuous assumptions, as its components are more readily determinable.

A cost-benefit analysis from the government's perspective is the most relevant perspective for formulating policy recommendations on the use of programs because the government is the one that decides whether it can fund

[84] For example, participants in Temporary Extra Jobs could produce social value if their task is to keep public parks clean. I noted in chapter 5, legislation stipulates that Temporary Extra Jobs be of value for society.

a given program. I thus focus on fiscal costs and benefits.[85] Specifically, I consider the benefits and costs depicted in table 6.1.

Table 6.1: Considered costs and benefits in the analyses

Benefits	Costs
• Social insurance contributions • Income tax • Solidarity surcharge • Reductions in UBII payments • Reductions in housing costs • Reductions in administrative costs	• All expenses directly associated with the operation of a program, e.g. payments for technical equipment, teaching materials, teacher reimbursement • In-work benefits

Remarks: Reductions in UBII payments concern the UBII base payment, social allowance, social insurance contributions, and expenses for additional needs.

Programs that help participants find employment can benefit the government in six ways. First, both employees and their employers must pay social insurance. Second, the income tax paid by the newly employed increases government revenue. Third, employees are also subject to the so-called solidarity surcharge, a tax introduced in 1991 to finance the costs of German reunification. Fourth and fifth, fewer welfare recipients mean that government has fewer UBII payments[86] and housing costs. And finally, the government saves administrative costs for general welfare services to the individuals and their household members.[87]

The cost side covers all operating expenditures necessary to run a program, including technical equipment, teaching materials, and teacher reimbursement. The government also faces costs after successful integration if in-work benefits have to be paid to integrated individuals, i.e. benefits that are conditional on being employed. For the analyses presented here, there

[85] The fiscal cost-benefit analysis is the dominant approach for evaluating the efficiency of German ALMP (see ZEW et al., 2006; IZA et al., 2006; and IAB et al., 2006). It is not the prevailing approach in the international literature, in which most studies conduct social cost-benefit analyses. But these studies do not usually account for displacement effects, value placed on leisure time, and the deadweight loss of taxation (see Heckman et al., 1999). Notable exceptions with regard to deadweight loss are Raaum et al. (2002) and Jespersen et al. (2008).

[86] In this chapter, reductions in UBII payments concern the UBII base payment, social allowance, social insurance contributions, and expenses for additional needs.

[87] I do not include reductions in other activation measures that would have been implemented in the case of program failure, as do IZA et al. (2006). I assume there to be no savings because the number of program slots is generally fixed and open slots are filled with other unemployed individuals. Neither do I include indirect tax benefits, such as additional value added tax revenue on account of increases in consumption expenditures. The reason: additional revenue is likely to be insignificant. As I show in section 6.3, post-welfare wages are generally low, which limits consumption expenditures.

is only one relevant in-work benefit: the housing allowance (*Wohngeld*). The housing allowance is paid to employed persons who need financial support for their rent or for other housing expenses. It is similar to the housing support received as part of welfare but its sum is usually much less. This means that the housing allowance can be seen either as a cost or (on the benefit side) as a reduction of savings.

Costs and benefits of a program occur at different points in time. The costs accrue at the time the program starts. Benefits – increased governmental revenue and reductions in welfare expenditures – materialize monthly after program start. If a program brings about quick integration into employment, there will be an immediate positive benefit for the government. In case of locking-in effects or negative treatment effects, however, the program generates negative benefits, i.e. additional costs, insofar as participants would have done better without the program. To make benefits and costs comparable, benefits have to be discounted. I base my analyses on a discount factor of 5% per year. This implies an equivalent monthly discount factor of 0.4074%.[88]

My analyses build directly on the treatment effects estimated in chapter 5. I consider all persons participating in Temporary Extra Jobs and short-term training during the first quarter of their welfare spell. For each of the first twelve months after program start I calculate the number of additional integrations into employment caused or impeded by a program. I multiply this number with the monthly benefit of integration (calculated in section 6.3) to determine the monthly fiscal benefit generated for the entire treatment group. The discounted and accumulated benefits for all twelve months are then compared with the costs of the program (also calculated in section 6.3). This comparison gives insight into whether the benefits of the program exceed its costs and, if so, how long it takes until the program becomes profitable, i.e. in which month after program start benefits start to outweigh costs. If benefits are greater than costs, the program is defined to be efficient. The ratio of benefits to costs provides a way to measure the return per euro invested in the program. By applying this approach uniformly to all programs and subgroups of interest (men and women with and without a migration background), I devise a way to compare program efficiency between different treatments and different treatment groups. Combined with the results established in chapter 5, my cost-benefit analyses thus provide rich and valuable information for formulating policy recommendations.

[88] The discount rate is not crucial for my analyses as my observation period is only one year. My results are insensitive to alternative discount rates around 5%.

6.3 Determination of Fiscal Costs and Benefits

In this section, I describe in detail how I calculate fiscal costs and benefits of the programs for my analyses. All figures in this section and the remainder of this chapter are based on 2007 prices.

6.3.1 Fiscal Costs

To calculate the fiscal costs of Temporary Extra Jobs and the four short-term training programs, I rely on FEA statistics regarding program participants and expenditures in 2006.[89] I use the year 2006 because my sample consists of individuals who entered the welfare system in 2006 and who started a program during the first quarter of welfare receipt. Hence, for the vast majority of the sample treatment took place in 2006. I nevertheless adjust program costs to 2007 prices using the Consumer Price Index (Source: Statistisches Bundesamt, 2008).

For Temporary Extra Jobs, the FEA statistics provide the overall expenditures for the program (1,148,067,241.94 euros) and the number of participants who entered it (704,531). Dividing the one figure into the other yields an average overall program cost per participant of 1,629.55 euros. This figure depicts all direct program costs, including the amount of money paid to the organizations providing Temporary Extra Jobs and the remuneration welfare recipients receive for their additional efforts. For the sake of simplification, I assume that average program costs are identical for all treated individuals irrespective of gender and migration background.

FEA statistics for the four short-term training programs are less detailed and only report the number of persons entering each program and the overall expenditure for training in general. To calculate the average costs per program, I assume that each program has the same costs per day. I multiply the number of participants in each program with the average duration of the programs to derive weights for the breakdown of overall training expenditures. Program durations are obtained from the administrative data used in chapter 5.[90] For each program I calculate the aggregated number of participation days. For example, I observe 198,389 participants entering the aptitude test program in 2006 and an average program duration of 14.98 days.

[89] These statistics have been specifically produced by the Federal Employment Agency for the analyses presented in this thesis. Like my administrative data, they are restricted to centralized welfare agencies. Source: *Finanzsysteme der Bundesagentur für Arbeit* and *Statistik der Bundesagentur für Arbeit, TM und AGH*, Nuremberg, June 2010.

[90] As noted in chapter 5, aptitude tests have an average duration of 14.98 days, job search training of 9.96 days, skill provision of 23.66 days, and combined training programs of 22.73 days. Temporary Extra Jobs last on average for 88.06 days.

Multiplying both figures gives me a product of 2,972,648.87 days of participation. Counting all the programs, I arrive at a total of 8,084,081.28 days of participation.[91] So for 2006, aptitude tests make up 36.77% of all training program participation. I then multiply this share by total training expenditures (133,449,453 euros) to arrive at aptitude test program costs of 49,071,546.05 euros. Dividing this figure by the number of participants, I determine per participant costs of 247.35 euros. Using this method, I calculate the costs of job search training to be 164.40 euros per participant, the costs of skill provision to be 390.60 euros per participant, and the costs of combined training programs to be 375.16 euros per participant. From this we can see that training costs are much lower than costs for Temporary Extra Jobs. The lower costs result from the short program durations, economies of scale (the possibility of training many welfare recipients at the same time in one classroom), and the absence of extra remuneration for participant efforts.

Tables 6.2 to 6.6 summarize the average costs per participant for the five programs and contrast them with their fiscal benefits. The next subsection explains how benefits are calculated.

6.3.2 Fiscal Benefits

To determine the fiscal benefits of a program that leads to employment uptake, I estimate the post-welfare wage earned in the new job, as this determines social insurance contributions, income tax, and solidarity surcharge. Unfortunately, the administrative data used in chapter 5 do not include reliable information on wages. I thus use an alternative data source, the German Socio-Economic Panel (GSOEP), described in subsection 2.3.2 for the estimation. Based on the estimation result and the characteristics of program participants recorded in the administrative data, I predict the post-welfare wages of individuals who participated in Temporary Extra Jobs or short-term training. I carry out the estimations separately for male and female native Germans and for male and female immigrants.

Even though the GSOEP is relatively large – about 11,000 households and more than 20,000 persons were sampled in 2007 – a single wave does not provide a sufficient data base to analyze subgroup wages. Hence, I use three waves: from 2006 to 2008. For each year, I consider persons age 18 to 57 who state that they were welfare dependent in the previous year but not in the current year and who now work either in full-time or part-time employment and earn a positive wage. I do not know whether the reported wage was causal for the drop-out from welfare receipt, but I assume this to

[91] The total days of participation also include training programs for self-employment, which have an average duration of 14.02 days. This form of training is accounted for to determine average program costs, but it is not considered in the cost-benefit analyses, as only 4,939 persons participated in this program in 2006.

be the case in the absence of information to the contrary. For my analyses, I pool all three waves together and adjust money values to 2007 prices.[92] I end up with 511 observations in total. 221 observations are native men, 189 are native women, 60 are immigrant men, and 41 are female immigrants. Despite the pooling of three waves, these numbers are very low. This should be kept in mind when interpreting the results.

For each subgroup, I estimate the post-welfare wage of integrated former welfare recipients by means of the following equation:

$$\ln WAGE_i = \beta_0 + \beta_1 AGE_i + \beta_2 SCHOOLING_i + \beta_3 SINGLE_i + \beta_4 HOUSEHOLD_i + \beta_5 EAST_i + \beta_6 HIST_i + U_i. \quad (6.1)$$

Here, ln represents the natural logarithm (in the following: log), WAGE is the monthly gross wage of individual i, AGE depicts the age categories (18-24, 25-34 (reference), 35-49, 50-57), SCHOOLING measures the educational attainment (no school leaving certificate, secondary general school (reference), intermediate secondary school, vocational diploma (*Fachabitur*), university entrance diploma, missing), and SINGLE is a dummy variable for being unmarried. HOUSEHOLD counts the number of persons living in the household, EAST is a dummy variable for being located in East rather than in West Germany, and HIST summarizes the labor market history of the individual by means of two variables (fraction of employment in the overall labor market career and fraction of unemployment in the overall labor market career). The employment variable measures the overall working experience of the individual in full-time and part-time employment during his or her labor market career divided by the time of potential labor market participation. Accordingly, the unemployment variable reflects the accumulated unemployment experience of the individual during his or her career divided by the time of potential labor market participation. The time of potential labor market participation is defined as the age of the individual minus 18. Finally, U is an error term assumed to have zero mean conditional on the explanatory variables.

The explanatory variables of equation (6.1) are included in both the GSOEP and the administrative data.[93] Means and standard deviations of the variables are displayed in the appendix to this chapter, in table A.6.1 for my GSOEP sample, and in tables A.6.2 and A.6.3 for my administrative sample of program participants. When comparing the descriptive statistics,

[92] I base my analyses on 2007 prices because I follow individuals for the first twelve months after programs start and most of this observation period takes place in 2007. For the same reason, I use 2007 legislation to calculate social insurance contributions and taxes.

[93] There is only one slight difference between the data sets. For the GSOEP, I use the entire labor market career to generate fractions of employment and unemployment, while for the administrative data, I consider only the final two years before treatment.

it has to be kept in mind that both samples refer to different populations of interest. The administrative sample contains individuals participating in short-term training programs and Temporary Extra Jobs, while the GSOEP sample considers persons who successfully left the welfare system by taking up employment. We must also recall that the GSOEP is a nationally representative data set, while the administrative data used here and in chapter 5 is representative only for the inflow of immigrants into the centralized part of the welfare system in 2006 (see subsection 2.3.2). It is thus not surprising that the samples differ to some degree. The most striking difference relates to the regional distribution of observations. While in the administrative data almost all individuals are located in West Germany, in the GSOEP it is only true for the immigrant subsample. The native German observations originate from the Eastern part of Germany with a probability of nearly 50%. There is also a notable difference in the age structure of both samples. The GSOEP sample is older on average and contains fewer people under 35 than the administrative sample. These differences are not of concern for my analyses, however. My approach in this chapter is to estimate an equation for post-welfare wages based on a representative sample of integrated former welfare recipients and then to use the estimated equation to predict wages for the subgroup of interest (participants in short-term training and Temporary Extra Jobs who leave the welfare system as a result of participation).

The log monthly gross wage reported in the GSOEP sample has a mean value of 7.2479. It varies from 6.9950 for immigrant women to 7.4191 for native men. For those observations in the sample that report monthly working hours (498 out of 511), I calculate a median hourly wage rate of 8.50 euros. The 10th percentile of the hourly wage distribution amounts to 5.43 euros and the 90th percentile to 14.23 euros. Employment uptake after welfare receipt thus mostly concentrates on the low pay sector. This finding is confirmed by Achatz and Trappmann (2009), who report a median post-welfare wage rate of 7.76 euros on the basis of the so-called Panel Study "Labour Market and Social Security" (PASS, in German: *Panel "Arbeitsmarkt und Soziale Sicherung"*).

I estimate equation (6.1) using Ordinary Least Squares (OLS). My estimation results are summarized in table A.6.4 in the appendix to this chapter. As the table shows, post-welfare wages are pre-dominantly influenced by the labor market history of individuals. The higher the fraction of overall working experience in full- and part-time employment is, the larger the (log) monthly gross wage after integration into employment. By contrast, a large fraction of unemployment during the previous labor market career tends to lower post-welfare wages. Surprisingly, age and schooling play no influential role. Only for native women does a university entrance diploma significantly raise wages over those associated with secondary general school degrees. While single native men earn less than their married counterparts, the opposite is true for native women. This could be due to single mothers with small children who require more financial resources than married women. It should be recalled,

however, that I look only at persons who take up employment and earn sufficiently to overcome welfare dependency. For this reason, post-welfare wages might tend to rise with household size. In East Germany, post-welfare wages are lower for native men than in West Germany. There is no significant difference between both regions for the other subgroups.

Using the estimated coefficients of the wage equation and the characteristics of program participants in the administrative data, I predict the expected post-welfare wages for participants in the programs of interest. For example, to predict the expected wage of former male native participants in aptitude tests, I consider the estimated coefficients for male natives and plug in the average characteristics of male native participants in aptitude tests. I then transform the predicted log wage to its level form. For female immigrants in the same program, I use the estimated coefficients for women with a migration background, multiply them by the average characteristics of female immigrant participants in aptitude tests as provided by the administrative data and then transform the log wage into the corresponding level. I predict the wage for single and married participants separately by setting the dummy variable *SINGLE* either to 1 or 0. This distinction between single and married individuals is necessary because tax payments depend on marital status. I estimate post-welfare wages for 40 different groups of individuals (five programs, men and women, native Germans and immigrants, single and married persons). Tables 6.2 to 6.6 display the predicted log wages, their standard errors, and their level form. Deviations between the log and level form of wages are due to rounding errors. The standard errors take into account out-of sample predictions, i.e. the estimation of the wage equation and the prediction of post-welfare wages use two different data samples. To allow for a clear representation of the analyses, I focus on the point estimate of the prediction only and disregard prediction uncertainty.[94] I do, however, take uncertainty into account by incorporating the standard errors I estimated in chapter 5 for the Average Treatment Effects on the Treated (ATT).

As can be seen from tables 6.2 to 6.6, gross monthly post-welfare wages in level form vary from 756.37 euros for married native women who participated in Temporary Extra Jobs to 1,678.72 euros for married native men who took part in job search training. These predicted post-welfare wages provide the basis for calculating social insurance contributions, income tax, and solidarity surcharge. I first consider social insurance contributions, which amounted to 40.55% in 2007 (see OECD, 2008). They comprise employer and employee contributions to unemployment, old age, health and long-term care insurance. In 2007, employees had to pay 2.25% of gross wages to unemployment insurance, 9.95% to old age insurance, 7.55% to health insurance, and 1.1% to

[94] The lower bound of the 95% confidence interval around the point estimate of the predicted wage represents a wage far below 1,000 euros in most cases. Consequently, government benefits materializing from this wage are low to zero. At the upper bound of the confidence interval, benefits are in some cases considerably larger than the ones presented here based on the point estimate.

long-term care insurance.[95] The employer contributed 2.25% of gross wages to unemployment insurance, 9.95% to old age insurance, 6.65% to health insurance, and 0.85% to long-term care insurance. In accordance with the estimated wages, social insurance contributions vary from 306.71 euros for married native women in Temporary Extra Jobs to 680.72 euros for married native men in job search training (see tables 6.2 to 6.6).

Table 6.2: Fiscal costs and benefits of aptitude tests

	Natives				Immigrants			
	Men		Women		Men		Women	
	Singles	Married	Singles	Married	Singles	Married	Singles	Married
Fraction of singles and married	0.673	0.327	0.552	0.448	0.422	0.578	0.372	0.628
Predicted post-welfare wages (in euros per month)								
Predicted log wage	7.1555	7.3135	6.9330	6.7408	7.1796	7.2721	6.7914	6.9945
Standard error	(0.3836)	(0.3875)	(0.4574)	(0.4600)	(0.3428)	(0.3423)	(0.6070)	(0.5901)
Predicted level wage	1,281.29	1,500.45	1,025.59	846.34	1,312.38	1,439.57	890.10	1,090.55
Benefits (in euros per month)								
Social insurance contributions	519.56	608.43	415.88	343.19	532.17	583.75	360.94	442.22
Income tax	65.08	0.00	15.83	0.00	72.58	0.00	0.00	0.00
Solidarity surcharge	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Reductions in UBII payments	454.39	505.59	456.03	467.83	498.38	652.52	576.03	647.46
Reductions in housing costs	280.32	281.31	327.51	278.63	354.56	458.49	436.30	507.89
Housing allowance	-1.25	-10.17	-9.45	-8.43	-7.27	-20.28	-0.00	-17.79
Reductions in administrative costs	77.93	77.93	77.93	77.93	77.93	77.93	77.93	77.93
Total benefits	1,417.97		1,227.92		1,657.86		1,580.88	
Costs (in euros per participant)								
Direct program costs	247.35							

Remarks: All monetary values are measured in 2007 prices. Deviations between the log and level form of wages are due to rounding errors. The standard errors take into account that two different data samples are used for the estimation and prediction of post-welfare wages. Reductions in UBII payments concern the UBII base payment, social allowance, social insurance contributions, and expenses for additional needs.

With knowledge of the predicted wages and the social insurance contributions, the income tax can then be computed. For the computation, I use the program ElsterFormular 2007 the official software of the German tax authorities (*Finanzverwaltung von Bund und Ländern*) for the online submission of tax returns. This software computes the personal tax burden as it is done in the tax offices (*Finanzämter*). As the tax burden depends on marital status, I distinguish between single and married individuals. I assume for both groups that no children are present in the household and that the earned wage is the only source of income, i.e. that there is no capital income. While the latter assumption is justified by the fact that I consider former welfare recipients

[95] For contributions to long-term care insurance, I assume that individuals have no children. Contributions for persons with children amounted only to 0.85%.

who – based on the eligibility criteria for welfare benefits – were unable to live on their own earnings and savings before entering employment, the former assumption is made merely to simplify the calculations (in reality there are many children living on welfare benefits; see section 2.2).

Bringing up children will lower the tax burden because of tax allowances for dependent children (*Kinderfreibetrag*).[96] Consequently, I overestimate the tax burden, but the resulting bias is minimal. As has been noted above, wages of integrated individuals are rather low, varying between 756.37 euros and 1,678.72 euros per month. Hence, tax burdens are low.[97] For married individuals, I calculate an actual tax burden of zero for all programs. I assume that married persons are assessed jointly by the so-called splitting method (*Ehegattensplitting*). In this case, the earnings of both partners are summed up to form a joint taxable income. Then, the income tax is calculated with respect to one half of this joint income. The resulting tax burden is doubled to obtain the income tax liability of the married couple (see OECD, 2008). Since the German income tax is progressive, the splitting method is attractive for married individuals to reduce the tax burden. If only one partner earns an income and if the income is low, the tax burden imposed by the splitting method will usually be low or, as in my analyses, zero. For singles, no splitting method is available. The tax burden is in general positive. Since I do not account for single individuals with children who could claim tax credits for their children, I overestimate the tax payments of singles. Yet the upward bias is limited by low wages and low tax payments for singles. The maximum tax burden amounts to 106.42 euros per month for single native men participating in job search training. Single immigrant women who take part in aptitude tests, skill provision, or Temporary Extra Jobs earn post-welfare wages too low to be subject to taxation (see tables 6.2 to 6.6).

On top of income tax, the so-called solidarity surcharge (*Solidaritätszuschlag)* is levied if the annual income tax liability is larger than 972 euros for singles or larger than 1,944 euros for married individuals (see OECD, 2008). Above these exemption limits, the solidarity surcharge increases gradually until it reaches a maximum amount of 5.5% for an annual income tax liability of 1,340.69 euros and above for singles or of 2,681.38 euros and above for married persons. Since tax liabilities in my analyses are quite low, solidarity surcharge is only levied for three subgroups: single native and immigrant men in job search training and single immigrant men participating in skill provision. The maximum amount of solidarity surcharge is 5.08 euros per month

[96] In 2007, the tax allowance per child amounted to 1,824 euros, including an additional allowance of 1,080 euros for educational expenditures (*Freibetrag für den Betreuungs- und Erziehungs- oder Ausbildungsbedarf*; see OECD, 2008).

[97] The German income tax system is designed progressively and includes a personal allowance (*Grundfreibetrag*). In 2007, the personal allowance amounted to 7,664 euros. For yearly incomes up to this amount, no income tax must be paid. For an income slightly above 7,664 euros, a tax rate of 15% is applied. The tax rate increases progressively up to 45% for incomes of 250,001 euros or more (see OECD, 2008).

Table 6.3: Fiscal costs and benefits of job search training

	Natives				Immigrants			
	Men		Women		Men		Women	
	Singles	Married	Singles	Married	Singles	Married	Singles	Married
Fraction of singles and married	0.686	0.314	0.563	0.437	0.400	0.600	0.322	0.678
Predicted post-welfare wages (in euros per month)								
Predicted log wage	7.2678	7.4257	6.9638	6.7716	7.2232	7.3157	6.9761	7.1793
Standard error	(0.3826)	(0.3856)	(0.4568)	(0.4603)	(0.3432)	(0.3414)	(0.5938)	(0.6108)
Predicted level wage	1,433.52	1,678.72	1,057.68	872.82	1,370.89	1,503.74	1,070.70	1,311.81
Benefits (in euros per month)								
Social insurance contributions	581.29	680.72	428.89	353.93	555.89	609.77	434.17	531.94
Income tax	106.42	0.00	21.25	0.00	88.75	0.00	23.58	0.00
Solidarity surcharge	5.08	0.00	0.00	0.00	1.55	0.00	0.00	0.00
Reductions in UBII payments	454.39	505.59	456.03	467.83	498.38	652.52	576.03	647.46
Reductions in housing costs	280.32	281.31	327.51	278.63	354.56	458.49	436.30	507.89
Housing allowance	-1.25	-10.17	-9.45	-8.43	-7.27	-20.28	-0.00	-17.79
Reductions in administrative costs	77.93	77.93	77.93	77.93	77.93	77.93	77.93	77.93
Total benefits	1,513.98		1,244.35		1,694.98		1,683.22	
Costs (in euros per participant)								
Direct program costs	164.40							

Remarks: All monetary values are measured in 2007 prices. Deviations between the log and level form of wages are due to rounding errors. The standard errors take into account that two different data samples are used for the estimation and prediction of post-welfare wages. Reductions in UBII payments concern the UBII base payment, social allowance, social insurance contributions, and expenses for additional needs.

for single native men who participate in job search training (see tables 6.2 to 6.6).

Besides the increased revenue due to social insurance contributions, income tax, and solidarity surcharge, the government benefits from successful integration of welfare recipients into self-sufficient employment through reductions in UBII payments and in housing and administrative costs. Instead of estimating the savings of UBII payments and housing costs, I rely on the average values for these figures reported by the individuals in my GSOEP sample. In the overall sample, individuals state that they and their household members received on average 502.03 euros of UBII payments per month in the year before their transition to employment. Housing costs, which are assumed to be identical with the reported rent in the year before employment uptake, amounted to an average of 320.60 euros per month; the total sum of monthly payments to welfare recipients amounted to 822.63 euros. This is nearly identical with the monthly value of 818 euros indicated in table 2.6 (see section 2.2), which is based on FEA statistics for average governmental payments to households on welfare in 2007.

To increase the precision of my analyses, I do not base my calculations on these sample averages but on the average of subgroups defined by migration background, gender, and marital status. Subgroup averages for

Table 6.4: Fiscal costs and benefits of skill provision

	Natives				Immigrants			
	Men		Women		Men		Women	
	Singles	Married	Singles	Married	Singles	Married	Singles	Married
Fraction of singles and married	0.635	0.365	0.584	0.416	0.360	0.640	0.286	0.714
Predicted post-welfare wages (in euros per month)								
Predicted log wage	7.1768	7.3347	6.9335	6.7413	7.2044	7.2969	6.7931	6.9963
Standard error	(0.3834)	(0.3870)	(0.4588)	(0.4605)	(0.3439)	(0.3406)	(0.6149)	(0.5917)
Predicted level wage	1,308.86	1,532.74	1,026.09	846.75	1,345.42	1,475.81	891.65	1,092.44
Benefits (in euros per month)								
Social insurance contributions	530.74	621.52	416.08	343.36	545.57	598.44	361.56	442.98
Income tax	71.67	0.00	15.92	0.00	81.67	0.00	0.00	0.00
Solidarity surcharge	0.00	0.00	0.00	0.00	0.13	0.00	0.00	0.00
Reductions in UBII payments	454.39	505.59	456.03	467.83	498.38	652.52	576.03	647.46
Reductions in housing costs	280.32	281.31	327.51	278.63	354.56	458.49	436.30	507.89
Housing allowance	-1.25	-10.17	-9.45	-8.43	-7.27	-20.28	-0.00	-17.79
Reductions in administrative costs	77.93	77.93	77.93	77.93	77.93	77.93	77.93	77.93
Total benefits	1,436.57		1,232.14		1,689.30		1,599.37	
Costs (in euros per participant)								
Direct program costs	390.60							

Remarks: All monetary values are measured in 2007 prices. Deviations between the log and level form of wages are due to rounding errors. The standard errors take into account that two different data samples are used for the estimation and prediction of post-welfare wages. Reductions in UBII payments concern the UBII base payment, social allowance, social insurance contributions, and expenses for additional needs.

monthly UBII payments vary between 454.39 euros for single native men and 652.52 euros for married immigrant men. For housing costs, subgroup averages vary between 278.63 euros per month for native married women and 507.89 euros per month for immigrant married women. Reductions in housing costs are offset by the housing allowance, which is paid as an in-work benefit to employed persons. The housing allowance after employment integration amounts to an average of 9.11 euros per month, ranging from single immigrant females, who report no housing allowance, to married immigrant men, who claim an average of 20.28 euros per month (see tables 6.2 to 6.6).

To calculate the saved administrative costs after an integration into employment, I use statistics specifically provided by the FEA.[98] I base my calculation of saved administrative costs on 2006, the year in which the costs accrued for the individuals in my sample (see also subsection 6.3.1). In 2006, the FEA spent 3.211 billion euros for administrative purposes. On average, there were 3.434 million households on welfare, indicating administrative costs per household of 77.93 euros per month. I assume that these administrative costs

[98] Source: *Statistik der Bundesagentur für Arbeit, Bedarfsgemeinschaften (Bestand), Januar 2006 bis Dezember 2007*, Nuremberg, June 2010.

Table 6.5: Fiscal costs and benefits of combined training programs

	Natives				Immigrants			
	Men		Women		Men		Women	
	Singles	Married	Singles	Married	Singles	Married	Singles	Married
Fraction of singles and married	0.698	0.302	0.565	0.435	0.394	0.606	0.427	0.573
Predicted post-welfare wages (in euros per month)								
Predicted log wage	7.1747	7.3326	6.8964	6.7043	7.1665	7.2590	6.8423	7.0454
Standard error	(0.3840)	(0.3880)	(0.4580)	(0.4614)	(0.3428)	(0.3420)	(0.6002)	(0.6089)
Predicted level wage	1,306.05	1,529.45	988.78	815.96	1,295.32	1,420.85	936.53	1,147.43
Benefits (in euros per month)								
Social insurance contributions	529.60	620.19	400.95	330.87	525.25	576.15	379.76	465.28
Income tax	70.92	0.00	10.00	0.00	68.17	0.00	3.42	0.00
Solidarity surcharge	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Reductions in UBII payments	454.39	505.59	456.03	467.83	498.38	652.52	576.03	647.46
Reductions in housing costs	280.32	281.31	327.51	278.63	354.56	458.49	436.30	507.89
Housing allowance	-1.25	-10.17	-9.45	-8.43	-7.27	-20.28	-0.00	-17.79
Reductions in administrative costs	77.93	77.93	77.93	77.93	77.93	77.93	77.93	77.93
Total benefits	1,430.92		1,212.45		1,655.07		1,592.24	
Costs (in euros per participant)								
Direct program costs	375.16							

Remarks: All monetary values are measured in 2007 prices. Deviations between the log and level form of wages are due to rounding errors. The standard errors take into account that two different data samples are used for the estimation and prediction of post-welfare wages. Reductions in UBII payments concern the UBII base payment, social allowance, social insurance contributions, and expenses for additional needs.

apply equally to all observations in my sample irrespective of gender, migration background, or marital status (see tables 6.2 to 6.6).

Finally, the sum of social insurance contributions, income tax, solidarity surcharge, reductions in UBII payments, reductions in housing costs after housing allowance, and reductions in administrative costs determines the total fiscal benefits associated with a successful integration of a welfare recipient into employment. I calculate the monthly total benefit for each program separately for natives and immigrants and for men and women. I weight single and married individuals by their share among all treated persons in a given program (see tables A.6.2 and A.6.3 in the appendix to this chapter).[99] The calculated monthly total benefit varies between 1,181.83 euros for native women participating in Temporary Extra Jobs and 1,694.98 euros for immigrant men in job search training (see tables 6.2 to 6.6). The government thus receives a substantial gain when individuals are integrated into employment.

[99] I do not consider the total fiscal benefit of a program separately for single and married individuals, as the treatment effects of program participation have not been estimated separately. The number of observations in the subsamples of singles and married individuals would have been too small for the estimation of valid treatment effects.

Table 6.6: Fiscal costs and benefits of Temporary Extra Jobs

	Natives				Immigrants			
	Men		Women		Men		Women	
	Singles	Married	Singles	Married	Singles	Married	Singles	Married
Fraction of singles and married	0.736	0.264	0.580	0.420	0.537	0.463	0.395	0.605
Predicted post-welfare wages (in euros per month)								
Predicted log wage	7.1104	7.2683	6.8206	6.6285	7.0222	7.1146	6.7270	6.9301
Standard error	(0.3863)	(0.3916)	(0.4595)	(0.4634)	(0.3437)	(0.3501)	(0.6121)	(0.6121)
Predicted level wage	1,224.75	1,434.24	916.56	756.37	1,121.24	1,229.90	834.60	1,022.55
Benefits (in euros per month)								
Social insurance contributions	496.64	581.58	371.67	306.71	454.66	498.72	338.43	414.64
Income tax	53.08	0.00	1.00	0.00	32.67	0.00	0.00	0.00
Solidarity surcharge	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Reductions in UBII payments	454.39	505.59	456.03	467.83	498.38	652.52	576.03	647.46
Reductions in housing costs	280.32	281.31	327.51	278.63	354.56	458.49	436.30	507.89
Housing allowance	-1.25	-10.17	-9.45	-8.43	-7.27	-20.28	-0.00	-17.79
Reductions in administrative costs	77.93	77.93	77.93	77.93	77.93	77.93	77.93	77.93
Total benefits	1,380.95		1,181.83		1,529.68		1,550.56	
Costs (in euros per participant)								
Direct program costs	1,629.55							

Remarks: All monetary values are measured in 2007 prices. Deviations between the log and level form of wages are due to rounding errors. The standard errors take into account that two different data samples are used for the estimation and prediction of post-welfare wages. Reductions in UBII payments concern the UBII base payment, social allowance, social insurance contributions, and expenses for additional needs.

The question that remains is whether this gain is achieved efficiently. In the next section I compare costs and benefits to provide an answer.

6.4 Comparison of Fiscal Costs and Benefits

In comparing fiscal costs and benefits of programs, I analyze those individuals in chapter 5 who were treated in the first quarter of their welfare spell. My results, therefore, are valid only for this group and do not allow conclusions on the efficiency of programs assigned in later quarters of welfare receipt. I start my cost-benefit comparisons with the four different short-term training programs and then turn to Temporary Extra Jobs. For all programs, we must remember that benefits accrue only for individuals who leave the welfare system due to program participation, whereas program costs have to be considered for all participants.

For aptitude tests, I observe 507 treated native men. Given the costs of an aptitude test of 247.35 euros per participant, total spending for native men in aptitude tests was 125,406.45 euros. I then compare program costs with

program benefits. Benefits accrue monthly after program start and must be discounted and accumulated. The discounting and accumulation of benefits from aptitude tests for native male participants is shown in table A.6.5 in the appendix to this chapter.

The basis for table A.6.5 is the program effects from chapter 5 for the first twelve months after program start. As can be seen from the table, aptitude tests increase the probability of a native male participant's being employed one month after program start by 5.3191 percentage points over non-participation. This means 26.96 additional employment integrations among treated individuals ($= 507 \cdot 0.053191$). The 26.96 jobs generate a benefit of 38,239.58 euros ($= 26.96 \cdot 1,417.97$), with 1,417.97 euros being the monthly benefit from an aptitude test for an integrated native male participant (see table 6.2). This value has to be discounted for one month to make it comparable with the costs of the program. Applying a monthly discount factor of 0.4074%, equivalent to an annual discount factor of 5%, I calculate a discounted benefit of 38,084.42 euros for the first month after program start.

Carrying out the same calculation for the remaining eleven months and accumulating all discounted benefits allows me to contrast the dynamic evolution of discounted and accumulated benefits with the costs of the program. This is visualized in figure A.6.1 in the appendix to this chapter. Here the black solid line displays the evolution of discounted and accumulated benefits over time, and the grey line graphs the costs. Note that program costs accrue at the time the program is started. The cost curve is drawn over the entire observation period only to facilitate the comparison of costs and benefits. The intersection between cost and benefit curve indicates the point at which costs and benefits are equal.

For male native participants, costs and benefits level each other after three months. Thereafter, benefits start to outweigh program costs. But the figure of three months is based purely on the point estimate of the treatment effect. Taking into account estimation uncertainty, I repeat my calculation for the upper and lower bound of the 95% confidence interval around the point estimate. The resulting curves are indicated in figure A.6.1 by the dotted lines. The upper bound of the confidence interval implies larger treatment effects and an even more rapid pay-off of the program. The lower bound is associated with smaller treatment effects and leads to a later intersection of costs and benefits (six months after program start). I thus conclude that, for aptitude tests targeted at native men, benefits significantly outweigh costs after half a year, i.e. it takes six months for the program to be efficient. Thereafter, benefits tend to rise substantially. After twelve months, the benefits add up to around 660,000 euros, based on the point estimate. The ratio of (discounted and accumulated) benefits to costs (benefit-cost ratio) amounts to 5.24. This means that one year after program start discounted and accumulated benefits are more than five times larger than program costs. In other words, the program generates 5.24 euros of benefits for every euro invested.

For all other subgroups and programs, I carry out the same calculations. My results are summarized in tables A.6.6 to A.6.24 and in figures A.6.2 to A.6.20 in the appendix to this chapter. Aptitude tests targeted at immigrant men show a picture similar to native men, but efficiency of the program is achieved somewhat later, and the surplus is more modest (see table A.6.6 and figure A.6.2). While the point estimate of the discounted and accumulated benefit curve intersects with the cost curve after three months as in the case of native men, it takes ten months before the entire 95% confidence interval exceeds the cost curve. One year after program start, discounted and accumulated benefits amount to about 480,000 euros, based on the point estimate. The benefit-cost ratio is 4.33, nearly 20% lower than the ratio for native men.

For native and immigrant women, I observe a nearly identical benefit-cost ratio (3.92 and 3.90, respectively) and a similar intersection date of the point estimate with the cost curve (five and four months after program start, respectively; see tables A.6.7 and A.6.8 and figures A.6.3 and A.6.4 in the appendix to this chapter). For native women, benefits significantly outweigh costs after eleven months. But for female immigrants, the 95% confidence interval of benefits overlaps with the cost curve over the entire observation period. So in the case of female immigrants and the 5% significance level, I reject the hypothesis that benefits outweigh costs at the end of the observation period.

A similar conclusion must be drawn for job search training irrespective of gender and migration background of participants (see tables A.6.9 to A.6.12 and figures A.6.5 to A.6.8 in the appendix to this chapter). For all four subgroups, the lower bound of the confidence interval does not exceed the cost curve in the first twelve months after program start. Nevertheless, the point estimate of benefits tends to rise over time for male participants and for native German women. For the latter subgroup, the point estimate intersects with the cost curve after four months, and discounted and accumulated benefits are 5.25 times larger than program costs one year after program start. For male native (male immigrant) participants, I observe an intersection of the point estimate and the cost curve after ten (nine) months and a benefit-cost ratio of 2.01 (2.97). This implies that the program generates a larger benefit per invested euro when targeted at male immigrant participants than when targeted at male natives. Due to negative treatment effects for immigrant women in the first eight months after program start, I observe negative discounted and accumulated benefits for this subgroup over the entire observation period. Even though the benefit curve starts to rise slightly in the ninth month after program start, the benefit-cost ratio still amounts to -1.87 at the end of the observation period.

At 4.71, the benefit-cost ratio looks better for immigrant women who participate in skill provision: benefits significantly outweigh costs after nine months, and the total sum of discounted and accumulated benefits amounts to 244,630 euros, based on the point estimate of benefits one year after pro-

gram start. For native German women in the same program, the benefit-cost ratio amounts only to 1.96 and benefits do not significantly exceed costs. The same is true for immigrant men, for whom the lower bound of the confidence interval is negative. In the case of male native German participants, benefits exceed costs significantly after eleven months and amount to 386,160 euros at the end of the observation period, with a benefit-cost ratio of 3.43. See tables A.6.13 to A.6.16 and figures A.6.9 to A.6.12 in the appendix to this chapter for more details on the efficiency of skill provision.

Combined training programs were found to have only small, mostly insignificant effects on employment chances. Consequently, discounted and accumulated benefits are relatively low (see tables A.6.17 to A.6.20 and figures A.6.13 to A.6.16 in the appendix to this chapter). For native and immigrant men and for female immigrants, the point estimate of benefits does not intersect with the cost curve until twelve months after program start, which means it takes one year for the program to pay off. This is also evident from the benefit-cost ratio, which amounts to 1.02 for native men, to 1.07 for immigrant men, and to 1.19 for immigrant women. Yet after factoring in uncertainty and the entire 95% confidence interval, I have to reject the hypothesis that benefits are significantly larger than costs at the end of the observation period. For female native Germans who participate in combined training programs, the point estimate of discounted and accumulated benefits is lower than program costs during the entire observation period. One year after program start, the benefit-cost ratio amounts to 0.72. Within one year, the benefits of the program recover only 72% of program costs, which total 70,530 euros for this subgroup.

The results for Temporary Extra Jobs are even worse (see tables A.6.21 to A.6.24 and figures A.6.17 to A.6.20 in the appendix to this chapter). There are two adverse factors at work: the program is both very costly and counterproductive. The estimated negative treatment effects project negative benefits for all subgroups over the entire observation period, opening up a substantial gap between costs and benefits. The benefit-cost ratio varies from -0.30 for native men to -0.08 for immigrant females. The overall sum of discounted and accumulated benefits twelve months after program start for all subgroups amounts to -711,818.01 euros, based on the point estimate (see table A.6.25 in the appendix to this chapter). It equals -1,523,975.45 euros at the lower bound of the 95% confidence interval and -167,863.43 euros at the upper bound. In combination with total program costs of 3,026,074.35 euros, these numbers indicate a significant loss for the government.

The loss generated by Temporary Extra Jobs is of such magnitude that it outweighs the surplus generated by the short-term training programs. Considering only the training programs and summing up the point estimates of discounted and accumulated benefits twelve months after program start for all subgroups, the total fiscal benefit amounts to 3,388,763.30 euros (see table A.6.25 in the appendix to this chapter). The program costs add up to 1,251,196.48 euros, yielding a surplus of 2,137,566.82 euros. The 95%

confidence interval for this value has bounds of -1,161,935.55 euros and 5,437,069.20 euros. When adding Temporary Extra Jobs to this calculation, the sum of benefits amounts to 2,676,945.29 euros, whereas costs increase to 4,277,270.83 euros. This implies a loss of -1,600,325.54 euros. The bounds of the 95% confidence interval for this value are -5,711,985.35 euros and 2,243,131.42 euros. Even though this confidence interval overlaps with zero, which suggests an equal costs and benefits, the inclusion of Temporary Extra Jobs into the calculation makes a loss for the government more likely. As a whole, the five programs run a substantial risk of being inefficient, i.e. program costs are likely to outweigh benefits.

The further development of government loss or surplus beyond the end of my observation period cannot be determined on the basis of my data. Benefits of training programs tend to rise due to the positive trends from aptitude tests and skill provision. Temporary Extra Jobs, however, seem to have persistently negative effects. A significant future surplus for all programs taken together is thus unlikely.

6.5 Summary

In this chapter, I supplemented my evaluation of short-term training programs and Temporary Extra Jobs with cost-benefit analyses determining program efficiency. To formulate policy recommendations on the use of programs, both effectiveness and efficiency must be taken into account. I focus my analyses on fiscal costs and benefits. On the benefit side, I consider increased governmental revenue and savings induced by the integration of welfare recipients into employment through program participation. The increased revenue comprises social insurance contributions, income tax, and solidarity surcharge. Savings can be realized for UBII payments, housing expenditures, and administrative costs. On the cost side, I consider all direct costs necessary to operate a program. Benefits are calculated monthly, then discounted, accumulated, and compared with program costs.

My results show that aptitude tests pay off relatively quickly usually three to five months after program start when evaluated at the point estimate of treatment effects. Benefits significantly outweigh program costs at the end of my observation period. The only exception is the subgroup of immigrant women. Nevertheless, for this subgroup, there are clear indications that programs are efficient, as discounted and accumulated benefits increase gradually over time.

Job search training has a pay-off period of eight to nine months for male participants when evaluated at the point estimate. Benefits are not significantly larger than program costs one year after program start however. The same is true for female participants. While for native German women benefits tend to outweigh costs within four months after program start, the benefits

for women with a migration background tend to be negative due to the adverse program effects within this subgroup.

By contrast, the use of skill provision for female immigrants is efficient. Benefits significantly outweigh costs eight months after program start. For the other subgroups participating in this program, benefits increase gradually. Only in the subgroup of native German men do I determine a significant fiscal gain for the government, which materializes after eleven months.

Combined training programs have a relatively long pay-off period – twelve months – for men and female immigrants when evaluated at the point estimate. In no subgroup do benefits significantly outweigh costs one year after program start. In the case of female native participants, benefits even tend to be lower than program costs over the entire observation period.

Temporary Extra Jobs have high costs and exhibit negative treatment effects and benefits in all subgroups, generating a substantial loss for the government. This loss outweighs the gains from short-term training programs. The overall loss in using all five considered programs is, however, not statistically significant.

Based on my findings on program effectiveness and efficiency, I have to conclude that Temporary Extra Jobs should be eliminated. The financial resources devoted to this program could be better invested in aptitude tests and skill provision. Combined training programs are generally ineffective. A significantly positive treatment effect can only be established for male immigrants from Eastern Europe. Women with an Eastern European migration background tend to benefit from the program as well. Since the program costs per participant are relatively high, the overall use of the program should be reconsidered and the targeting of the program to individuals who benefit most from it should be improved. The same is true for job search training. This program is counterproductive and inefficient for immigrant women and should not be used for this subgroup. Female immigrants do benefit from skill provision, however. A more frequent use of this program might help increase their low outflow rate from welfare to employment.

To the best of my knowledge, this study is the first one to evaluate the efficiency of ALMP targeted at welfare recipients in Germany. It applies the same methodology to measure costs and benefits for five different programs and for four subgroups of participants. It provides 20 easily comparable cost-benefit analyses to counteract the general lack of systematic efficiency analyses for German ALMP. Still my analyses contain shortcomings that must be avoided in future research. First, it is advisable to use only a single, large data source for estimating treatment effects and calculating their induced benefits. This would allow the estimation of post-welfare wages with greater sophistication, as this would lift the restriction to observe identical variables in different data sets. And, second, it would be an improvement for the analyses to follow individuals for more than one year, insofar as long-term effects may differ from short-term ones. To achieve these improvements, better and more comprehensive data are needed.

Chapter 7
Conclusions

In this thesis, I have evaluated two key characteristics of Germany's 2005 welfare reform. The first key characteristic is that the reform created two different organizational models for the labor market activation of welfare recipients. While in the majority of the German welfare districts a centralized organization was established, 69 districts were allowed to constitute their own decentralized welfare agencies as part of a policy experiment. Concerning this policy experiment, I analyzed whether centralized or decentralized welfare agencies are more successful at integrating welfare recipients into employment.

The second key characteristic is that the reform enforced the principle of "supporting and demanding" (*Fördern und Fordern*) in the activation of welfare recipients regardless of the organizational model and across all welfare agencies. According to this principle, welfare agencies act as service providers to help welfare recipients find a way out of welfare dependency. They support welfare recipients by counseling activities (e.g. counseling on job search strategies or counseling on individual obstacles to employment) and, in particular, by the provision of Active Labor Market Programs (ALMP). In turn, welfare recipients have to actively engage in job searches, they have to accept any reasonable job offer, and they have to participate in ALMP if assigned to them by the welfare agencies. In case of non-compliance during the activation process, welfare agencies are supposed to impose benefit sanctions, which reduce monetary payments to welfare recipients for a period of three months. To shed light on the principle of "supporting and demanding", I evaluated the employment effects of an intensified use of benefit sanctions and the effectiveness and efficiency of the most frequently assigned ALMP.

Since the primary goal of the welfare reform is to integrate welfare recipients into employment that provides a sufficient living income, I focus on the outcome variable self-sufficient employment, i.e. employment that generates a sufficient wage such that welfare benefits are no longer needed. I have placed particular emphasis on persons with a migration background. Though immigrants are highly over-represented in the German welfare sys-

tem, evidence on the effectiveness and efficiency of labor market activation of immigrant welfare recipients is scarce. This thesis tries to fill this research gap by determining which elements of activation foster labor market integration of immigrants and hence might improve their social integration.

In order to evaluate the relative performance of centralized and decentralized welfare agencies, I make use of exceptionally rich data that comprise a detailed survey of welfare recipients, administrative records from the Federal Employment Agency (FEA), regional variables that describe the local labor market, and unique information on the internal organization and activation strategies of welfare agencies. Using propensity score matching, I find that decentralized welfare agencies have a negative effect on the chances of male welfare recipients' taking up self-sufficient employment. The difference in individual employment chances between models amounts to more than three percentage points. Given the relatively low transition rate from welfare receipt into self-sufficient employment in general, the magnitude of the difference is substantial. For women, I also estimate a negative treatment effect of decentralized welfare agencies, but the effect is statistically insignificant. This means that gender differences can be observed with respect to the relative effectiveness of centralized and decentralized welfare administration. These gender differences are encountered in all subgroups of the analysis. They are found regardless of household size for single and non-single individuals and regardless of migration background for native Germans and immigrants.

Gender differences in the effectiveness of labor market activation have been found in many evaluation studies (see, for instance, Bergemann and van den Berg, 2008). In my analysis, they might be related to the fact that women are far less intensively activated than men. Regardless of agency type, women participate less frequently in ALMP. When small children are at home, they withdraw from active job search more often than fathers. Moreover, welfare agencies tend to concentrate their activation efforts on men for efficiency's sake, as they assume that men are more easily placeable than women.

To see whether the relative success of centralized agencies is due to the adoption of more successful organizational approaches that could also be used by decentralized agencies, I gathered data on the internal organization of welfare agencies. My findings show that the significantly negative effect of decentralized welfare agencies on employment chances for men is largely robust to the inclusion of further information on the organization of tasks. I thus conclude that the negative effect of decentralization is not due to the adoption of different forms of internal organization in centralized and decentralized welfare agencies. Instead, the advantages of centralized organization in bundling resources, collecting information from various sources, and imposing best-practice strategies for the local offices tend to outperform the favorable properties of decentralized organization (e.g. better information about the characteristics of the local labor market and the specific regional attributes relevant for a successful integration process).

In light of these results, the decision of the German government to increase the number of decentralized welfare agencies from 69 to 110 in January 2012 is doubtful. This decision is likely to reduce the effectiveness of the activation of welfare recipients in the agencies adopting a decentralized organization. It is also likely to decrease its efficiency. As has been reported by Deutscher Bundestag (2008, p. 151 ff.), it would be costly for the government to expand the decentralized organization, given the poor performance of decentralized welfare agencies in the early post-reform period. If all 439 welfare agencies in Germany were decentralized, annual fiscal benefits in terms of tax revenue, social insurance contributions, reductions in welfare benefits and in administrative costs would be about 3.3 billion euros lower than in a situation with centralized welfare agencies only.

To understand the second key reform characteristic, I analyzed the impact of benefit sanctions. Sanctions are the strongest measure available to welfare agencies for enforcing appropriate job search efforts by welfare recipients. If welfare recipients do not comply with their duties during the activation process, welfare agencies are legally required to impose a sanction by benefit revocation. For minor non-compliances, such as not showing up for an appointment with the caseworker, benefits have to be cut by 10%. More severe infringements like the refusal to accept a suitable job offer or to participate in ALMP receive a benefit reduction of 30%. In practice, however, there is substantial discretion at the agency level whether to impose sanctions, and benefit sanctions are not applied uniformly across welfare agencies. While some agencies are quite reluctant to use sanctions, others apply them frequently. I use differences in sanction strategies and rates across 154 welfare agencies as instrumental variables to estimate the effect of a benefit cut on the individual probability of taking up self-sufficient employment. Specifically, I estimate the effect of a sanction on the so-called compliers, i.e. on those individuals who are not sanctioned by an agency with a reserved sanction policy but who will be sanctioned if the agency decides to impose sanctions more frequently. This Local Average Treatment Effect (LATE) can be interpreted as an estimate of the effectiveness of an intensified use of sanctions.

My results show that an intensified use of sanctions is quite effective in reducing welfare dependency and enhancing employment uptake. A sanction increases the probability that compliers are employed and out of welfare six months after the benefit cut by about 58 to 68 percentage points. I find some effect heterogeneity across subgroups. First, I distinguish between individuals registered at centralized and decentralized welfare agencies. Though all sanctions exhibit strong positive employment effects, their impact tends to be larger in decentralized agencies than in centralized ones. Second, I look at gender differences and find that women tend to react more strongly to sanctions than men. Finally, I run separate estimations for native Germans and immigrants. At the end of the six-month observation period, individuals with a migration background show a more pronounced sanction effect than native Germans. Due to the small number of sanctioned individuals,

all subgroup analyses have to be interpreted with caution, yet all analyses verify a considerable sanction effect. I thus conclude that a more intensive use of legally mandated benefit cuts by welfare agencies will contribute to making the labor market activation of welfare recipients more effective and will substantially increase the outflow rate from welfare to employment. An intensified use of sanctions is also likely to be efficient, as fiscal benefits deriving from integrated individuals presumably outweigh costs for sanctions and monitoring. Monitoring costs are incurred in any case by welfare agencies even if sanctions are used less intensively and even if welfare agencies do not detect non-compliance of welfare recipients.

On the side of agency support for welfare recipients, I evaluate the most important ALMP assigned since 2005: Temporary Extra Jobs, aptitude tests, job search training, skill provision, and combined training programs. I am interested in both the effectiveness and the efficiency of programs. In particular, I investigate whether program effects differ between immigrants and native Germans and what the reasons are for these potential differences. The purpose of this investigation is to determine the differences between immigrants and native Germans in program effectiveness that are not caused by observable differences in composition but by unobserved differences between groups. In other words, I identify differences in treatment effects between natives and immigrants that result from attachment to the immigrant group. I refer to these differences as "immigrant fixed effects". For the estimation of program effects and immigrant fixed effects, I apply matching estimators that take into account the timing of treatment during the welfare spell. I carry out separate estimations for men and women, with and without a migration background. In addition, I estimate program effects and immigrant fixed effects for different subgroups of immigrants: individuals with a Turkish, an Eastern European, or a Southern European migration background.

The estimation results for the program effects and the immigrant fixed effects show considerable effect heterogeneity. Temporary Extra Jobs decrease rather than increase the chances of participants' taking up self-sufficient employment. No subgroup benefits from this program. Treatment effects are particularly negative for Temporary Extra Jobs assigned during the second quarter of the welfare spell. By contrast, aptitude tests exhibit positive employment effects for both immigrants and natives independently of gender. Job search training is ineffective for male welfare recipients. Women without a migration background benefit from participation in this program, while female immigrants are negatively affected. The differences in treatment effects between native and immigrant women are caused by an immigrant fixed effect of considerable size. During the first six months after program start, immigrant women have a treatment effect nearly 15 percentage points lower than otherwise identical women without a migration background. In the subgroups of Turkish and Eastern European women, the immigrant fixed effect is even larger and amounts to about 25 percentage points. A reversed picture can be observed for skill provision. This program exhibits positive employ-

ment effects for all subgroups when assigned early during the welfare spell. It is particularly effective for female immigrants. One year after program start, the probability that female immigrants will take up self-sufficient employment is 14 percentage points greater than that of female natives with otherwise identical characteristics. Finally, combined training programs are found to be rather ineffective. Only Eastern European immigrants tend to benefit from combinations.

To evaluate the efficiency of the five programs, I conduct fiscal cost-benefit analyses, i.e. I compare costs and benefits materializing for the government as the program provider. On the benefit side, I take into account increased governmental revenue (tax payments and social insurance contributions) and savings (reduced welfare payments and administrative costs) brought about by a transition of program participants from welfare to self-sufficient employment. And on the cost side, I consider all operating costs necessary to run a program. Benefits are calculated monthly, and then discounted, accumulated, and compared with program costs. Once again, I carry out the calculations separately for men and women with and without a migration background. In total, I present the results of 20 cost-benefit analyses. To the best of my knowledge, my study is the first to evaluate the efficiency of German ALMP targeted at welfare recipients. It counteracts the lack of systematic efficiency analyses for German ALMP in general.

The analyses reveal that aptitude tests pay off relatively quickly: around three to five months after program start when evaluated at the point estimate of treatment effects. For all subgroups except for immigrant women, fiscal benefits significantly outweigh program costs one year after program start. In the subgroup of immigrant women, there are clear indications that the program is efficient, as discounted and accumulated benefits increase gradually over time. For job search training, the results provide a somewhat more pessimistic picture. In no subgroup were benefits significantly larger than program costs at the end of the twelve-month observation period. Worse, the benefits for women with a migration background tend to be negative due to the adverse program effects within this subgroup. By contrast, the use of skill provision for female immigrants is efficient by eight months after program start. For the other subgroups participating in this program, benefits increase gradually over time. The only other subgroup for which the government experience a significant fiscal gain is the group of native German men. In this subgroup, fiscal benefits significantly outweigh program costs after eleven months. Combined training programs have a relatively long pay-off period due to their general ineffectiveness. For no subgroup do benefits significantly outweigh costs one year after program start. In the case of women without a migration background, benefits tend to be lower than program costs over the entire observation period. Temporary Extra Jobs generate a substantial loss to the government due to the program's high costs and the adverse program effects in all subgroups. This loss outweighs the fiscal gains from the short-term training programs. The overall loss of the government

arising from the use of all five considered programs, however, is statistically insignificant.

The results for effectiveness and efficiency of the programs allow me to draw a number of policy recommendations. Most importantly, Temporary Extra Jobs should not be used any longer since the program is counterproductive and inefficient irrespective of gender and migration background of participants. The recently announced government bill (*Gesetz zur Verbesserung der Eingliederungschancen am Arbeitsmarkt*) which is based inter alia on the results presented in this thesis and which proposes a reduction in the spending for Temporary Extra Jobs is a step in the right direction. The large amount of money spent for Temporary Extra Jobs could better be devoted to aptitude tests and skill provision. Both programs exhibit positive employment effects for all considered subgroups, and fiscal benefits deriving from the programs increase gradually over time. Skill provision should be assigned during the first three months after inflow into the welfare system since it is most effective during this period. It is particularly effective for female immigrants. By contrast, combined training programs are ineffective on average irrespective of the timing of treatment. Only immigrants from Eastern Europe tend to benefit from this program. Therefore, the overall use of the program should be reconsidered, and its assignment should be restricted to those individuals who benefit from it. A similar recommendation holds for job search training. This program decreases employment chances of participating female immigrants and generates a fiscal loss within this subgroup. Therefore, the program should either be redesigned for women with a migration background, or it should not be used for this subgroup any longer.

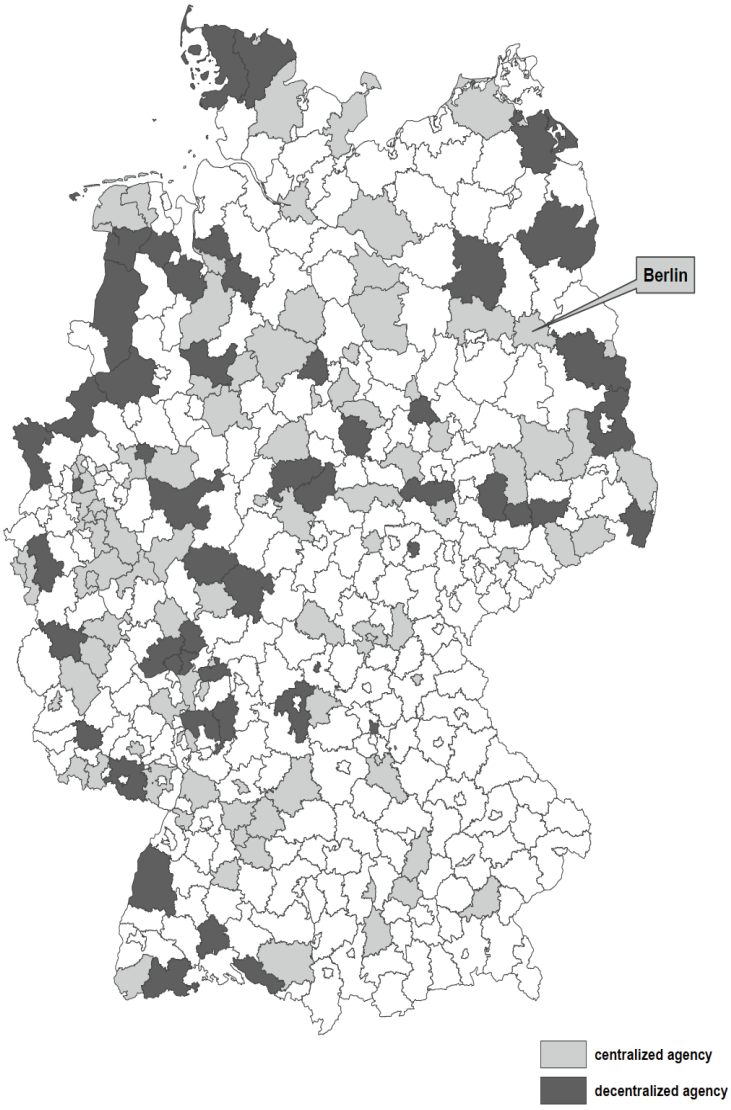
The results presented in this thesis give a valuable insight into which elements of labor market activation of welfare recipients work well and which do not. It has to be noted, though, that due to the relatively short time span between introduction of the reform and my analyses, the results represent short-run effects only. Moreover, the effects are estimated for a time period with relatively favorable macroeconomic conditions. As has been shown for example by Lechner and Wunsch (2009), the macroeconomic conditions at the start of treatment might influence the effectiveness of activation. Therefore, in the future, the presented short-run effects should be contrasted with long-run evidence and with effects estimated for time periods with less favorable macroeconomic conditions. Future research is also needed to look at other outcome variables besides self-sufficient employment. For example, considering outcome variables such as employment quality and stability as for instance measured by the amount of wages or the duration of a newly started job would allow to assess whether labor market activation of welfare recipients has a long-lasting impact on labor market integrations or whether it is only temporarily successful. Yet, to be able to analyze these outcome measures, better and more comprehensive data are needed. A promising data base for future research might be BASiD (*Biografiedaten ausgewählter Sozialversicherungsträger in Deutschland*), which will be presumably avail-

able in fall 2011 (see Hochfellner et al., 2010, for details). BASiD aims at combining administrative data of the Federal Employment Agency (FEA) with administrative data of the German Federal Pension Insurance (*Deutsche Rentenversicherung*). This combined data base might allow, on the one hand, to observe welfare recipients participating in ALMP and, on the other hand, to measure for each of these individuals post-welfare wages and job durations providing the basis for the construction of alternative outcome variables and also for the calculation of fiscal benefits to evaluate the efficiency of programs. However, irrespective of this intended combination of administrative data sources, it is indispensable that the quality of the FEA data improves. In particular, it is essential to completely harmonize the data collection process in centralized and decentralized welfare agencies.

Appendices

Appendix to Chapter 2

Fig. A.2.1: Sampled welfare agencies



Remarks: The data base comprises a sample of 154 out of 439 German welfare agencies. Of the 154 sampled agencies, 51 are decentralized (dark grey) and 103 are centralized agencies (light grey). In the city of Berlin, five different agencies are sampled, all of which are centralized. White colored agencies are not included in the sample.

Table A.2.1: Summary of data bases used in the empirical analyses

Data sources and time period covered	Available information
Data base used in chapters 3 and 4	
Individual information • Administrative data: Integrated Employment Biographies (IEB) and source files – Employee History (BeH) from January 1990 to December 2007 – Benefit Recipient History (LeH) from January 1990 to October 2008 – Welfare Recipient History (LHG) from January 2005 to October 2008 – Welfare Recipient History (XLHG) from January 2005 to October 2008 – Jobseeker History (ASU) from January 2000 to September 2008 – Jobseeker History (XASU) from January 2005 to October 2008 – Program Participation History (MTH) from January 2000 to September 2008 – Program Participation History (XMTH) from January 2005 to October 2008 • Survey data – First wave from January 2007 to April 2007 – Second wave from November 2007 to March 2008 Agency information • Case studies and management surveys providing information on the organizational structure and the strategies of welfare agencies for the year 2006 • Regional information on a monthly basis from January 1999 to December 2004	The individual part of the data base combines administrative and survey data. The administrative data provide daily information on spells of employment, unemployment, job search, benefit receipt, and participation in ALMP. This information allows the description of labor market history of individuals in detail. Additional covariates associated with the documented spells include, for example, sex, age, nationality, family status, household composition, schooling, professional qualification, profession, occupational status, industrial sector, desired form of employment, and regional information. The administrative data also allow the construction of the outcome variable self-sufficient employment. The survey data include individual characteristics (e.g. sex, age, marital and parent status, education, health and disability status), information on the members of the household (number and age of household members and interviewees' relation to them) and details concerning the labor market status, labor market history, and labor market activation (current labor market state, former spells of employment subject to social insurance contributions, former spells of minor employment, former spells of unemployment, receipt of welfare benefits, participation in ALMP). Moreover, they contain information about basic skills (e.g. reading, writing, math and computer skills), further qualifications (e.g. driver's license), job search activities, the concessions respondents would be willing to make to obtain a new job, information about benefit sanctions, and information on the migration background of interviewees and their parents. The case studies and management surveys at agency level provide information about the type of case management, the activation concept, the placement approach, the mix of ALMP and many other organizational and strategical aspects including, in particular, the use of benefit sanctions within welfare agencies. In addition, a wide range of variables reflecting local labor market conditions (e.g. unemployment ratio, GDP per employed person, rate of social assistance recipients) were collected for each agency on a monthly basis for the six years preceding the welfare reform in 2005.

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Table A.2.1: Summary of data bases used in the empirical analyses (continued)

Data sources and time period covered	Available information
Data base used in chapters 5 and 6	
Individual information from administrative data • Integrated Employment Biographies (IEB) and source files – Employee History (BeH) from January 1990 to December 2007 – Benefit Recipient History (LeH) from January 1990 to October 2008 – Welfare Recipient History (LHG) from January 2005 to October 2008 – Jobseeker History (ASU) from January 2000 to September 2008 – Program Participation History (MTH) from January 2000 to September 2008 • Verbleibsnachweise (VbN) from January 2006 to July 2008 Individual information from survey data • German Socio-Economic Panel (GSOEP) for the years 2006 to 2008	The data provide daily information on spells of employment, unemployment, job search, benefit receipt and participation in Active Labor Market Programs (ALMP). This information allows the description of the labor market history of individuals in detail. Additional covariates associated with the documented spells include, for example, sex, age, nationality, family status, household composition, schooling, professional qualification, profession, occupational status, industrial sector, desired form of employment, and regional information. The IEB data set and extended versions of the BeH (covering in addition the years 1975 to 1989) and of the ASU (covering the years 1990 to 1999 as well) are used to identify persons with a migration background. The VbN data set allows the construction of the outcome variable self-sufficient employment on a monthly basis from the sampling date until July 2008. The GSOEP is a large, nationally representative longitudinal data set that has been surveying households and individuals in Germany on a yearly basis since 1984 (see e.g. Haisken-DeNew and Frick, 2005; and Wagner et al., 2007). In this thesis, the waves from 2006 to 2008 are considered. Along with detailed sociodemographic information, the GSOEP surveys the labor market status and the monthly wage of interviewees. This self-reported information allows the estimation of post-welfare wages for former welfare recipients (see chapter 6 for details).

Remarks: All data sources are described in detail in section 2.3. The data base used in chapters 3 and 4 is confined to 154 welfare agencies, a subset of all 439 German agencies. Of the sample agencies, 103 are centralized and 51 are decentralized. The data base used in chapters 5 and 6 is confined to the 370 centralized welfare agencies in Germany.

Appendix to Chapter 3

Table A.3.1: Balancing of regional variables among the sampled welfare agencies

	Centralized agencies	Decentralized agencies	p-value
Unemployment rate	11.309	11.412	0.906
Unemployment rate of the young (age < 25)	10.628	10.505	0.860
Unemployment rate of foreigners	23.285	24.340	0.567
Ratio of caseworkers to unemployed in 2003	0.016	0.016	0.837
Ratio of placement officers with fixed-term contract to unemployed in 2003	0.002	0.002	0.895
Ratio of young (age < 25) to old (age > 50) unemployed	0.495	0.510	0.339
Ratio of long-term unemployed to all unemployed	0.332	0.333	0.896
Ratio of severely disabled unemployed to all unemployed	0.040	0.039	0.809
Rate of social assistance recipients in 2003	0.036	0.028	0.004
Unemployment-vacancy relation in textile industry	73.592	84.213	0.301
Unemployment-vacancy relation in construction sector	37.124	35.640	0.702
Unemployment-vacancy relation in engineering	16.267	17.857	0.567
Unemployment-vacancy relation in commerce sector	24.820	27.332	0.462
Unemployment-vacancy relation in service sector	20.753	24.232	0.212
Unemployment-vacancy relation in metal industry	15.261	14.610	0.661
Unemployment-vacancy relation in healthcare	6.346	6.356	0.983
Unemployment-vacancy relation in social sector	11.433	11.121	0.728
Unemployment-vacancy relation overall	30.208	32.386	0.471
FF per unemployed	0.007	0.009	0.408
FF per unemployed (men)	0.008	0.010	0.479
FF per unemployed (women)	0.006	0.008	0.337
FF per unemployed (age > 50)	0.004	0.005	0.405
FF per unemployed (age < 25)	0.014	0.019	0.253
Number of employer wage subsidies per unemployed	0.032	0.033	0.753
Number of employer wage subsidies per unemployed (age > 50)	0.062	0.065	0.763
Number of employer wage subsidies for long-term unemployed per unemployed	0.002	0.003	0.168

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Table A.3.1: Balancing of regional variables among the sampled welfare agencies (continued)

	Centralized agencies	Decentralized agencies	p-value
Number of employer wage subsidies for long-term unemployed per unemployed (men)	0.002	0.003	0.149
Number of employer wage subsidies for long-term unemployed per unemployed (women)	0.002	0.003	0.131
Number of start-up grants (*Überbrückungsgeld*) per unemployed (age > 50)	0.008	0.009	0.638
Number of start-up grants (*Überbrückungsgeld*) per unemployed (age < 25)	0.008	0.007	0.735
ABM/(unemployed+ABM)	0.017	0.019	0.430
ABM/(unemployed+ABM) (men)	0.017	0.020	0.389
ABM/(unemployed+ABM) (women)	0.016	0.018	0.488
FbW/(unemployed+FbW)	0.058	0.060	0.205
FbW/(unemployed+FbW) (men)	0.049	0.052	0.310
FbW/(unemployed+FbW) (women)	0.069	0.071	0.264
FbW/(unemployed+FbW) (age > 50)	0.014	0.015	0.360
FbW/(unemployed+FbW) (age < 25)	0.054	0.055	0.741
TM/(unemployed+TM)	0.022	0.022	0.637
TM/(unemployed+TM) (men)	0.022	0.021	0.539
TM/(unemployed+TM) (women)	0.023	0.023	0.763
TM/(unemployed+TM) (age > 50)	0.010	0.010	0.883
TM/(unemployed+TM) (age < 25)	0.036	0.035	0.828
JUMP per unemployed (age < 25)	0.121	0.136	0.209
Ratio of working population to resident population in 2003	0.465	0.424	0.075
Ratio of persons employed (subject to social insurance contributions) to resident population in 2003	0.320	0.322	0.535
Ratio of persons employed (subject to social insurance contributions) to resident population in 2003 (men)	0.357	0.361	0.450
Ratio of persons employed (subject to social insurance contributions) to resident population in 2003 (women)	0.284	0.285	0.823
Gross Domestic Product per employed person in 2003 (in thousands of euros)	51.657	51.343	0.826
Business foundations per 10,000 inhabitants age 15 to 64 in 2003	149.643	146.700	0.517
Commuter balance per 1,000 persons employed (subject to social insurance contributions) in 2003	-64.233	-172.431	0.034

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Table A.3.1: Balancing of regional variables among the sampled welfare agencies (continued)

	Centralized agencies	Decentralized agencies	p-value
Ratio of foreigners to resident population in 2003	0.084	0.065	0.032
Available infant care places per infant in 2003	0.637	0.655	0.339
Available child care places per child in 2003	0.281	0.285	0.777
Number of observations	103	51	

Remarks: All variables are measured for December 2003 or, if indicated, as the average for the entire year 2003. The depicted numbers refer to the 154 sampled welfare agencies. The p-values derive from equality-of-means tests of the displayed variables for centralized and decentralized agencies. FF denotes the number of participants in activation programs designed on the discretion of the local employment offices (*Freie Förderung*). ABM stands for the number of participants in job creation schemes (*Arbeitsbeschaffungsmaßnahmen*). FbW denotes the number of persons participating in long-term training (*Förderung der beruflichen Weiterbildung*), TM the number of persons participating in short-term training (*Trainingsmaßnahmen*), and JUMP the number of participants in a program for the activation of young unemployed persons (*Sofortprogramm der Bundesregierung zum Abbau der Jugendarbeitslosigkeit*).

Table A.3.2: Means of variables included in the propensity score specifications

	Men			Women			Data source	Specification
	Treated	Controls	p-value	Treated	Controls	p-value		
Age								
18 to 24 years	0.185	0.194	0.423	0.229	0.250	0.056	S	1
	0.185	0.186	0.922	0.231	0.231	0.980		
25 to 34 years	0.166	0.208	0.000	0.218	0.233	0.173	S	1
	0.166	0.168	0.848	0.217	0.221	0.767		
35 to 44 years	0.222	0.201	0.052	0.220	0.212	0.394	S	1
	0.221	0.219	0.872	0.220	0.216	0.749		
45 to 57 years	0.427	0.397	0.026	0.332	0.306	0.025	S	1
	0.427	0.426	0.931	0.332	0.332	0.999		
Schooling								
Secondary general school	0.465	0.500	0.008	0.421	0.449	0.023	S	1
	0.465	0.472	0.622	0.423	0.425	0.921		
Intermediate secondary school	0.303	0.264	0.002	0.386	0.350	0.003	S	1
	0.303	0.297	0.690	0.384	0.378	0.664		
University entrance diploma	0.167	0.151	0.100	0.144	0.133	0.182	S	1
	0.167	0.164	0.804	0.144	0.146	0.869		
Other or missing	0.066	0.084	0.010	0.049	0.068	0.002	S	1
	0.066	0.067	0.907	0.049	0.052	0.633		
Migration background								
Immigrant	0.244	0.260	0.174	0.248	0.258	0.379	S	1
	0.244	0.249	0.725	0.248	0.255	0.575		
Household size								
1 person	0.418	0.464	0.001	0.234	0.244	0.323	S	1
	0.418	0.422	0.816	0.233	0.236	0.771		

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Table A.3.2: Means of variables included in the propensity score specifications (continued)

	Men			Women			Data	Specifi-
	Treated	Controls	p-value	Treated	Controls	p-value	source	cation
2 persons	0.203	0.182	0.045	0.353	0.342	0.351	S	1
	0.203	0.205	0.841	0.353	0.348	0.751		
3 or more persons	0.379	0.354	0.049	0.414	0.414	0.965	S	1
	0.379	0.373	0.687	0.414	0.415	0.955		
Number of children								
No children	0.708	0.736	0.020	0.487	0.486	0.990	S	1
	0.708	0.714	0.672	0.485	0.490	0.746		
1 child	0.129	0.125	0.631	0.307	0.294	0.269	S	1
	0.129	0.130	0.925	0.308	0.304	0.775		
2 or more children	0.163	0.139	0.013	0.207	0.220	0.212	S	1
	0.163	0.156	0.542	0.207	0.206	0.941		
Obstacles to employment								
Disabled	0.162	0.127	0.000	0.076	0.067	0.169	S	1
	0.161	0.159	0.820	0.075	0.076	0.949		
Care obligation	0.024	0.020	0.229	0.043	0.042	0.748	S	1
	0.024	0.024	0.983	0.043	0.044	0.893		
Status before welfare receipt								
(Minor) employment	0.294	0.323	0.020	0.316	0.305	0.347	S	1
	0.294	0.298	0.800	0.317	0.311	0.652		
Labor market history from 2001 to 2004								
Number of half-months unemployed in 2004	12.264	12.300	0.888	9.582	8.871	0.004	A	1
	12.272	12.252	0.948	9.563	9.566	0.991		

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Table A.3.2: Means of variables included in the propensity score specifications (continued)

	Men			Women			Data source	Specification
	Treated	Controls	p-value	Treated	Controls	p-value		
Number of half-months unemployed in 2003	10.215	10.307	0.728	7.973	7.118	0.000	A	1
	10.224	10.171	0.863	7.914	7.828	0.757		
Number of half-months unemployed in 2002	8.105	8.059	0.856	6.093	5.532	0.009	A	1
	8.107	7.997	0.707	6.048	5.982	0.795		
Number of half-months unemployed in 2001	6.346	6.275	0.757	5.171	4.556	0.002	A	1
	6.346	6.193	0.564	5.122	5.000	0.607		
Number of half-months out of labor force from 2001 to 2004	17.056	19.778	0.000	23.952	28.477	0.000	A	1
	17.072	17.411	0.678	24.110	24.433	0.714		
Mean duration out of labor force from 2003 to 2004 in half-months	4.393	5.208	0.006	7.117	9.048	0.000	A	1
	4.397	4.450	0.765	7.173	7.291	0.775		
Number of programs from 2003 to 2004	0.351	0.384	0.060	0.278	0.262	0.259	A	1
	0.351	0.361	0.608	0.274	0.274	0.978		
Mean duration of programs from 2003 to 2004 in half-months	2.351	2.322	0.845	1.973	1.730	0.075	A	1
	2.354	2.366	0.946	1.917	1.941	0.883		
Current welfare spell								
Months in welfare before 10/2006	13.862	13.757	0.659	14.532	14.595	0.770	S	1
	13.861	13.747	0.678	14.539	14.485	0.829		
Start after 10/2006 or missing	0.158	0.157	0.965	0.129	0.126	0.721	S	1
	0.158	0.158	0.998	0.128	0.126	0.827		
Regional information								
Unemployment ratio (high)	0.250	0.223	0.017	0.282	0.231	0.000	R	1
	0.250	0.236	0.278	0.279	0.264	0.261		

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Table A.3.2: Means of variables included in the propensity score specifications (continued)

	Men			Women			Data	Specifi-
	Treated	Controls	p-value	Treated	Controls	p-value	source	cation
Urban district	0.167	0.369	0.000	0.163	0.371	0.000	R	1
	0.167	0.164	0.849	0.165	0.165	0.962		
Further regional variables								
GDP per employed person (high)	0.265	0.308	0.001	0.262	0.320	0.000	R	2
	0.266	0.300	0.013	0.263	0.299	0.005		
Population density (high)	0.210	0.393	0.000	0.205	0.392	0.000	R	2
	0.210	0.209	0.972	0.207	0.211	0.704		
Labor market conditions above average	0.358	0.284	0.000	0.346	0.304	0.000	R	2
	0.358	0.373	0.309	0.347	0.367	0.141		
Labor market conditions on average	0.311	0.315	0.750	0.318	0.304	0.233	R	2
	0.312	0.284	0.051	0.315	0.278	0.006		
Labor market conditions below average	0.331	0.388	0.000	0.336	0.382	0.000	R	2
	0.331	0.338	0.612	0.339	0.350	0.395		
East Germany	0.262	0.212	0.000	0.294	0.223	0.000	R	2
	0.262	0.247	0.283	0.290	0.277	0.312		
Further sociodemographic variables								
At least one child under 3 in the household	0.115	0.108	0.460	0.180	0.165	0.101	S	3
	0.115	0.113	0.889	0.180	0.182	0.900		
Lone parent status	0.023	0.019	0.300	0.295	0.299	0.752	S	3
	0.023	0.022	0.938	0.296	0.291	0.720		
Professional qualification								
None	0.229	0.272	0.000	0.272	0.327	0.000	S	3
	0.230	0.232	0.858	0.274	0.274	0.987		

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Table A.3.2: Means of variables included in the propensity score specifications (continued)

	Men			Women			Data	Specifi-
	Treated	Controls	p-value	Treated	Controls	p-value	source	cation
In-firm training	0.464	0.456	0.548	0.425	0.387	0.002	S	3
	0.465	0.465	0.986	0.423	0.423	0.998		
Off-the-job training	0.174	0.153	0.032	0.208	0.185	0.018	S	3
	0.174	0.175	0.967	0.208	0.205	0.807		
University degree	0.071	0.067	0.613	0.053	0.061	0.197	S	3
	0.071	0.069	0.837	0.054	0.056	0.783		
Other or missing	0.061	0.052	0.103	0.042	0.041	0.870	S	3
	0.061	0.059	0.839	0.042	0.042	0.891		
Self-assessment of overall state of health								
Good	0.556	0.576	0.130	0.593	0.620	0.029	S	3
	0.557	0.558	0.956	0.596	0.593	0.853		
Satisfactory	0.245	0.235	0.346	0.230	0.210	0.048	S	3
	0.245	0.241	0.778	0.228	0.227	0.969		
Poor	0.194	0.186	0.440	0.175	0.168	0.511	S	3
	0.194	0.197	0.825	0.175	0.178	0.778		
Missing	0.004	0.003	0.430	0.002	0.002	0.896	S	3
	0.004	0.005	0.935	0.002	0.002	0.996		
Impairments to health								
Gastro-intestinal diseases	0.152	0.155	0.701	0.152	0.176	0.010	S	3
	0.152	0.156	0.691	0.153	0.152	0.964		
Cardiovascular diseases	0.175	0.190	0.150	0.217	0.224	0.495	S	3
	0.175	0.178	0.856	0.217	0.218	0.925		

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Table A.3.2: Means of variables included in the propensity score specifications (continued)

	Men			Women			Data	Specifi-
	Treated	Controls	p-value	Treated	Controls	p-value	source	cation
Rheumatism and other articular trouble	0.288	0.288	0.971	0.261	0.247	0.198	S	3
	0.289	0.287	0.913	0.259	0.263	0.767		
Sleep disorders	0.230	0.244	0.222	0.260	0.280	0.062	S	3
	0.230	0.231	0.946	0.262	0.264	0.837		
Nervous disorders	0.171	0.177	0.585	0.224	0.232	0.454	S	3
	0.172	0.173	0.872	0.223	0.223	0.994		
Allergies	0.173	0.168	0.608	0.252	0.272	0.077	S	3
	0.173	0.171	0.887	0.254	0.252	0.890		
Back complaint	0.418	0.405	0.340	0.423	0.414	0.468	S	3
	0.418	0.415	0.858	0.422	0.422	0.973		
Other complaints	0.048	0.045	0.583	0.040	0.037	0.437	S	3
	0.048	0.050	0.846	0.040	0.040	0.998		
No health problems	0.282	0.289	0.550	0.273	0.257	0.158	S	3
	0.281	0.288	0.669	0.273	0.257	0.217		
Self-assessment of daily working capacity								
Less than 3 hours	0.042	0.041	0.908	0.039	0.044	0.377	S	3
	0.042	0.043	0.781	0.039	0.041	0.737		
3 to 6 hours	0.077	0.076	0.794	0.183	0.178	0.643	S	3
	0.078	0.077	0.987	0.183	0.182	0.966		
6 to 8 hours	0.131	0.124	0.437	0.235	0.223	0.232	S	3
	0.130	0.128	0.859	0.234	0.231	0.749		
8 or more hours	0.706	0.726	0.105	0.514	0.528	0.269	S	3
	0.706	0.708	0.925	0.514	0.518	0.821		

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Table A.3.2: Means of variables included in the propensity score specifications (continued)

	Men			Women			Data	Specifi-
	Treated	Controls	p-value	Treated	Controls	p-value	source	cation
Missing	0.044	0.034	0.045	0.029	0.028	0.754	S	3
	0.044	0.043	0.863	0.029	0.028	0.869		
Self-assessment of basic skills measured from 1 (= very good) to 6 (= fail); missing values are set to 3.5								
Reading and writing (in mother tongue)	2.121	2.080	0.138	1.920	1.885	0.155	S	3
	2.121	2.115	0.854	1.916	1.914	0.934		
Mathematics	2.370	2.326	0.108	2.549	2.595	0.090	S	3
	2.369	2.360	0.792	2.546	2.539	0.823		
Emails and Internet	2.993	2.984	0.845	3.113	3.079	0.415	S	3
	2.993	3.000	0.898	3.120	3.118	0.970		
Other skills								
Driver's license	0.700	0.637	0.000	0.635	0.586	0.000	S	3
	0.700	0.695	0.744	0.634	0.635	0.954		
Further information on the labor market history from 2001 to 2004								
Number of half-months employed in 2004	4.645	4.516	0.531	5.260	5.232	0.894	A	3
	4.642	4.700	0.808	5.299	5.279	0.935		
Number of half-months employed in 2003	6.488	6.105	0.108	6.353	6.463	0.630	A	3
	6.484	6.574	0.751	6.406	6.396	0.970		
Number of half-months employed in 2002	7.784	7.539	0.343	7.284	7.507	0.355	A	3
	7.785	7.897	0.714	7.299	7.324	0.928		
Number of half-months employed in 2001	8.562	8.649	0.747	7.507	7.877	0.133	A	3
	8.570	8.680	0.724	7.550	7.621	0.801		

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Table A.3.2: Means of variables included in the propensity score specifications (continued)

	Men			Women			Data source	Specification
	Treated	Controls	p-value	Treated	Controls	p-value		
Number of half-months seeking a job while employed in 2004	0.467	0.479	0.831	0.442	0.517	0.182	A	3
	0.467	0.476	0.898	0.446	0.464	0.777		
Number of half-months seeking a job while employed in 2003	0.263	0.268	0.895	0.260	0.310	0.222	A	3
	0.263	0.260	0.948	0.261	0.266	0.907		
Number of half-months seeking a job while employed in 2002	0.199	0.209	0.795	0.188	0.173	0.634	A	3
	0.199	0.205	0.879	0.189	0.186	0.943		
Number of half-months seeking a job while employed in 2001	0.143	0.151	0.820	0.169	0.147	0.472	A	3
	0.143	0.141	0.952	0.166	0.153	0.709		
Number of half-months in a program in 2004	1.818	1.756	0.594	1.551	1.278	0.006	A	3
	1.819	1.809	0.939	1.498	1.494	0.970		
Number of half-months in a program in 2003	1.411	1.401	0.927	1.132	1.074	0.545	A	3
	1.413	1.412	0.994	1.103	1.148	0.685		
Number of half-months in a program in 2002	1.459	1.529	0.562	1.387	1.188	0.055	A	3
	1.461	1.430	0.817	1.376	1.368	0.948		
Number of half-months in a program in 2001	1.527	1.507	0.867	1.394	1.150	0.018	A	3
	1.523	1.544	0.882	1.376	1.350	0.833		
Number of employment spells in 2004	0.306	0.310	0.779	0.298	0.316	0.168	A	3
	0.306	0.308	0.880	0.299	0.300	0.964		
Number of employment spells in 2003	0.216	0.240	0.075	0.168	0.199	0.007	A	3
	0.216	0.221	0.738	0.170	0.171	0.900		
Number of employment spells in 2002	0.232	0.244	0.389	0.219	0.234	0.222	A	3
	0.232	0.239	0.655	0.219	0.212	0.616		

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Table A.3.2: Means of variables included in the propensity score specifications (continued)

	Men			Women			Data	Specifi-
	Treated	Controls	p-value	Treated	Controls	p-value	source	cation
Number of employment spells in 2001	0.282	0.296	0.366	0.225	0.258	0.011	A	3
	0.282	0.285	0.867	0.226	0.225	0.911		
Number of unemployment spells in 2004	0.764	0.802	0.035	0.588	0.590	0.906	A	3
	0.765	0.771	0.773	0.589	0.587	0.912		
Number of unemployment spells in 2003	0.339	0.384	0.008	0.252	0.265	0.322	A	3
	0.339	0.347	0.668	0.252	0.256	0.773		
Number of unemployment spells in 2002	0.361	0.405	0.011	0.270	0.279	0.551	A	3
	0.360	0.376	0.432	0.269	0.265	0.783		
Number of unemployment spells in 2001	0.348	0.366	0.292	0.256	0.249	0.597	A	3
	0.348	0.344	0.818	0.253	0.247	0.717		
Number of spells of job seeking while employed in 2004	0.081	0.098	0.052	0.077	0.089	0.094	A	3
	0.081	0.082	0.947	0.077	0.079	0.884		
Number of spells of job seeking while employed in 2003	0.065	0.059	0.410	0.054	0.058	0.540	A	3
	0.065	0.065	0.966	0.055	0.054	0.940		
Number of spells of job seeking while employed in 2002	0.050	0.047	0.610	0.042	0.038	0.454	A	3
	0.050	0.054	0.665	0.042	0.044	0.808		
Number of spells of job seeking while employed in 2001	0.030	0.033	0.505	0.040	0.033	0.106	A	3
	0.030	0.030	0.955	0.039	0.039	0.956		
Number of programs in 2002	0.142	0.125	0.092	0.114	0.094	0.022	A	3
	0.142	0.140	0.842	0.112	0.110	0.837		
Number of programs in 2001	0.122	0.121	0.950	0.122	0.088	0.000	A	3
	0.122	0.121	0.935	0.117	0.113	0.750		

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Table A.3.2: Means of variables included in the propensity score specifications (continued)

	Men			Women			Data	Specifi-
	Treated	Controls	p-value	Treated	Controls	p-value	source	cation
Number of spells out of labor force in 2004	0.222	0.262	0.003	0.253	0.326	0.000	A	3
	0.222	0.223	0.956	0.254	0.257	0.856		
Number of spells out of labor force in 2003	0.168	0.214	0.000	0.145	0.172	0.011	A	3
	0.168	0.171	0.849	0.145	0.149	0.726		
Number of spells out of labor force in 2002	0.189	0.218	0.026	0.173	0.191	0.119	A	3
	0.189	0.197	0.596	0.175	0.169	0.659		
Number of spells out of labor force in 2001	0.175	0.218	0.001	0.150	0.195	0.000	A	3
	0.175	0.178	0.852	0.151	0.155	0.701		
Number of observations before matching	2,066	4,194		2,423	4,603			
Number of observations of the matched sample	2,064	4,194		2,401	4,603			

Remarks: Treated are those individuals who are registered at decentralized welfare agencies; the controls are registered at centralized agencies. The p-values derive from equality-of-means tests of the displayed variables for treated and controls before (first row) and after (second row) matching. The data sources for the variables are the survey of welfare recipients (S), administrative data (A), and regional data (R). Variables marked by 1 in the final column of the table are included in the preferred specification of the propensity score as well as in the sensitivity analyses. Variables indicated by 2 are used for the propensity score specifications in the sensitivity analyses with additional regional covariates and with additional regional and individual covariates. Variables marked by 3 are only included in the propensity score specification in the sensitivity analysis with additional regional and individual covariates. The displayed means and the number of observations after matching refer to this specification with additional regional and individual covariates. The macroeconomic variables (unemployment ratio, GDP per employed person, population density) are binary dummy variables. They were measured in December 2003 and indicate a value higher than the 0.75 quantile of the distribution of the respective variable across all German districts. The classification of labor market conditions (above average, on average, below average) is based on the results of Arntz et al. (2006). The upper tercile of districts that face the best macroeconomic conditions with respect to the regional variables relevant for employment uptake of the long-term unemployed are classified as having above average labor market conditions. The middle tercile of districts is presumed to have average labor market conditions, and the lower tercile has below average conditions (see also IAW and ZEW, 2006, for more details).

Table A.3.3: Preferred propensity score specifications

	Total sample		Singles		Non-singles		Native Germans		Immigrants	
	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women
Age (reference: 25 to 34 years)										
18 to 24 years	0.0433**	-0.0017	0.0379	0.0674	0.0323	-0.0161	0.0437	-0.0034	0.0724*	0.0111
	(0.0220)	(0.0199)	(0.0344)	(0.0430)	(0.0311)	(0.0215)	(0.0267)	(0.0218)	(0.0406)	(0.0361)
35 to 44 years	0.0551***	0.0042	0.0542*	-0.0080	0.0559**	0.0069	0.0737***	0.0205	0.0175	-0.0532
	(0.0205)	(0.0188)	(0.0284)	(0.0484)	(0.0262)	(0.0194)	(0.0251)	(0.0223)	(0.0329)	(0.0405)
45 to 57 years	0.0527***	0.0166	0.0571**	-0.0021	0.0444	0.0294	0.0575**	0.0064	0.0544*	0.0742*
	(0.0184)	(0.0199)	(0.0270)	(0.0369)	(0.0279)	(0.0216)	(0.0236)	(0.0225)	(0.0304)	(0.0422)
Schooling (reference: secondary general school)										
Intermediate secondary school	0.0376**	0.0147	0.0370	0.0136	0.0356*	0.0144	0.0334*	0.0116	0.0438	0.0094
	(0.0171)	(0.0184)	(0.0257)	(0.0325)	(0.0210)	(0.0203)	(0.0193)	(0.0213)	(0.0350)	(0.0318)
University entrance diploma	0.0642***	0.0440	0.0765***	0.0701	0.0529*	0.0350	0.0945***	0.0754**	0.0113	-0.0159
	(0.0200)	(0.0301)	(0.0281)	(0.0543)	(0.0274)	(0.0310)	(0.0243)	(0.0371)	(0.0313)	(0.0412)
Other or missing	-0.0164	−0.0458*	0.0203	-0.0034	-0.0404	−0.0562**	-0.0256	−0.0675**	-0.0094	-0.0330
	(0.0231)	(0.0239)	(0.0323)	(0.0661)	(0.0354)	(0.0261)	(0.0288)	(0.0326)	(0.0366)	(0.0365)
Migration background (reference: native Germans)										
Immigrant	0.0025	0.0334	-0.0073	0.0565	0.0047	0.0301				
	(0.0241)	(0.0262)	(0.0299)	(0.0458)	(0.0273)	(0.0260)				
Household size (reference: 2 persons)										
1 person	-0.0239	-0.0024					-0.0302	-0.0028	-0.0021	0.0149
	(0.0175)	(0.0198)					(0.0211)	(0.0225)	(0.0379)	(0.0373)
3 or more persons	-0.0281	0.0058					-0.0336	0.0201	-0.0168	-0.0308
	(0.0267)	(0.0184)					(0.0316)	(0.0216)	(0.0456)	(0.0283)

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Table A.3.3: Preferred propensity score specifications (continued)

	Total sample		Singles		Non-singles		Native Germans		Immigrants	
	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women
Number of children (reference: 1 child)										
No children	-0.0270	-0.0224			-0.0091	-0.0244	-0.0223	-0.0248	-0.0523	-0.0258
	(0.0266)	(0.0188)			(0.0214)	(0.0196)	(0.0313)	(0.0229)	(0.0460)	(0.0327)
2 or more children	0.0341	-0.0149			0.0284	-0.0136	0.0178	-0.0295	0.0508	0.0216
	(0.0227)	(0.0181)			(0.0232)	(0.0165)	(0.0297)	(0.0226)	(0.0332)	(0.0309)
Obstacles to employment										
Disabled	0.0539***	0.0416	0.0278	0.1213***	0.0796***	-0.0095	0.0562***	0.0435	0.0477	0.0252
	(0.0179)	(0.0261)	(0.0229)	(0.0384)	(0.0255)	(0.0332)	(0.0190)	(0.0292)	(0.0455)	(0.0537)
Care obligation	0.0360	0.0184	-0.0427	0.0558	0.1051*	0.0102	0.0221	0.0160	0.0732	0.0120
	(0.0457)	(0.0322)	(0.0566)	(0.0809)	(0.0637)	(0.0344)	(0.0484)	(0.0378)	(0.0922)	(0.0518)
Status before welfare receipt										
(Minor) employment	−0.0254*	0.0101	-0.0180	0.0527*	−0.0303*	-0.0057	-0.0069	0.0094	−0.0736***	0.0138
	(0.0151)	(0.0116)	(0.0234)	(0.0304)	(0.0165)	(0.0143)	(0.0181)	(0.0142)	(0.0237)	(0.0217)
Labor market history from 2001 to 2004										
Number of half-months unemployed in 2004	−0.0016*	-0.0007	-0.0018	−0.0039**	-0.0014	0.0004	−0.0022**	-0.0008	0.0003	-0.0008
	(0.0009)	(0.0011)	(0.0013)	(0.0018)	(0.0013)	(0.0014)	(0.0011)	(0.0013)	(0.0016)	(0.0017)
Number of half-months unemployed in 2003	-0.0009	0.0013	-0.0000	0.0043**	-0.0018	0.0001	-0.0004	0.0017	-0.0022	-0.0010
	(0.0009)	(0.0010)	(0.0012)	(0.0019)	(0.0013)	(0.0012)	(0.0012)	(0.0012)	(0.0018)	(0.0024)
Number of half-months unemployed in 2002	0.0003	-0.0008	0.0016	-0.0008	-0.0010	-0.0011	0.0001	-0.0004	0.0005	-0.0023
	(0.0009)	(0.0012)	(0.0014)	(0.0018)	(0.0014)	(0.0014)	(0.0009)	(0.0012)	(0.0019)	(0.0029)
Number of half-months unemployed in 2001	-0.0002	0.0005	-0.0013	0.0011	0.0009	0.0004	-0.0003	-0.0004	0.0001	0.0038
	(0.0009)	(0.0010)	(0.0015)	(0.0016)	(0.0012)	(0.0012)	(0.0010)	(0.0010)	(0.0019)	(0.0026)
Number of half-months out of labor force from 2001 to 2004	−0.0008**	−0.0006*	−0.0010**	-0.0010	-0.0006	-0.0006	−0.0010**	−0.0008**	-0.0003	0.0002
	(0.0004)	(0.0003)	(0.0005)	(0.0007)	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0007)	(0.0005)

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Table A.3.3: Preferred propensity score specifications (continued)

	Total sample		Singles		Non-singles		Native Germans		Immigrants	
	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women
Mean duration out of labor force	-0.0009	-0.0007	-0.0005	-0.0013	-0.0011	-0.0005	-0.0004	-0.0008	-0.0023	-0.0008
from 2003 to 2004 in half-months	(0.0008)	(0.0006)	(0.0009)	(0.0015)	(0.0011)	(0.0007)	(0.0009)	(0.0007)	(0.0018)	(0.0010)
Number of programs from 2003 to	-0.0238	-0.0074	−0.0412**	-0.0122	-0.0043	-0.0048	−0.0354**	0.0021	0.0125	-0.0413
2004	(0.0157)	(0.0184)	(0.0185)	(0.0236)	(0.0196)	(0.0203)	(0.0169)	(0.0184)	(0.0275)	(0.0325)
Mean duration of programs from	0.0003	0.0009	-0.0002	0.0012	0.0004	0.0006	0.0004	0.0007	0.0001	0.0009
2003 to 2004 in half-months	(0.0013)	(0.0013)	(0.0018)	(0.0023)	(0.0018)	(0.0015)	(0.0015)	(0.0014)	(0.0034)	(0.0025)
Current welfare spell										
Months in welfare before 10/2006	0.0006	0.0000	0.0003	0.0043**	0.0009	-0.0014	0.0011	0.0005	-0.0005	-0.0008
	(0.0012)	(0.0012)	(0.0015)	(0.0021)	(0.0014)	(0.0014)	(0.0013)	(0.0013)	(0.0023)	(0.0025)
Start after 10/2006 or missing	0.0278	0.0208	0.0202	0.1160**	0.0330	-0.0055	0.0524*	0.0212	-0.0304	0.0210
	(0.0247)	(0.0266)	(0.0367)	(0.0527)	(0.0317)	(0.0311)	(0.0277)	(0.0326)	(0.0427)	(0.0466)
Regional information										
Unemployment ratio (high)	0.0151	0.0427	0.0204	0.0429	0.0120	0.0405	0.0272	0.0449	-0.0916	-0.0201
	(0.0973)	(0.1001)	(0.0968)	(0.1036)	(0.1001)	(0.1007)	(0.0997)	(0.1016)	(0.0904)	(0.1100)
Urban district	−0.2127**	−0.2236**	−0.1942**	−0.2038**	−0.2273**	−0.2319**	−0.2232**	−0.2413**	−0.1797*	−0.1813*
	(0.0861)	(0.0872)	(0.0860)	(0.0916)	(0.0875)	(0.0862)	(0.0835)	(0.0827)	(0.1025)	(0.1100)
Observations	6,260	7,026	2,810	1,690	3,450	5,336	4,663	5,239	1,597	1,787
McFadden-R^2	0.047	0.046	0.044	0.051	0.050	0.049	0.051	0.055	0.052	0.038
Log-Likelihood	-3,783.36	-4,318.83	-1,656.71	-1,022.46	-2,118.80	-3,279.64	-2,821.32	-3,198.07	-944.64	-1,097.34

Remarks: Displayed are marginal effects and standard errors in brackets. The dependent variable is defined to be 1 if an individual is registered at a decentralized welfare agency. Otherwise, the variable is 0. The unemployment ratio variable is a binary dummy variable. It was measured in December 2003 and indicates a value higher than the 0.75 quantile of the distribution of the unemployment ratios across all German districts. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$.

Table A.3.4: Indicators for matching quality in subsamples

	Singles		Non-singles		Native Germans		Immigrants	
	Men	Women	Men	Women	Men	Women	Men	Women
Indicators before matching								
McFadden-R^2	0.044	0.051	0.050	0.049	0.051	0.055	0.052	0.038
LR-Test	152.850	110.170	224.190	337.040	302.700	373.260	103.740	87.540
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean standardized difference in %	7.026	7.826	6.594	7.761	6.683	8.534	8.942	6.137
Indicators after matching								
McFadden-R^2	0.001	0.001	0.001	0.002	0.001	0.001	0.001	0.002
LR-Test	1.520	0.910	3.340	8.650	2.930	6.470	0.940	2.570
p-value	1.000	1.000	1.000	0.998	1.000	1.000	1.000	1.000
Mean standardized difference in %	1.038	0.789	1.353	1.228	1.013	1.144	1.079	2.014
Smith and Todd (2005b) balancing test								
p-values > 0.05	20	18	22	15	17	15	24	21
p-values > 0.01	20	22	22	19	20	18	24	22

Remarks: All indicators are based on the preferred propensity score specifications. The McFadden-R^2 derives from a probit estimation of the propensity score on all covariates considered. The LR-statistic and the corresponding p-value derive from a likelihood-ratio test of the joint insignificance of all covariates. The mean standardized difference in % has been calculated as an unweighted average of all covariates. The Smith and Todd (2005b) balancing test displays the number of covariates passing the test at the indicated significance level. There are 22 covariates included in the preferred propensity score specifications for singles, 24 covariates for non-singles, and 25 covariates for native Germans and immigrants, respectively.

Table A.3.5: Estimated effects of decentralized welfare administration on self-sufficient employment

	January	February	March	April	May	June	July	August	September	October	November	December
Preferred propensity score specification												
Men	−0.0119*	−0.0111*	−0.0139*	-0.0127	−0.0214**	−0.0228**	−0.0306***	−0.0326***	−0.0293**	−0.0266**	−0.0275**	−0.0250*
	(0.0062)	(0.0067)	(0.0076)	(0.0079)	(0.0085)	(0.0100)	(0.0105)	(0.0117)	(0.0121)	(0.0129)	(0.0140)	(0.0142)
Women	-0.0065	-0.0008	-0.0026	-0.0099	-0.0071	-0.0033	-0.0063	-0.0058	-0.0050	-0.0037	-0.0042	-0.0047
	(0.0058)	(0.0066)	(0.0064)	(0.0063)	(0.0068)	(0.0068)	(0.0067)	(0.0069)	(0.0078)	(0.0093)	(0.0088)	(0.0096)
Additional regional covariates												
Men	−0.0121*	−0.0127*	−0.0160**	−0.0146*	−0.0236***	−0.0255**	−0.0335***	−0.0349***	−0.0313**	−0.0292**	−0.0302**	−0.0272*
	(0.0064)	(0.0069)	(0.0080)	(0.0084)	(0.0090)	(0.0101)	(0.0112)	(0.0122)	(0.0123)	(0.0136)	(0.0144)	(0.0147)
Women	-0.0068	-0.0010	-0.0038	-0.0099	-0.0076	-0.0041	-0.0065	-0.0059	-0.0066	-0.0055	-0.0049	-0.0049
	(0.0059)	(0.0066)	(0.0066)	(0.0065)	(0.0069)	(0.0069)	(0.0070)	(0.0071)	(0.0080)	(0.0098)	(0.0094)	(0.0103)
Additional regional and individual covariates												
Men	−0.0118*	−0.0125*	−0.0154*	-0.0130	−0.0224***	−0.0239**	−0.0328***	−0.0342***	−0.0298***	−0.0271**	−0.0275**	−0.0253*
	(0.0064)	(0.0067)	(0.0079)	(0.0081)	(0.0084)	(0.0096)	(0.0105)	(0.0111)	(0.0110)	(0.0126)	(0.0129)	(0.0133)
Women	-0.0047	0.0010	-0.0014	-0.0069	-0.0043	-0.0009	-0.0032	-0.0030	-0.0030	-0.0027	-0.0019	-0.0014
	(0.0059)	(0.0066)	(0.0064)	(0.0063)	(0.0065)	(0.0068)	(0.0067)	(0.0072)	(0.0078)	(0.0095)	(0.0089)	(0.0100)

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. The outcome variable for each month is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0. Standard errors were obtained through bootstrapping based on 250 resamples. The standard errors take into account clustering at the agency level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Treated are those individuals who are registered at decentralized welfare agencies; the controls are registered at centralized welfare agencies. In the sample of men, the number of matched treated in the preferred propensity score specification is 2,062. In the specification with additional regional covariates and in the specification with additional regional and individual covariates, the number of matched treated amounts to 2,064. The number of controls is 4,194 in all three specifications. In the sample of women, the number of matched treated in the preferred specification is 2,423. It is 2,388 in the specification with additional regional covariates and 2,401 in the specification with additional regional and individual covariates. The number of untreated controls is 4,603 in all three specifications. The displayed results refer to the year 2007. The sampling date is October 2006.

Table A.3.6: Estimated effects of decentralized welfare administration on self-sufficient employment for singles and non-singles

	January	February	March	April	May	June	July	August	September	October	November	December
Singles												
Men	−0.0149*	-0.0136	−0.0192*	−0.0201*	−0.0190*	−0.0277*	−0.0353**	−0.0401**	−0.0452**	−0.0396**	−0.0369**	−0.0398**
	(0.0084)	(0.0107)	(0.0114)	(0.0109)	(0.0116)	(0.0158)	(0.0156)	(0.0157)	(0.0176)	(0.0174)	(0.0187)	(0.0192)
Women	−0.0252**	-0.0173	−0.0218*	−0.0220*	-0.0113	-0.0157	-0.0040	0.0036	-0.0022	-0.0145	-0.0236	-0.0151
	(0.0107)	(0.0114)	(0.0127)	(0.0123)	(0.0142)	(0.0148)	(0.0150)	(0.0156)	(0.0175)	(0.0166)	(0.0168)	(0.0174)
Non-singles												
Men	-0.0110	-0.0106	-0.0111	-0.0090	−0.0244**	-0.0204	−0.0287**	−0.0292**	-0.0213	-0.0210	-0.0228	-0.0150
	(0.0091)	(0.0087)	(0.0091)	(0.0108)	(0.0120)	(0.0130)	(0.0132)	(0.0141)	(0.0142)	(0.0151)	(0.0164)	(0.0174)
Women	-0.0018	0.0024	0.0024	-0.0066	-0.0051	0.0008	-0.0072	-0.0086	-0.0052	-0.0002	0.0010	-0.0018
	(0.0064)	(0.0077)	(0.0070)	(0.0068)	(0.0077)	(0.0080)	(0.0083)	(0.0084)	(0.0092)	(0.0106)	(0.0103)	(0.0112)

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. Estimations are based on the preferred propensity score specification. The outcome variable for each month is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0. Standard errors were obtained through bootstrapping based on 250 resamples. The standard errors take into account clustering at the agency level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Treated are those individuals who are registered at decentralized welfare agencies; the controls are registered at centralized welfare agencies. Singles are defined to be persons living on their own. Non-singles are individuals living together with at least one other person in the same household. In the sample of male singles, the number of matched treated is 857, and the number of controls is 1,947. In the sample of male non-singles, the number of matched treated is 1,201, and the number of controls is 2,247. In the sample of female singles, the number of matched treated is 562, and the number of controls is 1,124. In the sample of female non-singles, the number of matched treated is 1,857, and the number of controls is 3,479. The displayed results refer to the year 2007. The sampling date is October 2006.

Table A.3.7: Estimated effects of decentralized welfare administration on self-sufficient employment for native Germans and immigrants

	January	February	March	April	May	June	July	August	September	October	November	December
Native Germans												
Men	-0.0067	-0.0088	-0.0130	-0.0132	−0.0186*	−0.0268**	−0.0299**	−0.0332**	−0.0346***	−0.0329**	−0.0333**	−0.0284*
	(0.0071)	(0.0072)	(0.0087)	(0.0091)	(0.0101)	(0.0117)	(0.0118)	(0.0129)	(0.0128)	(0.0135)	(0.0144)	(0.0146)
Women	-0.0075	0.0006	-0.0032	-0.0109	-0.0096	-0.0034	-0.0043	-0.0044	-0.0024	0.0001	0.0003	-0.0002
	(0.0071)	(0.0078)	(0.0076)	(0.0081)	(0.0087)	(0.0087)	(0.0085)	(0.0088)	(0.0096)	(0.0110)	(0.0103)	(0.0112)
Immigrants												
Men	−0.0274***	-0.0156	-0.0156	-0.0097	−0.0294*	-0.0122	−0.0345*	-0.0320	-0.0133	-0.0057	-0.0091	-0.0144
	(0.0100)	(0.0111)	(0.0100)	(0.0144)	(0.0157)	(0.0163)	(0.0184)	(0.0235)	(0.0237)	(0.0243)	(0.0248)	(0.0267)
Women	-0.0013	-0.0036	0.0004	-0.0053	0.0010	-0.0024	-0.0148	-0.0109	-0.0131	-0.0113	-0.0126	-0.0114
	(0.0080)	(0.0095)	(0.0103)	(0.0113)	(0.0118)	(0.0124)	(0.0132)	(0.0147)	(0.0149)	(0.0146)	(0.0155)	(0.0161)

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. Estimations are based on the preferred propensity score specification. The outcome variable for each month is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0. Standard errors were obtained through bootstrapping based on 250 resamples. The standard errors take into account clustering at the agency level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Treated are those individuals who are registered at decentralized welfare agencies; the controls are registered at centralized welfare agencies. In the sample of male natives, the number of matched treated is 1,560, and the number of controls is 3,102. In the sample of female natives, the number of matched treated is 1,822, and the number of controls is 3,417. In the sample of male immigrants, the number of matched treated is 504, and the number of controls is 1,092. In the sample of female immigrants, the number of matched treated is 600, and the number of controls is 1,186. The displayed results refer to the year 2007. The sampling date is October 2006.

Table A.3.8: Estimated probit effects of organizational features on self-sufficient employment for men

	January	February	March	April	May	June	July	August	September	October	November	December
Decentralized welfare agency	-0.0099	−0.0121*	−0.0112*	-0.0111	−0.0168*	-0.0158	−0.0238**	−0.0259**	−0.0202*	-0.0203	-0.0156	-0.0156
	(0.0062)	(0.0064)	(0.0063)	(0.0070)	(0.0086)	(0.0099)	(0.0104)	(0.0119)	(0.0121)	(0.0130)	(0.0141)	(0.0145)
Generalized case management	0.0039	0.0089	0.0046	0.0037	0.0004	-0.0023	-0.0009	0.0014	-0.0043	-0.0013	-0.0140	-0.0094
	(0.0063)	(0.0071)	(0.0071)	(0.0074)	(0.0090)	(0.0096)	(0.0108)	(0.0119)	(0.0122)	(0.0131)	(0.0135)	(0.0141)
Integration of activation and placement	0.0002	-0.0051	0.0005	-0.0011	0.0013	-0.0026	-0.0067	-0.0057	-0.0034	-0.0067	-0.0047	-0.0035
	(0.0057)	(0.0061)	(0.0066)	(0.0068)	(0.0075)	(0.0085)	(0.0088)	(0.0100)	(0.0104)	(0.0110)	(0.0118)	(0.0126)
Customer segmentation	-0.0039	-0.0074	-0.0061	0.0015	0.0029	-0.0060	-0.0084	-0.0087	-0.0064	-0.0027	0.0029	0.0066
	(0.0071)	(0.0077)	(0.0074)	(0.0066)	(0.0075)	(0.0091)	(0.0096)	(0.0098)	(0.0090)	(0.0104)	(0.0115)	(0.0117)
Employer service	-0.0032	-0.0040	-0.0113	-0.0085	-0.0086	-0.0092	-0.0073	0.0035	-0.0049	-0.0012	-0.0045	-0.0185
	(0.0072)	(0.0084)	(0.0099)	(0.0079)	(0.0097)	(0.0106)	(0.0126)	(0.0126)	(0.0138)	(0.0154)	(0.0169)	(0.0173)
Subcontracting of placement services	-0.0067	-0.0040	-0.0002	-0.0009	-0.0056	0.0019	0.0058	0.0010	0.0055	0.0010	0.0014	-0.0004
	(0.0057)	(0.0062)	(0.0063)	(0.0061)	(0.0070)	(0.0082)	(0.0089)	(0.0102)	(0.0102)	(0.0109)	(0.0118)	(0.0121)
McFadden-R^2	0.053	0.058	0.063	0.069	0.068	0.065	0.066	0.067	0.068	0.069	0.071	0.069
Log-Likelihood	-1,218.31	-1,266.86	-1,408.17	-1,559.59	-1,790.67	-2,027.48	-2,152.64	-2,368.69	-2,449.10	-2,480.57	-2,547.35	-2,571.75

Remarks: The dependent variable in each estimation and for each month is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0. Displayed are marginal effects and standard errors in brackets. The standard errors take into account clustering at the agency level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. The number of observations in each estimation is 6,217. One centralized welfare agency had to be dropped from the analysis due to missing information on organizational approaches. All estimations include the covariates used in the preferred propensity score specification of the matching analysis as additional regressors. The displayed results refer to the year 2007. The sampling date is October 2006.

Table A.3.9: Estimated probit effects of organizational features on self-sufficient employment for women

	January	February	March	April	May	June	July	August	September	October	November	December
Decentralized welfare agency	-0.0059	-0.0000	-0.0018	-0.0053	-0.0023	-0.0005	-0.0044	-0.0032	-0.0047	-0.0028	-0.0004	-0.0039
	(0.0045)	(0.0055)	(0.0054)	(0.0058)	(0.0071)	(0.0072)	(0.0069)	(0.0074)	(0.0080)	(0.0097)	(0.0101)	(0.0105)
Generalized case management	0.0017	0.0003	-0.0001	-0.0065	-0.0069	-0.0013	0.0034	0.0000	0.0038	-0.0031	-0.0039	0.0029
	(0.0048)	(0.0054)	(0.0055)	(0.0061)	(0.0067)	(0.0068)	(0.0068)	(0.0073)	(0.0082)	(0.0093)	(0.0094)	(0.0103)
Integration of activation and placement	0.0024	0.0042	-0.0017	0.0035	0.0076	0.0051	0.0054	0.0088	0.0015	-0.0012	0.0004	-0.0010
	(0.0044)	(0.0048)	(0.0052)	(0.0055)	(0.0061)	(0.0064)	(0.0064)	(0.0068)	(0.0074)	(0.0085)	(0.0080)	(0.0086)
Customer segmentation	-0.0046	-0.0054	-0.0037	0.0005	0.0017	-0.0065	-0.0080	-0.0040	-0.0014	0.0020	0.0013	0.0021
	(0.0051)	(0.0051)	(0.0057)	(0.0060)	(0.0063)	(0.0068)	(0.0072)	(0.0074)	(0.0077)	(0.0085)	(0.0081)	(0.0088)
Employer service	-0.0062	-0.0112	-0.0103	-0.0131	-0.0092	-0.0144	-0.0179^{*}	-0.0129	-0.0209^{*}	-0.0259^{*}	-0.0300^{**}	-0.0245^{*}
	(0.0067)	(0.0077)	(0.0085)	(0.0087)	(0.0100)	(0.0098)	(0.0098)	(0.0109)	(0.0118)	(0.0136)	(0.0130)	(0.0138)
Subcontracting of placement services	-0.0046	-0.0074	-0.0084^{*}	-0.0055	-0.0031	-0.0056	-0.0019	-0.0027	-0.0026	0.0008	-0.0045	-0.0077
	(0.0045)	(0.0049)	(0.0051)	(0.0060)	(0.0065)	(0.0068)	(0.0070)	(0.0072)	(0.0081)	(0.0091)	(0.0080)	(0.0090)
McFadden-R^2	0.083	0.089	0.083	0.081	0.079	0.075	0.079	0.078	0.078	0.076	0.075	0.072
Log-Likelihood	-992.28	-1,139.37	-1,243.38	-1,358.90	-1,510.01	-1,687.30	-1,763.29	-1,838.24	-1,953.05	-2,021.43	-2,106.28	-2,141.99

Remarks: The dependent variable in each estimation and for each month is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0. Displayed are marginal effects and standard errors in brackets. The standard errors take into account clustering at the agency level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. The number of observations in each estimation is 6,992. One centralized welfare agency had to be dropped from the analysis due to missing information on organizational approaches. All estimations include the covariates used in the preferred propensity score specification of the matching analysis as further regressors. The displayed results refer to the year 2007. The sampling date is October 2006.

Appendix to Chapter 4

Fig. A.4.1: Sanction strategies of welfare agencies

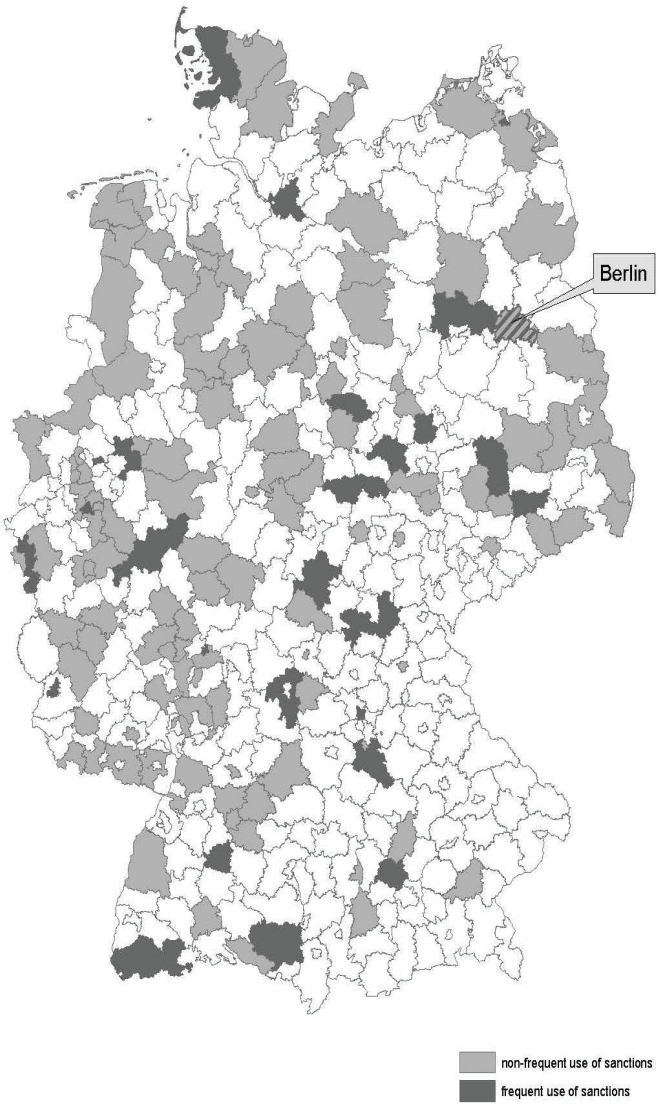

Fig. A.4.2: Sanction rates of welfare agencies

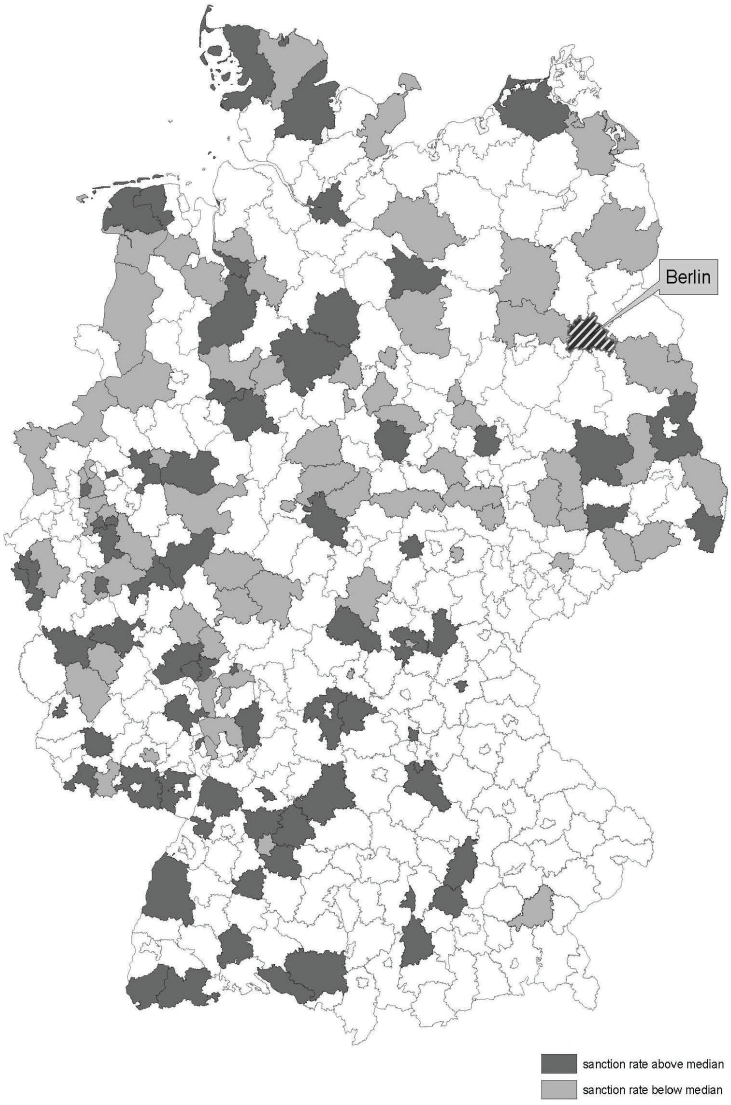

Table A.4.1: Regression of Z_1 and Z_2 on organizational and regional variables

	Z_1			Z_2		
	(a)	(b)	(c)	(a)	(b)	(c)
Organization 1b	-0.0760		-0.0766	-0.0806		-0.0629
	(0.0969)		(0.0998)	(0.1193)		(0.1183)
Organization 2a	-0.0403		-0.0212	-0.0806		-0.1202
	(0.1113)		(0.1149)	(0.1370)		(0.1362)
Organization 2b	-0.1449		-0.1358	-0.1443		-0.1694
	(0.0919)		(0.0941)	(0.1132)		(0.1116)
Urban district		0.0306	0.0300		-0.0043	-0.0083
		(0.0916)	(0.0921)		(0.1086)	(0.1092)
East Germany		0.0969	0.0758		-0.2564	-0.2785
		(0.1950)	(0.1975)		(0.2312)	(0.2341)
Unemployment ratio (high)		-0.0425	-0.0277		-0.0416	-0.0353
		(0.1954)	(0.1988)		(0.2317)	(0.2357)
GDP per employed person (high)		-0.0395	-0.0465		0.0407	0.0418
		(0.0846)	(0.0853)		(0.1003)	(0.1011)
Welfare ratio (high)		0.0179	0.0128		0.0385	0.0223
		(0.0886)	(0.0895)		(0.1051)	(0.1060)
Constant	0.2903***	0.1937***	0.2723***	0.5806***	0.5478***	0.6548***
	(0.0735)	(0.0520)	(0.0873)	(0.0905)	(0.0616)	(0.1035)
Observations	152	152	152	152	152	152
R^2	0.0185	0.0084	0.0260	0.0110	0.0733	0.0897

Remarks: Displayed are Ordinary Least Squares (OLS) estimation results. The specification of the organizational and regional variables is described in section 4.3. The regressions are based on those welfare agencies for which all regressors depicted in tables A.4.1, A.4.2, A.4.3 and A.4.4 are available. Z_1 refers to the sanction strategy of welfare agencies. $Z_1 = 0$ denotes less strict agencies, and $Z_1 = 1$ denotes agencies that frequently apply sanctions. Z_2 refers to the observed sanction rates within welfare agencies. $Z_2 = 0$ denotes welfare agencies with a sanction rate below the median across all 154 sampled agencies, and $Z_2 = 1$ denotes agencies with a sanction rate above the median. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$.

Table A.4.2: Regression of Z_1 and Z_2 on caseload variables

	Z_1	Z_1	Z_1	Z_2	Z_2	Z_2
Ratio of welfare recipients to overall	0.0004		0.0011	-0.0007		-0.0015
staff in welfare agency	(0.0016)		(0.0019)	(0.0020)		(0.0023)
Ratio of welfare recipients to staff		-0.0002	-0.0003		0.0002	0.0004
for labor market activation		(0.0004)	(0.0004)		(0.0005)	(0.0005)
Constant	0.1760	0.2510***	0.1872	0.5575***	0.4600***	0.5449***
	(0.1350)	(0.0845)	(0.1360)	(0.1655)	(0.1036)	(0.1668)
Observations	152	152	152	152	152	152
R^2	0.0005	0.0018	0.0042	0.0009	0.0012	0.0040

Remarks: Displayed are Ordinary Least Squares (OLS) estimation results. The regressors relate the number of welfare recipients to the full-time equivalent number of overall staff and to the full-time equivalent number of staff responsible for labor market activation in the welfare agency, respectively. The regressions are based on those welfare agencies for which all regressors depicted in tables A.4.1, A.4.2, A.4.3 and A.4.4 are available. Z_1 refers to the sanction strategy of welfare agencies. $Z_1 = 0$ denotes less strict agencies, and $Z_1 = 1$ denotes agencies that frequently apply sanctions. Z_2 refers to the observed sanction rates within welfare agencies. $Z_2 = 0$ denotes welfare agencies with a sanction rate below the median across all 154 sampled agencies, and $Z_2 = 1$ denotes agencies with a sanction rate above the median. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$.

Table A.4.3: Regression of Z_1 on strategical variables

High strategical value of ...	Z_1	Z_1	Z_1	Z_1	Z_1	Z_1	Z_1	Z_1	Z_1	Z_1	Z_1	Z_1	Z_1
Job creation schemes	0.0778												0.1072
	(0.0682)												(0.0758)
Start-up subsidies		-0.0990											-0.1328
		(0.0735)											(0.0860)
Wage subsidies for employees			-0.0128										0.0356
			(0.0674)										(0.0768)
Wage subsidies for employers				0.0141									0.0496
				(0.0708)									(0.0818)
Promotion of professional					-0.0829								-0.1306
qualifications					(0.0856)								(0.1034)
Training of key skills (e.g job						-0.0418							-0.0303
search training)						(0.0761)							(0.0866)
Internships							0.0606						0.1377
							(0.0789)						(0.0919)
Support for disadvantaged								-0.0276					-0.0424
young welfare recipients								(0.0720)					(0.0794)
Support for disabled individuals									0.0098				0.0116
									(0.0725)				(0.0813)
Supply of counseling services										-0.0526			-0.0474
										(0.0664)			(0.0821)
Supply of child care facilities											-0.0515		0.0232
											(0.0666)		(0.0885)
Supply of other social services												-0.0404	-0.0097
												(0.0733)	(0.0936)
Constant	0.1809***	0.2385***	0.2159***	0.2059***	0.2258***	0.2212***	0.1966***	0.2190***	0.2075***	0.2368***	0.2346***	0.2222***	0.2012***
	(0.0421)	(0.0391)	(0.0437)	(0.0406)	(0.0367)	(0.0386)	(0.0379)	(0.0400)	(0.0399)	(0.0470)	(0.0455)	(0.0395)	(0.0665)
Observations	152	152	152	152	152	152	152	152	152	152	152	152	152
R^2	0.0086	0.0120	0.0002	0.0003	0.0062	0.0020	0.0039	0.0010	0.0001	0.0042	0.0040	0.0020	0.0524

Remarks: Displayed are Ordinary Least Squares (OLS) estimation results. The regressors derive from the surveys at agency level in which agency managers were asked about their importance for the strategy of the welfare agency. Managers could answer on a scale from 1 (not important) to 5 (very important). In the estimations presented here, the regressors are defined to be 1 if their importance is above the median level across all German welfare agencies. Otherwise, they are 0. The regressions are based on those welfare agencies for which all regressors depicted in tables A.4.1, A.4.2, A.4.3 and A.4.4 are available. Z_1 refers to the sanction strategy of welfare agencies. $Z_1 = 0$ denotes less strict agencies, and $Z_1 = 1$ denotes agencies that frequently apply sanctions. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$.

Table A.4.4: Regression of Z_2 on strategical variables

High strategical value of ...	Z_2	Z_2	Z_2	Z_2	Z_2	Z_2	Z_2	Z_2	Z_2	Z_2	Z_2	Z_2	Z_2
Job creation schemes	0.1115												0.1248
	(0.0835)												(0.0930)
Start-up subsidies		-0.0486											-0.1143
		(0.0906)											(0.1054)
Wage subsidies for employees			-0.0270										0.0325
			(0.0827)										(0.0942)
Wage subsidies for employers				0.0298									0.0714
				(0.0869)									(0.1003)
Promotion of professional					-0.0876								-0.1648
qualifications					(0.1051)								(0.1268)
Training of key skills (e.g job						0.0517							0.0995
search training)						(0.0934)							(0.1062)
Internships							-0.0186						0.0322
							(0.0970)						(0.1127)
Support for disadvantaged								-0.0770					-0.1159
young welfare recipients								(0.0881)					(0.0973)
Support for disabled individuals									-0.0312				-0.0458
									(0.0888)				(0.0996)
Supply of counseling services										0.0263			0.0578
										(0.0816)			(0.1007)
Supply of child care facilities											0.0132		0.1112
											(0.0818)		(0.1085)
Supply of other social services												-0.0960	-0.1268
												(0.0897)	(0.1148)
Constant	0.4574***	0.5138***	0.5114***	0.4902***	0.5161***	0.4867***	0.5043***	0.5238***	0.5094***	0.4868***	0.4938***	0.5278***	0.4505***
	(0.0516)	(0.0482)	(0.0536)	(0.0498)	(0.0451)	(0.0473)	(0.0465)	(0.0490)	(0.0489)	(0.0577)	(0.0559)	(0.0482)	(0.0816)
Observations	152	152	152	152	152	152	152	152	152	152	152	152	152
R^2	0.0117	0.0019	0.0007	0.0008	0.0046	0.0020	0.0002	0.0051	0.0008	0.0007	0.0002	0.0076	0.0527

Remarks: Displayed are Ordinary Least Squares (OLS) estimation results. The regressors derive from the surveys at agency level in which agency managers were asked about their importance for the strategy of the welfare agency. Managers could answer on a scale from 1 (not important) to 5 (very important). In the estimations presented here, the regressors are defined to be 1 if their importance is above the median level across all German welfare agencies. Otherwise, they are 0. The regressions are based on those welfare agencies for which all regressors depicted in tables A.4.1, A.4.2, A.4.3 and A.4.4 are available. Z_2 refers to the observed sanction rates within welfare agencies. $Z_2 = 0$ denotes welfare agencies with a sanction rate below the median across all 154 sampled agencies, and $Z_2 = 1$ denotes agencies with a sanction rate above the median. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$.

Table A.4.5: Detailed estimation results for sanction effects based on instrument Z_1

Month	1	2	3	4	5	6
Sanction	0.0092	0.1347	0.1689	0.4475	0.5604*	0.6766***
	(0.2015)	(0.2383)	(0.2436)	(0.2795)	(0.2927)	(0.2607)
Gender (reference: female)						
Male	0.0178**	0.0170**	0.0094	0.0145	0.0197**	0.0234**
	(0.0070)	(0.0075)	(0.0080)	(0.0098)	(0.0092)	(0.0097)
Age (reference: 25 to 34 years)						
18 to 24 years	−0.0180**	−0.0179*	−0.0281***	−0.0417***	−0.0433***	−0.0504***
	(0.0090)	(0.0100)	(0.0104)	(0.0121)	(0.0128)	(0.0139)
35 to 44 years	−0.0168**	-0.0112	−0.0175*	−0.0178*	−0.0213*	-0.0101
	(0.0085)	(0.0090)	(0.0095)	(0.0102)	(0.0113)	(0.0132)
45 to 57 years	−0.0351***	−0.0339***	−0.0440***	−0.0470***	−0.0423***	−0.0400***
	(0.0111)	(0.0128)	(0.0126)	(0.0130)	(0.0146)	(0.0145)
Schooling (reference: secondary general school)						
Intermediate secondary	0.0245***	0.0270***	0.0340***	0.0371***	0.0362***	0.0365***
school	(0.0069)	(0.0073)	(0.0076)	(0.0082)	(0.0083)	(0.0098)
University entrance diploma	0.0353***	0.0373***	0.0428***	0.0421***	0.0406***	0.0392***
	(0.0116)	(0.0119)	(0.0134)	(0.0134)	(0.0134)	(0.0121)
Other or missing	−0.0188**	−0.0271***	−0.0210**	-0.0137	−0.0282**	-0.0289
	(0.0075)	(0.0078)	(0.0095)	(0.0151)	(0.0138)	(0.0199)
Migration background (reference: native Germans)						
Immigrant	−0.0192***	−0.0238***	−0.0278***	−0.0276***	−0.0292***	−0.0218**
	(0.0062)	(0.0059)	(0.0070)	(0.0076)	(0.0078)	(0.0087)
Household size (reference: 2 persons)						
1 person	-0.0078	0.0008	0.0084	0.0054	0.0100	0.0069
	(0.0074)	(0.0081)	(0.0088)	(0.0102)	(0.0100)	(0.0116)
3 or more persons	0.0116	0.0185**	0.0153*	0.0058	0.0065	0.0166
	(0.0079)	(0.0084)	(0.0088)	(0.0104)	(0.0105)	(0.0115)
Number of children (reference: 1 child)						
No children	0.0287***	0.0268***	0.0251**	0.0230**	0.0215**	0.0295**
	(0.0089)	(0.0093)	(0.0099)	(0.0101)	(0.0109)	(0.0121)
2 or more children	−0.0184**	−0.0252***	−0.0275***	−0.0216**	−0.0196*	−0.0285**
	(0.0078)	(0.0083)	(0.0096)	(0.0105)	(0.0118)	(0.0138)
Obstacles to employment						
Disabled	−0.0235***	−0.0232***	−0.0209*	-0.0090	−0.0208*	−0.0291**
	(0.0078)	(0.0090)	(0.0108)	(0.0119)	(0.0112)	(0.0123)
Care obligation	−0.0188**	−0.0238***	−0.0301***	-0.0117	−0.0260**	−0.0286*
	(0.0086)	(0.0089)	(0.0090)	(0.0219)	(0.0126)	(0.0148)
Status before receipt of welfare benefits						
(Minor) employment	0.0124*	0.0129*	0.0094	0.0070	0.0110	0.0117
	(0.0064)	(0.0071)	(0.0077)	(0.0084)	(0.0084)	(0.0098)
Number of previous unemployment spells (reference: 2 or 3)						
0 or 1	−0.0184***	−0.0122**	−0.0134**	-0.0095	-0.0121	-0.0104
	(0.0059)	(0.0058)	(0.0065)	(0.0078)	(0.0081)	(0.0088)
4 or more	-0.0027	-0.0030	-0.0053	0.0051	0.0013	-0.0043
	(0.0078)	(0.0084)	(0.0088)	(0.0097)	(0.0114)	(0.0129)
Missing	-0.0135	-0.0132	-0.0218	-0.0225	-0.0172	-0.0291
	(0.0150)	(0.0160)	(0.0157)	(0.0171)	(0.0176)	(0.0205)
Regional information						
Urban district	-0.0026	-0.0029	-0.0039	0.0004	-0.0109	-0.0141
	(0.0070)	(0.0075)	(0.0076)	(0.0079)	(0.0091)	(0.0093)
East Germany	−0.0271**	−0.0240*	-0.0042	-0.0136	-0.0188	-0.0230
	(0.0112)	(0.0133)	(0.0147)	(0.0180)	(0.0138)	(0.0149)
Unemployment ratio (high)	0.0237**	0.0190	-0.0110	0.0092	0.0152	0.0169
	(0.0102)	(0.0123)	(0.0135)	(0.0162)	(0.0127)	(0.0135)

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Table A.4.5: Detailed estimation results for sanction effects based on instrument Z_1 (continued)

Month	1	2	3	4	5	6
GDP per employed person (high)	0.0185^{***}	0.0157^{**}	0.0105	0.0139^{*}	0.0170^{*}	0.0192^{**}
	(0.0069)	(0.0074)	(0.0072)	(0.0080)	(0.0092)	(0.0094)
Welfare ratio (high)	-0.0203^{***}	-0.0201^{***}	-0.0143^{**}	-0.0207^{***}	-0.0174^{**}	-0.0165^{**}
	(0.0065)	(0.0071)	(0.0068)	(0.0074)	(0.0076)	(0.0084)
Current welfare spell						
Months in welfare before 10/2006	-0.0017^{***}	-0.0023^{***}	-0.0031^{***}	-0.0038^{***}	-0.0044^{***}	-0.0046^{***}
	(0.0005)	(0.0004)	(0.0004)	(0.0005)	(0.0005)	(0.0005)
Start after 10/2006 or missing	-0.0218^{**}	-0.0348^{***}	-0.0529^{***}	-0.0626^{***}	-0.0684^{***}	-0.0637^{***}
	(0.0110)	(0.0118)	(0.0104)	(0.0123)	(0.0120)	(0.0135)
Constant	0.0820^{***}	0.0866^{***}	0.1220^{***}	0.1268^{***}	0.1421^{***}	0.1412^{***}
	(0.0192)	(0.0205)	(0.0206)	(0.0234)	(0.0235)	(0.0235)
Observations	15,361					

Remarks: Estimations are based on instrument Z_1, which refers to the sanction strategy of welfare agencies. $Z_1 = 0$ denotes less strict agencies, and $Z_1 = 1$ denotes agencies that frequently apply sanctions. The dependent variable in each estimation and for each month is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0. Displayed are the estimated coefficients and standard errors in brackets. The results refer to the first six months after the (hypothetical) sanction date. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. The standard errors take into account clustering at the agency level.

Table A.4.6: Detailed estimation results for sanction effects based on instrument Z_2

Month	1	2	3	4	5	6
Sanction	0.2802**	0.2402**	0.3594***	0.4496***	0.4950***	0.5802***
	(0.1124)	(0.1216)	(0.1249)	(0.1353)	(0.1437)	(0.1396)
Gender (reference: female)						
Male	0.0131**	0.0151**	0.0061	0.0144*	0.0209***	0.0251***
	(0.0065)	(0.0070)	(0.0079)	(0.0086)	(0.0080)	(0.0093)
Age (reference: 25 to 34 years)						
18 to 24 years	−0.0230**	−0.0199*	−0.0316***	−0.0417***	−0.0421***	−0.0486***
	(0.0104)	(0.0108)	(0.0106)	(0.0109)	(0.0115)	(0.0127)
35 to 44 years	-0.0121	-0.0094	-0.0142	−0.0178*	−0.0224**	-0.0118
	(0.0077)	(0.0079)	(0.0094)	(0.0105)	(0.0110)	(0.0131)
45 to 57 years	−0.0260***	−0.0304***	−0.0376***	−0.0469***	−0.0445***	−0.0432***
	(0.0087)	(0.0092)	(0.0104)	(0.0112)	(0.0121)	(0.0128)
Schooling (reference: secondary general school)						
Intermediate secondary	0.0281***	0.0284***	0.0366***	0.0372***	0.0353***	0.0352***
school	(0.0067)	(0.0066)	(0.0072)	(0.0077)	(0.0082)	(0.0098)
University entrance diploma	0.0413***	0.0396***	0.0470***	0.0421***	0.0392***	0.0370***
	(0.0122)	(0.0120)	(0.0138)	(0.0124)	(0.0127)	(0.0125)
Other or missing	−0.0215***	−0.0282***	-0.0229**	-0.0137	−0.0276**	-0.0279
	(0.0081)	(0.0080)	(0.0097)	(0.0143)	(0.0127)	(0.0188)
Migration background (reference: native Germans)						
Immigrant	−0.0204***	−0.0243***	−0.0287***	−0.0277***	−0.0289***	−0.0214***
	(0.0064)	(0.0058)	(0.0072)	(0.0076)	(0.0075)	(0.0081)
Household size (reference: 2 persons)						
1 person	-0.0105	-0.0003	0.0066	0.0054	0.0106	0.0079
	(0.0074)	(0.0079)	(0.0087)	(0.0102)	(0.0097)	(0.0113)
3 or more persons	0.0127	0.0189**	0.0161*	0.0058	0.0062	0.0162
	(0.0079)	(0.0083)	(0.0090)	(0.0102)	(0.0101)	(0.0111)
Number of children (reference: 1 child)						
No children	0.0289***	0.0269***	0.0252**	0.0230**	0.0214**	0.0294**
	(0.0091)	(0.0094)	(0.0103)	(0.0101)	(0.0107)	(0.0118)
2 or more children	−0.0170**	−0.0246***	−0.0266***	−0.0216**	−0.0199*	−0.0290**
	(0.0084)	(0.0085)	(0.0101)	(0.0107)	(0.0118)	(0.0136)
Obstacles to employment						
Disabled	−0.0215**	−0.0225**	-0.0194	-0.0090	−0.0212*	−0.0298**
	(0.0097)	(0.0099)	(0.0124)	(0.0112)	(0.0111)	(0.0121)
Care obligation	−0.0158*	−0.0226**	−0.0280***	-0.0117	−0.0267**	−0.0297**
	(0.0093)	(0.0091)	(0.0098)	(0.0221)	(0.0120)	(0.0140)
Status before receipt of welfare benefits						
(Minor) employment	0.0086	0.0114*	0.0067	0.0070	0.0120	0.0131
	(0.0062)	(0.0063)	(0.0075)	(0.0081)	(0.0076)	(0.0086)
Number of previous unemployment spells (reference: 2 or 3)						
0 or 1	−0.0181***	−0.0120**	−0.0132**	-0.0095	-0.0121	-0.0105
	(0.0060)	(0.0059)	(0.0066)	(0.0078)	(0.0080)	(0.0086)
4 or more	-0.0066	-0.0045	-0.0079	0.0051	0.0022	-0.0029
	(0.0084)	(0.0082)	(0.0092)	(0.0096)	(0.0107)	(0.0121)
Missing	-0.0147	-0.0137	-0.0227	-0.0225	-0.0169	-0.0287
	(0.0151)	(0.0158)	(0.0158)	(0.0168)	(0.0173)	(0.0200)
Regional information						
Urban district	-0.0057	-0.0041	-0.0061	0.0004	-0.0101	-0.0130
	(0.0070)	(0.0075)	(0.0073)	(0.0080)	(0.0088)	(0.0096)
East Germany	−0.0228**	−0.0223*	-0.0012	-0.0136	-0.0199	−0.0245*
	(0.0102)	(0.0125)	(0.0130)	(0.0173)	(0.0136)	(0.0147)
Unemployment ratio (high)	0.0240***	0.0192	-0.0107	0.0092	0.0151	0.0168
	(0.0092)	(0.0119)	(0.0119)	(0.0162)	(0.0130)	(0.0138)

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Table A.4.6: Detailed estimation results for sanction effects based on instrument Z_2 (continued)

Month	1	2	3	4	5	6
GDP per employed person (high)	0.0179**	0.0155**	0.0101	0.0139*	0.0171*	0.0194**
	(0.0070)	(0.0074)	(0.0072)	(0.0080)	(0.0090)	(0.0091)
Welfare ratio (high)	−0.0188***	−0.0195***	−0.0132**	−0.0207***	−0.0178**	−0.0170**
	(0.0064)	(0.0069)	(0.0062)	(0.0073)	(0.0075)	(0.0083)
Current welfare spell						
Months in welfare before 10/2006	−0.0015***	−0.0022***	−0.0030***	−0.0038***	−0.0045***	−0.0047***
	(0.0004)	(0.0004)	(0.0004)	(0.0006)	(0.0005)	(0.0005)
Start after 10/2006 or missing	-0.0154	−0.0323***	−0.0484***	−0.0626***	−0.0699***	−0.0660***
	(0.0101)	(0.0107)	(0.0096)	(0.0125)	(0.0117)	(0.0132)
Constant	0.0648***	0.0799***	0.1098***	0.1266***	0.1462***	0.1474***
	(0.0145)	(0.0155)	(0.0169)	(0.0196)	(0.0192)	(0.0208)
Observations			15,361			

Remarks: Estimations are based on instrument Z_2, which refers to the observed sanction rates within welfare agencies. $Z_2 = 0$ denotes welfare agencies with a sanction rate below the median across all 154 sampled agencies, and $Z_2 = 1$ denotes agencies with a sanction rate above the median. The dependent variable in each estimation and for each month is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0. Displayed are the estimated coefficients and standard errors in brackets. The results refer to the first six months after the (hypothetical) sanction date. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. The standard errors take into account clustering at the agency level.

Table A.4.7: Sanction effects for subgroups of welfare recipients

Month	1	2	3	4	5	6
Total sample						
Sanction	0.2802**	0.2402**	0.3594***	0.4496***	0.4950***	0.5802***
	(0.1124)	(0.1216)	(0.1249)	(0.1353)	(0.1437)	(0.1396)
Observations: 15,361 (of which 667 were sanctioned)						
Individuals registered at centralized welfare agencies						
Sanction	0.1900	0.1405	0.3078*	0.3285**	0.4574***	0.4477***
	(0.1342)	(0.1490)	(0.1571)	(0.1612)	(0.1622)	(0.1547)
Observations: 9,962 (of which 494 were sanctioned)						
Individuals registered at decentralized welfare agencies						
Sanction	0.2890	0.2964	0.3562*	0.3811*	0.3321	0.6903***
	(0.2331)	(0.2361)	(0.1997)	(0.2229)	(0.2293)	(0.2314)
Observations: 5,399 (of which 173 were sanctioned)						
Men						
Sanction	0.2467*	0.1223	0.2692*	0.3384**	0.3633*	0.4309**
	(0.1364)	(0.1383)	(0.1463)	(0.1688)	(0.1854)	(0.1900)
Observations: 7,126 (of which 377 were sanctioned)						
Women						
Sanction	0.3010	0.4252**	0.4885**	0.6114***	0.7000***	0.8216***
	(0.1832)	(0.2109)	(0.2343)	(0.2351)	(0.2313)	(0.2711)
Observations: 8,235 (of which 290 were sanctioned)						
Native Germans						
Sanctions	0.2881**	0.2478	0.3734**	0.4161***	0.5084***	0.5143***
	(0.1417)	(0.1513)	(0.1502)	(0.1572)	(0.1664)	(0.1547)
Observations: 11,554 (of which 495 were sanctioned)						
Immigrants						
Sanction	0.2304	0.1923	0.3049	0.4566**	0.4536**	0.7281***
	(0.1746)	(0.1792)	(0.2078)	(0.2164)	(0.2190)	(0.2626)
Observations: 3,807 (of which 172 were sanctioned)						

Remarks: Estimations are based on instrument Z_2, which refers to the observed sanction rates within welfare agencies. $Z_2 = 0$ denotes welfare agencies with a sanction rate below the median across all 154 sampled agencies, and $Z_2 = 1$ denotes agencies with a sanction rate above the median. The dependent variable in each estimation and for each month is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0. Displayed are the estimated sanction effects for the indicated subgroups of welfare recipients and standard errors in brackets. The results refer to the first six months after the (hypothetical) sanction date. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. The standard errors take into account clustering at the agency level. All estimations include the covariates depicted in tables A.4.5 and A.4.6 as further regressors.

Appendix to Chapter 5

Fig. A.5.1: Density distributions of the estimated propensity scores for participants in Temporary Extra Jobs

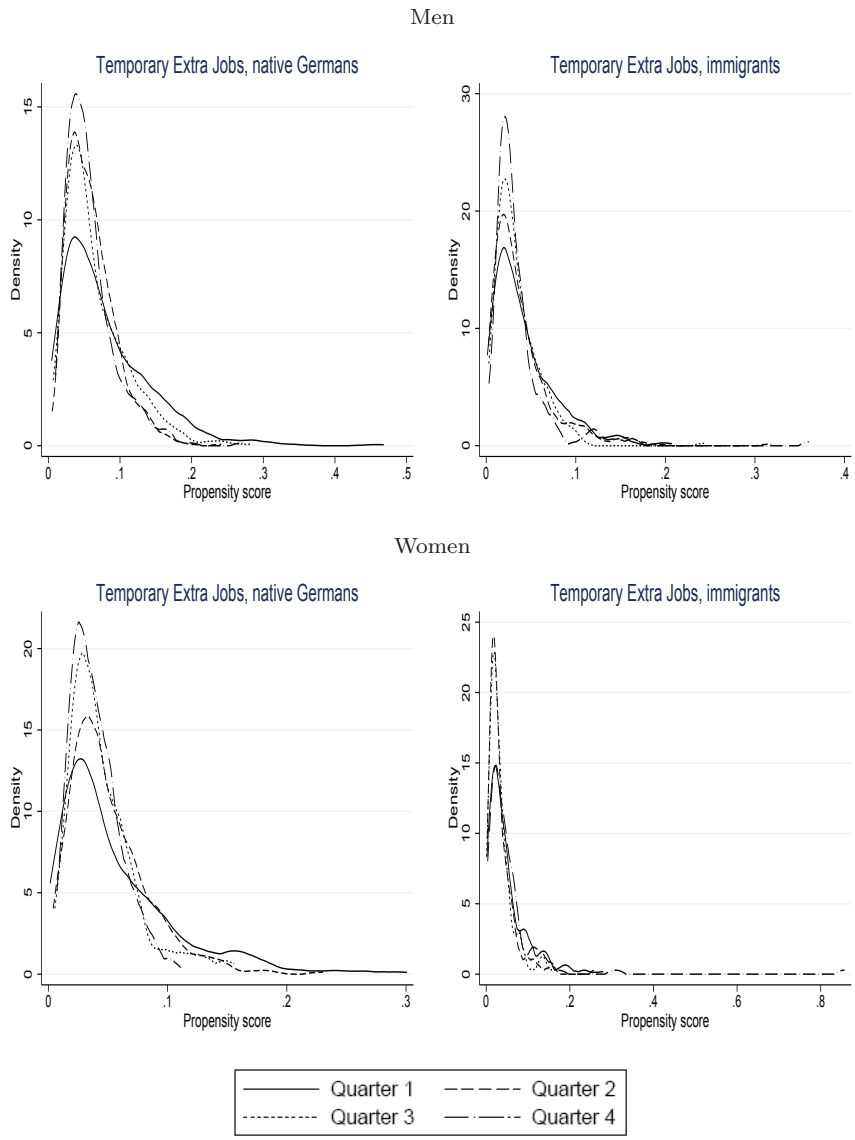

Fig. A.5.2: Density distributions of the estimated propensity scores for male participants in aptitude tests and job search training

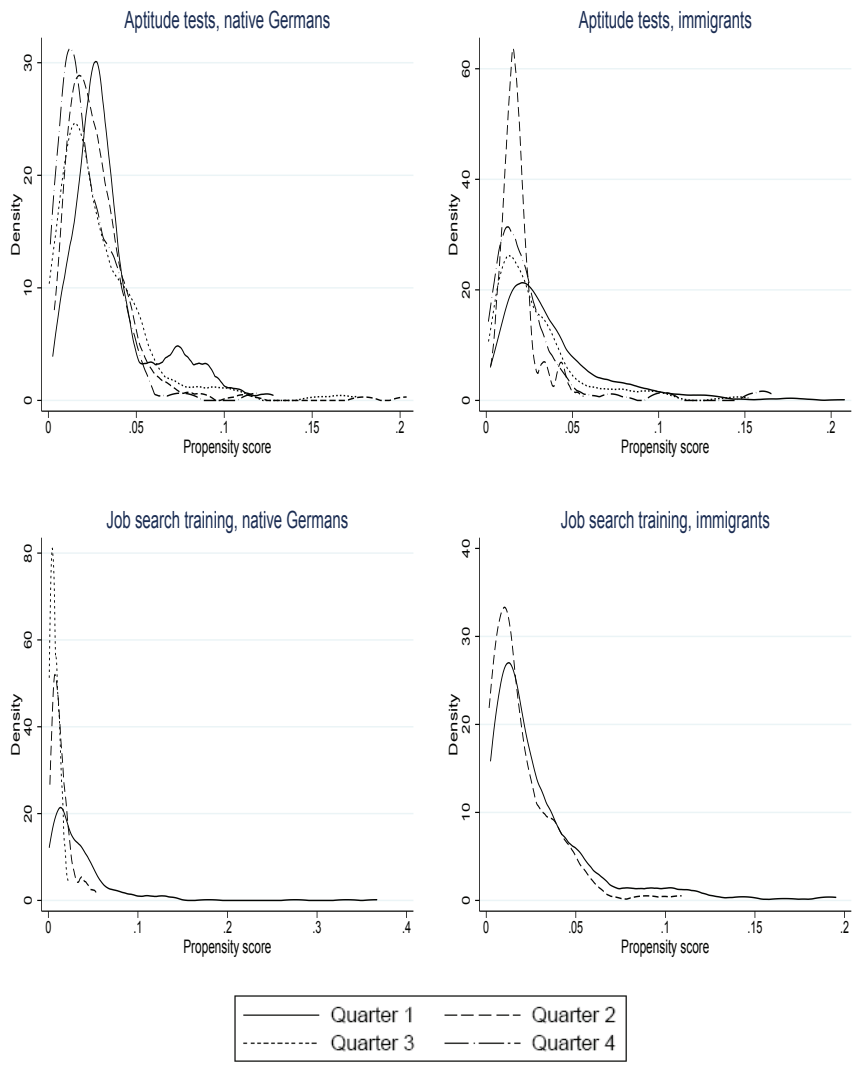

Fig. A.5.3: Density distributions of the estimated propensity scores for male participants in skill provision and combined training programs

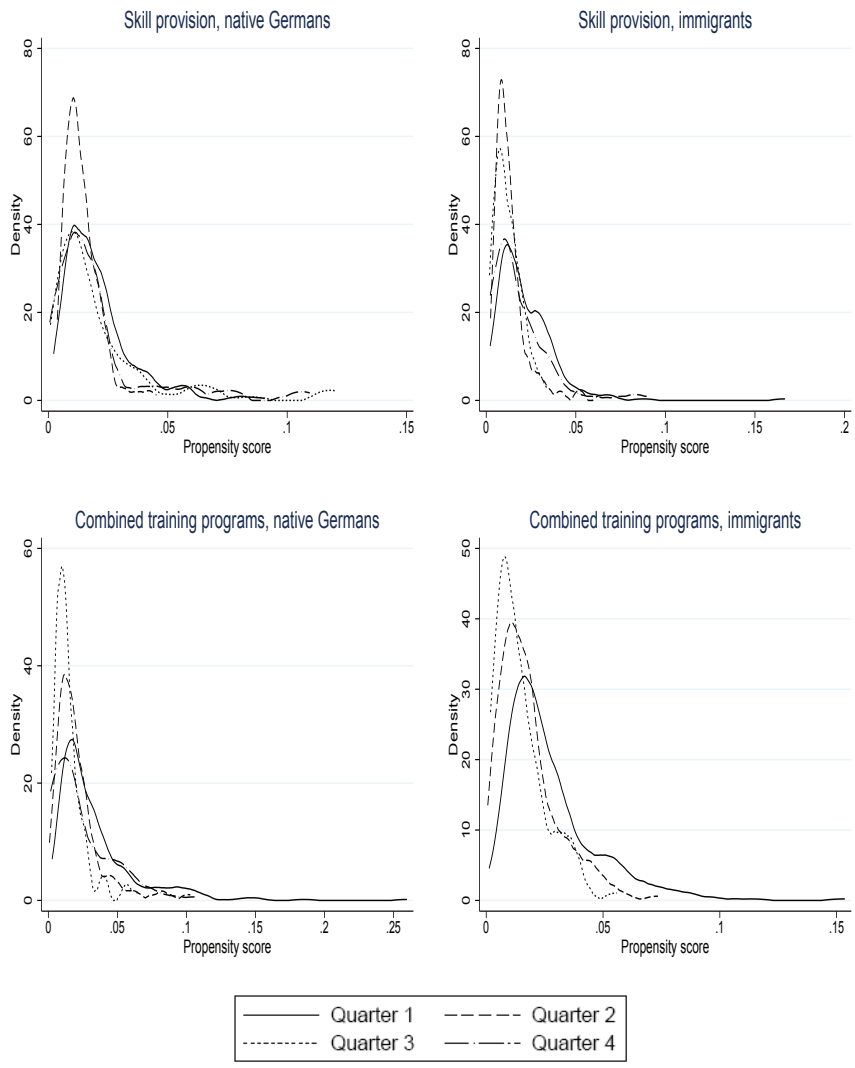

Fig. A.5.4: Density distributions of the estimated propensity scores for female participants in aptitude tests and job search training

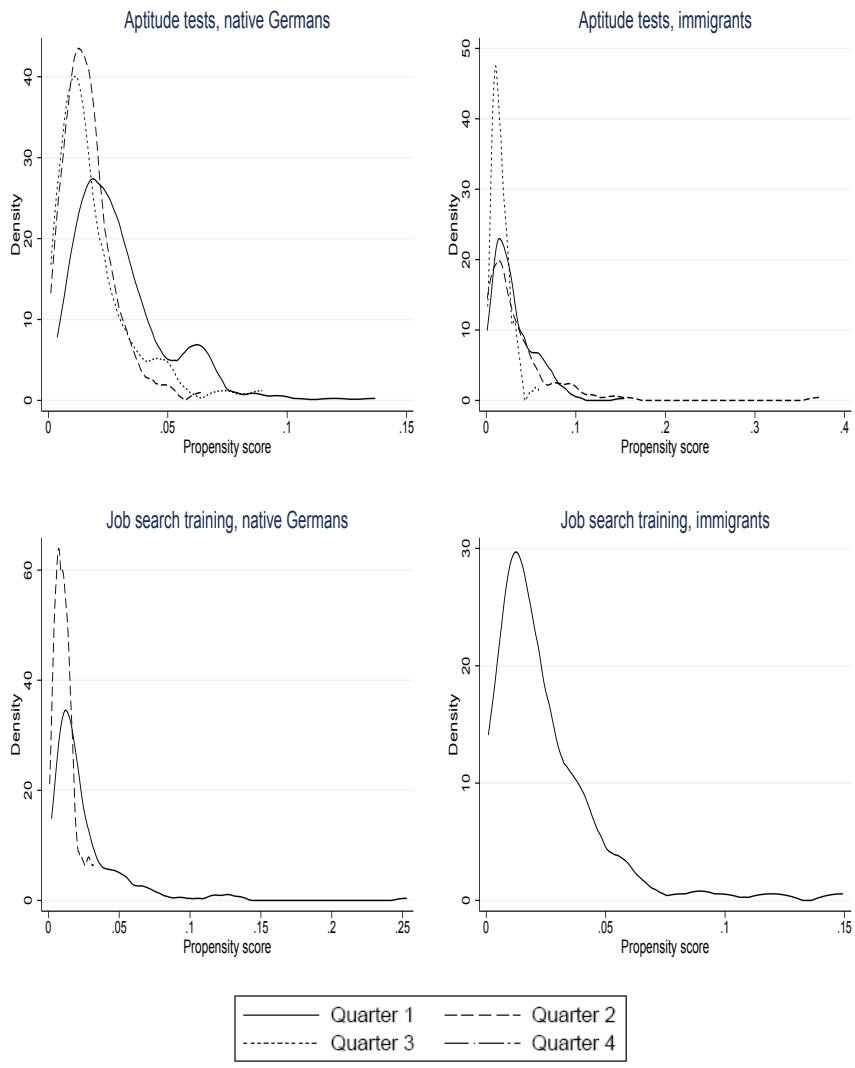

Fig. A.5.5: Density distributions of the estimated propensity scores for female participants in skill provision and combined training programs

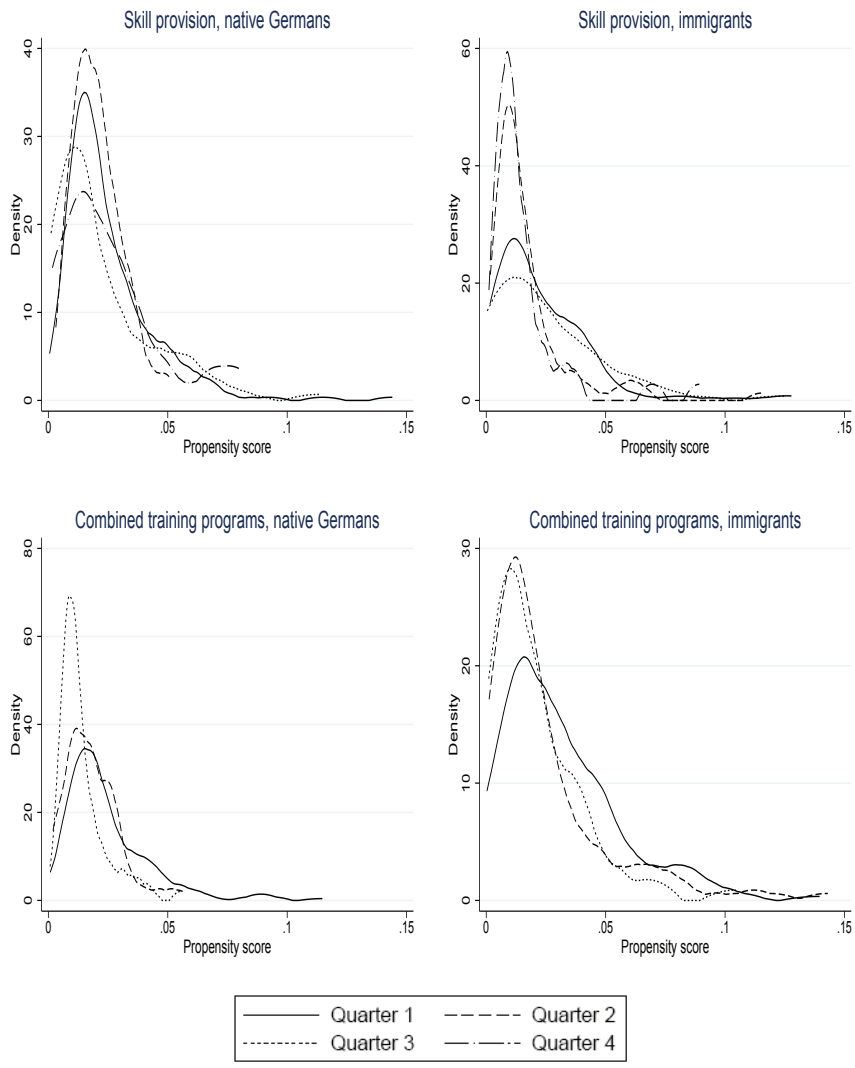

Table A.5.1: Descriptive statistics of the estimation samples for Temporary Extra Jobs and native males

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Controls	Treated	p-value	Controls	Treated	p-value	Controls	Treated	p-value	Controls	Treated	p-value
Age												
18-24	0.227	0.438	0.000	0.200	0.344	0.000	0.161	0.341	0.000	0.132	0.245	0.000
25-34	0.322	0.210	0.000	0.318	0.247	0.000	0.316	0.219	0.000	0.304	0.295	0.729
35-49	0.346	0.251	0.000	0.364	0.317	0.014	0.389	0.324	0.005	0.413	0.336	0.005
50-57	0.104	0.101	0.743	0.118	0.092	0.042	0.135	0.116	0.239	0.151	0.124	0.173
Schooling												
No school leaving certificate	0.090	0.194	0.000	0.093	0.155	0.000	0.097	0.156	0.000	0.093	0.159	0.000
Secondary general school	0.462	0.508	0.007	0.460	0.479	0.322	0.459	0.522	0.007	0.472	0.537	0.020
Intermediate secondary school	0.230	0.168	0.000	0.227	0.201	0.117	0.225	0.183	0.032	0.224	0.162	0.008
Vocational diploma (*Fachabitur*)	0.060	0.029	0.000	0.057	0.039	0.043	0.056	0.032	0.023	0.052	0.035	0.171
University entrance diploma	0.101	0.044	0.000	0.103	0.067	0.003	0.103	0.046	0.000	0.100	0.056	0.008
Missing	0.058	0.056	0.830	0.061	0.060	0.877	0.059	0.061	0.888	0.059	0.050	0.506
Family status												
Single	0.653	0.736	0.000	0.647	0.711	0.001	0.635	0.693	0.011	0.619	0.664	0.095
Household size												
Number of persons	1.419	1.394	0.423	1.400	1.335	0.065	1.399	1.345	0.198	1.409	1.398	0.836
Region												
East Germany	0.045	0.029	0.024	0.046	0.045	0.914	0.048	0.044	0.718	0.047	0.053	0.621
Labor market history in the final 2 years before (potential) program start (measured in number of half-months)												
Employment	12.577	7.577	0.000	10.002	7.882	0.000	7.483	6.903	0.223	5.814	5.295	0.272
Unemployment	15.344	17.162	0.000	19.867	20.484	0.213	24.569	25.131	0.304	28.870	29.012	0.809
Job seeking while employed	1.045	1.749	0.000	0.914	1.277	0.002	0.833	0.971	0.306	0.722	1.071	0.016
Program participation	2.808	3.042	0.353	2.603	2.939	0.216	2.414	2.665	0.395	2.215	2.206	0.979
Out of labor force	16.226	18.471	0.000	14.613	15.418	0.206	12.700	12.331	0.575	10.379	10.416	0.956
Labor market history in the final 4 years before (potential) program start (measured in number of half-months)												
Employment	30.502	19.932	0.000	27.094	22.055	0.000	23.503	22.573	0.411	21.325	19.469	0.136
Unemployment	24.901	26.869	0.007	30.072	31.083	0.216	35.808	36.255	0.636	41.001	42.316	0.222
Job seeking while employed	1.465	2.474	0.000	1.383	1.807	0.009	1.336	1.444	0.565	1.270	1.528	0.229
Program participation	4.798	5.057	0.481	4.760	5.256	0.242	4.794	5.183	0.439	4.760	4.292	0.419
Out of labor force	34.335	41.669	0.000	32.690	35.799	0.016	30.558	30.545	0.993	27.644	28.395	0.633
Observations	21,285	891		14,825	672		10,371	475		7,702	339	

Remarks: Displayed are means of selected variables for native male participants (treated) and non-participants (controls) in Temporary Extra Jobs during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for treated and controls.

Table A.5.2: Descriptive statistics of the estimation samples for Temporary Extra Jobs and native females

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Controls	Treated	p-value	Controls	Treated	p-value	Controls	Treated	p-value	Controls	Treated	p-value
Age												
18-24	0.251	0.435	0.000	0.218	0.392	0.000	0.179	0.326	0.000	0.145	0.246	0.000
25-34	0.306	0.153	0.000	0.302	0.176	0.000	0.291	0.264	0.350	0.287	0.246	0.241
35-49	0.333	0.299	0.152	0.356	0.308	0.070	0.388	0.293	0.003	0.403	0.386	0.654
50-57	0.110	0.113	0.835	0.124	0.124	0.999	0.142	0.117	0.281	0.165	0.123	0.144
Schooling												
No school leaving certificate	0.077	0.113	0.008	0.081	0.112	0.035	0.086	0.117	0.090	0.091	0.129	0.090
Secondary general school	0.353	0.432	0.001	0.365	0.403	0.148	0.386	0.410	0.448	0.399	0.386	0.737
Intermediate secondary school	0.277	0.216	0.007	0.262	0.274	0.614	0.256	0.251	0.864	0.251	0.263	0.714
Vocational diploma (*Fachabitur*)	0.062	0.060	0.900	0.060	0.040	0.131	0.053	0.046	0.613	0.048	0.047	0.961
University entrance diploma	0.125	0.075	0.003	0.118	0.081	0.032	0.105	0.075	0.134	0.093	0.076	0.441
Missing	0.106	0.103	0.831	0.114	0.089	0.156	0.114	0.100	0.527	0.119	0.099	0.440
Family status												
Single	0.507	0.580	0.004	0.475	0.597	0.000	0.434	0.561	0.000	0.396	0.480	0.029
Household size												
Number of persons	1.657	1.397	0.000	1.692	1.524	0.002	1.742	1.623	0.072	1.792	1.561	0.004
Region												
East Germany	0.042	0.040	0.887	0.041	0.035	0.547	0.039	0.046	0.600	0.039	0.035	0.786
Labor market history in the final 2 years before (potential) program start (measured in number of half-months)												
Employment	13.486	9.379	0.000	10.990	8.179	0.000	8.650	6.527	0.005	6.680	6.053	0.409
Unemployment	13.089	14.663	0.023	18.178	20.121	0.006	23.434	23.858	0.595	28.209	28.146	0.943
Job seeking while employed	1.032	1.528	0.004	0.918	1.548	0.000	0.792	0.929	0.471	0.651	1.193	0.008
Program participation	2.103	2.442	0.317	1.904	2.254	0.290	1.765	1.824	0.869	1.628	1.795	0.654
Out of labor force	18.289	19.987	0.078	16.010	15.899	0.905	13.359	14.862	0.119	10.832	10.813	0.984
Labor market history in the final 4 years before (potential) program start (measured in number of half-months)												
Employment	30.669	22.274	0.000	27.634	23.363	0.005	24.726	19.364	0.002	22.285	20.538	0.354
Unemployment	19.398	21.101	0.101	25.123	26.870	0.114	31.282	31.351	0.958	36.920	38.211	0.396
Job seeking while employed	1.417	2.070	0.005	1.360	1.988	0.007	1.305	1.669	0.189	1.228	1.708	0.133
Program participation	3.582	3.927	0.501	3.436	3.839	0.450	3.417	3.155	0.675	3.266	3.544	0.688
Out of labor force	40.935	46.628	0.002	38.447	39.939	0.427	35.270	40.460	0.014	32.300	32.000	0.898
Observations	14,650	398		10,319	347		7,404	239		5,590	171	

Remarks: Displayed are means of selected variables for native female participants (treated) and non-participants (controls) in Temporary Extra Jobs during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for treated and controls.

Table A.5.3: Descriptive statistics of the estimation samples for Temporary Extra Jobs and immigrant males

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Controls	Treated	p-value	Controls	Treated	p-value	Controls	Treated	p-value	Controls	Treated	p-value
Age												
18-24	0.156	0.400	0.000	0.132	0.299	0.000	0.112	0.245	0.000	0.099	0.189	0.000
25-34	0.389	0.250	0.000	0.393	0.248	0.000	0.390	0.316	0.017	0.386	0.279	0.002
35-49	0.365	0.269	0.000	0.377	0.358	0.480	0.392	0.292	0.001	0.398	0.418	0.576
50-57	0.090	0.082	0.577	0.098	0.096	0.899	0.106	0.146	0.041	0.117	0.114	0.926
Schooling												
No school leaving certificate	0.238	0.271	0.111	0.241	0.293	0.029	0.251	0.285	0.229	0.259	0.244	0.636
Secondary general school	0.414	0.460	0.052	0.408	0.406	0.931	0.401	0.462	0.049	0.398	0.428	0.393
Intermediate secondary school	0.137	0.105	0.054	0.133	0.131	0.934	0.132	0.123	0.666	0.130	0.100	0.198
Vocational diploma (*Fachabitur*)	0.036	0.023	0.169	0.036	0.042	0.543	0.035	0.020	0.194	0.034	0.040	0.639
University entrance diploma	0.078	0.070	0.539	0.081	0.069	0.405	0.081	0.055	0.138	0.079	0.065	0.464
Missing	0.097	0.070	0.060	0.101	0.060	0.013	0.100	0.055	0.019	0.101	0.124	0.268
Family status												
Single	0.338	0.537	0.000	0.327	0.457	0.000	0.316	0.466	0.000	0.299	0.388	0.006
Household size												
Number of persons	2.098	1.981	0.083	2.098	1.881	0.004	2.114	1.917	0.025	2.125	2.060	0.510
Region												
East Germany	0.044	0.009	0.000	0.046	0.024	0.056	0.048	0.024	0.077	0.050	0.010	0.010
Labor market history in the final 2 years before (potential) program start (measured in number of half-months)												
Employment	13.739	8.474	0.000	11.309	8.337	0.000	8.993	6.854	0.002	6.938	5.383	0.015
Unemployment	15.438	16.689	0.056	19.863	21.803	0.005	24.429	27.415	0.000	28.739	28.866	0.868
Job seeking while employed	0.689	1.530	0.000	0.611	1.036	0.003	0.538	0.933	0.008	0.456	0.358	0.498
Program participation	2.556	3.236	0.044	2.329	2.869	0.127	2.160	2.901	0.046	1.955	2.408	0.223
Out of labor force	15.578	18.070	0.006	13.888	13.955	0.942	11.880	9.897	0.031	9.913	10.985	0.220
Labor market history in the final 4 years before (potential) program start (measured in number of half-months)												
Employment	31.433	21.388	0.000	28.719	22.743	0.000	26.045	22.680	0.032	23.680	20.662	0.065
Unemployment	25.329	26.953	0.132	30.172	33.797	0.002	35.307	41.150	0.000	40.188	40.532	0.808
Job seeking while employed	1.054	2.442	0.000	1.009	1.672	0.002	0.973	1.577	0.010	0.890	0.861	0.903
Program participation	4.279	4.671	0.422	4.128	5.101	0.073	4.053	5.344	0.031	3.987	4.468	0.467
Out of labor force	33.905	40.547	0.000	31.971	32.687	0.704	29.621	25.249	0.030	27.255	29.478	0.296
Observations	21,194	428		15,666	335		11,448	253		9,069	201	

Remarks: Displayed are means of selected variables for immigrant male participants (treated) and non-participants (controls) in Temporary Extra Jobs during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for treated and controls.

Table A.5.4: Descriptive statistics of the estimation samples for Temporary Extra Jobs and immigrant females

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Controls	Treated	p-value	Controls	Treated	p-value	Controls	Treated	p-value	Controls	Treated	p-value
Age												
18-24	0.197	0.400	0.000	0.168	0.321	0.000	0.135	0.238	0.001	0.113	0.275	0.000
25-34	0.363	0.236	0.000	0.364	0.256	0.004	0.355	0.286	0.105	0.348	0.248	0.029
35-49	0.326	0.255	0.024	0.343	0.345	0.942	0.369	0.341	0.522	0.386	0.330	0.235
50-57	0.113	0.109	0.844	0.125	0.077	0.062	0.141	0.135	0.857	0.153	0.147	0.862
Schooling												
No school leaving certificate	0.225	0.177	0.091	0.231	0.256	0.440	0.246	0.230	0.682	0.257	0.183	0.080
Secondary general school	0.297	0.382	0.006	0.296	0.321	0.483	0.299	0.421	0.003	0.296	0.394	0.025
Intermediate secondary school	0.148	0.209	0.012	0.143	0.185	0.128	0.137	0.135	0.947	0.129	0.147	0.576
Vocational diploma (*Fachabitur*)	0.042	0.023	0.162	0.041	0.018	0.130	0.036	0.056	0.243	0.034	0.028	0.718
University entrance diploma	0.083	0.082	0.964	0.078	0.095	0.396	0.075	0.071	0.878	0.073	0.092	0.446
Missing	0.206	0.127	0.004	0.211	0.125	0.006	0.207	0.087	0.001	0.212	0.156	0.156
Family status												
Single	0.239	0.395	0.000	0.220	0.357	0.000	0.197	0.341	0.000	0.177	0.339	0.000
Household size												
Number of persons	2.126	1.732	0.000	2.148	1.804	0.000	2.186	1.929	0.015	2.221	1.798	0.000
Region												
East Germany	0.040	0.027	0.353	0.041	0.042	0.951	0.041	0.040	0.921	0.045	0.037	0.671
Labor market history in the final 2 years before (potential) program start (measured in number of half-months)												
Employment	11.394	7.695	0.000	9.345	10.107	0.453	7.421	7.468	0.962	5.905	5.972	0.940
Unemployment	12.253	17.064	0.000	17.315	17.494	0.862	22.585	26.095	0.002	27.185	28.532	0.213
Job seeking while employed	0.757	1.350	0.005	0.697	0.869	0.437	0.597	0.786	0.405	0.504	0.211	0.169
Program participation	1.466	3.159	0.000	1.312	2.137	0.029	1.271	1.722	0.251	1.263	2.642	0.000
Out of labor force	22.131	18.732	0.012	19.331	17.393	0.163	16.127	11.929	0.003	13.142	10.642	0.050
Labor market history in the final 4 years before (potential) program start (measured in number of half-months)												
Employment	24.980	19.236	0.003	22.555	24.673	0.314	20.338	21.214	0.698	18.917	18.908	0.997
Unemployment	17.840	25.250	0.000	23.403	22.429	0.543	29.452	34.341	0.008	34.658	36.963	0.226
Job seeking while employed	1.086	2.273	0.000	1.046	1.643	0.063	0.969	1.349	0.246	0.877	0.404	0.143
Program participation	2.483	5.209	0.000	2.370	3.988	0.009	2.377	3.913	0.027	2.348	5.092	0.000
Out of labor force	49.611	44.032	0.030	46.625	43.268	0.228	42.864	35.183	0.011	39.199	34.633	0.134
Observations	12,944	220		9,674	168		7,235	126		5,705	109	

Remarks: Displayed are means of selected variables for immigrant female participants (treated) and non-participants (controls) in Temporary Extra Jobs during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for treated and controls.

Table A.5.5: Means of selected variables for male participants in Temporary Extra Jobs

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.438	0.400	0.189	0.344	0.299	0.150	0.341	0.245	0.007	0.245	0.189	0.133
25-34	0.210	0.250	0.101	0.247	0.248	0.980	0.219	0.316	0.004	0.295	0.279	0.685
35-49	0.251	0.269	0.501	0.317	0.358	0.190	0.324	0.292	0.380	0.336	0.418	0.057
50-57	0.101	0.082	0.264	0.092	0.096	0.867	0.116	0.146	0.240	0.124	0.114	0.744
Schooling												
No school leaving certificate	0.194	0.271	0.002	0.155	0.293	0.000	0.156	0.285	0.000	0.159	0.244	0.016
Secondary general school	0.508	0.460	0.102	0.479	0.406	0.028	0.522	0.462	0.126	0.537	0.428	0.014
Intermediate secondary school	0.168	0.105	0.002	0.201	0.131	0.006	0.183	0.123	0.035	0.162	0.100	0.042
Vocational diploma (*Fachabitur*)	0.029	0.023	0.544	0.039	0.042	0.813	0.032	0.020	0.354	0.035	0.040	0.794
University entrance diploma	0.044	0.070	0.044	0.067	0.069	0.920	0.046	0.055	0.594	0.056	0.065	0.682
Missing	0.056	0.070	0.320	0.060	0.060	0.991	0.061	0.055	0.756	0.050	0.124	0.002
Family status												
Single	0.736	0.537	0.000	0.711	0.457	0.000	0.693	0.466	0.000	0.664	0.388	0.000
Household size												
Number of persons	1.394	1.981	0.000	1.335	1.881	0.000	1.345	1.917	0.000	1.398	2.060	0.000
Region												
East Germany	0.029	0.009	0.024	0.045	0.024	0.103	0.044	0.024	0.164	0.053	0.010	0.010
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	7.577	8.474	0.178	7.882	8.337	0.531	6.903	6.854	0.948	5.295	5.383	0.901
Unemployment	17.162	16.689	0.546	20.484	21.803	0.115	25.131	27.415	0.012	29.012	28.866	0.879
Job seeking while employed	1.749	1.530	0.396	1.277	1.036	0.334	0.971	0.933	0.872	1.071	0.358	0.005
Program participation	3.042	3.236	0.626	2.939	2.869	0.879	2.665	2.901	0.645	2.206	2.408	0.674
Out of labor force	18.471	18.070	0.694	15.418	13.955	0.167	12.331	9.897	0.023	10.416	10.985	0.597
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	19.932	21.388	0.313	22.055	22.743	0.669	22.573	22.680	0.953	19.469	20.662	0.541
Unemployment	26.869	26.953	0.949	31.083	33.797	0.062	36.255	41.150	0.002	42.316	40.532	0.333
Job seeking while employed	2.474	2.442	0.928	1.807	1.672	0.692	1.444	1.577	0.671	1.528	0.861	0.064
Program participation	5.057	4.671	0.491	5.256	5.101	0.825	5.183	5.344	0.856	4.292	4.468	0.823
Out of labor force	41.669	40.547	0.573	35.799	32.687	0.148	30.545	25.249	0.023	28.395	29.478	0.669
Observations	891	428		672	335		475	253		339	201	

Remarks: Displayed are means of selected variables for native German and immigrant men participating in Temporary Extra Jobs during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.6: Means of selected variables for female participants in Temporary Extra Jobs

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.435	0.400	0.404	0.392	0.321	0.121	0.326	0.238	0.079	0.246	0.275	0.582
25-34	0.153	0.236	0.010	0.176	0.256	0.034	0.264	0.286	0.652	0.246	0.248	0.969
35-49	0.299	0.255	0.241	0.308	0.345	0.401	0.293	0.341	0.343	0.386	0.330	0.347
50-57	0.113	0.109	0.881	0.124	0.077	0.112	0.117	0.135	0.625	0.123	0.147	0.565
Schooling												
No school leaving certificate	0.113	0.177	0.026	0.112	0.256	0.000	0.117	0.230	0.005	0.129	0.183	0.212
Secondary general school	0.432	0.382	0.224	0.403	0.321	0.072	0.410	0.421	0.846	0.386	0.394	0.887
Intermediate secondary school	0.216	0.209	0.839	0.274	0.185	0.027	0.251	0.135	0.010	0.263	0.147	0.021
Vocational diploma (*Fachabitur*)	0.060	0.023	0.034	0.040	0.018	0.181	0.046	0.056	0.690	0.047	0.028	0.420
University entrance diploma	0.075	0.082	0.775	0.081	0.095	0.581	0.075	0.071	0.893	0.076	0.092	0.642
Missing	0.103	0.127	0.360	0.089	0.125	0.209	0.100	0.087	0.687	0.099	0.156	0.159
Family status												
Single	0.580	0.395	0.000	0.597	0.357	0.000	0.561	0.341	0.000	0.480	0.339	0.021
Household size												
Number of persons	1.397	1.732	0.000	1.524	1.804	0.001	1.623	1.929	0.007	1.561	1.798	0.041
Region												
East Germany	0.040	0.027	0.407	0.035	0.042	0.690	0.046	0.040	0.779	0.035	0.037	0.944
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	9.379	7.695	0.120	8.179	10.107	0.090	6.527	7.468	0.416	6.053	5.972	0.946
Unemployment	14.663	17.064	0.046	20.121	17.494	0.032	23.858	26.095	0.093	28.146	28.532	0.780
Job seeking while employed	1.528	1.350	0.615	1.548	0.869	0.089	0.929	0.786	0.641	1.193	0.211	0.008
Program participation	2.442	3.159	0.170	2.254	2.137	0.849	1.824	1.722	0.836	1.795	2.642	0.206
Out of labor force	19.987	18.732	0.419	15.899	17.393	0.348	14.862	11.929	0.065	10.813	10.642	0.911
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	22.274	19.236	0.159	23.363	24.673	0.594	19.364	21.214	0.484	20.538	18.908	0.574
Unemployment	21.101	25.250	0.024	26.870	22.429	0.013	31.351	34.341	0.168	38.211	36.963	0.618
Job seeking while employed	2.070	2.273	0.676	1.988	1.643	0.573	1.669	1.349	0.511	1.708	0.404	0.007
Program participation	3.927	5.209	0.124	3.839	3.988	0.874	3.155	3.913	0.427	3.544	5.092	0.205
Out of labor force	46.628	44.032	0.382	39.939	43.268	0.300	40.460	35.183	0.124	32.000	34.633	0.471
Observations	398	220		347	168		239	126		171	109	

Remarks: Displayed are means of selected variables for native German and immigrant women participating in Temporary Extra Jobs during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.7: Means of selected variables for male participants in aptitude tests

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.302	0.190	0.000	0.281	0.155	0.001	0.250	0.140	0.019	0.261	0.163	0.100
25-34	0.337	0.413	0.016	0.365	0.469	0.022	0.400	0.448	0.420	0.272	0.480	0.003
35-49	0.312	0.346	0.260	0.308	0.329	0.627	0.321	0.357	0.533	0.435	0.316	0.093
50-57	0.049	0.051	0.886	0.046	0.047	0.968	0.029	0.056	0.255	0.033	0.041	0.766
Schooling												
No school leaving certificate	0.087	0.205	0.000	0.065	0.239	0.000	0.064	0.175	0.004	0.120	0.194	0.162
Secondary general school	0.473	0.444	0.367	0.542	0.460	0.075	0.464	0.476	0.850	0.446	0.418	0.706
Intermediate secondary school	0.250	0.161	0.001	0.242	0.150	0.013	0.286	0.175	0.027	0.293	0.153	0.020
Vocational diploma (*Fachabitur*)	0.057	0.049	0.579	0.062	0.038	0.238	0.079	0.021	0.025	0.033	0.031	0.938
University entrance diploma	0.085	0.094	0.629	0.054	0.061	0.738	0.071	0.077	0.861	0.098	0.102	0.923
Missing	0.047	0.047	0.973	0.035	0.052	0.361	0.036	0.077	0.134	0.011	0.102	0.007
Family status												
Single	0.673	0.422	0.000	0.685	0.324	0.000	0.593	0.308	0.000	0.663	0.286	0.000
Household size												
Number of persons	1.509	2.116	0.000	1.465	2.169	0.000	1.607	2.238	0.000	1.565	2.296	0.000
Region												
East Germany	0.055	0.022	0.009	0.058	0.033	0.203	0.036	0.084	0.089	0.054	0.092	0.325
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	14.225	15.855	0.080	11.915	12.300	0.735	9.536	10.203	0.606	8.283	9.755	0.302
Unemployment	15.570	15.839	0.737	19.781	20.000	0.836	24.707	23.776	0.496	28.543	27.847	0.626
Job seeking while employed	1.134	0.569	0.002	1.185	0.610	0.033	0.764	0.517	0.362	0.478	0.490	0.967
Program participation	2.406	3.009	0.166	2.608	3.056	0.478	2.686	1.895	0.306	3.696	1.888	0.035
Out of labor force	14.665	12.728	0.084	12.512	12.033	0.737	10.307	11.608	0.431	7.000	8.020	0.523
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	34.970	35.221	0.892	32.392	32.944	0.822	28.871	29.378	0.863	30.022	30.204	0.959
Unemployment	23.793	26.377	0.045	29.423	28.803	0.714	34.550	33.622	0.687	39.500	39.663	0.949
Job seeking while employed	1.448	0.944	0.024	1.854	0.962	0.037	1.157	0.832	0.353	0.935	0.939	0.992
Program participation	4.469	5.509	0.134	5.300	5.667	0.726	5.229	4.350	0.501	6.293	3.531	0.057
Out of labor force	31.320	27.949	0.114	27.031	27.624	0.833	26.193	27.818	0.653	19.250	21.663	0.537
Observations	507	448		260	213		140	143		92	98	

Remarks: Displayed are means of selected variables for native German and immigrant men participating in aptitude tests during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.8: Means of selected variables for female participants in aptitude tests

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.293	0.239	0.200	0.253	0.151	0.090	0.258	0.263	0.946	0.232	0.042	0.006
25-34	0.279	0.361	0.062	0.364	0.453	0.217	0.226	0.289	0.401	0.304	0.542	0.014
35-49	0.347	0.361	0.752	0.343	0.302	0.554	0.452	0.355	0.253	0.429	0.396	0.738
50-57	0.081	0.039	0.072	0.040	0.093	0.149	0.065	0.092	0.555	0.036	0.021	0.655
Schooling												
No school leaving certificate	0.071	0.139	0.014	0.061	0.209	0.003	0.065	0.132	0.197	0.071	0.208	0.042
Secondary general school	0.354	0.356	0.964	0.364	0.302	0.381	0.403	0.342	0.463	0.339	0.312	0.774
Intermediate secondary school	0.313	0.200	0.007	0.384	0.128	0.000	0.371	0.237	0.088	0.446	0.188	0.005
Vocational diploma (*Fachabitur*)	0.057	0.056	0.939	0.081	0.105	0.578	0.065	0.053	0.768	0.071	0.083	0.822
University entrance diploma	0.104	0.094	0.727	0.051	0.116	0.103	0.048	0.079	0.473	0.054	0.083	0.550
Missing	0.101	0.156	0.078	0.061	0.140	0.071	0.048	0.158	0.040	0.018	0.125	0.030
Family status												
Single	0.552	0.372	0.000	0.495	0.256	0.001	0.419	0.289	0.113	0.464	0.125	0.000
Household size												
Number of persons	1.502	1.933	0.000	1.455	2.140	0.000	1.645	2.171	0.006	1.464	2.417	0.000
Region												
East Germany	0.067	0.022	0.029	0.071	0.012	0.049	0.113	0.053	0.196	0.107	0.104	0.961
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	16.751	15.578	0.460	12.889	9.988	0.161	9.500	9.329	0.934	6.554	8.271	0.368
Unemployment	11.727	12.750	0.405	19.434	17.977	0.473	20.968	22.724	0.363	28.500	23.771	0.024
Job seeking while employed	0.862	0.822	0.889	1.061	0.942	0.836	1.855	0.763	0.139	1.018	0.333	0.147
Program participation	2.340	1.828	0.409	1.222	2.081	0.213	1.726	1.408	0.693	1.732	2.750	0.399
Out of labor force	16.320	17.022	0.697	13.394	17.012	0.144	13.952	13.776	0.945	10.196	12.875	0.288
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	36.158	32.106	0.172	32.626	22.314	0.014	25.710	27.895	0.649	23.018	20.938	0.655
Unemployment	17.569	17.694	0.944	25.152	26.500	0.671	27.661	28.355	0.822	38.536	30.542	0.043
Job seeking while employed	1.148	1.067	0.819	1.465	1.523	0.932	3.065	1.171	0.053	1.839	0.521	0.034
Program participation	4.158	3.039	0.216	3.141	3.163	0.984	3.516	2.382	0.450	2.518	7.854	0.032
Out of labor force	36.966	42.094	0.133	33.616	42.500	0.079	36.048	36.197	0.979	30.089	36.146	0.313
Observations	297	180		99	86		62	76		56	48	

Remarks: Displayed are means of selected variables for native German and immigrant women participating in aptitude tests during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.9: Means of selected variables for male participants in job search training

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.320	0.252	0.072	0.259	0.264	0.932	0.208	0.122	0.259	0.226	0.081	0.096
25-34	0.343	0.363	0.622	0.328	0.373	0.479	0.354	0.429	0.458	0.323	0.486	0.176
35-49	0.264	0.300	0.340	0.353	0.327	0.680	0.333	0.388	0.581	0.323	0.351	0.806
50-57	0.073	0.085	0.577	0.060	0.036	0.405	0.104	0.061	0.447	0.129	0.081	0.524
Schooling												
No school leaving certificate	0.053	0.233	0.000	0.095	0.255	0.001	0.104	0.102	0.973	0.097	0.324	0.024
Secondary general school	0.545	0.452	0.027	0.405	0.436	0.637	0.542	0.490	0.614	0.484	0.459	0.844
Intermediate secondary school	0.231	0.167	0.055	0.259	0.145	0.035	0.250	0.224	0.771	0.194	0.054	0.077
Vocational diploma (*Fachabitur*)	0.036	0.022	0.322	0.052	0.045	0.828	0.000	0.041	0.161	0.097	0.027	0.230
University entrance diploma	0.076	0.093	0.473	0.086	0.064	0.522	0.104	0.102	0.973	0.097	0.054	0.509
Missing	0.059	0.033	0.142	0.103	0.055	0.176	0.000	0.041	0.161	0.032	0.081	0.402
Family status												
Single	0.686	0.400	0.000	0.672	0.391	0.000	0.646	0.408	0.019	0.710	0.270	0.000
Household size												
Number of persons	1.465	2.126	0.000	1.362	2.100	0.000	1.438	2.184	0.008	1.452	2.000	0.047
Region												
East Germany	0.069	0.037	0.089	0.026	0.027	0.948	0.062	0.000	0.077	0.000	0.000	-
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	19.208	21.585	0.117	13.112	14.055	0.634	8.875	13.510	0.063	7.226	6.946	0.900
Unemployment	9.759	11.393	0.094	15.310	15.373	0.966	25.500	23.796	0.431	24.774	28.919	0.106
Job seeking while employed	0.957	0.870	0.748	1.000	0.627	0.254	0.812	0.082	0.011	1.742	0.135	0.075
Program participation	2.617	2.085	0.383	2.319	2.482	0.867	2.438	3.347	0.505	0.774	4.270	0.014
Out of labor force	15.459	12.067	0.020	16.259	15.464	0.718	10.375	7.265	0.212	13.484	7.730	0.063
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	40.092	44.663	0.100	33.284	32.009	0.749	30.812	43.102	0.019	23.065	26.432	0.573
Unemployment	17.297	18.481	0.449	22.121	23.309	0.587	34.917	31.857	0.398	31.258	41.757	0.013
Job seeking while employed	1.327	1.141	0.564	1.345	1.391	0.932	2.167	0.408	0.014	2.710	0.216	0.090
Program participation	4.142	3.656	0.572	4.267	4.255	0.994	5.562	4.653	0.709	3.645	7.919	0.137
Out of labor force	33.142	28.059	0.068	34.983	35.036	0.990	22.542	15.980	0.172	35.323	19.676	0.033
Observations	303	270		116	110		48	49		31	37	

Remarks: Displayed are means of selected variables for native German and immigrant men participating in job search training during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.10: Means of selected variables for female participants in job search training

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.337	0.331	0.909	0.333	0.304	0.725	0.250	0.308	0.632	0.227	0.071	0.233
25-34	0.326	0.331	0.940	0.319	0.357	0.655	0.219	0.308	0.451	0.318	0.429	0.515
35-49	0.253	0.254	0.975	0.275	0.268	0.926	0.344	0.385	0.753	0.364	0.429	0.707
50-57	0.084	0.085	0.987	0.072	0.071	0.982	0.188	0.000	0.019	0.091	0.071	0.842
Schooling												
No school leaving certificate	0.068	0.127	0.082	0.072	0.179	0.070	0.094	0.154	0.493	0.091	0.214	0.31
Secondary general school	0.353	0.441	0.124	0.435	0.411	0.789	0.344	0.154	0.104	0.318	0.571	0.141
Intermediate secondary school	0.311	0.161	0.003	0.261	0.196	0.400	0.312	0.346	0.790	0.364	0.071	0.05
Vocational diploma (*Fachabitur*)	0.100	0.051	0.125	0.058	0.036	0.566	0.000	0.038	0.271	0.000	0.071	0.215
University entrance diploma	0.084	0.136	0.152	0.130	0.107	0.693	0.094	0.192	0.287	0.136	0.000	0.158
Missing	0.084	0.085	0.987	0.043	0.071	0.503	0.156	0.115	0.660	0.091	0.071	0.842
Family status												
Single	0.563	0.322	0.000	0.522	0.286	0.007	0.469	0.269	0.124	0.500	0.143	0.03
Household size												
Number of persons	1.568	1.907	0.002	1.565	1.929	0.030	1.438	2.269	0.001	1.636	1.929	0.412
Region												
East Germany	0.068	0.042	0.345	0.072	0.125	0.325	0.000	0.000		0.045	0.071	0.749
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	19.921	19.059	0.691	13.913	14.500	0.828	15.500	7.538	0.027	9.545	11.214	0.675
Unemployment	9.105	9.780	0.634	16.087	17.571	0.531	21.344	18.346	0.276	25.727	29.214	0.341
Job seeking while employed	0.900	0.975	0.842	0.812	0.732	0.867	0.562	0.885	0.575	2.227	0.571	0.375
Program participation	2.168	1.280	0.286	0.841	2.232	0.179	1.406	0.577	0.265	1.455	1.286	0.911
Out of labor force	15.905	16.907	0.658	16.348	12.964	0.251	9.188	20.654	0.001	9.045	5.714	0.402
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	40.221	36.398	0.327	31.029	30.857	0.973	37.500	16.115	0.003	29.318	31.000	0.86
Unemployment	14.763	15.161	0.856	21.145	26.464	0.165	29.719	21.385	0.072	32.182	43.429	0.16
Job seeking while employed	1.132	1.381	0.593	1.203	0.875	0.539	1.250	1.308	0.945	4.318	1.071	0.447
Program participation	3.368	2.983	0.742	1.333	3.929	0.112	2.094	0.846	0.174	2.727	1.286	0.622
Out of labor force	36.516	40.076	0.396	41.290	33.875	0.211	25.438	56.346	0.000	27.455	19.214	0.398
Observations	190	118		69	56		32	26		22	14	

Remarks: Displayed are means of selected variables for native German and immigrant women participating in job search training during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.11: Means of selected variables for male participants in skill provision

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.299	0.152	0.000	0.213	0.171	0.362	0.245	0.110	0.021	0.087	0.075	0.794
25-34	0.330	0.413	0.044	0.281	0.329	0.368	0.415	0.524	0.148	0.275	0.463	0.023
35-49	0.312	0.390	0.056	0.412	0.404	0.882	0.298	0.354	0.433	0.565	0.328	0.005
50-57	0.059	0.045	0.476	0.094	0.096	0.949	0.043	0.012	0.229	0.072	0.134	0.238
Schooling												
No school leaving certificate	0.059	0.201	0.000	0.119	0.205	0.039	0.085	0.183	0.055	0.087	0.164	0.176
Secondary general school	0.500	0.447	0.213	0.519	0.397	0.033	0.404	0.585	0.016	0.478	0.463	0.857
Intermediate secondary school	0.233	0.182	0.142	0.188	0.171	0.712	0.298	0.098	0.001	0.261	0.134	0.065
Vocational diploma (*Fachabitur*)	0.073	0.034	0.045	0.038	0.027	0.621	0.053	0.061	0.825	0.043	0.045	0.971
University entrance diploma	0.094	0.091	0.909	0.112	0.103	0.784	0.128	0.024	0.011	0.087	0.060	0.546
Missing	0.042	0.045	0.828	0.025	0.096	0.008	0.032	0.049	0.570	0.043	0.134	0.063
Family status												
Single	0.635	0.360	0.000	0.625	0.377	0.000	0.766	0.341	0.000	0.681	0.299	0.000
Household size												
Number of persons	1.514	2.201	0.000	1.438	2.062	0.000	1.319	2.329	0.000	1.536	2.239	0.002
Region												
East Germany	0.035	0.023	0.402	0.044	0.062	0.484	0.106	0.049	0.161	0.029	0.015	0.580
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	16.264	18.189	0.151	12.137	13.144	0.517	9.064	12.512	0.054	6.812	8.701	0.260
Unemployment	15.052	14.750	0.769	20.031	18.575	0.277	23.319	23.927	0.693	27.812	28.731	0.618
Job seeking while employed	1.385	0.629	0.006	0.637	0.712	0.785	1.011	0.463	0.154	0.449	0.299	0.573
Program participation	3.354	2.523	0.158	2.356	2.466	0.872	2.223	1.939	0.711	4.203	2.597	0.203
Out of labor force	11.944	11.909	0.980	12.837	13.103	0.883	12.383	9.159	0.116	8.725	7.672	0.605
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	38.326	39.360	0.684	31.169	29.603	0.629	27.149	35.610	0.041	26.913	29.358	0.570
Unemployment	22.635	22.799	0.918	28.956	28.699	0.910	32.330	32.366	0.989	39.130	39.806	0.833
Job seeking while employed	1.830	1.197	0.091	1.050	1.288	0.608	1.691	1.000	0.319	1.043	0.433	0.294
Program participation	5.955	4.784	0.239	4.506	4.610	0.929	4.649	4.659	0.995	6.478	5.493	0.658
Out of labor force	27.253	27.860	0.828	30.319	31.801	0.690	30.181	22.366	0.080	22.435	20.910	0.752
Observations	288	264		160	146		94	82		69	67	

Remarks: Displayed are means of selected variables for native German and immigrant men participating in skill provision during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.12: Means of selected variables for female participants in skill provision

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.242	0.233	0.849	0.204	0.200	0.944	0.097	0.173	0.217	0.208	0.293	0.364
25-34	0.283	0.391	0.036	0.355	0.338	0.788	0.292	0.250	0.611	0.375	0.244	0.188
35-49	0.365	0.323	0.425	0.388	0.400	0.861	0.528	0.462	0.471	0.354	0.415	0.564
50-57	0.110	0.053	0.068	0.053	0.062	0.757	0.083	0.115	0.555	0.062	0.049	0.782
Schooling												
No school leaving certificate	0.023	0.150	0.000	0.026	0.188	0.000	0.083	0.038	0.319	0.083	0.171	0.216
Secondary general school	0.374	0.323	0.333	0.375	0.387	0.853	0.347	0.269	0.360	0.333	0.268	0.511
Intermediate secondary school	0.384	0.263	0.021	0.342	0.125	0.000	0.347	0.288	0.494	0.375	0.195	0.064
Vocational diploma (*Fachabitur*)	0.068	0.030	0.123	0.046	0.075	0.364	0.028	0.077	0.211	0.062	0.024	0.393
University entrance diploma	0.114	0.158	0.239	0.112	0.138	0.570	0.111	0.192	0.208	0.021	0.122	0.059
Missing	0.037	0.075	0.111	0.099	0.087	0.783	0.083	0.135	0.362	0.125	0.220	0.240
Family status												
Single	0.584	0.286	0.000	0.553	0.287	0.000	0.375	0.269	0.220	0.458	0.220	0.018
Household size												
Number of persons	1.580	1.797	0.033	1.586	2.138	0.000	1.764	2.096	0.102	1.833	2.195	0.115
Region												
East Germany	0.037	0.030	0.747	0.046	0.062	0.593	0.097	0.058	0.429	0.021	0.024	0.911
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	16.849	16.925	0.966	13.401	13.250	0.937	9.889	11.558	0.476	6.229	6.732	0.806
Unemployment	14.644	13.331	0.363	17.211	18.487	0.448	22.042	19.519	0.208	24.312	27.780	0.127
Job seeking while employed	0.858	0.902	0.888	1.007	0.863	0.746	0.847	0.500	0.409	0.271	0.683	0.400
Program participation	2.411	1.774	0.378	2.342	1.700	0.480	2.236	1.519	0.545	2.062	2.146	0.938
Out of labor force	13.237	15.068	0.337	14.039	13.700	0.881	12.986	14.904	0.490	15.125	10.659	0.118
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	36.986	34.805	0.519	33.368	31.013	0.559	24.708	28.500	0.434	18.896	23.732	0.353
Unemployment	22.064	21.098	0.661	22.776	24.275	0.529	31.167	26.654	0.179	32.562	31.634	0.791
Job seeking while employed	1.324	1.361	0.930	1.480	1.138	0.507	1.236	0.885	0.560	0.792	1.561	0.302
Program participation	4.205	3.158	0.324	4.447	2.800	0.248	5.069	2.212	0.183	3.271	2.195	0.401
Out of labor force	31.420	35.579	0.257	33.928	36.775	0.537	33.819	37.750	0.520	40.479	36.878	0.581
Observations	219	133		152	80		72	52		48	41	

Remarks: Displayed are means of selected variables for native German and immigrant women participating in skill provision during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.13: Means of selected variables for male participants in combined training programs

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.351	0.233	0.000	0.328	0.261	0.183	0.256	0.240	0.802	0.254	0.136	0.102
25-34	0.276	0.400	0.000	0.378	0.382	0.934	0.389	0.406	0.810	0.349	0.356	0.939
35-49	0.276	0.300	0.456	0.228	0.306	0.106	0.311	0.312	0.984	0.365	0.407	0.640
50-57	0.098	0.067	0.122	0.067	0.051	0.544	0.044	0.042	0.926	0.032	0.102	0.121
Schooling												
No school leaving certificate	0.117	0.275	0.000	0.100	0.204	0.007	0.100	0.292	0.001	0.032	0.271	0.000
Secondary general school	0.522	0.431	0.011	0.444	0.510	0.234	0.500	0.406	0.201	0.556	0.492	0.483
Intermediate secondary school	0.183	0.119	0.015	0.294	0.121	0.000	0.189	0.167	0.694	0.222	0.102	0.073
Vocational diploma (*Fachabitur*)	0.044	0.033	0.450	0.039	0.038	0.975	0.044	0.031	0.639	0.032	0.034	0.947
University entrance diploma	0.071	0.056	0.390	0.078	0.076	0.963	0.089	0.031	0.097	0.111	0.068	0.408
Missing	0.063	0.086	0.231	0.044	0.051	0.780	0.078	0.073	0.901	0.048	0.034	0.705
Family status												
Single	0.698	0.394	0.000	0.722	0.420	0.000	0.667	0.375	0.000	0.698	0.322	0.000
Household size												
Number of persons	1.412	2.050	0.000	1.333	2.032	0.000	1.422	2.260	0.000	1.270	2.288	0.000
Region												
East Germany	0.020	0.008	0.193	0.028	0.025	0.896	0.022	0.010	0.526	0.032	0.034	0.947
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	12.576	15.992	0.001	10.511	11.261	0.582	9.089	9.896	0.629	5.317	8.746	0.035
Unemployment	14.385	14.192	0.836	18.978	19.854	0.488	23.978	26.010	0.218	28.413	29.322	0.631
Job seeking while employed	0.890	0.558	0.088	1.000	0.516	0.102	1.022	0.677	0.365	0.825	0.254	0.127
Program participation	3.080	2.625	0.407	2.533	2.331	0.777	1.744	1.458	0.669	2.476	2.102	0.703
Out of labor force	17.068	14.633	0.066	14.978	14.038	0.588	12.167	9.958	0.273	10.968	7.576	0.122
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	30.420	36.836	0.002	27.878	30.006	0.471	29.422	29.927	0.895	20.635	30.254	0.022
Unemployment	22.593	22.619	0.985	27.539	30.076	0.198	31.022	36.146	0.052	41.698	41.881	0.961
Job seeking while employed	1.266	1.100	0.552	1.644	1.038	0.178	1.289	1.135	0.727	1.762	0.559	0.074
Program participation	4.837	4.217	0.411	4.939	4.573	0.743	4.167	3.510	0.678	6.397	3.508	0.097
Out of labor force	36.885	31.228	0.026	34.000	30.306	0.290	30.100	25.281	0.268	25.508	19.797	0.261
Observations	410	360		180	157		90	96		63	59	

Remarks: Displayed are means of selected variables for native German and immigrant men participating in combined training programs during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.14: Means of selected variables for female participants in combined training programs

Estimation sample for:	Quarter 1			Quarter 2			Quarter 3			Quarter 4		
	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value	Natives	Immigrants	p-value
Age												
18-24	0.345	0.281	0.169	0.327	0.326	0.986	0.197	0.268	0.358	0.188	0.091	0.234
25-34	0.246	0.421	0.000	0.327	0.253	0.240	0.288	0.500	0.016	0.354	0.545	0.090
35-49	0.345	0.236	0.017	0.265	0.337	0.265	0.394	0.179	0.009	0.375	0.212	0.122
50-57	0.065	0.062	0.907	0.080	0.084	0.905	0.121	0.054	0.197	0.083	0.152	0.344
Schooling												
No school leaving certificate	0.082	0.191	0.001	0.142	0.242	0.065	0.076	0.125	0.367	0.042	0.273	0.003
Secondary general school	0.332	0.303	0.540	0.398	0.389	0.898	0.470	0.536	0.472	0.375	0.485	0.331
Intermediate secondary school	0.254	0.169	0.037	0.292	0.147	0.013	0.227	0.107	0.081	0.375	0.121	0.011
Vocational diploma (*Fachabitur*)	0.086	0.039	0.058	0.035	0.063	0.354	0.061	0.054	0.869	0.062	0.061	0.973
University entrance diploma	0.073	0.084	0.682	0.035	0.032	0.880	0.045	0.054	0.838	0.062	0.000	0.147
Missing	0.172	0.213	0.295	0.097	0.126	0.509	0.121	0.125	0.950	0.083	0.061	0.705
Family status												
Single	0.565	0.427	0.006	0.584	0.326	0.000	0.500	0.339	0.075	0.542	0.212	0.003
Household size												
Number of persons	1.612	1.899	0.007	1.425	2.084	0.000	1.455	2.196	0.000	1.729	2.061	0.208
Region												
East Germany	0.017	0.006	0.289	0.044	0.011	0.149	0.015	0.000	0.359	0.042	0.061	0.703
Labor market history in the final 2 years before program start (measured in number of half-months)												
Employment	12.608	12.792	0.902	10.761	12.779	0.275	8.424	6.857	0.452	8.083	9.848	0.460
Unemployment	11.440	11.292	0.905	18.920	17.242	0.346	21.879	23.625	0.433	26.833	29.152	0.309
Job seeking while employed	0.716	0.820	0.747	0.743	1.032	0.435	0.773	0.786	0.978	0.958	0.485	0.412
Program participation	2.478	1.494	0.128	2.690	1.074	0.039	1.015	1.750	0.370	2.667	2.000	0.629
Out of labor force	20.759	21.601	0.670	14.885	15.874	0.673	15.909	14.982	0.742	9.458	6.515	0.249
Labor market history in the final 4 years before program start (measured in number of half-months)												
Employment	26.310	27.646	0.647	26.142	29.726	0.341	25.773	21.286	0.358	22.896	29.364	0.282
Unemployment	16.599	15.876	0.690	25.947	23.221	0.324	30.167	32.929	0.472	35.958	40.697	0.288
Job seeking while employed	1.043	0.961	0.836	1.257	1.758	0.309	0.985	1.125	0.799	1.062	0.515	0.350
Program participation	3.901	1.966	0.032	4.354	1.632	0.016	1.894	2.679	0.542	6.458	4.121	0.450
Out of labor force	48.147	49.551	0.706	38.301	39.663	0.770	37.182	37.982	0.897	29.625	21.303	0.202
Observations	232	178		113	95		66	56		48	33	

Remarks: Displayed are means of selected variables for native German and immigrant women participating in combined training programs during the indicated quarter of welfare receipt. The p-values derive from equality-of-means tests of the displayed variables for natives and immigrants.

Table A.5.15: Common support condition for the estimation of treatment effects

	Temporary Extra Jobs	Aptitude tests	Job search training	Skill provision	Combined training programs
Quarter 1					
Native males	889/891	507/507	303/303	288/288	410/410
Native females	347/347	297/297	189/190	218/219	188/188
Immigrant males	428/428	448/448	270/270	244/245	360/360
Immigrant females	193/193	180/180	102/102	133/133	135/135
Quarter 2					
Native males	671/672	260/260	116/116	160/160	180/180
Native females	347/347	99/99	69/69	152/152	113/113
Immigrant males	334/335	213/213	110/110	146/146	157/157
Immigrant females	167/168	85/86	-	80/80	95/95
Quarter 3					
Native males	474/475	138/140	48/48	94/94	90/90
Native females	239/239	62/62	-	72/72	66/66
Immigrant males	252/253	143/143	-	82/82	96/96
Immigrant females	126/126	76/76	-	52/52	56/56
Quarter 4					
Native males	339/339	91/92	-	69/69	63/63
Native females	171/171	-	-	48/48	-
Immigrant males	201/201	98/98	-	67/67	-
Immigrant females	108/109	-	-	41/41	-

Remarks: Displayed is the fraction of treated individuals satisfying the common support condition on all treated individuals in the respective sample. On support are those individuals whose propensity score is no smaller than the smallest and no larger than the largest propensity score in the respective group of non-treated individuals. Treated observations off support are not considered in the matching analysis to estimate treatment effects. Examples: All 507 native men participating in aptitude tests in the first quarter of welfare receipt are on support and thus part of the matching analysis. One of the 190 native females starting job search training during the first quarter of welfare receipt had to be dropped from the matching analysis because of the common support condition.

Table A.5.16: Smith and Todd (2005b) balancing test for the estimation of treatment effects

		Men				Women			
		$p > .1$	$p > .05$	$p > .01$	Regressors	$p > .1$	$p > .05$	$p > .01$	Regressors
Temporary Extra Jobs									
Quarter 1	Natives	60	63	66	75	75	77	79	85
	Immigrants	56	59	61	62	49	52	58	59
Quarter 2	Natives	51	52	53	56	44	45	46	49
	Immigrants	50	52	56	58	39	40	41	47
Quarter 3	Natives	49	52	58	62	33	37	40	42
	Immigrants	44	45	46	48	52	56	56	57
Quarter 4	Natives	46	48	53	54	33	33	35	35
	Immigrants	40	45	54	59	56	60	63	68
Aptitude tests									
Quarter 1	Natives	28	31	35	37	42	44	44	46
	Immigrants	66	70	72	74	40	42	44	44
Quarter 2	Natives	50	54	56	58	38	38	39	39
	Immigrants	36	36	38	40	51	53	54	59
Quarter 3	Natives	36	37	38	38	38	40	42	45
	Immigrants	66	67	68	74	27	29	30	32
Quarter 4	Natives	30	31	33	35	/	/	/	/
	Immigrants	45	48	53	55	/	/	/	/
Job search training									
Quarter 1	Natives	58	61	63	65	41	43	44	48
	Immigrants	51	54	59	66	47	48	49	50
Quarter 2	Natives	41	42	45	46	31	32	32	32
	Immigrants	73	74	77	80	/	/	/	/
Quarter 3	Natives	23	24	24	26	/	/	/	/
	Immigrants	/	/	/	/	/	/	/	/
Quarter 4	Natives	/	/	/	/	/	/	/	/
	Immigrants	/	/	/	/	/	/	/	/
Skill provision									
Quarter 1	Natives	48	51	52	54	44	46	49	52
	Immigrants	55	55	60	62	51	53	53	55
Quarter 2	Natives	33	36	37	42	35	35	36	38
	Immigrants	34	35	37	39	63	65	67	69
Quarter 3	Natives	57	58	61	68	40	43	47	50
	Immigrants	32	32	32	33	40	40	41	44
Quarter 4	Natives	45	49	52	55	34	34	36	39
	Immigrants	53	56	58	59	25	26	27	29
Combined training programs									
Quarter 1	Natives	51	55	56	62	36	39	40	44
	Immigrants	76	77	80	83	52	56	56	59
Quarter 2	Natives	50	53	54	57	32	32	32	34
	Immigrants	43	43	44	44	40	42	43	48
Quarter 3	Natives	34	36	37	38	27	28	29	30
	Immigrants	27	28	28	31	33	33	34	35
Quarter 4	Natives	36	36	38	39	/	/	/	/
	Immigrants	/	/	/	/	/	/	/	/

Remarks: The figures displayed denote the number of regressors for which the Smith and Todd (2005b) balancing test was passed for the respective significance level indicated by column. The number of regressors included in the respective propensity score specification is presented in the column *Regressors*. Cells marked by a / indicate that the number of treated individuals in the respective cell was too low to estimate valid treatment effects.

Table A.5.17: Selected propensity score specifications

	(1)	(2)	(3)	(4)
	Program participation		Immigrant fixed effect	
	Skill provision	Temporary Extra Jobs	Skill provision	Temporary Extra Jobs
	Male natives	Female immigrants	Men	Women
	Quarter 1	Quarter 1	Quarter 1	Quarter 1
Age (reference: 25 to 34 years)				
18 to 24 years	0.1127	0.4683***	−0.5463***	-0.2281
35 to 49 years	-0.0927	0.0664	-0.1287	−0.4136**
50 to 57 years	−0.2893***	-0.0682	-0.1997	−0.4298*
Schooling (reference: no school leaving certificate)				
Secondary general school	0.2008**	0.1909**	−0.5556***	-0.3160
Intermediate general school	0.1659	0.2254	−0.5534**	-0.2400
Vocational diploma (*Fachabitur*)	0.2456*	-0.2012	-0.3588	−0.7656**
University entrance diploma	0.1416	0.1929	-0.3588	0.0682
Missing	-0.1359	0.1503	-0.6397	0.3817
Professional qualification (reference: none)				
Vocational school	0.2160	0.0244	-0.5986	-0.2196
Off-the-job training	0.1896	0.1175	−0.5929*	-0.2480
Apprenticeship	-0.0063	-0.1205	−0.7575***	−0.6959***
Degree at college of higher education	0.0864		−1.2939**	
University degree	0.1841	-0.2987	−0.7136*	-0.2826
Missing	0.3685***	0.1130	−0.6561**	-0.2552
Household composition and total household size				
Number of UBII recipients	0.1334	0.2413*	−0.6057*	-0.2955
Number of social allowance recipients	0.0365	0.1156	-0.6796	0.6744
Number of UBII recipients under age 18	-0.1281	-0.1580	0.3470	0.3108
Number of children under age 15	-0.0083	0.0421	-0.2926	0.0551
Total household size	-0.0385	−0.1280*	0.4088***	0.2372
Personal information				
Household head	-0.0480	0.1855*	-0.1242	0.0319
Married	0.1343	-0.0858	0.4230*	1.1171***
Regional location (reference: North Rhine-Westphalia)				
Schleswig-Holstein	0.4204***	0.4993***	-0.1756	0.7771**
Hamburg	0.3123***	0.3476**	0.1026	-0.1210
Lower Saxony	0.0951	0.0451	0.2957	0.0632
Bremen	-0.1644	-0.1430	0.1276	-0.1527
Hesse	-0.1302	0.0316	-0.1090	0.6270**
Rhineland-Palatinate	0.1323	-0.1675	0.0159	-0.4993
Baden-Wuerttemberg	0.0076	0.0052	0.1259	0.1593
Bavaria	0.0546	−0.1977*	0.3784*	0.0449
Saarland	-0.0469	0.2920	-0.1524	0.2256
Berlin	-0.0651	-0.1067	0.4325*	-0.2658
Saxony	-0.6022	0.724		-0.7921
Saxony-Anhalt	0.2900	-0.2120	0.1921	
Thuringia	-0.3599	0.4298		
Brandenburg				-0.3231
Mecklenburg-Western Pomerania				-0.3288
Further regional information				
East Germany	-0.0061	-0.2101		0.4105

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Table A.5.17: Selected propensity score specifications (continued)

	(1)	(2)	(3)	(4)
	Program participation		Immigrant fixed effect	
	Skill provision	Temporary Extra Jobs	Skill provision	Temporary Extra Jobs
	Male natives	Female immigrants	Men	Women
	Quarter 1	Quarter 1	Quarter 1	Quarter 1
Number of employment spells within the following intervals before (potential) program participation:				
0 to 6 months	0.1328***		0.1683	
6 to 12 months	0.0860		0.3012*	
12 to 24 months	0.0044		0.1596	
24 to 36 months	-0.0321		0.1411	
36 to 48 months	-0.0455		-0.1127	
48 to 72 months	-0.0321		-0.0474	
Number of unemployment spells within the following intervals before (potential) program participation:				
0 to 6 months	0.1328***		-0.0800	
6 to 12 months	0.0860		-0.0872	
12 to 24 months	0.0044			
24 to 36 months	-0.0321			
36 to 48 months	-0.0455			
48 to 72 months	-0.0321			
Number of ALMP within the following intervals before (potential) program participation:				
0 to 6 months	0.1775**		0.2174	
6 to 12 months	-0.1364		0.1008	
12 to 24 months	0.0562			
24 to 36 months	0.1180*			
36 to 48 months	-0.1036			
48 to 72 months	0.0766			
Number of spells job seeking while employed within the following intervals before (potential) program participation:				
0 to 6 months			-0.1100	
6 to 12 months			0.2666	
Number of spells out of labor force within the following intervals before (potential) program participation:				
0 to 6 months	−0.1165**		-0.0458	
6 to 12 months	0.0200		-0.1968	
12 to 24 months	0.0369			
24 to 36 months	-0.0678			
36 to 48 months	−0.0018*			
48 to 72 months	0.0125			
Number of half-months employed within the following intervals before (potential) program participation:				
0 to 6 months		−0.0372**		0.0332
6 to 12 months		0.0681*		-0.0077
12 to 24 months		0.0179		
24 to 36 months		-0.0140		
36 to 48 months		0.0023		
48 to 72 months		-0.0060		
Number of half-months unemployed within the following intervals before (potential) program participation:				
0 to 6 months		-0.0234		0.0349
6 to 12 months		0.0904***		0.0192
12 to 24 months		0.0413***		
24 to 36 months		-0.0046		
36 to 48 months		-0.0008		

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Table A.5.17: Selected propensity score specifications (continued)

	(1)	(2)	(3)	(4)
	Program participation		Immigrant fixed effect	
	Skill provision	Temporary Extra Jobs	Skill provision	Temporary Extra Jobs
	Male natives	Female immigrants	Men	Women
	Quarter 1	Quarter 1	Quarter 1	Quarter 1
48 to 72 months		-0.0052		
Number of half-months in ALMP within the following intervals before (potential) program participation:				
0 to 6 months				0.0551
6 to 12 months		0.0075		-0.0087
Number of half-months job seeking while employed within the following interval before (potential) program participation:				
12 to 24 months		0.0786^{***}		
Number of half-months out of labor force within the following intervals before (potential) program participation:				
0 to 6 months		-0.0346^{**}		0.0243
6 to 12 months		0.0820^{**}		0.0055
12 to 24 months		0.0326^{**}		
24 to 36 months		-0.0208^{**}		
36 to 48 months		0.0073		
48 to 72 months		-0.0059		
Fraction within the following labor market states during the final 2 years before (potential) program participation:				
Employment		0.2083		
Unemployment		0.0548		
Participation in ALMP		1.6575^{***}		
Out of labor force		-0.0009		
Further information on the labor market history				
No employment during 2001 to 2005	-0.0115	0.0439	-0.3053	-0.4955^{***}
Constant	-2.3906^{***}	-3.3564^{***}	0.3281	-0.5915
Observations	21,573	10,219	532	540
McFadden-R^2	0.0444	0.1086	0.1874	0.1606

Remarks: Column (1) displays the propensity score specification used to estimate the employment effects of participation in skill provision during the first quarter of welfare receipt for native men. The dependent variable distinguishes between individuals who participate in skill provision during the first quarter of welfare receipt and individuals who do not participate at that time. Accordingly, column (2) shows the propensity score specification for the estimation of treatment effects of Temporary Extra Jobs for female immigrants during the first quarter of welfare receipt. Columns (3) and (4) illustrate the propensity score specifications for the estimation of immigrant fixed effects in case of treated men in skill provision and treated women in Temporary Extra Jobs, respectively. Here, the dependent variables distinguish between immigrants and natives treated during the first quarter of welfare receipt. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$.

Table A.5.18: Smith and Todd (2005b) balancing test for the estimation of immigrant fixed effects

	Men				Women			
	$p > .1$	$p > .05$	$p > .01$	Regressors	$p > .1$	$p > .05$	$p > .01$	Regressors
Temporary Extra Jobs	51	53	55	59	40	40	41	43
Aptitude tests	50	53	57	57	63	63	65	65
Job search training	63	64	68	69	68	70	70	70
Skill provision	43	44	46	47	50	50	51	52
Combined training programs	44	46	48	50	78	80	81	81

Remarks: The figures displayed denote the number of regressors for which the Smith and Todd (2005b) balancing test was passed for the respective significance level indicated by column. The number of regressors included in the respective propensity score specification is presented in the column *Regressors*.

Table A.5.19: Common support condition for the estimation of immigrant fixed effects

	Temporary Extra Jobs	Aptitude tests	Job search training	Skill provision	Combined training programs
Males	420/428	429/448	235/270	233/244	333/360
Females	182/193	170/180	97/102	106/133	116/135

Remarks: Displayed is the fraction of treated immigrants satisfying the common support condition on all treated immigrants in the respective sample. On support are those immigrants whose propensity score is no smaller than the smallest and no larger than the largest propensity score in the respective group of treated native Germans. Treated immigrants off support are not considered for the estimation of immigrant fixed effects. Example: 420 out of 428 immigrants participating in Temporary Extra Jobs in the first quarter of welfare receipt are on support and thus part of the analysis. The remaining 8 individuals had to be dropped from the analysis because of the common support condition.

Table A.5.20: Mean values of the outcome variable self-sufficient employment

Month after inflow into welfare	Men		Women	
	Natives	Immigrants	Natives	Immigrants
1	0.0086	0.0069	0.0084	0.0046
2	0.0334	0.0239	0.0273	0.0135
3	0.0654	0.0463	0.0472	0.0236
4	0.0896	0.0649	0.0673	0.0339
5	0.1134	0.0839	0.0833	0.0439
6	0.1420	0.1079	0.1091	0.0562
7	0.1672	0.1277	0.1291	0.0686
8	0.1794	0.1385	0.1408	0.0753
9	0.1901	0.1479	0.1502	0.0801
10	0.1993	0.1581	0.1588	0.0873
11	0.2081	0.1648	0.1658	0.0909
12	0.2203	0.1734	0.1788	0.0986
13	0.2310	0.1868	0.1882	0.1058
14	0.2396	0.1942	0.1960	0.1115
15	0.2501	0.2029	0.2028	0.1187
16	0.2590	0.2118	0.2075	0.1245
17	0.2669	0.2196	0.2128	0.1284
18	0.2761	0.2314	0.2214	0.1375

Remarks: Displayed are means of the outcome variable self-sufficient employment. For each month, the outcome variable is defined to be 1 if an individual is employed and no longer receives welfare benefits. Otherwise, the variable is 0.

Table A.5.21: Numbers of observations in subsamples of immigrants

	Temporary Extra Jobs		Aptitude tests		Job search training		Skill provision		Combined training programs	
	Controls	Treated	Controls	Treated	Controls	Treated	Controls	Treated	Controls	Treated
Men										
Turkish	6,855	139	6,855	130	6,855	96	6,855	85	6,855	125
Eastern European	3,399	83	3,399	83	3,399	41	3,399	41	3,399	62
Southern European	4,919	103	4,919	94	4,919	61	4,919	53	4,919	86
Women										
Turkish	3,677	62	3,677	60	3,677	42	3,677	34	3,677	51
Eastern European	3,366	66	3,366	49	3,366	33	3,366	47	3,366	59
Southern European	2,610	37	2,610	35	2,610	22	2,610	25	2,610	28

Remarks: Displayed are the number of observations in the subsamples of immigrants used to estimate the effects of Temporary Extra Jobs and short-term training programs on the employment chances of individuals with a Turkish, an Eastern European, or a Southern European migration background. The table distinguishes between participants (treated) and non-participants (controls) in the indicated programs during the first quarter of welfare receipt.

Table A.5.22: Temporary Extra Jobs: Effects for subgroups of immigrants

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Quarter 1								
Turkish	-0.0296	-0.0290	-0.0441	-0.0499	-0.0360^{*}	-0.0449	-0.0133	-0.0318
	(0.0209)	(0.0242)	(0.0290)	(0.0307)	(0.0219)	(0.0299)	(0.0445)	(0.0455)
Eastern European	-0.0363	-0.0368	-0.0471	-0.0552	-0.0512^{*}	0.0257	0.0602	0.0730
	(0.0293)	(0.0356)	(0.0381)	(0.0358)	(0.0304)	(0.0495)	(0.0501)	(0.0560)
Southern European	0.0118	0.0229	-0.0156	-0.0124	-0.0687^{**}	-0.0370	0.0104	-0.0092
	(0.0293)	(0.0341)	(0.0383)	(0.0414)	(0.0293)	(0.0450)	(0.0709)	(0.0691)

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. Standard errors were obtained through bootstrapping based on 250 replications. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Only Temporary Extra Jobs starting during the first quarter of welfare receipt are considered.

Table A.5.23: Temporary Extra Jobs: Differences in treatment effects between native Germans and subgroups of immigrants

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Turkish								
Δ_{Dif}^{ATT}	0.011	-0.004	-0.018	-0.019	-0.025	-0.019	0.012	-0.012
p-value	0.619	0.902	0.580	0.582	0.489	0.648	0.803	0.815
IFE	0.012	0.001	-0.013	-0.018	0.038	0.055	0.069	0.128
p-value	0.740	0.974	0.757	0.681	0.438	0.690	0.752	0.386
Eastern European								
Δ_{Dif}^{ATT}	0.005	-0.011	-0.021	-0.024	-0.041	0.052	0.085	0.093
p-value	0.870	0.760	0.608	0.573	0.261	0.229	0.074	0.071
IFE	0.008	0.002	0.006	0.009	-0.059	0.059	0.107	0.119
p-value	0.843	0.962	0.893	0.852	0.258	0.405	0.153	0.166
Southern European								
Δ_{Dif}^{ATT}	0.053	0.048	0.011	0.019	-0.058	-0.011	0.035	0.011
p-value	0.052	0.160	0.772	0.629	0.210	0.837	0.555	0.867
IFE	0.064	0.072	0.034	0.034	-0.025	-0.052	-0.012	0.094
p-value	0.143	0.150	0.517	0.537	0.651	0.552	0.914	0.385

Remarks: Only programs starting during the first quarter of welfare receipt are considered. Δ_{Dif}^{ATT} denotes the raw differential in ATTs between the respective subgroup of immigrants and native Germans. The p-value in the second row of each block displays the statistical significance of the difference in ATTs of immigrants and natives. IFE denotes the estimated difference in ATTs for immigrants and native Germans that is solely due to unobservable differences between the two groups, i.e. due to the immigrant fixed effect. The p-values in the final line of each block denote the significance of immigrant fixed effects. Standard errors of immigrant fixed effects were obtained through bootstrapping based on 250 replications.

Table A.5.24: Short-term training programs: Effects for subgroups of immigrants

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Aptitude test								
Turkish	0.0221	0.0067	0.0089	0.0480	0.0484	0.0788	0.0578	0.0191
	(0.0279)	(0.0302)	(0.0326)	(0.0375)	(0.0472)	(0.0514)	(0.0515)	(0.0503)
Eastern European	0.1007*	0.0569	0.0893	0.0889	0.0231	0.0621	0.0319	0.0781
	(0.0547)	(0.0583)	(0.0652)	(0.0713)	(0.0398)	(0.0500)	(0.0476)	(0.0574)
Southern European	0.1110**	0.1057**	0.1849***	0.1019*	-0.0178	0.0359	0.0105	-0.0000
	(0.0465)	(0.0455)	(0.0524)	(0.0549)	(0.0501)	(0.0661)	(0.0693)	(0.0673)
Job search training								
Turkish	-0.0364	-0.0189	0.0185	0.0085	-0.0126	-0.0405	0.0065	0.0108
	(0.0242)	(0.0322)	(0.0407)	(0.0444)	(0.0422)	(0.0492)	(0.0544)	(0.0592)
Eastern European	0.0116	0.0680	0.1644*	0.0974	−0.0667***	−0.0919***	-0.0361	0.0086
	(0.0775)	(0.0788)	(0.0879)	(0.0894)	(0.0151)	(0.0187)	(0.0423)	(0.0596)
Southern European	0.0178	0.0459	0.0742	0.0181	-0.0393	-0.0339	0.0550	0.1216
	(0.0485)	(0.0538)	(0.0585)	(0.0554)	(0.0613)	(0.0828)	(0.1112)	(0.1113)
Skill provision								
Turkish	0.0455	0.1065**	0.0817	0.0674	0.1186*	0.1827**	0.1355*	0.0903
	(0.0391)	(0.0515)	(0.0524)	(0.0504)	(0.0695)	(0.0821)	(0.0775)	(0.0759)
Eastern European	0.0202	-0.0209	0.0334	0.0978	0.0209	0.0403	0.0706	0.1574**
	(0.0644)	(0.0636)	(0.0720)	(0.0804)	(0.0455)	(0.0519)	(0.0587)	(0.0671)
Southern European	-0.0349	0.0061	0.0259	0.0058	0.0866	0.0668	0.1240	0.1398
	(0.0422)	(0.0514)	(0.0562)	(0.0596)	(0.0841)	(0.0863)	(0.0977)	(0.1036)
Combined training programs								
Turkish	0.0096	-0.0109	-0.0299	0.0017	0.0053	0.0184	0.0319	0.0069
	(0.0280)	(0.0332)	(0.0348)	(0.0393)	(0.0428)	(0.0397)	(0.0499)	(0.0533)
Eastern European	0.0863	0.1749***	0.1369**	0.1170*	-0.0579	-0.0335	0.1171	0.1081
	(0.0556)	(0.0653)	(0.0617)	(0.0615)	(0.0634)	(0.0776)	(0.1053)	(0.0982)
Southern European	-0.0434	0.0018	-0.0043	-0.0178	0.0514	-0.0225	-0.0080	0.0502
	(0.0311)	(0.0409)	(0.0419)	(0.0424)	(0.0634)	(0.0603)	(0.0675)	(0.0822)

Remarks: Displayed are Average Effects of Treatment on the Treated and standard errors in brackets. Standard errors were obtained through bootstrapping based on 250 replications. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$. Only programs starting during the first quarter of welfare receipt are considered.

Table A.5.25: Short-term training programs: Differences in treatment effects between native Germans and Turkish immigrants

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Aptitude tests								
Δ_{Dif}^{ATT}	-0.045	-0.080	-0.075	-0.046	-0.001	0.010	-0.009	-0.076
p-value	0.244	0.058	0.089	0.317	0.992	0.873	0.882	0.261
IFE	-0.008	-0.030	-0.006	0.012	-0.059	-0.002	-0.003	-0.041
p-value	0.886	0.590	0.917	0.863	0.615	0.988	0.980	0.732
Job search training								
Δ_{Dif}^{ATT}	-0.024	-0.050	-0.004	-0.024	-0.075	-0.099	-0.049	-0.080
p-value	0.512	0.286	0.937	0.638	0.212	0.124	0.479	0.284
IFE	-0.049	-0.165	-0.252	-0.170	-0.259	-0.239	0.022	-0.001
p-value	0.681	0.350	0.203	0.414	0.007	0.003	0.777	0.992
Skill provision								
Δ_{Dif}^{ATT}	0.003	0.030	-0.038	-0.035	0.087	0.127	0.072	0.031
p-value	0.957	0.577	0.514	0.544	0.176	0.090	0.362	0.696
IFE	0.051	0.013	-0.061	-0.069	0.1282	0.164	0.128	0.061
p-value	0.564	0.904	0.593	0.546	0.214	0.144	0.303	0.626
Combined training programs								
Δ_{Dif}^{ATT}	0.005	-0.041	-0.059	-0.027	-0.006	0.003	0.015	0.000
p-value	0.884	0.302	0.159	0.541	0.899	0.962	0.805	0.995
IFE	0.019	-0.065	-0.050	-0.018	0.082	0.114	0.059	0.041
p-value	0.723	0.266	0.392	0.780	0.291	0.176	0.536	0.686

Remarks: Only programs starting during the first quarter of welfare receipt are considered. Δ_{Dif}^{ATT} denotes the raw differential in ATTs between Turkish immigrants and native Germans for the respective program. The p-value in the second row of each block displays the statistical significance of the difference in ATTs of Turkish immigrants and natives. IFE denotes the estimated difference in ATTs for Turkish immigrants and native Germans that is solely due to unobservable differences between the two groups, i.e. due to the immigrant fixed effect. The p-values in the final line of each block denote the significance of immigrant fixed effects. Standard errors of immigrant fixed effects were obtained through bootstrapping based on 250 replications.

Table A.5.26: Short-term training programs: Differences in treatment effects between native Germans and Eastern European immigrants

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Aptitude tests								
Δ_{Dif}^{ATT}	0.034	-0.030	0.006	-0.005	-0.026	-0.007	-0.035	-0.017
p-value	0.499	0.586	0.918	0.932	0.634	0.913	0.585	0.811
IFE	-0.006	-0.031	0.024	-0.021	-0.038	0.036	0.004	0.012
p-value	0.941	0.725	0.783	0.818	0.661	0.715	0.972	0.922
Job search training								
Δ_{Dif}^{ATT}	0.024	0.037	0.142	0.064	-0.129	-0.150	-0.092	-0.082
p-value	0.673	0.593	0.052	0.397	0.052	0.036	0.232	0.326
IFE	-0.097	-0.041	0.093	0.001	-0.279	-0.270	-0.203	0.011
p-value	0.530	0.816	0.619	0.997	0.069	0.065	0.175	0.939
Skill provision								
Δ_{Dif}^{ATT}	-0.023	-0.097	-0.086	-0.005	-0.011	-0.015	0.007	0.098
p-value	0.718	0.174	0.268	0.952	0.838	0.807	0.921	0.164
IFE	0.062	-0.127	-0.104	0.054	0.023	-0.001	-0.005	0.057
p-value	0.497*	0.166*	0.318*	0.639*	0.830	0.996	0.976	0.718
Combined training programs								
Δ_{Dif}^{ATT}	0.082	0.144	0.108	0.088	-0.069	-0.049	0.100	0.101
p-value	0.090	0.011	0.068	0.149	0.218	0.457	0.177	0.182
IFE	0.001	0.092	0.057	0.022	-0.091	-0.088	0.092	0.066
p-value	0.989	0.354	0.574	0.839	0.490	0.536	0.558	0.700

Remarks: Only programs starting during the first quarter of welfare receipt are considered. Δ_{Dif}^{ATT} denotes the raw differential in ATTs between Eastern European immigrants and native Germans for the respective program. The p-value in the second row of each block displays the statistical significance of the difference in ATTs of Eastern European immigrants and natives. IFE denotes the estimated difference in ATTs for Eastern European immigrants and native Germans that is solely due to unobservable differences between the two groups, i.e. due to the immigrant fixed effect. The p-values in the final line of each block denote the significance of immigrant fixed effects. Standard errors of immigrant fixed effects were obtained through bootstrapping based on 250 replications.

Table A.5.27: Short-term training programs: Differences in treatment effects between native Germans and Southern European immigrants

	Men				Women			
Month:	3	6	9	12	3	6	9	12
Aptitude tests								
Δ_{Dif}^{ATT}	0.044	0.019	0.101	0.008	-0.067	-0.033	-0.057	-0.095
p-value	0.339	0.707	0.052	0.880	0.292	0.652	0.458	0.243
IFE	0.089	0.072	0.160	0.062	-0.049	-0.008	-0.034	-0.081
p-value	0.143	0.305	0.026	0.365	0.378	0.925	0.658	0.300
Job search training								
Δ_{Dif}^{ATT}	0.030	0.015	0.052	-0.015	-0.101	-0.092	-0.001	0.031
p-value	0.524	0.793	0.390	0.813	0.212	0.298	0.994	0.764
IFE	0.023	0.079	0.027	-0.013	/	/	/	/
p-value	0.817	0.522	0.940	0.972	/	/	/	/
Skill provision								
Δ_{Dif}^{ATT}	-0.078	-0.070	-0.093	-0.097	0.055	0.0112	0.060	0.081
p-value	0.159	0.273	0.177	0.168	0.450	0.893	0.503	0.382
IFE	-0.075	0.015	-0.019	-0.032	0.081	0.042	0.074	0.071
p-value	0.453	0.883	0.885	0.812	0.463	0.780	0.636	0.638
Combined training programs								
Δ_{Dif}^{ATT}	-0.048	-0.029	-0.034	-0.047	0.040	-0.038	-0.025	0.044
p-value	0.226	0.544	0.498	0.366	0.571	0.626	0.769	0.620
IFE	-0.088	-0.076	-0.064	-0.140	0.089	0.002	0.041	0.102
p-value	0.415	0.535	0.597	0.271	0.464	0.987	0.749	0.426

Remarks: Only programs starting during the first quarter of welfare receipt are considered. Δ_{Dif}^{ATT} denotes the raw differential in ATTs between Southern European immigrants and native Germans for the respective program. The p-value in the second row of each block displays the statistical significance of the difference in ATTs of Southern European immigrants and natives. IFE denotes the estimated difference in ATTs for Southern European immigrants and native Germans that is solely due to unobservable differences between the two groups, i.e. due to the immigrant fixed effect. The p-values in the final line of each block denote the significance of immigrant fixed effects. Standard errors of immigrant fixed effects were obtained through bootstrapping based on 250 replications. For women participating in job search training, the number of observations is too small to estimate immigrant fixed effects.

Appendix to Chapter 6

Table A.6.1: Selected descriptive statistics of the GSOEP sample

	Natives		Immigrants	
	Men	Women	Men	Women
Log monthly gross wage	7.4191	7.0523	7.4065	6.9950
	(0.4398)	(0.4814)	(0.3898))	(0.5165)
Age				
18 to 24 years	0.1041	0.1005	0.1000	0.0732
	(0.3060)	(0.3015)	(0.3025)	(0.2637)
25 to 34 years	0.2805	0.2434	0.4667	0.1220
	(0.4503)	(0.4303)	(0.5031)	(0.3313)
35 to 49 years	0.3891	0.4762	0.3500	0.5854
	(0.4887)	(0.5008)	(0.4810)	(0.4988)
50 to 57 years	0.2262	0.1799	0.0833	0.2195
	(0.4193)	(0.3851)	(0.2787)	(0.4191)
Schooling				
No school leaving certificate	0.0181	0.0159	0.1333	0.2195
	(0.1336)	(0.1253)	(0.3428)	(0.4191)
Secondary general school	0.4118	0.2328	0.3000	0.2439
	(0.4933)	(0.4237)	(0.4621)	(0.4348)
Intermediate secondary school	0.3710	0.5661	0.1167	0.1463
	(0.4842)	(0.4969)	(0.3237)	(0.3578)
Vocational diploma (*Fachabitur*)	0.0271	0.0476	0.0167	0.0732
	(0.1629)	(0.2135)	(0.1291)	(0.2637)
University entrance diploma	0.1131	0.0952	0.0333	0.0488
	(0.3175)	(0.2943)	(0.1810)	(0.2181)
Missing	0.0588	0.0423	0.4000	0.2683
	(0.2358)	(0.2019)	(0.4940)	(0.4486)
Family status				
Single	0.4118	0.3757	0.3667	0.1463
	(0.4933)	(0.4856)	(0.4860)	(0.3578))
Household size				
Number of persons	3.2896	3.4074	4.5167	3.7561
	(1.7600)	(1.9374)	(3.0945)	(2.0589)
Region				
East Germany	0.4480	0.4974	0.0500	0.0244
	(0.4984)	(0.5013)	(0.2198)	(0.1562)
Labor market history (fraction of (un)employment in the overall labor market career)				
Fraction of employment	0.6023	0.5744	0.5399	0.4480
	(0.2839)	(0.2621)	(0.2864)	(0.2510)
Fraction of unemployment	0.1303	0.1253	0.2054	0.0997
	(0.1468)	(0.1640)	(0.1782)	(0.1438)
Observations	221	189	60	41

Remarks: Displayed are means of selected variables and standard deviations in brackets. Data source is the GSOEP sample described in subsection 6.3.2.

Table A.6.2: Selected descriptive statistics of the administrative sample for male program participants

	Aptitude tests		Job search training		Skill provision		Combined training programs		Temporary Extra Jobs	
	Natives	Immigrants	Natives	Immigrants	Natives	Immigrants	Natives	Immigrants	Natives	Immigrants
Age										
18-24	0.302	0.190	0.320	0.252	0.299	0.152	0.351	0.233	0.438	0.400
25-34	0.337	0.413	0.343	0.363	0.330	0.413	0.276	0.400	0.210	0.250
35-49	0.312	0.346	0.264	0.300	0.312	0.390	0.276	0.300	0.251	0.269
50-57	0.049	0.051	0.073	0.085	0.059	0.045	0.098	0.067	0.101	0.082
Schooling										
No school leaving certificate	0.087	0.205	0.053	0.233	0.059	0.201	0.117	0.275	0.194	0.271
Secondary general school	0.473	0.444	0.545	0.452	0.500	0.447	0.522	0.431	0.508	0.460
Intermediate secondary school	0.250	0.161	0.231	0.167	0.233	0.182	0.183	0.119	0.168	0.105
Vocational diploma (*Fachabitur*)	0.057	0.049	0.036	0.022	0.073	0.034	0.044	0.033	0.029	0.023
University entrance diploma	0.085	0.094	0.076	0.093	0.094	0.091	0.071	0.056	0.044	0.070
Missing	0.047	0.047	0.059	0.033	0.042	0.045	0.063	0.086	0.056	0.070
Family status										
Single	0.673	0.422	0.686	0.400	0.635	0.360	0.698	0.394	0.736	0.537
Household size										
Number of persons	1.509	2.116	1.465	2.126	1.514	2.201	1.412	2.050	1.394	1.981
Region										
East Germany	0.055	0.022	0.069	0.037	0.035	0.023	0.020	0.008	0.029	0.009
Labor market history (fraction of (un)employment in the final 2 years before program participation)										
Fraction of employment	0.296	0.330	0.400	0.450	0.339	0.379	0.262	0.333	0.158	0.177
Fraction of unemployment	0.324	0.330	0.203	0.237	0.314	0.307	0.300	0.296	0.358	0.348
Observations	507	448	303	270	288	264	410	360	891	428

Remarks: Displayed are means of selected variables for native and immigrant men participating in the indicated programs during the first quarter of welfare receipt. Data source is the administrative sample described in subsection 5.4.1.

Table A.6.3: Selected descriptive statistics of the administrative sample for female program participants

	Aptitude tests		Job search training		Skill provision		Combined training programs		Temporary Extra Jobs	
	Natives	Immigrants	Natives	Immigrants	Natives	Immigrants	Natives	Immigrants	Natives	Immigrants
Age										
18-24	0.293	0.239	0.337	0.331	0.242	0.233	0.345	0.281	0.435	0.400
25-34	0.279	0.361	0.326	0.331	0.283	0.391	0.246	0.421	0.153	0.236
35-49	0.347	0.361	0.253	0.254	0.365	0.323	0.345	0.236	0.299	0.255
50-57	0.081	0.039	0.084	0.085	0.110	0.053	0.065	0.062	0.113	0.109
Schooling										
No school leaving certificate	0.071	0.139	0.068	0.127	0.023	0.150	0.082	0.191	0.113	0.177
Secondary general school	0.354	0.356	0.353	0.441	0.374	0.323	0.332	0.303	0.432	0.382
Intermediate secondary school	0.313	0.200	0.311	0.161	0.384	0.263	0.254	0.169	0.216	0.209
Vocational diploma (*Fachabitur*)	0.057	0.056	0.100	0.051	0.068	0.030	0.086	0.039	0.060	0.023
University entrance diploma	0.104	0.094	0.084	0.136	0.114	0.158	0.073	0.084	0.075	0.082
Missing	0.101	0.156	0.084	0.085	0.037	0.075	0.172	0.213	0.103	0.127
Family status										
Single	0.552	0.372	0.563	0.322	0.584	0.286	0.565	0.427	0.580	0.395
Household size										
Number of persons	1.502	1.933	1.568	1.907	1.580	1.797	1.612	1.899	1.397	1.732
Region										
East Germany	0.067	0.022	0.068	0.042	0.037	0.030	0.017	0.006	0.040	0.027
Labor market history (fraction of (un)employment in the final 2 years before program participation)										
Fraction of employment	0.349	0.325	0.415	0.397	0.351	0.353	0.263	0.267	0.195	0.160
Fraction of unemployment	0.244	0.266	0.190	0.204	0.305	0.278	0.238	0.235	0.305	0.356
Observations	297	180	190	118	219	133	232	178	398	220

Remarks: Displayed are means of selected variables for native and immigrant women participating in the indicated programs during the first quarter of welfare receipt. Data source is the administrative sample described in subsection 5.4.1.

Table A.6.4: Ordinary Least Squares (OLS) estimation results for the post-welfare wage equation

	Natives		Immigrants	
	Men	Women	Men	Women
Age (reference: 25 to 34 years)				
18 to 24 years	-0.0059	-0.1837	-0.1743	0.3747
	(0.0978)	(0.1354)	(0.1714)	(0.5016)
35 to 49 years	-0.0397	0.0254	-0.1722	-0.3878
	(0.0806)	(0.0977)	(0.1194)	(0.3992)
50 to 57 years	0.0060	0.2183^{**}	-0.2524	-0.0323
	(0.0986)	(0.1257)	(0.1826)	(0.4819)
Schooling (reference: secondary general school)				
No school leaving certificate	-0.0089	-0.0638	-0.0651	-0.2055
	(0.1964)	(0.2759)	(0.1508)	(0.2678)
Intermediate secondary school	-0.0471	0.0666	0.1734	-0.3411
	(0.0681)	(0.0907)	(0.1529)	(0.3025)
Vocational diploma (*Fachabitur*)	-0.0308	0.2525	0.8903^{**}	-0.3750
	(0.1637)	(0.1695)	(0.3877)	(0.4237)
University entrance diploma	-0.0639	0.3888^{***}	-0.2820	-0.2133
	(0.0975)	(0.1347)	(0.2838)	(0.5183)
Missing	-0.1218	0.1642	0.0293	-0.0686
	(0.1155)	(0.1752)	(0.1156)	(0.2700)
Family status				
Single	-0.1579^{**}	0.1921^{**}	-0.0925	-0.2031
	(0.0725)	(0.0915)	(0.1365)	(0.4372)
Household size				
Number of persons	0.0259^{*}	0.0208	0.0121	-0.0083
	(0.0152)	(0.0176)	(0.0167)	(0.0599)
Region				
East Germany	-0.1976^{***}	0.0551	-0.0827	0.2169
	(0.0614)	(0.0763)	(0.2329)	(0.5708)
Labor market history (fraction of (un)employment in the overall labor market career)				
Fraction of employment	0.2086	0.3172^{**}	0.6185^{**}	0.6382
	(0.1557)	(0.1706)	(0.2427)	(0.4354)
Fraction of unemployment	-0.7568^{***}	-0.3316	-0.0745	-0.7450
	(0.2322)	(0.2602)	(0.3246)	(0.7690)
Constant	7.5081^{***}	6.6158^{***}	7.1413^{***}	7.1967^{***}
	(0.1474)	(0.1670)	(0.2305)	(0.6090)
Observations	221	189	60	41
R^2	0.3025	0.1817	0.4453	0.2993

Remarks: The dependent variable in each estimation is the log monthly gross wage of an individual who left welfare for employment. The estimations are based on the GSOEP sample described in subsection 6.3.2. Displayed are estimated coefficients and standard errors in brackets. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.1$.

Table A.6.5: Cost-benefit analysis for aptitude tests and native men

Number of treated	507	Benefits per month (in euros)	1,417.97
Costs per participant (in euros)	247.35	Costs for all participants (in euros)	125,406.45
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0532	26.9678	38,239.58	38,084.42	38,084.42
2	0.0580	29.4295	41,730.11	41,392.15	79,476.57
3	0.0668	33.8817	48,043.23	47,460.78	126,937.35
4	0.0816	41.3554	58,640.69	57,694.71	184,632.06
5	0.0887	44.9531	63,742.15	62,459.41	247,091.47
6	0.0867	43.9742	62,354.08	60,851.35	307,942.82
7	0.0776	39.3275	55,765.26	54,200.50	362,143.32
8	0.0665	33.7161	47,808.36	46,278.32	408,421.65
9	0.0834	42.3038	59,985.48	57,830.13	466,251.78
10	0.0866	43.9213	62,279.10	59,797.71	526,049.49
11	0.0974	49.3694	70,004.30	66,942.38	592,991.88
12	0.0939	47.5873	67,477.33	64,264.12	657,256.00
Benefit-cost ratio 12 months after program start					5.24

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.6: Cost-benefit analysis for aptitude tests and immigrant men

Number of treated	448	Benefits per month (in euros)	1,657.86
Costs per participant (in euros)	247.35	Costs for all participants (in euros)	110,812.80
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0552	24.7453	41,024.21	40,857.75	40,857.75
2	0.0588	26.3476	43,680.70	43,326.94	84,184.69
3	0.0464	20.7952	34,475.49	34,057.53	118,242.22
4	0.0403	18.0665	29,951.80	29,468.62	147,710.84
5	0.0419	18.7700	31,118.02	30,491.80	178,202.63
6	0.0441	19.7465	32,737.00	31,948.04	210,150.68
7	0.0488	21.8634	36,246.43	35,229.37	245,380.04
8	0.0485	21.7059	35,985.37	34,833.71	280,213.75
9	0.0703	31.5163	52,249.63	50,372.23	330,585.99
10	0.0744	33.3283	55,253.71	53,052.24	383,638.22
11	0.0706	31.6374	52,450.46	50,156.33	433,794.55
12	0.0653	29.2569	48,503.86	46,194.15	479,988.70
Benefit-cost ratio 12 months after program start					4.33

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.7: Cost-benefit analysis for aptitude tests and native women

Number of treated	297	Benefits per month (in euros)	1,227.92
Costs per participant (in euros)	247.35	Costs for all participants (in euros)	73,462.95
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0218	6.4604	7,932.82	7,900.63	7,900.63
2	0.0335	9.9585	12,228.24	12,129.21	20,029.84
3	0.0490	14.5401	17,854.13	17,637.67	37,667.52
4	0.0730	21.6730	26,612.69	26,183.37	63,850.89
5	0.0684	20.3016	24,928.79	24,427.13	88,278.02
6	0.0690	20.4866	25,155.92	24,549.67	112,827.69
7	0.0714	21.1953	26,026.08	25,295.79	138,123.48
8	0.0745	22.1304	27,174.31	26,304.64	164,428.12
9	0.0673	19.9919	24,548.49	23,666.44	188,094.56
10	0.0924	27.4528	33,709.89	32,366.79	220,461.34
11	0.0984	29.2291	35,890.97	34,321.14	254,782.48
12	0.0948	28.1563	34,573.63	32,927.26	287,709.74
Benefit-cost ratio 12 months after program start					3.92

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.8: Cost-benefit analysis for aptitude tests and immigrant women

Number of treated	180	Benefits per month (in euros)	1,580.88
Costs per participant (in euros)	247.35	Costs for all participants (in euros)	44,523.00
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0548	9.8574	15,583.39	15,520.15	15,520.15
2	0.0448	8.0661	12,751.60	12,648.33	28,168.49
3	0.0425	7.6420	12,081.15	11,934.69	40,103.17
4	0.0290	5.2257	8,261.13	8,127.86	48,231.04
5	0.0385	6.9299	10,955.36	10,734.89	58,965.93
6	0.0727	13.0818	20,680.74	20,182.33	79,148.26
7	0.0605	10.8876	17,212.06	16,729.09	95,877.35
8	0.0522	9.3874	14,840.40	14,365.46	110,242.81
9	0.0608	10.9352	17,287.29	16,666.14	126,908.95
10	0.0597	10.7458	16,987.88	16,311.03	143,219.98
11	0.0597	10.7380	16,975.56	16,233.07	159,453.05
12	0.0524	9.4291	14,906.28	14,196.46	173,649.50
Benefit-cost ratio 12 months after program start					3.90

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.9: Cost-benefit analysis for job search training and native men

Number of treated	303	Benefits per month (in euros)	1,513.98
Costs per participant (in euros)	164.40	Costs for all participants (in euros)	49,813.20
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	-0.0065	-1.9765	-2,992.33	-2,980.19	-2,980.19
2	-0.0102	-3.0863	-4,672.64	-4,634.80	-7,614.99
3	-0.0123	-3.7290	-5,645.66	-5,577.22	-13,192.21
4	0.0176	5.3208	8,055.59	7,925.64	-5,266.57
5	0.0177	5.3539	8,105.77	7,942.65	2,676.08
6	0.0307	9.2986	14,077.96	13,738.69	16,414.77
7	0.0323	9.7747	14,798.68	14,383.44	30,798.20
8	0.0163	4.9534	7,499.32	7,259.32	38,057.52
9	0.0224	6.7854	10,272.93	9,903.81	47,961.34
10	0.0412	12.4956	18,918.09	18,164.33	66,125.67
11	0.0449	13.5916	20,577.47	19,677.44	85,803.10
12	0.0329	9.9742	15,100.81	14,381.72	100,184.83
Benefit-cost ratio 12 months after program start					2.01

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.10: Cost-benefit analysis for job search training and immigrant men

Number of treated	270	Benefits per month (in euros)	1,694.98
Costs per participant (in euros)	164.40	Costs for all participants (in euros)	44,388.00
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0039	1.0630	1,801.75	1,794.44	1,794.44
2	-0.0007	-0.1925	-326.35	-323.70	1,470.73
3	-0.0014	-0.3870	-656.03	-648.08	822.65
4	0.0023	0.6301	1,068.01	1,050.78	1,873.43
5	0.0055	1.4721	2,495.17	2,444.96	4,318.39
6	0.0193	5.2031	8,819.18	8,606.64	12,925.02
7	0.0193	5.2057	8,823.48	8,575.89	21,500.92
8	0.0417	11.2619	19,088.68	18,477.77	39,978.69
9	0.0637	17.1907	29,137.91	28,090.95	68,069.64
10	0.0611	16.5009	27,968.72	26,854.36	94,924.00
11	0.0427	11.5335	19,549.07	18,694.01	113,618.02
12	0.0414	11.1835	18,955.78	18,053.12	131,671.14
Benefit-cost ratio 12 months after program start					2.97

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.11: Cost-benefit analysis for job search training and native women

Number of treated	189	Benefits per month (in euros)	1,244.35
Costs per participant (in euros)	164.40	Costs for all participants (in euros)	31,071.60
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0178	3.3635	4,185.33	4,168.34	4,168.34
2	0.0450	8.4985	10,575.13	10,489.48	14,657.83
3	0.0621	11.7291	14,595.10	14,418.16	29,075.98
4	0.0683	12.9140	16,069.60	15,810.36	44,886.35
5	0.0415	7.8370	9,751.92	9,555.67	54,442.02
6	0.0585	11.0574	13,759.21	13,427.62	67,869.64
7	0.0665	12.5606	15,629.76	15,191.19	83,060.83
8	0.0590	11.1570	13,883.23	13,438.91	96,499.75
9	0.0556	10.5101	13,078.22	12,608.30	109,108.05
10	0.0684	12.9224	16,079.97	15,439.29	124,547.34
11	0.0815	15.4109	19,176.49	18,337.73	142,885.08
12	0.0910	17.2062	21,410.54	20,390.99	163,276.07
Benefit-cost ratio 12 months after program start					5.25

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.12: Cost-benefit analysis for job search training and immigrant women

Number of treated	102	Benefits per month (in euros)	1,683.22
Costs per participant (in euros)	164.40	Costs for all participants (in euros)	16,768.80
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	-0.0109	-1.1148	-1,876.54	-1,868.92	-1,868.92
2	-0.0135	-1.3780	-2,319.51	-2,300.73	-4,169.65
3	-0.0382	-3.8944	-6,555.17	-6,475.70	-10,645.35
4	-0.0420	-4.2848	-7,212.34	-7,095.99	-17,741.34
5	-0.0507	-5.1671	-8,697.29	-8,522.27	-26,263.60
6	-0.0564	-5.7561	-9,688.76	-9,455.26	-35,718.86
7	-0.0534	-5.4491	-9,171.97	-8,914.61	-44,633.47
8	-0.0177	-1.8096	-3,045.91	-2,948.43	-47,581.90
9	0.0131	1.3348	2,246.73	2,166.00	-45,415.89
10	0.0243	2.4757	4,167.07	4,001.04	-41,414.86
11	0.0249	2.5428	4,280.06	4,092.85	-37,322.01
12	0.0362	3.6928	6,215.83	5,919.83	-31,402.17
Benefit-cost ratio 12 months after program start					-1.87

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.13: Cost-benefit analysis for skill provision and native men

Number of treated	288	Benefits per month (in euros)	1,436.57
Costs per participant (in euros)	390.60	Costs for all participants (in euros)	112,492.80
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0216	6.2229	8,939.59	8,903.32	8,903.32
2	0.0431	12.4247	17,848.90	17,704.35	26,607.67
3	0.0430	12.3833	17,789.49	17,573.82	44,181.49
4	0.0674	19.4049	27,876.45	27,426.75	71,608.24
5	0.0689	19.8522	28,519.10	27,945.18	99,553.41
6	0.0764	22.0018	31,607.07	30,845.34	130,398.75
7	0.0916	26.3725	37,885.91	36,822.84	167,221.59
8	0.1035	29.8066	42,819.33	41,448.97	208,670.56
9	0.1192	34.3166	49,298.21	47,526.87	256,197.43
10	0.1191	34.2994	49,273.51	47,310.31	303,507.74
11	0.1067	30.7425	44,163.72	42,232.04	345,739.78
12	0.1026	29.5432	42,440.93	40,419.94	386,159.72
Benefit-cost ratio 12 months after program start					3.43

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.14: Cost-benefit analysis for skill provision and immigrant men

Number of treated	244	Benefits per month (in euros)	1,689.3
Costs per participant (in euros)	390.60	Costs for all participants (in euros)	95,306.40
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0275	6.7025	11,322.59	11,276.65	11,276.65
2	0.0229	5.5915	9,445.69	9,369.19	20,645.84
3	0.0218	5.3149	8,978.39	8,869.54	29,515.37
4	0.0463	11.2902	19,072.53	18,764.85	48,280.23
5	0.0388	9.4759	16,007.57	15,685.44	63,965.67
6	0.0497	12.1356	20,500.72	20,006.66	83,972.33
7	0.0673	16.4151	27,730.07	26,951.97	110,924.30
8	0.0559	13.6474	23,054.61	22,316.78	133,241.08
9	0.0522	12.7454	21,530.87	20,757.24	153,998.31
10	0.0540	13.1798	22,264.65	21,377.56	175,375.87
11	0.0463	11.2986	19,086.67	18,251.84	193,627.71
12	0.0617	15.0494	25,422.96	24,212.35	217,840.06
Benefit-cost ratio 12 months after program start					2.29

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.15: Cost-benefit analysis for skill provision and native women

Number of treated	218	Benefits per month (in euros)	1,232.14
Costs per participant (in euros)	390.60	Costs for all participants (in euros)	85,150.80
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0347	7.5612	9,316.43	9,278.63	9,278.63
2	0.0379	8.2598	10,177.29	10,094.86	19,373.49
3	0.0318	6.9263	8,534.14	8,430.68	27,804.17
4	0.0579	12.6206	15,550.38	15,299.53	43,103.69
5	0.0558	12.1566	14,978.63	14,677.20	57,780.89
6	0.0556	12.1270	14,942.18	14,582.07	72,362.96
7	0.0482	10.5007	12,938.37	12,575.33	84,938.29
8	0.0642	13.9897	17,237.26	16,685.61	101,623.90
9	0.0639	13.9278	17,160.95	16,544.33	118,168.23
10	0.0735	16.0299	19,751.07	18,964.13	137,132.36
11	0.0569	12.4075	15,287.79	14,619.12	151,751.48
12	0.0591	12.8879	15,879.75	15,123.57	166,875.05
Benefit-cost ratio 12 months after program start					1.96

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.16: Cost-benefit analysis for skill provision and immigrant women

Number of treated	133	Benefits per month (in euros)	1,599.37
Costs per participant (in euros)	390.60	Costs for all participants (in euros)	51,949.80
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0280	3.7183	5,946.86	5,922.73	5,922.73
2	0.0635	8.4398	13,498.42	13,389.10	19,311.83
3	0.0741	9.8502	15,754.19	15,563.19	34,875.03
4	0.0918	12.2074	19,524.11	19,209.15	54,084.18
5	0.1001	13.3177	21,299.93	20,871.29	74,955.47
6	0.0956	12.7109	20,329.44	19,839.50	94,794.97
7	0.1111	14.7799	23,638.45	22,975.16	117,770.13
8	0.1302	17.3186	27,698.88	26,812.42	144,582.56
9	0.1153	15.3370	24,529.52	23,648.14	168,230.70
10	0.1316	17.5008	27,990.26	26,875.05	195,105.75
11	0.1177	15.6564	25,040.36	23,945.12	219,050.87
12	0.1263	16.7931	26,858.44	25,579.47	244,630.33
Benefit-cost ratio 12 months after program start					4.71

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.17: Cost-benefit analysis for combined training programs and native men

Number of treated	410	Benefits per month (in euros)	1,430.92
Costs per participant (in euros)	375.16	Costs for all participants (in euros)	153,815.60
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0143	5.8788	8,412.13	8,378.00	8,378.00
2	0.0029	1.1802	1,688.81	1,675.13	10,053.13
3	0.0045	1.8638	2,666.92	2,634.58	12,687.71
4	0.0176	7.2036	10,307.80	10,141.52	22,829.23
5	0.0216	8.8368	12,644.71	12,390.25	35,219.48
6	0.0306	12.5319	17,932.20	17,500.03	52,719.52
7	0.0308	12.6175	18,054.58	17,547.97	70,267.49
8	0.0339	13.8970	19,885.42	19,249.02	89,516.51
9	0.0293	12.0066	17,180.49	16,563.17	106,079.68
10	0.0295	12.1100	17,328.45	16,638.03	122,717.72
11	0.0308	12.6173	18,054.40	17,264.72	139,982.44
12	0.0289	11.8444	16,948.34	16,141.28	156,123.72
Benefit-cost ratio 12 months after program start					1.02

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.18: Cost-benefit analysis for combined training programs and immigrant men

Number of treated	360	Benefits per month (in euros)	1,655.07
Costs per participant (in euros)	375.16	Costs for all participants (in euros)	135,057.60
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	-0.0087	-3.1470	-5,208.58	-5,187.45	-5,187.45
2	-0.0028	-1.0056	-1,664.32	-1,650.84	-6,838.29
3	0.0069	2.4951	4,129.55	4,079.48	-2,758.81
4	0.0405	14.5892	24,146.11	23,756.59	20,997.78
5	0.0351	12.6504	20,937.30	20,515.96	41,513.74
6	0.0316	11.3709	18,819.62	18,366.06	59,879.80
7	0.0244	8.7789	14,529.61	14,121.92	74,001.72
8	0.0200	7.1911	11,901.79	11,520.89	85,522.61
9	0.0208	7.4957	12,405.86	11,960.10	97,482.71
10	0.0352	12.6568	20,947.90	20,113.28	117,595.98
11	0.0191	6.8667	11,364.83	10,867.74	128,463.73
12	0.0272	9.8093	16,235.10	15,462.00	143,925.73
Benefit-cost ratio 12 months after program start					1.07

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.19: Cost-benefit analysis for combined training programs and native women

Number of treated	188	Benefits per month (in euros)	1,212.45
Costs per participant (in euros)	375.16	Costs for all participants (in euros)	70,530.08
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	0.0337	6.3310	7,676.04	7,644.89	7,644.89
2	0.0283	5.3130	6,441.76	6,389.59	14,034.48
3	0.0116	2.1839	2,647.87	2,615.77	16,650.25
4	0.0065	1.2289	1,489.96	1,465.92	18,116.17
5	0.0149	2.7993	3,394.01	3,325.71	21,441.88
6	0.0157	2.9443	3,569.80	3,483.77	24,925.65
7	0.0398	7.4898	9,081.02	8,826.21	33,751.86
8	0.0267	5.0213	6,088.02	5,893.18	39,645.04
9	0.0167	3.1370	3,803.44	3,666.78	43,311.82
10	0.0124	2.3251	2,819.08	2,706.76	46,018.58
11	0.0145	2.7246	3,303.50	3,159.01	49,177.58
12	0.0066	1.2347	1,497.02	1,425.74	50,603.32
Benefit-cost ratio 12 months after program start					0.72

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.20: Cost-benefit analysis for combined training programs and immigrant women

Number of treated	135	Benefits per month (in euros)	1,592.24
Costs per participant (in euros)	375.16	Costs for all participants (in euros)	50,646.60
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	-0.0043	-0.5753	-915.98	-912.26	-912.26
2	-0.0167	-2.2490	-3,580.96	-3,551.96	-4,464.22
3	-0.0027	-0.3653	-581.60	-574.55	-5,038.76
4	0.0200	2.7056	4,308.03	4,238.54	-800.22
5	0.0050	0.6740	1,073.17	1,051.57	251.35
6	0.0142	1.9224	3,060.92	2,987.15	3,238.50
7	0.0205	2.7700	4,410.50	4,286.74	7,525.25
8	0.0248	3.3470	5,329.19	5,158.63	12,683.88
9	0.0533	7.1968	11,459.07	11,047.33	23,731.21
10	0.0598	8.0771	12,860.60	12,348.20	36,079.41
11	0.0524	7.0692	11,255.81	10,763.49	46,842.90
12	0.0656	8.8555	14,100.10	13,428.67	60,271.57
Benefit-cost ratio 12 months after program start					1.19

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.21: Cost-benefit analysis for Temporary Extra Jobs and native men

Number of treated	889	Benefits per month (in euros)	1,380.95
Costs per participant (in euros)	1,629.55	Costs for all participants (in euros)	1,448,669.95
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	-0.0330	-29.3748	-40,565.11	-40,400.51	-40,400.51
2	-0.0380	-33.7415	-46,595.27	-46,217.91	-86,618.42
3	-0.0410	-36.4663	-50,358.19	-49,747.67	-136,366.09
4	-0.0353	-31.3606	-43,307.46	-42,608.83	-178,974.92
5	-0.0379	-33.7305	-46,580.17	-45,642.79	-224,617.72
6	-0.0254	-22.5798	-31,181.57	-30,430.10	-255,047.82
7	-0.0182	-16.2087	-22,383.39	-21,755.32	-276,803.14
8	-0.0250	-22.2429	-30,716.29	-29,733.26	-306,536.40
9	-0.0263	-23.3801	-32,286.72	-31,126.62	-337,663.02
10	-0.0248	-22.0763	-30,486.23	-29,271.56	-366,934.58
11	-0.0252	-22.3655	-30,885.58	-29,534.68	-396,469.26
12	-0.0312	-27.7698	-38,348.68	-36,522.55	-432,991.82
Benefit-cost ratio 12 months after program start					-0.30

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.22: Cost-benefit analysis for Temporary Extra Jobs and immigrant men

Number of treated	428	Benefits per month (in euros)	1,529.68
Costs per participant (in euros)	1,629.55	Costs for all participants (in euros)	697,447.40
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	-0.0141	-6.0458	-9,248.07	-9,210.55	-9,210.55
2	-0.0142	-6.0801	-9,300.58	-9,225.26	-18,435.81
3	-0.0080	-3.4082	-5,213.53	-5,150.33	-23,586.13
4	-0.0210	-8.9981	-13,764.15	-13,542.11	-37,128.24
5	-0.0205	-8.7774	-13,426.58	-13,156.39	-50,284.63
6	-0.0129	-5.5057	-8,422.03	-8,219.06	-58,503.69
7	-0.0317	-13.5616	-20,744.86	-20,162.76	-78,666.45
8	-0.0325	-13.9127	-21,281.97	-20,600.88	-99,267.33
9	-0.0242	-10.3728	-15,867.06	-15,296.93	-114,564.26
10	-0.0252	-10.7713	-16,476.71	-15,820.23	-130,384.49
11	-0.0166	-7.1044	-10,867.48	-10,392.15	-140,776.64
12	-0.0272	-11.6293	-17,789.13	-16,942.03	-157,718.67
Benefit-cost ratio 12 months after program start					-0.23

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.23: Cost-benefit analysis for Temporary Extra Jobs and native women

Number of treated	347	Benefits per month (in euros)	1,181.83
Costs per participant (in euros)	1,629.55	Costs for all participants (in euros)	565,453.85
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	-0.0286	-9.9071	-11,708.54	-11,661.03	-11,661.03
2	-0.0279	-9.6976	-11,460.97	-11,368.15	-23,029.18
3	-0.0105	-3.6345	-4,295.42	-4,243.34	-27,272.52
4	-0.0185	-6.4368	-7,607.22	-7,484.50	-34,757.02
5	-0.0091	-3.1489	-3,721.49	-3,646.60	-38,403.62
6	-0.0261	-9.0400	-10,683.80	-10,426.32	-48,829.94
7	-0.0215	-7.4585	-8,814.71	-8,567.37	-57,397.31
8	-0.0209	-7.2569	-8,576.44	-8,301.96	-65,699.27
9	-0.0249	-8.6561	-10,230.07	-9,862.49	-75,561.76
10	-0.0222	-7.7086	-9,110.30	-8,747.32	-84,309.08
11	-0.0130	-4.5160	-5,337.14	-5,103.70	-89,412.78
12	-0.0200	-6.9509	-8,214.78	-7,823.60	-97,236.37
Benefit-cost ratio 12 months after program start					-0.17

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.24: Cost-benefit analysis for Temporary Extra Jobs and immigrant women

Number of treated	193	Benefits per month (in euros)	1,550.56
Costs per participant (in euros)	1,629.55	Costs for all participants (in euros)	314,503.15
Annual discount rate	5%	Monthly discount rate	0.4074%

Month after program start	ATTs	Additional employment integrations	Benefits (in euros)	Discounted benefits (in euros)	Discounted and accumulated benefits (in euros)
1	-0.0227	-4.3903	-6,807.49	-6,779.87	-6,779.87
2	-0.0138	-2.6672	-4,135.66	-4,102.16	-10,882.03
3	-0.0390	-7.5205	-11,660.92	-11,519.55	-22,401.58
4	-0.0316	-6.1047	-9,465.74	-9,313.04	-31,714.63
5	-0.0178	-3.4431	-5,338.79	-5,231.36	-36,945.98
6	-0.0119	-2.3021	-3,569.58	-3,483.55	-40,429.54
7	0.0203	3.9110	6,064.32	5,894.15	-34,535.38
8	0.0317	6.1186	9,487.20	9,183.58	-25,351.81
9	0.0152	2.9336	4,548.66	4,385.22	-20,966.58
10	0.0037	0.7084	1,098.37	1,054.60	-19,911.98
11	-0.0048	-0.9260	-1,435.81	-1,373.01	-21,284.99
12	-0.0091	-1.7513	-2,715.47	-2,586.16	-23,871.15
Benefit-cost ratio 12 months after program start					-0.08

Remarks: Fiscal costs and fiscal benefits are considered. All monetary values are measured in 2007 prices. The Average Effects of Treatment on the Treated (ATTs) were estimated in chapter 5 and refer to the first quarter of welfare receipt.

Table A.6.25: Total fiscal costs and benefits at the end of the observation period

Short-term training programs	Point estimate	Lower bound of 95% confidence interval	Upper bound of 95% confidence interval
Benefits	3,388,763.30	89,260.93	6,688,265.68
Costs	1,251,196.48	1,251,196.48	1,251,196.48
Difference (benefits - costs)	2,137,566.82	-1,161,935.55	5,437,069.20
Temporary Extra Jobs	Point estimate	Lower bound of 95% confidence interval	Upper bound of 95% confidence interval
Benefits	-711,818.01	-1,523,975.45	-167,863.43
Costs	3,026,074.35	3,026,074.35	3,026,074.35
Difference (benefits - costs)	-3,737,892.36	-4,550,049.80	-3,193,937.78
All considered programs	Point estimate	Lower bound of 95% confidence interval	Upper bound of 95% confidence interval
Benefits	2,676,945.29	-1,434,714.52	6,520,402.25
Costs	4,277,270.83	4,277,270.83	4,277,270.83
Difference (benefits - costs)	-1,600,325.55	-5,711,985.35	2,243,131.42

Remarks: Displayed are the total fiscal costs and benefits accruing to all subgroups of interest (men and women with and without a migration background) at the end of the observation period (12 months after start of programs). All monetary values are measured in 2007 prices.

Fig. A.6.1: Cost-benefit analysis for aptitude tests and native men

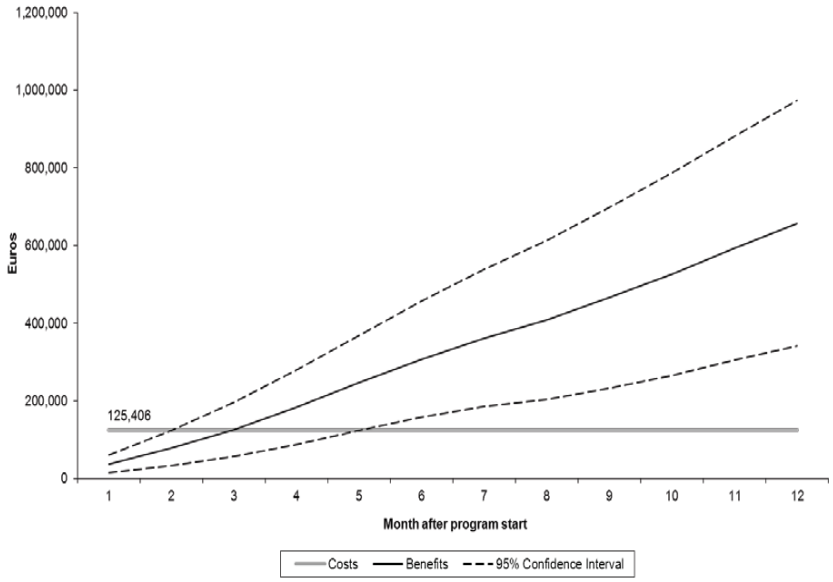


Fig. A.6.2: Cost-benefit analysis for aptitude tests and immigrant men

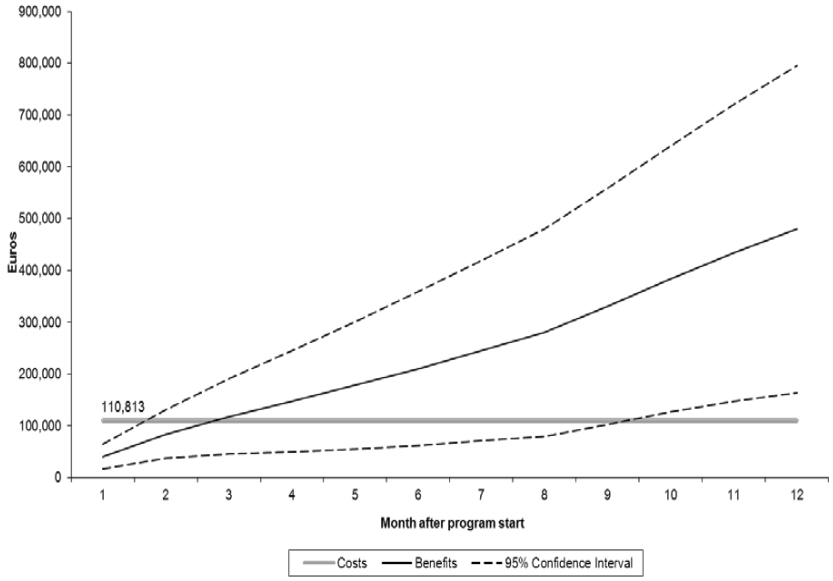


Fig. A.6.3: Cost-benefit analysis for aptitude tests and native women

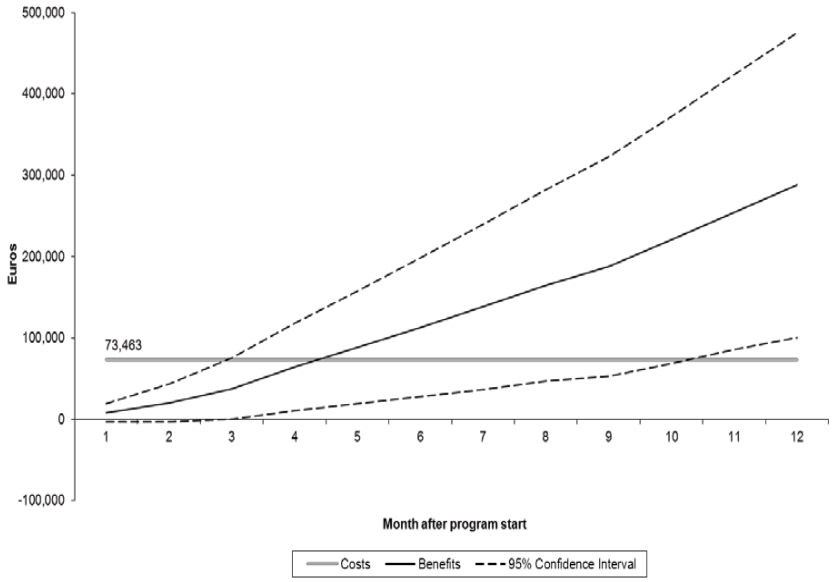


Fig. A.6.4: Cost-benefit analysis for aptitude tests and immigrant women

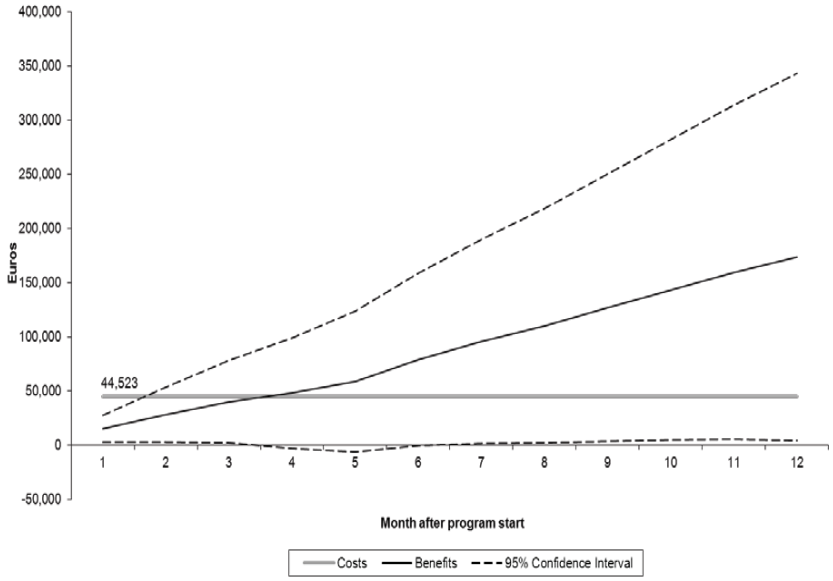


Fig. A.6.5: Cost-benefit analysis for job search training and native men

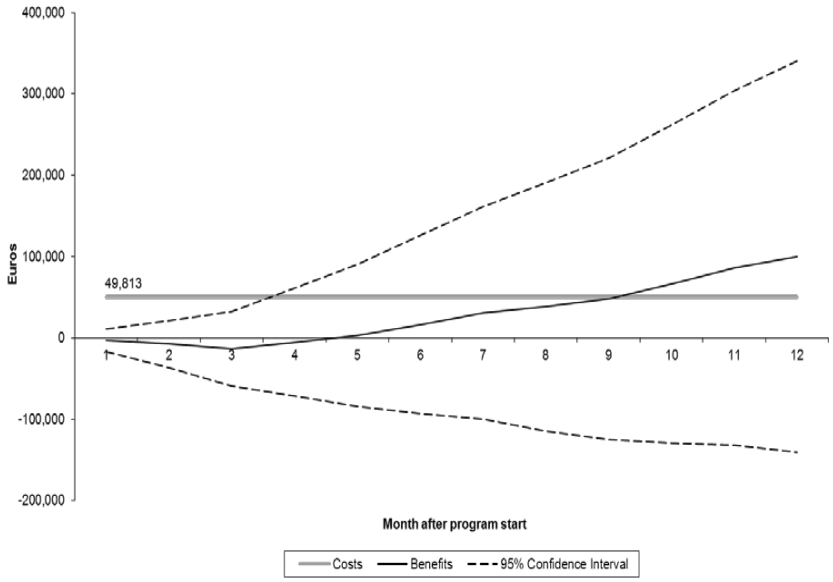


Fig. A.6.6: Cost-benefit analysis for job search training and immigrant men

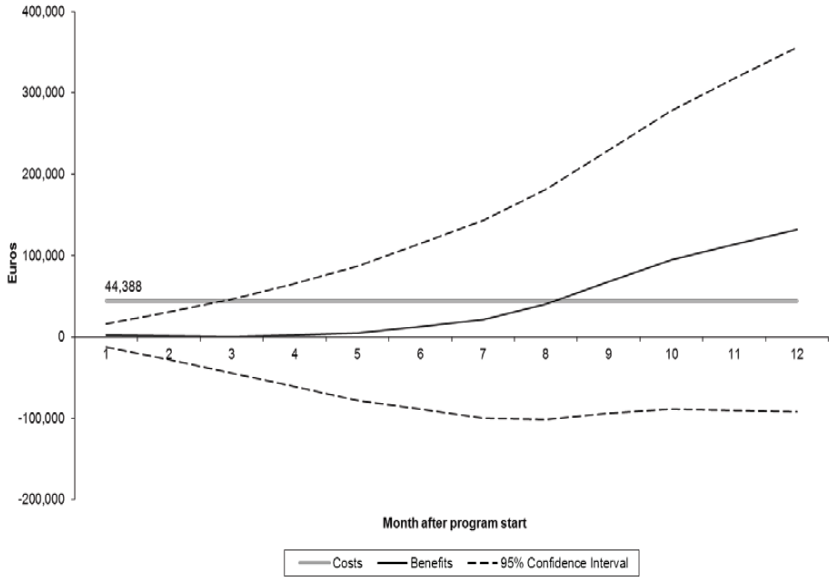


Fig. A.6.7: Cost-benefit analysis for job search training and native women

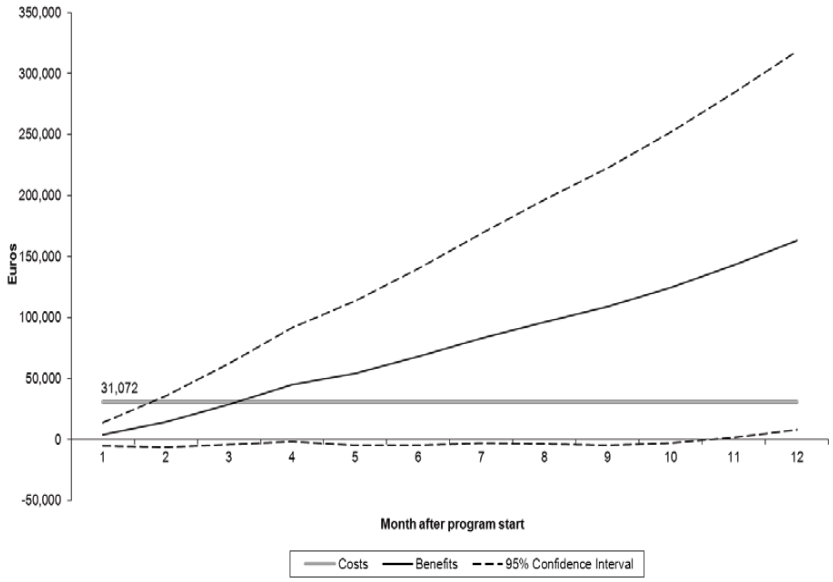


Fig. A.6.8: Cost-benefit analysis for job search training and immigrant women

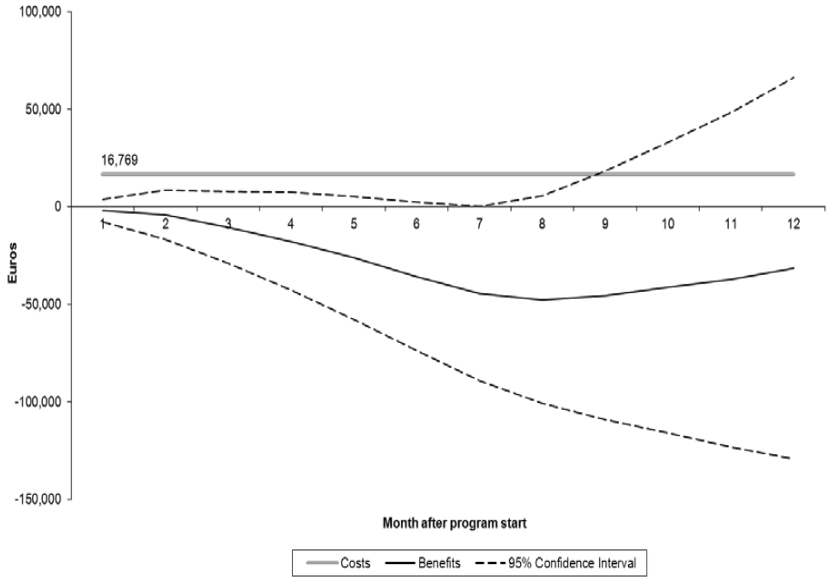


Fig. A.6.9: Cost-benefit analysis for skill provision and native men

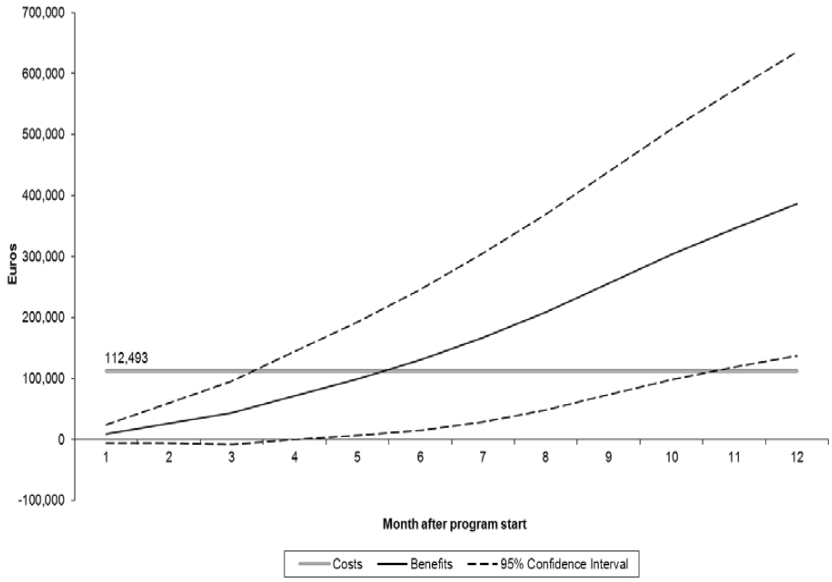


Fig. A.6.10: Cost-benefit analysis for skill provision and immigrant men

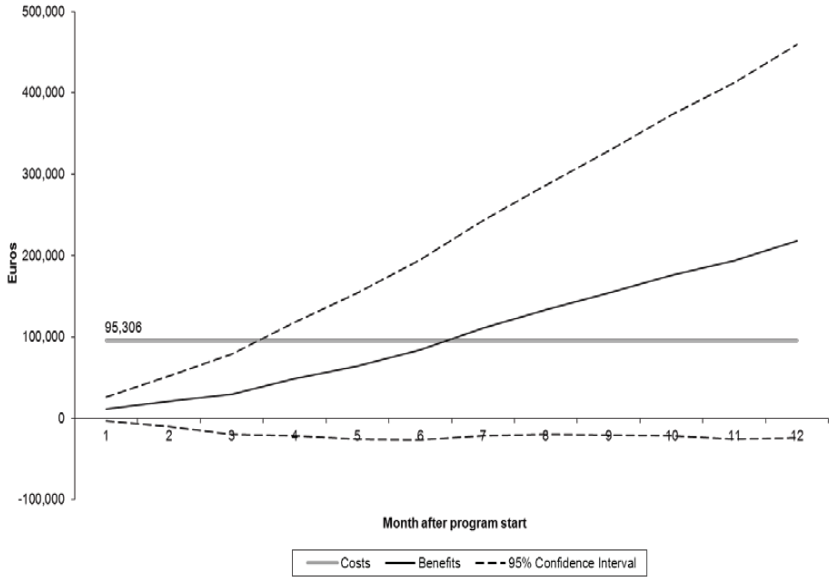


Fig. A.6.11: Cost-benefit analysis for skill provision and native women

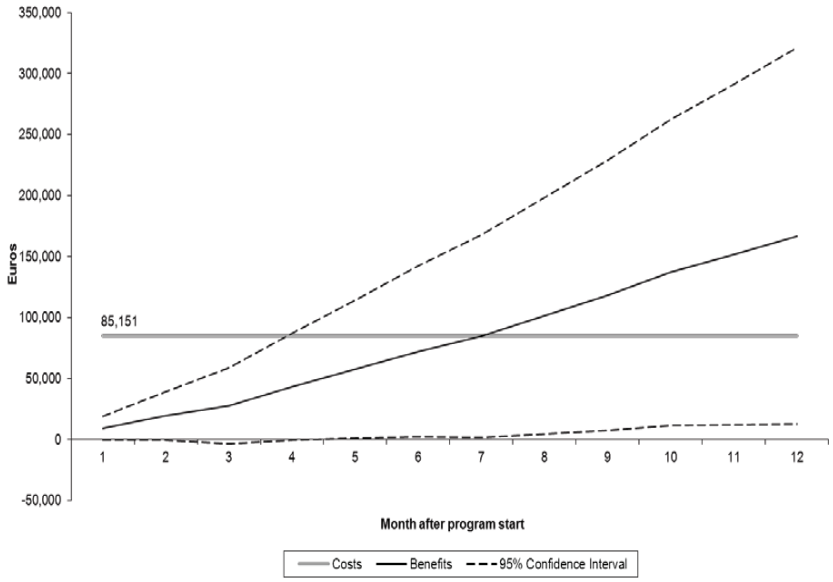


Fig. A.6.12: Cost-benefit analysis for skill provision and immigrant women

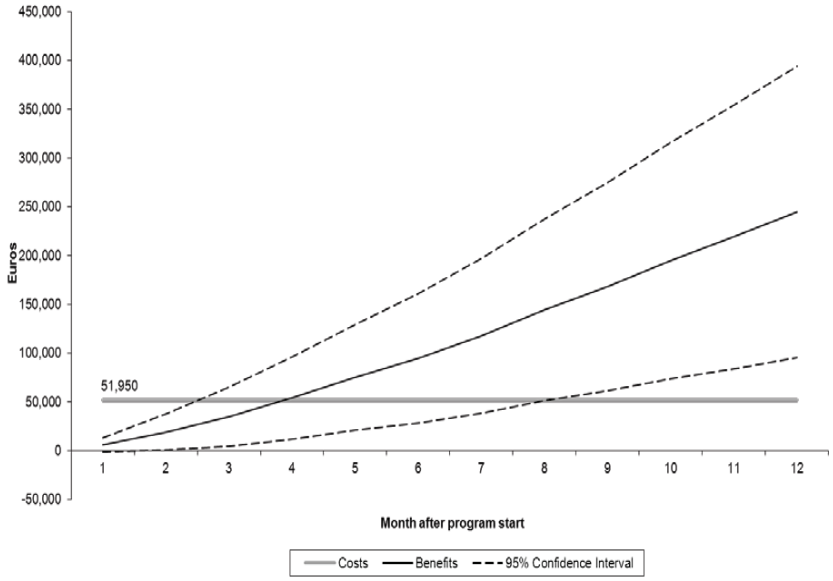


Fig. A.6.13: Cost-benefit analysis for combined training programs and native men

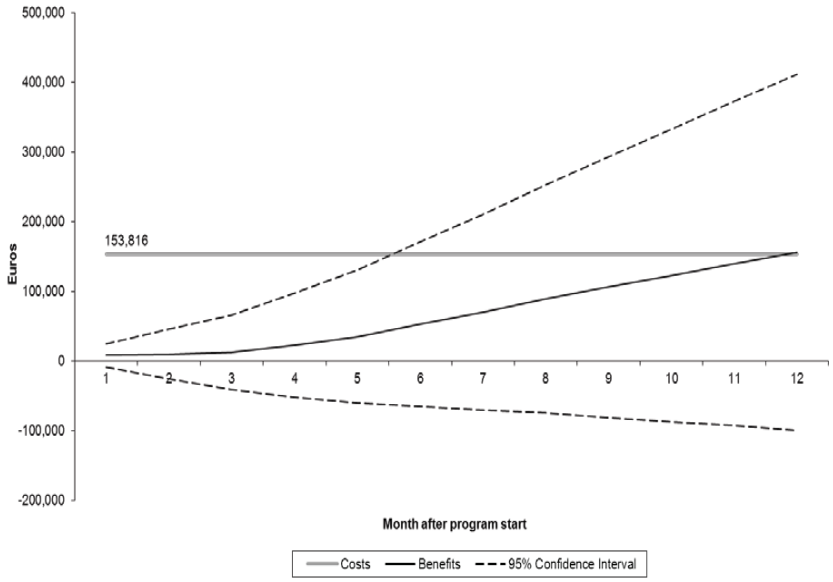


Fig. A.6.14: Cost-benefit analysis for combined training programs and immigrant men

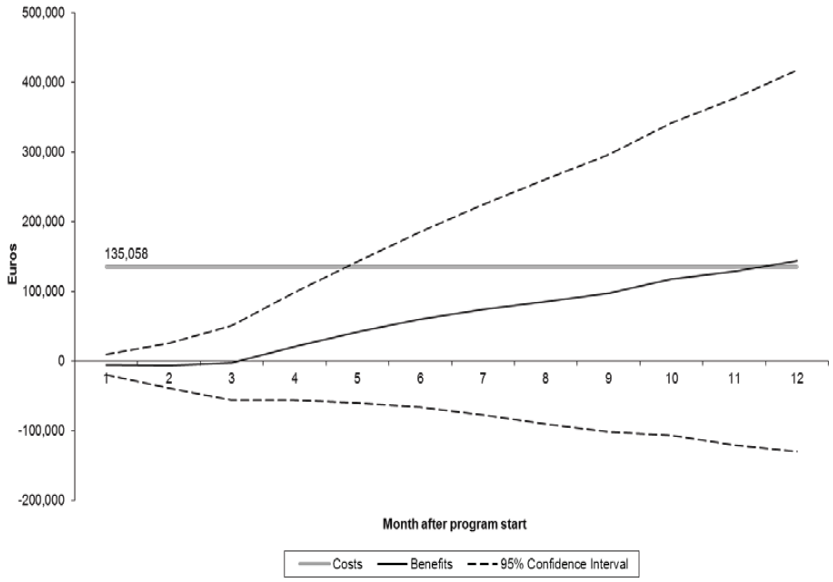


Fig. A.6.15: Cost-benefit analysis for combined training programs and native women

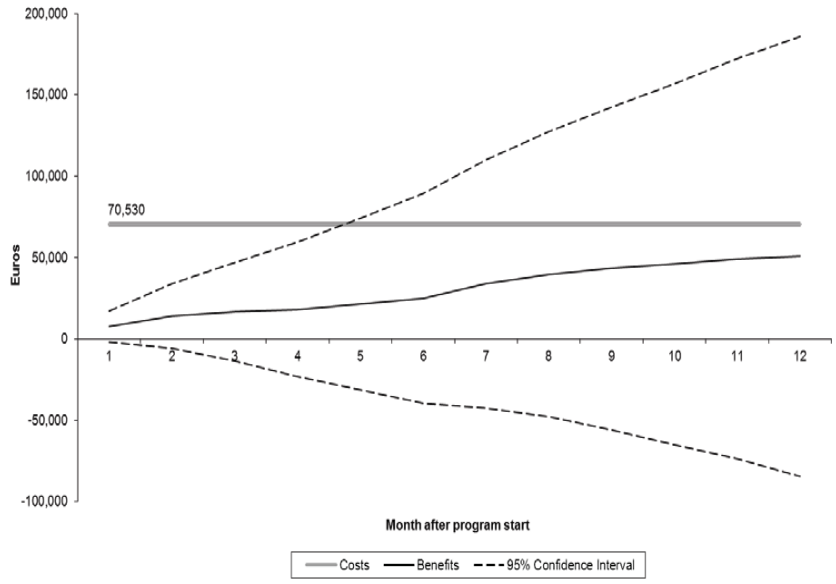


Fig. A.6.16: Cost-benefit analysis for combined training programs and immigrant women

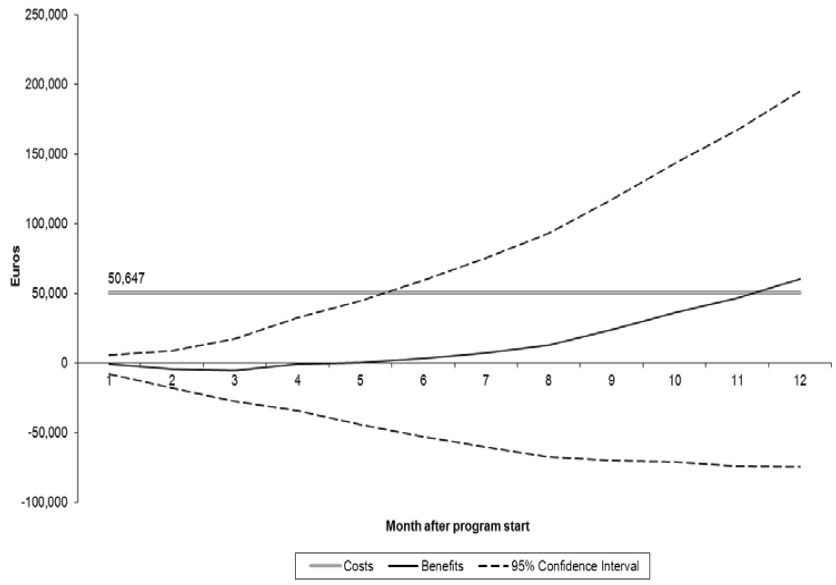


Fig. A.6.17: Cost-benefit analysis for Temporary Extra Jobs and native men

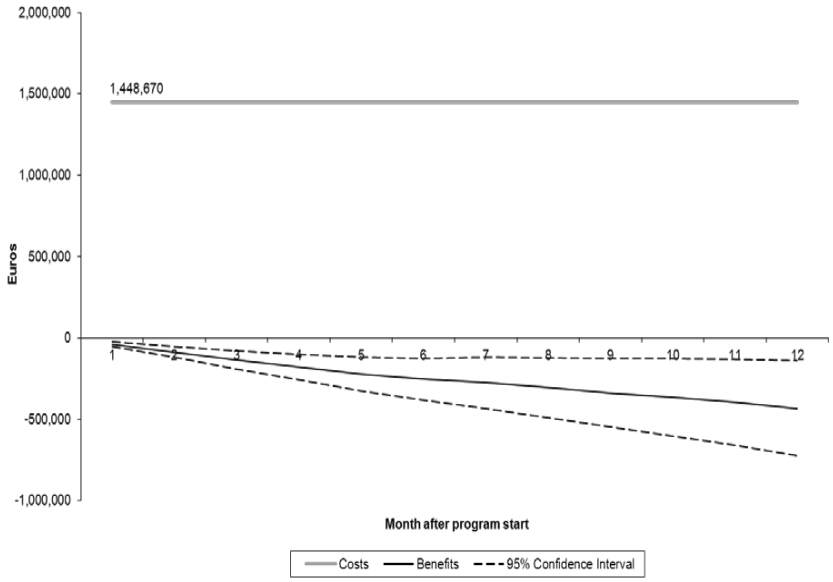


Fig. A.6.18: Cost-benefit analysis for Temporary Extra Jobs and immigrant men

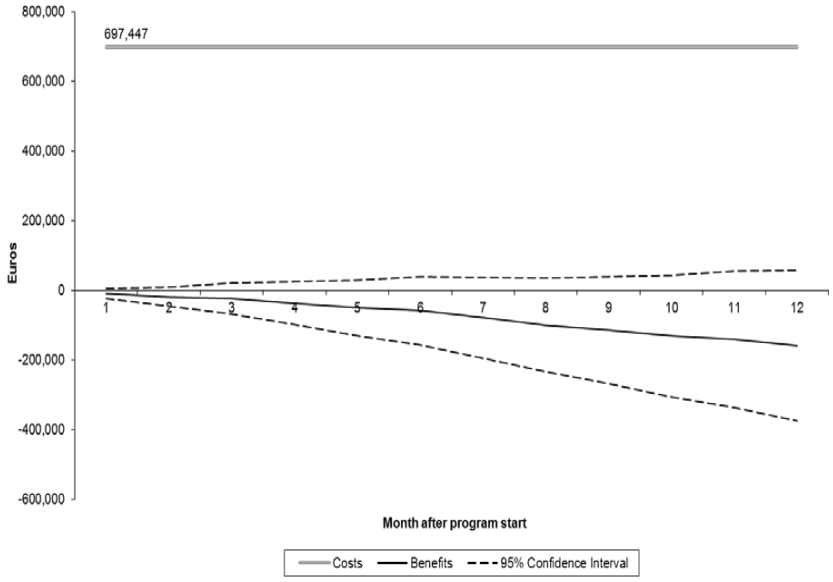


Fig. A.6.19: Cost-benefit analysis for Temporary Extra Jobs and native women

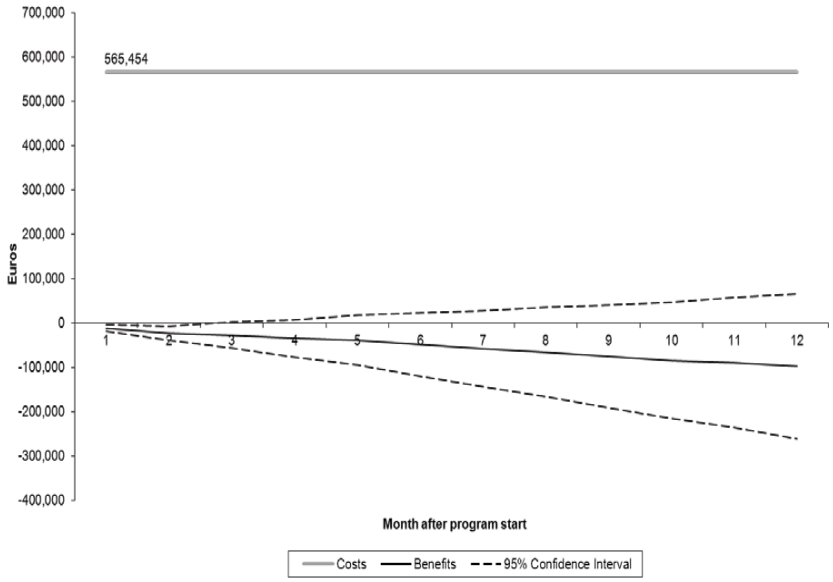
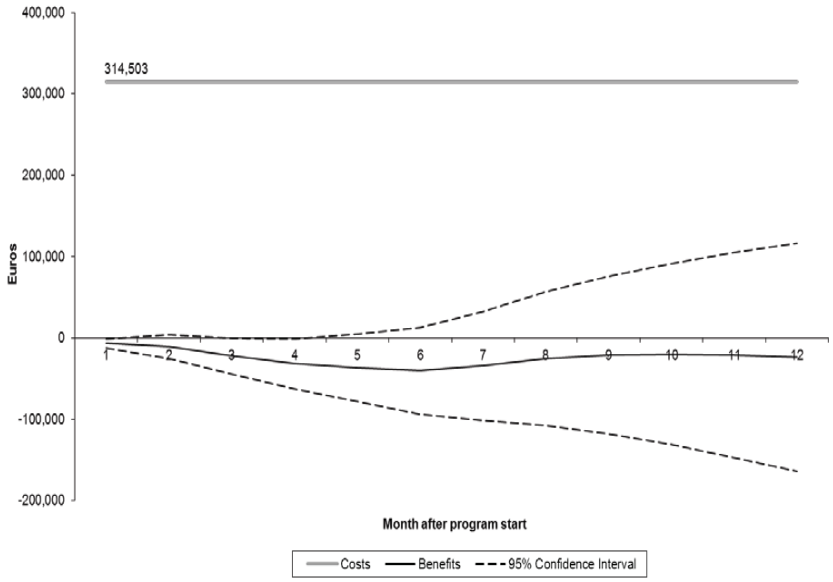


Fig. A.6.20: Cost-benefit analysis for Temporary Extra Jobs and immigrant women



List of Abbreviations

ALMP	Active Labor Market Programs
ARGE	Centralized welfare agency (*Arbeitsgemeinschaft*)
ASU/XASU	Jobseeker History (*Arbeitsuchenden-Historik*)
ATT	Average Effect of Treatment on the Treated
BASiD	Combined administrative data of the Federal Employment Agency and the German Federal Pension Insurance (*Biografiedaten ausgewählter Sozialversicherungsträger in Deutschland*)
BeH	Employee History (*Beschäftigten-Historik*)
CIA	Conditional Independence Assumption
DCIA	Dynamic Conditional Independence Assumption
FEA	Federal Employment Agency (*Bundesagentur für Arbeit*)
GDP	Gross Domestic Product
GSOEP	German Socio-Economic Panel
IAB	Institute for Employment Research (*Institut für Arbeitsmarkt- und Berufsforschung*)
IAQ	Institute for Work, Skills and Training (*Institut Arbeit und Qualifikation*)
IAW	Institute for Applied Economic Research (*Institut für Angewandte Wirtschaftsforschung*)
IEB	Integrated Employment Biographies (*Integrierte Erwerbsbiografien*)
IFE	Immigrant fixed effect
IFO	Ifo Institute for Economic Research at the University of Munich (*ifo Institut für Wirtschaftsforschung an der Universität München*)
IZA	Institute for the Study of Labor (*Forschungsinstitut zur Zukunft der Arbeit*)
LATE	Local Average Treatment Effect

LeH	Benefit Recipient History (*Leistungsempfänger-Historik*)
LEO	Local Employment Office (*Agentur für Arbeit*)
LHG/XLHG	Welfare Recipient History (*Leistungs-Historik-Grundsicherung*)
MTH/XMTH	Program Participation History (*Maßnahme-Teilnehmer-Historik*)
OECD	Organisation for Economic Co-operation and Development
OLS	Ordinary Least Squares
PASS	Panel Study "Labour Market and Social Security" (*Panel "Arbeitsmarkt und Soziale Sicherung"*)
SGB II	Book II of the German Social Code (*Sozialgesetzbuch Zweites Buch*)
SGB III	Book III of the German Social Code (*Sozialgesetzbuch Drittes Buch*)
SÖSTRA	*SÖSTRA Institut für sozialökonomische Strukturanalysen*
SUTVA	Stable Unit Treatment Value Assumption
UBI	Unemployment benefit I (*Arbeitslosengeld*)
UBII	Unemployment benefit II (*Arbeitslosengeld II*)
VbN	Data set providing information on the binary outcome variable self-sufficient employment (*Verbleibsnachweise*)
WZB	Social Science Research Center Berlin (*Wissenschaftszentrum Berlin für Sozialforschung*)
ZEW	Centre for European Economic Research (*Zentrum für Europäische Wirtschaftsforschung*)
zkT	Decentralized welfare agency (*zugelassener kommunaler Träger*)

List of Figures

3.1 Means of the outcome variable self-sufficient employment . . 43
3.2 Estimated treatment effects of decentralized welfare administration on self-sufficient employment for men 44
3.3 Estimated treatment effects of decentralized welfare administration on self-sufficient employment for women 45

A.2.1 Sampled welfare agencies 151
A.4.1 Sanction strategies of welfare agencies 177
A.4.2 Sanction rates of welfare agencies 178
A.5.1 Density distributions of the estimated propensity scores for participants in Temporary Extra Jobs 187
A.5.2 Density distributions of the estimated propensity scores for male participants in aptitude tests and job search training . 188
A.5.3 Density distributions of the estimated propensity scores for male participants in skill provision and combined training programs 189
A.5.4 Density distributions of the estimated propensity scores for female participants in aptitude tests and job search training 190
A.5.5 Density distributions of the estimated propensity scores for female participants in skill provision and combined training programs 191
A.6.1 Cost-benefit analysis for aptitude tests and native men 234
A.6.2 Cost-benefit analysis for aptitude tests and immigrant men . 234
A.6.3 Cost-benefit analysis for aptitude tests and native women . . 235
A.6.4 Cost-benefit analysis for aptitude tests and immigrant women 235
A.6.5 Cost-benefit analysis for job search training and native men 236
A.6.6 Cost-benefit analysis for job search training and immigrant men 236
A.6.7 Cost-benefit analysis for job search training and native women 237

A.6.8 Cost-benefit analysis for job search training and immigrant women 237
A.6.9 Cost-benefit analysis for skill provision and native men 238
A.6.10 Cost-benefit analysis for skill provision and immigrant men . 238
A.6.11 Cost-benefit analysis for skill provision and native women . . 239
A.6.12 Cost-benefit analysis for skill provision and immigrant women 239
A.6.13 Cost-benefit analysis for combined training programs and native men 240
A.6.14 Cost-benefit analysis for combined training programs and immigrant men 240
A.6.15 Cost-benefit analysis for combined training programs and native women 241
A.6.16 Cost-benefit analysis for combined training programs and immigrant women 241
A.6.17 Cost-benefit analysis for Temporary Extra Jobs and native men 242
A.6.18 Cost-benefit analysis for Temporary Extra Jobs and immigrant men 242
A.6.19 Cost-benefit analysis for Temporary Extra Jobs and native women 243
A.6.20 Cost-benefit analysis for Temporary Extra Jobs and immigrant women 243

List of Tables

2.1 Main features of centralized and decentralized welfare agencies . . . 12
2.2 Macroeconomic conditions in Germany (2000-2004) . . . 15
2.3 Macroeconomic conditions in Germany (2005-2009) . . . 16
2.4 Selected figures on Germany's population and welfare recipients (2005-2009) . . . 18
2.5 Number of participants in Temporary Extra Jobs and short-term training programs, and number of sanctions (2005-2009) . . . 20
2.6 Expenditures on welfare in Germany (2005-2009) . . . 21

3.1 Indicators for matching quality . . . 42
3.2 Definition of organizational approaches . . . 47

4.1 The use of benefit sanctions in welfare agencies . . . 57
4.2 Sanction rates and number of observations . . . 61
4.3 Descriptive statistics of the estimation sample . . . 62
4.4 Description of the outcome variable self-sufficient employment . . . 64
4.5 First stage estimation results based on instruments Z_1 and Z_2 . . . 66
4.6 Estimated sanction effects . . . 68

5.1 Effects of Temporary Extra Jobs . . . 101
5.2 Effects of aptitude tests . . . 104
5.3 Effects of job search training . . . 105
5.4 Effects of skill provision . . . 106
5.5 Effects of combined training programs . . . 107
5.6 Differences in treatment effects between native Germans and immigrants . . . 108

6.1 Considered costs and benefits in the analyses 122
6.2 Fiscal costs and benefits of aptitude tests 129
6.3 Fiscal costs and benefits of job search training 131
6.4 Fiscal costs and benefits of skill provision 132
6.5 Fiscal costs and benefits of combined training programs 133
6.6 Fiscal costs and benefits of Temporary Extra Jobs 134

A.2.1 Summary of data bases used in the empirical analyses 152
A.3.1 Balancing of regional variables among the sampled welfare agencies 155
A.3.2 Means of variables included in the propensity score specifications 158
A.3.3 Preferred propensity score specifications 168
A.3.4 Indicators for matching quality in subsamples 171
A.3.5 Estimated effects of decentralized welfare administration on self-sufficient employment 172
A.3.6 Estimated effects of decentralized welfare administration on self-sufficient employment for singles and non-singles 173
A.3.7 Estimated effects of decentralized welfare administration on self-sufficient employment for native Germans and immigrants 174
A.3.8 Estimated probit effects of organizational features on self-sufficient employment for men 175
A.3.9 Estimated probit effects of organizational features on self-sufficient employment for women 176
A.4.1 Regression of Z_1 and Z_2 on organizational and regional variables 179
A.4.2 Regression of Z_1 and Z_2 on caseload variables 179
A.4.3 Regression of Z_1 on strategical variables 180
A.4.4 Regression of Z_2 on strategical variables 181
A.4.5 Detailed estimation results for sanction effects based on instrument Z_1 182
A.4.6 Detailed estimation results for sanction effects based on instrument Z_2 184
A.4.7 Sanction effects for subgroups of welfare recipients 186
A.5.1 Descriptive statistics of the estimation samples for Temporary Extra Jobs and native males 192
A.5.2 Descriptive statistics of the estimation samples for Temporary Extra Jobs and native females 193
A.5.3 Descriptive statistics of the estimation samples for Temporary Extra Jobs and immigrant males 194
A.5.4 Descriptive statistics of the estimation samples for Temporary Extra Jobs and immigrant females 195
A.5.5 Means of selected variables for male participants in Temporary Extra Jobs 196

A.5.6 Means of selected variables for female participants in Temporary Extra Jobs . . . 197
A.5.7 Means of selected variables for male participants in aptitude tests . . . 198
A.5.8 Means of selected variables for female participants in aptitude tests . . . 199
A.5.9 Means of selected variables for male participants in job search training . . . 200
A.5.10 Means of selected variables for female participants in job search training . . . 201
A.5.11 Means of selected variables for male participants in skill provision . . . 202
A.5.12 Means of selected variables for female participants in skill provision . . . 203
A.5.13 Means of selected variables for male participants in combined training programs . . . 204
A.5.14 Means of selected variables for female participants in combined training programs . . . 205
A.5.15 Common support condition for the estimation of treatment effects . . . 206
A.5.16 Smith and Todd (2005b) balancing test for the estimation of treatment effects . . . 207
A.5.17 Selected propensity score specifications . . . 208
A.5.18 Smith and Todd (2005b) balancing test for the estimation of immigrant fixed effects . . . 211
A.5.19 Common support condition for the estimation of immigrant fixed effects . . . 211
A.5.20 Mean values of the outcome variable self-sufficient employment . . . 212
A.5.21 Numbers of observations in subsamples of immigrants . . . 213
A.5.22 Temporary Extra Jobs: Effects for subgroups of immigrants 214
A.5.23 Temporary Extra Jobs: Differences in treatment effects between native Germans and subgroups of immigrants . . . 214
A.5.24 Short-term training programs: Effects for subgroups of immigrants . . . 215
A.5.25 Short-term training programs: Differences in treatment effects between native Germans and Turkish immigrants . . . 216
A.5.26 Short-term training programs: Differences in treatment effects between native Germans and Eastern European immigrants . . . 217
A.5.27 Short-term training programs: Differences in treatment effects between native Germans and Southern European immigrants . . . 218
A.6.1 Selected descriptive statistics of the GSOEP sample . . . 219

A.6.2 Selected descriptive statistics of the administrative sample for male program participants 220
A.6.3 Selected descriptive statistics of the administrative sample for female program participants 221
A.6.4 Ordinary Least Squares (OLS) estimation results for the post-welfare wage equation 222
A.6.5 Cost-benefit analysis for aptitude tests and native men 223
A.6.6 Cost-benefit analysis for aptitude tests and immigrant men . 223
A.6.7 Cost-benefit analysis for aptitude tests and native women . . 224
A.6.8 Cost-benefit analysis for aptitude tests and immigrant women 224
A.6.9 Cost-benefit analysis for job search training and native men 225
A.6.10 Cost-benefit analysis for job search training and immigrant men 225
A.6.11 Cost-benefit analysis for job search training and native women 226
A.6.12 Cost-benefit analysis for job search training and immigrant women 226
A.6.13 Cost-benefit analysis for skill provision and native men 227
A.6.14 Cost-benefit analysis for skill provision and immigrant men . 227
A.6.15 Cost-benefit analysis for skill provision and native women . . 228
A.6.16 Cost-benefit analysis for skill provision and immigrant women 228
A.6.17 Cost-benefit analysis for combined training programs and native men 229
A.6.18 Cost-benefit analysis for combined training programs and immigrant men 229
A.6.19 Cost-benefit analysis for combined training programs and native women 230
A.6.20 Cost-benefit analysis for combined training programs and immigrant women 230
A.6.21 Cost-benefit analysis for Temporary Extra Jobs and native men 231
A.6.22 Cost-benefit analysis for Temporary Extra Jobs and immigrant men 231
A.6.23 Cost-benefit analysis for Temporary Extra Jobs and native women 232
A.6.24 Cost-benefit analysis for Temporary Extra Jobs and immigrant women 232
A.6.25 Total fiscal costs and benefits at the end of the observation period 233

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